Measuring the Effect of a School Reform on Educational Attainment and Earnings *

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ABSTRACT:
During the period 1960-1971 a major reform in the comprehensive school took place in Norway. We exploit the stage implementation of the reform as an instrument in estimating the returns to schooling. The reform allows us to study identical age cohorts of individuals going through two different school systems; one with seven years (old system), and one with nine years (new system) of compulsory schooling. We analyze whether the reform lead to increased educational attainment, particularly for the disadvantaged groups that were the main target for the reform. Furthermore, we test whether the increased attainment caused by the reform lead to higher returns to education; in general and for the targeted groups.

We find that the reform increases the probability of post-compulsory education for the lower educational groups, but not for the higher groups. Also the return to education varies according to educational groups. We find a substantial degree of heterogeneity in the unobserved return to education. One major finding is also that pupils from low income family background were picked up by the reform leading to positive selection into some types of education which increased the returns to education in general. In addition, we demonstrate that the results are highly dependent on our assumptions regarding heterogeneity and selection at the different stages of the model.

1 Introduction

Most industrial countries have experienced an increasing demand for high skilled workers due to a relative employment shift in favor of skilled workers during the last decades (OEDC 1998). Educational reforms have been at the center of the debate to alleviate this apparent scarcity of skills. The purpose of these reforms is to enhance educational attainment, and thereby increase the skill of the population. Informed public policy decisions about educational reforms and investment in human capital require rigorous analysis of the returns to education both in general and especially the returns to different groups targeted for increased educational attainment.

The main problem in measuring returns to education is the fact that the decision to take more education is a complex process. Factors like individual ability, financial constraint and preferences are usually unobserved for the researcher. This creates an endogeneity problem inherent in most evaluation and labor market studies (Heckman 1974, 1976, Gronau 1974). An additional problem relates to the heterogeneity in the return parameters of education and the interpretation of different return parameters. The return to education has both a common and a random part varying with different characteristics of subgroups of the population (Willis and Rosen 1979, Lang 1991, Card 1995, 1999, Heckman and Vytlacil 1999). This heterogeneity arises if individuals select into education based on their comparative advantages of education (Roy 1951, Becker 1979). A natural but mainly unexploited resource of information to overcome these problems are the educational reforms in the European countries in the postwar period.

The focus in the present paper is to exploit some interesting features of one of the school reforms in Norway - the school reform extending the mandatory years of schooling from 7 to 9 years. The reform took 10 years to implement and we observe same birth cohorts going through both compulsory school systems. We will assess both the effect of the reform on higher participation rates into higher education in general, and whether the impact of socio-economic and geographical factors were reduced after the reform since disadvantaged groups were especially targeted in the reform. Further, we test whether higher education levels caused by the reform lead to higher returns to education, again both in general for the population and for the groups especially targeted.[1]

For these purposes both the Norwegian reform and the very detailed data set available are ideal for analyzing the effects of a school reform which distinguishes it from most of the literature on the returns to education. First, the reform shares some common features of school reforms that took place at about the same time.[2]

[1] This reform was predated by a reform in the 1950s of a primary school system, and in the 1970s and 1980s of reforms of the secondary school system as well as the college and university system both by unifying the education system also at these levels as well as establishing regional university colleges.

[2] These school reforms have parallels in most other European countries in the same time

These reforms are characterized by extending the number of compulsory years of schooling and unifying the education system at higher and higher education levels. The aim of the reform was stated explicitly in several governmental background papers, and were 1) to increase the minimum level of education in society by extending the number of compulsory education from 7 to 9 years, 2) to smooth the transition to higher education in general by unifying the education system up to secondary education, and 3) to enhance equality of opportunities both along the socio-economic dimension and in particular the geographical dimension both by providing resources to establish the new comprehensive schools in all municipalities and by securing a common curriculum for all schools. Important to note is that the potential impacts are expected to be stronger and thus easier to measure in the case of Norway than in most other countries, since it has been pointed out that the Norwegian reform along with the Swedish reform went further both in the unification of the comprehensive school system as well as in promoting equality of opportunities (Leschinsky and Mayer 1990). The rich data set available both on the reform and family background variables such as parents income and education will be helpful in analyzing the effect of the reform on educational attainment and the effect of education on earning.

Secondly and most importantly, the introduction of school reform took the form of a social experiment in that the compulsory 9 years of schooling was introduced over a ten year period from 1960 to 1971, and thus the new and the old education system coexisted for more than ten years. This makes it possible for us not only to compare labor market outcomes such as earnings before and after the reform, but also to construct counterfactuals by comparing individuals of the same cohort participating in the reform with persons not participating, given the same macroeconomic environment, the same work experience and tenure, and not the least the family background variables.

The fact that we have a social experiment where we can think of education now being randomized will help controlling for ability bias in the case that returns to education are homogeneous across individuals. However, a lot of the findings when instrumenting suggest heterogeneity in returns to schooling. For a review of the literature see (Card 1999). In the case of heterogeneous returns to schooling, IV or selection techniques in general do not provide a measure of average returns to education in the population, and not without strict assumptions estimates of the returns to education of treatment of the treated (Heckman, 1997). As suggested by Angrist, Imbens and Rubin (1996), the interpretation of the estimated returns using an IV type of estimator is the average treatment effect or returns to increased education for those who comply with the instrument (the LATE effect (Local Average Treatment Effect)); in this case those individuals who were triggered to take higher education caused by the education reform,

period, notably here are Sweden, the UK, and to some extent France and Germany. (Palme and Meghir 1999) present results using Swedish data, and Blundell et al. 1997 using UK data.

a group Angrist, Imbens and Rubin (1996) call the "compliers". In our case this is a very convenient framework since we are especially interested in estimating the effect for particular groups targeted by the educational reform. We assess the use of the education reform as an instrument and thus the interpretation of the returns to schooling as a LATE, which in our case is the effect of education for individuals from low family income backgrounds that were triggered by the reform to take more schooling.

The next step in the estimation procedure is to estimate the average returns to education in the population and not for the returns for particular targeted groups and thus to attempt at overcoming the bias from heterogeneity in returns. We utilize a flexible framework for studying different return parameters of education, both in a linear and non-linear fashion, where we allow the effect of education to vary both in terms of observed and unobserved factors. This model is termed a random coefficient model, see Heckman and Robb (1985). The approach also follows from Willis and Rosen (1979), Garen (1987) and Bjørklund and Moffitt (1987), and in our context especially Meghir and Palme (2000).[3] The model allows agents to act on the effect of schooling, thus we assume forward looking agents who can predict their effect of different schooling levels to differ from the average effect of earning on wage outcomes.[4]

The remaining part of the paper is organized as follows: Section 2 provide more background information of the reform of compulsory education in Norway. Section 3 presents our econometric framework, while Section 4 discusses the data sets and defines variables. In Section 5, we present the results from educational choice. Section 6 discusses the earnings effects of the increase in relative supply of more educated workers, and presents our estimates for the change in educational earnings premiums. The final section concludes.

2 Compulsory Education in Norway

During the decades from 1960 to 1990 the Norwegian education system went through several large reforms. The starting point was the reform of the compulsory primary and junior secondary school, which mainly took place in the sixties. The reform process then continued to the (voluntary) upper secondary school in the mid seventies. Approximately at 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 nineties.

The focus of the present paper is (the impacts of) the compulsory school re-

[3] A semi-parametric version of the random coefficient model applied to the evaluatation of training programs on discrete outcomes is found in Aakvik, Heckman and Vytlacil (2000).

[4] Earning profiles are published constantly in different media for different educational groups. These earning profiles are usually also conditioned on public and private sector, and by age groups.

form. The reform was supposed to serve several purposes. While the pre-reform compulsory school lasted for seven years, this was now prolonged to nine years. Hence, the reform extended the length of the least educated. Furthermore, the fact that the post-reform children now spent two more years in the compulsory school system, was intended to stimulate the demand for higher education. Finally, the reform was a part of a massive centralization reform taking place in this period. Small, heterogeneous schools in rural areas were closed, and substituted by larger, standardized units. The arguments for the centralization process was partly cost reductions through economy of scale, but also that educational standardization and homogenization would endorse more equal opportunities across social classes, region and gender.

In the Norwegian pre-reform system the children started school at the age of seven and finished the compulsory education after seven years, i.e. at the age of fourteen. In the new system, the starting age is still seven years, but the time spent in compulsory education is now nine years. The nine years are divided into two levels; first six years of primary school, followed by three years of secondary school.

