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Education and adult health: Is there a causal effect?

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ABSTRACT

Many studies find a strong positive correlation between education and adult health. A subtler question is whether this correlation can be interpreted as a causal relationship. We combine multi-country data from two cross-sections of the European Union Statistics on Income and Living Conditions (EU-SILC) survey and use exogenous variation in compulsory years of schooling across countries and cohorts induced by compulsory schooling laws. We find no causal effect of education on any of our several health measures. This finding is extremely robust to different changes in our main specification and holds using other databases. We discuss different explanations for our results.

1. Introduction

Many studies have documented that people with a high educational level have consistently better health than those with a low educational level. No matter which measure of health is used, the evidence of a strong correlation is pervasive (Grossman, 2006; Cutler and Lleras-Muney, 2006; Oreopoulos, 2006; Cutler et al., 2015). A subtler point is whether this correlation can be interpreted as a causal relationship, and what the direction of this relationship is.

A common identification strategy consists of using compulsory schooling laws (CSLs) that increase the minimum school leaving age (SLA) as an instrument for education. Individuals in year-of-birth cohorts affected by CSLs are compelled to stay in school more years than earlier cohorts. Comparing cohorts that are not far apart and after controlling for cohort-specific factors, CSLs induce an exogenous change that only affects individuals' health through the increased education. This allows us to identify the causal effect of education on health outcomes (see Lochner, 2011; Cutler and Lleras-Muney, 2012, and Galama et al., 2018, among others). To the best of our knowledge, Lleras-Muney (2005) is the first work that used CSLs to identify the effect of education on mortality in the USA. Several authors focus on specific countries, while others use a multi-country approach exploiting variation induced by reforms across both birth cohorts and countries. The overall picture is not conclusive. Some studies find a positive and significant effect of education on health for specific countries, such as Oreopoulos (2006) or Silles (2009) for the UK; Van Kippersluis et al. (2011) for The Netherlands; Kemptner et al. (2011) for Germany; and Fischer et al. (2013) for Sweden. Brunello et al. (2013), Crespo et al. (2014), Mazzona (2014), Gathmann et al. (2015), and Brunello et al. (2016) also find positive effects using a multi-country approach. Conversely, several studies do not find a causal effect. These include Arendt (2005) for Denmark, Albouy and Lequien (2009) for France, Jürges et al. (2013) and Clark and Royer (2013) for the UK, Fletcher (2015) for the USA, Meghir et al. (2018) for Sweden, Malamud et al. (2018) for Romania, or Courtin et al. (2019) for France. Other studies, such as Janke et al. (2018) or Davies et al. (2018), find a positive effect on some health outcomes but not on others (see the recent review of this literature by Hamad et al., 2018). In the Online Appendix we provide additional details on the works mentioned above. Finally, a few studies use data from twins and find small or no effects on health, such as Fujiwara and Kawachi (2009), Amin et al. (2015), or Sudharsanan et al. (2016).

We present new evidence on the possible causal effect of education on health using multi-country data from the 2005 and 2011 cross sections of the European Union Statistics on Income and Living Conditions (EU-SILC). This is a rich database with information on education and health for European countries. Additionally, the 2005 and 2011 cross sections include retrospective information on family background, which provides us with information on socioeconomic status (SES) when the individual was young. Our main result is that we find no causal effect of education on any of the three health outcomes in EU-SILC. Our estimated IV coefficients are similar in magnitude to OLS coefficients, but statistically insignificant since standard errors are, as usual, larger for IV. We also explore the possibility of heterogeneous effects of education on health, but we get the same result for different subgroups according to gender, parental education, or family income.

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Our results are robust to different changes in our main specification. We discuss alternative explanations for why education may have no causal effect on health even if, as we see, it does affect income positively.

We contribute to the literature in several respects. First, we exploit a rich dataset that has information from several European countries. We are the first to find no conclusive causal effect of education on health using a multi-country approach. With this approach, lack of power due to a relatively small sample size can be more safely excluded as a possible explanation. We choose only those countries where the instrument works, thus avoiding additional problems due to lack of identification. This represents the most favorable scenario to obtain a positive effect of education and allows us to identify an upper bound to the causal effect of education on health outcomes. Second, the 2005 and 2011 cross sections of EU-SILC contain rich family information before individuals complete their compulsory education. We use this to explore whether educational reforms have heterogeneous effects on people's schooling, as some recent literature suggests (Brunello et al., 2013 or Crespo et al., 2014). Since education can affect people of different SES or gender differently and, since the average effects capture the combined effect on them, it is also important to unravel the impact of education on health for different groups. In particular, we consider dimensions pointed out as relevant by the previous literature: gender, family economic background (poor/non-poor families), and family sociocultural background (uneducated/educated families). Third, our dataset contains three different measures of health status including self-reported health, chronic illness, and limitation in daily activities. We also use an alternative dataset, the European Social Survey (ESS) that, although less detailed, contains information on education and health outcomes. Our use of different health measures and databases ensures that our results are not specific to a measure or dataset.

2. Data and empirical strategy

The EU-SILC database contains information on income, education, health, poverty, social exclusion, and living conditions in the European Union. The 2005, 2011 cross sections have special modules on the intergenerational transmission of disadvantages with retrospective information on the characteristics of the family in which individuals were raised: family composition, occupation and educational level of parents, and information about the economic situation in the household.

