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Measuring heterogeneity in the returns to education using an education reform

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ABSTRACT

In this paper, we explore the impact of a mandatory education reform as well as pre-reform availability of schools above the mandatory level, on educational attainment and returns to education in Norway. We contribute to the existing literature by focusing on the heterogeneity of the impact of reforms. Our results indicate that increased compulsory education from seven to nine years increased the general level of education beyond the compulsory education. We also find that the effect of family background on educational attainment was weaker after the reform. The average treatment effect on returns to education is surprisingly high for education of intermediate duration. This means that increasing the general level of education potentially generates high returns in the form of wages. We also find that the effect of treatment on the treated on the returns to education is 1–4 percentage points higher than the average treatment effect.

1. Introduction

Over the past 50 years, the education level in Norway has increased substantially. From being at the lower end of the education distribution of OECD countries in 1950, Norway is now at the higher end. From 1950 to 2000, the fraction of the population with secondary education increased from 13% to 56%, and the percentage of the population with university education increased from 3% to 22% (OECD, 1998; Statistical Yearbook Norway,). This increase in education is of the same order of magnitude as that of the United States in the first part of the 20th century, when the percentage graduating from high school increased from 9% to 51% (Goldin and Katz, 2003).

Many factors may explain this increase. An important factor was increased demand for educated workers because of changes in the sectoral composition of industry, from primary to secondary and tertiary industries. Another factor may be that, between the 1960s and the 1990s, the Norwegian education system went through several major reforms, one of the explicitly stated objectives of which was to increase educational attainment. The main reform extended the minimum period of mandatory education from seven to nine years in the 1960s. This reform was primarily implemented during a 12-year period from 1960 to 1972. Two distinct school systems coexisted in this period, providing us with a natural experiment in which some municipalities provided nine years of mandatory schooling, while others provided only seven years.

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In this paper, we explore the impact of the mandatory education reform and pre-reform availability of secondary and tertiary schools on educational attainment and returns to education. We contribute to the existing literature on the evaluation of school reforms in other countries by focusing on heterogeneity in the impact of the reform.[1]

First, we analyse to what degree the reform of mandatory education contributed to the increase in educational attainment in Norway in this period. We examine whether the reform explained any of Norway's move up the international education ladder, because preferences may change when students stay in school longer. It is important that we do not simply assess the effect of one-dimensional measures of years of education; rather, we must evaluate whether the reform had different effects on different types of education or qualification levels, such as vocational training (and different qualification levels of vocational training) and general high school education, a short college degree versus a bachelor degree, etc. Then, we assess whether the impact of the reform had different effects in areas with and without *pre-reform* vocational schools, high schools, regional colleges, and universities. In addition, we also analyse whether the impact of the reform depended on the social backgrounds of the pupils; thus, we test whether the reform increased the equality of opportunities for educational attainment. Because existing studies show that the propensity to participate in education differs between socioeconomic groups, we allow the impact of the reform to differ with parents' education levels and parental earnings.

Second, we examine the impact of the reform in terms of returns to education. We first establish the well-known result relating to *higher* returns to education when instrumenting for education (in our case using a control function approach) as compared with OLS results (Card, 1999). The first model is a standard wage regression in which we treat education as a continuous endogenous variable. We use the mandatory schooling reform as an identifying exclusion restriction. Not allowing for heterogeneity in returns may explain higher returns when instrumenting education as compared with OLS results.[2] To deal with the endogeneity and heterogeneity problems, we specify a generalized Roy model in the context of a control function framework with eight different types of education/qualification levels instead of years of education; see Roy (1951), Garen (1984), Willis and Rosen (1979), and Björklund and Moffitt (1987). Factors such as individual ability, time preferences, and financial constraints are usually the source of the endogeneity problem inherent in most evaluation studies; see Heckman (1974), Griliches (1977), and Gronau (1974). An additional econometric problem relates to observed and unobserved heterogeneity in the return parameters of education; see Willis and Rosen (1979), Card (1999), and Heckman and Vytlacil (1998). This heterogeneity arises if individuals select their education on the basis of their comparative advantages. Specifying the generalized Roy model requires one instrument for each education level unless restrictive functional form assumptions are imposed. We exploit the existence of *pre*-reform availability of other types of educational institutions from vocational and general high schools, via regional colleges and universities to estimate the returns to education when several levels of education are specified. We use instruments for each education level and the availability of other schools before the reform (in 1959) interacted with the compulsory reform dummy while including the first-order term as a covariate.

As well as using detailed data on education reforms, we exploit a rich database for Norway that includes detailed information on the human capital characteristics, including years and types of education, for cohorts that were exposed to the reform. In addition, we have detailed background information on their parents including their education, earnings, and the municipality in which they grew up. The sample covers individuals born between 1947 and 1958.

Our results indicate that the reform of compulsory education increased the general level of education significantly. We also find that the effect of family background on educational attainment was weakened by the mandatory schooling reform. We estimate the returns to education to be highly nonlinear and that the generalized Roy model is preferred to a traditional model in which schooling is a continuous variable. We find that upper secondary education, up to two years at a regional college, and a master's degree, have high returns in terms of wages. In addition, the average treatment effect (ATE) is surprisingly high for education of intermediate duration (that is, for up to two years of, usually regional, college education). This means that increasing the general level of education, which was the aim of the comprehensive school reform of the 1960s, has the potential to generate high returns in the form of increased wages. We find that the return to a bachelor's degree is almost 40%, while the return to a master's degree is up to 60%, relative to the returns to compulsory education. We also find that the effect of treatment on the treated (TT) is 1–4 percentage points higher than both the average treatment effect (ATE) and the conditional cumulative treatment effect (CCT) for intermediate and higher education levels. This implies that the effect of unobservables on selection into education is important.

The rest of the paper is organized as follows. In Section 2, we provide background information on the school reform and education system in Norway. Special attention is paid to our identification strategy and the robustness of the identifying exclusion restrictions. In Section 3, we discuss the data sets and define the variables used in the analysis. In Section 4, we describe the econometric framework used for estimating the returns to education. In Section 5, we report on the estimated determinants of school choice based on the regression analysis. Furthermore, we report results from earnings regressions and different estimated treatment effect parameters, such as the ATE and TT, from a model that allows for selection into education levels on the basis of comparative advantage. In the final section, we present our conclusions.

[1] See Acemoglu and Angrist (2001), Lleras-Muney (2002), and Goldin and Katz (2003) for analyses of the expansion in the US high school system between 1910 and 1940; see Oreopoulos (2006a, b) on the Canadian reform and the Canadian and UK reforms; and see Meghir and Palme (2005) on the Swedish mandatory schooling reform in the 1950s.

[2] See contributions by Aakvik et al. (2005), Card (1999), Carneiro et al. (2001), and Blundell et al. (2005).

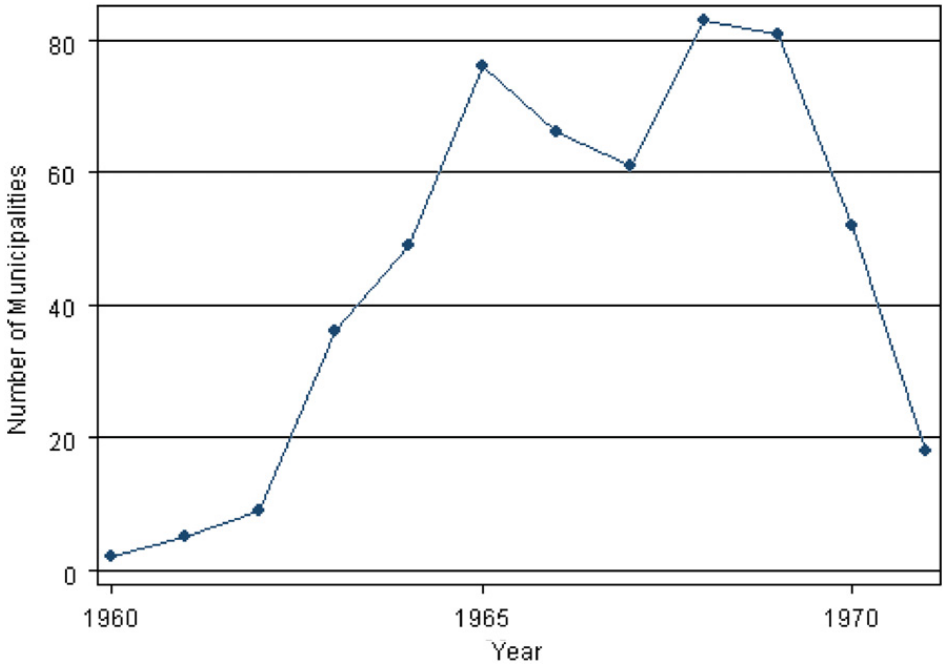

Fig. 1. Reform implementation. By number of municipalities.