The reform was implemented first in 1960, but lasted until 1971.[5] Hence, for more than a decade the Norwegian compulsory school was divided into two separate systems. The first cohort that was involved in the reform was the one born in 1947. 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 secondary school from 1960 to 1962. The last cohort who went through the old system was born in 1958. This cohort started school in 1965 and finished compulsory school in 1971.

The dual compulsory school system constitutes a natural experiment, since we, for the ten birth cohorts from 1947 to 1958[6], can identify children entering two different school systems. Either they went to the new, expanded compulsory school, or they entered the pre-reform system group. The assignment to the reform was not affected by individual or parental choices, but determined exogenously according to the municipality-wise reform implementation.[7]

For details on the reform implementation, construction of indicator variables, etc., see section 4.2.

[5]The reform started on a small and explorative basis already in the late fifties, but with an ignorable number of students, schools and municipalities involved.

[6]The number of individuals with background from new system in the 1947 cohort and with old system in the 1958 is, however, very small. Hence, due to lack of variation in the reform dummy we drop these cohorts in the analysis to come. In the analysis we use the birth cohorts from 1948 to 1957.

[7]Systematic action on the parental behalf (migration to municipalities with the preferred education system, etc.) cannot be totally ruled out. We have however reason to belive that this is a minor problem, and will ignore it in the present study. Thes costs' following the reform were reimbursed by the central Government. I addition, the Government had no explicit strategy for implementing the reform.

3 Model

Estimation of earnings equations and the effect of education on earnings is made difficult by the fact that selection into different educational groups is not random but a function of the marginal costs and benefits of education. Individuals will invest in schooling up to the point where the marginal change in discounted present value of future earnings of an extra year of schooling is equal to the intertemporal rate of substitution, (Becker 1975, Card 1995). The canonical model of schooling raises at least two type of biases that may complicate the interpretation of the results. One bias is related to the correlation between unobserved individual factors (for instance ability) and schooling choices. The econometric problem due to the endogeneity of the schooling variable is discussed extensively in the literature, see for instance Griliches (1977), Willis and Rosen (1979), Heckman and Robb (1985).

A second source of bias arises if individuals act on the returns to schooling when schooling choices are made. In this case we have an extra correlation between unobserved individual factors and the returns to taking more education, (Willis and Rosen 1979, Garen 1984, Bjørklund and Moffitt 1987, Card 1995, Heckman and Vytlacil 1998). This model is called a random coefficient model. If returns to schooling is known, individuals will sort into different schooling levels by comparative advantage, (Roy 1959).

Our approach is based on a random coefficient econometric model where we allow for the fact that the returns to education can vary from person to person, i.e. the individual return to education vary both with respect to observed and unobserved individual characteristics. The approach consists of estimating the educational choice using an ordered probit model where we interact our instrument (an indicator for pre-reform or post-reform assignment) with all observed background variables. The model also has a different error term (and cut-off levels) for the two school regimes.[8] From the ordered probit equations we construct selection correction terms for each educational level for individuals in the old and new regime that is included in the earnings equations. The selection correction term will capture unobserved factors. We specify earnings equations where we interact the selection correction term with educational choice to allow for unobserved heterogeneity in the returns to schooling that is generated by unobserved factors.

Observed heterogeneity in returns to schooling can be modeled in two ways. One by interacting observed variables with the schooling choices, and the other by running a full heterogeneity model based on a switching regression framework, where earnings regressions are estimated for each educational level, see for instance Heckman, Tobias, and Vytlacil (2000).

[8] We discuss the results from the schooling regression since our rich data set provide interesting and new results to the literature on the estimation of educational attainment.

3.1 Educational Attainment

We allow for heteroge in the individual returns to schooling and in the individual costs of schooling. The optimal level of schooling in the heterogeneity model is

$$S_i^* = \frac{b_i - r_i}{k} \tag{1}$$

where b_i is individual ability that generates heterogeneity in the marginal returns to schooling, r_i is individual liquidity constraint that generates heterogeneity in the marginal cost to schooling, and k is a non-negatve constant, see Card (1995). The school reform may affect both b_i and r_i. We use an ordered probit model to estimate the effect of controll variables and reform assignment on completed school outcome. The ordered probit model is build around a latent regression equation. To allow for a flexible specification of the effect of the school reform on educational attainment we sepcify separate schooling regressions in the old school system ($Z_i = 0$) and in the new ($Z_i = 1$)

$$S_i^{1*} = Q_i\beta_S^1 + V_i^1 \qquad \text{if} \qquad Z_i = 1 \tag{2}$$
$$S_i^{0*} = Q_i\beta_S^0 + V_i^0 \qquad \text{if} \qquad Z_i = 0 \tag{3}$$

where S_i^{j*} represents the optimal level of schooling ($j = 0, 1$), Q_i is a vector of individual and family background variables, β_S^j is the effect of observed background variables on educational attainment, and V_i^j is the error term distributed normally with $E(V) = 0$ and $\text{Var}(V) = 1$. We do not observe the latent variable S_i^{j*}. However, the observed optimal education choice can be modelled in the following way

$$S_i^j = l \text{ if } c_{l-1}^j < S_i^{j*} < c_l^j \tag{4}$$

where $l = 1, 2, \ldots, L$ and c_l^j are cut-off levels in the ordered probit model. We have divided qualification levels into eight discrete categories, i.e. $L = 8$, in both the new and old regime.[9] For more details on these categorical levels, see Table 5.1 and 5.2. 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 to be in the different qualification levels both in the old and new system. Thus we predict $\Pr(S_i^j = l|Q_i)$ for all persons in the sample, and different version of this equation. In the ordered probit model $\Pr(S_i j = l|Q_i)$ is calculated as

$$\Pr(S_i^j = l|Q_i) = \Phi(c_l^j - Q_i\beta_S^j) - \Phi(c_{l-1}^j - Q_i\beta_S^j). \tag{5}$$

[9] We also specify a model with 14 different educational groups based on the number of years of schooling. This variable varies from 7 years of schooling to 20 years of schooling. A linear regression model of the number of years of education will give the same results as an ordered probit model if the cut-off levels have the same distance apart. Here, we use an ordered probit model even under case were S is treated as continuous in the earnings equations.

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 in the following way

$$\xi_{li}^{j} = \frac{\phi(c_{l-1}^{j} - Q_i\beta_S^{j}) - \phi(c_{l}^{j} - Q_i\beta_S^{j})}{\Phi(c_{l-1}^{j} - Q_i\beta_S^{j}) - \Phi(c_{l}^{j} - Q_i\beta_S^{j})} \tag{6}$$

where ϕ is the probability density function of the normal distribution. The calculated generalized residual, ξ_{li}, that is used as a controll variable in the earnings equations is based on $\xi_{li} = Z_i\xi_{li}^{1} + (1 - Z_i)\xi_{li}^{0}$.

As explanatory variables, Q, in the ordered probit model where we predict educational outcome, we use age-cohort dummies for individuals potentially affected by the school reform (1948-1957), dummy variables for geographical location (19 counties), and family income in 1970 (10 groups).[10] More on these variables in section 4.

3.2 Earnings Specification

The estimation of the effect of education on earnings uses the school law reform as an instrument. We consider a model of log annual earnings ($\log y$) in 1995. For the moment we 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 versus part time work or no work. We consider first a model of log earning of the following form

$$\log y_i = \alpha_i + X_i\beta + \sum_{l=2}^{L} b_l S_{li} \tag{7}$$

where α_i is a person-specific constant, b_l the effect of schooling, and X is a vector that includes the following variables: tenure, tenure squared, actual work

[10] We have also background variables on father's and mother's education and occupation. Very few mothers were attached to the labor market in this period (1960s) and the general level of education among women was low at that time, creating a problem of "empty cells". Father's education and occupation are highly correlated with income. We get very similar results whether we use family income or father's education in our regressions. However, including both family income and father's income renders our results of the returns to schooling. Further work has to be conducted into the possible problem of multicolliniarity in family background variables.

We also have information on the age of the parents, both mother's and father's age. Although they turn out to be significant in the educational choice regression their economic meaning is unclear and we choose not to include them in our regressions.

experience, actual work experience squared, cohort dummies, and family income divided into 10 percentiles.[11]

The model has a relatively rich set of background variables and the data are of exceptionally high quality, although we do not have any measure of ability in our model. X and Q are not the same vectors although some of the elements are the same.

