COMPULSORY SCHOOLING, EDUCATION AND MENTAL HEALTH: NEW EVIDENCE FROM SHARELIFE

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CEMFI Working Paper No. 1304

April 2013

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Abstract

In this paper we provide new evidence on the causal effect of education on adult depression and cognition. Using SHARE data, we use schooling reforms in several European countries as instruments for educational attainment. We find that an extra year of education has a large and significant protective effect on mental health: the probability of suffering depression decreases by 6.5 percent. We find a large and significant protective effect on cognition as measured by word recall. We also explore whether heterogeneity and selection play a part in the large discrepancy between OLS and IV (LATE) estimates of the effect of education on depression and cognition. Using the data available in SHARELIFE on early life conditions of the respondents such as the individuals' socioeconomic status, health, and performance at school, we identify subgroups particularly affected by the reforms and with high marginal health returns to education.

JEL Codes: I1, I2, C3.
Keywords: Health-SES gradient, education reforms, instrumental variables treatment effects, SHARELIFE.

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1 Introduction

Education is the strongest contributor to the so-called "health – socioeconomic status (SES)" gradient. A positive association between education and multiple dimensions of adult health, even after controlling for other measures of socioeconomic status, has been documented in many countries and datasets (see Cutler and Lleras-Muney (2006) for a survey of the education gradient).[1] However, drawing causal inferences from these correlations is problematic because they may be driven by unobserved pre-schooling factors determining both how many years of schooling an individual obtains and her health as an adult. Examples of such common factors are genetic endowments, childhood environment, childhood health and cognitive ability, and to the extent that they are not controlled for one would expect OLS coefficients to overstate the true causal impact of education on health. A substantial body of recent research has tried to disentangle the causal component of the correlation and to understand the mechanisms driving the causal link (see Cutler and Lleras-Muney (2006) for an example of the latter and a discussion).

Some of the most compelling evidence of a causal link between education and adult health has come from studies which exploit the "quasi-experiments" provided by educational reforms which extended the age of compulsory schooling. School reforms were first used to study the wage returns to schooling, e.g. Pischke and von Wachter (2008); Brunello, Fort and Weber (2009). Several studies have used compulsory schooling laws to measure the causal effect of education on different dimensions of adult health in single countries: US: Lleras-Muney (2005), Glymour et al. (2008); UK: Oreopoulos (2006), Silles (2009), Clark and Royer (2009); France: Albouy and Lequien (2009); Germany: Kemptner et al. (2011); Denmark: Arendt (2005). The health outcomes included mortality, self-rated health, long-term illness, BMI, inflammatory markers and memory, and the results on the magnitude and statistical significance of the protective effect of education on health were mixed. Recent research has used this identification strategy on data from multiple countries: Fonseca and Zheng (2011) look at multiple outcomes using data from the Survey of Health, Ageing and Retirement in Europe (SHARE) and the English Longitudinal Study (ELSA); Brunello, Fort, Schneeweis and Winter-Ebmer (2011) ask if the causal effect of education on risk behaviors (e.g. drink, smoke) can explain the effect of education on self-rated health using SHARE data; and Brunello et al. (2013) look into the effect of education on BMI and the gender difference in this role using data from the European Community Household Panel (ECHP). A recurrent finding in this literature is that IV estimates using educational reforms yield estimates of the protective impact of education on health which are less precisely estimated but an order of magnitude larger than OLS coefficients. This is puzzling given the positive bias one would have expected in OLS estimates.

In this paper we make two contributions to this literature. First, we use data from the

[1] Smith (2004) finds that once we control for baseline health, the "protective" effect of education for multiple dimensions of adult health remains whereas that of other measures of SES does not. That is, education predicts the onset of disease for healthy individuals but income and wealth do not conditional on education.

Survey of Health, Ageing and Retirement in Europe (SHARE) to obtain new evidence on the effect of education on cognition and depression. Depression is one of the most important and common disorders affecting mental health. In fact, major depression is forecast by 2020 to become the second most burdensome health condition world-wide. Longitudinal population-based studies also suggest that the incidence and persistence of depression are both high, even accounting for a high mortality for those affected (Dewey and Prince (2005)). In spite of its prevalence, there is no compelling causal evidence on the potential protective effect of education on depression in the literature and, to the best of our knowledge, ours is the first IV-based evidence on the protective effect of schooling. Our results for cognition complement those that have been obtained using HRS data (Glymour et al. (2008)).

