Changes in Compulsory Schooling, Education and the Distribution of Wages in Europe

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Abstract: Using data from 12 European countries and the variation across countries and over time in the changes of minimum school leaving age, we study the effects of the quantity of education on the distribution of earnings. We find that compulsory school reforms significantly affect educational attainment, especially among individuals belonging to the lowest quantiles of the distribution of ability. There is also evidence that additional education reduces conditional wage inequality, and that education and ability are substitutes in the earnings function.

JEL Codes: J24; **Keywords**: Education reforms, returns to education, quantile treatment effects

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Does education affect earnings? This question has attracted enormous attention among labour economists, as reviewed by Card (2001). By and large, the empirical literature has focused on the mean returns to education, with substantial effort devoted to the identification of a causal relationship. Less has been done to investigate how additional education affects the distribution of earnings. Does education reduce (conditional) wage inequality? Are the returns to education heterogeneous and is this heterogeneity correlated to ability?

These are important policy questions. If education reduces the dispersion of earnings, and equality is valued by the policy maker, then additional schooling can be a powerful tool to combat inequality. It is well known that individual ability is strongly affected by genetic and environmental factors (see Cunha and Heckman, 2007). If education and ability are substitutes in the production of human capital and earnings, then additional investment in the former can contribute to reducing the differences generated by the latter (see Ashenfelter and Rouse, 1998).

How education and ability interact in the generation of earnings and human capital has important implications for optimal education policy. For instance, De Fraja (2002) shows that optimal public policy is more elitist than market provision in the following sense: the difference in educational attainment between bright and less able children is greater than it would be if education were only provided privately. In this case, redistributive education policies that target the less able are bound to have a substantial cost in terms of efficiency. His results, however, require that education and ability are complements in the generation of human capital and earnings.

The paper addresses these questions by investigating the relationship between the quantity of attained education and the conditional distribution of (gross) hourly earnings in a unique sample of 12 European countries, which we have constructed by pooling together information drawn from three different surveys. Our empirical methodology is an instrumental variable approach to the endogeneity of education in a quantile regression framework, as in recent work by Chesher (2003), and Ma and Koenker (2006). We identify the causal effects of education on earnings by using the country and time variation provided by the compulsory school reforms implemented in Europe after the end of the Second World War.[1]

The exogenous variation provided by minimum school leaving age laws has been used in the empirical literature since Angrist and Krueger (1991) to identify the causal relationship between education and earnings. Since these laws have been targeted at the lesser educated, who typically belong to the lower quantiles of the distribution of earnings, their use in the current context prompts the question whether the changes in compulsory education observed in Europe after the last war have been particularly beneficial to

[1] Moretti and Lochner (2004), Lleras Muney (2005) and Oreopoulos (2006) use a similar approach by exploiting regional variation within a single country.

the targeted population or have spread instead their effects to the population at large. We provide evidence that in a host of European countries the effect of compulsory schooling laws on educational attainment is statistically significant for all but the top deciles of the distribution of male education (all but the very top for females). As expected, the size of this effect declines as we move from the bottom to the top quantile. The statistically significant effect of compulsory school reforms on individuals with higher educational attainment (that is also found for Sweden in Meghir and Palme, 2005) suggests that better educated individuals react to increases in compulsory schooling by raising their own attainment, possibly in an effort to maintain their educational advantage over the less educated, who are more directly affected by the reforms.

When we treat education as exogenously assigned to individuals, we find that one additional year of schooling increases conditional wage inequality both for males and for females, in line with previous findings in the US (Buchinsky, 1994) and Europe (Martins and Pereira, 2004). In our approach, we allow earnings to depend on both luck and ability, while education depends only on ability. This makes education endogenous in the earnings equation. When we allow for endogeneity, we find instead that conditional (gross) wage inequality is reduced by marginal increases in education for all ability levels.

Focusing on the mean quantile treatment effect, we find that assigning an extra year of education to males in the sample marginally reduces the estimated $90-10$ conditional wage differential by 0.99 percentage points; the reduction is 1.34 percentage points in the case of females. By conditioning on selected quantiles of the conditional distribution of earnings, we investigate how the returns to education vary as we move from the bottom to the top quantile of the distribution of ability. Our key finding is that returns decrease with ability, which points to substitutability in the relationship between education and ability.

Overall, these results do not lend support to the elitist education policy discussed by De Fraja (2002). They are in line with the findings of Ashenfelter and Rouse (1998), in the US context and suggest that education policies which target the less fortunate and / or less talented group in the pursuit of equality of opportunity are not necessarily inefficient. Substitutability also indicates that since ability and parental background are closely intertwined, education policy can contribute to undo the differences generated by the latter.

The paper is organized as follows: Section 1 reviews the empirical literature and Section 2 presents the empirical model. Our identification strategy is described in Section 3. Next, we turn to the data in Section 4 and to the results in Section 5. Robustness checks are discussed in Section 6. Conclusions follow.

1. Review of the Literature

By using quantile regression techniques one can trace the entire conditional wage distribution and examine how the shape of this distribution is affected by schooling, age and experience. In his pioneering work on the impact of education on the distribution of US wages, Buchinsky (1994), uses quantile regressions and finds that returns to education in the US are higher at the higher quantiles of the conditional distribution of wages. He uses the 0.90-0.10 spread as a measure of within group inequality and finds substantial changes over time.

In the European context, Harmon *et al.* (2003), use UK data and find that the returns to schooling are higher for those at the very top of the wage distribution compared to those at the very bottom. Since individuals with higher talent are more likely to be located in the upper part of the (conditional) wage distribution, this result – they argue - points to complementarity between ability and schooling. Martins and Pereira (2004), use data from 15 European countries and find that individuals who receive higher wages conditional on their characteristics enjoy higher education – related earnings growth. They suggest that over-education, poor school quality and the selection of fields of study with limited ex-post prospects can explain their results. In contrast with the previous findings, Denny and O'Sullivan (2007), use UK data and find evidence that cognitive and non cognitive ability and education are substitutes in the earnings function. On a similar line, Schultz and Mwabu (1996), find complementarity among white South African males, and substitutability among black South Africans.

The idea that individuals located in the upper part of the conditional wage distributions are of higher ability than those located in the bottom part is plausible but ignores that the allocation of individuals to different points of the distribution could also depend on other factors which are orthogonal to ability itself. Lang (1993), for instance, distinguishes between cognitive ability, which matters for school and the labour market, and the ability which matters mainly for work.[2] Hornstein *et al.* (2006) show that ex-ante identical individuals could end up with different wages because of random matching with available vacancies. Chesher (2003) calls this labour market fortune. When conditional wages are driven by more than one unobservable factor, the observation that returns to education are higher in the upper quantiles of the conditional distribution of earnings does not suffice to establish complementarity between schooling and ability.

The reviewed studies have in common that they do not address the endogeneity of education. Because of this, their results are best interpreted as interesting associations and correlations, with little to say about causal effects. Some recent papers have estimated returns to education within an IV framework. Ichino

[2] Lang's approach is different from that of Heckman *et al.* (2006), who distinguish between cognitive ability and social skills. For these authors, both types of ability affect schooling and earnings.

and Winter Ebmer (1999) and Aakvik *et al.* (2003), for instance, compare local average treatment effects using different sets of instruments, which allows them to evaluate the returns to education at different levels of education and sometimes at different points in the distribution of individual ability. These studies show that returns of education are heterogeneous, at least at the points where the estimator is defined – a similar conclusion can be reached on the basis of Meghir and Palme's (2005) careful examination of the impact of the Swedish reforms of the late 1940s on both education and earnings for different groups of individuals. This evidence suggests the need for a more thorough investigation of the impact of education on the whole distribution of earnings – as provided by quantile regressions.

The econometric literature provides a few approaches for the identification and estimation of causal effects in quantile regressions. One such approach is due to Chesher[3], who considers non-parametric identification of a structural model with a recursive structure. Chesher (2001), points out that the continuity of the endogenous regressor is needed for the unambiguous definition of quantiles[4], and guarantees the point identification of the quantiles of interest. When the continuity assumption fails, Chesher's approach can be extended (see Chesher, 2003; 2005) but does not generally lead to point identification of the function describing the impact without further assumptions. Importantly, the case of an endogenous binary regressor cannot be dealt within this set-up.

The estimation of the exogenous impact functions and inference in the parametric case are discussed by Ma and Koenker (2006). They assume that the conditional quantile functions are known up to a finite number of parameters and add some technical regularity conditions. In their framework, the conditional quantile functions need not be linear in the parameters and the asymptotic theory is developed for nonlinear quantile regression estimation.

Arias *et al.* (2001), use data on US twins to address the issue of the endogeneity of education in quantile wage regressions. They propose a two stages estimator and find that returns to education increase with the quantiles of the conditional distribution of earnings. They interpret this as evidence that ability and education are complements. Their methodology, however, has been recently questioned by Ma and Koenker (2006), who use Monte Carlo simulations to show that the two-stage quantile regression which replaces education with predicted education from the ordinary least square (mean) regression of schooling on the set of instruments performs rather badly in terms of simulated bias when compared with Chesher's

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[3] Alternative approaches have been developed by Abadie *et al.* (2002) and Chernozhukov *et al.* (2005). The first methodology cannot be applied in our context because it requires that both the instrument and the endogenous regressors are binary variables. The approach by Chernozhukov *et al.* (2005) assumes that there is a single latent factor, unaffected by the treatment variable (schooling, in our application), that determines the relative ranking of individuals in the outcome distribution: individuals who are "highly ranked" earners without additional schooling remain "highly ranked" earners after achieving higher qualifications. We find the assumption too restrictive in our setting since we cannot condition on a large number of individual characteristics.

[4] In Chesher (2001), there is no requirement on the scale of the regressors and of the instruments but a completeness condition has to be met.

WAD estimator and with the control variate approach suggested by Ma and Koenker and implemented in this paper.

2. The Empirical Model

Following Card (2001) and Ashenfelter and Rouse (1998), assume that individuals - or their parents - choose years of schooling to maximize

$$u(w_i, s_i) = \ln(w_i) - c(s_i) \tag{1}$$

where $w_i = g(s_i)$ is (net) earnings, s_i is years of schooling, $c(s_i)$ is the cost of schooling and the index i is for the individual. At the optimum, individuals select s_i so as to equate the marginal costs to the (expected) marginal benefits of schooling. Let marginal costs $mc(s_i)$ be increasing in schooling, decreasing in cognitive ability a_i and a function of exogenous controls X_i and z

$$mc(s_i) = r(X_i, z) + \theta s_i - \kappa a_i \tag{2}$$

and assume the following Mincerian earnings function

$$\ln(w_i) = \beta s_i + s_i(\lambda a_i + \phi u_i) + \gamma_w X_i + a_i + u_i \tag{3}$$

where the constant term is included in **X**, cognitive ability $a \sim G_a(0, \sigma_a{}^2)$ is known to individuals at the time of their choice, and $u \sim G_u(0, \sigma_u{}^2)$ is an idiosyncratic error orthogonal to ability. In the language of Chesher (2003), the latent random variable u_i is fortune in the labour market, but other interpretations are possible, as discussed in the introduction, and include a zero mean demand shock which affects the relative productivity of jobs and skills (Gosling *et al.* 2000; Machin and Van Reenen, 1998).

