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"For One More Year with You":

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 quantile of the distribution of ability. Contrary to previous findings in the relevant literature, we find that additional education reduces wage inequality below median income and increases it above median income. There is also evidence in our data that education and ability are complements in the production of human capital and earnings. While these results support an elitist education policy - more education to the brightest - they also suggest that investing in the less fortunate but bright could payoff both on efficiency and on equity grounds.

Keywords: Education reforms, distribution of earnings, Europe

JEL Codes: J24

"For One More Year with You": Changes in Compulsory Schooling, Education and the Distribution of Wages in Europe

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1 Introduction[1]

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 additional education reduce 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 equity 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 et al. [2005]). If education and ability are substitutes in the production of human capital and

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earnings, then additional investment in the former can contribute to reducing the differences induced 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. De Fraja [2002], shows that optimal 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 depend on education and ability being complements.

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. These laws have been targeted at the less educated component of the population, which typically belongs to the lower quantiles of the distribution of earnings. Have the changes in compulsory education observed in Europe after the war been particularly beneficial to the targeted population or have they spread their effects to the population at large?

This paper addresses these questions by investigating the relationship between the quantity of attained education and the distribution of (gross) hourly earnings in a unique sample of 12 European countries, which we have constructed by pooling information drawn from three different surveys. We deal with the endogeneity of education in a quantile regression framework following Chesher [2003], and Koenker and Ma [2006][2]. We identify the causal effects of education on earnings by using the country and time variation provided by compulsory school reforms implemented in Europe after the end of the Second World War[3].

When we treat education as exogenously assigned to individuals, we find evidence that one additional year of schooling increases wage inequality, measured by the $90-10$ log wage differential, in line with previous research both in the US (Buchinsky [1994]) and in Europe (Martins and Pereira [2004]). However, when we explicitly allow for the possibility that education is endogenous with respect to earnings, we find that the relationship between the private returns to education and the distribution of earnings is U - shaped, with declining returns below median earnings and increasing returns above the median.

Conditional on any quantile of the distribution of ability, the returns to education are highest among the individuals who are located both in the bottom and top quantile of the distribution of earnings. Focusing on the mean quantile treatment effect, we find that assigning an extra year of education to the individuals

[2]According to this approach the structural model has a triangular structure, both in the observables and in the latent variables.

[3]Moretti and Lochner [2004], Lleras-Muney [2005], and Oreopoulos [2006] exploited regional variation within a single country.

in the sample reduces by 0.9 percentage points the estimated $90 - 10$ log wage differential. This reduction is generated by a 11.3 percentage points decline in the $50 - 10$ differential, which is almost entirely compensated by a 10.4 percentage points increase in the 90-50 differential.

By conditioning on a selected quantile of the 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 increase with ability - albeit in a non-monotonic fashion - which points to complementarity in the relationship between education and ability.

Our empirical estimates also show that the effect of compulsory schooling laws on educational attainment is statistically significant for all the selected quantiles of the distribution of education. As expected, the size of this effect declines - again in a non-monotonic fashion - as we move from the bottom to the top quantile. The statistically significant effect of compulsory school reforms on individuals with higher educational attainment can be intepreted as suggesting that better-educated individuals react to increases in compulsory schooling by raising their own attainment, in an effort to maintain their educational advantage over the less educated, who are more directly affected by the reforms.

The finding that expected returns are highest both in the bottom and in the top quantile of the distribution of earnings suggests that education policies which target the former group can be justified not only on equity grounds - if the less fortunate are so because of circumstances beyond their control - but also for efficiency reasons if the costs of education do not vary much after conditioning for ability, because the potential productivity gains are highest. Moreover, the fact that education and ability are complements is supportive of the elitist education policy suggested by De Fraja [2002]: since education costs typically decline with ability, the brightest should receive more education because they earn higher returns, independently of the position they have in the distribution of earnings. Complementarity also suggests that since ability and parental background are closely intertwined, education policy could produce relatively high returns by replicating for the less privileged the conditions associated with a “good” parental background, for instance by investing in child education (see Cunha and Heckman [2006]).

The paper is organized as follows: Section 2 describes the empirical model and Section 3 introduces the econometric methodology. Our identification strategy is discussed in Section 4. Next, we turn to the data in Section 5 and to the results in Section 6. Conclusions follow.

2 The Empirical Model

In his pioneering work on the impact of education on the distribution of US wages using quantile regressions, Buchinsky [1994], finds that returns to education in the US have increased dramatically over the quantiles of the conditional distribution of wages. If we use the $90 - 10$ log wage differential as the measure of inequality, this finding suggests that education is associated with higher earnings inequality. In the European context, Buchinsky's results are confirmed by Martins and Pereira [2004], who study the evidence from 15 European countries. Both authors also find that ability and education are complements in the production of human capital.

Since these studies do not address the endogeneity of education for the distribution of wages, their results are best interpreted as showing the presence of interesting associations and correlations, with little to say about causal effects. Arias et al. [2001], use data on twins and address the issue of the endogeneity of education by using an instrumental variable estimator for quantile regression, exploiting data on twins. They find that returns to education increase with the quantiles of the conditional distribution of earnings, and provide evidence that ability and education are complements, in contrast with almost contemporaneous evidence to the contrary provided by Ashenfelter and Rouse [1998], who also use data on US twins[4].

Our approach differs from Buchinsky in that we explicitly address the endogeneity of education, and from Arias et al. [2001], both in the estimation method - we follow Chesher [2003] and Koenker and Ma [2006] - and in the selection of the instruments for education: rather than using information on twins, we exploit the cross-country and time variation in the reforms to the compulsory school leaving age which have occurred in Europe since the end of the Second World War.

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

$$\mathtt{U}(\mathtt{W},\mathtt{S}) = \mathtt{ln}(\mathtt{W}) - \mathtt{f}(\mathtt{S}), \tag{1}$$

where $\mathtt{W}$ is (net) earnings and $\mathtt{S}$ is the years of schooling. Furthermore assume the following relationship between earnings and schooling

$$\mathtt{W} = \mathtt{g}(\mathtt{S}). \tag{2}$$

At the optimum, individuals select $\mathtt{S}$ so as to equate the marginal costs to the (expected) marginal benefits of schooling. Let marginal costs $\mathtt{MC}$ have the simple form

$$\mathtt{MC}(\mathtt{S}) = \mathtt{r} + \theta\mathtt{S} - \eta\mathtt{A}, \tag{3}$$

[4] See also Denny and O'Sullivan [2004].

where $\mathtt{A} \sim \mathtt{G}_2(0, \sigma_{\mathtt{A}}^2)$ is individual ability, which we take to be known to individuals at the time of the choice, and assume the following Mincerian earnings function

$$\mathtt{ln}(\mathtt{W}) = \alpha + \beta\mathtt{S} + \phi\mathtt{AS} + \lambda\mathtt{FS} + \gamma_{\mathtt{W}}\mathtt{X} + \mathtt{A} + \mathtt{F}, \tag{4}$$

where $\mathtt{X}$ is a vector of controls and $\mathtt{F} \sim \mathtt{G}_1(0, \sigma_{\mathtt{F}}^2)$ is an idiosyncratic wage shock orthogonal to ability. For instance, $\mathtt{F}$ could be luck in the individual job matching process following the completion of education. Hornstein et al. [2006], show that random matching of ex-ante identical individuals can generate frictional wage dispersion, with luckier individuals having a better draw from the distribution of wage offers. Alternatively, $\mathtt{F}$ could be a shock to the composition of labour demand, which either increases or reduces the market value of the skills learned at school.

We posit that the relationship between ability $\mathtt{A}$ and log wages is affected by schooling, and in particular that $\phi > 0$ when ability and schooling are complements in the production of human capital, and $\phi < 0$ when they are substitutes. Similarly, we allow for the possibility that shocks to the composition of labour demand affect earnings differently according to the level of accumulated schooling. The expected marginal benefits of schooling are given by

$$\mathtt{MB}(\mathtt{S}) = \beta + \phi\mathtt{A} \tag{5}$$

and optimal schooling $\mathtt{S}^*$ is equal to

$$\mathtt{S}^* = \frac{\beta - \mathtt{r}}{\theta} + \frac{\phi + \eta}{\theta}\mathtt{A}. \tag{6}$$

In the private optimum, schooling increases with individual ability $\mathtt{A}$. Ceteris paribus, this increase is stronger if ability and schooling are complements in the production of human capital. Furthermore, we assume

$$\begin{aligned} \phi + \eta &> 0 \\ 1 + \lambda\mathtt{S} &> 0. \end{aligned} \tag{7}$$

The former condition guarantees that optimal schooling is monotonic in individual ability, and the latter condition implies that log earnings are increasing in the shock $\mathtt{F}$.

