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Education and Earnings in Europe

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Education and Earnings in Europe^{*}

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Abstract

Schooling increases earnings but getting causal information on the impact of schooling requires generating evidence from policy changes. In this paper, we estimate causal effects of additional education on earnings using new compulsory schooling law data across 16 European countries. We use recent and comparable microdata and analyze the impact of reforms on schooling and earnings. Results show that a year of education substantially increases earnings, especially for those with tertiary education.

JEL: I26; J31; C26

Keywords: Returns to education; compulsory schooling reforms; instrumental variables; causal inference; human capital; earnings; Europe

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1. Introduction

Few findings in economics are as well established as the positive relationship between education and earnings. Evidence from thousands of studies across diverse countries consistently shows that additional schooling is associated with higher wages (Psacharopoulos and Patrinos 2018; Montenegro and Patrinos 2023). The central question, however, is whether this association reflects the causal effect of education. Since individuals choose how much schooling to acquire, rather than being randomly assigned to different levels of education, conventional estimates may be biased by endogeneity arising from selection, omitted variables, or measurement error (Card 2012).

A widely used approach to identifying the causal returns to education exploits policy reforms that induce some individuals to remain in school longer (Card 2001). Such reforms generate plausibly exogenous variation in educational attainment, creating natural experiments that allow researchers to estimate the causal effect of schooling on earnings. Because these policy-induced changes are unrelated to many of the factors that ordinarily influence schooling decisions, they reduce bias arising from selection and omitted variables. When the reforms produce substantial increases in educational attainment, they identify the earnings gains attributable to additional schooling for the affected individuals. Reviews of this literature suggest that an additional year of schooling raises earnings by approximately 10 percent (Psacharopoulos and Patrinos 2026).

In this paper, we address the challenge of causal inference by assembling a new database of compulsory schooling laws (CSLs) (Angrist et al. 2026) to estimate the causal effect of educational attainment on earnings across Europe. Compulsory schooling laws (CSLs) have long served as a source of plausibly exogenous variation in educational attainment in labor and education economics (Angrist and Krueger 1991; Oreopoulos 2006). Owing largely to data limitations, however, most studies have focused on individual countries. Although several multi-country analyses have been conducted for Europe (Brunello et al. 2009; Harmon et al. 2001; Trostel et al. 2002), they rely on a limited set of reforms. This paper makes three main contributions. First, we assemble a new database identifying 17 compulsory schooling reforms across 16 European countries. Second, we provide harmonized causal estimates of the returns to education using

comparable microdata from EU-SILC. Third, we offer one of the most comprehensive cross-country assessments of causal returns to education in Europe.

Existing European evidence suggests that compulsory schooling reforms provide credible identification of the causal returns to education. Brunello et al. (2009) show that reforms raising the minimum school-leaving age increased educational attainment, particularly among individuals with lower initial ability. Trostel et al. (2002) document substantial heterogeneity in returns across European countries, highlighting the importance of institutional and labor market differences. Harmon et al. (2001) compare alternative instrumental-variable estimators across 15 European countries and conclude that IV estimates generally exceed OLS estimates. They attribute this finding to the fact that policy reforms primarily affect individuals with relatively low levels of education, whereas family-based instruments generate estimates closer to OLS because siblings exhibit relatively little variation in educational attainment.

Europe provides an ideal setting for this study. Throughout the twentieth century, European countries implemented numerous compulsory schooling reforms that increased the minimum years of required education. The region also benefits from large, harmonized household surveys, enabling consistent within- and cross-country analyses. In particular, the European Union Statistics on Income and Living Conditions (EU-SILC) provides comparable information on educational attainment and earnings across countries and forms the basis of our empirical analysis.

Educational attainment has continued to increase throughout Europe in recent decades. Average years of schooling increased from 10.8 to 11.1 years in Eastern Europe and Central Asia between 2010 and 2015 (Barro and Lee 2013). Over the same period, average schooling in EU countries and closely integrated European economies (Norway, Switzerland, and the United Kingdom) increased from 11.9 to 12.1 years.

The importance of accurately estimating the returns to education extends beyond individual earnings. Europe's transition toward a knowledge-based economy has increased demand for highly educated workers and reinforced the importance of human capital for innovation and long-run growth (Dalvit et al. 2023; Akcigit et al. 2025).

Our estimates indicate that education has a significant positive effect on earnings across Europe. An additional year of schooling raises earnings by between 5 and 9 percent, while the average return to tertiary education exceeds 9 percent. We also document substantial heterogeneity across countries, with returns increasing in 12 of the 16 countries examined. Overall, our estimates are broadly consistent with the international evidence suggesting average causal returns of approximately 10 percent per additional year of schooling (Psacharopoulos and Patrinos 2026).