Equation (7) does not take into account the possibility of heterogeneity in the returns to education. However, the model allows for 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_l into two parts, $b_l = \bar{b}_l + b_{li}$, where $\bar{b}_l$ is the common (average) return to education for educational level l and b_{li} is the random return parameter for educational level l varying from person to person. We split α_i into two parts, where $\alpha_i = a_0 + a_i$. We rewrite equation (7)

$$\log y_i = a_0 + X_i\beta + \sum_{l=2}^{L}(\bar{b}_l + b_{li})S_{li} + a_i \tag{8}$$

where b_{li} represents the heterogeneity in the returns to education level l, and a_i is a heterogeneity that affects the level of earnings. Both b_{li} and a_i are unobserved in the data.

There are thus two potential sources of bias in equation (8). The first is the standard ability bias problem which is due to the correlation between a_i and S_{li}. The second source of bias is due to the heterogeneity in the returns to education, that is the correlation between b_{li} and S_{li}. If b_{li} is known to the decision makers they will select into educational levels based on comparative advantage. We solve this problem by specifying a regression where the effect of unobservables are allowed to be different for each educational level.

We use a control function approach in modelleling the effect of unobserved factors. The model is closely related to IV estimation of a random coefficient model, see for instance Card (1999). We assume

$$E[a_i|S_{li}, Q_i, Z_i] = \lambda\xi_i$$

and

$$E[b_{li}|S_{li}, Q_i, Z_i] = \psi_l\xi_{li}$$

where ξ_{li} is defined from equation (6). To have a tractable model we assume that λ_l is equal for all educational levels. We thus have the following earnings

[11] Work experience is number of years with labour earnings of more than 1 G since 1967. G is linked to the social security system and is adjusted from year to year. In 1995, 1 G was NOK 45000, or around USD 5000.

equation

$$E[\log y_i] = a_0 + X_i\beta + \sum_{l=2}^{L} \bar{b}_l S_{li} + \sum_{l=2}^{L} \psi_l \hat{\xi}_{li} S_{li} + \lambda \hat{\xi}_i \tag{9}$$

Applying OLS on equation (9) will give us a consistent estimate of the effect on earnings of each educational level.[12] In all cases we use the change in the number of years of compulsory education as our instrument in addition to this variable interacted with all background variables in the ordered probit model.[13] In the context of this model the equivalent of standard instrumental variables is obtained by assuming a homogeneous effect of education, i.e. the effect of different educational levels is the same ($\psi_l = \psi \ \forall \ l$).

A more flexible framework can be specified by using a switching regression in the earnings regression based on educational outcomes. This is a demanding framework in the sense that it requires a lot of observations to avoid the problem of missing cells. This model specifies

$$E[\log y_{li}] = a_0 + X_i\beta_l + \psi_l \hat{\xi}_{li} \tag{10}$$

for each educational level l.We can then estimate the return to education by comparing the different estimated model parameters. For instance, the average return to schooling when comparing schooling level l to $l-1$ for a given x is simply calculated from

$$\Delta^{ATE(x)} = x_i(\beta_l - \beta_{l-1}) \tag{11}$$

see for instance Heckman, Tobias, and Vytlacil (2000). Unconditioned estimates of equation (9), i.e. Δ^{ATE} can be found by integrating $\Delta^{ATE(x)}$ over the distribution of X. However, the $\Delta^{ATE(x)}$ is not a very relevant return parameter, see

[12] We will also estimate the effect of schooling when schooling is considered to be a continuous variable. There are several reasons for doing this. First, most studies of the return to education defines education to be the number of years spent in school. Thus, to compare our results with previous studies we define S to be continous. Second, the interpretation on the return to schooling is made easier if S is continous since the number of estimated parameters are reduced. However, there are also strong arguments for defining S to be discrete in particular in relation to our instrument and the interpretation of the return parameters. First, using the school reform as an instrument makes the interpretation of the returns to schooling for the highest level of education difficult since there is no significant effect of the reform on those levels, thus violating the assumption for IV estimation. Second, evidence on the returns to education shows non-linearities, see Layne-Farrar et. al. (1996) and Bound and Jaeger (1996). A model where S is treated as continous implies $\bar{b}_l = \bar{b}$ and $b_{li} = b_i$ which is clearly more restrictive than the specification in equation (6).

[13] Due to the considerable time lag between the decision to enter more than compulsory schooling (1960s) and the time we measure earnings (1995), it is not natural to include the county dummies in the education equation in the earnings equation. We thus have an additional identifying exclusion restriction that can be used to test our initial instrument.

Heckman (1997). The effect of treatment on the treated $\Delta^{TT(x)}$ is a more relevant return parameters. The $\Delta^{TT(x)}$ is given by

$$\Delta^{TT(x)} = x_i(\beta_l - \beta_{l-1}) + (\psi_l - \psi_{l-1})\hat{\xi_{li}} \tag{12}$$

based on the estimation of β and ψ from equation (10) for each educational level, and ξ from the ordered probit model.

The switching framework is challenging since we need to run earnings regressions for each educational level. The estimation of treatment effects is also sensitive to imprecise parameter estimates in the earnings equation. A less demanding framework to incorporate observed heterogeneity in the returns to education is to use interaction terms between educational outcomes and other (or all) observed background variables. The advantage of using a switching regression framework is the flexibility of allowing the returns to education to vary both in terms of observed and unobserved individual factors.

4 Data Set and Variables

The main data sources for our study are data from administrative registers from Statistics Norway. Each individuals are characterized by their personal identity code and information from different administrative registered are merged for each person in the population. The data set utilized here covers person working in all sectors - private manufacturing, private services and the public sector - in 1995 for the birth cohorts 1948 to 1957 which are the cohorts we are using in our analysis. For the wage regressions we use information on experience, seniority, years of education and type of education, annual income and employment relationship. Since the data set also contains a plant identifier, the data set is thus a merged employer-employee data set and the seniority and employment relationship can be calculated.

In addition to information for each person on education etc., we use family background information for the period the person grew up and started compulsory education. This includes parents or guardian persons income, education, occupation and county where the parents lived when (s)he started compulsory schooling. This information is collected and merged in from survey data from the National Censuses of Population and Housing in 1960, 1970 (for documentation, see Vassenden 1987).

The annual *earnings* in 1995 was derived using the annual taxable income as reported in the tax register. For the two different sets of models we used both years of education and types of education. *Years of Education* level is based on the normal duration of the education and includes only completed (and highest attained) education, and all formal education courses exceeding 300 hours are registered. In addition we used a 8 level groupings of types of education in addition which is provided in Table 5.1. *Tenure* is defined as the number of years

worked for each employer. Seniority at plant level was calculated based on the individual wages. *Experience* is based on the number of year earnings is higher than 1 G, where G is related to social security pensions. In 1995, G equals NOK 40,000 (around USD 5000). This is the closest we get actual number of years in the labor market, see Bratberg and Vaage (2000).

As an labor market measure we use an hourly wage rate, and excluded an hourly wage rate below 30 kroner per hour and above 500 kroner per hour, since these are obviously either below or above possible wage rates. Further we use only full time workers defined as working more than 30 hours a week, and since we have information on start and stop dates within a year, we used this to calculate full year equivalent full time earnings. Workers holding multiple jobs were excluded as well as self-employed workers. Workers participating in labor market programs or receiving unemployment benefit were also excluded.

For family income we use the sum of father's and mother's income in 1970 collected from the census data for that year. We divide family income into groups based on percentiles. Father's education is divided into 6 categories based on the standard classification system in Norway. Father's occupation is divided into 11 categories. Our categories also follows from the standard classification system of occupation used by Statistics Norway.

Appendix A contains a comprehensive discussion of the data set and summary statistics for the variables used in our analysis. (to be added).

4.1 Data on the Education Reform

There is no direct information in our data registers regarding the individual reform assignment. Hence, an indicator has to be constructed, and we proceed as follows:

While we have information on the municipalities in which the individuals work and reside, there is, however, no information regarding in which municipality they went to school. From the 1960 and 1970 Census Data we therefore identify the residing municipality of the parents (mother, in case of lone breadwinners), and assign schooling municipalities accordingly.[14]

From register data we have, for each municipality, access to information on the year of implementation of the education reform.[15] Unfortunately, it turns out that this is not sufficient for our purpose. Firstly, a fraction of the municipalities (some 15%) use more than one year to switch from the old to the new system. Secondly, commuting between school and residence municipality is not reflected in these data. Thirdly, several of the municipalities offered special arrangement for the transition of the "pioneer cohorts". In most cases this introduced a

[14]The 48-52 cohorts attended the seventh grade from 1960-1965; 1966-1971 for the 53-57 cohorts. Hence, we use the 1960 Census Data information for the first group, and the 1970 data for the latter.