It is apparent from the inspection of equations [4] and [6] that years of schooling are correlated with unobserved ability, which affects log earnings both directly and via its effects on education. Unless we can adequately control for ability, the standard orthogonality condition for consistency of ordinary least squares estimation fails. Still, consistent estimates can be obtained if there exists at least one variable which is correlated with schooling but not with individual ability conditional on schooling. Let $\mathtt{Z}$ be this instrumental variable (see Card [2001], for an extensive discussion). In this paper, $\mathtt{Z}$ is the number of years of compulsory education $\mathtt{YCOMP}$. The empirical counterparts of equations [4] and [6] can be written as

$$\ln(\mathtt{W}) = \beta(\mathtt{A},\mathtt{F})\mathtt{S} + \mathtt{A} + \mathtt{F} + \gamma_{\mathtt{W}}\mathtt{X} \tag{8}$$

$$\mathtt{S} = \gamma_{\mathtt{s}}\mathtt{X} + \pi\mathtt{Z} + \xi\mathtt{A}, \tag{9}$$

where $\xi = \frac{\phi+\eta}{\theta}$.

Rather than focusing on conditional mean effects, in this paper we are interested in the effects of education on the distribution of earnings. Define $\tau_1 = \mathtt{G}_1(\mathtt{F}_{\tau_1})$ and $\tau_2 = \mathtt{G}_2(\mathtt{A}_{\tau_2})$, where $\mathtt{F}_{\tau_1}$ and $\mathtt{A}_{\tau_2}$ are the τ_1 and τ_2 - quantiles of the distributions of $\mathtt{F}$ and $\mathtt{A}$, respectively. Then the conditional quantile functions $\mathtt{Q}_\mathtt{i}$ corresponding to equation [8] and [9] are (see Koenker and Ma [2006])

$$\mathtt{Q}_1[\tau_1|\mathtt{Q}_2(\tau_2|\mathtt{X},\mathtt{Z}),\mathtt{X},\mathtt{Z}] = \mathtt{Q}_2[\tau_2|\mathtt{X},\mathtt{Z}]\pi(\tau_1,\tau_2) + \gamma_{\mathtt{W}}\mathtt{X} \tag{10}$$

$$\mathtt{Q}_2\left[\tau_2|\mathtt{X},\mathtt{Z}\right] = \gamma_{\mathtt{s}}\mathtt{X} + \pi\mathtt{Z} + \xi\mathtt{G}_2^{-1}(\tau_2), \tag{11}$$

where $\mathtt{Q}_1$ refers to the earnings distribution and $\mathtt{Q}_2$ to the distribution of education. Since the distribution of education, conditional on the controls $\mathtt{X}$ and $\mathtt{Z}$, is affected by the distribution of ability $\mathtt{A}$, these expressions describe the effects of a perturbation in the distribution of ability on the various quantiles of the distribution of earnings. Rather than exogenously altering the value of $\mathtt{S}$, we alter its various quantiles $\mathtt{Q}_2$, and study how the quantiles $\mathtt{Q}_1$ of the distribution of earnings are affected. In this approach, the empirical outcome is illustrated using a matrix that associates to the exogenous shift in a quantile of the distribution of education the response of a quantile of the distribution of earnings.

The function $\pi(\tau_1,\tau_2)$ represents the quantile treatment effect of a change in schooling on earnings. If we set τ_1 so that $\mathtt{F}$ is fixed at its τ_1 quantile, changes in τ_2 in $\pi(\tau_1,\tau_2)$ reflect how the distribution of $\mathtt{A}$ affects the τ_1 quantile of the response $\ln(\mathtt{W})$. On the other hand, if we fix τ_2 and allow τ_1 to vary, we can shed light on how the τ_2 quantile of $\mathtt{S}$ affects the entire distribution of $\ln(\mathtt{W})$ (Koenker and Ma [2006], p.6). By integrating $\pi(\tau_1,\tau_2)$ with respect to τ_2 we obtain the mean quantile treatment effect, which describes how the returns to education vary across the different quantiles of the distribution of earnings. One advantage of this approach is that it allows us to deconstruct the mean effect into its components, which makes it easier to answer the questions we ask about the effect of education on wage inequality and about the relationship between (unobserved) ability and education in the production of human capital and earnings.

3 The Causal Effect of Education on the Distribution of Wages

We identify the causal effect of education on the distribution of wages by using the exogenous variation in 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 Oreopoulos [2006]) is that our analysis is not limited to the exploration of the conditional mean impact of schooling on wages; conversely, we allow for heterogeneity in the impact of education at different points of the distribution of earnings.

The econometric literature provides a few methods for identifying and estimating causal effects in non-additive error models. In this paper we follow Chesher's approach, which is described in more detail in the Appendix [5]. Chesher [2001], considers a structural model with a recursive structure in the variables $\mathtt{ln}(\mathtt{W})$ and $\mathtt{S}$ and in the errors $\mathtt{F}$ and $\mathtt{A}$, like the one illustrated by equations [8] and [9]. Crucial for identification is that: (i) there exists a variable $\mathtt{Z}$, or instrumental variable, that impacts on the quantile of the endogenous variable $\mathtt{S}$ and does not have a direct impact on the quantiles of the dependent variable $\mathtt{ln}(\mathtt{W})$; (ii) the change in the quantiles of $\mathtt{S}$ can be fully attributed to $\mathtt{Z}$ and not to other regressors (quantile invariance conditions). As remarked by Chesher [2001], the continuity of the endogenous regressor is needed for the unambiguous definition of quantiles[6], and guarantees the point identification of the quantiles of interest. When the continuity assumption on the endogenous regressor fails, the framework proposed in Chesher [2001] can be extended (see Chesher [2003] and Chesher [2005]) but does not generally lead to point identification of the exogenous impact function without further assumptions. Importantly, the case of an endogenous binary regressor cannot be dealt with in this set-up.

The estimation of the exogenous impact functions and inference in the above setup are discussed by Koenker and Ma [2006], for the parametric case, and we follow their approach in the current study. 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.

[5] Alternative approaches have been developed by Chernozhucov and Hansen [2005], and Abadie et al. [2002]. We found the assumptions of Chernozhucov and Hansen [2005] too restrictive in our setting and we could not apply the methodology of Abadie et al. [2002] since it requires that both the instrument and the endogenous regressors are binary.

[6] In Chesher [2002], there is no requirement on the scale of the regressors and of the instruments but a completeness condition has to be met.

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

- individuals with higher ability stay in school longer (monotonicity with respect to $\mathtt{A}$ in equation [9]);
- individuals with a luckier draw from the distribution of wage offers have higher wages (monotonicity with respect to $\mathtt{F}$ in equation [8]);
- when the schooling decision is made, individuals do not have information about their draw from the distribution of wage offers (triangular structure of the unobservables - see Section 2 for further details);
- the reforms have an (exogenous) impact on the distribution of years of education and/or the qualification levels of individuals: more education (the treatment) is assigned to individuals on the basis of their date of birth and the latter cannot be precisely chosen by their parents on the basis of future education-related wage gains;
- the 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 assume linear 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 fixed. By pooling data from several countries, we can increase the number of points on the support of the instrumental variable YCOMP[7] and this allows us to use the framework proposed by Chesher [2001]. The same approach cannot be applied at the country level essentially because the assumptions on the scale of the instrument are not met.

4 Our Strategy

Our strategy identifies the impact of education on the distribution of earnings at specific values (i.e. quantiles) of ability $\mathtt{A}$ and the wage shock $\mathtt{F}$ (Chesher [2001], Chesher [2003]), holding constant the value of conditioning variables.

We select for each country a school reform affecting compulsory education and define $\mathtt{T} \equiv (\mathtt{C} - \bar{\mathtt{c}}_{\mathtt{k}})$ as the distance between birth cohort $\mathtt{C}$ and the cohort

[7]The support of this variable consists of 7 points. Choosing years of education as the measure of the endogenous treatment (education), we can essentially consider it as continuous (in the sample it takes 33 distinct values).

$\bar{c}_k$, 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 $\bar{c}_k$. 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], we refer to the individuals who have changed their educational attainment as a result of the reforms as "compliers"[8].

Table 1 presents for each country in our 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 $\bar{c}_k$ for each country are relegated to Section B of the 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[10]. 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 B. 1 in the Appendix).

The modal compulsory number of years of education before the reforms in

[8]Individuals whose nationality is unknown and/or who are not citizens of the country in which they live at the 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 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.

[10]The first birth cohort potentially affected by the selected school reform in Germany was: 1934 for Hamburg, 1941 for Schleswig-Holstein, 1943 for Bremen, 1947 for Niedersachsen, 1949 for Saarland, 1953 for Nordrhein-Westphalen, Hessen, Rheinland-Pfalz and Baden Wurtemberg, and 1955 for Bayern (see Pischke and Watcher [2005]).

our sample of countries is 8. The first cohorts potentially affected by the reforms were born between 1934 and 1969, with a relative concentration in the late 1940s and late 1950s - early 1960s. Furthermore, the most commonly expected change in qualifications is the attainment of ISCED level 3 (upper secondary education).

Tables B. 3, B. 4 and B. 5 in the Appendix summarize the existing empirical evidence on the effects of some of these compulsory school reforms on individual education, earnings and 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.

Although the estimates of the effect of compulsory school reforms on educational 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[11], in some other countries returns to longer compulsory schooling are as high as 15%-20%. 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).

[11] This is the case for France (see Grenet [2004]) and Germany (see Pischke and Watcher [2005], and Fertig and Kluwe [2005]).