The rest of the paper is organized as follows. The next section describes our data. Section 3 details our empirical strategy and Section 4 presents our results. Section 5 concludes.

2. Data

Our main data source is the European Union Statistics on Income and Living Conditions (EU-SILC) 2022 survey. The EU-SILC collects data on income, poverty, social exclusion, and living conditions across EU countries and other partner countries. The EU-SILC 2022 edition was collected in 25 out of the 27 EU countries (Germany and Malta did not participate) plus Switzerland. We use previous editions of the survey to incorporate two more partner countries into the analysis: Norway (2020) and Serbia (2021). Besides collecting sociodemographic information, this survey asks the participants about their employment status and annual wages in the previous year. Hence, our returns to education analysis corresponds to 2021.

EU-SILC respondents report their highest educational attainment (or level). We translate educational attainment into schooling years using the [ISCED 2011](#) framework applicable to each country included in the database.

Our sample is limited to positive employees aged between 25 and 65 years old. We include only positive wage earners in our sample following the standard practice in the literature to estimate the returns to education (Mincer 1974; Card 1999) and to implement the IV analysis (Angrist and Krueger 1991; Oreopoulos et al. 2006). The age restriction is imposed to focus the analysis on those who have completed their educational trajectory (age 25) and considering that the average statutory retirement age in the EU is 65 years old. This gives us a pooled cross-country sample of 27 countries with 186,808 observations. The summary statistics of our sample are in Annex Table 1.

We use the new dataset on Compulsory Schooling Laws (CSLs) in Europe and Central Asia (Angrist et al. 2026) to analyze the causal effect of education on earnings. This dataset contains CSLs taking place in the region between 1763 and 2020. We carefully revised which CSLs targeted the age cohorts included in our sample (those born between 1956 and 1991) and found that such reforms took place between 1962 and 1997.

To calculate the first birth cohort affected by each reform, we need the number of compulsory schooling years prior to the reform and the school starting age. The dataset provides the former for each CSL and the latter for most of them. We assume the most common starting age (6) in the dataset for those CSLs without this information. We follow a two-step strategy to identify the first birth cohort affected by each reform: (i) we add the compulsory schooling years prior to the reform to the school starting age to estimate the school leaving age, then (ii) we subtract the compulsory school leaving age prior to the reform from the year when the reform was implemented. Once the birth cohort is identified, we assume that the following five birth cohorts were also affected by the reform before it was internalized.

3. Empirical Strategy

3.1. OLS estimates

We use the Mincerian function method to estimate the returns to schooling. This method relies on the fitting of a function of log-wages ($\ln W$), with years of schooling (S), years of labor market experience (EX) and its square as independent variables:

$$\ln W_i = \alpha + \beta S_i + \gamma_1 EX_i + \gamma_2 EX_i^2 \quad (1)$$

We construct the labor market experience variable following the standard practice in the literature (Patrinos 2024), which implies using the following formula: age - S - 6, with age 6 being the usual school starting age and assuming that experience begins after the completion of schooling.

The coefficient on years of schooling, β , can be interpreted as the rate of return to one additional year of schooling.

The earnings function method can be used to estimate returns to education at different levels by converting the continuous years of schooling variable (S) into a series of binary variables, say D_{SEC} and D_{TER} , indicating that a person has completed the corresponding level of education. People with less than secondary education should also be included in the sample to avoid matrix singularity. The extended earnings function to estimate the returns by level of education is the following:

$$\ln W_i = \alpha + \beta_s SEC_i + \beta_t TER_i + \gamma_1 EX_i + \gamma_2 EX_i^2 + \varepsilon_i \quad (2)$$

where SEC and TER refer to complete secondary and complete tertiary education, and the reference category is less than complete secondary; β_s and β_t are the coefficients on secondary and tertiary education, respectively. After fitting this extended earnings function, the rate of return to levels of education can be derived from the following formulas (3 and 4):

$$r_s = \frac{\beta_t}{S_{SEC}} \quad (3)$$

$$r_t = \frac{\beta_t - \beta_s}{S_{TER} - S_{SEC}} \quad (4)$$

where S_{SEC} and S_{TER} stand for the total number of years of schooling required to obtain a secondary diploma and a tertiary degree.