Second, we investigate the discrepancy between OLS and IV estimates of the effect of education on adult health. It has been noted in discussions of this finding that the effect of additional schooling on adult health is likely to vary in the population and IV estimates obtain the local average treatment effect which is the average effect of an extra year of education on health for those "compliers" or "marginal" individuals whose schooling was actually changed by the reforms. If heterogeneity and selection play a large part in the discrepancy between IV and OLS estimates, the variation in the effect of schooling in the population has to be large, the number of "compliers" with reforms has to be relatively small and the reforms must have increased the schooling of individuals disproportionately at the top of the distribution of effects. We obtain new evidence which is informative about this issue. Our dataset merges measures of education and adult health available for individuals aged 50 and over with baseline interviews in wave 1 of SHARE with retrospective life histories collected in wave 3 (SHARELIFE) which describes childhood environment, health and cognition preceding completion of compulsory schooling.[2] These variables (CHVARS hereafter) were not observed in earlier studies exploiting educational reforms. Because childhood environment and circumstances are determinants of both schooling choices and adult outcomes, we expect CHVARS to be relevant sources of heterogeneity in the impact of the reforms as well as in the effect of education on adult health.

Our main findings based on IV estimates show that an extra year of education has a large and significant protective effect on mental health in adulthood: the probability of suffering depression decreases by 6.5 percent. We also find a large and significant protective effect on cognition as measured by word recall. Besides, these effects are

[2]This paper uses data from SHARE wave 1 and 2 release 2.5.0, as of May 24th 2011 or SHARELIFE release 1, as of November 24th 2010. The SHARE data collection has been primarily funded by the European Commission through the 5th Framework Programme (project QLK6-CT-2001-00360 in the thematic programme Quality of Life), through the 6th Framework Programme (projects SHARE-I3, RII-CT-2006-062193, COMPARE, CIT5- CT-2005-028857, and SHARELIFE, CIT4-CT-2006-028812) and through the 7th Framework Programme (SHARE-PREP, N° 211909, SHARE-LEAP, N° 227822 and SHARE M4, N° 261982). Additional funding from the U.S. National Institute on Aging (U01 AG09740-13S2, P01 AG005842, P01 AG08291, P30 AG12815, R21 AG025169, Y1-AG-4553-01, IAG BSR06-11 and OGHA 04-064) and the German Ministry of Education and Research as well as from various national sources is gratefully acknowledged (see www.share-project.org for a full list of funding institutions).

much larger than those obtained from Probit/OLS estimates, which seem to understate the impact of education on health. Regarding heterogeneity and selection, we find that lower socio-economic status, poorer health and poorer performance at school at age 10 contribute to fewer years of schooling but no significant differences are found in terms of returns to education among subgroups differently affected by the reforms.

2 Data

The data we use comes from the first and third waves of SHARE, which were collected in 2004, and 2008, respectively. The target population of this survey is individuals aged 50+ and their partners for several countries in Europe ranging from Scandinavia (Denmark and Sweden) through Central Europe (Austria, France, Germany, Switzerland, Belgium, and the Netherlands) to the Mediterranean (Spain, Italy and Greece). The first wave of SHARE provides data on a very extensive range of measures of the respondents' health status and socioeconomic status. The third wave of SHARE (SHARELIFE) provides retrospective data on respondents' life histories on important dimensions such as childhood, fertility, partnership formation and dissolution, employment and health. We use the SHARELIFE data because it provides information on childhood environment, health and cognition before the completion of compulsory schooling, which seem a plausible source of heterogeneity in the impacts of interest.