The specification in Equation (3) implies that schooling influences both the location and the scale of the earnings distribution. Ability affects earnings both directly and via its interaction with schooling, as in Ashenfelter and Rouse (1998), who distinguish between the absolute and the comparative advantage of higher talent. Education and ability are complements in the production of human capital when $\lambda > 0$, and substitutes when $\lambda < 0$. Another feature of the selected specification is the possibility that shocks to the composition of labour demand - or random luck u_i - have different effects on earnings depending upon the level of accumulated schooling. When $\phi > 0$ these shocks are skill – biased (Katz and Murphy, 1992). With expected marginal benefits of schooling $mb(s_i)$ given by

$$mb(s_i) = \beta + \lambda a_i \tag{4}$$

optimal schooling s_i^* is equal to

$$s_i^* = \frac{\beta - r(X_i, z_i)}{\theta} + \frac{\lambda + \kappa}{\theta} a_i \tag{5}$$

Notice that the relevant variation in ability for the schooling decision involves the slope of the log earnings function, not its intercept.[5] In the private optimum, schooling increases with individual ability a_i if $\lambda + \kappa > 0$. This is the case either if ability and schooling are complements or if the effect of ability on the marginal costs is large enough to offset the substitutability between a_i and s_i. Two economic models suggest a positive relationship between ability and schooling: the signalling model and a variant of the human capital model (see Blackburn and Neumark, 1993). In the former model, more able individuals have lower marginal costs of schooling and self-select into higher education. In the latter model, ability increases the marginal benefits of education and reduces the marginal costs of schooling.

We assume that $1 + \phi s_i > 0$, which guarantees that log wages are a monotonic function of the random effect u_i. A feature of the model is that schooling in (5) is correlated with ability a_i, which affects log earnings both directly and via its effects on education in (3), but not with the random shock u_i. Unless we can adequately control for ability, the standard orthogonality condition required for the consistency of ordinary least squares estimation of (3) fails. Consistent estimates can be obtained, however, if there exists a variable z which is correlated with schooling but not with individual ability conditional on schooling (see Card (2001) and Blundell *et al.* (2005) for extensive discussions). As discussed in detail in the next section, z in this paper is the number of years of compulsory education *ycomp*.

Omitting subscripts for simplicity, the earnings - cum - education model presented above can be

[5] As discussed in Card (1995), "..individuals with higher earnings opportunities at each level of education (i.e. with higher intercepts in their log earnings functions) may well invest less in schooling, since they have a higher opportunity cost of attending school..".

written in the format of an exactly identified triangular model, as in Chesher's approach

$$\ln(w) = \beta s + s(\lambda a + \phi u) + \gamma_w X + a + u \tag{6}$$

$$s = \gamma_s X + \pi z + \xi a \tag{7}$$

where $\xi = \frac{\lambda + \kappa}{\theta}$. Define $\tau_a = G_a(a_{\tau_a})$ and $\tau_u = G_u(u_{\tau_u})$, where a_{τ_a} and u_{τ_u} are the τ - quantiles of the distributions of a and u, respectively. Furthermore define $Q_w[\tau_u \mid s, X, z]$ and $Q_s[\tau_a \mid X, z]$ as the conditional quantile functions corresponding to log wages and years of education. Ma and Koenker (2006) show that recursive conditioning yields the following model

$$Q_w[\tau_u \mid Q_s[\tau_a \mid X, z], X, z] = Q_s[\tau_a \mid X, z]\Pi(\tau_a, \tau_u) + \gamma_w X + G_a^{-1}(\tau_a) + G_u^{-1}(\tau_u) \tag{8}$$

$$Q_s[\tau_a \mid X, z] = \gamma_s X + \pi z + \xi G_a^{-1}(\tau_a) \tag{9}$$

Given the restrictions imposed by equations (6) and (7), the key parameter of interest $\Pi(\tau_a, \tau_u)$ is a matrix with the following structure

$$\Pi(\tau_a, \tau_u) = \beta + \lambda G_a^{-1}(\tau_a) + \phi G_u^{-1}(\tau_u) \tag{10}$$

which describes the quantile treatment effect of education on earnings. It offers a panoramic view of the stochastic relationship between schooling s and log wages, and describes the effects of a perturbation in the distribution of schooling on the various quantiles of the distribution of earnings. Rather than exogenously altering the value of s, we alter its various quantiles Q_s, and study how the quantiles Q_w of the distribution of earnings are affected (Ma and Koenker, 2006).

If we set τ_u so that u is fixed at its τ_u quantile, changes of τ_a in $\Pi(\tau_a, \tau_u)$ reflect how the distribution of a affects the τ_u quantile of the response $\ln(w)$. On the other hand, if we fix τ_a and allow τ_u to vary, we can shed light on how the τ_a quantile of s affects the entire distribution of log wages (Ma and Koenker, 2006). By integrating $\Pi(\tau_a, \tau_u)$ with respect to τ_a, we obtain the mean quantile treatment effects, which show how returns to education vary along the distribution of labour market luck for individuals of average ability. Further integration yields the mean treatment effect, which corresponds to the two stages least squares estimate. Conditioning on different values of τ_u one can investigate whether returns to education increase as we move from lower to higher quantiles of the distribution of ability at different points of the (conditional) wage distribution.

Estimation and inference for $\Pi(\tau_a, \tau_u)$ are discussed extensively by Ma and Koenker (2006), who focus on two approaches, namely the "weighted average derivative" (WAD) approach and the "control variate" (CV) approach. They show that the latter approach yields a more efficient estimator when the model is correctly specified and both estimators have a superior performance compared to alternative parametric estimators (see Ma and Koenker (2006) for details). In this paper, we implement the CV approach, which consists of the following three steps. First, we estimate the conditional quantile functions of schooling s and compute the control variate

$$a(\tau_a) = s - \overline{Q}_s(\tau_a \mid X, z) \tag{11}$$

where s is observed schooling and $\overline{Q}_s(\tau_a \mid X, z)$ is the estimated conditional quantile. Second, we augment the conditional quantile functions of $\ln(w)$ with the relevant control variate and its interaction with schooling. Finally, we simultaneously estimate the selected quantiles of $\ln(s)$ conditional on $Q_s[\tau_a \mid X, z]$, $\mathbf{X}$ and z as in the hybrid model in equation (8) and obtain the variance - covariance matrix by bootstrapping.

3. Our Empirical Strategy

We identify the causal effect of education on the distribution of wages by using the exogenous variation of schooling induced by compulsory school reforms implemented at different times and with different intensity in 12 European countries after the Second World War. The crucial difference between our study and previous literature using the same instruments (see for instance Lang and Krupp, 1986; Chevalier *et al.* 2004; Oreopoulos, 2006; and the references therein) is that our analysis is not limited to the exploration of the conditional mean impact of schooling on wages. Rather, we permit heterogeneity in the impact of education at different points of the conditional distribution of earnings.

In our empirical application, identification relies on the following assumptions:

- individuals with higher ability stay in school longer (monotonicity with respect to *a* in equation (7));
- individuals with a luckier draw from the distribution of wage offers have higher wages (monotonicity with respect to *u* in equation (6));

- when the schooling decision is made, individuals can only form expectations about their future draw from the distribution of wage offers (triangular structure in the unobservables);
- years of compulsory schooling *ycomp* have an exogenous impact on the distribution of years of education and/or the educational attainment of individuals: more education (the treatment) is assigned to individuals on the basis of their date of birth and the latter was not chosen by their parents on the basis of future education - related wage gains;
- when the timing of the implementation of school reforms varies across municipalities of the same country, it is uncorrelated with general education levels;[6]
- educational reforms do not affect log wages other than through the individual's education level, in other words they are excluded from the wage equation (triangular structure in the observables).

We use conditional quantile functions as in equations (8) and (9) and allow the conditional quantiles of years of education to differ across countries up to a constant, holding the value of the other conditioning variables as fixed. We pool data from several countries to increase the number of points on the support of the instrumental variable *ycomp*.[7] By pooling countries, we exploit the fact that the timing of compulsory school reforms varies across countries and by so doing we can distinguish school reform from cohort fixed effects. We return to this assumption in the Section on robustness checks.

We select for each country a school reform affecting compulsory education and define $t = (b - \bar{b}_k)$ as the distance between birth cohort b and the cohort $\bar{b}_k$, defined as the first cohort potentially affected by the change in mandatory school leaving age in country k. Since each selected reform occurs at a different point in time, our instrument varies both across countries and over cohorts. For each country, we construct a pre - treatment and post - treatment sample composed of the individuals born within the range defined by 7 years before and 7 years after the year of birth of cohort $\bar{b}_k$. The breadth of the window is designed to exclude the occurrence of other compulsory school reforms, which would blur the difference between pre- and post-treatment in our data. Our choice also trades off the increase in sample size with the need to reduce the risk that unaccounted confounders affect our results.

Borrowing from Angrist *et al.* (1996), the individuals with $t \geq 0$ who have changed their educational attainment as a result of the reforms are defined as "compliers".[8] Table 1 presents for each country in our

[6] Black *et al.* (2003) test this hypothesis in the case of Norway and find no systematic relationship.
[7] The support of this variable consists of 7 points.
[8] Individuals whose nationality is unknown and/or who are not citizens of the country in which they live at time of the interview are excluded from the analysis. The relative share of compliers is affected by migration flows within Europe. If for instance a German citizen belongs to the first cohort potentially affected but migrated as an adult from Italy, where he received his education, we cannot expect his education to be affected by the change in German

sample the selected reform, the year of birth of the first cohort potentially affected by the reform, the change in the minimum school leaving age and in the years of compulsory education induced by the reform, and the expected change in school attainment, expressed in terms of the ISCED classification. Our information is drawn from Eurybase, the Eurydice database on education systems in Europe, from personal communications with national experts and from other country-specific sources. The description of each reform and the explanation of our choice of $\bar{b}_k$ for each country are relegated to the Technical Appendix.

The selected reforms increased the minimum school leaving age by one year in Austria, Germany, Ireland, Netherlands and Sweden; by two years in Denmark, France and Spain; by three years in Finland, Greece, Italy and by four years in Belgium.[9] In some of these countries, the timing of the introduction of the reform varied by region - this is the case of Germany, Finland and Sweden. Since we do not have access to data at the municipality level, in Finland and Sweden we define the year of the reform in each area as the year when the largest share of municipalities in the area experienced the change in the schooling legislation (see Table A.1 for Finland in the Technical Appendix).

In our sample, the modal compulsory number of years of education before the reforms is 8 years. The first cohorts potentially affected by the reforms were born between 1941 and 1969, with a relative concentration between the late 1940s and the late 1950s.[10] Furthermore, the most commonly expected change in qualifications is the attainment of ISCED level 3 (upper secondary education). To illustrate the effects of school reforms on years of schooling, we purge the latter from the influence of country effects, country specific trends, individual and macro controls and plot the residuals in Figure 1 for a few years before and after the pivotal cohorts, who were first potentially affected. The upward jump at the time of the reforms is clearly visible and close to 0.3 additional years of schooling.

Tables A.2, A.3 and A.4 in the Technical Appendix summarize the existing empirical evidence on the effects of some of these compulsory school reforms on individual education and earnings as well as the instrumental variable estimates of the average returns to schooling. While the increase in compulsory schooling induced by each reform varies across countries, ranging from 1 additional year of schooling to 3 or 4, the estimated impact on educational attainment (in terms of years of education) is close to 0.3 additional years of schooling, with little cross-country variation. This number is very close to the one shown in Figure 1. Although the estimates of the effect of compulsory school reforms on educational

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schooling laws.

[9] Notice that in Italy, Belgium, Finland, France and in the Netherlands, these reforms were accompanied by a change in school design, typically the postponement of tracking. In the UK – a country not included in our analysis - two major compulsory schooling reforms were enacted in 1947 and 1973. Both reforms have been shown to have been very effective in raising educational standards and have been extensively studied in the literature.

