The effect of education on old age health and cognitive abilities - does the instrument matter?*

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Abstract

This paper estimates the causal effects of education on old age health and cognition in eight European countries using data from the Survey of Health, Ageing and Retirement in Europe. The empirical strategy is based on an instrumental variables approach which uses two different instruments: the time and geographical variation in compulsory schooling laws and birth order information, namely whether the respondent was the first born. The results using compulsory schooling as instrument show a positive effect of education on old age memory and health only for men. In addition, the affected population shows a higher occupational level suggesting an effect on health via employment. When I use birth order as instrument, I find positive and significant results also for women. I show that such results might be explained by the fact that the two instruments affect different populations with different labor force attachment in the case of women. In particular, the changes in compulsory schooling laws affected only women at the lower end of the education distribution who show a very low labor force attachment.

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1 Introduction

Economists have traditionally considered schooling as a financial investment aimed at increasing individual's lifetime earnings. More recently, attention has been paid to the broader effects of schooling on health and social behaviors. In particular, a growing body of literature has been able to identify the causal effect of education, by exploiting plausibly exogenous variations in schooling – like changes in compulsory schooling (CS) – to overcome the well-known problem of endogeneity of educational choices (see for a review Oreopoulos and Salvanes, 2011 and Lochner, 2011).

Even though the evidence on positive returns to education using CS as instrument is well-established (e.g. Acemoglu and Angrist, 2001; Oreopoulos, 2006; Brunello *et al.* 2009), the evidence on health is mixed. Exploiting changes in CS in more than 30 US states, Lleras-Muney (2005) estimates a 6-percentage points reduction in ten-year mortality rates[1]. However, Mazunder (2008) shows that the estimated effects are not robust to the inclusion of state-specific cohort trends. According to the large literature surveyed by Lochner (2011), education seems to have smaller (or no) effects on health, self-reported health (SRH) and physical activity in Europe than in the United States. More recently, using data from several European countries, Brunello et al. (2011) find positive but rather small effects on SRH[2]. Also in the UK the evidence is mixed, despite the attention has been paid to the 1947 CS reform – an extraordinary reform that affected half of the population of the 14 year-olds. On the one hand there is robust evidence of small or no effects on many health outcomes (Jürges *et al.* 2009, Clark and Royer 2010). On the other hand that reform had a strong positive effect on men's cognitive abilities (Banks and Mazzonna 2012). Banks and Mazzonna (2012) argue that the positive effect only on men's old age cognitive functioning does not come via health or mortality but via higher employment or more cognitively demanding occupations that lead also to the observed positive effect on earnings (Oreopoulos 2006). As a consequence, women did not benefit from it because of their lower labor force attachment.

More generally, CS affects only a very small portion of the population at the lower end of the education distribution. This implies that the first stage (i.e. the effect of CS on education) might not be sufficiently powerful to find a significant effect in the second stage, in particular with survey

[1] She estimates a reduction in the probability of dying between successive decennial censuses.

[2] They use both an IV strategy based on CS and an identification strategy based on aggregation, differencing and selection on observables (ADS). In both cases they found that education decreases the probability of reporting poor health by roughly 3 percentage points.

data given the poor finite sample property of the IV estimator. Not less importantly, since the affected individuals – the so-called compliers (Imbens and Angrist 1994) – are those at the lower end of the education distribution, in the case of heterogeneous effects we can recover only a local parameter (LATE) instead of the average effect on the whole population (ATE). Finally, another possible explanation for the small causal evidence of an education effect on health concerns the trade-off between quality and quantity of education, since one "forced" year of additional schooling might have very small effects on adult health.

This paper wants to shed light on this relationship by estimating the causal effect of one extra year of education on old age health and cognition in Europe using data from the Survey of Health, Ageing and Retirement in Europe (SHARE). As in Brunello *et al.* (2009) – who estimates returns to schooling – the first empirical strategy exploits the time and geographical exogenous variation in CS laws across 8 European countries. The results show positive and significant effects of education on men's old age memory and SRH but not on that of women. As discussed before, if the effect of this additional year of schooling on old age health and cognition is (at least in part) via labor market participation, this apparently puzzling result might be explained by the low women's labor force participation in particular considering very old cohorts as in SHARE (see also Section 1.1 for a literature review). Consistently with this hypothesis, I show that compulsory school reforms increased also the occupational level[3] of the affected cohorts, an effect that may explain both the positive return on education in Brunello *et al.* (2009) and the positive spillovers on men's old age health and memory estimated in this paper.