Given our identification strategy, we draw a sample of individuals aged 50 and over with baseline interviews in waves 1 and available data from SHARELIFE who were affected by reforms in the years of compulsary education. Therefore, we include only countries where we observe reforms which increased the number of years of compulsory schooling for cohorts represented in the SHARE population. In particular, we consider changes in the compulsory schooling laws in the period 1950-1969 that were implemented in Austria, Germany (at the lander level), Sweden, the Netherlands, Italy, France and Denmark as documented in Brunello, Fort and Weber (2009), Murtin and Viarengo (2007) and Arendt (2005).[3] For each country and year of birth, years of compulsory education (*ycomp* hereafter) are calculated by substracting the mandatory enrollment age from the minimum drop out age. For example, countries that required enrollment at age six years and permitted drop out at 14 years had compulsory education of 8. For each individual we computed the variable years of compulsory education as the value that applied at her drop out year in her country of residence. This last criterium requires us to assume that individuals studied in countries (or landers for Germany) where they live at the moment of being interviewed. To minimize the potential effects of confounders correlated to secular trends of education and health improvements, we restrict our sample to include cohorts in a window of 7 years before and after the first cohort affected by a particular reform in a

[3] We do not include Greece in our analysis since in this country as documented in Murtin and Viarengo (2008) two opposite reforms were implemented in a very short period of time. In 1964, compulsory years of schooling were increase in 3 years whereas in 1967 they were reduce in 3 years.

specific country.[4] More specifically, let $b(c,t)$ be the birth year of the first cohort affected by a reform in country c. We include in our "country c" subsample all individuals from all birth cohorts in a "window" of 7 years at either side of the "pivot" cohort $b(c,t)$. That is, all individuals from country c born in year $[b(c,t)-7, b(c,t)-6, ..., b(c,t), ..., b(c,t)+6]$ as long as they were interviewed in one of the first wave of SHARE and wave 3. Table 1 contains the information used relative to the reforms considered in the analysis (country, source of information, year of the implementation of the reform, pivotal cohort or first cohort affected and the extreme cohorts in the 7 year-window).

The main variables of interest are those that measure the individuals' education attainment, cognition and the incidence of depression. Regarding education, SHARE collects in wave 1 information on the highest level of education attained by the individual and it derives imputed ISCED-97 categories and number of years of education correspondingly ($yedu$ hereafter). The incidence of depression is identified from the variable "$eurodcat$" which is obtained from variable "$eurod$", the score that the respondent got in the EURO-D 12-item scale. This is implemented through a list of 12 questions which identify the presence of several conditions in the last month previous to the interview such as depression, pessimism, suicidality, guilt, sleep, loss of interest, irritability, loss of appetite, fatigue, loss of concentration, enjoyment, and tearfulness. As noted by Dewey and Prince (2005), this scale has been validated in an earlier cross-European study of depression prevalence, EURODEP (Prince et al. (1999a), Prince et al. (1999b)). Similar to them, we defined clinically significant depression as a EURO-D score greater than 3. This cutpoint had been validated in the EURODEP study, across the continent, against a variety of clinically relevant indicators. Those scoring above this level would be likely to be diagnosed as suffering from a depressive disorder, for which therapeutic intervention would be indicated. For cognition, we use a measure of respondents' memory skills ($memory$) based on a test of verbal learning and recall where respondents are required to learn and recall immediately and again later a list of 10 common words. Immediate and delayed word recall scores are summed and standardised by substracting the mean and dividing by the standard deviation.

In addition to our interest in the estimation of the causal effect of education on health, we will explore the role of individuals' childhood characteristics as a potential source of heterogeneity in the relationship between $eurodcat$, $eurod$, $memory$, $yedu$ and $ycomp$. In particular, we consider variables which describe childhood conditions (when 10 years old) in three different domains: measures of socioeconomic status such as whether the dwelling had two or fewer rooms ($room2l$), it had less than 25 books ($book25l$), it was in a rural area ($rural$), the breadwinner did not have an urban and qualified job ($bwnusj$), and the number of features-facilities in the dwelling ($numaccofea$); measures of cognition such as not being a particularly good student in language class or maths ($badlanguage$ and $badmath$, respectively); and measures of health such as self-reported bad health status

[4] Brunello et al. (2009) used "window-based" samples and compulsory schooling reforms to study the effects of education on the distribution of earnings. In particular, they find that additional education reduces conditional wage inequality.

through age 15 (*badchsph*), mental problems (*chmental*) and parents drank excessively, or smoked (*padict*) or had mental problems (*pmental*).