Our database includes twelve countries in EU-SILC for which we have reliable information about CSLs: Austria, Czech Republic, France, Greece, Ireland, Italy, Malta, the Netherlands, Poland, Portugal, Slovakia, and the United Kingdom. We exclude countries for which we lack information on CSLs (Bulgaria, Cyprus, Estonia, Croatia, Iceland, Latvia, Lithuania, Luxembourg, Romania, and Slovenia) or where CSLs were implemented gradually over several years or at the regional/local level (Finland, Norway, Sweden, and Switzerland). We exclude Germany since reforms were implemented gradually at the regional level and EU-SILC lacks regional identifiers for Germany. We exclude Belgium because the reform was much later (1983). We exclude three countries other authors have used (Denmark, Spain, and Hungary) because we find no effect of the proposed CSL on educational levels. The inclusion of these countries would exacerbate any potential bias of our IV estimates towards OLS. In the Online Appendix we show that our results do not change when we add them.

Table 1 shows the reforms, which increased compulsory schooling by one or more years. The first cohort potentially affected (FCA) goes from 1946 to 1964. In each country, the control group includes cohorts born before the FCA, and the treatment group those born after the FCA. Following the literature, we eliminate the FCA from the analysis, since it is not clear the extent of this cohort's exposure to the educational reform. The only exception is France, where the 1959 reform affected all people born on or after 1 January 1953. See the Online Appendix for details on the reforms.

EU-SILC contains three health questions: 1) "How is your health in general?" ("very good," "good," "fair," "bad," and "very bad"). We code this information into a dummy variable (*good health*), which takes a value of one when the answer is "very good" or "good." 2) "Do you have any longstanding illness or health problem?" ("yes", "no"). We build a dummy variable (*no chronic*) that takes a value of one when the answer is "no." 3) "For at least the last 6 months, to what extent have you been limited because of a health problem in activities people usually do?" ("not limited," "limited," "very limited"). We build a third dummy variable (*not limited*) that takes value one when the answer is "not limited."

Table 1
Compulsory education reforms.

Country	Reform year	FCA	Years comp. education	Entry age
Austria	1966	1953	8 to 9	6
Czechia/Slovakia	1960	1946	8 to 9	6
France	1967	1953	8 to 10	6
Greece	1976	1964	6 to 9	6
Ireland	1972	1958	8 to 9	6
Italy	1963	1951	5 to 8	6
Malta	1972	1960	8 to 10	5
Netherlands	1975	1959	9 to 10	7
Poland	1966	1952	7 to 8	7
Portugal	1964	1956	4 to 6	6
UK	1972	1957	10 to 11	6
Additional reforms used in the robustness check				
Denmark	1971	1957	7 to 9	7
Hungary	1961	1947	8 to 10	6
Spain	1970	1957	6 to 8	6

Notes: See the Online Appendix for more details.

We use schooling years to measure education. EU-SILC reports the highest level of education attended, together with the year that level was completed. We exclude individuals still in education. Our measure (*years_educ*) is calculated as the year when the highest level was attained minus the year of birth minus school entry age (see the Online Appendix for details). We use as an alternative measure a dummy that takes value one if the individual has completed secondary education (*SE*). This variable can give a better idea of educational output than *years_educ*. For instance, a repeater who does not finish high school may have more years of schooling than one who has completed that level.

Fig. 1 shows our health measures as a function of years of education. The age range is 25–66. The relationship between education and health is increasing only up to 19 years. We find the same result when dividing the sample into different age groups (25–34, 35–44, 45–54 and 55–66), separately by country, by gender, and by SES (see the Online Appendix). In addition, this is not related to the typical number of schooling years for most individuals in the sample (12 years as their

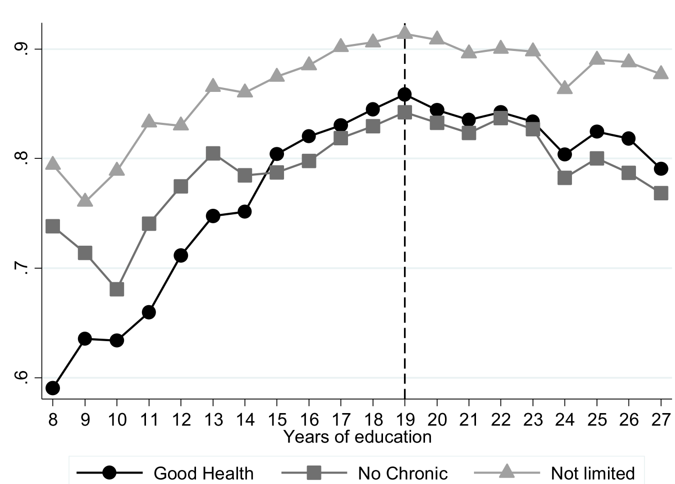

Fig. 1. Years of education and health measures.

highest education level is upper secondary). Given this evidence, we restrict our sample to individuals with at most 19 years of education. Including subjects with more than 19 years of education would make it easier to find no effect of education on health outcomes. Our strategy is to select the worst case scenario for a no-effect result.

Since we use the 2005 and 2011 special modules, we have to exclude all individuals not in the age range of the modules (25–66) or that are not the selected respondent. This restricts our sample to individuals born after 1939. We exclude individuals who did not live with their parents. Finally, we exclude all individuals not born in the country of residence, since we do not know where they went to school. After we do this, there are a few individuals born in the country, but that are not citizens. We define the dummy variable *noncitizen* (= 1 if the individual is not a citizen) and include it in all our regressions since it can have an effect on our outcomes of interest (see the Online Appendix for details).