5 The Data[12]

We pool together data drawn from the 8^{th} wave of the European Community Household Panel ($ECHP$) (2001), the first wave of the Survey on Household Health, Ageing and Retirement in Europe, or $SHARE$, (2004), and the 1993 to 2002 waves 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. Tables C. 1, C. 2, C. 3 in the Appendix show for each country in the dataset the sample size for each survey and wave, and the relevant age range.

Our dependent variable is the log of hourly earnings expressed at 2000 prices and purchasing power parity (PPP) units. Table C. 4 in the Appendix reports the country-specific consumer price indices, exchange rates and PPP values. Additional information on earnings, hours worked and the proportion employed is also given in the Appendix, Tables C. 6 to C. 11. We measure educational attainment by using the number of 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 takes the customary number of years to achieve a qualification.

We assume that educational attainment does not change after age 25, and restrict our sample to include only individuals aged 26 to 65[13]. The final sample consists of $18,328$ individuals, and its distribution across 12 countries is shown in the last column of Table 2, which also includes 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 re-

[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 Ageing (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 contained in Börsch-Supan and Jürges [2005].

[13]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 dropping from the analysis individuals potentially affected by the reforms in some countries, for instance in Spain and Finland.

forms.

6 The Findings

Since we intend to identify from the data the causal relationship between education and the distribution of earnings, we need to control as accurately as possible for additional factors affecting the dependent variable. For this purpose, we include in the empirical specification both country and survey dummies, a gender dummy, individual age and its square. We also control for country-specific macroeconomic effects by using the first lags of the unemployment rate and of aggregate productivity, measured by real GDP per head.

Trend-like changes in log wages relative to the time of the reform are controlled with a second order polynomial in T and its interactions with country dummies[14]. 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[15]. 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, suppose that the reform increases the minimum school-leaving age from 14 to 15. 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 three variables, the unemployment rate, the fertility 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 criti-

[14] The relatively low order of the polynomial follows the suggestions by Lee and Card [2006]. Compared to higher order polynomials, the second order specification is the most parsimonious and provides an adequate fit of the data.

[15] 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.

cal 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.

With these premises, we start the presentation of our results by looking at the relationship between education and the distribution of wages when education is treated as exogenous. The estimates are shown in Table 3 below, and show that the returns to one year of education increase as we move from the lowest to the highest quantile of the distribution. A standard measure of wage inequality is the $90-10$ log wage differential (see for instance Katz and Murphy [1992]). Based on the estimates in Table 3, one additional year of education equally distributed across the sample would increase this measure of inequality by 2.4 percentage points.

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`. The latter always attracts a statistically significant and positive coefficient. The size of the effect, however, is much larger for the lowest quantile of the distribution of schooling and ability. For individuals below the 10th quantile of the distribution of ability, a one-year increase in compulsory education raises actual attainment by close to half a year, which compares to at most a quarter of a year for individuals with above median ability. This result is in line with expectations, which suggests that the bulk of compliers should be among the less able (and wealthy).

We test the hypothesis that the selected instrument is weak and find that the null hypothesis is always rejected at the 5% level of confidence (see Table 4). We conclude that the effects of compulsory school reforms are extended - albeit at a lower rate - beyond the lowest quantile to the upper part of the distribution of schooling. One natural explanation is that the better-educated may 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.

Following the methodology of Koenker and Ma [2006] and the discussion in Section 3 of the paper, we evaluate the treatment effect of education on the distribution of earnings by considering the various quantiles of the treatment distribution. We focus for brevity on the following quantiles: 0.10, 0.30, 0.50, 0.70 and 0.90. In the first step, we run quantile regressions of years of education on the full set of controls described above and on the instrument `YCOMP`, and compute the residuals as differences between `S` and the linear predictions from the regressions. In the second step, we augment with these residuals the quantile regressions of log hourly earnings on the set controls and education `S`. Koenker and Ma [2006], present Monte Carlo evidence showing that this approach is superior to the two-stage quantile regressions used by Arias et al. [2001].

The coefficient of the estimated residual in the log wage equation is different from zero if education is endogenous. Table 5 reports that we cannot reject the null hypothesis of endogenous treatment for the first and third quantiles of the distribution of earnings. For the other quantiles, the evidence in favour of endogenous treatment is weaker.

The estimated percentage increase in log earnings associated with one additional year of education and its standard error are reported in Table 6 for the selected quantiles of the distribution of ability A (τ_2) and earnings (τ_1). Considering for instance the 10^{th} quantile of the distribution of ability, we find that the estimated returns to education are equal to 7.5 percent for the individuals at the 10^{th} quantile of the distribution of earnings, 8.8 percent for the individuals at the 90^{th} quantile and lower for individuals at intermediate quantiles. The relationship between returns and quantiles is U-shaped, with returns decreasing from the bottom quantile to the median and increasing for higher than median earnings. Conditional on the selected quantile of the distribution of ability, the $90-10$ log wage differential is positive but small ($+1.3$ percentage points) and results from the combination of a substantial decline in the $50-10$ log wage differential (-2.8 percentage points) and an increase in the $90-50$ log wage differential ($+4.1$ percentage points). These findings warn against looking exclusively at the $90-10$ log wage differential as the measure of wage inequality. While a focus on this indicator suggests that one additional year of education assigned to the sample has little impact on wage inequality, a more detailed look at the distribution of earnings reveals that this small impact is the result of two relatively larger effects, a reduction of wage inequality below the median and an increase above the median.

The U-shaped pattern in the returns to education remains as we move from the bottom to the top quantile of the distribution of ability, and the estimated $90-10$ log wage differential turns negative while remaining small (see Tables 6 and 7). When we average returns across the distribution of abilities, the mean quantile treatment effect implies a decline of close to 1 percentage points in our measure of wage inequality. Therefore, our estimates based on instrumental variables do not confirm the findings in the relevant empirical literature, which suggest that additional education widens wage inequality.

The mean quantile treatment effect is a synthetic indicator of the distribution of returns to education, but it is not necessarily the most informative when the exogenous variation in educational attainment is provided by changes in compulsory schooling laws. As we have seen above, such laws are particularly effective in the bottom part of the distribution of abilities, and much less effective in the upper part. By decomposing the mean quantile treatment effect into the components associated with the different quantiles of the distribution of ability, we can highlight in Table 6 how returns to education vary between groups which are differently affected by these reforms.

Table 6 also provides evidence on the relationship between unobserved ability and years of schooling in the generation of earnings. Independently of the selected column, we notice a tendency for the estimated returns to schooling to increase as we move from the lowest to the highest quantile τ_2 of the treatment, with the exception of the 70^{th} quantile. This finding points to complementarity between ability and education. In more detail, when we increase education by an additional year, individuals located in the upper part of the distribution of ability gain more than three times as much as individuals with less than median ability. Since ability and parental background are closely intertwined - see Cunha and Heckman [2006] - our results also point to the fact that those better endowed have more to gain from additional education.

The policy implications of our results are important. First of all, suppose that earnings and productivity are closely related - a plausible assumption - and that the individuals earning less than the 10^{th} quantile of the conditional distribution of earnings do so at least partly because of circumstances beyond their control. Assume also that, conditional on ability, the cost of education does not vary significantly across quantiles. Then education policy aimed at raising the educational attainment of the less fortunate is grounded not only on equity considerations but also on efficiency grounds, because the labour market returns of the investiment are highest, especially for the brightest among the unlucky.

The neglected complementarity between education and ability is also relevant for policy. If ability is assigned randomly by nature, our results imply that education policy should focus on the brightest, as suggested by De Fraja [2002]. For instance, scholarships and fee waivers should be based not only on income but also on merit. On the other hand, if ability is closely intertwined with parental background, then policies that try to reproduce a “good” background for the less privileged - for example by focusing on early childhood education - are going to produce a better payoff across the entire distribution of earnings.

6.1 Robustness

Since log hourly wages are only available for employees, our sample is the outcome of a selection involving the decision to participate in the labour market and have a job. Unless we take this selection process explicitly into account, the error term in equation [8] 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.

To investigate this, define `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 to attract a statistically significant coefficient. It turns out that the estimated coefficient is equal to $.067$, with a bootstrapped standard error equal to $.045$ (p-value: 0.13). Therefore, we do not find evidence in the data supporting the view that the years of compulsory education are significantly associated with the endogenous selection of workers into paid employment, and conclude that our instrument is not invalidated by the failure to explicitly consider such a selection.

7 Conclusions

In this paper, 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 took place in each region at a different point in time. By so doing, we have been able to generate the country and time variation in the instrument that was absent in previous European research (see Harmon and Walker [1995]).

There are three main results: first, compulsory school reforms mainly affect 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 on average into half a year of additional education. This percentage falls to between 30 and 10 percent of a year for the rest of the sample. Second, and in contrast with most of the empirical literature, we find that additional education only marginally affects overall earnings inequality, measured by the $90-10$ wage differential. This result, however, is the combination of a significant decline in the 50-10 differential and a significant increase in the $90-50$ differential. Third, there is evidence that education and ability are complements in the generation of earnings, a necessary pre-condition for an optimal education policy to be elitist.