Table 2 reports the means of the variables used in the analysis for the resulting sample of 3708 individuals with information in all the variables used in the analysis.

3 The Empirical Model

In order to study the causal impact of years of schooling on depression we adopt a two-equation model of health and years of schooling. The main (second-stage) equation is:

$$H_i = \gamma_1 + \gamma_2 E_i + \gamma_{31}' X_{1i} + \gamma_{32}' X_{2i} + \upsilon_i \tag{3a}$$

were H_i is the standardized word recall measure in the model of cognition. In the study of depression we consider both a linear model where H_i is the *eurod* score and a probit model for the depression indicator *eurodcat*. Variable E_i represents the years of education (*yedu*) of individual i, and X_i is a vector of individual i's characteristics, determined *before* schooling, that affect adult mental health. Variables in subvector X_{1i} include gender and all cohort-country effects needed to make *ycomp* a valid instrument. Vector X_{2i} collects childhood information - the variables we have labelled CHVARS. Finally, υ_i is the error term of health equation.

The main coefficient of interest is γ_2, the causal effect of an extra year of schooling on the cognition and depression outcomes measured in the SHARE interview. Note that equation (3a) need not be interpreted as a health production function. We are agnostic about the specific mechanisms linking additional schooling with a reduced probability of depression when person i is observed by us many years later. However, γ_2 still measures the total causal impact of an extra year of schooling operating through all possible channels, net of all other pre-schooling determinants of adult health.[5]

OLS or ML estimation of parameter γ_2 in equation (3a) would likely be biased for two reasons: (1) the existance of measurement error in education; and, more importantly, (2) omitted factors which result in correlation between υ_i and E_i. The latter source of bias is usually associated with ommitted childhood circumstances, ability, discount rates, etc. A comparison of OLS estimates obtained with and without controls X_{2i} may be informative about the magnitude of this bias.

To deal with the potential endogeneity of E_i in the health equation and obtain consistent estimates of γ_2, we use the exogenous variation provided by the compulsory schooling reforms through the following first-stage equation:

$$E_i = \delta_1 + \delta_2 Z_i + \delta_{31} X_{1i} + \delta_{32} X_{2i} + \delta_{33} X_{2i} Z_i + \varepsilon_i \tag{3b}$$

[5] Cutler and Lleras-Muney (2010) explore possible explanations between education and health behaviours. In particular, they find that factors such as income, health insurance, family background, knowledge and cognitive ability explain most of the gradient. Brunello et al. (2011) ask if the causal effect of education on risk behaviors (e.g. drink, smoke) can explain the effect of education on self-rated health.

where Z_i represents the years of compulsory schooling for individual i which is assumed to be a source of exogenous variation in the years of education, uncorrelated with υ_i and correlated with depression only through its effect on educational attainment. The rest of covariates included in the vector of individual characteristics X_i are assumed to be uncorrelated to υ_i but υ_i and ε_i may be correlated and, in the probit model, are assumed to have a joint normal distribution. In our baseline specification, we include as covariates only gender, country dummies, and quadratic country-specific cohort trends (interactions between country of residence and a quadratic specification for age). In our preferred specification we add the CHVAR controls and we interact some of them with compulsory schooling to obtain additional instruments.

4 Results

Later in this section we present the results on the OLS and IV estimation of the effect of years of schooling on adult depression and cognition. We confirm the recurrent puzzle found in the literature that the IV estimate based on compulsory schooling reforms is less precise but much larger than the OLS estimate. We explore the role that the heterogeneity in this effect across individuals with different environments and conditions during their chilhood might have in the explanation of the gap. In particular, as Glymour et al. (2008) state, differences in estimates could be caused by larger effects of education in lifecourse periods when the reforms increase schooling or larger returns to education for individuals affected by the reforms. If heterogeneity and selection play a role, the instrumental variable estimator can be interpreted as the local average treatment effect associated to the instrument. In this particular case, it would measure the marginal returns to education for the subpopulation of individuals whose schooling decisions are changed by the reform (the so-called compliers). One might argue that children who drop from school typically have test scores below average or come from low socio-economic households and that the effects of an extra year of education for them might be substantially larger than the average. Besides, these might be also the individuals who are more likely to be affected by the reforms since they might face a higher cost of education. We provide evidence on the plausibility of these hypothesis by using retrospective data drawn from SHARELIFE on individuals' socioeconomic status, cognitive ability and health status during childhood.