2. Implementation of the reform

The mandatory schooling reform, implemented by the Norwegian Parliament in 1959, began in 1960 and lasted until 1972. The reform extended the number of compulsory years of schooling and unified the education system. Under the Norwegian pre-reform system, children started school in the year they turned seven (so they were between six and a half and seven and a half, since school starts in early August) and finished their compulsory education after seven years; i.e., at the age of about 14. The old system had a parallel system of voluntary lower secondary education, comprising two years of junior high school ("realskole"), designed to prepare students for upper secondary school followed by university studies. The parallel route was the so-called continuation school ("framhaldsskole"), which prepared students not undertaking higher professional training and theoretical education for vocational training. Selection into junior high school was based on grades, and it was available in some municipalities but not in others. Under 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 parts: first, six years of primary school; and second, three years of secondary school, which prepared students for high school.

In addition, the reform standardized the curriculum and increased access to schools, because the nine years of mandatory school was eventually made available in all municipalities. The goal of standardizing the curriculum was to improve the average level of quality of the schools; the increase in mandatory education was therefore likely accompanied by an improvement in school quality. As a result, our estimates will incorporate the increase in years of education along with an improvement in the quality. Given the positive correlation between the two, we will probably overestimate the effect of extra years of education on children's educational attainment.

The parliament mandated that all municipalities (the lowest level of local administration) must have implemented the reform by 1973; as a result, although it was started in 1960, implementation was not completed until 1972. Thus, for more than a decade, Norwegian schools were divided into two separate systems; which system you were in depended on the year you were born and the municipality in which you lived. The first cohort that could have been involved in the reform was the one born in 1947.[3] They started school in 1954, and (i) either finished the pre-reform compulsory school in 1961, or (ii) went to primary school from 1954 to 1960, followed by the post-reform middle school from 1960 to 1963. The last cohort who could have passed through the old system was born in 1958.

To receive funds from the government to implement the reform, municipalities had to present a plan to a committee under the Ministry of Education. Once approved, the costs of teachers and buildings were provided by the national government. While the criteria determining selection are somewhat unclear, the committee did want to ensure that implementation was representative of the country, conditional on having an acceptable plan (Telhaug, 1969).[4] The

[3] There were no grandfather clauses forcing pupils already in the old system to switch to the new system.

[4] In 1960, there were 728 municipalities in Norway. In the mid 1960s, the number of municipalities was reduced to about 450. Because we use the municipalities that existed in 1960 to identify the reforming municipalities, this change has no effect on the identification strategy used in this paper. The average size of a municipality in 1960 was 4807 inhabitants and about 185,000 inhabitants for counties.

Table 1
Definition of education levels applied in the analysis, birth cohorts 1947–1958.

Level	Description
1	Compulsory, pre reform (7 years)
2	Compulsory, post reform (9 years)
3	Upper secondary school 1 year; mainly vocational
4	Upper secondary school 2–3 years; mainly vocational
5	Upper secondary school 2–3 years; high school
6	University I, post upper secondary school, 1–2 years
7	University II, post upper secondary school, 3–4 years
8	University III, master level, university degree, 5+ years

implementation profile displayed in Fig. 1 illustrates the gradual change in compulsory schooling in Norway for municipalities.

Because we are using municipality fixed effects, it is not necessary that the timing of the reform be unrelated to municipality characteristics. However, it is useful to understand the determinants of the timing of the reform across municipalities. Previous research has found no relationship between municipality characteristics, such as average earnings, taxable income, and educational levels, and the timing of implementation (see Lie, 1973, 1974). To examine this issue further, we have regressed the year of implementation on different background variables based on municipality averages, including parental income and the level of education, age, size of the municipality, urban/rural status, and the unemployment rate. We also include two measures of industry composition, county dummies, and a dummy indicating support for the Labour Party at the 1961 Parliamentary election. The results, presented in Table A1 in the Appendix, indicate that there is no systematic pattern in the rate of introduction of the reforms in relation to parental average earnings, education levels, age, or the urban/rural location variable. When controlling for localization (by using dummy variables for counties), we find that none of the other variables are statistically significant. Hence, implementation of the reform seems to have been exogenous, at least to the observable factors in our data set.

3. Data sets 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. Information from different administrative registers is merged for each person in the population. The data set covers men working in all sectors—private manufacturing, private services, and the public sector—in 1995, for the birth cohorts 1947–1958.

To determine whether persons were affected by the reform, we need to link each pupil/parent to the municipality in which they grew up. We do this by matching the administrative data to the 1960 census. From the 1960 census, we know the municipality in which the pupil's mother lived in 1960.[5] At that time, the persons we are using in the estimation are aged between 2 and 13.[6] Our primary data source on the timing of the reform in individual municipalities is the volume by Ness (1971). To verify the dates provided by Ness (1971), we examined the data to determine whether or not there appears to be a clear break in the percentage of students with less than nine years of education. In rare instances when the data seems inconsistent with the timing stated in Ness (1971), we check these individual municipalities by contacting local sources. If the reform took more than one year to implement in a particular municipality or we were not able to verify the information given in Ness (1971), we cannot not assign a reform indicator to that municipality and the municipality is dropped from our sample. We are able to successfully calculate reform indicators for 545 out of the 728 municipalities in existence in 1960 (which constitutes 74% of the individuals in our sample).

In the empirical analysis, we use a levels variable for the type of education. We group our sample into the eight levels of mandatory education, through different high school routes presented and described in Table 1.

This approach to defining education categories in Norway is common and is used in the education statistics. It reflects the school system and is based on degrees taken by the students. It incorporates education in vocational schools and upper secondary schools, education of up to two years in regional colleges, university education lasting three to four years, and higher education lasting five years or more. This classification fits neatly with the Roy model of education, assuming that individuals have comparative advantages at certain education levels and act on these when choosing education. One would

[5] Because very few children live with their father in the cases where parents live apart, we should only have minimal misclassification by applying this rule.

[6] One concern is that there may be selective migration into or out of municipalities that implement the reform early. However, because the reform implementation did not occur before 1960, this could only be a problem for us to the extent that families anticipated where the reform would be implemented first and made mobility decisions prior to the 1960 census. Any reform-induced mobility subsequent to 1960 may affect the precision of our control function estimates but not their consistency. Evidence from Meghir and Palme (2005) for Sweden and Telhaug (1969) for Norway suggest that reform-induced migration was not a significant consideration.

Table 2
Availability of different types of schools in 1959, individual data.

	All individuals	All municipalities
Number of observations	203,387	542
Compulsory 9 year (reform)	0.000	0.000
Vocational school	0.727	0.507
Upper secondary school	0.586	0.304
Regional college	0.385	0.114
University	0.274	0.158

expect students to initially consider the type of education; for instance, they may choose to become a history teacher. Then, one would expect them to consider years of education; for instance, they may become a history teacher in the primary school system, which requires an undergraduate university degree, or they may choose to become a high school teacher, which requires a graduate degree. A model that specifies the type of education is expected to explain the results of student choices based on comparative advantages. Another reason for defining education categories, instead of using years of education when assessing the effect of the education reform, is that most education reforms have nonlinear effects on educational attainment when measured by years of education. Hence, it is preferable to specify types of education or degrees for comparison between the old and new education systems because a scalar measure based on years of education might not capture the effects of the reform.

We use information on the pre-reform availability of educational institutions by municipalities and counties in our identification strategy for estimating returns to education in the Roy model. Interacted with the compulsory schooling reform, the availability of schools at higher education levels, is used to form an instrument in the Roy model. This variable indicates whether an individual had access to different education levels in the same municipality for different types of high schools and colleges, and at the county level for universities. In Table 2 we report the availability of education types before the reform (1959) by the percentage of individuals who have access, and by the percentage of municipalities hosting the respective schools.[7] The availability of different types of school is an important determinant of education choice. More than 70% of those in our sample lived in a municipality that offers vocational training and education. Around 60% of the sampled individuals lived in a municipality that has an upper secondary school. About 40% have a regional college in the municipality in which they grew up. Almost 30% can attend a university in their home municipality.

We use information on family background for the period in which the person grew up and began compulsory education. This includes the parents' or guardians' income, their education, and municipality and county of residence. This information is from the census of population and housing primarily from 1960; see Vassenden (1987). For family income, we sum the father's and mother's incomes in 1970, from census data, because income is not available in the 1960 census.[8] We divide family income into quartiles. The father's and mother's education are represented by dummy variables indicating whether parents have college education.

Annual earnings in 1995 used in the wage equations 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 $5,000 US).