[10] Close to 83% of the individuals belonging to the first cohorts affected by the reforms were born between 1947 and 1959.

attainment are broadly similar across European countries, this does not hold when one looks at the effects of longer schooling on wages: while in some countries the evidence suggests zero returns to compulsory schooling, in some other countries returns to longer compulsory schooling are as high as 15%-20%.[11]

4. The Data

We pool together data drawn from the 8th wave of the European Community Household Panel (ECHP) for the year 2001, the first wave of the Survey on Household Health, Ageing and Retirement in Europe, or SHARE, for the year 2004, and the waves 1993 to 2002 of the International Social Survey Program (ISSP). The countries included in our study are: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Spain and Sweden. Table A.5 (panel A, B, C) in the Technical Appendix shows for each country in the dataset the sample size for each survey and wave, and the relevant age range.[12]

Our dependent variable is the log of (gross) hourly earnings expressed at 2000 prices and purchasing power parity units. Additional information on earnings, hours worked and the proportion employed is also in the Technical Appendix, see Table A.6. We measure educational attainment with years of education. Since in some countries and datasets the available information is on the highest attained qualification, we convert it into years of education by assuming that each individual requires the customary number of years to complete a degree.[13] The induced measurement error has implications for our estimates which are discussed in the Section devoted to robustness checks.

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[11] As discussed by Pischke and Watcher (2005), for the case of (West) Germany, the following factors may lead to finding no returns to compulsory schooling: (i) measurement errors; (ii) wage rigidity; (iii) the role of apprenticeship; (iv) the heterogeneity of returns, with individuals affected by compulsory schooling being the low-return group; (v) the type of skills learned in school around the time of school leaving age and the relevance of these skills for the labour market. Another reason might be that returns to education depend on the qualification individuals achieve, regardless of whether the issued certification has legal value, or of the actual time spent in full-time education. As Grenet (2004), suggests for France, the actual quantity of education attained is far less important than the qualifications held by individuals in determining these returns (p.30).

[12] The European Community Household Panel data used in this paper are from the December 2003 release (contract 14/99 with the Department of Economics, University of Padova). This paper uses data from SHARE 2004. The SHARE data collection has been primarily funded by the European Commission through the 5th framework programme (project QLK6-CT-2001-00360 in the thematic programme Quality of Life). Additional funding came from the US National Institute on Aging (U01 AG09740-13S2, P01 AGO05842, P01 AGO8291, P30 AG12815, Y1-AG-4553-01 and OGHA 04-064). Data collection in Austria (through the Belgian Science Policy Office) and Switzerland (through BBW/OFES/UFES) was nationally funded. The SHARE data set is introduced in Börsch – Supan *et al.* (2005); methodological details are in Börsch -Supan and Jürges (2005) .

[13] In the ECHP we measure years of schooling with the age when full time education was stopped. This measure in not available for the UK. The conversion of categorical variables into years of schooling is applied to SHARE data and to part of ISSP data. Since the UK is not included in the SHARE dataset and the quality of the ISSP data for that country is rather poor, we have decided to omit it from our sample of European countries. See the Technical Appendix for further details.

We assume that educational attainment does not change after age 25, and restrict our sample to include only individuals aged 26 to 65.[14] The final sample consists of 18328 individuals, and its distribution across the 12 countries is shown in the last column of Table 2, which includes also the sample mean by country of log real earnings, years of schooling, years of compulsory schooling, average age and percentage of males. Educational attainment is highest in Finland (15.15 years) and lowest in Spain (11.05 years). Average age is highest in Sweden (50.41) and lowest in Belgium (33.13), which reflects the different timing of the selected reforms.

Since we intend to identify from the data the causal relationship between the distribution of schooling and the distribution of earnings, we need to control as accurately as possible for additional factors affecting the dependent variable. Therefore, we include in the empirical specification both country and survey dummies, individual age and its square. We also control for country - specific macroeconomic effects by using the first lags of the unemployment rate, aggregate productivity, measured by real GDP per head, and the OECD index of the strictness of employment protection. Because of the existing evidence on gender differences in education and earnings, we estimate gender-specific years of education equations, and allowed a gender dummy to interact with a number of covariates in the earnings equation (as explained below).

Trend-like changes in log wages relative to the time of the reform are controlled with a second order polynomial in $q = t + 7$ – where t is the distance between each cohort and the first cohort potentially affected by the reform - and its interactions with country dummies.[15] Empirical research has shown that individual earnings are significantly affected by the conditions prevailing in the labour market at the time of first labour market entry (see for instance Baker *et al.*, 1994). To capture these effects, we match to each individual the country and gender specific labour participation rate at the age of estimated labour market entry.[16] The underlying idea is that entry wages are likely to be higher when the labour market is tight and labour participation rates are high.

Changes in educational attainment after a compulsory school reform could be due to the reform itself or to confounding factors, which may alter the incentives to invest in education at the time of the reform but independently of it. To illustrate, take a reform that increases the minimum school leaving age from 14

[14] We also exclude individuals with more than 30 years of schooling. We repeated the analysis by considering only individuals who were aged at least 28 at the time of the interview. Results are robust and are not reported for brevity. We prefer not to exclude individuals aged between 26 and 28 since this procedure would lead to drop from the analysis individuals potentially affected by the reforms in some countries, for instance in Spain and Finland.

[15] The relatively low order of the polynomial follows the suggestions by Lee and Card (2008). Compared to higher order polynomials, the second order specification is the most parsimonious and provides adequate fit of the data.

[16] We estimate entry to occur after the completion of schooling. We use a three-years moving average of the macro variables to smooth out measurement errors in the date of labour market entry.

to 15 in a certain year. If individuals at age 14 - or their parents - find it more attractive to invest in education because of a reduction in the opportunity costs generated by a contemporaneous increase in the unemployment rate, they might invest more independently of the reform. Similarly, the actual implementation of school leaving laws may vary across countries and over time with changes in economic conditions. Implementation is known to be more difficult in poorer countries, and, ceteris paribus, in households with a higher number of children. To control for these confounders, we construct two variables, the unemployment rate and the real GDP per head, and match these variables to individuals around the age when the school reform is supposed to have taken place. For instance, assume that the critical age is 14 for Austrian citizens born in 1957. For these individuals the relevant values of the three variables described above are those corresponding to 1971. The inclusion of aggregate income and unemployment at the age when most individuals were in school allows us also to control for the country – specific macroeconomic factors affecting the access to funds and labour market opportunities, which influence the schooling decision by altering the parameter r in equation (5).

5. Empirical Evidence

We start the presentation of our results with the relationship between education and the conditional distribution of wages when education is treated as exogenous. The estimates are shown in Table 3 separately for males and females, and show that the returns to one year of education increase as we move from the lowest to the highest quantile of the distribution. Moreover, returns are consistently higher for females than for males, in line with previous results in the literature – see Harmon *et al.* (2001). Using the 90-10 log wage differential as a measure of conditional wage inequality, we find that this is increased by 2.0% for one additional year of education for males, by 2.4% for females.

Education, however, cannot be treated as exogenous in the presence of unobserved ability. Table 4 presents the results of the first stage regression of years of education against all the exogenous controls plus the instrument *ycomp*. In most cases this variable attracts a statistically significant and positive coefficient, with the exception of the top decile of the distribution of ability for males. Using the Stock and Staiger rule of thumb, which suggests that when there is a single endogenous variable the selected instrument is weak if the F – test for its inclusion in the auxiliary regression is lower than 10, we find evidence of weakness for the two top deciles in the case of males and for the top decile in the case of females. Furthermore, the size of the effect of compulsory school leaving laws on attained education is much larger for the lowest quantile of the distribution of ability.

In all specifications we include the following controls: individual age and its square, country effects, a quadratic polynomial in q and its interactions with country dummies, and macroeconomic variables which include employment protection, GDP per head and the unemployment rate at the time earnings are observed, the gender specific labour force participation rate at the estimated time of labour market entry, and the unemployment rate and GDP per capita around the age when minimum school reforms are implemented. We use F – tests to verify the joint hypothesis that the included covariates are jointly equal to zero, and always reject the null. We interpret this as evidence that schooling and estimated ability are indeed two different variables.

These findings suggest that compulsory schooling laws are particularly effective at the lower tail of the distribution of ability: for individuals located below the 10th quantile, a one year increase in compulsory education increases actual attainment by 0.40 years in the case of females and by 0.30 years in the case of males, which compares to 0.10 years for individuals with median ability. These results are in line with expectations, which suggest that the bulk of compliers should be among the less able (and wealthy). While for males the impact of school reforms on educational attainment tends to die out at the upper decile of the conditional distribution of ability, for females the effect remains statistically significant above median ability.[17] One explanation is that – in conformity with the predictions of the signalling model, better educated individuals, especially females, react to the increase in the minimum school leaving age by upgrading their own education, in an effort to maintain at least in part their relative advantage over the less educated. An additional factor at play could be that since in some countries included in the sample the average educational attainment was very low before compulsory attendance reforms, a large fraction of the population may have been "caught up" by the increase in the minimum school leaving age.[18]

We apply the control variate approach due to Ma and Koenker (2006), and estimate the full matrix of quantile treatment effects, which describe the impact of education on earnings for different quantiles of the distribution of ability and labour market fortune. We focus for brevity on the following quantiles: 0.10, 0.30, 0.50, 0.70 and 0.90 and proceed as follows: first, we run separately for males and females quantile regressions of (9) and compute the control variate for each selected quantile of the conditional distribution of ability. Second, we augment (8) with each estimated control variate and estimate separate quantile regressions by pooling all data and allowing for the full set of interaction of the explanatory variables with the gender dummy. For each regression, we test whether blocs of interactions with gender are statistically significant, and simplify the empirical specification by dropping those blocs which fail to pass the test. Last, we estimate simultaneous quantile regressions for each quantile of the distribution of ability, using the parsimonious specification, and obtain the estimate of the variance–covariance matrix by

[17] When we examine the distribution of educational attainment in the three years before and after the reforms, we notice that the combined reduction in the frequency of low attainment and increase in the frequency of higher than average attainment is more pronounced for females than for males.

[18] More than a quarter of the Spaniards and Greeks and close to one fifth of Italians in our sample had less than 8 years of education before compulsory reforms were enacted which raised minimum school leaving age at 8 or above it.

bootstrapping.[19] In all cases, we never reject the hypothesis that the control variate and its interaction with schooling are significantly different from zero, and that education is endogenous in the wage regressions.

The estimated percentage increase in log earnings associated to one additional year of education and its standard error are reported separately for males and females in Table 5 for the selected quantiles of the distribution of ability a (τ_a) and labour market fortune u (τ_u). The final row in each section of the table is obtained by integrating the quantile treatment effects with respect to ability, which produces the mean quantile treatment effect. If we compare this effect to the one estimated by treating education as exogenous, we notice that the former is higher than the latter, and that the gap is larger for the lowest decile of the distribution of the random term *u*.

To interpret our findings, select for instance the bottom decile of the distribution of ability, $\tau_a = 0.10$. We find that the estimated returns to education for males are equal to 7.48% for the individuals at the bottom decile of the distribution of earnings, to 5.55% for the individuals at the median decile and to 5.98% for individuals at the top decile. The corresponding returns for females are higher but follow a similar pattern: the profile of estimated returns is broadly declining, albeit not always monotonically, as we move from the lowest to the highest decile of the distribution of labour market fortune, and independently of the selected decile of the distribution of ability.

Table 6 presents tests for equal coefficients for each selected decile of the distribution of ability. Differences in these parameters can be interpreted as measures of education-induced conditional inequality: one should remember, however, that coefficients on conditioning variables also vary across deciles, thus conditional inequality will also reflect these differences. With this caveat in mind, we can say that education-related conditional inequality declines with additional education, and significantly so – in a statistical sense – with the exclusion of the index involving the highest decile. When we average returns across the distribution of abilities, we find that the reduction in conditional inequality induced by an additional year of education is close to 1.5 percentage points. With constant marginal returns, this corresponds to about 5 percentage points for a three – years degree.