4.1 The effect of compulsory schooling laws on schooling

Before showing the IV results, we focus on the first-stage equation to estimate the impact of the reforms on education. Besides, under the assumption of valid instruments, this estimate will give us an indication of the mass of compliers, that is, the prevalence of individuals whose decision of dropping out from school after compulsory schooling is changed by the reform. We estimate equation (3b) under different specifications. Results are presented in Table 3. Column 1 contains the results for the baseline specification where we just include as covariates individual characteristics such as gender (a female dummy), dummies for country of residence, age (cohort) and quadratic country-specific cohort trends (interactions between country dummies and a quadratic specification for age). The coefficient for the variable years of compulsory schooling suggests that an extra year of compulsory education results in approximately 0.15 years of schooling on average. However, this effect is not significant. This result indicates that on average the explanatory power of the reforms might be weak because for example they were not binding for most of the population. However, one might argue that the costs/returns of education might vary significantly across the population as a function of individuals' circumtances. This variability might translate into heterogenity with respect to how individuals' schooling decisions are affected by the reforms. In order to explore this hypothesis, we consider the vector of CHVARS as defined in the data section. In particular, we add measures of the individuals' socioeconomic status ($room2l$, $book25l$, $rural$, $bwnusj$, $numaccofea$), cognitive function ($badlanguage$, $badmath$) and health status ($badchsph$, $chmental$, $padict$, $pmental$) as early life conditions that might influence individuals' compliance types. First, we analyse the role of those variables as determinants of schooling choices and we include them in levels in the vector of covariates. Results in Column 2 show that the signs of the coefficient estimates are as expected for most of the variables. Measures of socioeconomic status and cognition are highly significant and suggest that individuals with a higher socioeconomic status and better cognitive ability reach higher levels of education. Regarding health status, none of the variables are found to be significant. Finally, the coefficient of the years of compulsory education is around 0.12 but non-significant, suggesting again that education attainment was not much affected by compulsory schooling reforms on average. Next, to test whether the reforms affected the schooling decisions of particular groups of individuals, we add interactions of the instrument $ycomp$ with the vector of CHVARS. In particular, we show the interaction with the variables $room2l$, $badhealth$, and $badmath$ since these are the variables that turn out to be relevant as determinants of the mass of compliers.[6] As results in Column 3 show, we can see that these interactions matter. In particular, the coefficient is positive, large and significant for low socio-economic status ($room2l$), and large, negative and significant for poor chilhood health ($badhealth$). This means that individuals with a lower socio-economic status and better health status as measured by these CHVARS are more responsive to the reforms.[7] With respect to cognition, we can see that individuals with a bad performance in math ($badmath$ equals to one) are less affected than those with a good performance but this last effect is in the margin of significance. The bottom panel of Table 3 shows the marginal effects for specific groups. Whereas the average effect is around 0.10 and not significant, the increase in schooling for children of low socio-economic status is 0.27 and significant

[6] Results from the extended regression where we include the interactions with the complete vector of CHVARS is available upon request.

[7] Brunello et al. (2012) also use data from SHARELIFE and consider living in a rural area at age 10 as a determinant for being exposed to a higher extent to reforms that increase the years of compulsory education. However, they do not include in their estimations any additional information on early life conditions. Similarly, we included living in a rural area in our vector of CHVARS but it turned out to be not significant as long as the variable $room2l$ (household with 2 or less rooms) was included in the regressions.

at 5 percent level. In addition, the increase in schooling for those with low socio-economic status in normal health is 0.30 and also significant at 5 percent. These are important effects that show that selection plays a role in the estimation of the effect of education on health based on the compulsory schooling reforms as instruments.