[15]These data reflect the municipality structure of 1980, which is the newest available.

lag in the implementation process. Finally, the centralization process that took place during the decades under study also included a major reformation of the municipality structure where, basically, small units were merged into larger ones. (This is the main explanation to why some of the municipalities are registered with several years of implementation.) In addition, the fact that the majority of the municipalities in which the reform was implemented is non-existing today (merged into larger units), introduces extra uncertainty into our reform indicator variable.

There is, however, another source of information regarding reform assignment. Statistics Norway registers the highest level of education for each individual. For those who left school after finishing the compulsory education, which is about 16% of the sample, we have information on whether they went through the old or the new system. For each cohort in each municipality we count the number of individuals who passed through the old and the new system, respectively. We define the year of reform implementation for a given municipality as the year when the number of individuals sorting to the new compulsory school system outnumber the ones from the old system. Hence, we utilize the information from the sub-sample of the least educated to impute an alternative reform indicator for all inhabitants in a given municipality. This information is in turn compared to the official year of implementation as reported in the municipality register.

We prefer the individual-based, because of its robustness against noise from merging of municipalities, lags in the implementation process, etc. But first of all we use both of the two sources to delete cohorts in municipalities where the indicators reveal conflicting information. Typically, this is the cohort from the year when the reform was introduced, and sometimes the cohorts in the immediate neighborhood. Whether this action will have any systematic impact on the estimated parameters depends on randomness of the exclusion process. So far we work under the (untested) assumption that the procedure basically removes noise stemming from uncertain information. Note that all municipalities still will be represented in the analysis. The ones where the implementation process took several years and/or where the discrepancy between the two indicators is particularly high will loose weight, however.

Table 4.1 about here.

Finally, we summarize the data selection process in the following table:

Table 4.2.about here.

5 The Effect of the Reform on Education Levels

Based on characteristics of the Norwegian education system and Statistics Norway's standard classification of education, we group our sample into the following 8 levels.

Table 5.1 about here.

Table 5.2 reports the unconditional share of persons in different qualification groups by reform status, and the difference in educational attainment between the two sub-samples.[16]

Table 5.2 about here.

Needless to say, the reform practically brings the level 1 education to an end. For the remaining levels, the educational attainment is higher for pre-reform compared to post-reform individuals, particularly for lower educational levels. For example, the share of persons with upper secondary school is 4.3 percent for the pre-reform sample, and 5.6 percent for the post-reform, which is an increase of approximately 32 percent. Note, however, that the cohorts in question were all exposed to a major expansion of the general education system. The figures in Table 5.2 are unconditional of this trend.

The government had explicit goals and targets for the reform implementation, and we intend to shed some light on whether they have been fulfilled. For that reason our intention is to isolate a pure reform effect, controlled for other sources of (observable) heterogeneity in the sample:[17]

Because the reform was implemented sequentially from 1960 to 1971, the fraction of individuals with post-reform compulsory education will be higher the younger the cohorts. Furthermore, the sixties was a period with major changes in the municipality structure, including changes in the localization of the schools. Consequently, cohorts and regions should be controlled for. Two other sources frequently debated in the literature (see e.g. Card, 1999), are differences in ability and differences in liquidity constraints. Unfortunately, direct information on ability (scores from exams, IQ-tests, etc.) is very limited in Norway, and is not included in our data set. On the other hand, our data is rich on other sorts of background information, like income, education, occupation, etc. In addition, they include parental background information, notably family income.

The explanatory variables included in the schooling equation are cohort dummies (10 cohorts), dummy variables for geographical location (19 counties), and family income in 1970 (quantiles - 10 groups). The educational choices are estimated separately for the pre- and post-reform groups, allowing different impacts from observables as well as unobservable components.

Instead of discussing parameter estimates from the ordered probit model, for which the interpretation is rather awkward, we adopt a graphical exposition. Figure 5.1 (1-8) illustrate the predicted (post-reform) change in the probability

[16] The descriptive statistics and results from estimation in the rest of the paper refer to males only.

[17] Some of the covariates might themselves serve as instruments, but for now they act primarily as control variables in the schooling equation.

of obtaining a given level of education (vertically), plotted against the predicted pre-reform probability at the respective level (horizontally). Contrary to the unconditional shares reported in Table 5.2 the probabilities are now conditional on the covariates listed above, denoted Q in the figure. The remaining symbols are S for highest achieved schooling levels (S=1,2, ... ,8). Z is the reform indicator (Z=0 of old and Z=1 for new regime). The solid lines in the centre of the scatter plots represent the respective mean probabilities.

Figure 5.1 about here.

Loosely speaking, "clouds" scattered around the zero-line in the figures indicate that the model predicts no reform effect. Conversely, the more the scatters are separated from the zero line (positively or negatively) the stronger the effect from the reform. Not unexpectedly, the upper left most window (Figure 5.1.1) very distinctly illustrates a negative probability change. Since the lowest level practically was phased out by the reform, the post-reform probability approaches zero, and the scatter approximately takes the form of a 45 degrees line. More interestingly, the Figures 5.1.2-5 indicate positive probability changes.[18] The effect is strongest for level 2. Once again, this is highly expected, since an essential aim of the reform was to extend the minimum level of education by extending the number of years of compulsory schooling. Note, however, that the positive effect prevails through levels 3, 4 and 5, as illustrated in Figures 5.1.3-5, indicating that the effect was beyond the change in compulsory level of schooling. Hence, whether we calculate the unconditional distribution of education levels (Table 5.2) or predict the conditional attainment probabilities, our data indicate that there is a reform effect up to and including the upper secondary school (level 5).[19] For the highest educational levels, illustrated in Figures 5.1.6-8, the effect is zero, or even negative but not significant (level 8). Hence, the reform appears to have its strongest effect on the lower end of the education scale, which is exactly where it was targeted.

The predicted probabilities referred to in Figure 5.1 reflect the compound effect from all covariates in the schooling education. It is, however, of interest to evaluate the effects of the individual covariates and, in particular, to see whether their impacts are different in the pre versus the post reform case. To study the effect of family income on educational choice, we calculate the change in probability for the respective educational attainments as response to a change from the 10th to the 20th percentile etc. in the income distribution. Basically, we

[18] In all these cases we see a more or less postive trend in means. The steeper the lines, the closer the relationship between the probabilities in the pre and the post reform cases. Contrary, a horizontal line indicates that the probability of a given attainment is decreasing as the pre reform probablity increases. The primary information in these figures is, however, the position of the schatter-plots above or below the zero-line; not the respective slopes.

[19] Meghir and Palme (2000) reveal the same pattern for Sweden.

find that increased income matters at the lowest range of the income distribution. An increase from the 10th to the 20th percentile appears to reduce the probability of choosing the lowest levels of education, while we find the reverse effect for the highest educations.[20] The pre and post reform findings are illustrated in Figure 5.2:

Figure 5.2 about here.

The steeper the lines, the stronger the response to income changes. Hence, income appears to contribute less to educational attainment for the post-reform than for the pre-reform sample. This finding lends support to the view that reforms of the type in question might stimulate to prolonged education through reduced liquidity constraints for the poorest. Figure 5.3 illustrates that the income response ceases from the 20th to the 30th percentile. The same goes for even higher income levels (not reported).

Figure 5.3 about here.

6 Results from estimation of the wage equation

In this section we will present the result from the wage equation estimation for the different models. We start with the model where education is a continuous variable and present results from the OLS, IV and random coefficient models. Then we present the result for the model where education is not years of education but instead defined in types of education allowing for differences in returns for different qualifications. After discussing the results taking into account unobserved heterogeneity we present the results of a model taking into account heterogeneity in returns from observed family background variables.

6.1 Constant returns to years of education

In Table 6.1 we show the results of the earnings equation where we assume constant returns to education by specifying the education variable as years of education. In column one we tabulate the OLS returns of education for male workers for all sectors where education is now assumed to be exogenously determined. Note that we are estimating the wage equation on the birth cohorts 1947-58, which means that they are 37-48 years of age in 1995, and since we then are estimating on prime working age men it is expected that returns to education is a bit higher than using a wider range of age groups. The average returns to education using OLS is 5.90 percent which is in accordance with results from

[20]Note, however, that the reform is a poor predictor for educational attainment above the level of upper secondary school (level 5), so the findings for the highest levels should be weighted accordingly.

other studies (Hægeland, Klette and Salvanes, 1999, Hægeland, 2001, Raaum and Aabø, 2000). Our results indicate a slightly higher returns to education than the returns reported implicitly from Hægeland, Klette and Salvanes (1999), but identical to the coefficient for male workers for cohorts born 1942 for 1995 in Hægeland (2001, Table 1), which is 5.90 percent. Note that the birth cohort is included - but not reported - in the wage equation we are using in order to compare our results when controlling for selection where this is essential.