Overall, our evidence suggests that the pursuit of an elitist education policy on efficiency grounds is not necessarily in contrast with a policy which focuses on equality of opportunity, especially if the targeted population consists of the brightest among the less fortunate and if the less fortunate are so because of reasons beyond their control. Since the costs of providing additional education are unlikely to vary significantly across the sample once we condition on a given quantile of the distribution of ability, targeting the less fortunate is likely to pay off both on efficiency and on equity grounds.

Technical Appendix

A Chesher's approach

Chesher's approach is summarized in Figure A1. Identification requires that

1. $\mathtt{F}$ and $\mathtt{A}$ are continuously distributed with independent support;
2. defining $\mathtt{X}$ as the vector of controls and $\mathtt{Z}$ as the instrument, quantile independence conditions/quantile invariance holds at $\mathtt{X} = \mathtt{x}$ $\mathtt{Z} = \mathtt{z}$ or

$$\mathtt{Q}_{\mathtt{F}|\mathtt{AXZ}}(\tau_{\ln \mathtt{W}}, \mathtt{A}, \mathtt{x}, \mathtt{z}) = \mathtt{Q}_{\mathtt{F}}(\tau_{\ln \mathtt{W}}) \text{and} \mathtt{Q}_{\mathtt{A}|\mathtt{XZ}}(\tau_{\mathtt{S}}, \mathtt{x}, \mathtt{z}) = \mathtt{Q}_{\mathtt{A}}(\tau_{\mathtt{S}});$$

3. at $\mathtt{X} = \mathtt{x}$, $\mathtt{Z} = \mathtt{z}$, $\mathtt{Y_S} = \mathtt{Q}_{\mathtt{Y_S}|\mathtt{XY}}(\tau_{\mathtt{S}}, \mathtt{x}, \mathtt{z})$, $\mathtt{A} = \mathtt{Q}_{\mathtt{A}}(\tau_{\mathtt{S}})$, and the partial derivatives of $\mathtt{h}_{\mathtt{ln(W)}}$ with respect to both $\mathtt{Y_S} - \nabla_{\mathtt{y_S}}\mathtt{h}_{\mathtt{ln(W)}}$ and $\mathtt{A}$ -$\nabla_{\mathtt{A}}\mathtt{h}_{\mathtt{ln(W)}}$ exist and are finite;
4. at $\mathtt{X} = \mathtt{x}$, $\mathtt{Z} = \mathtt{z}$, $\mathtt{A} = \mathtt{Q}_{\mathtt{A}}(\tau_{\mathtt{S}})$, the partial derivative of $\mathtt{h_S}$ with respect to $\mathtt{Z_i}$ -$\nabla_{\mathtt{z_i}}\mathtt{h_S}$- exists and is non-zero.

Under the above assumptions, the functional describing the impact of an exogenous shift in $\mathtt{S}$ on $\mathtt{ln(W)}$ (or "exogenous impact function" in Chesher [2001]) is identified at $\mathtt{X} = \mathtt{x}$, $\mathtt{Z} = \mathtt{z}$ $\mathsf{F}= \mathtt{Q}_{\mathtt{F}}(\tau_{\ln \mathtt{W}})$, $\mathtt{A} = \mathtt{Q}_{\mathtt{A}}(\tau_{\mathtt{S}})$. As noted in Koenker and Ma [2006], holding $\tau_{\mathtt{ln(W)}}$ fixed and varying $\tau_{\mathtt{S}}$, the functional informs on how the distribution of $\mathtt{S}$ affects the $\tau_{\mathtt{ln(W)}}$ quantile of the response; conversely, holding $\tau_{\mathtt{S}}$ fixed and varying $\tau_{\ln \mathtt{W}}$, the functional informs on how the $\tau_{\mathtt{S}}$-quantile of $\mathtt{S}$ affects the distribution of $\ln \mathtt{W}$.

B 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].

B.1 Austria

The 1962 School Amendement 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.

B.2 Belgium

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

B.3 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 two additional years (from 7 to 9). Pupils who were 14 years old (or younger) in 1971 were potentially affected by the 1971 reform. We only consider this reform in this study.

B.4 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 B. 1 below. Following Pekkarinen [2005], we consider the cohort aged 11 when the reform was implemented as the first cohort potentially affected.

B.5 France

During the XX century, the 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]).

B.6 Germany

The peculiar political situation of the country produced the separate evolution of two distinct education systems between 1949 and 1990. We refer the reader to Pischke and Watcher [2005], Table B. 2, and Pischke [2003] for a description of the compulsory school reforms and for the selection of the first cohort potentially affected in each German Lander.

B.7 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 in 1963 and later were compelled to attend 3 additional years of schooling, whereas those born in 1962 were not.

B.8 Ireland

Compulsory schooling was modified in 1972, when the school leaving age was raised to 15. A further rise in the 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 those 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.

B.9 Italy

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

B.10 The Netherlands

From 1975 onwards, all three-year educational programs in the Netherlands were extended to four years and the compulsory school-leaving age was increased by one year, from 15 to 16. Osterbeek and Webbink [2004], highlight the fact that "the implementation of the extension of lower vocational education started in 1973. Since August 1 1973 all lower vocational programs had a length of four years. Students who were below the second year could still graduate from 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 Osterbeek and Webbink [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".

B.11 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 the 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 [2002], p.753 and Table A1 p.767).

B.12 Sweden

According to Meghir and Palme [2005], 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 as a result of personal communication with Marten Palme, we considered as potentially affected by the reform all individuals born after 1950.

C The Construction of the Key Variables

Is this section we provide further detail on the costruction of the key variables used in the empirical analysis.

C.1 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).

C.2 Wages

The heterogeneity across the surveys increases as one examines earnings, labour force status, and hours worked. Since earnings in the $ISSP$ surveys are recorded in scales by categories, with the number of categories varying across countries and surveys (see Table C. 5), we use the 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[1]. We transform the available information on earnings from the three surveys to 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 ($ECHP$); or total number of hours worked per week in the main job ($ISSP$). In the 1997 $ISSP$ survey hours are reported in scales by categories, 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 ($ISSP$). Tables C. 6-C. 11 present detailed information on earnings, hours and proportion employed by country, year and survey used.

C.3 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.

[1] An algorithm to impute missing values has been implemented; for details, see the documentation on the survey (see Börsch-Supan and Jürges [2005]).

D Tables and Figures

Table 1: Selected compulsory school reforms, by country.

Country	Reform	First cohort potentially affected	Change in min. sch. leav. age	Change in years of comp. school.	Expected change in qualification (ISCED)	Age at school entry at the time of the reform
Austria	1962	1947	14 → 15	8 → 9	↑ ISCED 2	6
Belgium	1983	1969	14 → 18	8 → 12	↑ ISCED 3	6
Denmark	1971	1957	14 → 16	7 → 9	↑ ISCED 3	7
Finland (Uusima)	1977	1966^{+}	13 → 16	6 → 9	↑ ISCED 3	7
Finland (Etela-Suomi)	1976	1965^{+}	13 → 16	6 → 9	↑ ISCED 3	7
Finland (Ita-Suomi)	1974	1963^{+}	13 → 16	6 → 9	↑ ISCED 3	7
Finland (Vali-Suomi)	1973	1962^{+}	13 → 16	6 → 9	↑ ISCED 3	7
Finland (Pohjois-Suomi)	1972	1961^{+}	13 → 16	6 → 9	↑ ISCED 3	7
France	1959^{++}	1953	14 → 16	8 →10	↑ ISCED 3	6
Germany	see text & Table B. 2		14 → 15	8 → 9	↑ ISCED 3	6
Greece	1975	1963	12 → 15	6 → 9	↑ ISCED 2	6
Ireland	1972	1958	14 → 15	8 → 9	↑ ISCED 3	6
Italy	1963	1949	11 → 14	5 → 8	↑ISCED 2	6
Netherlands	1975$^{\bullet}$	1959$^{\bullet}$	15 → 16	9 →10	↑ ISCED 2	6
Spain	1970	1957*	12 → 14	6 → 8	↑ ISCED 2	6
Sweden	1962	1950**	14/15 → 15/16	8 → 9	↑ ISCED 3	6/7

* Pons and Gonzalo [2002], p.753 and Table A1 p.767. $^{+}$Pekkarinen [2005], p.5. $^{++}$ Reform implemented in 1967, see Grenet [2004]. $^{\bullet}$ Reform implemented in 1973 (see Osterbeek and Webbink [2004]). ** Personal communication with Martin Palme.

Table 2: Means of the key variables. Sample size: 18,328.

	`log(W)`	`S`	`YCOMP`	Age	%Males	N.obs.
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	15.153	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	39.331	0.574	1260
Italy	2.367	12.556	7.097	49.066	0.590	1762
Netherlands	2.574	14.166	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

Legend: `log(W)` logarithm of (gross) hourly wages in PPP at 2000 prices; `S` years of schooling; `YCOMP` years of compulsory schooling.

Table 3: Quantile effects when education is treated as exogenous.