Regarding the relevance of the instruments used in this analysis, we can see in Column 3 that the variable *ycomp* and its interactions with *room2l*, *badhealth*,and *badmath* are jointly significant at the 1 percent level and the F-statistic is greater than 5. This shows that the instruments are strong and good predictors of the number of years of schooling.

4.2 The effect of schooling on adult depression and cognition

Tables 4 and 5 contain our main findings, which can be described as follows:

We first estimate equation (3a) by OLS for memory and as a Probit model for depression given the discrete nature of our measure. Columns 1 and 2 of Table 4 show the marginal effects of interest based on the corresponding estimates with and without CHVARS. Column 1 shows the results for our baseline specification, which includes just country dummies, gender and country-specific cohort trends as covariates. The average effect of an extra year of education is significant and small at around -0.7 percent for the probability of depression and positive and at 0.07 standard deviations for memory. If we add the vector of CHVARS to covariates (column 2), we see that marginal effects remain the same for depression but are mitigated for memory, although the (slightly) dampened coefficients remain significant. This latter result is somewhat surprising for the case of depression given that we expected that the omission of significant variables affecting health in the same direction as education would bias OLS estimates upwards.[8]

Column 4 presents the estimates for our preferred specification based on the instruments given by the variable *ycomp* plus its interactions with the CHVARS that have been shown to be most relevant as determinants of the mass of compliers (*room2l*, *badhealth* and *badmath*). In particular, the average effect for the whole population suggests that an extra year of education decreases the probability of depression by 6.5 percent. This is significant at the 5 percent level. This is a very large impact given that the prevalence of depression in the sample is 22 percent aproximately. A significant effect is also found from the IV linear regression estimation using the variable *eurod* (which has been standardised by substracting the mean and dividing by the standard deviation). The IV estimate suggests that an additional year of schooling reduces the EURODEP index score by 0.20 standard deviations. For cognition, we also find a large and significant protective effect which shows that an extra year of education improves memory scores by 0.12 standard deviations. This result is comparable to Glymour et al. (2008), who find an effect of 0.18 standard deviations after controlling for gender, birth year and state birth indicators.

For the sake of comparison, column 3 presents the IV-Probit estimates based on *ycomp* as the only exclusion restriction. They show a somewhat larger average effect for depres-

[8]For all other health outcomes that we have considered such as self-reported bad health, and minor chronic conditions (hypertension, cholesterol, stroke, diabetes and arthritis), the marginal effect of education is mitigated by the addition of the CHVARS, although it remains significant.

sion using the variable *eurodcat* compared to the overidentified IV estimates. However, they are very imprecise for *eurod* and *memory*.

From the comparison between columns 2 and 4, we confirm the recurrent puzzle found in the literature that the IV estimate based on compulsory schooling reforms is less precise but much larger than the OLS estimate for every outcome considered in the analysis. As discussed above, one of the potential explanations for this gap might come from the fact that individuals whose education attainment is increased by the increase in the number of years of compulsory schooling might have higher marginal returns to education. If this was the case, the IV estimate would measure a larger effect given that it identifies a "Local Average Treatment Effect", that is the effect of interest for the group of individuals whose decisions are affected by the instrument. In the previous section, we have shown evidence which suggests that compulsory schooling reforms affect significantly individuals with low socio-economic status and in good health during chilhood. In line with this, we have computed our estimates for different subsamples which might exhibit larger returns to education. Table 5 presents the results for subsamples defined by particular values of the selected CHVARS. Three remarks can be made regarding these results. First, we obtain large and statistically significant effects in spite of the fact that samples are much smaller. This gives support to the hypothesis that the average effect in the full sample is driven mainly by the existence of larger returns to education for individuals affected by the reforms who are mostly drawn fom subsamples with particular childhood environment and circumstances. Second, whereas for the probability of depression low socio-economic status ("*room2l*") seems to be a salient feature linked to larger returns to education, in the case of memory poor health or bad performance in math are more important. Third, point estimates show some variation across subsamples, but standard errors are too large for us to identify statistically significant differences across particular subpopulations.[9]

5 Conclusions

In this paper we provide new evidence on the causal effect of education on adult depression and cognition. We use schooling reforms in several European countries as instruments for educational attainment and specific measures of depression and cognition from the Survey of Health, Ageing and Retirement in Europe (SHARE).