Table 6.1 about here.

In column two the results with selection is presented where the compulsory school reform is used as the identifying instrument. In order to construct the selection parameter, ξ, the order probit model is estimated separately for pupils participating in the new compulsory school system and those who did not. In addition the cohort and county of birth are included. The county of birth is included since our school reform indicator could pick up the effect of area of birth which also may serve as an instrument for education (Card, 1995; Hægeland, Klette and Salvanes, 1999). We want our school reform dummy to have an impact above the effect of county of birth - interpreted as distance to higher education - and therefore we include both. Cohort is included in both the school choice probit model and the wage regression in order to control for cohort effect since we do know from Section 2 that more recent cohorts have higher education attainment in general. Since the probit model is estimated separately for individuals going through the new and the old school system, this is equivalent to estimating with a complete set of interaction terms.

The results when using the selection model shows that the returns to education increases from 5.90 to 7.27 percent. This is a standard result now for instance in the literature using a measure of distance to higher education (Card, 1999, and for Norway see Hægeland, Klette and Salvanes, 1999). It indicates that there is heterogeneity in returns to education in that the instrument used picks up the returns to education for the group that comply with the treatment. In this case it is reasonable to think that the compliers are from a family background with budget constraint but with high marginal returns, which are being pushed to higher education attainment when the new compulsory school system was introduced and thus lowering the costs. So the LATE interpretation of returns to education which is the interpretation of IV estimates in the case of heterogeneous returns (Angrist, Imbens and Rubin, 1996), is that our estimated parameter of returns to education is the average returns to education for a person acquiring an extra year of education just because of the educational reform and would have been a drop out from education above 7 years otherwise. This result is in contrast to a similar specification in Meghir and Palme (2000) where they do not find any significant effect on returns to education using participation in the compulsory school reform as the instrument.

From our probit school choice results reported in Section 5, we do know that family income becomes less important in explaining the choice of higher education for low income families after the reform. This indicates that it is pupils from this family background we are picking up when using the school reform as an instrument. This was also one of the groups particularly targeted by the government when introducing the reform. An indirect test of this in the wage equation then is to introduce family income - defined as before in Section 4 - in both the school choice variable and in the wage equation. The counterfactual in this case then is that we are comparing reform students to non-reform students in the same cohort and also within the same family income groups. The result reported in column three shows that the returns to education now drops to 4.22 which neutralizes the effect of the IV results and gets us back to results similar to OLS.[21] This means that the reform variable as an instrument indeed appears to pick up able students from a poor background in terms of family income, and when controlling for that we actually get back to the OLS results also with a slight reduction in the returns to education.[22]

As the fourth specification we estimate the random coefficient model where now the interpretations is an ATE effect since both endogeneity and heterogeneity are purged from the wage equation. The returns to education now is 7.14 percent on average per year of education which is significantly higher than in the OLS specification but not different from the IV result reported in column two. If we now interpret this result literally, it means that the returns to education in the population is higher when heterogeneity and endogeneity is controlled for. In our case this implies that there must have been a negative selection into higher education caused by the reform since the average returns to education is significantly higher when this is controlled for. This result is then partly in contrast to the LATE result obtained from the IV estimates presented in column two of Table 6.1, where the results indicated positive selection into higher education for students going through the reformed 9 years compulsory school system. However, we notice that the interaction term between the selection parameter, ξ, and years of education is not significant in this model. This means that for this specification of the model the results of the random coefficient model basically is the IV model and the result should accordingly be interpreted as the average returns for the compliers. However, this may also mean that specifying the random coefficient model with years of education it not necessarily a good one, and especially in our case where we also assume constant returns to education for each extra year of education. First, the random coefficient results assumes that individuals have comparative advantages on certain educational levels and act upon this when choosing education. One would expect that students primarily

[21] The returns to education with OLS but where family income is included in the wage equation gives 5.45 percent in returns to education.

[22] In fact, using fathers education instead of family income (or both or also interacted) gives almost identical and not significant different results as using family income only.

think in terms of types of education - for instance to become a teacher in history and then perhaps in years of education, e.g. to become a history teacher in the primary school system requiring an undergraduate university degree or teach in high school with a graduate level degree. Second, since constant returns to education is assumed between any years of education one would expect that much of the potential differences which we think students make their choices from disappears. A model which specifies types of education is expected to make the results from the students choices based on comparative advantages clearer, and also to estimate differences in selection over education types. Thus we turn to the results using qualification levels in the next section.

6.2 Returns to education levels

In Table 6.2 we first present the OLS results of returns to education defining education in qualification levels from one year vocational training to master/doctoral university degrees. The reference level is 7 years pre-reform compulsory education. In addition to the fact that it may in our setting be more natural to define education in qualification levels than in years of education, defining education in qualification levels also allows for non-linear effects which in previous studies has been found important in Norway (Hægeland, Klette and Salvanes, 1999).

Table 6.2 about here.

The OLS results show that comprehensive school either in the new compulsory school system or in the pre-reform system provides 10 percent in return for the two years of education. Three years of vocational training gives 16.7 percent in returns or 6.7 percent above 9 years. There is actually a negative return in taking 2 years of vocational training as compared to one year of vocational training. Upper secondary or gymnasium gives a return of 35.7 percent compared to 7 years, or 25.7 percent as compared to 9 years comprehensive school. Noticeable is that undergraduate regional college or short university degrees do not give very much extra in returns as compared to gymnasium. This is a result that also has been noticed before (Hægeland, Klette and Salvanes, 1999).[23] The returns to a masters degree is 62.8 percent compared to pre-reform 7 years or labor 52 percent compared to 9 years compulsory. All of these results compare well with results obtained for instance Sweden (Edin and Holmlund, 1997, Meghir and Palme, 2000).

[23]Note that these categories of 1-2 years of college/university and especially 3-4 years of college/university degress comprise many different types of education as one year of university without any degree via two years college degrees to 4 year university degrees both in universities (cand.mag in the Norwegian system) or degrees from technical universities and degrees from business schools where it is very hard to enter based on high school marks. Hægeland (2000) has analysed this in detail and the hetereogeneity in returns noticeable.

Turning to the IV result controlling for absolute ability levels reported in column 2 of Table 6.2, we notice that returns to all levels increase as in the case of defining education in years reported in Table 6.3. Note that we expect that the education reform of compulsory education only is a reasonably good instrument for the first levels of education and the reform does not identify the last education levels. This is seen by Figure 5.2 where we showed that the reform did not influence the attendance at the last three levels of education. Hence, we report but do not comment these results.

A couple of the results for the first qualification levels are notable though. For instance, one year of vocational education now increases from 7.6 percent in return to 14.3 percent and now also there is an increase in return from 9 years of compulsory school to one year vocational. Also there is a strong increase in returns from one year vocational to 2-3 years of vocational; from 16.7 percent in the OLS case to 27.2 percent in the IV case which is an increase of 63 percent. For the upper secondary also an increase in returns exists, but less in size.

Again interpreting these estimates as LATE effects indicates that it is pupils from low income background but talented that are picked up since the results appear to support positive selection. An indirect test to this was undertaken by including family income both in ordered probit and in the wage equation, and as seen from column three the results drop back to basically the OLS results again neutralizing the effect of the IV results.

Turning to the result when controlling also for heterogeneity in returns by specifying the random coefficient model, we notice now that there is a less clear picture than for the model assuming constant returns to education. For compulsory nine years of education for the population when the effect of both heterogeneity as well as ability bias is purged from the results, the results show that the returns to education have increased to 31.1 percent as compared to 10.2 for the OLS results and 12.8 percent from the IV results. This means that there has been a negative selection into this type of education due to the reform which is of course natural since it is now a mandatory education. For one year of vocational education however, there is a decrease in returns both compared to the OLS results and to the IV results when controlling for heterogeneity. This means that there has been positive selection into this type of education caused by the reform. We notice also that the fourth level which is two to three years of vocational training also has a lower return as compared to the IV results but slightly above the OLS results. However for upper secondary education the results are very similar but slightly above the results for the IV estimate but definitely above the OLS results, indicating less selective recruitment to this education type after the education reform. This results are very interesting and in contrast to the results from a similar analyses of the Swedish reform where no differences between the OLS, IV and random coefficient results were obtained. The results also go in the direction expected *a priori* from this type of educational reform. The reform improved the recruitment to vocational education, but the recruitment to the

previously more elitist gymnasium education is less selective after the reform.