	$\tau = 0.10$	$\tau = 0.30$	$\tau = 0.50$	$\tau = 0.70$	$\tau = 0.90$
Coeff. (s.e)	$.029^{***}(.002)$	$.037^{***}(.001)$	$.043^{***}(.001)$	$.048^{***}(.001)$	$.053^{***}(.001)$

Note: `tau` denotes the conditional quantile of the distribution of wages. Each regression, run on a sample of 18,328 units, includes a constant, country dummies, `T` $\texttt{T}^2$ and their interactions with country dummies, survey dummies, age, age squared, a gender dummy, 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. Details on these coefficients are available from the authors upon request. Three stars,two stars and one star for statistically significant coefficients at the 1%, 5%, and 10% confidence level respectively.

Table 4: First stage effect of YCOMP on S. Sample size: 18,328.

	$\tau = 0.10$	$\tau = 0.30$	$\tau = 0.50$	$\tau = 0.70$	$\tau = 0.90$
Coeff. (s.e)	$.425^{***}(.005)$	$.162^{***}(.012)$	$.100^{***}(.013)$	$.264^{***}(.035)$	$.077^{***}(.013)$
$\widehat{\texttt{const}}(s.e.)$	$3.60^{***}(.07)$	$9.66^{***}(.18)$	$10.34^{***}(.20)$	$12.68^{***}(.53)$	$12.84^{***}(.33)$
95% C.I. $^{+}$	[.416,.434]	[.139,.186]	[.074,.125]	[.195,.333]	[.051,.103]

Note: see Table 3. Three stars,two stars and one star for statistically significant coefficients at the 1%, 5%, and 10% confidence level respectively. The estimates at the quantiles $\tau_1 \in \{0.10, 0.30, 0.70, 0.90\}$ are robust to changes in the specification of the model.

Table 5: Estimates of the first stage residual in the log wage regressions.

	$\boldsymbol{\tau_1 = 0.10}$	$\boldsymbol{\tau_1 = 0.30}$	$\boldsymbol{\tau_1 = 0.50}$	$\boldsymbol{\tau_1 = 0.70}$	$\boldsymbol{\tau_1 = 0.90}$
$\boldsymbol{\tau_2 = 0.10}$	$-.046^{*}_{.025}$	$-.030^{**}_{.014}$	$-.003_{.013}$	$-.011_{.016}$	$-.035_{.023}$
95% C.I.	[-.097,.005]	[-.058,-.002]	[-.030,0.023]	[-.045,.018]	[-.082,.011]
$\boldsymbol{\tau_2 = 0.30}$	$-.121^{*}_{.067}$	$-.078^{**}_{.037}$	$-.009_{.035}$	$-.029_{.042}$	$-.092_{.061}$
95% C.I.	[-.254,.012]	[-.152,-.005]	[-.078,.060]	[-.111,.053]	[-.214,.029]
$\boldsymbol{\tau_2 = 0.50}$	$-.197^{*}_{.110}$	$-.127^{**}_{.061}$	$-.015_{.057}$	$-.047_{.068}$	$-.151_{.100}$
95% C.I.	[-.414,.019]	[-.247,.007]	[-.128,.099]	[-.181,.086]	[-.349,.047]
$\boldsymbol{\tau_2 = 0.70}$	$-.074^{*}_{.041}$	$-.048^{**}_{.023}$	$-.005_{.021}$	$-.018_{.025}$	$-.057_{.038}$
95% C.I.	[-.156,.008]	[-.093,-.003]	[-.048,.037]	[-.068,.032]	[-.131,.018]
$\boldsymbol{\tau_2 = 0.90}$	$-.255^{*}_{.143}$	$-.165^{**}_{.079}$	$-.019_{.074}$	$-.061_{.088}$	$-.195_{.130}$
95% C.I.	[-.535,.025]	[-.319,-0.010]	[-.165,.127]	[-.234,.111]	[-.451,.061]

Note: see Table 3. τ_1 denotes the quantile of the distribution of luck F, i.e. wages. τ_2 denotes the quantile of the distribution of ability A, i.e. years of schooling. Sample size: 18,328. Three stars, two stars and one star for statistically significant coefficients at the 1%, 5%, and 10% confidence level respectively. Standard errors in small characters.

Table 6: Returns to schooling. Quantile treatment effects. Sample size: 18,328.

	$\tau_1 = 0.10$	$\tau_1 = 0.30$	$\tau_1 = 0.50$	$\tau_1 = 0.70$	$\tau_1 = 0.90$
$\tau_2 = 0.10$	$.075^{***}$ .026	$.067^{***}$ .014	$.047^{***}$ .013	$.059^{***}$ .016	$.088^{***}$ .023
95% C.I.	[.024,.126]	[.039,.095]	[.020,.074]	[.028,.090]	[.042,.135]
$\tau_2 = 0.30$	$.150^{**}$ .067	$.115^{**}$ .037	.053 .035	$.077^{*}$ .042	$.145^{**}$ .061
95% C.I.	[.017,.282]	[.042,.189]	[-.017,.122]	[-.005,.159]	[.024,.267]
$\tau_2 = 0.50$	$.226^{**}$ .110	$.165^{**}$ .061	.058 .057	.095 .068	$.204^{**}$ .100
95% C.I.	[.009,.442]	[.045,.228]	[-.055,.171]	[-.181,.086]	[.006,.401]
$\tau_2 = 0.70$	$.103^{**}$ .041	$.085^{***}$ .023	$.049^{**}$ .021	$.066^{**}$ .025	$.110^{***}$ .038
95% C.I.	[.021,.185]	[.040,.131]	[.007,.092]	[.012,.116]	[.035,.184]
$\tau_2 = 0.90$	$.284^{**}$ .143	$.220^{**}$ .079	.063 .074	.109 .088	$.248^{*}$ .130
95% C.I.	[.004,.564]	[.047,.357]	[-.084,.209]	[-.063,.282]	[-.008,.503]

	$\tau_1 = 0.10$	$\tau_1 = 0.30$	$\tau_1 = 0.50$	$\tau_1 = 0.70$	$\tau_1 = 0.90$
Mean Quantile Treatment Effect	.167	.127	.054	.081	.158
Quantile effect^{+}	.029	.037	.043	.048	.053

Note: see Table 3. τ_1 denotes the quantile of the distribution of luck `F`, i.e. wages. τ_2 denotes the quantile of the distribution of ability `A`, i.e. years of schooling. Three stars,two stars and one star for statistically significant coefficients at the 1%, 5%, and 10% confidence level respectively. Standard errors in small characters.$^{+}$ Effect of `years of education` on quantiles of the earnings distribution, when education is treated as exogenous (see Table 3).

Table 7: Impact of education on wage inequality.

τ_1	Δ_{50-10}	Δ_{90-50}	Δ_{90-10}
$\tau_2 = 0.10$	-.028	.041	.013
$\tau_2 = 0.30$	-.097	.092	-.005
$\tau_2 = 0.50$	-.168	.146	-.022
$\tau_2 = 0.70$	-.054	.061	.007
$\tau_2 = 0.90$	-.221	.185	-.036
Mean Quantile Treatment Effect	-.113	.104	-.009
Quantile Effect $^+$	.014	.010	.024

See Table 6. $^+$ Effect of `years of education` on quantiles of the earnings distribution, when education is treated as exogenous (see Table 3). τ_1 denotes the quantile of the distribution of luck $\mathtt{F}$, i.e. wages. τ_2 denotes the quantile of the distribution of ability $\mathtt{A}$, i.e. years of schooling.

Figure A. 1: Chesher framework.

$\mathtt{lnW} = \mathtt{h}_{\mathtt{lnW}}(\mathtt{S}, \mathtt{X}, \mathtt{F}, \mathtt{A})$
$\mathtt{h}_1(\cdot, \cdot, \cdot)$ continuous in all the arguments at the point of interest
$\mathtt{S} = \mathtt{h}_{\mathtt{S}}(\mathtt{X}, \mathtt{Z}, \mathtt{A})$ at the point of interest
$\mathtt{h}_{\mathtt{S}}(\cdot, \cdot)$ continuous in all the arguments
$\mathtt{h}_{\mathtt{S}}(\cdot, \mathtt{A})$ monotonic (increasing) wrt $\mathtt{A}$ at the point of interest
$\mathtt{h}_1(\cdot, \cdot, \mathtt{F})$ monotonic (increasing) wrt $\mathtt{F}$ at the point of interest
$\mathtt{Y}_{\mathtt{S}}$ potentially endogenous

Table B. 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

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

Table B. 2: Compulsory schooling reform in Germany. Key features.

Country	1st Year when all students have to graduate after 9 yrs of school	1st Birth Cohort with with 9 yrs of school
Schleswig-Holstein	1956	1941
Hamburg	1949	1934
Niedersachsen	1962	1947
Bremen	1958	1943
Nordrhein-Westphalen	1967	1953
Hessen	1967	1953
Rheinland-Pfalz	1967	1953
Baden-Würtemberg	1967	1953
Bayern	1969	1955
Saarland	1964	1949

The first three columns of the table are taken from Pischke et al.[Pischke and Watcher, 2005, Table 1].