In summary, our estimates point to a relationship of substitutability between schooling *s* and the random effect *u*: individuals who are less fortunate in the labour market can partially compensate for poor

[19] To facilitate the convergence of the estimates in the bootstrapping exercise in the presence of a large number of regressors, we operate as follows: define the relevant regression as $Q(y) = \Gamma X + \Omega Y$, where **X** and **Y** are suitable matrices of variables. We first estimate each quantile regression separately and compute the index $\overline{\Gamma} X$. Next, we redefine the dependent variable as $Q(y) - \overline{\Gamma} X$ and estimate quantile regressions for different quantiles of the distribution of wages simultaneously using STATA built in functions. In practice, the vector X includes two survey dummies, the polynomial in the second order in *q* and its interactions with country dummies.

luck with higher returns from investment in education. Therefore, when poor luck depends on circumstances beyond individual control, education policies targeted at the less fortunate which encourage additional schooling contribute to reducing conditional inequality.

Next consider again Table 5 but select a decile in the distribution of the random error u. Independently of gender and of the selected decile, there is evidence that the returns to education fall as we move from the bottom to the top decile of the distribution of ability. This qualitative finding remains even if we disregard the two top deciles, because of the weakness of our instrument, especially in the case of males. To illustrate, returns to education fall from 7.88% to 5.88% when we select the median decile in the distribution of error u, focus on females and on the 10th and 70th decile of the distribution of ability.

We interpret this as evidence that ability and education are substitutes in the production of human capital. When we increase education by an additional year, individuals located in the upper part of the distribution of ability gain less compared to individuals with less than median ability. Since ability and parental background are closely intertwined - see Cuhna *et al.* (2005) - our results point to the fact that those better endowed have less to gain from additional education. The size of the gap is again not small, and is equal at most to slightly more than 3 percentage points.

Since earnings in our model depend both on ability a and on the random error u, an alternative reading of Table 5 is along the main diagonal: as we move from the upper left to the bottom right corner, we consider individuals who are increasingly endowed in both ability and labour market fortune. Independently of gender, our estimates suggest that the better endowed have lower returns to investment in education.

Overall, our findings confirm the results by Ashenfelter and Rouse (1998), based on a sample of American twins. Using a different methodology – quantile regressions – and a sample of European individuals, we find – as they do - that better endowed individuals have lower returns to education. The fact that better endowed individuals typically have higher attained education but lower marginal benefits of schooling point either to lower costs of funds or to a negative relationship between the marginal costs of schooling and individual ability, as in equation (2).

Table 5 can also be used to calculate the corresponding structural parameters β, ϕ and λ, according to equation (10). All we have to do is to compute the empirical c.d.f.'s of the error terms, a and u, and invert them to obtain estimates of ${G_a}^{-1}(\tau_a)$ and ${G_u}^{-1}(\tau_u)$. Then from the estimated values of $\Pi(\tau_a, \tau_u)$ one can estimate β, ϕ and λ by GLS. GLS in this context is an optimal minimum distance estimator, but requires estimating the variance-covariance matrix of the whole $\Pi(\tau_a, \tau_u)$ matrix, something that is beyond the scope of this paper. Card and Krueger (1992) show that almost identical estimates of both coefficients and standard errors obtain if a weighted least squares estimator (WLS) is used instead of GLS (that is, if observations are weighed inversely to the estimated variance of the dependent variable, and covariance terms are ignored).

We find that the WLS estimated value of β is 0.051 for males and 0.070 for females (see Table 7). Furthermore, λ is negative, statistically significant and in the range -0.0021 / -0.0025, depending on gender. Finally, estimated ϕ is also negative, and in the range -0.0089 / -0.0119.[20] These estimates confirm that both unobserved ability a and labour market luck u are substitutes to education in the production of human capital and earnings.

The policy implications of our results are important. First, suppose that earnings and productivity are closely related, a plausible assumption. Then education policies aimed at raising the educational attainment of the less fortunate and talented are grounded on equity considerations if less lucky individuals are so because of circumstances outside their own control. Equity, however, trades off with efficiency if the marginal returns to schooling are higher for the more talented, because this group has lower marginal costs of education. Our findings that labour market returns to schooling are highest among the less fortunate and talented has the important implication that equity and efficiency need not trade-off, because these higher returns can more than compensate the higher schooling costs.

To illustrate, consider a lump sum fixed subsidy paid to the less privileged that improves access to schooling, funded by a lump sum education tax on the more privileged. Since ability and parental background are strongly correlated, such policy increases equality of opportunity by raising the educational attainment of the less fortunate and reducing that of the more fortunate. If ability and schooling are substitutes, an implication of this policy is that the gain in marginal benefits for the less talented more than compensates the loss for the more talented. Whether this translates into higher efficiency is more difficult to gauge, and requires a careful cost-benefit analysis that takes into account both the monetary cost of such reforms and the potentially important general equilibrium effects that could arise if compulsory attendance laws were designed to increase the educational attainment of a large fraction of the population.[21]

Second, the uncovered substitutability between education and ability has implications for the design of an optimal education policy. Such policy should be elitist and favour brighter children only if education and talent are complements in the production of human capital. When this is not the case, as our results suggest, redistributive education that targets the less able and fortunate can pay off both on equity and efficiency grounds. Before drawing too strong conclusions, however, one needs to remember that our

[20] These results do not significantly change when we omit the two top quantiles of the distribution of ability – see columns (2) and (4) in Table 7. The estimates of ϕ suggest that the constraint 1+ ϕs is always verified.

[21] For instance, a large shift in the labour supply of educated workers could reduce the wages of the low-skilled workers who were educated before the enforcement of the new compulsory schooling level.

results show that less well-endowed individuals have higher returns to *compulsory* education than the better endowed. This does not necessarily mean that they would enjoy larger returns for higher levels of education, since the instrumental variable strategy does not allow for identification of the returns to schooling beyond the minimum school leaving age. In order to conclude that elitist policies are not efficient in general, one would need also to show that high-ability individuals have lower returns to post-compulsory education than low-ability individuals.

Last but not least, individual ability is strongly affected both by genetic and by environmental factors (see Cunha and Heckman, 2007). If education and ability are substitutes in the production of human capital and earnings, then additional investment in the former can contribute to reducing the differences produced by the latter.

6. Robustness checks

Since log hourly wages are only available for employees, our sample is the result of a selection process involving the decision to participate to the labour market and having an employee job. Unless we take this selection process explicitly into account, the error term in equation (6) is unlikely to have zero mean. More important for our purposes is the concern that selection into employment may be affected by the number of years of compulsory education. If this was the case, the validity of our instrument would fail to hold.

We investigate this by defining B as a dummy variable equal to 1 if log earnings are observed and to 0 otherwise. Failure to observe wages could be due to the participation decision, to the choice between employment, unemployment and self-employment or to the presence of missing wage data. We estimate a probit model for variable B using all the controls described above plus the predicted years of schooling from the first stage regression of years of schooling on compulsory years of education. If the latter affected the selection process, we would expect that predicted years attract a statistically significant coefficient. It turns out that the estimated coefficient is equal to 0.023 for males, with a bootstrapped standard error equal to 0.076 (p-value: 0.300), and to 0.088 for females (standard error: 0.055 and p-value: 0.107). Therefore, there is no evidence in our data supporting the view that the years of compulsory education are significantly associated to the endogenous selection of workers into paid employment, and we conclude that our instrument is not invalidated by failure to explicitly consider such selection.

An important assumption of model (6)-(7) is that it contains no more than two latent variables, ability and luck (no excess variation). A first issue here is measurement error, that is particularly likely to occur when years of education are indirectly predicted using the information on the highest qualification, as it occasionally happens in this paper. This is not the case, however, with the ECHP data, because years of education are computed there by using the information on the age when full time education was stopped. We re-estimate our model for the ECHP sub-sample, which does not include valid information for France, the Netherlands and Sweden, and check whether our key qualitative results are preserved. As shown in Table A.7 in the Technical Appendix, the findings that ability and education are substitutes and that the better endowed have lower returns to schooling still hold.[22]

A second issue is whether ability is the sole source of unobserved heterogeneity in schooling decisions. Many factors other than individual ability may indeed explain differences in educational attainment: family background, credit constraints and different labour market opportunities. Partly, the last two factors are affected by differences in country – specific macroeconomic conditions, and we control for these with the unemployment rate and the GDP per capita at schooling age. Unfortunately, our data do not allow us to construct fully satisfactory measures of parental background and credit constraints at the individual level. In an attempt to gauge the importance of omitting these controls, we use information on co-habitation patterns in the ECHP to retrieve one such measure, the education of parents – a dummy equal to 1 if at least one parent has ISCED 3 education or higher. Given the well known correlation between education and income, this measure is also a good proxy of individual access to funds. Due to the numerous missing values, we are forced to restrict our sample to four countries, Italy, Spain, Greece and Belgium. If parental background does explain a substantial part of unobserved heterogeneity, we would expect that omitting it from the first stage quantile regressions significantly affects the distribution of residuals. Yet we find that the correlation of residuals with and without controlling for parental background is never smaller than 93% and that the two distributions closely track each other.[23]

A key assumption in this paper is that we can treat the pooled data from multiple countries as one population and therefore treat the timing of the natural experiment in different countries as regional variation in the timing in the same way as US researchers would use state–by–state variation in implementation. One aspect of this assumption is that the conditional impact of school reforms on education and earnings does not vary within the sample of 12 countries under study. To verify this, we start with the ECHP sub-sample of 9 countries, as we have already shown that omitting France, the Netherlands and Sweden does not affect our qualitative results in a significant way. Next, we rank countries according to the average educational attainment of individuals aged between 30 and 50 and classify them in two groups, a "low education" group – composed of Spain, Italy, Austria and Ireland – and a "high education" group – consisting of Germany, Finland, Belgium and Denmark. Finally, we

[22] Arias et al (2001), also discuss the effects of measurement errors on their estimates and conclude that failure to account for these errors seems to create slight downward biases in the estimates of the returns to schooling only at lower quantiles, which are stronger in models that control for family effects in school attainment.

[23] Not reported here but available from the authors upon request are the estimates of parental background, which always attract a statistically significant coefficient in the first stage regressions.

interact years of schooling and the first stage residuals with a dummy equal to 1 for the "high education" group and to 0 for the other group, and test whether these interactions are jointly different from zero. Table A.8 in the Appendix reports the p-values of the tests: with a few exceptions in the case of females, the general pattern is that we cannot reject the hypothesis that the estimated returns are the same across the two groups of countries.

Next, notice that Chesher's model requires that outcomes and covariates exhibit continuous variation. In the specification adopted in this paper, wages are indeed continuous but the schooling variable and the instruments are not. We investigate whether this problem affects our estimates with the help of Monte Carlo simulations, and find the effects to be negligible (see Table A.9 in the Technical Appendix for details). Finally, we check whether our findings are sensitive to the selected bandwidth and find that they are not.

7. Conclusions

In this paper, we have used data from twelve European countries and the variation across countries and over time in the changes of minimum school leaving age to study the effects of the quantity of education on the distribution of earnings. We have treated the countries of Europe as regions of a single country, and country specific compulsory school reforms as episodes of a broad European reform, which has taken place in each region at a different point in time. By so doing, we have been able to generate the country and time variation that was absent in previous European research (see Harmon and Walker, 1995). Confirming that there are no free lunches, we have made this progress at the price of pooling together data from three fairly heterogeneous surveys, which is likely to lower the precision of our estimates.

There are three main results: first, we find evidence that additional schooling reduces conditional wage dispersion. Second, compulsory school reforms affect mainly the individuals at the lower end of the distribution of educational attainment: for these individuals one additional year of compulsory education is estimated to translate into 0.30 to 0.40 years of additional education or higher. This figure falls to 0.10 or below for the rest of the population. Third, there is evidence that education and ability are substitutes in the generation of earnings.