Our results based on IV estimates show that an extra year of education has a large and significant protective effect on mental health in adulthood: the probability of suffering depression decreases by 6.5 percent. We also find a large and significant protective effect on cognition as measured by word recall. Furthermore, these effects are much larger than those obtained from Probit/OLS estimates. We use unique information on childhood environment available in SHARELIFE to investigate the hypothesis that heterogeneity and selection play a role in the gap between IV and OLS estimates. We show that the

[9] Additionally, we estimated overidentified regressions including interactions of the years of education with each of the three CHVARS *room2l*, *badhealth* and *badmath* for all of our outcomes of interest but we did not find these interactions statistically significant in any of them.

impact of changes in the age of compulsory schooling was much larger for particular subsamples characterized by childhood environment covariates, and that IV estimates carried out on those subsamples were similar to the full sample estimates. Even though the estimated marginal effects of education are somewhat larger for particular subgroups, we have been unable to obtain statistically significant evidence of heterogenous effects because our samples are not large enough and standard errors do not allow for such sharp inferences.

Our results are of policy interest given that they provide evidence on the protective role of education and early life policy interventions for mental health later in adulthood.

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TABLES

Table 1. Reforms in minimum school drop out age laws

Country	Source	Year of reform	FCA[a]	Left window	Right window	YCE[b] before	YCE[b] after
Austria	Brunello et al. (2009)	1962	1947	1940	1953	8	9
Germany1[c]	Brunello et al. (2009)	1956	1941	1934	1947	8	9
Germany2[c]	Brunello et al. (2009)	1949	1934	1927	1940	8	9
Germany3[c]	Brunello et al. (2009)	1962	1947	1940	1953	8	9
Germany4[c]	Brunello et al. (2009)	1958	1943	1936	1949	8	9
Germany5[c]	Brunello et al. (2009)	1967	1953	1946	1959	8	9
Germany6[c]	Brunello et al. (2009)	1967	1953	1946	1959	8	9
Germany7[c]	Brunello et al. (2009)	1967	1953	1946	1959	8	9
Germany8[c]	Brunello et al. (2009)	1967	1953	1946	1959	8	9
Germany9[c]	Brunello et al. (2009)	1969	1955	1948	1961	8	9
Germany10[c]	Brunello et al. (2009)	1964	1949	1942	1955	8	9
Sweden	Brunello et al. (2009)	1962	1950	1943	1956	8	9
Netherlands	Murtin and Viarengo (2007)	1950	1937	1930	1943	6	8
Italy	Murtin and Viarengo (2007)	1963	1949	1942	1955	5	8
France	Brunello et al. (2009)	1967	1953	1946	1959	8	10
Denmark[d]	arendt (2005)	1958	1945	1938	1951	5	7

a. First cohort affected.
b. Years of compulsory education.
c. Germany1: Schleswig-Holstein; Germany2: Hamburg; Germany3: Niedersachsen; Germany4: Bremen; Germany5: Nordrhein-Westphalia; Germany6: Hessen; Germany7: Rheinland-Pfalz; Germany8: Baden-Würtemberg; Germany9: Bayern; Germany10: Saarland.
d. Danish 1958 reform is more complicated than others, but to start with I interpreted it as an increase by two years of compulsory schooling. See Arendt 2005.

Table 2. Descriptive statistics (means)

Eurodcat (%)	21.60
Eurod (standardised)	-0.009
Memory (standardised)	0.017
Years of schooling (*yedu*)	11.06
Years of compulsory schooling (*ycomp*)	7.47
Age	58.13
Female (%)	54.77
$\leq$ 2 rooms (room2l) (%)	23.24
Badhealth (%)	9.62
Badmath (%)	64.07
Badlanguage (%)	62.35
<25 books (book25l) (%)	59.25
Breadwinner not qualified (bwnusj) (%)	70.81
# of features in accommodation (numaccofea)	2.57
Parents with addictions (padict) (%)	11.14
Parents with mental health problems (pmental) (%)	2.7
Mental health problems (chmental) (%)	1.32
Living in rural area (rural) (%)	42.37
Austria (%)	9.73
Germany (%)	9.49
Sweden (%)	18.47
Netherlands (%)	15.13
Italy (%)	19.23
Denmark (%)	13.02
France (%)	14.91
Sample Size	3708