3.2. IV using CSLs

We implement the Instrumental Variable (IV) approach using Two-State Least Squares (2SLS) to address the potential endogeneity of education. We instrument education using the CSLs because they are commonly used in the economics literature as an instrument for educational attainment. This is because this type of education reform induces changes in schooling but does not change wages.

We use Angrist et al. (2026) CSL database to identify the educational reforms targeting the birth cohorts included in our sample of countries. Although most of our IV analysis uses 2021 data, we resort to previous EU-SILC data (2018 - 2020) to cover the largest possible extent of CSLs. After identifying the reforms implemented in the countries included in our sample, we limit the CSLs considered in our analysis to those targeting people born between 1956 and 1996. This reflects the

fact that our sample is restricted to the employed population aged 25 to 65. Although our data include individuals born in 1953 (aged 65 in 2018), we set the lower bound year to 1956 to ensure at least three pre-reform birth cohorts are available to implement the IV. The 1996 bound corresponds to the birth year of individuals aged 25 in 2021. This process yields 32 CSLs targeting part of our sample (Annex Table 2).

Following Angrist et al. (2026), we consider only CSLs that legally mandated an increase in schooling and were binding. This involves a three-step process. First, we identify reforms implemented to increase compulsory schooling, excluding reforms aimed at restructuring the education system rather than expanding access to schooling. This is the case for the reforms implemented between 1990 and 1992 in Czechia, Estonia, Latvia, Lithuania, and the Slovak Republic, where Soviet-era education systems were replaced with Western basic-education benchmarks and general secondary education was reestablished (Greger and Walterová 2018; Filer et al. 1999). Second, we select reforms that aimed to increase compulsory schooling above the average educational attainment of the population at the time the reform was implemented. We identify such reforms by comparing the new compulsory schooling years enforced by each reform with the closest available average educational attainment of the population reported in Barro and Lee (2013). These first two steps of the selection process yield 20 reforms aiming at increasing compulsory schooling beyond the population's existing educational attainment for our sample of countries.

The final step of the CSL selection process is to identify the binding reforms. While the binding condition may seem obvious, and this relationship holds for many reforms, it does not necessarily hold for all. A CSL is binding if it is enforced and rolled out rapidly, inducing a longer stay in school for those students who would have dropped out earlier in the absence of the reform. A binding CSL corresponds to a significant schooling coefficient in the first stage in the IV approach. While legally enforceable, changes to CSLs will only have a strong first stage if they affected a large enough share of the intended population (Oreopoulos 2006). Annex Table 3 presents the 12 binding compulsory schooling reforms considered in our analysis, including the compulsory schooling duration before and after each reform, the intended versus actual increase in schooling, and the latest data year in which affected cohorts are still active in the labor market.

Another necessary condition for the IV to work is that the CSL must affect labor market earnings through the educational attainment channel and not be confounded by other factors. Given that the passing of CSLs is a national, exogenous shock, resulting gains in education are largely orthogonal to other factors that would otherwise make the individual schooling decision endogenous. CSLs overcome this confounder by compelling individuals to obtain greater educational attainment, regardless of these factors.

The plausibility of the assumption that CSLs affect earnings only through the education channel is further bolstered by the fact that most of the possible effects of CSLs on other mediating factors likely increase as a direct result of the education channel. This means our estimate is the bundled effect of education, including changes in income and other mediators, which come with an exogenous increase in schooling.

We run a two-stage least squares regression where the second stage regresses the log of wage earnings on predicted education based on the applicable CSLs. For a given individual i we estimate:

$$S_i = \alpha + \beta CSL_i + \gamma_1 X_i + \gamma_2 X_i^2 + \varepsilon_i \quad (5)$$

$$Y_i = \alpha + \beta \hat{S}_i + \gamma_1 X_i + \gamma_2 X_i^2 + \varepsilon_i \quad (6)$$

where CSL_i is a binary indicator of whether an individual i is a member of a cohort affected by the reform and is therefore in the treatment group. We estimate effects across multiple countries and reforms. In Equation (5) we estimate the first stage of the effect of CSLs on educational attainment S_i . We estimate Equation (6), the causal effect of additional education on earnings Y_i , with 2SLS, where the first stage is estimated from Equation (5) with educational attainment instrumented by CSL reforms. Y_i is the log of hourly wages, and X_i is experience; β is interpreted as the rate of return to schooling; namely, the percentage change in wages due to an additional year of schooling (Mincer 1974)

This specification mirrors those most common in the economics literature (Acemoglu and Angrist 2000; Oreopoulos 2006). It is important to note that these strategies all identify Local Treatment Effects (LATE) of education that are applicable to individuals on the margin of dropping out in

the absence of the CSL. This is the policy-relevant estimate if the policy in question is to increase minimum schooling requirements.