We believe that the policy implications of our results are rather clear. First, compulsory schooling law reforms have had a rather pervasive effect, highest among the less talented but present also – albeit to a substantially lower extent - among the better endowed, especially females. Second, the pursuit of an elitist education policy because of efficiency considerations is open to question, because the basic assumption of complementarity between education and ability fails to hold for the individuals affected by changes in the minimum school leaving age. Third, education policies which focus on the equality of opportunity for the less fortunate and less talented can be justified not only on equity but also on efficiency grounds if the additional benefits – which we have shown to be higher than for the more talented – are sufficiently large to offset the additional costs and the potentially important general equilibrium effects.

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Table 1. Selected compulsory school reforms, by country.

	Reform	*First cohort potentially affected*	*Change in min. school leaving age*	*Change in years of comp. school.*	*Expected change in ISCED*	*Age at school entry at the time of the reform*
Austria	1962	1947	14 to 15	8 to 9	to ISCED 2	6
Belgium	1983	1969	14 to 18	8 to 12	to ISCED 3	6
Denmark	1971	1957	14 to 16	7 to 9	to ISCED 3	7
Finland (Uusima)	1977	1966+	13 to 16	6 to 9	to ISCED 3	7
Finland (Etela-Suomi)	1976	1965+	13 to 16	6 to 9	to ISCED 3	7
Finland (Ita-Suomi)	1974	1963+	13 to 16	6 to 9	to ISCED 3	7
Finland (Vali-Suomi)	1973	1962+	13 to 16	6 to 9	to ISCED 3	7
Finland (Pohjois-Suomi)	1972	1961+	13 to 16	6 to 9	to ISCED 3	7
France	1959++	1953	14 to 16	8 to 10	to ISCED 3	6
Germany(Schleswig-Holstein)	1956	1941	14 to 15	8 to 9	to ISCED 3	6
Germany(Hamburg)	1949	1934	14 to 15	8 to 9	to ISCED 3	6
Germany(Niedersachsen)	1962	1947	14 to 15	8 to 9	to ISCED 3	6
Germany(Bremen)	1958	1943	14 to 15	8 to 9	to ISCED 3	6
Germany(Nordrhein-Westphalia)	1967	1953	14 to 15	8 to 9	to ISCED 3	6
Germany(Hessen)	1967	1953	14 to 15	8 to 9	to ISCED 3	6
Germany(Rheinland-Pfalz)	1967	1953	14 to 15	8 to 9	to ISCED 3	6
Germany(Baden-Wurtenberg)	1967	1953	14 to 15	8 to 9	to ISCED 3	6
Germany(Bayern)	1969	1955	14 to 15	8 to 9	to ISCED 3	6
Germany(Saarland)	1964	1949	14 to 15	8 to 9	to ISCED 3	6
Greece	1975	1963	12 to 15	6 to 9	to ISCED 2	6
Ireland	1972	1958	14 to 15	8 to 9	to ISCED 3	6
Italy	1963	1949	11 to 14	5 to 9	to ISCED 2	6
Netherlands	1975°	1959°	15 to 16	9 to 10	to ISCED 2	6
Spain	1970	1957*	12 to 14	6 to 8	to ISCED 2	6
Sweden	1962	1950**	14/15 to 15/16	8 to 9	to ISCED 3	6 or 7

+ Pekkarinnen (2005) p.5 and his elaborations provided for this paper. ++ Reform implemented in 1967, see Grenet (2004). ° Reform implemented in 1973, see Oosterbeek *et al.* (2004). * Pons and Gonzalo (2002), p.753 and Table A.1 p.767. ** Personal communication with Martin Palme.

Table 2. Means of the Key Variables. Sample Size: 18328

	Log w	*s*	*ycomp*	*age*	*% Males*	*Nobs*
Austria	2.220	12.181	8.767	50.900	0.492	920
Belgium	2.470	14.887	9.782	33.125	0.465	853
Denmark	2.798	13.667	8.030	44.186	0.477	2235
Finland	2.366	15.133	7.511	37.151	0.496	1409
France	2.399	13.410	9.017	47.074	0.525	1293
Germany	2.439	12.127	8.620	45.649	0.590	1690
Greece	2.005	12.929	7.509	38.270	0.562	984
Ireland	2.265	12.356	8.534	39.330	0.574	1260
Italy	2.367	14.166	7.097	49.066	0.590	1762
Netherlands	2.574	11.049	9.445	37.702	0.592	1294
Spain	2.116	11.049	7.099	43.136	0.626	2284
Sweden	2.328	12.197	8.465	50.410	0.480	2344

Table 3. Quantile effects when education is treated as exogenous. Sample size: 18328. By gender (9936 males and 8392 females)

	$\tau = 0.10$	$\tau = 0.30$	$\tau = 0.50$	$\tau = 0.70$	$\tau = 0.90$
Males	0.019*** (0.002)	0.026*** (0.001)	0.033*** (0.001)	0.035*** (0.001)	0.039*** (0.002)
Females	0.027*** (0.003)	0.037*** (0.001)	0.043*** (0.001)	0.050*** (0.001)	0.051*** (0.002)

Note: each regression included a constant, country dummies, q, q^2 and their interaction with country dummies, survey dummies, age, age squared, a gender dummy, the lagged country specific unemployment rate and GDP per capita, the country and gender specific labour force participation rate at the estimated time of labour market entry, the country specific GDP per head and unemployment rate at the age affected by the country specific reform. Details on these coefficients are available from the authors upon request. τ denotes the quantile of the distribution of wages. Three stars, two stars and one star for statistically significant coefficients at the 1%, 5% and 10% confidence level. Robust standard errors within parentheses.

Table 4. First Stage Effect of *ycomp* on *s*. Sample size:18328

Males	$\tau_a = 0.10$	$\tau_a = 0.30$	$\tau_a = 0.50$	$\tau_a = 0.70$	$\tau_a = 0.90$
Coeff. (s.e.)	0.354*** (0.007)	0.056*** (0.012)	0.120*** (0.006)	0.078*** (0.035)	0.026 (0.071)
F-test (p-value)	2146.6 (0.000)	19.1 (0.000)	307.6 (0.000)	4.86 (.027)	0.13 (0.714)
Females	$\tau_a = 0.10$	$\tau_a = 0.30$	$\tau_a = 0.50$	$\tau_a = 0.70$	$\tau_a = 0.90$
Coeff. (s.e.)	0.416*** (0.016)	0.284*** (0.020)	0.072*** (0.007)	0.219*** (0.029)	0.135*** (0.065)
F-test (p-value)	643.8 (0.000)	195.4 (0.000)	88.7 (0.000)	57.4 (0.000)	4.26 (0.039)

Note: see Table 3. τ_a denotes the quantile of the distribution of ability.

Table 5. Heterogeneous returns to schooling. Quantile treatment effects. Sample size: 18328

Males	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.0748***	0.0583***	0.0555***	0.0550***	0.0598***
	0.004	0.004	0.003	0.004	0.006
$\tau_a = 0.30$	0.0625***	0.0476***	0.0462***	0.0420***	0.0503***
	0.007	0.004	0.003	0.005	0.006
$\tau_a = 0.50$	0.0665***	0.0492***	0.0478***	0.0432***	0.0469***
	0.006	0.004	0.004	0.004	0.006
$\tau_a = 0.70$	0.0486***	0.0396***	0.0448***	0.0411***	0.0471***
	0.006	0.004	0.004	0.004	0.005
$\tau_a = 0.90$	0.0468***	0.0329***	0.0384***	0.0332***	0.0452***
	0.006	0.004	0.003	0.004	0.006
Mean effect+	0.0598	0.0456	0.0465	0.0429	0.0499
Females	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.0952***	0.0780***	0.0788***	0.0820***	0.0759***
	0.007	0.004	0.004	0.005	0.007
$\tau_a = 0.30$	0.0838***	0.0701***	0.0713***	0.0730***	0.0702***
	0.007	0.003	0.003	0.004	0.006
$\tau_a = 0.50$	0.0847***	0.0679***	0.0690***	0.0707***	0.0646***
	0.006	0.004	0.003	0.004	0.006
$\tau_a = 0.70$	0.0689***	0.0573***	0.0588***	0.0615***	0.0612***
	0.005	0.003	0.003	0.003	0.005
$\tau_a = 0.90$	0.0631***	0.0502***	0.0527***	0.0555***	0.0567***
	0.006	0.003	0.003	0.004	0.006
Mean effect+	0.0792	0.0645	0.0655	0.0655	0.0674

Note: see Table 3; more details in the text. τ_u denotes the quantile of the distribution of labour market fortune and τ_a denotes the quantile of the distribution of ability. Bootstrapped standard errors (100 replications) in small characters. + Mean effect: average (over τ_a) quantile treatment effect.

Table 6. The effect of a marginal increase in schooling on the conditional log wage differential

Males	$\delta_{30}-\delta_{10}$	$\delta_{50}-\delta_{10}$	$\delta_{70}-\delta_{10}$	$\delta_{90}-\delta_{10}$
$\tau_a = 0.10$	-0.0165	-0.0193	-0.0198**	-0.0150
$\tau_a = 0.30$	-0.0149**	-0.0163**	-0.0266**	-0.0122
$\tau_a = 0.50$	-0.0172**	-0.0187***	-0.0233***	-0.0196**
$\tau_a = 0.70$	-0.0090*	-0.0038	-0.0075	-0.0015
$\tau_a = 0.90$	-0.0139**	-0.0084	-0.0136*	-0.0016
Mean effect	-0.0143	-0.0133	-0.0169	-0.0099
Females	$\delta_{30}-\delta_{10}$	$\delta_{50}-\delta_{10}$	$\delta_{70}-\delta_{10}$	$\delta_{90}-\delta_{10}$
$\tau_a = 0.10$	-0.0172***	-0.0164***	-0.0132*	-0.0193**
$\tau_a = 0.30$	-0.0137**	-0.0125**	-0.0108	-0.0136
$\tau_a = 0.50$	-0.0168***	-0.0158**	-0.0140*	-0.0201**
$\tau_a = 0.70$	-0.0116**	-0.0101**	-0.0074	-0.0077
$\tau_a = 0.90$	-0.0129**	-0.0104	-0.0076*	-0.0064
Mean effect	-0.0144	-0.0130	-0.0106	-0.0134

Note: see Tables 3 and 5. Legend $\delta_{\tau 2} - \delta_{\tau 1} = \frac{\partial Q_Y(\tau_2, X, s, a)}{\partial s} - \frac{\partial Q_Y(\tau_1, X, s, a)}{\partial s}$