Other control variables capture early-life conditions. The dummy variable *nef* (for "non-educated family") takes value one when the highest level of education of parents is primary education. The results are similar using only the mother's or father's educational level. We classify a family as "educated" when at least one of the parents has a secondary education or higher. We use this variable both as a control and to see whether the impact of CSLs on individuals' schooling depends on parental education. We conjecture that this effect should depend on how much parents value children's education. We should expect a stronger effect of CSL for individuals from families with little education, since they are more likely to leave school right at the end of the compulsory period.

The survey contains information about the family's financial situation when the individual was an adolescent. The dummy variable *poor past* is equal to one for those individuals who lived in a family with frequent financial difficulties when young (see the Online Appendix for details). The main reason for using these binary variables is that we can split the sample into two distinct groups so that we can perform the analysis separately for each one of them. In addition, our results remain unchanged when we consider additional categories for both background variables.

Other explanatory variables included in our regressions are *father only* (= 1 if the mother was not present in the family) and *CS2011* (= 1 if corresponds to the 2011 cross section). Our main sample comprises 47,269 individuals. Of these, 53.4% belong to the 2005 wave (25,250 individuals) and 46.6% (22,019 individuals) to the 2011 wave (see the Online Appendix for summary statistics).

3. Empirical model

We use a two-equation model. The first-stage equation is:

$$E_i = \gamma_0 + \gamma_1 R_i + \gamma_2 X_i + \varepsilon_i, \tag{1}$$

where E_i is education, R_i is the reform dummy or the number of years of compulsory education, and X_i includes individual and family characteristics determined before school completion. The relationship of interest between education and health is the second-stage equation:

$$H_i = \beta_0 + \beta_1 E_i + \beta_2 X_i + \nu_i, \tag{2}$$

where H_i is a measure of adult health. We add country and cohort fixed effects and country-specific quadratic trends in age. Country fixed effects control for invariant factors within countries, such as national differences in institutions affecting health or in reporting styles. Since treated individuals are younger than controls, we include time trends to account for secular tendencies. In this way, we identify the effect of the reform on those people who, even with the positive trend, would not have acquired more education without the reform. If we do not include these trends, secular health improvements may be incorrectly attributed to school reforms, thus biasing the results (Lochner, 2011). For a similar specification, see Brunello et al. (2016). In addition, from Stephens and Yang (2014) we know that it is crucial that these temporal trends are country-specific. We also interact *CS2011* (the survey dummy) with the country dummies. Since we use two cross sections, one from 2005 (pre-crisis) and one from 2011 (post-crisis), these interaction terms capture differential effects of the financial crisis across countries.

The error term in (2) is likely to contain unobserved individual characteristics that affect both education and health in the same direction. Estimating by OLS may yield biased estimators of the parameter of interest. To tackle this problem, we exploit the exogenous variation of schooling induced by changes in CSLs that raised the SLA. Individuals in year-of-birth cohorts affected by CSLs must attend more years of schooling. Under the selection-on-observables assumption that a (country-specific) trend controls for factors that make cohorts different in terms of health and education, the remaining variation in education can be attributed to the CSLs and is truly exogenous (that is, no direct effect of being affected by the reform and therefore a younger cohort in education and health, since time effects have been controlled for). This is a sensible assumption when the cohorts are not very distant and we use a flexible specification for trends.

Our central measure of reform exposure is a dummy variable (*reform*) that takes value one for the affected cohorts. We also check our results using the number of years of schooling each cohort must attend by law as an alternative instrument. Control and treatment groups are country specific. As an example, consider the Austrian reform (1966) that increased SLA from 14 to 15 years of age. According to Gathmann et al. (2015), the FCA is 1953. This means that for those born between 1946 and 1952 (control), the reform dummy takes value 0, while for those born between 1954 and 1960 (treatment) it takes value 1. We face a trade-off when deciding the number of year-of-birth cohorts to include in each group. The more cohorts we include, the larger the sample size. However, including many cohorts makes it more difficult to assume that both groups are comparable. As a compromise, we follow most of the literature and include seven cohorts in each group. In the Online Appendix we show that our main results do not change when we have five or nine cohorts in both groups.

In Fig. 2 we represent the average years of schooling according to the distance in years since the year of the reform. Distance 0 corresponds to those born in Austria in 1952, in the Czech Republic in 1946, etc. There is an upward jump of approximately 0.3 years with respect to the trend, which means that reforms have an impact on education. This effect is consistent with prior studies on European countries (see Brunello et al., 2016).