We restrict our sample to full-time workers, defined as those working more than 30 h a week. Workers holding multiple jobs, the self-employed, and workers participating in labour market programmes and those receiving unemployment benefits are all excluded from the sample. This amounts to about 19% of the sample. In addition, 5% of the observations on individual and family background variables are missing. The corresponding individuals are also excluded from our sample.

The 10 different male cohorts born between 1947 and 1958 comprise 314,165 individuals. Data on parents are missing for around 18% 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 municipalities they went to school. This is significant information because it is used to construct our identifying exclusion restrictions (the availability of different types of school). Second, family background variables, such as parental education and income, are important factors in modelling the level of education. We have removed from the sample individuals for whom information on their parents is missing from either the 1960 or the 1970 census. Descriptive statistics for the final sample of 203,387 individuals are presented in Table 3.

The age in 1995 of the individuals in our sample range from 38 to 47 years. Mean work experience is 21 years with a standard deviation of 4, and mean tenure is 7.7 years with a standard deviation of almost 6. Mean years of education is 12.1.

[7] Although we have information on eight different levels of education, we only have information on five different types of school pre-reform. The reason is, for instance, that two vocational levels correspond to the same type of school, and the two upper education levels correspond to university degrees (bachelor's and master's degrees or higher).

[8] This is the first year of income data available for the population. It would have been preferable to have pre-reform income data because post-reform income may also reflect decisions to work, for example for mothers when the children had to attend school longer as a consequence of the reform. All the other family background variables are pre-reform.

Table 3
Descriptive statistics for the net sample of 203,387 individuals.

Variables	Mean	Standard deviation
Education in years 1995	12.09	2.77
Compulsory, pre reform (7 years)*	7.3	2.6
Compulsory, post reform (9 years)*	10.53	30.6
Upper secondary school 1 year; mainly vocational*	16.60	37.2
Upper secondary school 2–3 years; mainly vocational*	21.94	41.4
Upper secondary school 2–3 years; high school*	10.76	30.9
University I, post upper secondary school, 1–2 years*	13.70	34.4
University II, post upper secondary school, 3–4 years*	9.46	29.2
University III, master level, university degree, 5+ years*	9.69	29.6
Age in 1995	42.26	3.41
Tenure 1995	7.74	5.94
Earnings 1995 in NOK	293,342	134,570
Log earnings 1995	12.51	0.39
Experience in years in 1995	21.08	4.35
Father has college 1960 (dummy variable)	0.095	0.293
Mother has college 1960 (dummy variable)	0.031	0.173

Note: * indicates that the numbers are presented in percent.

Around 10% of the sample has a father with a college degree. This is not surprising because the level of education for persons with children born in the 1950s is low. Only 3% of the sample has a mother with a college degree.

4. Models for estimating educational attainment and returns to education

We estimate two separate models for assessing the effect of the reforms. The main model is used to estimate the returns to different levels, or types, of education in a comparative advantage framework. A simpler model, in which schooling is treated as a continuous endogenous variable (in which we also use the eight-level categorization reported in Table 1) is also estimated. We compare the results from the main model with those from the simpler model. The latter is estimated so that we can compare our results with those from other studies, which, in most cases, define education in terms of the number of years spent at school. For both models, we estimate returns to education by using a control function approach. We first present the estimation procedure for educational attainment that forms the basis for calculating the endogeneity correction terms in both models of returns to education.

4.1. Educational attainment: ordered probit specification

We use an ordered probit model to estimate the effect of school reforms on completed school outcomes.[9] This equation is used as the first step when estimating the earnings equation. The ordered probit model is based on the latent regression equation:

$$S_i^* = \beta_R R_i + F_i\beta_F + (R_i \times F_i)\beta_{RF} + X_i\beta_X + \gamma_C + \delta_M + \gamma_C \cdot \delta_M + V_i, \quad (1)$$

where S_i^* represents the optimal level, or type, of schooling. The explanatory variables include the reform indicator variable for the mandatory schooling reform, R_i. In addition, we include a vector of family background variables, F_i, which comprise the father's and mother's income in 1970 (in quartiles), and the father's and mother's education (represented by a dummy variable indicating whether they have college degrees). We interact all the family background variables with the instruments ($R_i \times F_i$) to allow the education reform to depend on the family variables. Furthermore, we also include birth cohort dummies, γ_C, to capture the secular trend in educational attainment in Norway in this period, and an indicator of the municipality in which the person grew up, δ_M, which reflects Norway's municipality structure in 1960. Lastly, we test for a municipality specific linear trend ($\gamma_C \cdot \delta_M$), but it turned out that this trend did not make any difference to the results.[10] As it was found to be insignificant we excluded this term from the final regressions. V_i denotes an error term that is assumed to be normally distributed, $E(V) = 0$ and $Var(V) = 1$. Coefficients for the three vectors, γ_C, δ_M, and $\gamma_C \cdot \delta_M$, are not reported in the tables. We also include the variables that are in the wage equation, experience and tenure polynomials (X_i), because we use this as the first stage to form the generalized residual to be used in the earnings equation.[11]

Three points about Eq. (1) are worth noting. First, the cohort, or age, effects are needed to allow for secular changes in educational attainment over time that might be unrelated to the reform. Second, the municipality fixed effects capture

[9] The sequential probit model can be used as an alternative to the ordered probit model when the dependent variable (the level of schooling) can be separated into a sequence of binary choices (see Cameron and Heckman, 1998).

[10] This result is in line with Black et al. (2005).

[11] We also estimated Eq. (1) without using the X_i vector, but the results are very similar so we only report the results with the X_i vector.

unmeasured time-invariant characteristics that could have affected the adoption of the mandatory schooling reform and school choice, thereby allowing for non-random timing of the reform with respect to education choice. Third, we also interact parents' education and income levels with the reform dummy. Thus, we control for different socioeconomic groups and allow the school reform to affect these groups differently.

Turning now to the specification of the ordered probit model for the comparative advantage model, it requires a valid instrument at each level of education. In this version of the probit model, we include information on the pre-reform availability of different types of schools at the municipality level (O_i) in addition to the reform variable. In particular, we use interaction terms between the reform of compulsory schooling (R_i) and the availability of other types of schools, such as the presence of vocational schools, upper secondary schools, regional colleges, and universities, as identifying instruments in addition to the compulsory schooling reform:

$$S_i^* = \beta_R R_i + O_i\beta_O + (R_i \cdot O_i)\beta_{RO} + F_i\beta_F + X_i\beta_X + \gamma_C + P_M\beta_M + V_i, \tag{2}$$

where O is a vector of other school types. In this specification, we capture municipality fixed effects by using a vector of proxy variables P_M rather than by using a dummy variable for each municipality (δ_M). There is insufficient variation in the availability of other types of schools (vocational schools, upper secondary schools, regional colleges and universities) to include both municipality dummy variables and indicator variables for these types of schools.[12] The municipality proxy variables P_M include the municipality average proportion of mothers and fathers with college degrees, municipality average income for mothers and fathers according to the 1970 census, the municipality average age of mothers and fathers, the size of the municipality, the average unemployment rate, the ratio of the number of workers in the manufacturing sector to the number in the service sector, and the share of the popular vote won by the Labour Party, which was the largest political party at the time. Although these variables capture many of the factors that a simple fixed effects specification incorporating dummy variables for all municipalities would capture, they have the additional advantage of exhibiting time series variation. We tested the use of these municipality characteristics as compared with the use of municipality fixed effects in the simple probit model in Eq. (1) where only the mandatory reform is used as an instrument, and obtained very similar results. As we already have noted, municipality specific trends were rejected by an F-test and are not included in the final estimation of Eqs. (1) and (2).

We use the results from the estimated education choice model, reported in Eq. (2) of Table 5, to predict the probabilities that individuals attain different qualifications. We also use the model to construct the selection term (the generalized residual) used in estimating the earnings equation. For economy of notation, let Z_i comprise the excluded variables in our earnings specification (R_i and O_i and their interactions), and let Q_i include all the other variables. Thus, we predict $\Pr(S_i = l|Q_i, Z_i)$ for all persons in the sample. In the ordered probit model, $\Pr(S_i = l|Q_i, Z_i)$ is

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

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

We also use the ordered probit model to calculate the generalized residual for each level of education, called ξ. This new variable is used to model the unobserved variables in the earnings equations. The ξ s for each education level are calculated as follows:

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

where ϕ is the probability density function of the normal distribution. These two equations are of course estimated separately for the two models.