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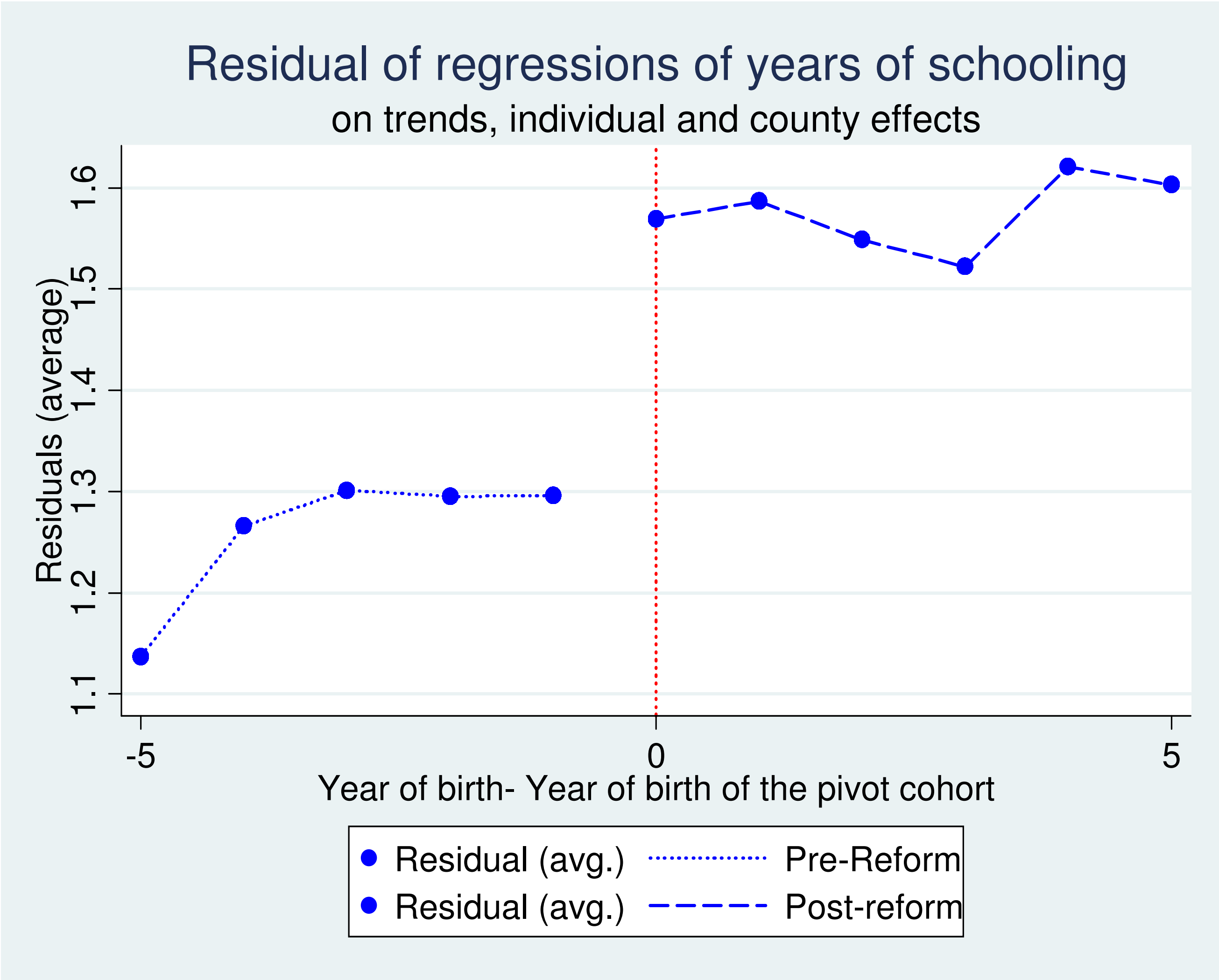
Table 7. Estimates of β, λ and ϕ

	Males	*Males*	*Females*	*Females*
	(1)	(2)	(3)	(4)
β	0.051***	0.050***	0.070***	0.072***
	0.0015	0.0026	0.0009	0.0013
λ	-0.0021***	-0.0022***	-0.0025***	-0.0021***
	0.0004	0.008	0.003	0.0016
ϕ	-0.0089***	-0.013***	-0.0091***	-0.0119***
	0.003	0.0032	0.0017	0.0016
Observations	25	15	25	15
R squared	0.680	0.692	0.856	0.836

Note: columns (1) and (3) are estimates based on the 25 estimated returns in Table 5 (by gender). Columns (2) and (4) are based instead on excluding the bottom two rows of Table 5 and retaining 15 estimated returns (by gender). The regressors $G_a^{-1}(\tau_a)$ and $G_u^{-1}(\tau_u)$ are the estimated deciles of the empirical distributions of the first stage and second stage residuals respectively.

Fig. 1. The effect of school reforms on educational attainment



Note: the OLS regression included a constant, country dummies, q, q^2 and their interaction with country dummies, survey dummies, age, age squared, a gender dummy, the lagged country specific unemployment rate and GDP per capita, the country and gender specific labour force participation rate at the estimated time of labour market entry, the country specific GDP per head and unemployment rate at the age affected by the country specific reform.

Technical Appendix to 'Changes in compulsory schooling, education and the distribution of wages in Europe' by Brunello *et al.*

A. The Educational Reforms used in this Study

In this section we provide a brief description of the educational reforms considered in the study. Furthermore, we motivate the choice of the first cohort potentially affected. We devote a paragraph to each country considered. Further details on country specific education systems and reforms are in Fort (2006).

Austria

The 1962 School Amendment Act increased compulsory schooling by one year, from 8 to 9 years. Pupils who were 14 years old or younger at the time the reform was introduced were compelled to attend an additional year of schooling. This suggests that the individuals potentially affected by the reform are those born in 1948 and afterwards. However, individuals born in 1947 who might have already left school when the reform was introduced were required to go back to school and complete the additional year. Therefore, we select the cohort born in 1947 as the first cohort potentially affected by the reform.

Belgium

In 1983 (Law of 28 June 1983), the length of compulsory schooling was increased to 18 years (from 8 to 12 compulsory years of education), which could be completed with part time scholling during the final three years. Student potentially affected by the reform were those aged 14 or younger in 1983, i.e. those born after 1969.

Denmark

Two major reforms of compulsory schooling took place in Denmark in 1958 and 1971. In 1958 compulsory schooling years were increased by 3 years (from 4 to 7) and in 1971 they were further increased by additional two years (from 7 to 9). Pupils who were 14 years (or younger) in 1971 were potentially affected by the 1971 reform. We only consider this reform in this study.

Finland

The relevant reform considered in this study took place during the 1970s. The reform introduced a new curriculum and changed the structure of the educational system, increasing compulsory education from 6 to 9 years. The reform was adopted gradually by Finnish municipalities. Since we do not have access to data at the municipality level, we define the year of the reform in each area as the year when the largest share of municipalities in the area experienced the change in the schooling legislation, as reported in Table A.1 below. Following Pekkarinen (2005), we consider the cohort aged 11 when the reform was implemented as the first cohort potentially affected.

France
During the XXth century, compulsory schooling age in France was extended twice: from 13 to 14 in 1936 and from 14 to 16 in 1959 (Bethoin Reform). The 1936 reform affected mainly pupils born after 1923, whereas the 1959 reform - which was implemented from 1967 after a long transition period - affected individuals who were born from 1953 onwards (see Grenet (2004)).

Germany
The peculiar political situation of the country produced the separate evolution of two distinct education systems between 1949 and 1990. We refer to reader to Pischke et al. (2005), and Pischke (2003), for a description of the compulsory school reforms and for the selection of the first cohort potentially affected in each German Länder.

Greece
In 1975 the Greek Parliament increased compulsory education by three years (from 6 to 9). Individuals potentially affected by this change are those who were 12 in 1975. In particular, those born 1963 and later were compelled to attend 3 additional years of schooling, whereas those born in 1962 were not.

Ireland
Compulsory schooling was modified in 1972, when the school leaving age was raised to the age 15. A further raise in compulsory schooling age (to 16 years) announced in 1998, came into effect when the Education (Welfare) Act (2000) became law. Individuals potentially affected by the 1972 reform are the individuals who were 14 in 1972. These individuals where compelled to attend an additional year of schooling, whereas individuals who were 14 in 1971 were not. Therefore we choose 1958 as the first cohort potentially affected.

Italy
Junior high school became effectively compulsory in Italy only since 1963. Compliance with the 1963 reform was not instantaneous: only in 1976 the proportion of children attending junior high school approached 100%. According to Brandolini and Cipollone (2002), the individuals potentially affected by the reform were those born after 1949.

The Netherlands
From 1975 onwards, all three-year educational programs in the Netherlands were extended to four years and compulsory schooling leaving age was increased by one year, from 15 to 16. Oosterbeek et al. (2004), highlight that the implementation of the extension of lower vocational education started in 1973. Since August 1 1973 all lower vocational programs had a lenght of four year. Students who were behind the second year could still graduate in a three-year course. Students who started a three-year course of lower vocational education on August

1 1971 could still graduate in 1974. All the following cohorts had to take a four-year course. According to Oosterbeek et al. (2004) '..students born on or after August 1, 1959 had to follow ten years of full time education. This is also the first cohort of students to encounter a complete four-year lower vocational education regime'.

Spain

The compulsory school reform considered in this study was carried out under the 1970 General Act on Education and Financing of Educational Reform (LGE), and increased compulsory years of education from 6 to 8. Individuals potentially affected by the reform were those born in 1957 and after (see Pons and Gonzalo (2003), p.753 and Table A1 p.767).

Sweden

According to Meghir and Palme (2005, 2003), compulsory school reform in Sweden was gradually implemented between 1949 and 1962. The take-up of the experiment varied over the period 1949-1962 across municipalities with the largest number of municipalities involved in the years 1961/1962 (39.4%; 18,665 classes; 436,595 students). It was fully implemented only in 1962. Unfortunately, we do not have access to data at the municipality level but only at the county level. For the purposes of this paper, and based on personal communication with Marten Palme, we considered as potentially affected by the reform all the individuals born after 1950.

B. The Construction of the Key Variables

In this section we provide further details on the construction of the key variables used in the empirical analysis.

Education

The ISSP survey generally includes information on the highest qualification attained at the time of the interview and on the number of years spent at school. When the latter variable is missing, years of education are computed using the information on the highest qualification attained and/or the age at which the individual finished his/her studies.
The SHARE survey collects information on the highest attained qualification and generates a variable corresponding to years of education (see the survey documentation at http://www.share-project.org/ for further details). Finally, the ECHP survey collects information on both years of education, age at which the individual finished his/her studies and the highest attained qualification. We use the variable pt024 in this dataset (see the User Manual for more details).

Wages

The heterogeneity across surveys increases as one examines earnings, labour force status, and hours worked. Since earnings in the ISSP surveys are recorded on a categorical scale, with the number of categories varying across countries and surveys, we use mid-points of each category. The ECHP data include information both on net and on gross monthly earnings, and we use the latter. Finally, the SHARE survey collects information only on gross yearly earnings . We transform the available information on earnings from the three surveys on a monthly basis, using 2000 consumer prices and PPP units. Depending on the survey, data on working hours are: total hours worked per week in the main job (SHARE); total hours - including paid overtime - worked per week in the main job or business (ECHP); total number of hours worked per week in the main job (ISSP). In the 1997 ISSP survey hours are reported on a categorical scale, and we take mid-points. Finally, information on employment status is: self reported current employment status (SHARE); self-reported current employment status (ECHP); self -reported economic position (ISSP). Table A.6 presents detailed information on the earnings, hours, proportion employed and proportion of females by country and survey.

Additional variables

The aggregate variables used in the estimates are: - labour force, population and unemployment: ILO Labor Force Statistics, www.laborsta.org. - completed fertility rate: Eurostat online statistics - gdp per head in 1990 international dollars: Maddison (2007). We use linear interpolation to replace the few missing values in each of these variables.

Table A.1. The distribution of individuals born in 1960-66 across the major regions of Finland and the year of adoption of the comprehensive school reform.

	Adoption of the comprehensive school reform					
Major Regions	1972	1973	1974	1975	1976	1977
Uusima	0.00	1.15	0.00	6.19	25.16	67.50
Etela-Suomi	4.26	6.65	12.22	28.36	48.51	0.00
Ita-Suomi	1.07	31.13	37.69	30.11	0.00	0.00
Vali-Suomi	4.79	37.61	26.14	31.45	0.00	0.00
Pohjois-Suomi	52.43	5.40	42.17	0.00	0.00	0.00

Note: we thank Tuomas Pekkarinnen for kindly providing this table. The table is based on data from the Finnish Longitudinal Census Data Files ($FLCD$, Statistics Finland, years 1970,1975,1980,1985,1990,1995 and 2000).

Table A.2. Effect of school reforms on educational attainment across European countries: evidence from the literature. Countries: Denmark, Finland, France, Germany (West), Italy, Ireland, Netherlands, Sweden.

