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Schooling and adult health: Can education overcome bad early-life conditions?*

Pedro Albarrán, Marisa Hidalgo-Hidalgo and Iñigo Iturbe-Ormaetxe**

Abstract

We provide new evidence on the causal effect of schooling on self-reported health and focus on its possible heterogeneous impact. We use data from the 2005 and 2011 cross sections of EU-SILC, exploiting quasi-experimental evidence from schooling reforms in 16 European countries that extend the period of compulsory schooling. Our estimation strategy uses the number of years of compulsory education as an instrument for education levels. We find that reforms affect positively the schooling level only for those individuals from low-educated families. The education level is a strong determinant of adult self-perceived health: one additional year of schooling raises the probability of reporting good health by about seven percentage points. However, this effect is not homogeneous. On the contrary, the effect concentrates on individuals who were raised in relatively well-off families. Our interpretation is that we identify the effect of an exogenous variation in education that occurs in the adolescent years, when it may be too late to have a significant impact on individuals with a poor family background.

Keywords: Schooling reforms, compulsory education, health outcomes, EU-SILC.

JEL classification numbers: I1, I2, I3, J6.

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1. Introduction

Education and health are strongly correlated. This association has been observed in many countries and time periods, and for many different health measures. There is also substantial evidence on the existence of a causal effect of schooling on several health outcomes (see Lochner, 2011). Nevertheless, little is known about the possible heterogeneous impact of education on health status: does it work for all? Who does it really affect to?

Our objective is to estimate the causal effect of schooling on adult health combining data from several countries. We explore whether the effect of education on health is homogeneous or if some groups are more affected than others, focusing on two individuals' characteristics: gender and family economic background (during childhood), which is a well-known determinant of initial health conditions.

We use data from the 2005 and 2011 cross sections of EU-SILC (European Union Statistics on Income and Living Conditions). EU-SILC is a very rich database containing information on education and health for several European countries. Additionally, the 2005 and 2011 cross sections include retrospective information on family background, which allows us to analyze whether the effect of education on health is homogeneous.

As most of the literature, we use education reforms to identify the effect of education on health. Several European countries have reformed their educational systems during the 20th century, increasing the number of years students have to remain at school. These reforms are a valid instrument for schooling as long as they are correlated with schooling levels and uncorrelated with adult health, conditional on other individual characteristics. The increase in compulsory schooling affects those born from a given year, but does not affect those born previously. Children who are different in age by a few years are exposed to different years of compulsory education.[1] Our rich dataset also allows us to explore whether educational reforms have more impact on some specific groups, as some recent literature suggests.[2]

Most estimates of the causal impact of schooling using education reforms from several countries as instruments use specifications that assume common time trends across countries in the factors affecting different birth cohorts. That is, they assume that all other changes, which occur across countries during the period under study, are uncorrelated with the education reforms, the schooling increases, and adult health. However, it might be the case that countries (or specific subgroups of individuals within countries) with larger education increases have also greater secular health improvements. Then, failing to control for country-specific time trends will cause overestimation of the effect of schooling on health. Indeed,

[1] See Cutler and Lleras-Muney (2012).
[2] See Brunello *et al*. (2013) or Crespo *et al*. (2014).

Stephens and Yang (2014) criticize this common trend assumption and find that the results in several works for the US are not robust to the inclusion of region-specific time trends. In this paper, we relax the common trend assumption using instead country-gender-family background-specific time trends to capture any underlying differential trends that might be driving both education and health for different subgroups in a given country.

We find that education has a positive effect on self-reported health: each additional year of education increases the probability of reporting good health by about 7 percentage points. This is a large effect: it represents almost 12% of average proportion of individuals reporting good health. Interestingly, this effect is quite heterogeneous. First, when we divide individuals according to their family economic background we find that the above effect only works for individuals raised in relatively well-off families, while we do not find an effect for those raised in poor families. This may be because the instruments we use are weak for this sub-group, which prevents us from identifying an effect for them. This result is remarkable, since it means that education only has an effect on those individuals starting from good enough initial health conditions. In other words, education by itself cannot compensate the negative impact that early-life conditions have on adult health. A possible explanation could be that, as most education reforms affect individuals that are in secondary education, education increases at this level are too late if family economic background conditions are bad. These results are in line with some recent literature that finds lower returns to college for individuals who grew up in disadvantaged households (Heckman, 2000; Cunha and Heckman, 2007 or Brunello *et al.*, 2017). Second, we find a stronger effect for women than for men in line with recent literature (see Brunello *et al.* 2016). Finally, we also find that education reforms have a much larger effect on the education level of some specific groups. In particular, women from non-educated families are the ones mostly affected by the reforms.

We add to the literature in several directions. First, we explore both the heterogeneous effects of education reforms on years of schooling and of education on health status focusing on gender, parental education and initial economic conditions. Second, we add more flexibility to the standard common trend assumption in the literature by allowing for separated time trends according to both gender and parental education. Third, we contribute by exploiting a very large database that has information from several European countries. The sample is quite homogeneous in terms of years of birth, and contains a rich set of controls regarding family characteristics determined well before decisions on education. In addition, its large sample size allows us to use smaller windows than other works (a too large window makes results difficult to believe). Finally, we also explore possible drivers of our result, by analyzing activity and occupation of individuals. Contrary to previous results in the literature, our results suggest that activity and occupation choices can explain very little of the education-health gradient.

The paper is organized as follows. Section 2 reviews the literature on the possible causal relationship between education and health. Section 3 describes the data used in the paper and presents some preliminary results. Section 4 presents the empirical model and Section 5 contains our main findings. Section 6 contains a number of robustness checks. Finally, Section 7 concludes. In the Appendix, we list all the variables we use, we provide a summary of the sixteen educational reforms, and we comment on the impact of education on other health measures.

2. Literature review

A large number of works have documented the existence of a strong positive association between education and different measures of health (Grossman, 2005 and 2008, Cutler and Lleras-Muney, 2006, Eide and Showalter, 2011, Lochner, 2011). This correlation remains after controlling for a number of individual characteristics. Grossman and Kaestner (1997) and Grossman (2000) conclude that years of schooling are the most important correlate of good health. In particular, it is more important than income or occupation. The problem is how to identify if this correlation is causal, and which is its direction. Is schooling affecting health, or health affects education? Or is it an omitted variable that is affecting both?[3]

Several studies try to uncover a causal effect from education to health outcomes, using educational reforms as a source of exogenous variation in education levels. They are, therefore, helpful to identify the effect of education on different outcomes.[4] As far as we know the first paper using education reforms to identify the effect of education on health is Lleras-Muney (2005).[5] Using U.S. censuses of 1960, 1970 and 1980, she considers state-variation in education laws across the USA as an instrument for education. She finds that one additional year of schooling reduces the probability of dying in the next 10 years by between three and six percentage points. Mazumder (2008) finds that the effect found in Lleras-Muney (2005) disappears when including state-specific time trends. However, when he uses data

[3] Fuchs (1982) suggests individual time preferences.

[4] Changes in years of compulsory education have been extensively used in the literature to estimate the effect of education on wages. See Oosterbeek and Webbink (2007) for a review of this literature.

[5] There is an extensive literature trying to find a causal effect of education and health. To get a good approximation to the topic the reader is referred to the survey by Lochner (2011). Good summaries of the state-of-the-art can be found in Eide and Showalter (2011) and Cutler and Lleras-Muney (2012).

from SIPP (Survey of Income and Program Participation), he finds a significant positive effect of education on general health, a result that is robust to the inclusion of state-specific time trends. As Lochner (2011) claims, without the inclusion of these trends it may happen that secular improvements in health are incorrectly attributed to school reforms, biasing the results.

A number of authors have used a similar methodology using data from other countries, with mixed findings. Silles (2009) uses data from the General Household Survey for England, Scotland, and Wales. She uses changes in minimum school leaving age (SLA) that took place in 1947 and 1973 as instrument for educations, finding that one additional year of schooling increases the probability of reporting good health about 4.5–percentage points. Banks and Mazzona (2012) use the English Longitudinal Study of Ageing (ELSA) and the 1947 reform, finding a large impact of education on male memory and executive functioning at old age. Other authors who find positive effects of education using a similar strategy are Kemptner *et al.* (2011) for Germany (who find a strong and significant causal effect of years of schooling on long-term illness and disabilities for men, but no effect for women), Arendt (2005) for Denmark, and Van Kippersluis *et al.* (2011) for The Netherlands. On the contrary, Clark and Royer (2013), Oreopoulos (2006), and Jürges *et al.* (2013) using data from England do not find significant effects of compulsory education on health. In the same line, Albouy and Lequien (2009) find no effect on mortality in France. Some papers study the effect of education on health biomarkers. Powdthavee (2010) uses data from 1991 to 2007 from the Health Survey for England and finds that an additional year of school from the 1947 reform reduces the probability of hypertension by between 7-10 percentage points. However, he finds no effect for the 1973 reform.