The key identification assumption is that, within each country, additional education was assigned to people only based on their date of birth and regardless of their future health. This can be justified, since it is difficult to argue that exposure to reform can have a direct effect on

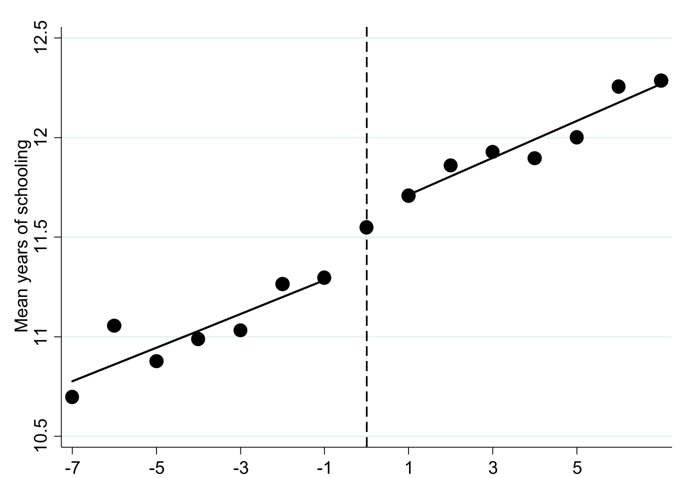

Fig. 2. Distance (years) from the reform and mean years of education.

Table 2
First stage.

	I	II	III	IV	V	VI	VII
	All	Men	Women	Non- poor	Poor	Educated	Non- educated
A. Reform dummy							
1st stage coef.	0.368*** (0.069)	0.362*** (0.098)	0.367*** (0.077)	0.302*** (0.087)	0.496*** (0.117)	0.067 (0.108)	0.463*** (0.077)
F-test	28.278	13.773	22.997	12.089	17.885	0.385	36.296
p-value	0.000	0.000	0.000	0.001	0.000	0.536	0.000
B. Compulsory education years							
1st stage coef.	0.168*** (0.040)	0.184*** (0.056)	0.153*** (0.051)	0.132*** (0.045)	0.227*** (0.057)	0.015 (0.075)	0.196*** (0.045)
F-test	17.772	10.760	8.935	8.727	15.971	0.040	19.354
p-value	0.000	0.001	0.003	0.004	0.000	0.842	0.000
Observations	45,767	21,172	24,595	28,237	17,530	11,162	34,605

Notes: All models include *noncitizen*, *father only*, country-specific quadratic trends, cohort fixed effects, and country-specific survey effects as controls. Model I includes the dummies *gender*, *non-educated family*, and *poor past* as controls, together with all their possible interactions. Models II-VII are for specific groups; the corresponding dummy variables are dropped.

adult health, once we control for educational achievement, family economic and sociocultural characteristics, parents' education and time trends.

The effect of education on health in (2) and of reforms on schooling in (1) is homogeneous across individuals. However, we also estimate the model for different subgroups according to gender and family background. Finally, errors are clustered at the country cohort-of-birth level in all our regressions.

4. Results

4.1. First stage

Table 2 presents our first-stage results using a 7-year window. In Panel A the instrument is the reform dummy and in Panel B is the number of years of compulsory education. In Column I, we estimate the model for all individuals and in columns II-VII we estimate it for six different sub-groups. The effect of CSLs on education is positive and statistically significant, with an F-statistic above 10 except for the subsample of individuals with educated parents. This suggests that we should not be very concerned about weak instruments in our case. We get similar results using LIML estimators.

Our results are in line with previous literature using a multi-country approach (see, among others, Brunello et al., 2016). According to Model I, Panel A, exposure to reform has a positive and significant effect on education. The size of the effect (0.368) represents an increase of about 4.4 additional months of schooling. In Panel B we see that for each additional year of compulsory education, schooling increases on average by 0.168 years ($\approx$2 months). The reform dummy seems to be a better instrument than years of compulsory education. In the rest of the analysis (Tables 3–7), we present the results corresponding to the reform dummy. The Online Appendix provides the results with the other instrument.

Women and men are similarly affected by the reforms. Individuals from disadvantaged families (poor or poorly educated) are strongly affected by CSLs. Without the reforms, many of them would probably have dropped out from school before, as these families value less or cannot afford their children's education (Piopiunik, 2014).

4.2. Main equation

Table 3 reports the results corresponding to Equation (2) and our three health measures in panels A, B, and C, respectively.

The OLS estimates suggest that education has a positive but moderate effect on health. With our first measure of health, estimated coefficients are around 0.02. One additional year of education increases the probability of reporting good health by 2 percentage points (sample mean is 55%). For the other two variables, the effects are smaller: about 1.3–1.4 percentage points for our second measure (mean 77%) and about 1 percentage point for the last one (mean 67%).

Our OLS results above show that the association between education and self-reported health is stronger for individuals with better backgrounds. More specifically, it is stronger for individuals from non-poor families (0.020) than for those raised in poor families (0.017). Additionally, the coefficient for those from educated families (0.024) is higher than for those from families with little education (0.019). The first difference is not significant, but the second one is. This may be due to the fact that pre-existing non-observable conditions of individuals (e.g., child health) have lasting effects on adult health. It could happen that, in order for education to have a causal effect on adult health, these pre-existing conditions must be good enough (that is, child health and education are complements in the production of adult health). These results are in line with recent findings (Brunello et al., 2016) and add to the growing literature on the importance of early interventions which finds, for example, lower returns to university for people who grew up in disadvantaged families (Cunha and Heckman, 2007).

The OLS estimates here are similar in size to others in the literature. Our estimate for *self-reported good health* is close to the 0.026 found by Silles (2009) for the UK. However, it is much lower than the one found by Oreopoulos (2006) for the same country, 0.065. Regarding *no chronic illness* and *not limited*, our estimates are 0.01 and 0.013, respectively; again comparable to those found by Silles (2009) (0.008 each).