4. Results

4.1. OLS results

The OLS estimation shows that there is a positive and significant association between schooling and earnings. The return to an additional year of schooling in the 28 EU countries included in our sample is, on average, 9.6 percent (Table 1). This is in line with the global average rate of return to schooling (Psacharopoulos and Patrinos 2018). Norway shows the lowest return to schooling (3.0 percent) while the higher return to schooling takes place in Cyprus (13.5 percent) (Annex Table 4).

Table 1. Average OLS returns to schooling and by education attainment

Years of Schooling	Secondary	Tertiary	N countries	N observations
9.6	2.2	9.7	27	1,868,080

Notes: Estimates are based on 2021 data, except for Switzerland and Serbia (2020) and Norway (2019), for which the most recent year of EU-SILC participation was used reported in Annex Table 4.

Source: Own estimates using EU-SILC 2019 - 2022.

When analyzing the returns by education level, the average returns to secondary and tertiary education are 2.2 and 9.7 percent, respectively. Low returns to secondary education are common in countries where the average educational attainment of the population is close to secondary, which is the case of most European countries. The returns to tertiary education range from a minimum of 3.5 percent in Sweden to a maximum of 14.9 in Ireland.

4.2. IV results

Following Angrist et al. (2026) and Oreopoulos (2006), we carefully identify the CSLs for which there is a binding first stage – that is, where an increase in required years of schooling by the reform increases average educational attainment, net of the time trend, for a large enough share of the intended population. As mentioned earlier, a binding CSL corresponds to a statistically significant first-stage coefficient in the IV estimation. Applying this criterion, we find 12 binding reforms for our sample of countries (Annex Table 5).

The CSL reforms increased the statutory years of compulsory schooling by between one and six years, with an average increase of 2.3 years. Although the increase in schooling varies considerably across reforms according to the first stage of the IV, the average increase of schooling resulting from these reforms amounts to 1.4 years.

The second stage of the IV estimation uses the predicted values of schooling from the first stage to estimate the causal return to education for compliers. A statistically significant coefficient on predicted schooling indicates that additional schooling increases earnings for this group. Using this approach, we find significant returns to schooling for compliers in 8 of the 12 binding CSL reforms (Table 2). The four reforms that do not yield statistically significant returns increased average schooling by no more than 0.7 years. Although these reforms were intended to raise schooling by an average of 3 years, they increased educational attainment by only 0.5 years on average. This limited first-stage effect is consistent with a relatively small group of compliers, reducing the statistical power to detect significant earnings effects.

Since the second stage of the IV estimation identifies the local average treatment effect (LATE) for compliers, an insignificant estimate should not be interpreted as evidence that education does not raise earnings in those countries. Instead, it indicates that we are unable to detect a statistically significant effect of the additional schooling induced by the reform for individuals whose educational attainment changed because of the policy. One possible explanation is that the additional schooling generated relatively little additional human capital because of low educational quality (Clark 2023), thereby limiting its effect on subsequent earnings. Other explanations, including limited statistical power, cannot be ruled out.

Although none of these reforms directly targeted post-secondary education, they increased the probability of attaining tertiary education for the affected population (Annex Table 4). The reforms increased schooling beyond the new compulsory level by reducing dropout, increasing secondary school completion, and expanding the pool of students eligible for tertiary education, thereby reducing talent misallocation. On average, individuals exposed to the reforms were 14.7 percentage points more likely to attain tertiary education than they would have been in the absence of the reforms. This finding is consistent with Meghir and Palme (2005), who show that a

compulsory schooling reform in Sweden increased educational attainment beyond the new compulsory level, particularly among high-ability individuals from disadvantaged backgrounds.

The second stage of the IV reveals significant returns to tertiary education for 10 of the 12 binding CSLs explored before. On average, those affected by the reforms who continue their studies until completing tertiary education earned an estimated average return of 10.3 percent (Table 2), with a minimum estimate of 4.6 percent in Norway and a maximum estimate of 17.6 percent in Ireland. This IV average closely matches the corresponding OLS estimate of 9.8 percent for the same set of countries, indicating that selection bias is not a major concern for the tertiary education margin and that the OLS and IV estimates are broadly consistent with one another.