4.2. Estimating returns to education: the extended Roy model

The most flexible way to incorporate both nonlinear returns to education and heterogeneity in the returns is to specify a Roy model where we estimate an earnings regression for each level of education; see, for instance, Garen (1984) and Heckman et al. (2003). The advantage of using such a switching regression framework is that the returns to education are allowed to vary in terms of both observed and unobserved individual factors at all levels of education. For each education level l, the model has the following regression specification:

$$E[y_{li}|F_i, X_i, \gamma_C, P_M, \xi_{li}; D_{li} = l] = a_{l0} + F_i\beta_{lF} + X_i\beta_{lX} + \gamma_{lC} + P_M\beta_{lM} + \psi_l\xi_{li}. \tag{5}$$

Note that the parameters in Eq. (5) vary for each education level, l, which enables us to analyse heterogeneity in terms of observed factors. The results from the different regressions in Eq. (5) can be used to predict the outcomes for education levels other than the one observed for individual i. The returns to education are 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 receiving schooling level l and the predicted outcome for the same person if he or she had only compulsory schooling. The return to schooling is then an average of these

[12] For instance, four municipalities have a university. However, these universities existed for the entire sample period. Thus, including both a municipality dummy variable and a dummy variable for the presence of a university would cause perfect multicollinearity.

differences. For instance, averaging over the whole population yields the ATE. Averaging over the sub-sample of persons with the schooling level l yields the TT. The TT parameter, given specific values of X and S, is given by

$$\Delta^{TT(S=l,q)} = F_i(\beta_{lF} - \beta_{1F}) + X_i(\beta_{lX} - \beta_{1X}) + (\gamma_{lC} - \gamma_{1C}) + P_M(\beta_{lM} - \beta_{1M}) + (\psi_l - \psi_1)\xi_{li}. \quad (6)$$

The parameters β_F, β_X, β_M and ψ are obtained by estimating Eq. (5) for each education level. Unconditional estimates of Eq. (6), i.e. $\Delta^{TT(S=l)}$, can be found by integrating $\Delta^{TT(S=l,q)}$ over the distribution of background variables, $Q = (F_i, X_i, \gamma_C, P_M)$.

The TT and the ATE are interesting summary measures of the effect of reforms such as the reform of comprehensive schooling. The ATE parameter measures the effect of selecting a person at random from the population into a different education level than the one in which that person is currently in. 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 particularly useful when calculating the returns to schooling for a group of individuals with a particular level of education.

A new parameter that we propose is a summary measure of the effect on earnings of a given schooling level (l) for individuals at or below level l. We can then compare the returns to education level l for those who have that level l of schooling with the returns to l for those who have a level of less than schooling level l. We call this parameter the conditional cumulative treatment effect (CCTE).

The returns to education parameters in the Roy model, such as the one given in Eq. (6), have two heterogeneity components. The first is related to observable variables. In Eq. (6), the effects of observed individual and family background variables (X_i and F_i) are allowed to differ for different schooling levels. Thus, the effect of, for instance, family income during childhood is allowed to differ for different education levels. We also allow the effect of the municipality variables, P_M, and those of the birth cohorts, γ_C, to differ for each education level. Second, the effect of the unobservables (ξ_i) is also allowed to differ for different schooling levels. Thus, the effect of, for instance, unobserved ability can vary between education levels for the same individual. We now have a comparative advantage model in terms of both observed and unobserved factors, in which individual behaviour is based on individual differences. The estimation of treatment effects can be sensitive to imprecise parameter estimates in the earnings equation.

A note on the specification of the earnings model is in order. We do not include the current area of residence or the industry of employment in the equation because these variables are potentially endogenous to education choice. For instance, suppose that the reform caused students to undertake more education and that this led them to settle in urban areas where the returns to education are higher than in rural areas. Alternatively, they may have taken jobs in high-paying industries. Not including industry and residential area variables in the wage equation (or in the education choice equation) implies that these variables are considered to be caused by education.

4.3. The earnings specification when education is continuous

We briefly present the procedure for estimating the returns to education within a more standard framework where education is a continuous variable. Consider the following model of log earnings:

$$y_i = a_0 + F_i\beta_F + X_i\beta_X + \gamma_C + \delta_M + (\overline{b} + b_i)S_i + a_i, \quad (7)$$

where S is the continuous schooling variable, and F_i, X_i, γ_C, and δ_M are variables already defined. Heterogeneity in the returns to education is specified by decomposing the returns to education parameter b into two parts; i.e., $b = \overline{b} + b_i$, where $\overline{b}$ is the common (average) return to education and b_i is the random return parameter of schooling, which we allow to differ between individuals. Let α_i be the heterogeneity that affects the level of earnings, and we divide α_i into two parts, so that $\alpha_i = a_0 + a_i$. Both b_i and a_i are unobserved variables.

There are two potential sources of bias in equation (7). First, there is the standard ability bias problem, which is a result of the correlation between a_i and S_i. The second problem is a result of heterogeneity in the returns to education, which arises if there is a correlation between b_i and S_i. We use a control function approach to model the effect of unobserved factors. We assume that:

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

and that:

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

where $Q_i = (F_i, X_i, \gamma_C, \delta_M)$, $Z_i = (R_i, R_i \times F_i)$, $\lambda = Cov(a_i, S_i)/Var(S_i)$, and $\psi = Cov(b_i, S_i)/Var(S_i)$.

In this context, ξ_i is pooled across education levels because S_i is a continuous variable. Hence, we have the following earnings equation:

$$E[y_i|Q_i, S_i, \xi_i] = a_0 + F_i\beta_F + X_i\beta_X + \gamma_C + \delta_M + \overline{b}S_i + \psi\xi_iS_i + \lambda\xi_i. \quad (8)$$

Card (1999) and Wooldridge (2003) use similar specifications. Applying OLS to Eq. (8) yields a consistent estimate of the average effect of schooling on earnings. This is a random coefficients model of earnings. We also estimate a simpler

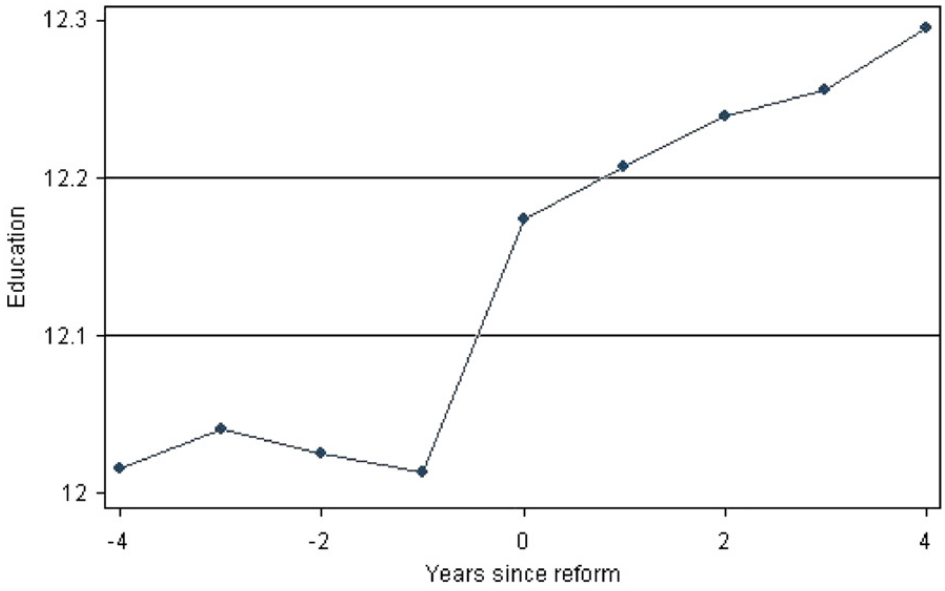

Fig. 2. Average level of education in years surrounding the year of reform.

selection model (equivalent to a two-stage selection model). Although this model does not allow for heterogeneity in the returns to education, it does allow for endogeneity as follows:

$$E[y_i|Q_i, S_i, \xi_i] = a_0 + F_i\beta_F + X_i\beta_X + \gamma_C + \delta_M + \overline{b}S_i + \lambda\xi_i, \tag{9}$$

where the vector Q_i includes family background variables, work experience, and tenure, cohort, and municipality dummy variables.

Within the framework of a selection model of (9), the interpretation of $\overline{b}$ is similar to that of the local average treatment effect (LATE) for discrete treatments; see Angrist et al. (1996), and Oreopoulos (2006a). Thus, the returns to education are returns for those individuals who are motivated to increase their education because of the reform. For a given level of ability, the school reform reduces the cost to individuals of increasing their education. Hence, we expect able pupils from low income or poor families to benefit from the school reform and take up more education. Because those who increase their education have greater ability, we expect that the LATE of education is higher than the average effect for the population. By capturing both the ability and the cost effects, the random coefficients specification in equation (8) implies that the $\overline{b}$ parameter is interpreted as standard average returns to education.

5. Results: estimating educational attainment

We begin analysing the impact of the compulsory schooling reform on educational attainment by presenting some descriptive statistics. Fig. 2 shows the average years of completed education for cohorts up to four years before and four years after the reform for each municipality. The figure shows an increase in the average years of education from 12 to 12.25 years after the reform.