Our IV estimates are less precisely estimated, so we cannot rule out either the possibility that education has no effect on health outcomes or that this effect is large. The 95% confident intervals for our measures (all individuals) are [-0.030, 0.086], [-0.038, 0.062], and [-0.022, 0.097], respectively. The OLS estimates always lie within the corresponding confidence interval. The 95% confidence interval does not include the zero only for the estimated coefficient of our third measure of health in the sub-sample of women: [0.0007, 0.1571].

4.3. Alternative measures of education

Our measure of education excludes individuals with twenty or more years of completed education. We estimate the model again including these individuals, which increases the sample size by 5%. The results are similar; however, as expected, the instrument is now weaker (first-stage F-statistic 22.49 compared to 28.27; see Table O4 in the Online Appendix).

Table 3
Second-stage results.

	I	II	III	IV	V	VI	VII
	All	Men	Women	Non- poor	Poor	Educated	Non-educated
A. Dep. variable good reported health							
OLS coeff.	0.020***	0.018***	0.022***	0.020***	0.017***	0.024***	0.019***
schooling	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
IV coeff.	0.028	−0.004	0.053	0.057	0.012	0.496	0.006
schooling	(0.030)	(0.039)	(0.037)	(0.050)	(0.029)	(0.857)	(0.027)
Observations	45,767	21,172	24,595	28,237	17,530	11,162	34,605
B. Dep. variable not limited in daily activities							
OLS coeff.	0.013***	0.013***	0.013***	0.014***	0.013***	0.014***	0.014***
schooling	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
IV coeff.	0.012	0.022	0.004	0.018	0.009	−0.173	0.012
schooling	(0.026)	(0.038)	(0.032)	(0.041)	(0.035)	(0.440)	(0.022)
Observations	45,710	21,136	24,574	28,202	17,508	11,151	34,559
C. Dep. variable no chronic illness							
OLS coeff.	0.010***	0.009***	0.012***	0.011***	0.009***	0.012***	0.010***
schooling	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
IV coeff.	0.038	−0.004	0.079**	0.039	0.049	0.295	0.022
schooling	(0.030)	(0.038)	(0.040)	(0.049)	(0.034)	(0.507)	(0.027)
Observations	45,728	21,150	24,578	28,216	17,512	11,154	34,574

Note: Same controls as Table 2.

Next, we use our alternative measure of education (*SE*). As above mentioned, this measure could be a more appropriate proxy for educational achievement (learning) than years of schooling. Table 4 shows that CSLs have a positive effect on completing at least secondary education, but the instrument is weaker than in Table 2 (see Section 5 for a deeper discussion on this alternative measure). Nevertheless, again we see that finishing secondary education does not seem to have an effect on our health measures.

We perform several checks to show that our results are robust to different changes in our main specification (first stage), measure of education, selection of countries, using a different dataset (ESS), window size, excluding individuals who are not the potential target of the reforms, and performing a placebo test. The results, which can be found in the Online Appendix, are in line with the findings in Tables 2 and 3.

5. Discussion

We explore possible drivers of our findings, including the sample characteristics, the adequacy of the timing of the reforms, and the role of education quality.

5.1. Sample age

A difference between our work and those who find a causal effect of education on health outcomes is the average sample age. The average age is 65.5 in Brunello et al. (2016) and 58.13 in Crespo et al. (2014). In our EU-SILC sample it is 53.2 and in the ESS sample it is 54.5.

Table 4
Measure of education is SE.

	I	II	III	IV	V	VI	VII
	All	Men	Women	Non- Poor	Poor	Educated	Non- educated
A. Dep. variable good reported health							
OLS coeff.	0.117***	0.103***	0.130***	0.119***	0.109***	0.145***	0.114***
	(0.005)	(0.008)	(0.007)	(0.007)	(0.007)	(0.013)	(0.005)
IV coeff.	0.388	−0.131	0.553	0.618	0.284	4.863	0.094
	(0.438)	(0.956)	(0.431)	(0.603)	(0.649)	(10.528)	(0.416)
B. Dep. variable not limited daily activities							
OLS coeff.	0.084***	0.083***	0.084***	0.086***	0.076***	0.101***	0.083***
	(0.004)	(0.006)	(0.007)	(0.006)	(0.006)	(0.015)	(0.005)
IV coeff.	0.167	0.525	0.044	0.191	0.216	−1.834	0.185
	(0.359)	(1.002)	(0.324)	(0.455)	(0.765)	(4.713)	(0.343)
C. Dep variable no chronic							
OLS coeff.	0.063***	0.052***	0.073***	0.068***	0.054***	0.082***	0.062***
	(0.006)	(0.009)	(0.007)	(0.007)	(0.008)	(0.016)	(0.006)
IV coeff.	0.513	−0.104	0.802*	0.425	1.092	3.079	0.335
	(0.458)	(0.887)	(0.474)	(0.558)	(0.907)	(6.737)	(0.441)
1st stage	0.027***	0.015	0.036***	0.028**	0.021	0.007	0.030***
Coeff.	(0.009)	(0.011)	(0.012)	(0.013)	(0.015)	(0.014)	(0.010)
F-test	9.115	1.934	9.207	4.823	2.067	0.237	9.171
p-value	0.003	0.166	0.003	0.030	0.153	0.627	0.003
Observations	45,767	21,172	24,595	28,237	17,530	11,162	34,605

Table 5
Effect of reforms on finishing secondary and tertiary education.