Table B. 3: 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)yrs of edu 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 Pekkarinen [2005]
France	1957	nearly +0.34 yrs of school. using control group (effect varies according to parental backgr.) -20% drop-out rates among farm-workers' sons	French National Labour Force Survey see Grenet [2004]
Germany (West)	1947-1969	+ 0.28% yrs of school. (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 [
Italy	1963	+2.1% enrolment in 8^{th} grade +0.21 yrs of schooling +38%(women), +12% men prop. of those achieving 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]
Ireland	1967 (Fees Abolition)	(-0.1)- (-1.8) yrs of schooling	see Denny and Harmon [2000]
Netherlands	1968	+0.71 yrs (males) + 1.33 yrs (females)	OSA- Labour market Survey (1985,1986 ,1988, 1990, 1992 and 1994) see Plug [2001]
Sweden	1950	+10% (males), +8% (females) prop. of those achieving jun. high sch. +0.27(males), +0.22(females) yrs of sch. (*via* propensity score match.) effect varies with ability level	Individual Statistics project data merged with administrative data, 1985-1996 see Meghir and Palme [2005]

Table B. 4: Effect of school reforms on earnings across European countries: evidence from the literature. Countries: Denmark, Finland, France, Germany(West).

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 Pekkarinen [2005]
Germany (West)	1947-1969	0.004-0.019 (all) -0.013-0.010 (basic track) (log gross monthly wage)	Qualification and Career Survey (QaC)
		0.003-0.005 (all) 0.001-0.002 (basic track) (log net monthly income)	MicroCensus
		-0.003-0.005 (all) -0.003-0.005 (all) (log earnings)	security security records records (1% sample) period 1975-1995, see Pischke and Watcher [2005]

Table B. 5: 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
Italy	1963	Females, ft workers, (various IV-based identification strategies) -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]

Table C. 1: Summary of the age at which individuals are surveyed. Finland.

Major Regions	Ref. $\bar{c}$	($\bar{c}$-7,$\bar{c}$+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 to 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 C. 2: Summary of the age at which individuals are surveyed. Germany.

	Ref. $\bar{c}$	($\bar{c}$-7,$\bar{c}$+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 C. 1.

Table C. 3: 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	see Table C. 1	Spain	ISSP,1993	343	30-44
	ISSP, 2002	298	see Table C. 1		ISSP,1995	271	31-45
	ECHP, 2001	1,332	see Table C. 1		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	see Table C. 2		ISSP,1996	352	39-53
	ISSP, 1995	327	see Table C. 2		ISSP,1998	304	41-56
	ISSP, 1996	578	see Table C. 2		ISSP,2000	270	44-58
	ISSP, 1997	304	see Table C. 2		ISSP,2002	270	45-59
	ISSP, 1998	265	see Table C. 2		SHARE,2004	1,167	47-61
	ISSP, 2000	273	see Table C. 2				
	ISSP, 2002	216	see Table C. 2				
	ECHP, 2001	1,047	see Table C. 2				

Note: see Table C. 1. + 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 C. 4: Price Indices used in the application.

Country	Consumer Price Index						
	1993	1994	1995	1996	1997	1998	1999
Austria	96.84	98.18	96.49	96.61	98.10	99.10	99.36
Belgium	90.86	92.70	93.88	95.16	97.31	99.16	99.13
Denmark	85.04	87.53	90.21	91.71	94.91	96.88	97.52
Finland	106.31	106.51	98.09	96.67	97.09	98.97	98.94
France	91.23	91.96	93.07	94.11	95.81	97.43	97.83
Germany	96.41	97.99	98.98	99.56	100.98	101.10	100.71
Greece	67.07	75.95	82.40	88.17	91.82	95.85	98.15
Ireland	82.40	85.13	87.57	89.06	90.42	94.12	96.98
Italy	84.62	87.65	93.04	96.72	96.58	97.58	98.46
Netherlands	92.75	94.49	94.85	94.83	96.37	98.60	99.74
Spain	84.29	88.83	93.16	96.31	95.65	96.71	97.98
Sweden	101.10	102.85	104.32	97.60	97.44	98.54	99.10
Country	Consumer Price Index					Exchange	PPP
	2000	2001	2002	2003	2004	rates	(2000)
Austria	100	103.64	105.08	106.94	108.99	13.76	13.33
Belgium	100	104.57	107.00	109.19	110.52	40.34	40.38
Denmark	100	103.89	106.13	107.66	106.57	7.46	9.04
Finland	100	104.43	107.44	108.08	108.92	5.95	6.90
France	100	105.50	108.39	110.85	111.41	6.56	6.68
Germany	100	104.48	105.33	105.17	104.81	1.96	1.95
Greece	100	105.12	110.71	116.24	116.85	340.75	263.75
Ireland	100	106.52	110.19	111.79	111.48	0.79	0.85
Italy	100	104.11	107.90	111.31	113.72	1936.27	1715.15
Netherlands	100	104.77	108.86	110.96	112.10	2.20	2.20
Spain	100	106.48	108.57	111.41	112.65	166.39	138.07
Sweden	100	105.90	111.16	115.71	120.19	9.18	10.79

Note: consumer price indices are from OECD, various publications (base year: 2000). Exchange rates are from Table 10.1 in SHARE, 2005 - for countries covered in SHARE rel. 1 - and from the EU Commission *DG II* (Dec. 2000) for the other countries. PPP in the year 2000 are from the European Community Houseohold Panel.

Table C. 5: Measures of earnings (currency) in ISSP 1993-2002. Differences between countries and surveys.

Code	ISSP93	ISSP94	ISSP95	ISSP96	ISSP97
Austria	-			-	-
Belgium	-	-	-	-	-
Denmark	-	-	-	-	(kr)/mm+
Finland	-	-	-	-	-
France	-	-	-	/mm (Francs)	(Francs)
Germany	net/mm (DEM)	net/mm (DEM)	net/mm (DEM)	net/mm (DEM)	(DEM)
Greece		-	-	-	-
Ireland	net/w	net/w	gross/yy	gross/yy	-
	(Ir. Pounds)	(Ir. Pounds)	(Ir. Pounds)	(Ir. Pounds)	-
Italy	net/mm	net/mm			(1000 Lire)
	(1000 Lire)	(1000 Lire)			(1000 Lire)
Netherlands	net (Gld)	net (Gld)	net (Gld)	-	-
Spain	(Pta)	(Pta)	/mm (Pta)		(Ptas)
Sweden	`NA`	gross/mm (Sk)	gross/mm (Sk)	gross/mm (Sk)	/mm (Sk)
Austria	net/mm (Sh.)		net/mm (Sh.)		net/mm (EURO)
Belgium	-	-	-	-	net/mm (EURO)
Denmark	gross/yy (Dkr)	-	gross/yy (Dkr)	/yy (Dkr)+	gross/yy
Finland	-	-			
France	/mm (Francs)		-		net/mm (EURO)
Germany	net/mm (DEM)		net/mm (DEM)		net/mm (EURO)
Greece	-	-	-	-	-
Ireland	gross/w	-	gross/w	-	
	(Ir. Pounds)	-	(Ir. Pounds)	-	
Italy	net/mm	-	-		-
	(1000 Lire)	-	-		-
Netherlands		-		-	
Spain	net/mm (Ptas)		/mm (Ptas)		/mm (EURO)
Sweden	gross/mm (SEK)		gross/mm (SEK)	-	gross/mm (SEK)

Note: "-" indicates that the survey was not conducted; empty cells indicate that either additional information is not available or there were no departures from the general coding of the question; `net` and `gross` stand for net and gross income respectively, and `/yy` `/mm` and `/w` indicate that the reference period is either a year, the month or the week. + the reference time period was not clearly stated in the codebook and the information has been deduced by comparing the summary statistics of the other surveys.

Table C. 6: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Males.