In Table 4, we present the effect of the reform on the distribution of education using our grouping of education presented in Table 1. Table 4 shows the unconditional proportion of persons in different qualification groups by reform status, and the difference in educational attainment between the two sub-samples using the average of cohorts three, four, and five years prior to and after the reform in each municipality.

Note first a decrease in the percentage of students with less than seven years' education after the school reform, which is expected. For the remaining levels with the exception of the highest university level, educational attainment is higher for post-reform than pre-reform individuals. For example, the proportion of people with upper secondary school is about 10% for the pre-reform sample, and 12% for the post-reform period, which represents an increase of more than 15%. Similarly, we see an 18% increase for students who have completed three years of vocational education. All the effects are significant up to the completed high school level with the strongest effect obviously at the bottom of the education distribution. The only exception to the result of a positive effect on education levels of the reform is an insignificant effect above high school level. At the highest level of education the education attainment is lower for the post-reform sample and the difference is here significantly different from zero. There was no increase for university education in this period that may explain this result.

We now go on to present the results from estimating the ordered probit model of education choice for the simple specification given in Eq. (1). The results for the ordered probit model are presented in Table 5. An important result is that the reform indicator has a positive and highly significant effect. This means that pupils from reformed compulsory

Table 4
Observed distribution of qualification levels by reform status, birth cohorts 1947–1958.

Levels		Post-reform	Pre-reform	Change	*t*-value	Change %
1	Compulsory, pre reform	0.0041	0.1327	−0.1286	(70.6)	−3137
2	Compulsory, post reform	0.1316	0.0801	0.0515	(23.5)	39.13
3	Vocational I	0.1756	0.1539	0.0217	(8.1)	12.36
4	Vocational II	0.2425	0.1978	0.0447	(15.0)	18.43
5	Upper secondary	0.1189	0.1006	0.0183	(8.2)	15.39
6	University I	0.1384	0.1351	0.0033	(1.4)	2.38
7	University II	0.0956	0.0952	0.0004	(0.1)	0.42
8	University III	0.0935	0.1045	−0.0110	(5.1)	−11.76

Note: The pre-reform group consists of cohorts 3, 4 and 5 years prior to the implementation of the reform in a municipality. The post-reform consists of cohorts 3, 4 and 5 years after the reform took place in a municipality.

Table 5
Educational choice estimated by ordered probit model, birth cohort 1947–1958.

	Eq. (1)		Eq. (2)	
	Coefficient	St. error	Coefficient	St. error
Father college	0.410	0.011	0.399	0.010
Mother college	0.358	0.014	0.353	0.014
Family income 2	0.093	0.008	0.078	0.007
Family income 3	0.234	0.008	0.205	0.008
Family income 4	0.385	0.008	0.344	0.009
Reform dummy	0.139	0.008	0.161	0.011
Vocational school			0.026	0.012
Upper secondary school			0.003	0.012
Regional college			0.003	0.011
University			−0.001	0.012
Reform x Vocational			−0.008	0.016
Reform x Upper secondary			0.029	0.012
Reform x Regional college			0.023	0.015
Reform x University			−0.031	0.014
Pseudo R-squared	0.1222		0.1237	
LR chi2	360,715		361,129	
Observations	203,387		203,387	
Experience and Tenure (X_i)	Yes		Yes	
Cohorts dummies (y_C)	Yes		Yes	
Municipality fixed effects (δ_M)	Yes			
Municipality variables (PM)			Yes	

Note: Family income is divided into quartiles. Robust standard errors, clustering adjusted. Municipality variables (P_M) are defined in Section 4.1. Municipality fixed effects (δ_M) are dummy variables for each municipality. Eq. (2) reported here is used to calculate ξ_{li} in Eq. (4), which is then included in Eq. (5) (earnings regressions/Roy model).

schooling generally have a significantly higher level of education than those from non-reformed schools, and that this yields a potentially strong identifying exclusion restriction for use in the earnings regression. In addition, the other variables are statistically significant.

Because the results presented under Eq. (1) in Table 5 are also the first-stage results for the simple control function wage equation, we provide some additional information regarding the impact of the reform analysed using a placebo experiment. Here we use up to six cohorts in each municipality, where none of these cohorts were affected by the reform. This means that they are too old to be affected by the reform. We then create a dummy variable that has the value 1 for the cohorts two and three years before the reform was implemented in a municipality. The reform placebo variable takes the value zero for the cohorts four, five and six years prior to the implementation of the reform in a municipality. This is Model 1 in Table A2 in the Appendix. We also try other variants of the placebo experiment. In Model 2 in Table A2, the cohorts one, two, and three years prior to the reform have a reform dummy equal to 1, while the cohorts four, five, and six years prior to the reform have a reform dummy equal to 0. In Model 3, the cohort one year prior to the reform has a reform variable equal to 1, and the cohort two years prior to the reform has a reform dummy equal to 0. In the last model in Table A2, we randomize a reform dummy for all cohorts prior to the reform. In all our placebo experiments (or falsification tests) the reform variable is not significantly different from zero. This means that we reject the hypothesis that the reform had an effect for cohorts not affected by the reform.

Table 6
Educational choice.

	Level 1	Level 2	Level 3	Level 4	Level 5	Level 6	Level 7	Level 8
Father college	−0.0233	−0.0419	−0.0590	−0.0382	0.0112	0.0503	0.0536	0.0473
Mother college	−0.0197	−0.0355	−0.0500	−0.0322	0.0097	0.0428	0.0453	0.0396
Family income 2	−0.0066	−0.0105	−0.0130	−0.0055	0.0043	0.0123	0.0110	0.0081
Family income 3	−0.0169	−0.0278	−0.0356	−0.0173	0.0104	0.0328	0.0307	0.0238
Family income 4	−0.0264	−0.0446	−0.0590	−0.0324	0.0148	0.0528	0.0520	0.0428
Reform dummy	−0.0121	−0.0188	−0.0226	−0.0087	0.0080	0.0217	0.0189	0.0137
Ref x Father coll	−0.0000	−0.0001	−0.0001	−0.0000	0.0000	0.0001	0.0001	0.0000
Ref x Mother coll	−0.0014	−0.0023	−0.0028	−0.0011	0.0009	0.0027	0.0023	0.0017
Ref x Fam inc 2	0.0001	0.0001	0.0001	0.000	−0.0000	−0.0001	−0.0001	−0.0000
Ref x Fam inc 3	0.0022	0.0034	0.0041	0.0015	−0.0015	−0.0040	−0.0034	−0.0024
Ref x Fam inc 4	0.0041	0.0063	0.0074	0.0026	−0.0027	−0.0072	−0.0061	−0.0043

Marginal effects from ordered probit model. Only coefficients are reported. *Note*: other control variables are experience and tenure (X_i), cohort dummies (Y_C) and municipality fixed effects (δ_M). Robust standard error, clustering adjusted. All coefficients are significant on at least the 1% level.

Table 7
Predicted distribution of qualification levels by reform status based on ordered probit models.

Levels		Post-reform	Pre-reform	Change	*t*-value	Change%
1	Compulsory, pre reform	0.0188	0.1082	−0.0894	(59.4)	−475.53
2	Compulsory, post reform	0.1412	0.0749	0.0663	(33.1)	46.95
3	Vocational I	0.1743	0.1514	0.0229	(3.4)	13.14
4	Vocational II	0.2189	0.2125	0.0064	(0.7)	2.92
5	Upper secondary	0.1148	0.1001	0.0147	(4.5)	12.80
6	University I	0.1399	0.1341	0.0058	(2.8)	4.15
7	University II	0.0958	0.0981	−0.0023	(0.1)	−2.40
8	University III	0.1018	0.0976	0.0042	(2.6)	4.13

Birth cohorts 1947–1958. *Note*: The pre-reform group consists of cohorts 3, 4 and 5 years prior to the implementation of the reform in a municipality. The post-reform consists of cohorts 3, 4 and 5 years after the reform took place in a municipality. Control variables include experience and tenure (X_i), cohort dummies (Y_C) and municipality fixed effects (δ_M).

Possible heterogeneous effects of the reform are explored in several ways. In Table 6, we interact the reform dummy with the parental variables. The interaction between father's education and the reform is denoted Ref x Father coll, etc. The rest of the estimated coefficients are suppressed for ease of exposition. We now report the marginal effects of the covariates, and for each schooling level. The effect of the parental variables on the offspring's level of education confirms the findings from Table 5: high education and high earnings both decrease the probability of choosing a low level of education (up to level 4 Vocational II) and increase the probability of choosing a relatively high level (from level 5 Upper secondary). As for earnings, this effect increases from being quite modest for the second quartile to being quite large for the fourth.