The stronger evidence for tertiary education than for additional years of schooling may reflect Europe's already high average level of educational attainment. In such settings, marginal increases in compulsory schooling may generate relatively modest earnings gains, whereas completing tertiary education continues to yield substantial labor-market returns. These findings suggest that the reforms were particularly valuable for individuals who, in the absence of the policy change, would not have continued to higher education.

Table 2. Returns to schooling years and higher education using OLS and IV

Country	CSL year	Schooling years		Higher education	
		OLS	IV	OLS	IV
Cyprus	1962	13.2 ***	7.4 ***	14.8 ***	12.9 ***
Denmark	1972	10.2 ***	4.6	6.4 ***	4.9 ***
Estonia	1980	9.7 ***	-8.5	8.6 ***	5.6
Finland	1974	7.6 ***	-4.7	8.4 ***	4.5 ***
Greece	1976	8.6 ***	7.0 ***	9.5 ***	9.3 ***
Ireland	1972	12.4 ***	17.7 ***	14.9 ***	17.6 ***
Latvia	1980	12.0 ***	-14.8	10.7 ***	6.3
Luxembourg	1977	8.3 ***	7.8 ***	10.0 ***	11.0 ***
Netherlands	1969	9.4 ***	14.7 ***	9.1 ***	12.8 ***
Netherlands	1973	8.0 ***	8.6 ***	8.7 ***	10.2 ***
Norway	1969	4.6 ***	4.5 ***	4.5 ***	4.6 ***
Spain	1970	10.7 ***	12.3 ***	11.8 ***	15.1 ***
Average ^(a)		9.6	6.7	9.8	8.6
Median ^(a)		9.6	7.2	9.3	9.7
Average of significant IV and their matching OLS		9.4	10.0	9.8	10.3

Notes: (a) = considering non-significant coefficients equal to 0. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own estimations based on EU-SILC (2019 to 2022).

4.3. Returns to schooling using OLS and IV estimates, ECA and EU

As an additional robustness check, we estimate causal returns to education for countries in Eastern Europe and Central Asia (ECA) using father's schooling as an instrument and compare these estimates with the compulsory-schooling-law IV estimates for the European Union (EU) (Table 3). CSLs are not used as an instrument for ECA countries because their smaller sample sizes relative to EU countries (717 observations in ECA versus 7,092 in EU, on average) provide insufficient statistical power to identify the binding reforms. We, therefore, use father's schooling as our second-best instrument for education, while acknowledging that family background variables may not fully satisfy the strict validity assumptions of IV analysis, as they can be correlated with other income-related characteristics, such as family wealth and occupation or industry of employment (Trostel et al. 2002; Hoogerheide et al. 2012).

The results of the IV analysis for both groups of countries (ECA and EU) are broadly consistent with the existing literature. In both regions, the estimated causal effect of education on earnings is positive whenever it can be identified. In the ECA sample, IV estimates generally exceed the

corresponding OLS estimates, with the average return to an additional year of schooling increasing from 4.7 percent under OLS to 10.9 percent under IV. Returns to tertiary education are also higher under IV. In the EU, the average IV estimate for years of schooling across all reforms is lower than the corresponding OLS estimate because it includes reforms with statistically insignificant second-stage estimates. Restricting the comparison to reforms with statistically significant estimated causal effects, however, raises the average IV return to 10.0 percent, exceeding the corresponding OLS estimate of 9.4 percent. Similarly, the average IV return to tertiary education increases from 8.6 percent across all reforms to 10.3 percent when only reforms with statistically significant estimated causal effects are considered, compared with an OLS estimate of 9.8 percent. Overall, the ECA and EU results point to the same conclusion: when causal effects can be credibly identified, IV estimates are generally comparable to, or larger than, conventional OLS estimates.

Table 3. Returns to education: OLS and IV estimates in Europe and Central Asia and the European Union (%)

Country	OLS	IV	Difference (IV-OLS)	OLS	IV	Difference (IV-OLS)
<i>Europe and Central Asia</i>				<i>Tertiary education</i>		
<i>Years of schooling</i>						
Armenia	4.2	4.9	0.7	10.2	11.4	1.2
Bosnia and Herzegovina	5.6	8.8	3.2	9.8	13.3	3.5
Kazakhstan	5.9	16.4	10.4	6.3	10.0	3.7
Kosovo	5.5	9.3	3.8	10.7	14.3	3.7
Moldova	0.6	..		5.7	7.6	1.9
Montenegro	8.9	16.7	7.8	11.0	13.1	2.2
North Macedonia	3.5	4.6	1.1	7.0	8.3	1.3
Russia	5.1	22.2	17.1	6.0	11.6	5.6
Serbia	5.8	7.3	1.5	10.0	12.2	2.2
Tajikistan	3.3	7.9	4.5	4.4	7.1	2.7
Ukraine	1.4	..		2.1	4.3	2.1
Uzbekistan	7.0	..		9.2	11.4	2.2
ECA average	4.7	10.9	5.6	7.7	10.4	2.7
<i>EU estimates from Table 2</i>						
EU average (all reforms)	9.6	6.7	-2.9	9.8	8.6	-1.2
EU average (significant reforms only)	9.4	10.0	0.6	9.8	10.3	0.5