Some recent papers combine data from several countries, exploiting variation induced by reforms across countries and birth cohorts. Oreopoulos (2006) considers reforms in USA, Canada, and the UK. Fonseca and Zheng (2011) use data from the Survey of Health, Ageing and Retirement in Europe (SHARE), from ELSA and from the Health and Retirement Study (HRS) in the USA. They find that education reduces significantly the probability of reporting bad health. It also reduces the prevalence of diabetes and hypertension, but has no effect on other health outcomes as heart disease or stroke. Brunello *et al.* (2013) study the effect of education on the body mass index (BMI) for 13 European countries finding that education has a protective effect on BMI, but only for females. Crespo *et al.* (2014) use SHARE data finding a significant effect of education on mental health in adulthood. An extra year of schooling reduces the probability of suffering depression in 6.5 percentage points. They find that these effects are heterogeneous and identify the typology of individuals that are particularly affected by the reforms. Brunello *et al.* (2016) use data from both SHARE and ELSA, finding that one additional year of schooling reduces self-reported poor health by between 4-6.4 percentage points for women and between 4.8-5.4 points for men. They suggest that health behaviors as smoking, drinking, exercising and eating a proper diet may account for at most 45% of that effect. Mazzona (2014) uses also SHARE data and finds that education has a positive effect on self-rated health and on reducing depression, but only for men. He claims that a possible mechanism is that schooling improves occupational patterns of men by decreasing time spent in blue-collar jobs. Gathmann *et al.* (2015) use data from SHARE and other sources finding that education yields small reductions in mortality for men, but no effect for women.[6]

3. Data and preliminary results

Since our focus is on heterogeneity, we need rich enough data containing information on the characteristics of the family in which individuals were brought up. We combine data from the 2005 and 2011 cross sections of EU-SILC (EU Statistics on Income and Living Conditions). These databases contain information on income, education, health, poverty, social exclusion and living conditions in the European Union. The reason for using the 2005 and 2011 cross sections is that they include a special module on intergenerational transmission of disadvantages.[7] These modules provide retrospective information on parental background and childhood circumstances including family composition, occupation and the educational level of parents. They also provide information about the economic situation in the household when the individual was a teenager. This is valuable information about circumstances that happened well before the end of the compulsory education period. Since we use the intergenerational modules, we have to exclude all individuals who are not in the age range of the module (26-66 in 2005, 26-59 in 2011) or are not the selected respondent.[8] This restricts our sample to individuals born in 1939 or later. Since we want to assess the long-run effect of household characteristics, we exclude those individuals who did not live with their parents, but in a collective house or in some institution.

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[6] Other papers apply a similar methodology using a multi-country approach to study the effect of education in various outcomes. This is the case of fertility (Fort *et al.*, 2016), wages (Brunello *et al.*, 2009), religious attitudes and superstition (Mocan and Pogolerova, 2014), or attitudes toward immigrants (D'Hombres and Nunziata, 2016).

[7] For an overview of EU-SILC, see Wolff, Montaigne, and Rojas González (2010). To access further information about EU's regulations concerning the SILC, data documentation provided by Eurostat, and SILC variable lists, we recommend the EU-SILC web portal provided by the GESIS research institute at http://www.gesis.org/

[8] The intergenerational modules do not consider as eligible for inclusion in the survey individuals who are not the selected respondent in register countries and individuals not in the age range (26-66). For additional details see http://ec.europa.eu/eurostat/web/income-and-living-conditions/methodology/list-variables

We build a database selecting 16 European countries out of the 31 countries in EU-SILC, which are those for which we have reliable information about reforms in compulsory schooling. The list of countries is: Austria, Czech Republic, Denmark, France, Greece, Hungary, Iceland, Ireland, Italy, Malta, The Netherlands, Poland, Portugal, Slovak Republic, Spain, and United Kingdom.[9]

Table 1 displays information of the reforms we use. All of them increased the number of mandatory years of schooling by one or more years. They cover the period from 1960 (Czech Rep. and Slovak Rep.) to 1976 (Greece). We also show the first cohort potentially affected by the reform, which goes from 1946 to 1964. In the Appendix (Section B) we provide additional details on the reforms we use.

We focus on a measure of self-reported health. In the survey, individuals answer the question "How is your health in general?" The five possible answers are very good, good, fair, bad, and very bad.[10] We code this information into a dummy variable called "*good_health*" that takes value 1 if self-reported health is "very good" or "good."[11] Table 2 contains summary statistics for this and all other variables used in our analysis.

There are several mechanisms behind the impact of early-life conditions on adult health. First, exposure to adverse economic conditions early in life may alter forever the structure and/or functions of organs and systems (see Case *et al*., 2005 among others). Second, individuals who lived when young in a deprived environment may lack some healthy habits that a better-off family provides at home (see Kuh and BenShlomo, 2004, Lochner, 2011, or more recently Alessie *et al*., 2017 and references therein). Third, the financial situation of the family may impact health because it determines access to health insurance, medical treatments, nutrition, etc. All these arguments call for including controls that capture

[9] We exclude those countries for which we lack information on reforms (Cyprus, Estonia, Croatia, Latvia, Lithuania, Luxembourg, Romania and Slovenia), those countries whose reforms affected cohorts born before 1939 (Bulgaria), and those whose reforms were implemented gradually through a number of years or at the regional/local level (Finland, Norway, Sweden and Switzerland). We also exclude Germany since the reforms we are aware of were implemented gradually between 1949 and 1969 across the 10 länder. Unfortunately, in the EU-SILC we lack regional identifiers for Germany. In addition, using German reforms implies to assume that individuals went to school in the same region in which they are currently living. Finally, we exclude Belgium since the reform took place in 1983, much more recently than in the other countries.
[10] See Section A of the Appendix for a detailed description of how variables are constructed.
[11] In the Appendix (Section C), we replicate the analysis for other two health measures: not-limited by health problems and non-chronic illness or condition.

Table 1: Reforms in compulsory education

Country	Year of reform	First cohort affected	Change in years of compulsory education	School entry age
Austria	1966	1952	8 to 9	6
Czech Rep. / Slovakia	1960	1946	8 to 9	6
Denmark	1971	1957	7 to 9	7
France	1967	1953	8 to 10	6
Greece	1976	1964	6 to 9	6
Hungary	1961	1947	8 to 10	6
Iceland	1974	1960	8 to 9	7
Ireland	1972	1958	8 to 9	6
Italy	1963	1952	5 to 8	6
Malta	1972	1958	8 to 10	5
Netherlands	1975	1960	9 to 10	7
Poland	1966	1952	7 to 8	7
Portugal	1964	1956	4 to 6	6
Spain	1970	1957	6 to 8	6
UK	1972	1958	10 to 11	6

Note: See Section B of the Appendix for details on these education reforms.

Table 2: Summary Statistics

Variable	Mean	Std.Dev.	Min	Max	Obs
Female	0.526	0.499	0	1	78,151
Age	53.064	5.224	39	66	78,151
Years of education	12.400	5.172	0	27	77,989
Years of compulsory education	8.027	1.543	4	11	78,151
Reporting good health	0.577	0.494	0	1	77,886
Not limited	0.765	0.424	0	1	77,810
No chronic	0.671	0.470	0	1	77,834
Poor family	0.359	0.480	0	1	78,151
Non educated family	0.749	0.434	0	1	76,828
Non citizen	0.001	0.025	0	1	78,146
Lived with father only	0.014	0.119	0	1	78,136
Active	0.649	0.477	0	1	78,022
White collar job	0.596	0.491	0	1	77,314
Sample size by country					
AT					1,195
CZ					2,474
DK					2,605
EL					2,620
ES					11,987
FR					2,709
HU					4,551
IE					2,896
IS					1,283
IT					14,712
MT					2,062
NL					4,750
PL					13,653
PT					2,094
SK					2,852
UK					5,708
Total					78,151

Note: The sample corresponds to a window size of 7 years and contains all individuals for which years of compulsory education and *poor_past* are not missing. Source: EU-SILC 2005 and 2011.

these initial conditions. In the survey, we have information about the financial situation in the family when the individual was young. We build a dummy variable (*poor_past*) that takes value 1 for those individuals who lived when young in a family with frequent financial difficulties. We interpret this variable as a proxy of several family background characteristics that may condition health status in childhood. Education might modulate this well-known impact of early-life economic conditions on adult health status. As commented above, the goal of this paper is precisely to analyze the potentially heterogeneous effect of education on health by gender and family economic background conditions.

Table 3 below illustrates the correlation between family economic background and adult health. We compute probabilities (and standard errors) for the current self-reported health status, conditional on the two possible values of *poor_past*. We also do it separately for men and women. As Table 3 shows, there is a strong correlation between both variables.

The probability of reporting good health status among individuals from well-off families is 0.6577 for men and 0.6177 for women. However, for individuals from poorer families these probabilities fall to 0.5018 and 0.4437, respectively. We see that family background affects dramatically the probability of declaring good health in adulthood. We also see that women tend to report worse health than men do.[12]

As previously commented in the paper we study if education has a positive impact on adult health and, furthermore, if this effect helps to mitigate the effect of pre-existing conditions as shown in Table 3. This would be the case if we find a bigger beneficial effect of education on health for those individuals who come from a disadvantaged household.

The survey also contains some questions on family background that allow us to measure parental education. This is very important information since education of parents has been shown to be the most important factor in explaining the education of their children (see, among others, the review by Haveman and Wolfe, 1995). The existing literature on the intergenerational transmission of education provides some evidence on the potential transmission channels, for instance, labor-market effects, better home environments (Carneiro *et al*, 2013) or the fact that more schooling can increase parents' valuation of children's education (Piopiunik, 2014). To summarize parental education, that is, family educational background, we build a dummy variable (*non_educated_family)* that takes value 1 when the

[12] See Case and Paxson (2005).

Table 3: Self-reported health and family economic background

	Men		Women		All	
	Mean	**SE**	**Mean**	**SE**	**Mean**	**SE**
Non-poor	0.6577	0.0031	0.6177	0.0030	0.6366	0.0022
Poor	0.5018	0.0043	0.4437	0.0041	0.4713	0.0030
Difference test *p*-value	<0.0001		<0.0001		<0.0001	

Note: The sample corresponds to a window size of 7 years and contains all individuals for which years of compulsory education and *poor_past* are not missing. Source: EU-SILC 2005 and 2011.

highest level of education of parents is primary education.[13] We propose to use this variable not only as a control, but also to see whether education reforms have a different impact on individuals depending on parental education. We conjecture that this effect should depend on how much parents value children's education. We expect, therefore, this effect to be stronger for those individuals from families with little education.