Country	Survey Year	Sample size	Prop.	Average (standard error) [min-max]	
		wage-hours-status	Empl.	Wage/month	Hours/week worked
Austria	ISSP,1994	23-28-29	96.6	1,910.2 (1,049.9) [534.8-3,820.3]	45.9 (15.5) [7-85]
	ISSP,1995	31-28-38	87.9	1,527.3 (625.1) [311-2,527.7]	40.3 (12.4) [15-80]
	ISSP,1998	20-24-27	88.9	1,703 (673.2) [300.1-3,150.8]	43.9 (10.7) [20-80]
	ISSP,2000	19-0-26	92.3	1,985.9 (780.1) [675.1-3,750.7]	n.a.
	ISSP,2001	23-0-33	78.8	1,834.6 (731.8) [651.4-3,618.9]	n.a.
	ECHP,2001	289-346-419	68.5	2,454.5 (1,191.6) [150.8-7,838.5]	44.6 (11.1) [17-90]
	SHARE,2004	133-177-292	56.8	1,861 (2,020.2) [19.7-13,4616.6]	44.4 (13.4) [0-100]
Belgium	ISSP,2002	43-156-159	82.4	1,256.8 (399.6) [512.6-2,2427.5]	44.1 (13) [20-90]
	ECHP,2001	361-415-435	83	2,229.8 (844) [457.9-6,276.1]	43.7 (9.5) [16-90]
Denmark	ISSP,1997	123-119-129	84.5	2,743.4 (1,094.9) [485.5-5,826.2]	40.5 (7.7) [10-70]
	ISSP,1998	124-109-133	81.2	2,679.1 (1,267.7) [665.9-6,183.7]	39.9 (8.5) [5-65]
	ISSP,2000	102-89-102	87.3	3,220.7 (1,303.5) [645.1-5,990.6]	40.7 (7.5) [15-70]
	ISSP,2001	128-112-128	84.4	3,153.4 (1,217.9) [443.6-5,322.6]	41.4 (8.0) [24-81]
	ISSP,2002	140-139-142	85.2	3,156.8 (1,291.8) [607.9-5,644.4]	41.8 (8.7) [20-80]
	ECHP,2001	435-487-523	82.8	2,980.8 (1,169.6) [255.5-9,261.4]	41 (8.7) [15-91]
	SHARE,2004	142-149-174	81.6	3,324.3 (5,067.5) [2.4-60,538.6]	41 (10.4) [8-84]
Finland	ISSP,2001	141-125-148	77.0	2,006.9 (2,621.9) [1.4-30,518.5]	40.4 (10.3) [8-90]
	ISSP,2002	130-114-139	71.2	1,805.1 (863.3) [.8-4,249]	40.6 (10.8) [6-85]
	ECHP,2001	513-641-679	75.3	2,033.4 (886.9) [41.6-8,323.2]	44.6 (11.9) [15-96]
France	ISSP,1996	211-202-221	93.2	2,440.2 (1,717.9) [317.9-3,743]	44.0 (9.7) [18-80]
	ISSP,1997	131-136-136	83.8	2,353.5 (1,474.2) [624.6-7,807.4]	40.7 (13.6) [0-75]
	ISSP,1998	115-101-121	86.8	2,437.4 (1,850.9) [307.1-8,445]	43.8 (9.5) [20-80]
	ISSP,2002	165-147-111	78.4	2,232.0 (1,707.8) [276.2-8,971.8]	43.3 (9.4) [30-70]
	SHARE,2004	159-159-196	81.6	3,079.7 (3,267.5)	43 (12.5) [0,105]
Greece	ECHP,2001	554-993-990	55.0	1,350.9 (817.4) [245.3-12,198.8]	45.2 (11.1) [15-90]

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".

Table C. 7: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Males.

Country	Survey Year	Sample size wage-hours-status	Prop. Empl.	Average (standard error) [min, max] Wage/month	Hours worked
Germany	ISSP,1993	87-111-117	95.7	1,978.5 (959.8) [479.2-7,986.2]	42.0 (6.8) [20-61]
	ISSP,1995	183-182-188	97.3	1,823.6 (584.2) [259.3-2,593]	45.2 (11.4) [9-96]
	ISSP,1996	249-0-313	88.8	1,889.3 (980.4) [257.3-6,4441]	n.a.
	ISSP,1997	159-157-162	74.7	1,799.8 (761.9) [254.1-3,049.8]	38.3 (18) [0-80]
	ISSP,1998	78-108-126	85.7	1,740.4 (568.9) [691.9-3,522.6]	41.7 (8.5) [20-80]
	ISSP,2000	104-113-125	88.8	2,132.8 (1,178.8) [256.6-7,699.4]	46.1 (11.3) [8-84]
	ISSP,2002	92-93-105	77.1	2,412.4 (1,703.3) [190.6-14,296.3]	46.3 (11.0) [25-96]
	ECHP,2001	400-460-525	74.5	3,149.0 (1,358.9) [98.3-8,196.0]	45.6 (9.8) [20-96]
	SHARE,2004	+	+	+	+
Ireland	ISSP,1993	126-107-134	80.0	1,116.4 (574.3) [142.9-4,002.6]	45.3 (11.4) [16-80]
	ISSP,1994	103-82-114	77.2	1,309.2 (699.9) [221.4-4,427.7]	44.7 (10.1) [11-70]
	ISSP,1995	142-151-153	88.2	1,782.5 (996.9) [145.7-4,080.1]	46.1 (13.0) [13-96]
	ISSP,1996	141-148-151	86.1	1,767.1 (1,025.1) [143.2-4,012.0]	45.9 (13.1) [13-96]
	ISSP,2000	0-118-153	86.3	n.a.	46.6 (11.7) [6-85]
	ECHP,2001	315-420-475	65.9	2,413.2 (1,293.7) [239.9-10,127.5]	44.7 (12.9) [10-90]
Italy	ISSP,1993	103-105-111	94.6	1,577.4 (701.3) [103.4-3,513.9]	42.3 (8.5) [18-70]
	ISSP,1994	99-103-105	97.1	1,602.7 (798.2) [166.3-4,356.8]	40.1 (10.3) [8-80]
	ISSP,1997	57-82-82	63.4	2,313.1 (1,090.4) [603.7-5,614.2]	41.1 (14.7) [0-89]
	ISSP,1998	60-77-89	89.9	1,667.3 (968.1) [304.7-7,169.9]	42.8 (13) [6-90]
	ECHP,2001	663-903-1,071	61.6	2,033.3 (1,117.1) [336-10,192.5]	41.4 (8.9) [15-80]
	SHARE,2004	125-185-267	66.7	1,917.9 (2,196.4) [.1-19,853.7]	40 (16.2) [0-100]

Note: See Table C. 6. `n.a.` stands for "not available".

Table C. 8: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Males.

Country	Survey Year	Sample size	Prop.	Average (standard error) [min,max]	
		wage-hours-status	Empl.	Wage/month	Hours worked
Netherlands	ISSP,1993	235-232-268	85.1	1,753.1 (724.9) [429-5,025.1]	41.2 (9.7) [10-90]
	ISSP,1994	238-223-259	84.6	1,710.2 (733.8) [280.7-4,451.4]	39.1 (8.6)[8-70]
	ISSP,1995	290-275-319	85.9	1,869.4 (893.2) [179.8-4,913.8]	38.8 (8.8) [8-84]
	SHARE,2004	104-123-137	88.3	3,075.7 (1,607.1) [123.9-8,519.2]	42.7 (12.5) [6-80]
Spain	ISSP,1993	100-117-139	76.3	1,033.8 (623.5) [193.3-3,867]	40.8 (9.7)[8-84]
	ISSP,1995	92-95-133	76.7	952.8 (496.9) [174.9-2,429.5]	39.8 (9.4) [8-70]
	ISSP,1997	98-125-129	57.4	1,008.4 (614.7) [189.3-3,786.2]	30.9 (20.9) [0-80]
	ISSP,1998	173-185-238	77.8	1,231.2 (598.7) [299.6-3,370.1]	43.5 (8.7) [25-80]
	ISSP,2000	50-51-62	90.3	1,303.7 (683.4) [181.1-3,259.3]	43.3 (6.4) [35-60]
	ISSP,2002	167-209-230	65.7	1,552.8 (974.1) [271-7,589.1]	44.3 (11.6)[8-90]
	ECHP,2001	865-1,107-1,220	70.5	1,938 (1,329.2) [119.3-15,442.8]	44.6 (10.2) [15-96]
	SHARE,2004	94-107-129	79.1	1,651.7 (2,321.5) [.5-20,889.5]	41.7 (14.7) [0-80]
Sweden	ISSP,1994	171-173-175	92	1,516 (576.2) [270.3-2,703.3]	41.1 (8.1)[9-84]
	ISSP,1995	144-139-148	85.1	1,573.2 (508.2) [177.7-2,487.5]	43.4 (6.7) [30-70]
	ISSP,1996	177-172-185	86.5	1,694.7 (628.6) [284.9-2,848.5]	43.6 (9.2) [1-75]
	ISSP,1998	125-125-132	87.9	2,009.7 (2,093) [470.3-23,512.7]	42.7 (10.9) [8-90]
	ISSP,2000	137-126-141	93.6	2,350.8 (2,042) [556.1-23,169.6]	43.6 (8.8) [10-80]
	ISSP,2002	139-139-143	75.6	2,146.9 (1,285.3) [698.1-13,001.6]	40.9 (9.1) [8-75]
	SHARE,2004	520-526-638	82.6	2,354.5 (1,881.2) [2.1-21,840.4]	42.6 (10.8) [1-100]

Note: see Table C. 6. `n.a.` stands for “not available”.

Table C. 9: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Females.