The strongest effect of the interaction terms comes from the category of parents in the fourth earnings quartile. We see that the way in which high parental earnings reduces the probability of choosing a low level of education and increases the probability of choosing a high level of education is significantly modified after the reform. Some of this pattern is also sizeable in the third quartile. As for the first earnings quartile and parents with college education, the reform does not alter the effect across levels of education.[13] This lends support to the view that the reform achieved some of its central aims, namely to smooth transition to higher education, and to increase equality of opportunity along socioeconomic lines. Table 7 presents the predicted distribution based on Eq. (3) of qualification levels.[14]

It appears that the reform, taking account of the positive trend in educational attainment, parents' income, and so on, had an impact, particularly at the lower levels of higher education. We also find a positive effect of the reform for the highest level of education. However, the effect of the reform is not statistically different from zero for the groups Vocational II and University II (master degree) in Table 7.

[13] Possible heterogeneous effects of the reform were also explored by estimating the probit model for sub-samples defined by parental education and parental earnings, respectively. The reform had a positive effect on children's educational attainment whether they have parents with college education or not, but the effect is much stronger for the latter category. A similar pattern is revealed for parental earnings: the effect is strongest for the lowest quartile of the earnings distribution, and then diminishes monotonically.

[14] Presenting predicted pre- and post-reform education levels is an alternative to presenting the parameter estimates from the ordered probit model, which have a more awkward interpretation.

Table 8
Percentage of individuals with different levels of education matched with availability of different types of schools at the municipality.

		Reform status		
		1	0	
Vocational school	1	0.3970	0.3451	0.0519 (20.7)
	0	0.4573 *−0.0603 (31.7)*	0.3917 *−0.0466 (22.8)*	0.0656 (10.9) ***−0.0137 (0.18)***
Upper secondary	1	0.1271	0.1114	0.0157 (20.3)
	0	0.1009 *0.0262 (43.7)*	0.0851 *0.0263 (30.9)*	0.0158 (13.3) ***−0.0001 (0.17)***
Regional college	1	0.2602	0.2501	0.0101 (49.3)
	0	0.2182 *0.0420 (18.2)*	0.2152 *0.0349 (36.9)*	0.0030 (15.8) ***0.0071 (2.8)***
University	1	0.1148	0.132	−0.0172
	0	0.0805 *0.0343 (30.0)*	0.0871 *0.0449 (22.3)*	−0.0066 ***−0.0106 (5.0)***

Note: *t*-value in parenthesis. The proportion of persons with vocational education (groups 3 and 4 in Table 1) is matched with vocational schools, the proportion of persons with upper secondary education (high school) is matched with upper secondary schools, the proportion with college degrees from a regional college (group 6 in Table 1) is matched with regional colleges, and the proportion with university degrees (from groups 7 and 8 in Table 1) is matched with universities.

5.1. Result for municipalities conditional on the availability of other schools

The effect of the mandatory schooling reform may differ between municipalities depending on the pre-reform availability of other schools. We expect the effect of the reform to be stronger in municipalities in which other schools are available because the cost of attaining higher education is lower. On the other hand, it might be that the marginal student considering, say, vocational training in an area where vocational education is available, differs from the marginal student considering vocational training in an area without vocational education. If the proportion of students taking vocational training is higher in an area close to vocational schools, one would expect the marginal student considering vocational training following mandatory schooling reform to have a lower marginal utility of vocational training although the costs of attending vocational school are lower. Such a student would not necessarily attend the school because of this countervailing effect. We attempt to evaluate these effects in this section.

We present the result for the estimated education participation (first-stage) equation for the Roy model given in Eq. (2), in which we use the full set of identifying exclusion restrictions.[15] We also report the predicted effects for participation in different types of education given that this particular type of education is available. We use information on the availability of these schools prior to the reform (1959), *interacted* with the reform of the compulsory schooling dummy as our identifying exclusion restrictions. In addition, we use the availability of different schools prior the reform as main effects in the regressions.

Rather than reporting the estimation results from the probit model in Eq. (2), we illustrate the results in difference-in-difference tables. In Table 8, we illustrate the effect of the mandatory schooling reform conditional on the availability of different school types (see Table 2). Thereby we illustrate the rationale behind the difference-in-differences strategy in the Roy model by using a two-by-two table for each type of school (vocational, upper secondary, regional college, university). The table shows the predicted participation rates using Eq. (3) for individuals in municipalities that introduced the mandatory schooling reform and those that did not, and in municipalities in which other types of schools were available and in those in which they were not. These predictions are based on the regression model given in Eq. (2). We have matched each type of school to the appropriate education level, which originally consists of eight levels (see Table 1).

The results from the difference-in-difference model are mixed. We first present the results from the two-by-two table for reforms status and vocational schools. The top panel in Table 8 shows the proportion of persons with vocational education (groups 3 and 4 in Table 1) as their highest education by reform status and the availability of vocational schools in the municipalities in which they grew up. 45.7% of the pupils had vocational education in reform municipalities compared with 39.2% in municipalities that imposed only seven years of compulsory schooling, conditional on the fact that they did *not* have vocational schools in their municipality. Both these proportions are lower for persons in municipalities

[15] The results from the first-stage estimation from the simpler equation (1) is reported under equation (1) in Table 5, while the more complete first stage probit regression is presented under Eq. (2) in Table 5.

Table 9
Earnings equations, full time employed men, cohorts 1947–1958.

	Model 1		Model 2		Model 3	
	Coefficient	St. error	Coefficient	St. error	Coefficient	St. error
Experience	0.057	(0.002)	0.055	(0.002)	0.060	(0.002)
Experience sq/100	−0.084	(0.004)	−0.067	(0.007)	−0.095	(0.007)
Tenure	0.013	(0.001)	0.013	(0.001)	0.014	(0.001)
Tenure squared	−0.006	(0.001)	−0.006	(0.001)	−0.006	(0.001)
Father college	0.048	(0.003)	0.041	(0.004)	0.057	(0.004)
Mother college	0.034	(0.005)	0.028	(0.006)	0.044	(0.006)
Family income 2	0.007	(0.002)	0.008	(0.002)	0.005	(0.002)
Family income 3	0.022	(0.003)	0.018	(0.003)	0.026	(0.003)
Family income 4	0.073	(0.003)	0.068	(0.004)	0.082	(0.004)
Education	0.082	(0.001)	0.094	(0.002)	0.074	(0.005)
ξ_i			0.020	(0.007)	0.051	(0.007)
Education ξ_i					−0.015	(0.004)
Constant	11.11	(0.032)	11.01	(0.051)	11.46	(0.017)
R-squared	0.209		0.208		0.215	
Observations	203,387		203,387		203,387	
Cohorts dummies (Y_C)	Yes		Yes		Yes	
Municipal. fixed effects (δ_M)	Yes		Yes		Yes	

Note: Family income is divided into quartiles. All the estimated coefficients are significantly different from zero except for some of the municipality fixed effects (δ_M). The standard error have been adjusted for clustering at the municipality level over time and for the two-stage procedure used.

that had vocational schools. However, there are interesting results on changes induced by the compulsory schooling reform. The proportion of persons with vocational education increased more because of the reform in municipalities with no vocational schools (6.56 percentage points) than in municipalities that had vocational schools (5.19 percentage points). This implies a difference-in-difference of 1.37 percentage points.

The next panel in Table 8 shows the proportion of persons with upper secondary education (high school, see Table 1) as their highest level of education by reform status and by availability of upper secondary schools. The school reform increased the proportion of persons with a upper secondary diploma in municipalities with and without an upper secondary school. The increase in the proportion of persons with such an education following the reform was almost the same in municipalities that had an upper secondary school than in those that did not. The difference-in-difference of 0.01 percentage points is small and not statistically different from zero.

The third panel in Table 8 shows the proportions with college degrees (undergraduate degrees from a regional college, i.e., group 6 in Table 1) by reform status and by availability of regional colleges. The reform affected the proportion of persons with a college degree marginally, and the difference-in-difference estimates are relatively small (0.71 percentage points). However, the difference-in-difference for the college degrees is statistically significant.

The bottom panel in Table 8 shows the proportions with university degrees (from groups 7 and 8 in Table 1) by reform status and by the availability of universities. The results in Table 4 indicate that the reform had a negative effect on the take up of university degrees. The reform had a stronger negative effect on the highest level of education in municipalities that had a university than in municipalities without a university; the difference-in-difference estimates are negative by 1 percentage point.

The results from Table 8 show that the difference-in-difference estimates for Vocational and Upper Secondary schools are not significantly different from zero. We find a positive effect for Regional college and a negative effect for the availability of University.