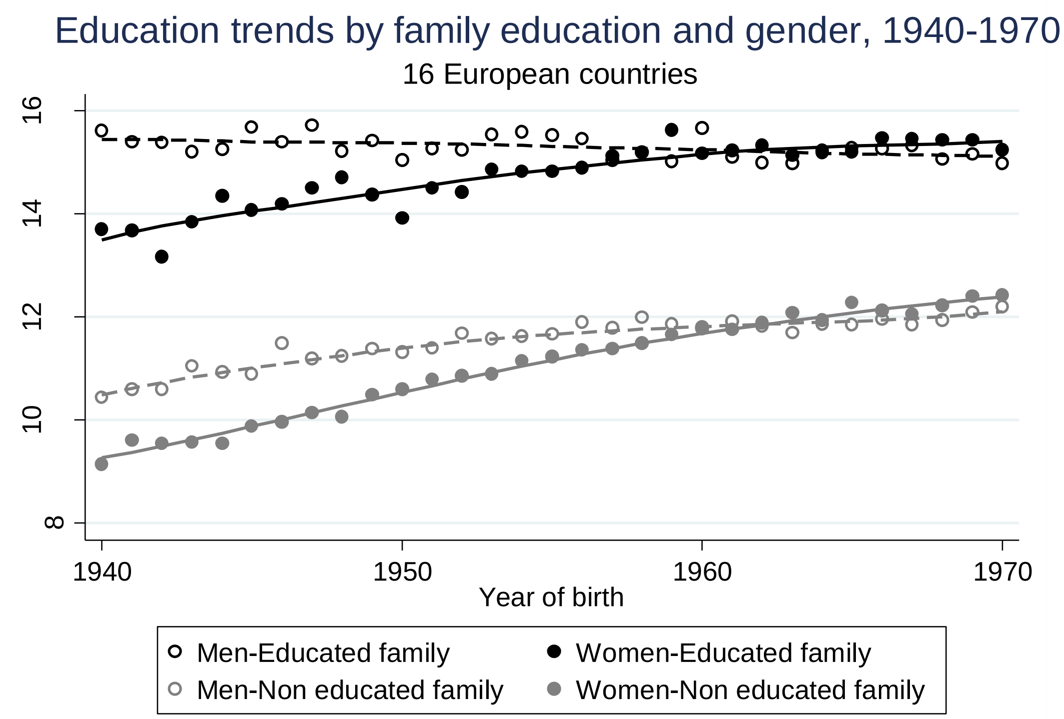
We use years of schooling as our measure of education. EU-SILC reports the highest level of education attended by individuals, together with the year when this highest level was achieved. We eliminate from our sample all individuals still in education. The number of years of schooling ("*years_educ*") is constructed as the year when the highest level was attained minus the year of birth minus school entry age. In Table 1 we see that there is some variation across countries and time in school entry age. We exclude all individuals for which this variable takes negative numbers. We also restrict the variable years of education to be in a given interval for each level of education.[14] We also exclude individuals who were not born in the country of residence, since we are not sure that they attended schooling in the country they live.[15] We represent in Figure 1 average years of schooling for the period we will consider corresponding to our 16 countries, together with locally weighted regressions, separated by gender and family education.

[13] We have tried with using only the educational level of the mother or the father and results are similar.

[14] There are six levels of education in EU-SILC (variable *pe040*): 0 pre-primary; 1 primary; 2 lower secondary; 3 (upper) secondary; 4 post-secondary non-tertiary; 5 tertiary. Years of education are restricted to be in the interval [0, 12] for those with pe040 = 1, [6, 14] if pe040 = 2, [9, 17] if pe040 = 3, [12, 25] if pe040 = 4, and [14, 27] if pe040 = 5. By doing so we get country average years of schooling similar to the educational attainment expressed in average number of years in formal education provided by the OECD in its annual report (see, for example, OECD-Education at a Glance 2005). In addition, we checked the robustness of our results to several definitions of the variable "years of education" by modifying the intervals for the different education levels. We have also done the analysis using as our measure of schooling the median value for each country-cohort-education level. The results obtained are quantitatively very similar to the main ones in the paper and available from the authors upon request.

[15] According to the EU-Labor Force Survey (see EU-LFS, 2014), EU citizens working and living in an EU country other than their own represent only 3.3% of total employment in the EU. Still, we are aware that not considering immigrants might provide an incomplete view of the protective effect of education on health status in Europe. Unfortunately, the EU-SILC database does not provide information neither about the individuals' country of birth (when it belongs to the EU) or the year of migration without which we cannot claim whether or not these individuals were exposed to some education reform.

Figure 1: Average years of education by gender and family education, 1940-1970



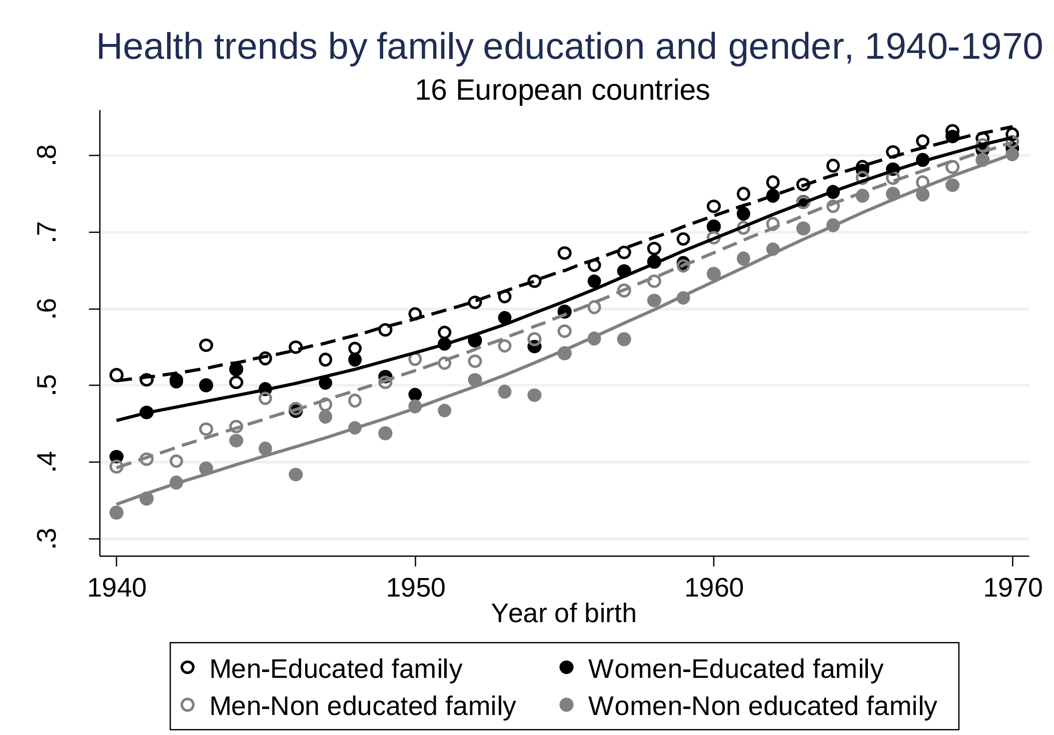
We see in Figure 1 that education time trends are quite different among the four subgroups of individuals. All trends are increasing, except for men from educated families. We find that average education levels converge between family types and also that within family types the gender gaps tend to disappear. For individuals raised in families with little education, average schooling increases from 9.75 years in 1940 to 12.32 years in 1970. For individuals raised in educated families the increase is from 14.59 to 15.12 years. This means the gap between these two groups shrinks from 4.84 years in 1940 to 2.80 years in 1970. Within each family type, we see that women start from a worst position, but eventually catch up men.[16] Women raised in families with a low education level are the ones experiencing the largest increase in the period. For women in this group, average years of education jumps from 9.15 years in 1940 to 12.42 in 1970, an increase of 3.27 years. The change for men from low educated families goes from 10.44 years in 1940 to 12.20 years in 1970, an increase of just 1.76 years. In this group, the increase for women is almost twice as big as the one for men. Focusing on individuals raised in educated families, we find that average schooling of women raises from 13.7 years in 1970 to 15.24 years in 1970, an increase of more than one and a half years. For men, average schooling actually declines from 15.61 to 14.98 years. As a

[16] See Figure A1 in the Appendix, Section D which shows the years of schooling trend by gender at the country level. As can be observed, the pattern commented above is quite similar for most countries.

result of this, the gender gap disappears from 1962 for the two groups. Even women seem to beat men from that date onwards.

In Figure 2 we represent the fraction of people reporting good health for the period we consider corresponding to our 16 countries, together with locally weighted regressions, again separated by gender and family education. Although now it is not as clear as in the previous figure, we still find different trends for the four subgroups. We also see that all groups tend to converge.