6.3 The effect on earnings of observed background variables

Equal opportunity along the socio-economic dimension was one of main aims of the comprehensive school reform. We have already seen that there exists a direct effect on earnings of parents' family income, and also an indirect effect via education. In particular it appeared that pupils from low income background but talented were picked up by the reform and leading to a positive selection into vocational education. In this section we will assess further the effect of family income on earnings and whether the direct effect has changed as a consequence of the reform. The framework we will use is presented in equation 11 in Section 3.2 and the distinguishing feature of this model is that we allow for heterogeneity in *returns to education* conditioned on observed background variables such as family income. This model then allows us to assess whether there are heterogeneity in returns to education also from observed background variables, e.g. whether students from high income families also have higher returns to education and at which level of education. Further, this model allows a measure of private returns to education purged of the effect of heterogeneity in returns to education from background variables. By estimating the model given in Equation 11 by OLS and by the IV and as well as introducing unobserved heterogeneity by estimating the random coefficient model, we are able to consider the private returns controlled for both unobserved and observed heterogeneity in returns to education.

Table 6.3 about here.

In Table 6.3, column one, the OLS results for the model of education levels is presented including both the family income variable grouped into ten percentiles as well as interacted with the education levels. Note that the OLS results should be interpreted as a weighted average of the marginal returns to education in the population. For the interaction terms only the significant parameters are presented. First we note there exist a general effect from family income directly on earnings. It is significantly positive only for the five highest percentiles of family income, and the impact is increasing in income level. The interpretation of this effect could be both that the family income of the student picks up socio-economic status and ability and cognitive competence, as well as an direct effect of family background on earnings as a network effect for instance by increasing the opportunity of getting a well paying job coming from a well to do family. As seen from the table heterogeneity in returns to education does exist from parents' income since some of the interactions terms between education level and income are significant. However, only the 80th and 90th percentiles of the family income distribution are important, and it is only important for some of the education

levels. The returns to education is reduced for all levels of education when parents's income is introduced when compared with the OLS results presented in the first column of Table 6.2. Hence, controlling for family income and observed heterogeneity in family background, the returns to education is reduced.

The IV specification presented in column 2, is identical to the specification presented in Table 6.2 column 3, except that heterogeneity in returns caused by observed family income is included in addition to the effect of the level of family income. When we compare the results in column 2 of Table 6.3 to the OLS results of Table 6.3 and the OLS and IV results of Table 6.2, some interesting results appear. First of all, family income matters on earnings as before and family income matters for returns to education but only coming from the 80th an 90th family income percentiles. The returns to education parameters are now interpreted as LATE effects as the returns to the students complying with the reform but now interpreted as within each income category and purged of heterogeneity in returns to education from parents' income. The returns to all levels of education are reduced as compared to the OLS results reported in this table, and the ξ becomes negative. This means that the compliers of the reform within each income category and purged for heterogeneity in returns from income, is a negatively selected group with returns below the average returns given by the OLS results. Note also that the region where the pupils grew up are both included in the school choice and the earnings function. There appears to be especially negative selection into the two vocational educational levels. Again this result points to the fact that low income groups targeted by the reform indeed where picked up by the reform.

The third specification, presented in the third column of Table 6.3, shows the returns to education purged of both the effect of observed and unobserved heterogeneity. The result from this models reports the returns to education for a person drawn randomly in the population when both absolute and relative earnings capacity are controlled for on both unobservables and observed family income. We notice the returns to education increases for all levels which our instrument can identify. The results are very similar to the results obtained using the random coefficient model reported in the third column of Table 6.2. Purged of the heterogeneity in returns to education of unobserved factors, the interaction terms between education levels and family income come in for all education levels and mostly from the 50th percentiles of family income and up and not only from the highest percentiles.

7 Concluding Remarks

In this paper we have used a rich data set to explore the relation between increasing the number of compulsory years of education on educational attainment. Supply side "shocks" lend them self well to be used as instruments in demand

type models. We explore the use of the compulsory reform as an instrument in identifying and interpreting different return parameters of education.

We find that the school reform had an effect of enhancing educational attainment for low achievers. However, we can not find any effect of the reform on more than 2 years of university education. Our selection model provide insightful information about the return to education. Several recent papers have found that IV estimates based on changes on the supply side of the educational sector is higher than OLS estimates of the return to schooling. This may be reasonably explained if the reforms tend to recover returns to schooling for a group of individuals with relatively high return to schooling. If these individuals have high marginal cost of education, and this is the reason they did not take more education prior to the reform, and not because they had a low return to schooling, capturing cost components to education would tilt the return downwards.

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Table 4.1. Reform implementation by cohort. Number and shares (in parenthesis)

	Reform		
cohort	Pre	Post	Total
48	14035	165	14200
	(18.71)	(0.28)	(10.53)
49	13585	441	14026
	(18.11)	(0.74)	(10.40)
50	12217	964	13181
	(16.29)	(1.61)	(9.77)
51	10809	2618	13427
	(14.41)	(4.37)	(9.95)
52	9268	4505	13773
	(12.36)	(7.52)	(10.21)
53	5658	5991	11649
	(7.54)	(10.01)	(8.64)
54	4574	8624	13198
	(6.10)	(14.40)	(9.78)
55	2875	10486	13361
	(3.83)	(17.51)	(9.91)
56	1295	12409	13704
	(1.73)	(20.72)	(10.16)
57	689	13676	14365
	(0.92)	(22.84)	(10.65)
Total	75005	59879	134884
	(100.00)	(100.00)	(100.00)

Table 4.2. Data selection process due to misssing observation.

All men 1948-1957 cohorts	295 646
With data on reform implem.	204 858
Full time employed (1995)	147 998
Missing observations on variables	134 884

Table 5.1. Definition of education levels used in this paper.

1	Pre-reform compulsory school (7 years)
2	Post-reform compulsory school or pre-reform junior secondary school (9 years)
3	Upper secondary school 1 year; mainly vocational
4	Upper secondary school 2-3 years; mainly vocational
5	Upper secondary school 3 years; gymnasium
6	University; post upper secondary school 1-2 years
7	University; post upper secondary school 3-4 years
8	University; master level university degree 5- years; law, medicine, engineering

Table 5.2. Observed distribution of qualification levels by reform status. Birth cohorts 1948-57.

Level	Pre-reform	Post-reform	Increase	Increase %
Basic school	0.1340	0.0066	-0.1274	-95.1
Comprehensive	0.0796	0.1343	0.0547	68.7
Vocational I	0.1708	0.1886	0.0178	10.4
Vocational II	0.2551	0.2884	0.0333	13.1
Upper secondary	0.0426	0.0563	0.0137	32.2
University I	0.1338	0.1423	0.0085	6.4
University II	0.0887	0.0916	0.0029	3.3
University II	0.0954	0.0959	0.0005	0.5

Table 6.1 Earnings equations, full time men, cohorts 1947-1957.