Notes: .. = IV estimate unavailable because the compulsory schooling reform did not produce a statistically significant first stage. The instruments are father's schooling for ECA and CSL for EU.

Source: Own estimations based on LiTS 2016 for ECA, EU-SILC for EU.

5. Conclusion

This paper provides new evidence on the causal returns to education in Europe using a newly assembled database of compulsory schooling reforms and harmonized EU-SILC microdata. Exploiting the exogenous variation in educational attainment generated by these reforms, we estimate the causal effect of additional schooling on earnings for individuals whose education was affected by the policy changes.

Our results provide strong evidence that additional education causally increases earnings. Conventional Mincerian estimates imply average returns of about 9 percent per additional year of schooling across Europe, while instrumental-variable estimates based on compulsory schooling reforms confirm that these returns are causal. Although returns vary across countries and reforms, the estimated causal effects are broadly consistent with the international literature. We also find

that reforms increased tertiary educational attainment and that returns to tertiary education remain substantial despite the expansion of higher education across Europe.

An important finding is that compulsory schooling reforms increased educational attainment beyond the new legal minimum. By reducing dropout and increasing secondary school completion, the reforms increased the probability of attending/completing tertiary education and increased the likelihood that affected cohorts obtained university degrees. This suggests that the long-run effects of schooling reforms extend beyond compliance with minimum schooling requirements and contribute to the accumulation of advanced skills that are increasingly valued in modern labor markets.

These estimates have important implications for education policy. Compulsory schooling reforms not only increase years of education but also promote progression to tertiary education, generating long-run labor-market benefits that extend beyond compliance with minimum schooling requirements. As Europe continues its transition toward a knowledge-based economy, policies that expand educational attainment and facilitate progression to higher levels of education are likely to yield substantial economic returns. More broadly, our results reinforce decades of evidence showing that education remains one of the most productive investments individuals and societies can make. Taken together, they demonstrate that policies promoting higher educational attainment can generate substantial long-run economic benefits, even in countries where average schooling levels are already high.

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Annex Table 1. Basic statistics