Figure 2: Health trends by gender and family education, 1940-1970

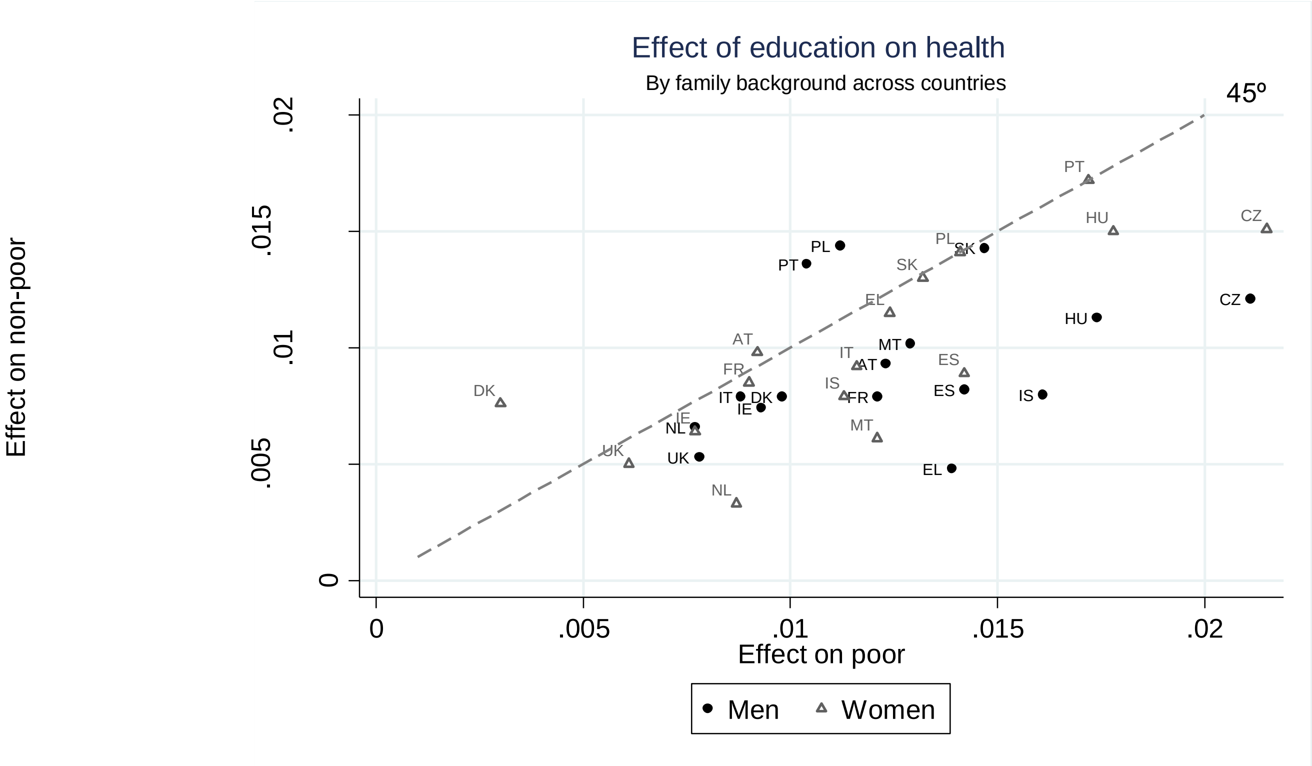


In addition to the family economic background and family educational background we use the variable *father_only* (=1 if the mother was not present in the family when the individual was young) to capture other family background characteristics. Finally, we include some additional individual characteristics as *noncitizen* (=1 if the individual is not a citizen of the country), and *CS2011* (=1 if the observation corresponds to the 2011 cross section). Our final sample consists of 78,151 individuals from 16 countries. A 54.3% belong to the 2005 wave (42,416 individuals) and the remaining 45.7% (35,735 individuals) to the 2011 wave.

Our preliminary evidence suggests that, indeed, there is a large amount of heterogeneity in the effect of schooling on health. We begin by studying the association between our measures of education and health for each one of the 16 countries in our sample. To do this, we run an OLS regression for each country in which the dependent variable is *good_health* and the main explanatory variable is *years_educ*. To capture the possible heterogeneous effects of education depending on initial economic conditions mentioned above we interact *years_educ* with *poor_past*, *gender*, and *poor_past*gender*.[17] This allows us to compute the effect of education on health separately for poor men, poor women, non-poor men, and non-poor women. Later on we will more deeply analyze these four groups by estimating separate equations (see Section 5). As Figure 1 above shows, education time trends for men and women are very different. Thus, in order to capture that fact we include in each regression a separated time trend for each gender, adding year-of-birth fixed effects by gender.

In Figure 3, we represent the estimated effects of education on health, dividing individuals in our sample according to both family economic background and gender. Each point corresponds to one country. On the horizontal axis, we represent the effect of education for individuals from poor families and in the vertical axis the effect for individuals from non-poor families. We do this separately by gender. The only case in which the coefficient of education is not statistically different from zero is the one corresponding to poor women in Denmark.

Figure 3: Effect of education on health, by family background



[17] As additional explanatory variables we include *poor_past*, a *gender* dummy (=1 for women), *non_educated_family*, *noncitizen*, *father_only*, and *CS2011*. We also include all possible interactions between *poor_past*, *non_educated_family*, and *gender*. That is, *poor_past*gender*, *non_educated_family*gender*, *non_educated_family*poor_past* and the triple interaction *non_educated_family*poor_past*gender*.

We see that in some cases the dots are close to the 45º line, meaning that the effect that education has on health within a country is similar for individuals from poor and non-poor families. However, we also observe that many are far away from the 45º line. Moreover, when this happens, in general they lie below the line. This is the case of men in Greece, for instance, where the effect on poor men is about three times bigger than for non-poor men. This tells us that in many countries the association between education and health seems to be stronger for individuals who suffered adverse economic conditions when young.

It is also important to remark that heterogeneity is bi-dimensional, since the effect of education seems to work differently according not only by family background but also by gender. To sum up, this preliminary evidence points out that in our sample of European countries the association between schooling and adult health depends on both gender and family background as measured by early-life economic conditions.

4. Empirical model

To study the possible causal effect of education on health outcomes we focus on a model with two equations. The relationship of interest between education and health is given by the second-stage equation:

$$H_i = \beta_0 + \beta_1 E_i + \beta_2 X_i + \beta_3 V_i + v_i, \quad (1)$$

where H_i is our measure of adult health, E_i denotes years of education, X_i denotes a vector of individual characteristics, and V_i denotes family background variables determined before schooling is completed that could potentially affect adult health. We include a set of dummy variables to control for invariant factors within countries.

Since we use data corresponding to different years, we always include country-specific linear time trends.[18] As commented above, Stephens and Yang (2014), using regional data from the US, show that moving from a common time trend to region-specific time trends has a dramatic effect on the estimation results. In this paper, we add more flexibility to the model by allowing for different time trends according to both gender and parental education. Figure 1 illustrates this choice for education. We observe that the evolution of education over time is clearly positive for both men and women, but with very different growth rates. The same applies to health as we show in Figure 2. If we do not allow for these differential time trends we may end up attributing to the reforms an effect on women that simply comes from a long-term increase in education. In other words, the variation we have identified is not truly

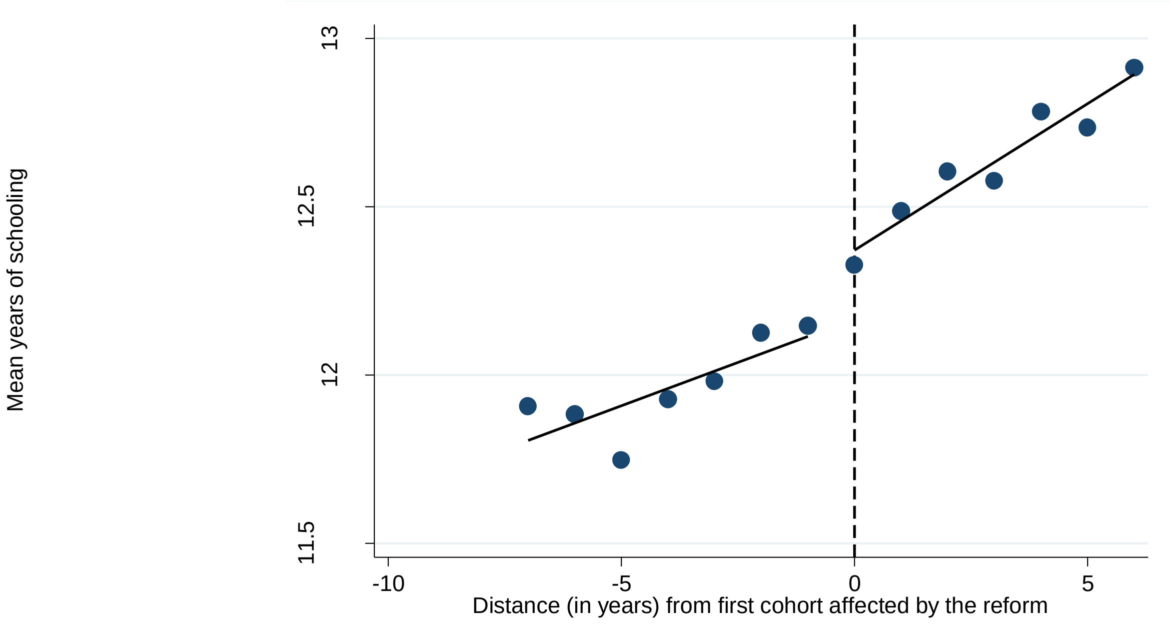
[18] We have tried with country-specific quadratic terms, and the results are similar.

exogenous. In our preferred specification we, therefore, include country-gender-family background-specific time trends to capture any underlying differential trends that might be driving both education and health for different subgroups in a given country. In this way we identify the effect of the reform on those individuals that without the reform would not have acquired more education, even with the increasing time trend. This means that the tendency was not enough to affect them, and it is the reforms what really affected them.

The error term in (1) is likely to contain unobserved individual characteristics that affect both education and health in the same direction. Estimating it by OLS may produce biased estimators of the parameters of interest. To tackle this problem we exploit the exogenous variation of schooling induced by changes in the number of years of compulsory schooling that happened in many European countries in the 1960s and 1970s. All these reforms imposed increases in the number of years individuals had to stay at school (see Table 1). Our measure of exposure to the reform is the number of years of schooling that each cohort is required to attend by law. We define control and treatment groups that are specific of each country. In this way, children who are only a few years apart were exposed to different levels of compulsory schooling, and this in turn should affect their education levels. The crucial assumption for identification is that cohorts of treatment and control groups are comparable except for exposure to treatment. We face a trade-off when defining the number of year-of-birth cohorts included in the treatment and control groups. The larger is the number of cohorts included, the larger the sample size is. However, including many cohorts makes more difficult to assume that both groups are comparable. It may happen that there are trends in education and health improvement driving the results. As a first approximation, we include seven cohorts in each group. Later on, we also check the robustness of our results to the inclusion of fewer or more cohorts in both groups.

As an example, consider the case of Austrian reform (1966). Years of schooling increased from 8 to 9, while minimum leaving age increased from 14 to 15 (Gathmann *et al.*, 2015). The first cohort potentially affected by this reform was those born in 1952, since they turn 14 in 1966. Cohorts born before 1952 were not affected by the reform, while all cohorts born after 1952 were also affected. This means that all individuals born in Austria between 1945 and 1951 (control group) are assigned 8 years of compulsory schooling, while those born between 1952 and 1958 (treatment group) are assigned 9 years of compulsory education. Note that we are implicitly assuming that all individuals included in our sample went to school in the country in which they were living at the time of the survey.

Figure 4: Average years of education according to distance (in years) from the reform



In Figure 4 above we plot average values of years of schooling for the 16 countries in our sample according to distance in years from the year of reform. For instance, distance 0 corresponds to individuals born in Austria in 1952, individuals born in Czech Republic in 1946, etc. There is a jump upwards of about 0.1 years with respect to the trend, which means that reforms seem to have an impact on years of schooling (see also Brunello *et al.*, 2013 and Brunello *et al.*, 2016 for a similar finding).