Country	Reform	Effect on Ed. Attainment	Data and References
Denmark	1958	+0.35(women)-+0.4(men) years of education controlling for trend	Danish National Work Environment Cohort ($WECS$) Study, 1990,1995, see Arendt (2005)
Finland	1972-1977	+0.36 in gender gap	Finnish Longitudinal Census Data ($FLCD$) yrs 1970,1975,) 1980,1985,1990,1995,2000) see Pekkarinnen (2005)
France	1957	nearly +0.34 years of schooling using control group (effect varies according to parental background) -20% drop-out rates among farm-workers' sons	French National Labour Force Survey see Grenet (2004)
Germany (West)	1947-1969	+ 0.28% years of schooling (applies to students in the basic track)	Qualification and Career Survey (QaC), MicroCensus, social security records (1% sample) period 1975-1995, see Pischke and Watcher (2005)
Italy	1963	+2.1% enrolment in 8^{th} grade +0.21 years of schooling +38%(women), +12% men prop. of those achieving high school degree +3%-6% proportion of women achieving exactly junior high school degree	Annual Report on Schooling (1948-1979) Labor Force Survey (October 1992-1997) see Brandolini and Cipollone (2002) Survey on Household Income and Wealth, 1991, see Flabbi (1999) Census data (1981, 1991) see Fort (2007)
Ireland	1967 (Fees Abolition)	(-0.1)- (-1.8) years of schooling	see Denny and Harmon (2000)
Netherlands	1968	+0.71 years of schooling (males) + 1.33 years of schooling (females)	OSA- Labour market Survey (1985,1986, 1988,1990,1992 and 1994) see Plug (2001)
Sweden	1962	+10% (males), +8% (females) prop. of those achieving jun. high sch. +0.27(males), +0.22(females) years of schooling (*via* propensity score match.) effect varies with ability level	Individual Statistics project data merged with administrative data, 1985-1996 see Meghir and Palme (2003)
Sweden	1962	+10% (males), +8% (females) prop. of those achieving jun. high sch. +0.25(males), +0.34(females) years of schooling (average across ability levels and parental background but the effect varies with parental background and with individuals ability level (IQ level))	Individual Statistics project data merged with administrative data, 1985-1996 see Meghir and Palme (2005)

Table A.3. Effect of school reforms on earnings across European countries: evidence from the literature. Countries: Finland, Germany(West), Italy, Sweden.

Country	Reform	Effect on Earnings	Data and References
Finland	1972-1977	-0.029 (men) 0.012 (women) -0.004 (all) non ac. fathers: -0.032 (men) -0.004 (women) -0.013 (all) academic fathers: -0.027 (men) 0.038 (women) 0.005 (all) (log taxable income, euros)	Finnish Longitudinal Census Data (*FLCD*) yrs 1970,1975,) 1980,1985,1990,1995,2000) see Pekkarinnen (2005)
Germany (West)	1947-1969	0.004-0.019 (all) -0.013-0.010 (basic track) (log gross monthly wage) 0.003-0.005 (all) 0.001-0.002 (basic track) (log net monthly income) -0.003-0.005 (all) (log earnings)	Qualification and Career Survey (*QaC*) MicroCensus social security records records (1% sample) period 1975-1995, see Pischke and Watcher (2005)
Italy	1963	-0.009 (men) 0.026 (women) north: -0.011 (men) 0.002 (women) center: -0.018 (men) 0.058 (women) south: 0.011 (men) 0.065 (women) (log weekly real wage)	Annual Report on Schooling (1948-1979) Labor Force Survey (October 1992-1997) Brandolini and Cipollone (2002)
Sweden	1962	0.044 (all) 0.026 (men) 0.053 (women) low fath. education & low/high/both ability: 0.039/0.075/0.062 (all) 0.037/0.066/0.060 (men) 0.028/0.074/0.061 (women) high fath. education -0.046 (all) -0.053 (men) -0.006 (women) (log pre-tax earnings)	Individual Statistics project data merged with administrative data, 1985-1996 Meghir and Palme (2003)
Sweden	1962	1.42 (all) 0.88 (men) 2.11 (women) low fath. education & low/high/both ability: 2.62/4.53/3.36 (all) 1.66/6.71/3.79 (men) 3.23/2.97/3.06 (women) high fath. education -5.59 (all) -7.66 (men) -4.22 (women) (percentage change in earnings)	Individual Statistics project data merged with administrative data, 1985-1996 Meghir and Palme (2005)

Table A.4. Returns to education across European countries (identification exploiting instrumental variables, i.e. reforms of the schooling system). Evidence from the literature. Countries: France, Germany (West), Italy, Ireland, Netherlands, Sweden.

Country	Reform	Returns to Education	Data and References
France	1957	0.043-0.046 (DD estimate) 0.018-0.027 (DDD estimate)	French National Labour Force Survey see Grenet (2004)
Germany (West)	1947-1969	0.007-0.032 0.005-0.010	Qualification and Career Survey (QaC), MicroCensus, social security records (1% sample) see Pischke adn Watcher (2005)
Italy	1963	Females, ft workers, (various IV-based id. strat.) -0.028-0.024 (1992) 0.051-0.138 (1997) 0.031-0.088 (1992-97) (log real gross weekly earnings)	Annual Report on Schooling (1948-1979) Labor Force Survey (October 1992-1997) see Brandolini and Cipollone (2002)
		-0.022-0.018 (1992) 0.039-0.109 (1997) 0.024-0.072 (1992-97) (log real net weekly earnings)	see Brandolini and Cipollone (2002)
		0.03 (women) 0.05 (men) (log annual earnings less tax plus no monetary integration)	Survey on Household Income and Wealth, 1991, see Flabbi (1999)
Ireland	1967 (Fees Abolition)	0.136 over diff. specif. 0.1274-0.1680	see Denny and Harmon (2000)
Netherlands	1968	0.033-0.051 (males) -0.028-0.047 (females)	OSA- Labour market Survey (1985,1986, 1988,1990, 1992 and 1994) see Plug (2001)
Sweden	1962	0.036	Individual Statistics project data merged with administrative data, 1985-1996 Palme and Meghir cited by Card (2001)

Table A.5. Panel A. Summary on the age at which individuals are surveyed. Finland.

Major Regions	Ref. $\bar{b}$	($\bar{b}$-7,$\bar{b}$+7)	ECHP 2001 ISSP 2001	ISSP 2002
Uusima	1966	1957-1975	28-42	29-43
Etela-Suomi	1965	1956-1974	29-43	30-44
Ita-Suomi	1963	1954-1972	31-45	32-46
Vali-Suomi	1962	1953-1971	32-46	33-47
Pohjois-Suomi	1961	1952-1970	34-47	34-48

Note: sub-sample of individuals born at most 7 years before and 7 years after the year of birth of the first cohort potentially affected by the reform with no missing data on the following variables relevant for the analysis: age, gender, lagged country specific unemployment rate and GDP per capita, country and gender specific labour force participation rate at the estimated time of labour market entry, the country specific fertility rate, GDP per head and unemployment rate at the age affected by the country specific reform, employment status).

Table A.5. Panel B. Summary on the age at which individuals are surveyed. Germany.

	Ref. $\bar{b}$	($\bar{b}$-7,$\bar{b}$+7)	International Social Survey							ECHP
Country			1993	1995	1996	1997	1998	2000	2002	2001
Schleswig-Holstein	1941	1932-1950	45-57	47-59	48-60	49-61	51-60	55-64	59-65	53-65
Hamburg	1934	1925-1943	52-56	54-59	56-60	56-61	57-61	60	64-66	60-65
Niedersachsen	1947	1938-1956	39-52	41-55	42-56	43-57	44-58	46-60	49-62	47-61
Bremen	1943	1934-1952	50-55	n.a.	49-60	51-60	48-62	51-64	61	51-65
Nordr.-West., Hessen Rhein.-Pf.,Baden-W.	1953	1944-1962	33-47	35-49	36-50	37-51	38-52	40-54	42-56	41-55
Bayern	1955	1946-1964	31-45	33-47	34-48	35-49	36-50	38-52	40-54	n.a.
Saarland	1949	1940-1958	34-57	39-53	47-49	49	43-56	53-56	48	45-57

Note: see Table A.5, panel A.

Table A.5. Panel C. Descriptive statistics. Sub-sample of individuals born at most 7 years before and 7 years after the year of birth of the first cohort potentially affected by the reform.

Country	Survey Year	Sample Size	Age Range+	Country	Survey Year	Sample Size	Age Range+
Austria	ISSP, 1994	172	41-55	Ireland	ISSP, 1993	298	28-42
	ISSP, 1995	191	41-55		ISSP, 1994	271	29-43
	ISSP, 1998	192	45-59		ISSP, 1995	315	30-44
	ISSP, 2000	177	47-61		ISSP, 1996	313	31-45
	ISSP, 2001	184	47-61		ISSP, 2000	349	36-50
	ECHP, 2001	1,065	47-61		ECHP, 2001	968	36-50
	SHARE, 2004	781	50-64	Italy	ISSP, 1993	237	37-51
Belgium	ISSP, 2002	329	26-40		ISSP, 1994	235	38-52
	ECHP, 2001	999	26-39		ISSP, 1997	198	41-55
Denmark	ISSP, 1997	297	33-47		ISSP, 1998	197	42-56
	ISSP, 1998	286	34-48		ECHP, 2001	2,447	45-59
	ISSP, 2000	200	38-52		SHARE, 2004	783	48-62
	ISSP, 2001	264	38-52	Netherlands	ISSP, 1993	630	27-41
	ISSP, 2002	343	38-52		ISSP, 1994	630	28-42
	ECHP, 2001	1,034	37-51		ISSP, 1995	711	29-43
	SHARE, 2004	381	41-54		SHARE, 2004	343	39-52
Finland	ISSP, 2001	335	Table A.5 pan. A	Spain	ISSP,1993	343	30-44
	ISSP, 2002	298	Table A.5 pan. A		ISSP,1995	271	31-45
	ECHP, 2001	1,332	Table A.5 pan. A		ISSP,1997	277	33-47
France	ISSP, 1996	342	37-51		ISSP,1998	262	34-48
	ISSP, 1997	280	38-52		ISSP,2000	480	36-50
	ISSP, 1998	243	38-52		ISSP,2002	174	39-53
	ISSP, 2002	477	42-56		ECHP,2001	461	37-51
	SHARE, 2004	424	44-58		SHARE,2004	2,435	41-54
Greece	ECHP, 2001	2,010	31-45	Sweden	ISSP,1994	345	37-51
	SHARE, 2004	113	38-48		ISSP,1995	321	38-52
Germany	ISSP, 1993	245	Table A.5 pan. B		ISSP,1996	352	39-53
	ISSP, 1995	327	Table A.5 pan. B		ISSP,1998	304	41-56
	ISSP, 1996	578	Table A.5 pan. B		ISSP,2000	270	44-58
	ISSP, 1997	304	Table A.5 pan. B		ISSP,2002	270	45-59
	ISSP, 1998	265	Table A.5 pan. B		SHARE,2004	1,167	47-61
	ISSP, 2000	273	Table A.5 pan. B				
	ISSP, 2002	216	Table A.5 pan. B				
	ECHP, 2001	1,047	Table A.5 pan. B				

Note: see Table A.5, panel A. + at the time the survey was carried out. German data from SHARE 2004 have been excluded because there was no available information on the region of residence. Such information is necessary to assign individuals to the pre- or post-reform groups.

Table A.6. Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, proportion employed, and proportion of women. Sub-sample of individuals born at most 7 years before and 7 years after the year of birth of the first cohort potentially affected by the reform. Data: ISSP 1993-2002, ECHP 2001, SHARE 2004 (release 1).