	I	II	III	IV	V	VI	VII
	All	Men	Women	Non-poor	Poor	Educated	Non-educated
A. Dep. variable is SE							
Reform	0.027***	0.015	0.036***	0.028**	0.021	0.007	0.030***
	(0.009)	(0.011)	(0.012)	(0.013)	(0.015)	(0.014)	(0.010)
F-test	9.115	1.934	9.207	4.823	2.067	0.237	9.171
p-value	0.003	0.166	0.003	0.030	0.153	0.627	0.003
Observations	45,767	21,172	24,595	28,237	17,530	11,162	34,605
B. Dep. variable is SE, excluding those with tertiary education							
Reform	0.023**	0.005	0.039***	0.023	0.022	0.011	0.025**
	(0.010)	(0.013)	(0.013)	(0.016)	(0.015)	(0.020)	(0.011)
F-test	5.376	0.123	8.632	2.228	2.208	0.318	4.912
p-value	0.022	0.727	0.004	0.138	0.139	0.573	0.028
Observations	40,757	18,808	21,949	24,229	16,528	8318	32,439
C. Dep. variable is having tertiary education							
Reform	0.002	0.008	−0.003	0.003	0.004	−0.014	0.009
	(0.006)	(0.009)	(0.007)	(0.008)	(0.010)	(0.019)	(0.006)
F-test	0.098	0.824	0.248	0.101	0.202	0.585	2.420
p-value	0.755	0.366	0.619	0.751	0.654	0.446	0.122
Observations	45,767	21,172	24,595	28,237	17,530	11,162	34,605

Notes: In panels A and B the endogenous variable is SE. In panel B we exclude individuals with tertiary education. In panel C the endogenous variable is a dummy that takes the value of one if the individual has tertiary education.

Table 6
South vs. North.

	Southern Europe	Rest of countries
A. Dep. variable good reported health		
OLS coeff. of education	0.016***	0.024***
	(0.001)	(0.001)
IV coeff. of education	−0.001	0.034
	(0.029)	(0.053)
B. Dep. variable not limited daily activities		
OLS coeff. of education	0.010***	0.017***
	(0.001)	(0.001)
IV coeff. of education	0.010	0.036
	(0.021)	(0.044)
C. Dep. variable no chronic		
OLS coeff. of education	0.007***	0.015***
	(0.001)	(0.001)
IV coeff. of education	0.021	0.066
	(0.028)	(0.048)
First-stage	1.131***	0.259***
	(0.235)	(0.059)
F-test	23.123	19.035
p-value	0.000	0.000
Observations	17,492	28,275

Notes: South = Greece, Italy, Malta, and Portugal. Rest = other countries. Same controls as Tables 2 and 3.

Previous studies have proposed that differences in health change over the life-course (Mehta et al., 2019). According to the cumulative (dis)advantages hypothesis, health differences between individuals of different educational levels are only present at a relatively later age. Lower-educated individuals are more exposed and more vulnerable to the adverse consequences of health risks due to the lack of both monetary and information resources. While public health systems may offset these differences and reduce the effects of selection due to premature mortality, differences tend to appear later in life. Among others, Leopold and Leopold (2018) report evidence in favor of this hypothesis using data for Germany. This could explain the differences between previous works and our results.

Alternatively, it could be a selection problem. Subjects differ in terms of pre-existing unobservable conditions (think, for instance, of child health). For education to have a causal effect on adult health, pre-existing conditions must be good enough. We could model this assuming that children's health and education are complements in the production of adult health. Along these lines, Cunha and Heckman (2007), who propose a technology of skill formation characterized by dynamic complementarity, argue that the best moment to intervene is in the early stages of education, particularly for disadvantaged children and their families. Since our sample combines individuals with different pre-existing conditions, the existence of a causal effect of education on health depends on the proportions of individuals with different conditions. If mortality is higher among individuals with poor pre-existing conditions, the older the sample the more likely we are to find a causal effect of education on health. VandenBerg et al. (2006) find a

Table 7
Effect of education on household income.

	I	II	III	IV	V	VI	VII
	All	Men	Women	Non- poor	Poor	Educated	Non- educated
Dep. variable household income (hy022)							
OLS coeff. schooling	0.067***	0.064***	0.068***	0.068***	0.063***	0.078***	0.063***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
IV coeff. schooling	0.203***	0.176*	0.224***	0.126	0.260***	1.122	0.153**
	(0.073)	(0.090)	(0.085)	(0.096)	(0.087)	(1.934)	(0.064)
Observations	44,479	20,562	23,917	27,564	16,915	10,922	33,557

Notes: Household income is hy022. Same controls as Tables 2 and 3.

significant negative effect of early-life economic conditions on mortality rates at all ages. Kelly-Irving et al. (2013) also show that adverse childhood conditions are related to premature mortality. As the average age of the sample increases, individuals with pre-existing poor conditions will be less represented in the sample. In our data, we lack appropriate controls for pre-existing conditions. However, our two family background measures (poor/non-poor, non-educated/educated families) could be seen as proxies of those conditions. We have some evidence on this in our data since, as mentioned above, the association between education and self-reported health measured by our OLS coefficients is stronger for individuals from better backgrounds (see Table 3).

Finally, according to Brunello et al. (2018) the increase of the minimum retirement age has a greater effect on educated people by improving healthy habits. As a consequence, the health differences between people retired or close to their retirement age with different educational levels are widened. This could explain why other researchers who use older samples find a causal effect of health education.