The key assumption for identification of the effect of education on health status is that, within each country, additional schooling was assigned to individuals only based on their birth date, and independently of their future health. The exclusion restriction can be justified since it is hard to argue that the number of years in compulsory education may have a direct effect on adult health, once we have controlled for educational attainment of the individual and for several background characteristics like education of parents, financial situation of the family, etc. Our claim is that its effect on health operates indirectly through the schooling level of individuals.

Following results in the most recent literature, we study whether educational reforms have more impact on some particular groups. For instance, Brunello *et al.* (2013) have found that extensions of compulsory education are more helpful for women. Similarly, it is found that individuals from disadvantaged families, as measured by those living in rural areas (Brunello *et al.*, 2017) or with poor socioeconomic status (Crespo *et al.*, 2014), should be the ones more affected potentially by the reforms. In order to capture these heterogeneous effects we include as additional instruments different interaction terms between years of compulsory schooling and both the gender dummy and the dummy for parents' education. Here the exclusion restriction will hold if the effect of either of these two variables on health does not depend directly on the number of years an individual is compelled to be at the school. There may be an effect, but again we assume this effect operates indirectly through the education level of the individual. In addition, this specification allows us to have more instruments for education, which could improve the precision of our first stage. The first-stage equation that we estimate is:

$$E_i = \gamma_0 + \gamma_1 C_i + \gamma_2 C_i G_i + \gamma_3 C_i NEF_i + \gamma_4 C_i G_i NEF_i + \gamma_5 X_i + \gamma_6 V_i + \varepsilon_i, \qquad (2)$$

where C_i describes the number of years of compulsory education corresponding to individual i, G_i is the gender dummy and NEF_i is the *non_educated_family* dummy.

Notice that the effect of education on health in (1) is homogeneous across individuals. However, our preliminary evidence suggests that there is a large degree of heterogeneity on the effect of schooling on health. Indeed, the results in Figure 2 suggest that heterogeneity is bi-dimensional: the impact of years of schooling on health seems to work differently according to both gender and initial economic conditions. In this paper, we study whether and to what extent this result holds once we account for the endogeneity of the individuals' levels of schooling and for flexible time trends. To address this issue and to allow for heterogeneous effects, we run separate regressions according to individuals' initial family economic background conditions. In this sense, we can check if more education can overcome a disadvantaged background.

5. Results

Table 4 presents the results of the first-stage equation corresponding to three specifications that have different time trends. In Column I, we show the estimates corresponding to a model in which we only include a country-specific time trend. In Column II, we have a separate country-specific time trend by gender. Finally, in Column III we also allow these trends to vary according to parental education. We use the same set of instruments in all cases as described in Equation (2).

Table 4: Effect of compulsory education on years of schooling

	(I)	(II)	(III)
Overall	0.0530	0.0533	0.0991**
	(0.0459)	(0.0462)	(0.0431)
Decomposition by gender			
Male	0.0165	-0.0156	0.0320
	(0.0556)	(0.0522)	(0.0505)
Female	0.0858*	0.1153**	0.1594***
	(0.0454)	(0.0557)	(0.0521)
Difference test p-value	0.1018	0.0213	0.0237
Decomposition by parental education			
Educated family	-0.4188***	-0.4177***	-0.0340
	(0.0736)	(0.0740)	(0.0663)
Non educated family	0.2112***	0.2113***	0.1438***
	(0.0436)	(0.0439)	(0.0440)
Difference test p-value	<0.0001	<0.0001	0.0039
Decomposition by gender and parental education			
Male and educated family	-0.3835***	-0.4575***	-0.0604
	(0.0932)	(0.0848)	(0.0941)
Male and non-educated family	0.1499***	0.1317***	0.0628
	(0.0513)	(0.0495)	(0.0503)
Female and educated family	-0.4502***	-0.3822***	-0.0106
	(0.0677)	(0.0816)	(0.0842)
Female and non-educated family	0.2664***	0.2829***	0.2166***
	(0.0474)	(0.0547)	(0.0554)
Time Trends			
Country-specific	yes	yes	yes
Country-gender-specific	no	yes	yes
Country-gender-family background-specific	no	no	yes
F-Statistic for time-trends	5.22	18.19	25.66
p-value	<0.0001	<0.0001	<0.0001
F-Statistic for instruments	35.8735	34.9219	5.3672
p-value	<0.0001	<0.0001	0.0004
Observations	76,396	76,396	76,396

Note: Robust standard errors (in parentheses) are clustered by country and birth cohort. *** p<0.01, ** p<0.05, * p<0.1. All regressions include as controls the variables *noncitizen*, *father_only*, *CS2011*, *poor_past*, *non_educated_family*, and *gender* together with all interaction terms among the last three ones. The F-test for time-trends refers to the joint significance of specific time trends parameters (15 in Column I, the additional 15 in Column II with respect to Column I and the additional 61 in Column III with respect to Column II). The F-test for instruments refers to the joint significance of the instruments in each case.

The F-test for the time-trends refers to the joint significance of the specific time trends parameters (15 in Column I, the additional 15 in Column II with respect to Column I and the additional 61 in Column III with respect to Column III). The results for these tests show that these specific time trends are jointly significant. This suggests that not including these separated time trends produces biased estimators for the impact of education on adult health.

The effect of years of compulsory education on years of schooling is positive in the three specifications and statistically significant in the model in Column III, our preferred specification. One additional year of compulsory education raises the level of education in 0.099 years, about 1.2 months. The reason for the discrepancy between columns I and II on the one hand, and column III on the other is that in the former we are wrongly attributing to the reforms an effect that comes simply from a differential time trend. This can also be seen in the dramatic change in the F statistic for the joint validity of the instruments, which goes from around 35 in columns I-II to about 5 in Column III.

Our first stage results also show evidence in favor of our choice of instruments. The instruments are partially correlated with the endogenous regressor, even if the value of the F-Statistic in column III is low (p-value = 0.0004). This is because in this model we have removed much of the variability wrongly associated with the instrument (the interaction between years of compulsory schooling and the dummy for parental education) in the two previous specifications. We also find that reforms have heterogeneous effects on educational attainment. In general, we see that it is women and individuals from low-educated families who benefit more from educational reforms. This is very much in line with the recent literature, as discussed above (see Brunello *et al.*, 2013).

Table 5 reports the results corresponding to the main equation (1). The three models coincide with the ones estimated in Table 4. The F-test results confirm again that failing to control for separate time trends will cause biased estimates of the effect of schooling on adult health.

In general, we have a positive and significant effect of education on health. However, the results in the last specification are quite different from the first two ones. In columns I and II, the effect of education on health is low, but precisely estimated. In Column III our estimate is less precisely estimated, but much larger. The point estimate of the effect of education on health in our preferred specification (Column III) is large. One additional year of schooling is associated with an increase of about seven percentage points in the probability of reporting

Table 5: Effect of education on self-reported health

	(I)	(II)	(III)
Overall	0.0240***	0.0237***	0.0681***
	(0.0057)	(0.0056)	(0.0240)
Time Trends			
Country-specific	yes	yes	yes
Country-gender-specific	no	yes	yes
Country-gender-family background-specific	no	no	yes
F-Statistic	129.38	129.93	189.37
p-value	<0.0001	<0.0001	0.0004
Observations	76,396	76,396	76,396

Note: Robust standard errors (in parentheses) are clustered by country and birth cohort. *** p<0.01, ** p<0.05, * p<0.1. All regressions include as controls the variables *noncitizen*, *father_only*, *CS2011*, *poor_past*, *non_educated_family*, and *gender* together with all interaction terms among the last three ones. The F-test for time-trends refers to the joint significance of specific time trends parameters (15 in Column I, the additional 15 in Column II with respect to Column I and the additional 61 in Column III with respect to Column III).

good health.[19] This is about 11.4% of the mean value of our health measure. However, we want to stress that the 95% confidence interval is [0.021, 0.115] which is so wide that it includes the point estimates of columns I-II. Note that the effect we have obtained has a LATE interpretation in that it measures the effect that has education only on those individuals who were affected by the instruments.

5.1 The role of initial economic conditions

In Table 5, we have found a strong effect of education on health for individuals affected by the reforms. Now we want to study if that effect depends on pre-existing economic conditions. As we have argued in Section 3, we use the variable *poor_past* as a proxy of these conditions. If education plays a remedial role, it may help to overcome a disadvantaged background. If this is the case, education should have a stronger impact on those individuals raised in a family with poor economic conditions. If, in contrast, these conditions and education are complements in the production of adult health, we should observe that the effect of education is stronger on those individuals raised in a well-off family. Since there is no ex-ante clear theoretical prediction, it remains an empirical question worth to study (see Brunello *et al.*, 2017, or Cunha and Heckman, 2007, for recent evidence on the heterogeneous impact of education on earnings for individuals with different family background).

To check this we have estimated again the model in Column III of Table 5, separately by family economic conditions. Table 6 presents these results.

Our main finding is that education only has an effect on health for those individuals whose pre-existing conditions as proxied by *poor_past* were good enough. The size of the effect is slightly higher than the one found in Table 5, although again with a large range of variability. Interestingly, we do not find an effect for those individuals for which *poor_past* is one. We interpret this evidence in line with the idea that education cannot compensate bad initial conditions.

This result differs very much from the one obtained in an OLS regression. When we run separate OLS regressions, we find a larger effect for individuals with a poor family economic background, as also suggested in Figure 2. The OLS estimates, which are well

[19] This effect is very similar in size to the estimated impact of education on being in good health found by Silles (2009) for the UK.