Variable	Austria	Belgium	Bulgaria	Croatia	Cyprus	Czech Republic	Denmark
Wage per hour	22.4 (12.0)	23.7 (11.8)	3.9 (3.0)	6.8 (3.3)	12.7 (9.4)	7.9 (3.7)	35.1 (16.0)
Schooling years	12.7 (2.1)	13.7 (3.0)	12.8 (2.9)	11.1 (2.0)	13.0 (2.6)	13.3 (1.8)	13.6 (1.7)
Experience	25.5 (10.8)	23.5 (11.2)	28.0 (10.9)	28.8 (11.1)	25.2 (11.5)	26.3 (10.6)	27.1 (12.0)
Share (%) of the population with:							
Less than complete secondary	5.0	9.1	12.2	7.7	12.3	3.5	9.2
Complete secondary	53.5	32.2	57.4	63.2	38.8	70.1	38.7
Complete tertiary	41.5	58.7	30.4	29.1	48.9	26.4	52.1
Obs.	4,213	4,779	5,996	5,357	3,699	6,022	4,626
Variable	Estonia	Finland	France	Greece	Hungary	Ireland	Italy
Wage per hour	8.8 (6.0)	24.1 (21.3)	17.7 (10.2)	8.2 (4.2)	5.7 (3.6)	29.4 (20.7)	14.8 (8.6)
Schooling years	13.3 (1.9)	14.4 (2.1)	12.1 (2.3)	12.7 (2.7)	12.6 (3.1)	14.7 (2.5)	12.5 (3.2)
Experience	26.8 (10.8)	26.1 (10.8)	26.9 (10.6)	26.5 (10.6)	27.1 (11.3)	25.8 (10.6)	28.0 (11.2)
Share (%) of the population with:							
Less than complete secondary	8.5	4.1	12.6	16.3	11.6	6.7	24.6
Complete secondary	45.0	36.9	41.6	42.5	60.5	25.3	48.1
Complete tertiary	46.5	59.0	45.8	41.2	27.9	68.1	27.3
Obs.	4,650	7,708	12,764	4,714	5,587	3,800	12,479
Variable	Lithuania	Luxembourg	Latvia	Netherlands	Norway	Poland	Portugal
Wage per hour	8.7 (6.6)	38.8 (23.6)	7.4 (5.2)	26.7 (13.5)	30.0 (14.3)	6.5 (3.5)	8.6 (6.5)
Schooling years	13.6 (1.7)	13.2 (3.1)	13.3 (1.7)	12.8 (1.9)	13.2 (3.2)	13.5 (3.0)	10.8 (3.5)
Experience	28.1 (11.0)	24.1 (10.4)	27.4 (11.1)	28.1 (11.6)	26.0 (11.1)	25.1 (11.1)	30.0 (11.7)
Share (%) of the population with:							
Less than complete secondary	4.0	11.2	5.3	12.8	13.7	3.6	40.2
Complete secondary	47.0	32.3	49.7	36.9	32.5	54.2	28.8
Complete tertiary	49.0	56.5	45.0	50.3	53.8	42.2	31.0
Obs.	4,270	3,323	4,441	10,227	6,034	12,633	10,216
Variable	Romania	Serbia	Slovakia	Slovenia	Spain	Sweden	Switzerland
Wage per hour	5.5 (2.4)	3.1 (1.7)	5.3 (2.2)	12.8 (7.2)	13.0 (8.8)	22.0 (10.9)	41.5 (22.4)
Schooling years	12.6 (1.8)	12.0 (1.6)	13.2 (2.0)	13.8 (2.2)	12.0 (2.6)	13.8 (1.6)	13.4 (1.5)
Experience	26.1 (9.8)	25.4 (10.8)	26.6 (10.9)	24.8 (10.6)	27.9 (10.4)	25.9 (10.7)	26.1 (11.0)
Share (%) of the population with:							
Less than complete secondary	5.6	7.5	3.0	6.2	25.4	9.5	5.9
Complete secondary	69.4	65.2	66.7	47.5	23.7	39.9	39.5
Complete tertiary	24.9	27.3	30.4	46.4	50.9	50.5	54.6
Obs.	5,548	4,108	4,189	7,717	20,152	7,556	7,057

Notes: These basic statistics correspond to 2021 (EU-SILC 2022), except for three countries. The statistics for Switzerland and Serbia correspond to 2020, while those of Norway correspond to 2019; as these are that latest year in which such countries were included in the survey. Standard deviation in parenthesis.

Source: Own estimations based on EU-SILC (2022, 2021 and 2020).

Annex Table 2. CSL selection process

Country	Implementation year	Compulsory schooling years		Current schooling years attained	Inclusion criteria		Included in the analysis
		Before	After		Increases CS	Above current schooling attained	
Belgium	1983	8	12	9	Yes	Yes	Yes
Bulgaria	1991	8	9	9	Yes		
Cyprus	1962	0	6	5	Yes	Yes	Yes
Cyprus	1986	6	9	9	Yes		
Cyprus	2005	9	10	10	Yes		
Czech Republic	1979	9	10	11	Yes		
Czech Republic	1990	10	9	11			
Denmark	1972	7	9	7	Yes	Yes	Yes
Estonia	1980	8	11	9	Yes	Yes	Yes
Estonia	1992	11	9	10			
Finland	1974	6	9	7	Yes	Yes	Yes
Greece	1976	6	9	7	Yes	Yes	Yes
Ireland	1972	8	9	8	Yes	Yes	Yes
Ireland	2000	9	10	11	Yes		
Italy	2003	8	12	10	Yes	Yes	Yes
Latvia	1980	8	11	7	Yes	Yes	Yes
Latvia	1991	11	9	9			
Lithuania	1980	8	11	8	Yes	Yes	Yes
Lithuania	1991	11	9	10			
Luxembourg	1977	9	10	8	Yes	Yes	Yes
Luxembourg	1993	10	11	10	Yes	Yes	Yes
Luxembourg	2009	11	12	11	Yes	Yes	Yes
Netherlands	1969	8	9	8	Yes	Yes	Yes
Netherlands	1973	9	10	9	Yes	Yes	Yes
Netherlands	1985	10	12	10	Yes	Yes	Yes
Norway	1969	7	9	8	Yes	Yes	Yes
Norway	1997	9	10	11	Yes		
Poland	1999	8	9	11	Yes		
Poland	2005	9	10	11	Yes		
Portugal	1986	6	9	6	Yes	Yes	Yes
Slovakia	1979	9	10	10	Yes		
Slovakia	1990	10	9	11			
Slovakia	1998	9	10	12	Yes		
Spain	1970	6	8	5	Yes	Yes	Yes
Spain	1990	8	10	8	Yes	Yes	Yes
Switzerland	1970	8	9	10	Yes		

Notes: The CSLs included in this table correspond to implemented in our sample of countries targeting individuals born between 1956 and 1996, considering that our data goes from 2018 to 2021 and our sample is limited employed individuals age between 25 and 65.