5.2. Reform characteristics

All our reforms affect subjects in secondary education (see Table 1). According to our results, interventions that increase education *at this particular level* have no impact on health outcomes. This does not imply that any intervention is useless. James Heckman has repeatedly stressed that the best moment to intervene is in the early stages of education, particularly for disadvantaged children and their families (Cunha and Heckman, 2007). In this sense, all the reforms we consider can be seen as late interventions when individuals are already too old for the reform to take effect, particularly those with a low SES.

Another possibility is that for the reforms to be successful, they must have an effect on the likelihood of subjects completing an educational stage. We verify this by focusing on the first-stage results when the education measure is SE (completing at least secondary education). The results are in Table 5, panels A (all individuals) and B (removing those who have completed tertiary education).

Reforms have a positive effect on the probability of completing secondary studies, but the effect is weaker than in Table 2. The F-statistics are always below 10. If we use SE as our measure of education to explain health outcomes, we face a weak instruments problem. There is no effect at all for men, poor people, and those from educated families. Conversely, there is an effect for women, for the non-poor, and for individuals from uneducated families. The strongest effect is that of women (0.036), which indicates that women affected by the reform are 3.6 percentage points more likely to finish secondary education. This effect is not negligible, since 59.8 percent of the women in the sample have completed secondary education. The results in Panel B are very similar, although the weak instruments problem worsens. In summary, if SE is a better measure of educational achievement than years of education, we can hardly identify the effect of education due to a weak instruments problem.

Alternatively, it could be that increases in tertiary education have an effect on health. Buckles et al. (2016) find that having a college education has a positive effect on mortality reduction, mostly because of a reduction in deaths due to cancer and heart disease. On the contrary, Janke et al. (2018) find that an expansion of college education in the UK only has an effect on reducing the prevalence of diabetes. We check if CSLs have any effect on the probability of obtaining tertiary education by estimating a model in which the endogenous variable is a dummy that takes value one when subjects have tertiary education. The main explanatory variable is the reform dummy and we include the same covariates as in Table 2. The results are in Table 5, Panel C. Reforms do not seem to have an effect on the likelihood of completing tertiary education. This would mean that reforms affect individuals very much in the margin. The law forces them to remain in secondary education and some complete this level, but most of them do not continue beyond this stage. If what really matters to improve health is to have tertiary education, our reforms cannot identify this effect.

Another reason has to do with a LATE interpretation of our results, since we can only identify an effect for those individuals affected by the reforms. Studies that use old reforms typically find a greater effect than those that use more recent reforms. The UK had two major reforms in the 20th century, one in 1947 and another in 1972. The first raised SLA from 14 to 15 years of age and the second one from 15 to 16. The first one affected 50% of individuals in the relevant cohorts, while the second one affected about 25% (Clark and Royer, 2013). This difference is due to the secular positive trend in education and could explain the lack of precision of our estimates. In addition, the type of affected individuals is potentially very different. In 1947, it was likely that among those affected there were many people of great skill who had left school without the reform. This was less likely in 1972. The more recent the reforms, the worse the average ability of the individuals affected. If the causal effect of education on health is mediated by skill or ability, we should observe a much smaller effect for these marginal subjects when we use relatively recent reforms. Note that this has nothing to do with education quality.

A final related argument can explain why studies using old reforms find an impact of education on health whereas we, using later reforms, do not. Those early reforms reduced individuals' exposure to dangerous factory or tough agriculture jobs. By attending secondary education, individuals in the early 20th century improved their access to white-collar jobs, but not in subsequent reforms. In considering later reforms, we do not observe these health gains (Clark and Royer, 2013; Galama et al., 2018). According to Malamud et al. (2018), the unclear association between education and health might be due to changes in the occupation structure. Even if education reduces individuals' exposure to manufacture and agriculture jobs, this does not always translate into better working conditions. If, for example, these white-collar occupations are associated with more stress than other unskilled jobs or are more sedentary occupations, it could be detrimental to health (see Böckerman et al., 2008).

5.3. Education quality

Pischke and VonWachter (2008) provide evidence for Germany that reforms increase time at school, but not the quality of education provided. Reforms force adolescents to continue in school, but if this additional education is of poor quality, the effect may be weak. We check this using data on education spending as a quality indicator. The UNESCO database has data on public spending on education as a percentage of GDP for the years 1970–1980 for all the countries we use, except for former communist countries. For the four Southern European countries, the average public expenditure was 3.04% of GDP, while for the default category it was 5.09%. In Table 6 we estimate our model separately, first for the southern countries (Greece, Italy, Malta, Portugal) and then for the rest of countries.

Reforms have a much stronger effect in the southern countries. The first-stage coefficient is 1.13 in the South and 0.26 in the rest. This may be because the years of compulsory education increase more in the South (Table 1). The weighted average of the increase is 2.8 years in the South, and only 1.1 in the rest of countries. Weights are country sample sizes. However, if we divide the first-stage coefficients by the corresponding averages, the effect of the reforms remains greater in the South. For every additional year of compulsory education, our education measure increases in the South by 0.40 years (1.13/2.8) and only 0.24 years in the rest (0.26/1.1). This means that a large part of the exogenous variation produced by the instrument occurs in the South. This could be problematic if one of these two conditions is met: 1) additional education is of poorer quality in the South; ii) in the South there is less health inequality among people of different educational levels. The first condition is supported by the data on public spending in education described above. We add more years of education but it is low-quality, so we should not expect a major impact on health. The second argument receives some support in Table 6. For our three health measures, the correlation between education and health is much smaller in the South. We have an instrument that especially affects countries with low-quality education and where there are no major health differences between people of different educational levels.