Table 6: Heterogeneous effects on health according to initial conditions

Main equation: Effect of education on self-reported health				
	IV		**OLS**	
	Non-Poor	**Poor**	**Non-Poor**	**Poor**
	0.0923***	-0.0033	0.0105***	0.0120***
	(0.0257)	(0.0377)	(0.0005)	(0.0006)
First stage: Effect of compulsory education on years of schooling				
Overall	0.0763	0.1095*		
	(0.0540)	(0.0573)		
Decomposition by gender				
Male	-0.0271	0.1058		
	(0.0615)	(0.0717)		
Female	0.1684**	0.1130*		
	(0.0664)	(0.0667)		
Difference test p-value	0.0052	0.926		
Decomposition by parental education				
Educated family	-0.0439	-0.0042		
	(0.0766)	(0.1559)		
Non educated family	0.1312**	0.1280**		
	(0.0562)	(0.0549)		
Difference test p-value	0.0148	0.3796		
Decomposition by gender and parental education				
Male and educated family	-0.1183	0.1287		
	(0.1014)	(0.2053)		
Male and non-educated family	0.0151	0.1023		
	(0.0595)	(0.0703)		
Female and educated family	0.0235	-0.1132		
	(0.0998)	(0.1996)		
Female and non-educated family	0.2340***	0.1517**		
	(0.0722)	(0.0644)		
F-Statistic	4.67	1.75		
p-value	0.0012	0.1408		
Observations	48,816	27,580	48,816	27,580

Note: Robust standard errors (in parentheses) are clustered by country and birth cohort. *** p<0.01, ** p<0.05, * p<0.1.

known to represent an upward biased measure of the average treatment for the whole population, are small. On the one hand, it is not surprising that the IV estimates for the non-poor are higher than the corresponding OLS estimates, since they reflect the educational return for the particular groups affected by the reforms. On the other hand, not even those individuals from a poor family background and potentially affected by the reform, seem to have any benefit in adult health.

As previously mentioned, our first-stage results suggest the group mostly affected by reforms is women raised in families with low parental education. These are individuals at the margin, meaning that without the reform they would have probably dropped out earlier from school. One explanation is that this type of families with low education gives little value to their children's education, particularly for girls (see Piopiunik, 2014). We want to point out that our instruments are reasonably valid when *poor_past* is zero (p-value = 0.0012), but seem to be much weaker when *poor_past* is one (p-value = 0.1408).[20] This leads us to an alternative interpretation for the null effect of education on health for individuals raised in poor families, which is that reforms may not even succeed in raising their education levels. In that sense, it is not that education does not have a positive effect on health for this group, but simply that we cannot identify this effect since they do not benefit from the reforms.

5.2 Disaggregating by gender

An interesting aspect of our empirical model is the inclusion of separated country-family education background-specific time trends for men and women. This is similar to estimate separate models for men and women, but assuming that the remaining coefficients do not differ by gender. We gain because of the larger sample size, which allows us to estimate more precisely the coefficients of the model. This is very important for two reasons. First, as it is clear in Figure 1, education time trends for men and women are very different. In fact, there are bigger differences between men and women than between individuals of different countries. Second, if we estimate a common gender trend for each country, similar to the specification in Column I in Tables 4 and 5, we may be attributing to educational reforms an effect that it is simply due to a positive time trend.

Table 7 below shows the results corresponding to three models. In the first column, we present the marginal effects by *gender* corresponding to a model in which there are two endogenous regressors: *years_educ* and an interaction term between *years_educ* and *gender*. In this model, there are three variables (*CS2011*, *father_only*, *noncitizen*) that are not

[20] This p-value corresponds to the joint test for the four instruments. However, some of these instruments are relevant. In particular, the reforms seem to have a significant impact on women from non-educated families.

interacted with the gender dummy. In the second column, we estimate a model similar to the previous one in which we also interact *CS2011* with the *gender* dummy, since this is the only of these three interactions that is significant. In the third column, we show estimates from separate regressions by *gender*. We use here in all cases the same set of instruments as described in Equation (2) and Table 4.

Interestingly, we find that the effect of education on women from non-poor families is similar in magnitude for the three specifications. On the contrary, also for the non-poor, the effect of education for men is not robust to the particular specification we use. When we allow for more flexible models, that effect vanishes. Thus, we do not find here a LATE effect for men.[21] Finally, consistent with results in Table 5, education does not have any positive impact on adult health among individuals, both males and females, from poor families.

Our result for the larger impact of schooling on self-reported health for women than men is in line with the findings by Brunello *et al* (2016). A possible explanation, also suggested by these authors, might be that women in our sample are less educated than men (average years of schooling are 12.69 for men and 12.13 for women) and that marginal returns to education are decreasing.[22]

6. Robustness checks

We now perform some robustness checks of our main specification. We first run a placebo test. Second, we add more regressors to study the possible channels through which our result works. Third, we consider different number of year-of-birth cohorts in the treatment and control groups. Finally, we check that our results are robust to removing each country one at a time from the estimation.

[21] Similar to the result regarding individuals from poor backgrounds, this does not mean that education does not have an impact on adult health for men in general, but just that it does not have an effect among those men affect by the reforms.

[22] On the contrary, Mazzona (2014) finds effects of schooling on self-reported health only for men. Their differing results might be due to the specific sample of countries used in each of them: Mazzona (2014) uses France, Germany, Italy, Netherlands, Poland and Sweden whereas Brunello *et al* (2016) use Austria, Czech Republic, Denmark, England, France, Italy and the Netherlands. In addition, Mazzona's explanation for his finding relies on the assumption that the mediating role for the impact of education on health is the labor market status, mechanism for which we do not find evidence here.

Table 7: Effects of education on health by initial economic conditions and gender

	1	**2**	**3**
		Non-Poor	
Male	0.1195**	0.0501	0.0506
	(0.0524)	(0.0359)	(0.0362)
Female	0.0972***	0.0835*	0.0834*
	(0.0264)	(0.0493)	(0.0494)
Observations	48,816	48,816	Male = 23,009 Female = 25,807
		Poor	
Male	-0.0370	-0.0180	-0.0368
	(0.0384)	(0.0405)	(0.0383)
Female	0.0398	0.0068	0.0398
	(0.0521)	(0.0377)	(0.0522)
Observations	27,580	27,580	Male = 13,103 Female = 14,477

Notes: The first column shows estimates from a model in which there are two endogenous regressors, *years_educ* and the interaction term between *years_educ* and *gender*. We show the marginal effects by *gender*. The second column shows estimates from a model similar to the previous one but where we add an interaction term between *CS2011* and the *gender* dummy. In the third column, we present estimates from separate regressions by *gender*. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

6.1 Placebo test

The idea of this test is to check whether the effect of education on adult health still remains under circumstances (time periods and countries), where it should be not. In Section 5, we have seen that educational reforms have a positive effect on the schooling levels of individuals. Now, if we artificially change the dates of reforms, we should not observe an effect on schooling levels. To check this, we move back in time eight years the date of reforms. This guarantees there is no overlap between the treatment group in our main specification and the treatment group in the placebo test. Moving backwards in time ensures that the date of the artificial reform is not close to the date of some other more recent educational reform not considered in the analysis above, as it is the case for Czech Republic, Slovakia or Spain (see Appendix, Section B for details). Consider the case of Denmark in which the first cohort potentially affected was 1957. The treatment group comprises all cohorts born in 1957-1963. In the placebo test, every cohort born in 1949-1955 compounds the treatment group.

Table 8 shows the results of the placebo analysis corresponding to the model in Column III (Table 4). We estimate the model by considering three different set of countries. In the first column we consider all countries in our sample. However, for some of them now we lack observations in the control group. In the second column, we exclude seven countries for which we lack complete data for early cohorts.[23] Finally, in the third column, we move forward in time the date of reforms eight years for these seven countries. We find that now these artificial reforms do not affect schooling levels.[24]

6.2 Channels

Previous works have tried to study the potential channels through which education may affect health. For instance, Brunello *et al.* (2013) find that education has a positive effect on health behaviors (smoking, drinking, exercising, etc.), and this may account for about 23-45% of the effect. Mazzona (2014) suggests that the positive effect of education on health for men can be partially due to occupational choices. In the survey, we have information on

[23] These are Austria, Czech Republic, France, Hungary, Malta, Portugal, Slovak Republic

[24] We have also estimated the models in columns I and II (Table 4), using the same set of countries as in the last specification in Table 8 (available upon request). Interestingly, in models I and II where we do not estimate a separate time trend according to family education, reforms seem to have a negative impact on educational levels for some groups. In particular, we find that reforms affect negatively the education levels of individuals raised in educated families. This effect disappears when we estimate separate trends according to family education. In our view, this gives more support to our model choice.

Table 8: Placebo test

First stage: Effect of compulsory education on years of schooling

	I	II	III
Overall	-0.0199	-0.0170	-0.0347
	(0.0650)	(0.0650)	(0.0463)
Decomposition by gender			
Male	-0.0159	-0.0118	-0.0583
	(0.0800)	(0.0802)	(0.0570)
Female	-0.0236	-0.0219	-0.0136
	(0.0732)	(0.0728)	(0.0522)
Decomposition by family background			
Educated family	-0.2577	-0.2561	-0.0582
	(0.1562)	(0.1562)	(0.0868)
Non-educated family	0.0395	0.0393	-0.0264
	(0.0746)	(0.0747)	(0.0508)
Decomposition by gender and family background			
Male and educated family	-0.3167	-0.3124	-0.1610
	(0.1963)	(0.1965)	(0.1126)
Male and non-educated family	0.0600	0.0598	-0.0224
	(0.0950)	(0.0951)	(0.0693)
Female and educated family	-0.2037	-0.2035	0.0327
	(0.2006)	(0.2006)	(0.1159)
Female and non-educated family	0.0210	0.0205	-0.0300
	(0.0766)	(0.0766)	(0.0555)
F-Statistic	0.8	0.79	0.65
p-value	0.5241	0.5355	0.6306
Observations	39,337	34,585	63,700

Notes: Robust standard errors (in parentheses) are clustered by country and birth cohort. *** p<0.01, ** p<0.05, * p<0.1. The F-test refers to the joint significance of the instruments in each case.

activity and occupation. We first construct a dummy variable, called *active*, that takes the value one if the individual is either currently working or unemployed, and it is equal to zero otherwise (retired, disabled, doing housework). The second dummy variable, called *wcollar*, takes value one if the occupational code (ISCO-88 Classification) is less than 7, which corresponds roughly to non-manual jobs.