Earnings equations, full time men, cohorts 1947-57.				
Education	5,898E-02 *	7,265E-02 *	4,216E-02 *	7,137E-02 *
	(4,024E-04)	*(1,403E-03)*	*(1,489E-03)*	*(1,811E-03)*
Lambda		3,875E-02 *	-3,347E-02	4,005E-02 *
		(3,809E-03)	*(3,876E-03)*	*(3,981E-03)*
Education*Lambda				-3,767E-04
				(3,361E-04)
Tenure	1,049E-02 *	1,050E-02 *	1,094E-02 *	1,051E-02 *
	(5,041E-04)	*(5,039E-04)*	*(4,978E-04)*	*(5,040E-04)*
Tenure2	-4,829E-04 *	-4,830E-04 *	-5,029E-04 *	-4,832E-04 *
	(2,340E-05)	*(2,340E-05)*	*(2,310E-05)*	*(2,340E-05)*
Experience	4,461E-02 *	4,537E-02 *	4,650E-02 *	4,565E-02 *
	(1,646E-03)	*(1,647E-03)*	*(1,626E-03)*	*(1,666E-03)*
Experience2	-7,032E-04 *	-7,140E-04 *	-7,206E-04 *	-7,207E-04 *
	(4,090E-05)	*(4,090E-05)*	*(4,040E-05)*	*(4,130E-05)*
R-Squared	0.1679	0.1685	0.1685	0.1884
Number of Obs,	134884	134884	134884	134884
Family income	No	No	Yes	No
Selection	No	Yes	No	Yes
Error Coefficient	No	No	No	Yes

Earnings equations, male full time workers, cohorts 1947-57				
Comprehensive	1,017E-01 *	1,275E-01 *	4,253E-02 *	3,111E-01 *
	(4,146E-03)	*(4,837E-03)*	*(5,051E-03)*	*(2,390E-02)*
Vocational I	7,602E-02 *	1,426E-01 *	-4,528E-02 *	3,015E-02 *
	(3,767E-03)	*(7,455E-03)*	*(8,036E-03)*	*(2,289E-02)*
Vocational II	1,676E-01 *	2,717E-01 *	-2,122E-02 °	2,278E-01 *
	(3,621E-03)	*(1,070E-02)*	*(1,172E-02)*	*(2,104E-02)*
Upper Secondary	3,565E-01 *	4,851E-01 *	1,035E-01 *	5,494E-01 *
	(5,172E-03)	*(1,346E-02)*	*(1,490E-02)*	*(2,497E-02)*
University, I	3,997E-01 *	5,459E-01 *	1,246E-01 *	6,180E-01 *
	(4,156E-03)	*(1,473E-02)*	*(1,629E-02)*	*(2,601E-02)*
University, II	3,694E-01 *	5,413E-01 *	4,763E-02 *	8,936E-01 *
	(4,616E-03)	*(1,725E-02)*	*(1,909E-02)*	*(3,714E-02)*
University, III	6,283E-01 *	8,421E-01 *	2,188E-01 *	1,072E+00 *
	(4,790E-03)	*(2,122E-02)*	*(2,371E-02)*	*(5,652E-02)*
Lambda		6,201E-02 *	-1,004E-01 *	3,628E-02 *
		(5,995E-03)	*(6,400E-03)*	*(1,259E-02)*
Comprehensive*Lambda				3,396E-01 *
				(3,423E-02)
Vocational I*Lambda				2,304E-01 *
				(2,259E-02)
Vocational II*Lambda				4,236E-01 *
				(2,807E-02)
Upper Secondary*Lambda				2,138E-01 *
				(2,964E-02)
University, I*Lambda				1,068E-02 *
				(4,937E-04)
University, II*Lambda				-4,899E-04 *
				(2,290E-05)
University, III*Lambda				4,425E-02 *
				(1,658E-03)
Tenure	1,055E-02 *	1,059E-02 *	1,100E-02 *	-5,859E-04 *
	(4,955E-04)	*(4,953E-04)*	*(4,898E-04)*	*(4,110E-05)*
Tenure2	-4,832E-04 *	-4,845E-04 *	-5,024E-04 *	6,517E-03 °
	(2,300E-05)	*(2,300E-05)*	*(2,280E-05)*	*(3,735E-03)*
Experience	4,511E-02 *	4,363E-02 *	5,064E-02 *	1,582E-02 *
	(1,649E-03)	*(1,654E-03)*	*(1,644E-03)*	*(3,865E-03)*
Experience2	-6,176E-04 *	-5,821E-04 *	-7,309E-04 *	2,769E-02 *
	(4,090E-05)	*(4,100E-05)*	*(4,080E-05)*	*(3,987E-03)*
R-Squared	0,1967	0.1973	0.2138	0.2028
Number of Obs.	134884	134884	134884	134884
Family background	No	No	Yes	No
Selection	No	Yes	No	Yes
Error Coefficient	No	No	No	Yes

Table 6.2. Earnings equations, male full time workers, cohorts 1947-57.

Table 6.3 Earnings equations, male full time workers			
Comprehensive	8,380E-02 *	3,333E-02 *	2,649E-01 *
	(1,143E-02)	*(1,182E-02)*	*(2,377E-02)*
Vocational I	4,879E-02 *	-7,281E-02 *	5,323E-02 *
	(9,563E-03)	*(1,206E-02)*	*(2,173E-02)*
Vocational II	1,500E-01 *	-3,688E-02	1,316E-01 *
	(9,061E-03)	*(1,449E-02)*	*(2,157E-02)*
Upper Secondary	2,880E-01 *	5,143E-02	8,751E-02 *
	(1,876E-02)	*(2,359E-02)*	*(4,284E-02)*
University, I	3,766E-01 *	1,136E-01 *	2,729E-01 *
	(1,146E-02)	*(1,961E-02)*	*(3,421E-02)*
University, II	3,220E-01 *	1,216E-02 *	1,310E-01
	(1,351E-02)	*(2,311E-02)*	*(5,096E+05)*
University, III	6,064E-01 *	2,267E-01 *	4,748E-01 *
	(1,454E-02)	*(2,719E-02)*	*(8,107E-02)*
Income perc 2	-4,959E-03	-8,137E-04	-1,004E-01 *
	(1,068E-02)	*(1,068E-02)*	*(6,400E-03)*
Income perc 3	3,695E-03	9,649E-03	-5,841E-03
	(1,086E-02)	*(1,085E-02)*	*(1,068E-02)*
Income perc 4	1,542E-02	2,090E-02 °	2,874E-03
	(1,083E-02)	*(1,082E-02)*	*(1,086E-02)*
Income perc 5	2,020E-02 °	3,312E-02 *	1,475E-02
	(1,160E-02)	*(1,161E-02)*	*(1,083E-02)*
Income perc 6	2,554E-02 *	4,249E-02 *	1,907E-02
	(1,208E-02)	*(1,212E-02)*	*(1,168E-02)*
Income perc 7	5,996E-02 *	8,988E-02 *	2,411E-02 *
	(1,295E-02)	*(1,307E-02)*	*(1,224E-02)*
Income perc 8	7,652E-02 *	1,145E-01 *	5,773E-02 *
	(1,357E-02)	*(1,375E-02)*	*(1,344E-02)*
Income perc 9	9,370E-02 *	1,505E-01 *	7,388E-02 *
	(1,644E-02)	*(1,678E-02)*	*(1,432E-02)*
Income perc 10	1,123E-01 *	2,113E-01 *	9,023E-02 *
	(2,243E-02)	*(2,320E-02)*	*(1,779E-02)*
Tenure	1,077E-02 *	1,084E-02 *	1,085E-02 *
	(4,651E-04)	*(4,647E-04)*	*(4,644E-04)*
Tenure2	-5,318E-04 *	-5,349E-04 *	1,085E-02 *
	(2,330E-05)	*(2,330E-05)*	*(4,644E-04)*
Experience	4,541E-02 *	4,892E-02 *	-5,369E-04 *
	(1,629E-03)	*(1,641E-03)*	*(2,330E-05)*
Experience2	-5,961E-04 *	-6,865E-04	4,827E-02 *
	(4,030E-05)	*(4,070E-05)*	*(1,646E-03)*

Table 6.3 continued			
Income perc 10*Comp	7,068E-02 *	6,971E-02 *	2,357E-01 *
	(2,651E-02)	*(2,649E-02)*	*(3,030E-02)*
Income perc 10*Voc I	4,563E-02 °	5,038E-02 *	4,100E-02
	(2,395E-02)	*(2,506E-02)*	*(3,344E-02)*
Income perc 9*Upper Sec	6,105E-02 *	7,364E-02 *	2,508E-01 *
	(2,567E-02)	*(2,565E-02)*	*(4,054E-02)*
Income perc 10*Upper Sec	9,864E-02 *	1,091E-01 *	3,956E-01 *
	(2,987E-02)	*(2,984E-02)*	*(5,748E-02)*
Income perc 10*Uni II	9,301E-02	9,875E-02 *	4,192E-01 *
	(2,600E-02)	*(2,598E-02)*	*(4,282E-02)*
R-Squared	0.2143	0.2159	0.2138
Number of Obs.	134884	134884	134884
Family background	Yes	Yes	Yes
Selection	No	Yes	Yes
Error Component	No	No	yes
Family background het.	Yes	Yes	Yes