5.4. Is education useless?

The variation in education induced by CSLs has no clear effect on our health measures. This may be because that variation is of little importance, because the reforms affected few people, or because they were late. Here we show that this is not the case, since that same variation does have an effect on other important results such as income. In Table 7 we estimate our model again, replacing health outcomes with a measure of income. In particular, we choose the variable *hy022*, total disposable household income before social transfers other than old-age and survivor benefits. Other measures, such as *hy020* (total disposable household income) or *hx090* (equivalized disposable income), include redistributive public transfers.

According to our OLS estimates, one additional year of education is associated with an increase in disposable income of 6–7 percentage points. The correlation is similar for all sub-groups. All IV point estimates are larger, around 0.15–0.26 in most cases, and with more variability (around 0.06–0.09) than OLS. For people from poorly educated families, the effect is not identified because the instrument is weak. In other cases, except for the poor, the OLS coefficient is within the 95% confidence interval of the corresponding IV estimate. For the poor, the 95% confidence interval is to the right of the IV estimate. The confidence intervals of the IV estimates do not include zero, except for the non-poor. This is in stark contrast to our results using health measures, where the IV results always include the lack of effect, while OLS never does.

Since we find a mostly positive effect of education on income, why we do not find an effect on health? One reason may be that income does not always have a positive effect on health. People with more income can spend more on alcohol and cigarettes (see Clark and Royer, 2013; Davies et al., 2018; or Malamud et al., 2018). Furthermore, income effects on health might be positive but small, as suggested by Cesarini et al. (2016) and Janke et al. (2018). Although the more educated can process health information better (Lochner, 2011), it can also happen that new treatments reduce costs relatively more for people with little education, as Goldman and Lakdawalla (2005) suggest. Although some studies find that the use of health services is greater among educated people in Europe (Wagner et al., 2011; Clark and Royer, 2013), it could be the case that these differences in use only appear when subjects are relatively old. If this is the case, our sample may be too young to detect a causal effect of education. Unfortunately, EU-SILC lacks information on these individual behaviors to verify this hypothesis.

Another reason can be the existence of a universal public health service in European countries. Lochner (2011) suggests that education may have less effect on health in European countries than in the United States, because the former have good universal health systems. Other authors use this argument to justify the differences between the USA and Europe (Galama et al., 2018; Meghir et al., 2018). Moreno-Serra and Smith (2012) review the cross-country empirical evidence and conclude that broader health coverage leads to better population health, particularly among poor people. This is related to the discussion on the average age in the sample. When individuals are relatively young, public health services can mitigate health differences between the rich and the poor. As individuals get older, medical conditions get worse and it is more difficult for public health services to compensate for these differences. Again, this may be a reason why previous works that use an older sample find an effect of education on health. To conclude, even if higher education translates into higher earnings, differences in income are not crucial for access to better medical care.

Finally, a potential criticism of our work is that self-reported health measures are not a good indicator of objective health. However, there is evidence suggesting that self-reported health measures are good predictors of objective measures as, for instance, mortality (Idler and Benyamini, 1997). Moreover, our results are similar for the three health measures we use. All of them are self-reported, but both *not limited* and *no chronic* can be seen as more objective than *good health*. In particular, stating that you have a chronic disease or condition should require that the condition has been diagnosed by a doctor. Another issue is the possibility of differential health reporting by educational level. Mackenbach et al. (1996) find more underreporting of chronic conditions among less educated people. In contrast, Johnston et al. (2009) find no differences in false reporting rates across educational levels. The literature has not reached a consensus on this issue yet.

6. Conclusions

We study the causal effect of education on different health outcomes using EU-SILC data. Our identification strategy exploits exogenous variation from CSLs in Europe. We find no effect on any of the three health measures considered. We cannot exclude positive and large effects either due to the large variability of our IV estimates. We reach the same conclusion when we estimate our model for specific groups by sex, parental education, or economic situation of the family. This finding is robust to different alternative specifications.

We discuss different explanations for our results. First, consistently with the cumulative (dis)advantages hypothesis, our sample is too young to observe the potential positive benefits of education. Second, the exogenous variation that we exploit occurs in secondary education and we cannot rule out that interventions at other levels may have a protective effect on health. Third, a large part of the increase in education occurs in countries where education was of poor quality at the time of the reforms. Forcing young people to spend more time in schools that give them a mediocre education may not be enough for this to be reflected in better adult health. Finally, it could also be the case that the observed improvement in schooling as a result of the reforms is sufficient to improve income but maybe not for non-income related outcomes as health (even though a few papers in the literature using multi-country reforms with comparable first-stage results end up finding a positive impact on health outcomes as discussed above).

CRediT authorship contribution statement

Pedro Albarrán: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing - original draft, Writing - review & editing. **Marisa Hidalgo-Hidalgo:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing - original draft, Writing - review & editing. **Iñigo Iturbe-Ormaetxe:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing - original draft, Writing - review & editing.

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Appendix A. Supplementary data

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