A possible approach consists of adding these two variables as regressors in our model to see if they attenuate the effect of education on self-reported health. However, both of them are problematic. First, activity status conditional on age can be directly affected by health. Individuals with bad health conditions are more likely to retire early or being disabled, raising a problem of reverse causality. Second, the occupational dummy is also potentially endogenous. We could solve these problems by using instrumental variables, but we lack valid instruments for them in the survey. The survey contains information on activity and occupational status of parents, which could be considered as candidates for instruments. However, it is difficult to argue that they satisfy the exclusion restriction since they may have a direct impact on adult health outcomes. For instance, the fact that the mother was active in the labor market could imply that she had less time to care after her children, and this may have long-term effects of health. Nevertheless, we explore this possible mechanism and show the results in Table 9. The first column corresponds to the model in Column III (Table 5), our preferred specification, but only for the non-poor individuals and adding *wcollar* together with its interaction with the *gender* dummy. In Column II, we also add *active* and its interaction with the *gender* dummy.

In the first two rows we show the marginal effects of years of schooling on adult health by *gender*. Rows 3 and 4 present the marginal effect of the individual occupation status (as measured by the *wcollar* dummy), by *gender*. Finally, rows 5 and 6 shows the marginal effect of the labor market status (as measured by the *active* dummy), by *gender*.

We see that the marginal effects of education on adult health are similar to those in Table 7. In Column II, we find that, for men, the effect of education on health disappears, which is along the lines of our results of Table 7 (columns I-II). The effect of the occupational dummy is of a similar magnitude to the values in the first column for both sexes, although now it is only significant for women. Interestingly, the occupational dummy has a negative effect for both men and women. In the literature, it has been suggested that this could be due

Table 9: Exploring channels: impact of education, occupation and activity status on health

VARIABLES	1	2
Years of education		
Male	0.1503**	0.1206
	(0.0744)	(0.0797)
Female	0.1291***	0.1444***
	(0.0418)	(0.0472)
White collar		
Male	-0.4029*	-0.3227
	(0.2437)	(0.2541)
Female	-0.3328**	-0.3662***
	(0.1409)	(0.1392)
Active		
Male		0.1697**
		(0.0773)
Female		-0.0695
		(0.0721)
Observations	48,262	48,225

Notes: The model estimated in the first column is the same as in Column III of Table 5, but adding *white collar* and its interaction with the gender dummy as an additional regressors. The model estimated in the second column is the same as in the first one, but we add both *white collar* and *active* and their interactions with the gender dummy as regressors. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to white-collar jobs being associated with more sedentary tasks that could be detrimental for health (see Patel *et al.*, 2010). Finally, we also find a positive effect of being active, but only for men. This is probably related to our discussion above on reverse causality.

To sum up, we find that the effect of education on health does not change when we include information on activity and occupation. Possible explanations for this could be that there is a direct effect of education through increased knowledge and skills, and also that the mechanism works through changes in health behavior as suggested by Brunello *et al.* (2013). Unfortunately, we lack information on this in EU-SILC.

6.3 Treated and control groups size

We check whether individuals' education has a similar impact on health status regardless of the number of cohorts included in the treatment and control samples. In particular, we first reduce the number of year-of-birth cohorts from 7 to 5. We refer to this sample as "Window 5". The idea is that by reducing the number of cohorts it is easier to assume that treated and control individuals share similar characteristics. We then increase the number of year-of birth cohorts from 7 to 9 and refer to this sample as "Window 9". The idea is just to increase the sample size. In Table 10, we estimate the model in Column III of Table 5 (IV only) for these two samples.

As can be seen in Table 10, our results in our main specification with a window size of 7 years are essentially unchanged.

6.4 Removing countries one at a time

We finally check if it is one specific reform in some country that drives our results. To see this, we estimate again the model in Column III of Table 3, excluding one country each time (only by IV). The results are shown in Table 11.

Dropping each country one by one, we get results very close to those in Table 6. For the non-poor we get estimates in the interval [0.0708, 0.1035], very much in line with the value of 0.0923 that we obtained with all the countries. The smallest value (0.0708)

Table 10: Treated and control group size

	Window=5		Window=9	
	Non poor	Poor	Non poor	Poor
Overall effect	0.1023***	-0.0440	0.0812***	-0.0016
	(0.0367)	(0.0531)	(0.0241)	(0.0346)
Observations	34,830	19,777	61,583	34,350

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 11: Effect of education on self-reported health: excluding countries one by one

Country excluded	Non Poor	Poor	Country excluded	Non Poor	Poor
AT	0.0920***	-0.0041	IS	0.0885***	-0.0029
	(0.0257)	(0.0385)		(0.0249)	(0.0365)
CZ	0.0915***	-0.0049	IT	0.0955***	0.0123
	(0.0251)	(0.0356)		(0.0368)	(0.0290)
DK	0.0945***	-0.0126	MT	0.0909***	0.0029
	(0.0259)	(0.0351)		(0.0259)	(0.0370)
EL	0.0867***	0.0125	NL	0.0936***	-0.0086
	(0.0247)	(0.0406)		(0.0288)	(0.0398)
ES	0.0708**	-0.0158	PL	0.0811***	-0.0325
	(0.0288)	(0.0341)		(0.0245)	(0.0466)
FR	0.1035***	0.0047	PT	0.0890***	-0.0010
	(0.0255)	(0.0366)		(0.0259)	(0.0380)
HU	0.0980***	-0.0005	SK	0.0975***	-0.0010
	(0.0250)	(0.0454)		(0.0268)	(0.0417)
IE	0.0879***	-0.0083	UK	0.0783***	0.0025
	(0.0244)	(0.0382)		(0.0272)	(0.0445)

Notes: The set of regressors is the same as in model in Column III in Table 3. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

corresponds to the case in which we drop Spain and it is statistically significant at the 5% level. All other estimates are significant, at least at the 1% level. We get the highest estimate (0.1035) when we drop France. The estimates we get for the poor lie in the interval [-0.0325, 0.0125], similar to the value we get with all the sixteen countries (-0.0033). None of them is statistically different from zero.

7. Conclusions

This paper provides new evidence on the causal effect of schooling on self-reported adult health, with special attention to the possible existence of heterogeneous effects. Our identification strategy exploits exogenous variation from schooling reforms in 16 European countries. We use data from the 2005 and 2011 cross sections of EU-SILC that contains rich information on education and health and retrospective information on family background. This allows us to explore heterogeneity both in the effect of schools reforms on educational attainment and, more importantly, in the causal effect of education on health. When checking if some groups are more affected than others, we focus on individuals' characteristics like gender and family educational and economic background (during childhood); the latter is a well-known determinant of initial health conditions (and thus, adult health status).

We show that proper control for country-gender-family background-specific time trends is crucial to avoid obtaining a biased estimation of the causal effect of interest. As opposed to the use of a common trend, our flexible specification captures any underlying differential trends that might be driving both education and health for different subgroups in a given country.

We find that reforms affect positively schooling levels, but only for those individuals from low-educated families. As far as our main effect of interest, our estimation results suggest that schooling has a strong positive impact on self-reported health: one additional year of schooling raises the probability of reporting good health by about seven percentage points. However, when we split our sample according to family economic background, we find that the effect concentrates on those individuals whose families enjoyed a sufficiently good economic position. While this can be explained to some extent because the instruments are weak for those raised in poor families, we also suggest two alternative explanations for this result. On the one hand, it seems that education cannot play a remedial role to compensate for the negative effect that bad conditions in the early stages of life can have on adult health. On the other hand, it is worth noting that we identify the effect of an exogenous variation in education that occurs in the adolescent years, when it may be too late to have a significant impact on individuals with a poor family background.

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APPENDIX

A. List of variables used

Years of education (*years_educ*): We construct this variable using the year when the highest level of education was attained (*pe030*), birth year (*pb140*) and school entry age in each country. First, we calculate *pe030-pb140-entry_age*. Second, we drop all individuals for which this number is either negative or above 30. Third, we constaint the variable to be within a particular interval, according to the highest level of education attained. For those with ISCED level 1 we restrict the variable to be in the interval [0, 12]. This means that for those individuals for which the variable is higher than 12 we recode it to take the value 12. For those with ISCED levels 2, 3, 4, and 5 we restrict the variable to be in the intervals [6, 14], [9, 17], [12, 25], and [14, 27], respectively.

Good health: A dummy variable that takes value 1 if the individual reports to have either good or very good health (*ph010* is either 1 or 2). Individuals are asked, "How is your health in general?" Possible answers are: very good, good, fair, bad, and very bad.

Poor family (*poor_past*): Individuals are asked how frequent financial problems in the household were when they were young teenagers (age 14). In the 2005 cross section, it is a categorical variable taking five possible values: 1 (most of the time), 2 (often), 3 (occasionally), 4 (rarely), and 5 (never). In the 2011 cross section there are six possible answers: 1 (very bad), 2 (bad), 3 (moderately bad), 4 (moderately good), 5 (good), and 6 (very good). We summarize the information of these questions by constructing a binary variable that takes value 1 when the corresponding variable is either 1 or 2 in the 2005 cross section and when it is 1, 2, or 3 in the 2011 cross section.[25] We lose some observations from the 2005 wave since four countries do not report this variable in that wave (Austria, France, Greece, and Portugal).

Parental education (non_educated_family): A dummy variable that is one if neither the father nor the mother attained a medium education level (upper secondary and post-secondary non-tertiary education).

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[25] The way in which we code this variable guarantees that we have similar frequencies of the variable *poor_past* in the two cross sections.

Non citizen: A dummy variable that takes value 1 if the individual is not a citizen of the country where he/she lives (when *pb220a* is different from LOC).

Lived with father only (*father_only*): A dummy variable that takes value 1 if the individual lived with young with the father only (when *pt010* = 2).

Compulsory education (*years_comp*): the number of years of education that each individual is required to attend.

Active worker (*active*): We use *pl031*, a categorical value that describes which describes self-defined current economic status. Possible values are 1 (full-time employee), 2 (part-time employee), 3 (full-time self-employed), 4 (part-time self-employed), 5 (unemployed), 6 (student, training), 7 (retirement), 8 (disabled), 9 (military service), 10 (domestic tasks), 11 (other inactive). The dummy *active* takes value one if *pl031* is less than 6.

Non-manual occupation (*wcollar*): We use the categorical value pl050, which describes the occupation according to the ISCO-88 Classification. The dummy *wcollar* takes value one if *pl050* is less than 7.