Table 7 shows the isolated effect of the reform variables of the eight different educational levels defined in Table 1 for sub-groups before and after the reform. Here we find a significant positive effect for most categories except for the negative effect for the University II education level. Our instruments are thus not perfect in the sense of being significant. Finding good instruments is difficult, and we think that using the availability of schools prior to the implementation of the reform in a municipality at least brings us one step closer to valid estimation of the returns to education when these are categories and not years of education.

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 an OLS model, a two-stage selection model (based on the control function approach), and a random coefficients model. These results are presented in order to compare with previous results in the literature. We then present the results from our main model, in which education is

Table 10
Earnings equations, full time employed men, cohorts 1947–1958, Roy model.

	Level = 1	Level = 2	Level = 3	Levels = 4	Level = 5	Level = 6	Level = 7	Level = 8
Tenure	0.0123	0.0284	0.0107	0.0086	0.0172	0.0160	0.0084	0.0059
	(0.0022)	(0.0020)	(0.0014)	(0.0013)	(0.0023)	(0.0020)	(0.0028)	(0.0026)
Tenure squared	−0.0050	−0.0121	−0.0036	−0.0033	−0.0082	−0.0094	−0.0064	−0.0055
	(0.0009)	(0.0009)	(0.0007)	(0.0005)	(0.0011)	(0.0010)	(0.0014)	(0.0014)
Experience	0.0489	0.1128	0.0496	0.0673	0.0694	0.0819	0.1045	0.0310
	(0.0086)	(0.0085)	(0.0064)	(0.0061)	(0.0096)	(0.0081)	(0.0093)	(0.0079)
Experience sq/100	−0.0008	−0.0047	0.0006	−0.0001	−0.0011	−0.0014	−0.0022	0.0021
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0005)	(0.0004)	(0.0005)	(0.0004)
Father college	0.1951	0.2431	−0.0310	−0.0256	0.0456	0.0236	0.0001	−0.1290
	(0.0619)	(0.0267)	(0.0183)	(0.0153)	(0.0251)	(0.0194)	(0.0246)	(0.0170)
Mother college	0.2529	0.2634	0.0148	−0.0271	0.0251	0.0080	0.0037	−0.1155
	(0.1230)	(0.0448)	(0.0297)	(0.0203)	(0.0279)	(0.0201)	(0.0242)	(0.0156)
Family income 2	−0.0077	0.0137	−0.0133	−0.0205	−0.0115	0.0043	−0.0024	−0.0659
	(0.0073)	(0.0071)	(0.0051)	(0.0048)	(0.0092)	(0.0074)	(0.0094)	(0.0098)
Family income 3	0.0206	0.1057	−0.0030	0.0056	0.0146	0.0183	0.0134	−0.0928
	(0.0151)	(0.0116)	(0.0083)	(0.0076)	(0.0142)	(0.0114)	(0.0143)	(0.0124)
Family income 4	0.0753	0.2218	0.01137	0.0131	0.0794	0.0633	0.0896	−0.1124
	(0.0249)	(0.0182)	(0.0131)	(0.0119)	(0.0217)	(0.0171)	(0.0216)	(0.0167)
Time trend	0.0113	−0.0841	0.0570	0.0421	0.0129	0.0170	0.0141	0.0975
	(0.0132)	(0.0098)	(0.0068)	(0.0063)	(0.0116)	(0.0091)	(0.0115)	(0.0084)
ξ_i	−0.0754	−0.4462	0.1676	0.1546	0.0392	−0.0034	0.0003	0.5411
	(0.0529)	(0.0508)	(0.0348)	(0.0324)	(0.0582)	(0.0465)	(0.0587)	(0.0541)
Constant term	11.885	12.714	10.868	11.063	11.341	11.047	11.208	12.831
	(0.3419)	(0.2447)	(0.1626)	(0.1363)	(0.2581)	(0.1947)	(0.2443)	(0.2788)
R-squared	0.1134	0.0971	0.1295	0.0768	0.0822	0.067	0.0928	0.0890
Observations	14,869	21,423	33,770	44,618	21,893	27,863	19,241	19,710
Cohort dummies (Y_C)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality variabl. (P_M)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Predicted log wage	12.283	12.384	12.327	12.425	12.525	12.671	12.627	12.856

Note: Standard error in parenthesis. Family income is divided into quartiles. Municipality variables (P_M) are defined in Section 4.1.

treated as a discrete variable representing types of education. In this model, we allow for differences in returns for different education levels. The model also allows for heterogeneity that depends on observed and unobserved variables.

5.3. Constant returns to years of education

In Table 9, we report the results from the earnings equation in which we assume constant returns to education by specifying the education variable as a continuous variable. We use the level of education, but treat this variable as a continuous (eight level) variable. In column 1, we tabulate the estimated OLS returns to education for male workers for all sectors. Because the individuals in our sample were between 37 and 48 years of age in 1995, returns to education are higher than if individuals of a wider age range were used. The OLS estimates of the returns to education are 8.2%.[16]

The model used in column 2 is a standard selection model estimated by using a control function approach; see Eq. (9) in Section 4.3. For these results, the reform of compulsory schooling is used as the identifying exclusion restriction. The results indicate returns to education of 9.4%. This result is consistent with existing results obtained by using a measure of distance to higher education as an identifying exclusion restriction (see Card, 1999) and is consistent with other results for Norway (see Hægeland et al., 1999). The results indicate heterogeneity in the returns to education because the identifying exclusion restriction captures the returns to education for the group that complies with the treatment (compulsory school reform). In this case, it is reasonable to assume that those who comply, i.e., who were encouraged to undertake higher educational attainment when the new compulsory school system was introduced, have higher returns to schooling than those groups that were unaffected by the reform (such as the "always takers" and the "never takers"). The LATE interpretation of the estimated returns to education, which is the interpretation given to selection model estimates when discrete instrumental variables are used (Angrist et al., 1996), implies that our estimated returns to education are the returns gained by a person who acquires additional education solely because of the education reform and who would otherwise have left education after seven years. This result contrasts with that obtained from a similar specification by

[16] Because we are using levels, not the number of years, of education, this estimate is not directly comparable with the estimated returns to the number of years of education. The OLS estimate of the returns to education from the model incorporating the number of years of education (not reported) is around 5%, which is consistent with estimates from other studies for Norway; see Hægeland et al. (1999).

Table 11
Returns to education, Roy model.

Education	Descriptive statistics		Roy model, selection		
	Earnings	Diff	ATE	CCTE	TT
Comprehensive 7	12.31				
Comprehensive 9	12.40	8.8	10.1 (9.5)	12.3 (4.4)	8.2 (14.3)
Vocational I	12.37	5.6	4.4 (18.7)	6.4 (19.3)	6.4 (15.3)
Vocational II	12.44	13.3	14.2 (25.3)	14.2 (21.3)	14.7 (26.1)
Upper secondary	12.54	23.2	24.2 (8.4)	22.1 (21.1)	25.3 (15.8)
University I	12.66	34.7	38.8 (18.5)	38.8 (23.6)	39.4 (18.9)
University II	12.61	29.6	34.5 (29.1)	33.8 (7.6)	37.4 (15.3)
University III	12.84	52.7	57.3 (27.5)	56.9 (21.7)	61.2 (17.4)

Effects are measured in percent. *Note*: Earnings are in log. All the effects (ATE, CCTE, TT) are based on the earnings regressions in Table 10 and calculated based on $E(y|x, S = l) - E(y|x, S = 1)$ both for total sample (ATE) and for subgroups (TT). t- values in parentheses based on bootstrap using 100 replications.

Meghir and Palme (2005), who find no significant effect on returns to education by using participation in the reform of compulsory schooling as an instrument.

For the third specification in Table 9, we estimate a random coefficients model in which the results are interpreted as average effects given that endogeneity and heterogeneity have been purged from the wage equation. Estimation is based on Eq. (8) in Section 4.3. The return to education is 7.4% on average per unit of education, which is slightly lower than the OLS estimate and lower than the estimate from the selection model reported in column 2 of Table 10. Interpreted literally, this result suggests that the returns to education in the population are lower when heterogeneity and endogeneity are controlled for. In our case, this implies that the reform must have induced some positive selection into higher education. This is because the average return to education is lower when heterogeneity and endogeneity are controlled for. This result is consistent with the LATE result obtained from the selection model presented in column 2, which indicates positive selection into higher education. Note also that the interaction term between the selection parameter, ξ, and years of education is negative in this model, which confirms this finding.

5.4. Returns to education levels

In Table 10, we present regression results from an estimated model for returns to education by defining education in terms of qualification levels ranging from one year of vocational training to master's degree level; see Eq. (5) in Section 4.2. Using Eq. (5), we estimate an earnings equation for each level of education that includes the selection correction term from the ordered probit model estimated in the first stage and presented in Table 5 (defined under Eq. (2)).