Table 3. The effect of the laws on schooling (yedu)

	(1)	(2)	(3)
Years of compulsory schooling (ycomp)	0.145	0.113	0.141
	(0.125)	(0.122)	(0.134)
Female	-0.784***	-0.811***	-0.807***
	(0.120)	(0.120)	(0.121)
Badlanguage		-1.139***	-1.143***
		(0.128)	(0.128)
Badmath		-1.025***	-0.274
		(0.110)	(0.521)
Book25l		-1.441***	-1.447***
		(0.135)	(0.136)
Bwnusj		-1.038***	-1.027***
		(0.124)	(0.124)
Numaccofea		0.417***	0.420***
		(0.042)	(0.043)
Room2l		-0.377***	-2.081***
		(0.130)	(0.582)
Badhealh		-0.084	2.056**
		(0.155)	(0.826)
Padict		-0.330	-0.320
		(0.205)	(0.203)
Pmental		0.595*	0.580
		(0.352)	(0.355)
Chmental		0.055	0.085
		(0.464)	(0.467)
Rural		-0.189*	-0.175
		(0.113)	(0.113)
Room2l*ycomp			0.230***
			(0.078)
Badhealth*ycomp			-0.280***
			(0.104)
Badmath*ycomp			-0.099
			(0.070)
F-test statistic			8.26
Marginal Effect of compulsory schooling on years of education			
Average effect			0.103
			(0.123)
Subgroups:			
Room2l=1			0.272**
			(0.134)
Room2l=1 & Badhealth=1			0.303**
			(0.135)

Note: All regressions include controls for age and country-specific quadratic cohort trends (interactions of age and its square with country dummies). Robust standard errors are clustered by cohort and country and they are reported in parenthesis. The F-test statistic in specification (3) refers to the joint significance of the instruments (the variable ycomp and its interactions with room2l, badhealth and badmath).

Table 4. The effect of schooling (yedu) on the mental health

Average Marginal Effects				
	(1)	(2)	(3)	(4)
	Probit	Probit	IV-Probit	IV-Probit
Eurodcat	-0.0076***	-0.0077***	-0.082***	-0.065**
	(0.0019)	(0.0018)	(0.017)	(0.027)
	OLS	OLS	IV	IV
Eurod	-0.0303***	-0.0294***	-0.6664	-0.1919*
	(0.0045)	(0.0043)	(0.7557)	(0.1152)
	OLS	OLS	IV	IV
Memory	0.0683***	0.0531***	0.2143	0.1183**
	(0.0039)	(0.0047)	(0.2533)	(0.0574)
CHVARS	No	Yes	Yes	Yes
Interactions as instruments	No	No	No	Yes

Note: All regressions include controls for age and country-specific quadratic cohort trends (interactions of age and its square with country dummies). Robust standard errors are clustered by cohort and country and they are reported in parenthesis.

Table 5. The effect of schooling on mental health

Marginal Effects for Subgroups from IV estimation (r>k)			
	Eurodcat (IV-Probit)	Eurod (IV)	Memory (IV)
Room2l=1	-0.0647***	-0.2917**	0.0693
	(0.0243)	(0.1369)	(0.0957)
Badhealth=1	-0.0541*	-0.1379	0.1751**
	(0.0326)	(0.1119)	(0.0912)
Badmath=1	-0.0813***	-0.3221*	0.2499*
	(0.0146)	(0.1913)	(0.1435)
Room2l=1&Badhealth=0	-0.0716***	-0.3699**	0.0432
	(0.0160)	(0.1695)	(0.1456)

Note: All regressions include controls for age and country-specific quadratic cohort trends (interactions of age and its square with country dummies). Robust standard errors are clustered by cohort and country and they are reported in parenthesis.

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