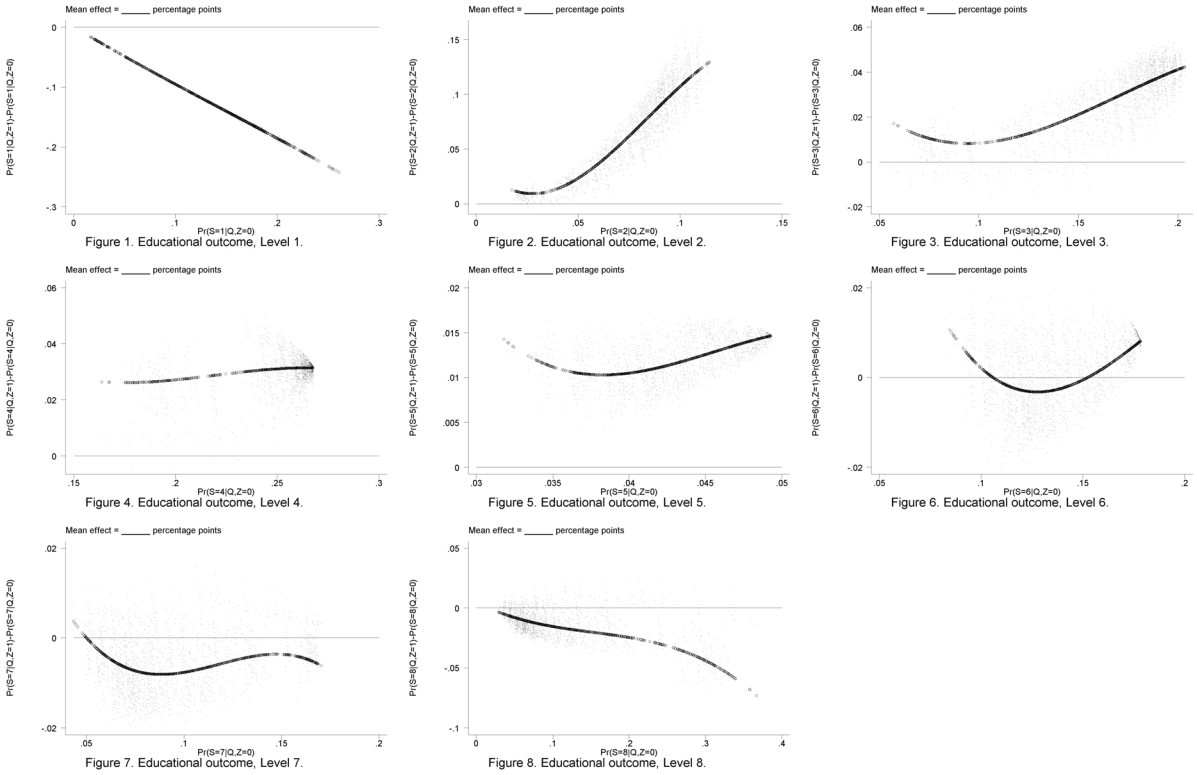

Figure 5.1. The effect of the reform on educational attainments.

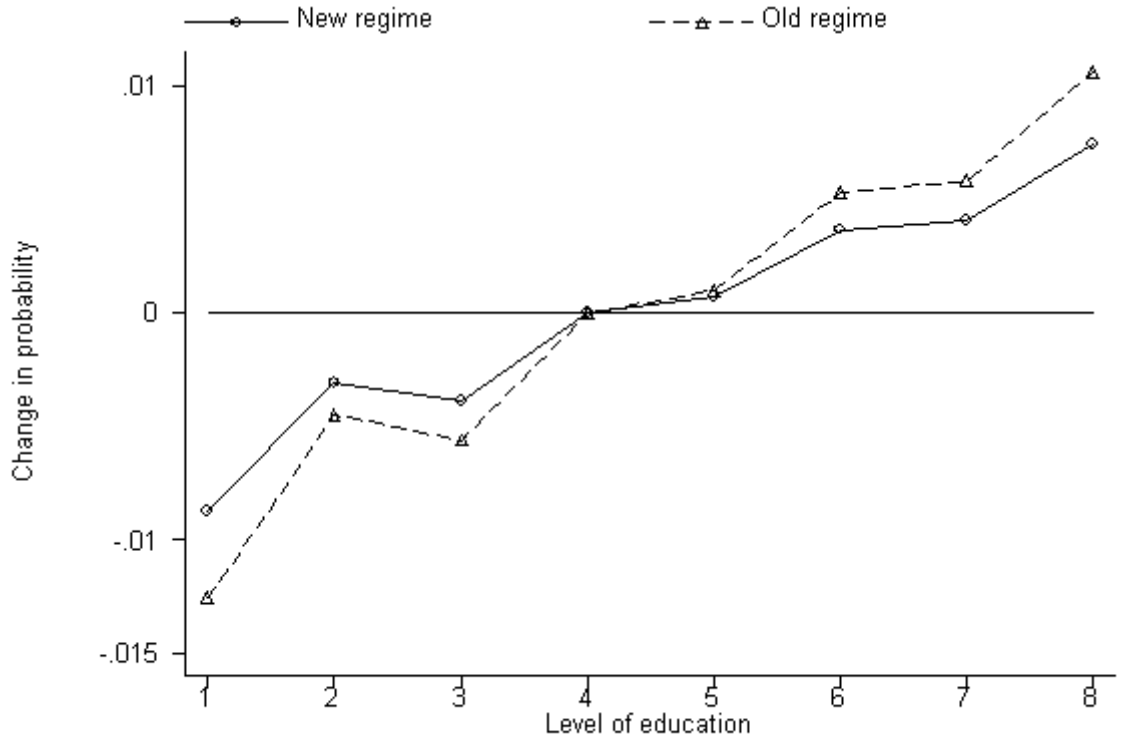

Figure 5.2. The effect of family income in new and old regime at different schooling levels.

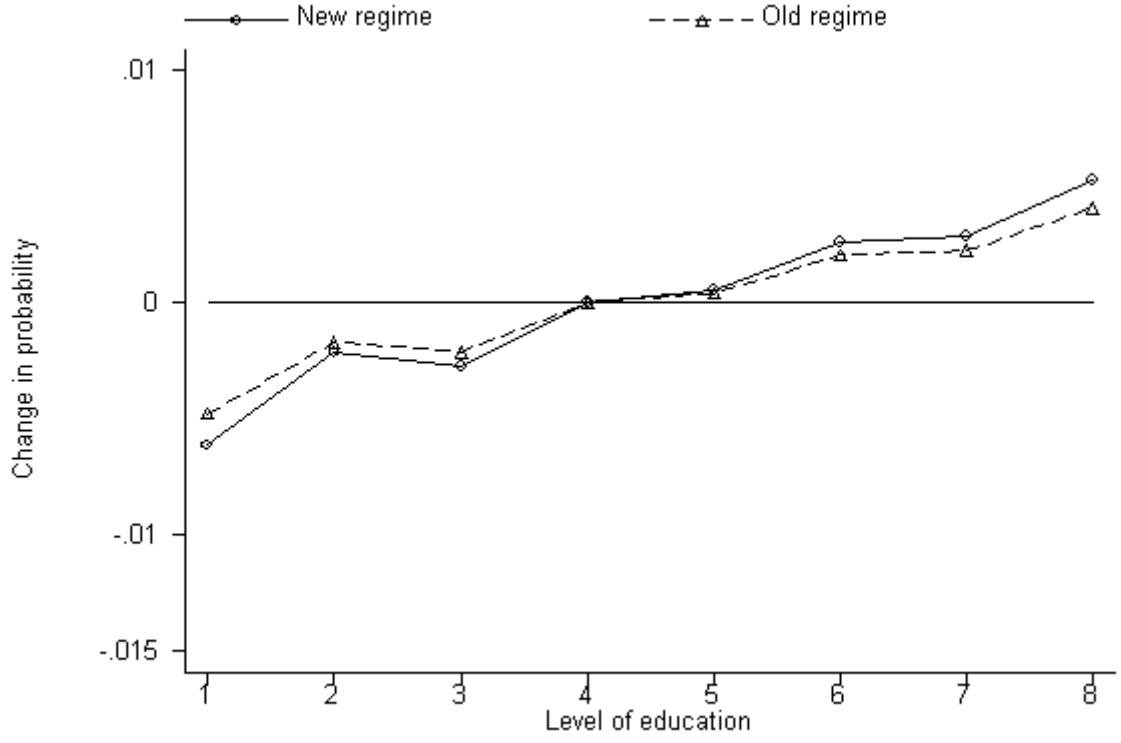

Figure 5.3. The effect of family income in new and old regime at different schooling levels.