If we considered the effect of family background first, we notice that earnings increase with work experience, but at a decreasing rate, at all education levels except levels 3 and 8. 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 higher for the two highest levels of education (university degrees) than for the lower levels of education, taking into account the second-order effect. The effect of family background characteristics on earnings is relatively small for higher levels of education. However, the earnings effect of family education is substantial for the two lowest levels of education. 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 for the lower levels of education. Only 9.5% of those in the sample had a father with a college education, and only 3% had a mother with a college education; see Table 3. The effect of higher family income on individual earnings is mainly positive, but not always significantly different from zero. For the highest level, it is actually significantly negative.

The effect of the selection correction term is significantly negative for the second lowest education level. The highest positive effect is found for master's programmes and PhDs, for which the effect is large and significant. This means that individuals with high earnings, given this level of education, also select into this level of education. In many cases, the selection correction term is not significantly different from zero.

We now turn to the estimated returns to different levels of education; see Table 11. The estimation of different returns parameters is based on Eq. (6) in Section 4.2. The coefficient estimates from Table 11 are used to form the β_l and ψ_l coefficients in Eq. (6).

We report the unconditional mean log earnings and differences in the returns to education in the first two columns. The unconditional results show that upper secondary school generates a 23% return in the three years following compulsory schooling. Two to three years of vocational training yields a much lower return of about 13%, and one year of vocational training yields a return of about 5.6%. Note that short university degrees generate a slightly lower returns compared with undergraduate regional college degrees. This nonlinearity in returns is a striking result but is not new; see Hægeland et al.

(1999).[17] The return to a master's degree is 52%, relative to that of compulsory schooling. These results compare favourably with the results of others; for instance, for Sweden, see Meghir and Palme (2005).

The next three columns provide the ATE results, the CCTE results, and the TT results allowing for selection based on observable and unobservable background characteristics in the context of the comparative advantage model. The results are quite similar to the unconditional results, but there are slight differences. First, the returns to upper secondary school are slightly higher when measured as an ATE than are the unconditional averages in the population. The model's predicted average effect implies a gain in earnings from having an upper secondary school diploma of 24.2 percentage points. 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. The CCTE results, obtained by averaging over the population up to and including the particular education level being analysed, are slightly smaller than the ATE results.

For the TT effect, the final column in Table 11 shows that these effects provide higher returns at all levels than do the ATE, CCTE, and unconditional effects. This means that selection into higher education is important. The TT effect results indicate that returns to education are high for higher education. The ATE results, which we can interpret as the result of an experiment in which everyone receives the same level of education, suggest that the return to education is lower than for those students at exactly that experimental education level. This result implies that selection based on comparative advantage is important given that the returns gained because of selection into different education levels are important. This finding supports earlier findings in the literature that test for only two levels of education (college and no college education); see Willis and Rosen (1979) and Garen (1984).

The results in the final column of Table 11 indicate that the effect of a bachelor's degree (level 7/University II) is considerably higher for those students taking a bachelor's degree than would be the case for persons selected at random to pursue a degree. The effect of having a bachelor's degree is to increase earnings by 37.4 percentage points, while the ATE and the unconditional effect is 34.5 and 29.6 percentage points, respectively. In addition, the effect of randomly selecting a person into either upper secondary school or a short (one- or two-year) college programme implied by the ATE is surprisingly large. Thus, increasing the general level of education, the aim of the comprehensive school reform of the 1960s and other school reforms, potentially generates a high return to wages up to the level of short college programmes. We also find that there is a difference between the ATE and the TT for master's degrees at universities. Thus, selection effects are important for university degrees.

6. Concluding remarks

In this paper, we used a rich data set and a flexible framework to estimate the effect of education reforms on educational attainment and the returns to education when education is defined in levels.

We estimated different parameters representing the returns to education by using linear and nonlinear frameworks that allow the effect of education to vary with observed and unobserved factors. The model allows agents to act on the effects 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 as instruments in demand type models and, hence, we used information on a mandatory education reform as well as pre-reform availability of schools in the municipality above the mandatory level to identify and interpret different parameters representing the returns to education.

Our results indicate that the compulsory education reform increased the general level of education significantly. We also found that the effect of family background on educational attainment, especially fathers' and mothers' education, was weaker after the mandatory schooling reform was introduced. We also found that better access to higher education strengthened the impact of the mandatory schooling reform.

We found that the returns to education are strongly nonlinear and that, because of these nonlinearities, it is better to use the Roy model than the traditional two-stage selection model, in which schooling is a continuous variable. In particular, we found evidence of high returns to wages for upper secondary school education, for one or two years of education at a regional college, and for university master's degrees. We also find that the average treatment effect is surprisingly high for education of intermediate duration (up to two years of college). This means that increasing the general level of education, which was the intention of the comprehensive school reform in the 1960s and other school reforms, potentially yields high returns to wages. However, we did not consider the social cost of increasing education. We also find that for upper secondary education and above, the effect of TT is 1–4 percentage points higher than both the ATE and the CCT. This implies that selection into education on the basis of unobservables is important at each education level.

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[17] Note that one-to-two-year, and particularly three-to-four-year, college/university degrees encompass various types of education, such as one year of university without a degree, a two-year college degree, and a four-year degree from either a university (cand.mag. in the Norwegian system), a technical university, or a business school, to which entry is difficult based on high school grades. In this context, Hægeland et al. (1999) finds that the differences in returns are substantial.

Appendix A

Table A1 indicates that there is no systematic pattern in the rate of introduction of the reforms in relation to parental average earnings, education levels, age, or the urban/rural location variable. When controlling for localization (by using dummy variables for counties), we find that none of the other variables are statistically significant. In all our placebo experiments reported in

Table A1
Timing of the implementation of the reform, with year of reform as dependent variable.

	Coefficient	Standard error
County2	−1.84	0.66
County3	−1.90	4.36
County4	0.16	0.70
County5	0.05	0.74
County6	−0.99	0.92
County7	−1.65	0.74
County8	−2.31	0.68
County9	−0.72	0.59
County10	−3.55	0.93
County11	−1.41	0.65
County12	−0.21	0.64
County13	−6.12	1.26
County14	−0.18	0.79
County15	1.90	0.53
County16	−1.94	0.62
County17	0.76	0.57
County18	−0.61	0.54
County19	0.44	0.63
Share of Fathers with Some College	5.05	5.59
Share of Mothers with Some College	21.98	11.32
Father's Income (mean)	−0.004	0.005
Mother's Income (mean)	−0.037	0.014
Father's Age (mean)	−0.06	0.20
Mother's Age (mean)	−0.19	0.25
Share of Municipality with Fewer than 9 Years of Education	0.18	1.23
Size of Municipality/100	0.19	0.30
Unemployment Rate 1960	−16.30	15.48
Share Workers in Manufacturing 1960	0.98	4.47
Share Workers in Private Services 1960	5.74	8.18
Share Labour Vote 1961	1.57	2.80
Constant term	1980.51	9.69

Note: Robust standard errors. All variables are municipality level variables.

Table A2
Placebo experiments for reform variable, birth cohorts 1947–1958.

	Model 1		Model 2		Model 3		Model 4	
	Coeff	St. error	Coeff	St. error	Coeff	St. error	Coeff	St. error
Father college	0.371	0.018	0.375	0.017	0.363	0.026	0.416	0.017
Mother college	0.334	0.023	0.356	0.096	0.411	0.031	0.358	0.096
Family income 2	0.106	0.015	0.099	0.014	0.092	0.022	0.067	0.014
Family income 3	0.246	0.016	0.242	0.014	0.020	0.022	0.198	0.014
Family income 4	0.379	0.017	0.384	0.015	0.372	0.024	0.346	0.015
Reform dummy	−0.005	0.011	−0.008	0.011	−0.017	0.013	−0.017	0.006
Pseudo R^2	0.137		0.135		0.134		0.121	
LR ξ^2	84,310		11,526		10,483		31,640	
Observations	58,103		74,724		32,360		102,110	
Experience and tenure (X_i)	Yes		Yes		Yes		Yes	
Cohorts dummies (Y_C)	Yes		Yes		Yes		Yes	
Municipality fixed effects (δ_M)	Yes		Yes		Yes		Yes	

Model 1: Cohorts 2 and 3 years prior to the reform has reform dummy = 1, and cohorts 4, 5 and 6 years prior to the reform has reform dummy = 0.
Model 2: Cohorts 1, 2 and 3 years prior to the reform has reform dummy = 1, and cohorts 4, 5 and 6 years prior to the reform has reform dummy = 0.
Model 3: Cohorts 1 year prior to the reform has reform dummy = 1, and cohorts 2 years prior to the reform has reform dummy = 0.
Model 4: Random reform dummy for cohorts prior to the reform.

Table A2 the reform variable is not significantly different from zero. This means that we reject the hypothesis that the reform had an effect for cohorts not affected by the reform.

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