B. Summary of reforms used

Austria: A reform of compulsory education was passed in 1962, raising the school leaving age from 14 to 15. The number of years of compulsory education was increased from 8 to 9. The law came into effect on September 1 in 1966 (Fort *et al.*, 2016). Since the cut-off date for school entry was mostly September 1st, the first pupils potentially affected by the reform are those born in September-December 1951. Those who turn 14 before September 1st 1966 could not be affected. As Gathmann *et al.* (2015) we code those born in 1952 as the first cohort affected by this reform.

Czech Republic and Slovakia: Several educational reforms were implemented in former Czechoslovakia after the Second World War. Garrouste (2010) reports reforms in 1948, 1953, 1960, 1979, and 1990. Those of 1948, 1960 and 1990 increased the length of compulsory education from 8 years to 9, while those of 1953 and 1979 reduced it from 9 years to 8. We use the reform of 1960. School leaving age was increased from 14 to 15 years. The first cohort potentially affected by this reform are those born in 1946, since they turn 14 in 1960.

Denmark: The literature reports two reforms in Denmark in the second part of the 20th century, one in 1958 and another one in 1971 (Brunello *et al.*, 2009; Murtin and Variengo, 2011; Garrouste, 2010). We use the second one that extended compulsory schooling from 7 to 9 years. The first cohort potentially affected by this reform should be those born in 1957, since they turn 14 in 1971.

France: Compulsory schooling was increased in 1967 from 8 to 10 years. School leaving age raises from 14 to 16 years (Albouy and Lecquien, 2009; Gathmann *et al.*, 2015, Brunello *et al.*, 2009; Borgonovi *et al.*, 2010). The first cohort potentially affected are those born in 1953.

Greece: In 1976 Greece raised years of compulsory education from 6 to 9 (Law 309/1976). School leaving age was raised from 12 to 15 years (Murtin and Viarengo, 2011; Garrouste, 2010). The first cohort potentially affected are those born in 1964.

Hungary: According to Borgonovi *et al.* (2010), Hungary increased the length of compulsory education from 8 to 10 years in 1961, raising minimum school leaving age from 14 to 16. The first cohort potentially affected is the cohort of 1947 since they turn 14 in 1961. See also Mocan and Pogorelova (2014).

Iceland: In 1974, compulsory schooling age changed from 7-15 years to 7-16 years (Birgisdóttir, 2013). That means an increase from 8 to 9 years of compulsory education. The first cohort potentially affected corresponds to those born in 1960. Cohorts affected by this reform are those born in 1960 and later.

Ireland: The reform of 1972 increased school leaving age from 14 to 15 (Murtin and Viarengo, 2011; Mocan and Pogorelova, 2014). Cohorts born in 1958 and later were affected.

Italy: A reform that made junior high school compulsory was passed at the end of 1962 and implemented in 1963. Years of compulsory education were raised from 5 to 8 and school leaving age increased from 11 to 14 (Fort, 2006). Those born in 1952 are potentially affected by this reform since they turn 11 in 1963.

Malta: The Maltese government reformed education in the scholastic year 1972-73, introducing comprehensive secondary education (Zammit-Marmarà, 2001). The number of years of compulsory education was increased from 8 to 10. The first cohort affected should be those born in 1958 since they turn 14 in 1972.

The Netherlands: Several reforms were passed in the 20th century. Most authors use the reform of 1975 that increased minimum school leaving age from 15 to 16 (Brunello *et al.*, 2009; Gathmann *et al.*, 2015; Fort, 2006). All students born after August 1, 1959 should complete 10 years of education. All cohorts born in 1960 and after should be affected.

Poland: On July 15th 1961, the Polish Parliament passed a reform of the educational system raising the minimum age of graduation from 14 to 15. The reform was implemented gradually from 1962 to 1966.[26] Cohorts fully affected by this reform are those born in 1952 (they turn 14 in 1966).

Portugal: In 1964, compulsory schooling was increased from 12 to 14 years, establishing 6 years of compulsory schooling. According to Brunello *et al.* (2013) and Vieira (1999), the first cohort affected was the cohort of 1956.

Spain: Several authors (Gathmann *et al.*, 2015; Borgonovi *et al.*, 2010; Brunello *et al.*, 2009; Fort, 2006) have proposed to use two education reforms, one in 1970 and one in 1990. The reform of 1970 increased minimum school leaving age from 12 to 14 (years of compulsory education from 6 to 8). Here the first cohort affected should be the cohort of 1957. The reform of 1990 increased minimum school leaving age from 14 to 16 (years of compulsory education from 8 to 10). Now the first cohort affected should be the cohort of 1976. We choose only the first reform since the second one is too recent.

United Kingdom: In March 1972, minimum school leaving age was increased from 15 to 16, starting in September 1, 1972. Since school entry age was 5, the number of years of compulsory education raised from 10 years to 11. All individuals born September 1957 or later were affected by this reform (Fort, 2006; Gathmann *et al.*, 2015). We take all cohorts born 1958 or later as affected by the reform.

[26] "Education in the Polish People's Republic", Wikipedia.

C. Other health measures

EU-SILC has two other questions in which individuals provide information on health. In one of them individuals answer the question (*ph020*), “Do you have any longstanding illness or health problem?” Possible answers are yes or no. In the other, they answer the question (*ph030*) “For at least the last 6 months, to what extent have you been limited because of a health problem in activities people usually do?” There are three possible answers: “Severely limited”, “limited but not severely” and “not limited at all”. We construct two dummy variables called “*no chronic*” (=1 when the individual reports not having a longstanding illness or health problem) and “*not limited*” (=1 when the individual reports not being limited).

Table C.1: Marginal effects on “Non chronic” and “Not limited”

Non chronic					
w=5		w=7		w=9	
Non_poor	Poor	Non_poor	Poor	Non_poor	Poor
0.0011	-0.1170	-0.0013	-0.1005*	-0.0024	-0.0840**
(0.0189)	(0.0810)	(0.0148)	(0.0535)	(0.0145)	(0.0415)
34,813	19,758	48,789	27,557	61,549	34,316
Not Limited					
w=5		w=7		w=9	
Non_poor	Poor	Non_poor	Poor	Non_poor	Poor
0.0034	-0.1299	-0.0014	-0.1227*	-0.0043	-0.1009*
(0.0257)	(0.0805)	(0.0202)	(0.0653)	(0.0189)	(0.0521)
34,798	19,754	48,769	27,553	61,524	34,309

Notes: In the upper part of the table, the dependent variable is *no_chronic*. In the bottom part, it is *not_limited*. The set of regressors is the same as in model in Column III in Table 3. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

We separately estimate our central model for non-poor and poor individuals, using *no chronic* and *not limited* as dependent variables, respectively. We do this for three different sizes for the treated and control groups (five, seven and nine years). We show the corresponding marginal effects in Table C.1.

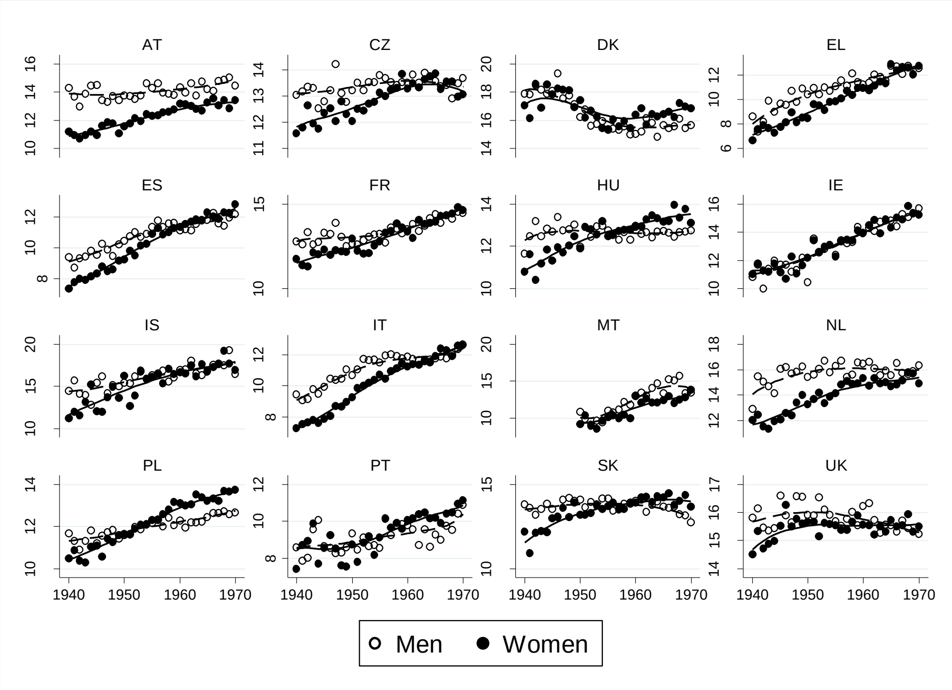
We find that in almost all cases, the effect of education on these two measures of health is negative. Moreover, this negative effect seems to be concentrated on individuals from poor families. Even though these effects are marginally significant, we think this is a surprising finding that contrasts very much with our findings on self-reported health. We could think of two possible explanations for this discrepancy:

1. In the case of *no chronic* and *not limited* conditions, it may happen that individuals that are more educated are more conscious of their conditions, and report them more often. There is some evidence that those who report incorrectly have lower income (see Johnston *et al.*, 2009). If these measures of health suffer from reporting error, and this reporting error is systematically related to the level of education (and, therefore, income) we should expect a negative bias in the estimated effect on these measures of health. Moreover, if this argument is true, we should expect that this effect is stronger when we focus on individuals from poor family background, which is what we observe in Table C.1. If low educated people have a different concept of what health means, they may report that their health level is better than what actually is. This would bias the result.

2. If the receipt of welfare payments is conditional on health status, individuals may have an incentive to report their health or extent of disability to be worse than it really is (see Kapteyn *et al.*, 2007 among others). Mackenbach *et al.* (1996) find more underreporting of self-reported chronic conditions among less educated people. One reason for this could be that less educated people do not receive payments or are less aware of the existence of these benefits.

D. Supplementary material

Figure A1: Average years of education by gender, 1940-1970, disaggregated by country.



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