Country	Survey	Sample size	Prop.	Prop.	Average (st.err) [min-max]	
		wage-hours-status	Empl.	Fem.	Wage/month	Hours/week worked
Austria	ISSP*	614-322-916	49.8	83.8	1056.4 (709.1) [0-3820.3]	38.6 (15.8) [3-85]
	ECHP	503-623-1065	45.6	60.7	2063.5 (1180.3) [57.0-7838.5]	42.9 (13.4) [15-96]
	SHARE	255-328-781	36.0	62.6	1578.8 (2128.6) [.1-13416.5]	39.3 (15.5) [0-100]
Belgium	ISSP*	70-294-329	78.7	51.7	1417.8 (1666.4) [266.1-14751.6]	39.9 (13.9) [6-96]
	ECHP	815-875-999	80.3	56.5	1915.0 (846.5) [106.6-6276.1]	39.4 (10.6) [15-90]
Denmark	ISSP*	1347-1245-1390	82.9	54.4	2529.1 (1152.9) [443.6-6183.7]	38.0 (8.1) [5-81]
	ECHP	867-928-1034	83.2	49.4	2607.1 (1067.7) [114.6-9261.4]	38.4 (8.9) [15-91]
	SHARE	321-328-381	82.7	54.3	2660.8 (3478.8) [1.7-60538.6]	37.5 (10.5) [0-84]
Finland	ISSP*	575-489-633	70.1	54.7	1656.4 (1576.3) [.8-30518.5]	38.4 (9.6) [2-90]
	ECHP	1022-1200-1332	76.1	49.0	1762.2 (804.9) [41.6-8323.2]	41.4 (10.9) [10-96]
France	ISSP*	1220-1127-1342	80.0	51.6	1928.2 (1427.9) [234.2-8971.8]	38.4 (12.4) [0-80]
	SHARE	318-323-424	76.9	53.8	2376.1 (2804.1) [.1-22902.7]	39.3 (13.9) [0-105]
Germany	ISSP*	1656-1324-2208	72.6	48.6	1555.0 (1014.3) [25.7-14296.3]	37.0 (15.1) [0-96]
	ECHP	768-798-1047	68.0	49.9	2412.0 (1470.6) [24.6-9334.1]	40.7 (11.9) [2-96]
	SHARE	+	+	+	+	+
Greece	ECHP	964-1524-2010	47.6	50.7	1245.7 (694.9) [46.3-12198.8]	42.6 (11.1) [15-90]
	SHARE	40-45-113	37.2	99.1	1292.9 (759.3) [69.1-4053.9]	30.2 (21.6) [0-80]
Ireland	ISSP*	1060-1176-1546	65.6	54.4	1115.6 (886.7) [142.9-4427.7]	40.9 (14.9) [4-96]
	ECHP	592-689-968	58.3	50.9	1908.0 (1248.1) [47.5-10127.5]	39.0 (14.0) [9-90]
Italy	ISSP*	541-664-867	64.5	55.4	1508.7 (897.6) [103.4-7169.9]	35.3 (16.4) [0-90]
	ECHP	1127-1474-2447	45.7	56.2	1816.2 (972.3) [224.0-10192.5]	38.9 (9.9) [15-80]
	SHARE	251-347-783	41.5	65.9	1755.9 (1953.7) [.1-19853.7]	35.0 (16.7) [0-100]
Netherlands	ISSP*	1736-1316-1971	59.9	57.1	1751.4 (818.8) [179.8-5413.8]	32.8 (13.1) [1-90]
	SHARE	245-268-343	74.9	60.0	2232.1 (1867.3) [46.5-19129.4]	33.7 (14.6) [4-80]
Spain	ISSP*	1143-1267-1925	54.8	51.6	1059.3 (693.4) [174.9-7589.1]	36.5 (15.6) [0-96]
	ECHP	1392-1714-2435	55.7	49.9	1758.2 (1216.1) [54.7-15442.7]	42.2 (10.7) [14-96]
	SHARE	191-214-354	55.9	63.5	1807.4 (5001.8) [.5-62668.5]	32.8 (13.1) [1-90]
Sweden	ISSP*	1778-1719-1862	85.9	50.4	1613.1 (1130.7) [94.1-23512.7]	39.3 (9.4) [1-90]
	SHARE	926-974-1167	81.1	54.7	1991.4 (1544.5) [.7-21840.4]	39.2 (11.0) [0-100]

Note: data on wages in the 1999 International Social Survey Program are reported as deciles of the wage distribution. n.a stands for "not available". * Not all the waves of ISSP provide data on all the European countries considered; the following list applies: Austria, ISSP 1994, 1995, 1998, 2000, 2001; Belgium, ISSP 2002; Denmark, ISSP 1997, 1998, 2000-2002; Finland, ISSP 2001, 2002; France, ISSP 1996-1998,2002; Germany, ISSP 1993,1995-1998, 2000, 2002; Ireland, ISSP 1993-1996, 2000; Italy, ISSP 1993, 1994, 1997, 1998; Nethelands: ISSP 1993-1995; Spain, ISSP 1993, 1995, 1997, 1998, 2000, 2002.

Table A.7. Heterogenous returns to schooling. Quantile treatment effects. Sample size: 7804; ECHP 2001 only.

Males	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.0768^{***}	0.0667^{***}	0.0607^{***}	0.0582^{***}	0.0815^{***}
	0.0638	0.0072	0.0073	0.0182	0.0101
$\tau_a = 0.30$	0.0642^{***}	0.0561^{***}	0.0528^{***}	0.0476^{***}	0.0647^{***}
	0.0090	0.0058	0.0055	0.0078	0.0088
$\tau_a = 0.50$	0.0717^{***}	0.0612^{***}	0.0563^{***}	0.0549^{***}	0.0747^{***}
	0.0092	0.0064	0.0058	0.0086	0.0107
$\tau_a = 0.70$	0.0669^{***}	0.0532^{***}	0.0543^{***}	0.0512^{***}	0.0636^{***}
	0.0093	0.0067	0.0068	0.0077	0.0101
$\tau_a = 0.90^{++}$	0.0553^{***}	0.0381^{***}	0.0451^{***}	0.0418^{***}	0.0540^{***}
	0.007	0.005	0.005	0.006	0.009
Mean Effect^{+}	0.0670	0.0551	0.0538	0.0507	0.0677

Females	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.0980^{***}	0.0830^{***}	0.0836^{***}	0.0898^{***}	0.0883^{***}
	0.0289	0.0055	0.0062	0.0170	0.0088
$\tau_a = 0.30$	0.0863^{***}	0.0723^{***}	0.0751^{***}	0.0797^{***}	0.0826^{***}
	0.0070	0.0055	0.0057	0.0061	0.0091
$\tau_a = 0.50$	0.0894^{***}	0.0702^{***}	0.0700^{***}	0.0756^{***}	0.0869^{***}
	0.0073	0.0059	0.0063	0.0064	0.0097
$\tau_a = 0.70$	0.0824^{***}	0.0656^{***}	0.0687^{***}	0.0754^{***}	0.0857^{***}
	0.0067	0.0056	0.0064	0.0068	0.0098
$\tau_a = 0.90^{++}$	0.0693^{***}	0.0505^{***}	0.0550^{***}	0.0602^{***}	0.0678^{***}
	0.007	0.005	0.005	0.005	0.009
Mean effect^{+}	0.0851	0.0683	0.0705	0.0826	0.0823

Note: each quantile regression included a constant, country dummies, q, q^2 and their interaction with country dummies, a gender dummy, the country and gender specific labour force participation rate at the estimated time of labour market entry, the country specific GDP per head and unemployment rate at the age affected by the country specific reform and their interaction with the gender dummy, the interaction of age and age squared with the gender dummy. Countries included: Austria, Belgium, Denmark, Finland, Germany, Greece, Ireland, Italy, Spain. $^{++}$ These estimates are reported for completeness but should be treated with caution since the corresponding first stage estimates are imprecise. $^{+}$Mean effect: average (over τ_a) quantile treatment effect. Three stars, two stars and one star for statistically significant coefficients at the 1%, 5% and 10% confidence level.

Table A.8. P-values of the test that the marginal effect of years of schooling does not vary between subsamples. Sample size: 7804; ECHP only.

Males	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.787	0.945	0.898	0.325	0.486
$\tau_a = 0.30$	0.965	0.820	0.970	0.536	0.397
$\tau_a = 0.50$	0.375	0.724	0.631	0.352	0.893
$\tau_a = 0.70$	0.290	0.576	0.748	0.282	0.878
$\tau_a = 0.90$	0.695	0.984	0.831	0.451	0.453

Females	$\tau_u = 0.10$	$\tau_u = 0.30$	$\tau_u = 0.50$	$\tau_u = 0.70$	$\tau_u = 0.90$
$\tau_a = 0.10$	0.990	0.458	0.314	0.056*	0.724
$\tau_a = 0.30$	0.687	0.146	0.311	0.074*	0.828
$\tau_a = 0.50$	0.037	0.009***	0.947	0.442	0.953
$\tau_a = 0.70$	0.085	0.007	0.0741	0.0643	0.954
$\tau_a = 0.90$	0.053*	0.021**	0.840	0.667	0.896

Note: each quantile regression included a constant, country dummies, q, q^2 and their interaction with country dummies, survey dummies, age, age squared, a gender dummy, the lagged country specific unemployment rate and GDP per capita, the country and gender specific labour force participation rate at the estimated time of labour market entry, the country specific GDP per head and unemployment rate at the age affected by the country specific reform; a dummy taking the value 1 for the 'high education' group of countries, namely Belgium, Denmark, Finland, and Germany and its interaction with macro variables, survey dummies, age and age squares. Three stars, two stars and one star for statistically significant coefficients at the 1%, 5% and 10% confidence level.

C. The Monte Carlo Simulations

Chesher's model requires that outcomes and covariates exhibit continuous variation. In the specification adopted in this paper, wages are indeed continuous but the schooling variable and the instruments are not. We investigate how severely this problem may affect our estimates with the help of Monte Carlo simulations. Our Monte Carlo exercise is designed adapting the design of the Monte Carlo simulations in Ma and Koenker (2006) to our setting. We assume the following specification

$$Y_1 = \alpha_1 + \alpha_2 x + \alpha_3 Y_2 + \alpha_4 \alpha_5 v_2 Y_2 + \alpha_4 v_1 Y_2 + v_1 + v_2$$

$$Y_2 = \gamma_0 + \gamma_1 x + \gamma_2 z + v_2$$

where Y_1 and Y_2 are for the log wage and the years of schooling, z is the instrument - $z \sim N(8, 1.315)$ - the two errors are $v_1 \sim N(0,1)$ and $v_2 \sim N(0, 0.5^2)$, there is a single covariate $x \sim t_3$ and we set the parameters at $(\alpha_1, \alpha_2, \alpha_3, \alpha_4, \alpha_5) = (2, 4, 0.05, -0.0089, 0.236)$ and $(\gamma_0, \gamma_1, \gamma_3) = (3, 2, 1)$ to mimic the results in Table 7. We draw samples of 100 units from v_1, v_2, z, x and generate Y_1 and Y_2 according to the equations above. We then use the generated variables to estimate the model both when Y_2 and z are continuous and when they are rounded to their nearest integer. Table A.9 in the Appendix compares the true estimates of the diagonal of matrix π based on the continuous and rounded data, and shows that the differences among these estimates are in general rather small. Therefore, we conclude that the use of non continuous variables in our empirical model does not produce a significant bias.

Table A.9. Monte Carlo simulation results. Number of replications: 100. Sample size: 1000.

τ_1	τ_2	$\Pi(\tau_1, \tau_2)$ (true)	$\widehat{\Pi(\tau_1, \tau_2)}$ (continuous data est.)	$\widehat{\Pi(\tau_1, \tau_2)}$ (rounded data est.)
0.10	0.10	0.0627	0.0546	0.0541
0.30	0.30	0.0552	0.0524	0.0560
0.50	0.50	0.05	0.0498	0.0514
0.70	0.70	0.0448	0.0443	0.0419
0.90	0.90	0.0372	0.0442	0.0398