Source: Own elaboration based on Angrist et al. (2026) and Barro and Lee (2013).

Annex Table 3. Binding CSLs mandating an increase in schooling

Country	Implementation year	Years of compulsory schooling		Schooling increase		Data year
		Before	After	Intended	Attained	
Cyprus	1962	0	6	6	2.1	2019
Denmark	1972	7	9	2	0.7	2021
Estonia	1980	8	11	3	0.3	2021
Finland	1974	6	9	3	0.7	2021
Greece	1976	6	9	3	0.8	2021
Ireland	1972	8	9	1	1.9	2021
Latvia	1980	8	11	3	0.3	2021
Luxembourg	1977	9	10	1	2.7	2021
Netherlands	1969	8	9	1	1.4	2018
Netherlands	1973	9	10	1	1.4	2021
Norway	1969	7	9	2	2.0	2018
Spain	1970	6	8	2	2.3	2021
Average		7	9	2.3	1.4	

Sources: Own elaboration based on Angrist et al. (2026) and EU-SILC (2019 to 2022).

Annex Table 4. Returns to schooling years and education levels using OLS

Country	Schooling years	Secondary (vs. less than secondary)	Tertiary (vs. Secondary)
Austria	9.8	2.9	8.3
Belgium	8.7	2.7	10.4
Bulgaria	11.2	4.3	13.8
Switzerland	13.0	2.8	8.5
Cyprus	13.5	3.4	14.4
Czech Republic	10.1	2.3	8.6
Denmark	10.2	2.3	6.4
Spain	10.7	1.8	11.8
Estonia	9.7	2.1	8.6
Finland	7.6	0.1	8.4
France	8.8	1.2	9.8
Greece	8.6	1.7	9.5
Croatia	10.6	2.6	9.5
Hungary	5.5	1.9	7.2
Ireland	12.4	1.5	14.9
Italy	7.6	2.8	9.0
Lithuania	12.3	1.0	13.1
Luxembourg	8.3	3.1	10.0
Latvia	12.0	1.4	10.7
Netherlands	8.0	1.8	8.7
Norway	3.0	2.3	3.9
Poland	7.1	1.6	9.8
Portugal	12.9	2.9	14.5
Romania	10.1	1.7	11.4
Serbia	12.7	1.9	11.0
Slovak Republic	6.7	2.1	6.3
Slovenia	9.8	1.6	10.0
Sweden	7.8	2.1	3.5
<i>Average</i>	9.6	2.2	9.7

Note: The estimates for Switzerland and Serbia correspond to 2020, while that of Norway corresponds to 2019; these are that latest year in which such countries were included in the survey.

Source: Own estimates using EU-SILC 2019 - 2022.

Annex Table 5. IV First Stage and Wald F-Test

Country	CSL year	Schooling years		Higher education	
		1st. Stage	Wald F-test	1st. Stage	Wald F-test
Cyprus	1962	2.1 ***	134.1	23.0 ***	119.5
Denmark	1972	0.7 ***	73.4	11.6 ***	54.5
Estonia	1980	0.3 ***	8.7	4.8 ***	18.0
Finland	1974	0.7 ***	48.2	5.0 ***	15.3
Greece	1976	0.8 ***	56.6	11.0 ***	50.7
Ireland	1972	1.9 ***	101.7	19.0 ***	56.2
Latvia	1980	0.3 ***	15.1	5.4 ***	25.6
Luxembourg	1977	2.7 ***	73.7	25.0 ***	46.9
Netherlands	1969	1.4 ***	112.3	17.9 ***	111.2
Netherlands	1973	1.4 ***	87.6	16.5 ***	112.4
Norway	1969	2.0 ***	96.3	18.3 ***	104.0
Spain	1970	2.3 ***	465.0	26.8 ***	387.0
Average		1.3		14.7	

Notes: *** p<0.01, ** p<0.05, * p<0.1.

Source: Own estimations based on EU-SILC (2019 to 2022).