Country	Survey Year	Sample size wage-hours-status	Prop. Empl.	Average (standard error) [min, max] Wage/month	Hours/week worked
Austria	ISSP,1994	84-81-143	58.0	947.8 (490.0) [382.0-2,368.6]	34.9 (14.3) [3-84]
	ISSP,1995	112-87-158	56.3	921.5 (535.3) [310.0-2,526.7]	38.2 (17.8) [4-80]
	ISSP,1998	88-74-165	41.2	1040.9 (593.9) [300.1-3,150.8]	38.1 (16.5) [5-84]
	ISSP,2000	88-0-151	31.1	976.9 (539.8) [225.0-2,850.5]	n.a.
	ISSP,2001	126-0-151	25.2	658.3 (614.4) [0 - 2,750.3]	n.a.
	ECHP,2001	214-277-646	30.8	1535.5 (935.8) [57.0-5-283.6]	40.8 (15.6) [15-96]
	SHARE,2004	122-151-489	23.5	1,270 (2,208.1) [.1-13-416.5]	33.2 (15.6) [0-100]
Belgium	ISSP,2002	27-138-170	75.3	1,675.0 (2,645.7) [266.1-14,751.6]	34.7 (13.1) [6-85]
	ECHP,2001	454-460-564	78.2	1,664.8 (761.4) [106.6-6,272.7]	35.5 (10.0) [15-84]
Denmark	ISSP,1997	156-148-168	79.8	1,936.9 (861.7) [455.5-5,826.2]	34.6 (7.4) [5-50]
	ISSP,1998	147-130-153	77.1	2,085.2 (816.7) [665.9-6,183.7]	35.8 (7.0) [18-75]
	ISSP,2000	96-88-98	89.8	2,273.8 (867.6) [645.1-5,068.9]	35.5 (8.4) [10-60]
	ISSP,2001	134-119-136	86.0	2,212.8 (960.5) [443.6-5,322.6]	35.5 (7.2) [20-80]
	ISSP,2002	197-192-201	80.1	2,209.5 (919.6) [607.9-5,644.4]	36.1 (6.5) [15-60]
	ECHP,2001	434-441-511	83.6	2,230.8 (793.7) [114.6-8,622.7]	35.5 (8.2) [15-90]
	SHARE,2004	179-179-207	83.6	2,134 (886.2) [1.7-6,572.7]	34.6 (9.6) [0-60]
Finland	ISSP,2001	164-132-187	65.2	1,350.1 (727.4) [1.1-4,161.6]	36.8 (8.5) [2-94]
	ISSP,2002	140-118-159	68.6	1,524 (1,323) [28.1-12,827.3]	35.8 (7.8) [7-71]
	ECHP,2001	509-559-653	77.0	1,488.8 (600.0) [166.5-5,548.8]	37.7 (8.3) [10-96]
France	ISSP,1996	105-90-121	80.2	1,579 (1,027.6) [317.9-7,153.4]	35.8 (9.8) [6-65]
	ISSP,1997	129-144-144	84.7	1,450.7 (863.1) [234.2-5,465.2]	28.7 (16.0) [0-77]
	ISSP,1998	105-95-122	81.1	1,621.7 (933.8) [307.1-5,374.1]	37.1 (8.9) [14-67]
	ISSP,2002	259-212-306	64.4	1,379.7 (798.2) [276.2-4,830.4]	34.2 (10.4) [7-70]
	SHARE,2004	159-164-228	72.8	1,672.6 (2,025.5) [.1-22,021.8]	35.7 (14.3) [0-105]
Greece	ECHP,2001	410-591-1,020	39.6	1,103.6 (444.6) [46.3-3,051.9]	38.4 (9.6) [15-75]
	SHARE,2004	39-44-112	36.6	1,283.5 (766.9) [69.1-4,013.9]	29.1 (20.4) [0-66]

Note: see Table C. 6. `n.a.` stands for “not available”.

Table C. 10: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Females.

Country	Survey Year	Sample size	Prop.	Average (standard error) [min-max]	
		wage-hours-status	Empl.	Wage/month	Hours worked
Germany	ISSP,1993	62-68-128	53.9	1,099.6 (662.8) [106.5-3,726.9]	32.5 (10.3) [12-60]
	ISSP,1995	113-89-139	60.4	878.9 (594.6) [259.3,2,593.0]	28.4 (10.8) [9-50]
	ISSP,1996	158-0-265	59.6	1,051.1 (737.2) [70.6-6,186.3]	n.a.
	ISSP,1997	121-139-142	50.7	1,027.1 (740.5) [254.1-3,049.8]	22.2 (18.2) [0-80]
	ISSP,1998	71-89-139	50.4	999.2 (518.6) [251.6-2,390.3]	30.8 (12.6) [3-80]
	ISSP,2000	98-108-148	65.5	1,078.3 (629.5) [25.7-3,695.7]	29.6 (13.2) [1-80]
	ISSP,2002	81-67-111	54.0	1,187.2 (1,404.5) [190.6-11,437.1]	33.6 (11.6) [15-80]
	ECHP,2001	368-338-522	61.4	1,611.6 (1,130) [24.5-9,334,1]	34.1 (11.4) [2-85]
Ireland	ISSP,1993	135-78-164	46.3	582.4 (485.1) [142.9-2,573.1]	35.6 (9.2) [7-60]
	ISSP,1994	114-72-157	45.9	793 (644.1) [221.4-4,427.7]	36.3 (11.3) [5-72]
	ISSP,1995	150-159-162	53.7	812.1 (755.1) [145.7-4,080.1]	37 (17.1) [5-96]
	ISSP,1996	149-158-162	49.4	766.3 (743.2) [143.2-4,012]	37.9 (19) [5-96]
	ISSP,2000	0-103-196	55.1	n.a.	30.3 (13.8) [4-99]
	ECHP,2001	277-269-493	50.9	1,333.5 (899.5) [47.5-4,926.1]	30 (10.6) [9-90]
Italy	ISSP,1993	68-64-126	49.2	1,062.4 (560.9) [103.4-3,513.9]	33.7 (10.8) [7-60]
	ISSP,1994	80-76-130	56.9	1,164 (752) [166.3-4,356.8]	37 (13.1) [10-90]
	ISSP,1997	28-113-116	33.6	1,808.9 (1,073.7) [664.1-4,829.5]	14.5 (18.5) [0-60]
	ISSP,1998	46-44-108	41.6	1,025.2 (783.7) [119.5-4,899.5]	35.5 (13.7) [6-60]
	ECHP,2001	464-571-1,376	33.2	1,506 (592.2) [224-5,040.2]	34.9 (10.1) [15-80]
	SHARE,2004	126-162-516	28.4	1,595.3 (1,672.2) [.3-12,408.6]	29.4 (15.3) [0-70]

Note: see Table C. 6. `n.a.` stands for "not available".

Table C. 11: Descriptive statistics. Earnings (at 2000 prices and in PPP units), hours worked, and proportion employed. 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. Females.

Country	Survey Year	Sample size wage-hours-status	Prop. Empl.	Average (standard error) [min, max] Wage/month	Hours worked
Netherlands	ISSP,1993	298-176-362	38.7	1,672.5 (800.5) [183.8-5,025.1]	24.7 (12.2) [2-60]
	ISSP,1994	325-187-371	38.3	1,673.2 (821.6) [280.7-5,413.8]	25.1 (13.2) [1-60]
	ISSP,1995	330-223-392	45.1	1,824.3 (893.2) [179.8-4,913.8]	23.3 (11.4) [2-56]
	SHARE,2004	141-145-206	66.0	1,609.9 (1,805) [46.5-19,129.4]	25.9 (11.5) [4-80]
Spain	ISSP,1993	67-47-132	31.8	766.7 (530.3) [193.3-2.685.4]	38.2 (14) [6-96]
	ISSP,1995	55-50-144	34.7	836.5 (528.9) [174.9-1,846.4]	33.8 (12.5) [4-66]
	ISSP,1997	82-130-133	30.1	572.5 (489.9) [189.3-2,366.4]	12.5 (17.6) [0-60]
	ISSP,1998	107-92-242	37.6	921.4 (555.9) [299.6-2,621.2]	36.6 (10.1) [9-60]
	ISSP,2000	48-45-112	41.1	848.8 (505.5) [181.1-1,810.7]	36.1 (11.1) [10-70]
	ISSP,2002	104-121-231	46.3	959.6 (524.3) [271-2927.2]	34.4 (10) [6-55]
	ECHP,2001	527-607-1,215	40.9	1,389.1 (888.3) [54.7-6,941.7]	37.7 (10) [14-90]
	SHARE,2004	97-107-225	42.7	1,958.3 (6,651.9) [20.9-62,668.5]	34.3 (13.4) [0-70]
Sweden	ISSP,1994	116-151-170	82.9	1,093.8 (471.4) [270.3-2,703.3]	34.9 (8.7) [10-60]
	ISSP,1995	170-159-173	88.4	1,774.8 (423.6) [177.7-2,487.5]	36.3 (7.6) [8-55]
	ISSP,1996	152-152-167	85.6	1,255.0 (506) [284,9-2,848.7]	35.6 (9.8) [8-60]
	ISSP,1998	160-155-172	83.7	1,429 (660.7) [94.1-4,984.7]	36.2 (9) [8-72]
	ISSP,2000	119-111-129	83.7	1,820.9 (1,552.9) [556.1-16,682.1]	36.9 (6.7) [15-50]
	ISSP,2002	118-117-127	84.3	1,638.7 (755.3) [436.3-7,417]	36.5 (9.8) [5-75]
	SHARE,2004	520-526-638	79.8	1,707.9 (1,142.9) [.7-16,016.3]	36.3 (10.3) [0-80]

Note: see Table C. 6. `n.a.` stands for “not available”.

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