To confirm this intuition and verify the robustness of the results, I use birth order information – namely whether the respondent is the first-born – as additional instrument. It is immediately evident the two instruments show a somewhat trade-off between relevance and exogeneity. On the one hand, CS reforms are a plausibly exogenous instrument but they might show some problems of relevance. On the other hand, birth order information is a strong predictor of educational outcomes and affects a larger proportion of the population (Black *et al.* 2005), but endogeneity concerns may arise about the mechanisms through which it affects educational choices and adult outcomes. As far as I know, only Black *et al.* (2005) use birth order information as instrument for education in the wage equation[4], mainly because of lack of proper data and probably because of

[3] As in Case *et al.* (2005) the occupations are ranked according to their assumed skill level. See Section 2 for further details.

[4] However, they do not show the results in any table of their paper.

potential concerns about the endogeneity of this variable as excluded instrument in a 2SLS setting. More generally, this variable has never been used as instrument in the case of health. In order to estimate credible and arguably exogenous birth order effects on education this paper follows most of the stringent data requirements in Blake (1989) and Black *et al.* (2005) such as controlling for sibling size, cohort effects and family background (e.g. breadwinner's occupation and characteristics of the childhood accommodation, see Section 2.1 for further details). Moreover, the evidence from J-tests of overidentifying restrictions and other indirect exogeneity tests – implemented to verify whether birth order is correlated with those unobservables characteristics that may drive both educational choices and old age outcomes – never casts doubts on the exogeneity of this second instrument. In particular I show that birth order does not affect observable characteristics, such as childhood health, adult height and psychological traits, that might be correlated with those (genetic) unobservable factors that should strongly drive both education and adult health.

The results using this second instrument are very similar for men but not for women where I find now positive and significant effects on SRH. I think that the reasonable concerns about the potential endogeneity of the first-born variable as instrument cannot explain why the results differ only for women. As before the occupational channel and the local effect of CS as instrument might explain such a puzzle. I show, in fact, that women at the end of the education distribution, those mostly affected by changes in CS laws, show a very low labor force attachment compared to those affected by the first-born instrument.

The remainder of this paper is organized as follows. In the rest of this section I briefly review the literature on education and health and on the relationship between birth order and education. Section 2 describes the data used for my analysis; Section 3 describes the empirical strategy along with the main identification issues. Section 4 presents the results and, finally, Section 5 offers some conclusions.

1.1 How does education affect health?

Health and mortality gaps by education are large and have been growing for decades (e.g. Cutler et al. 2010). The economic literature has identified many reasons why education may improve health and reduce mortality. Taking the Grossman's model (1972) as a reference, we can identify several potential mechanisms through which education might affect health. Firstly, education may directly increase health production by raising the marginal productivity of health inputs

or behaviors (productive efficiency) and by enhancing individuals' ability to acquire and process health information (allocative efficiency). To this end, Cutler and Lleras-Muney (2007) stress the importance of cognitive abilities as mediating factor in this relationship in particular through the fundamental role played by health behaviors. They show in fact, that over 20% of the education gradient in health behaviors is associated with general cognitive abilities. The effect of education on health might also work through the labor market. Education generally increases earnings, which allows individual to have command over resources, such as costly insurance or medical treatments and the possibility of living in healthy housing conditions. Another possibility is through the trade-off between higher lifetime earnings for shorter (and sicker) life as stressed by Case and Deaton (2005). The main intuition is that low educated individual can be paid-off to accept risky occupations. At the same time education might allow individuals to have more cognitively demanding occupations which in turns might have positive effects on cognitive abilities and health later in life.

Another interesting open issue – largely discussed in this paper – is that of gender differences in the education-health association. The presence of a steeper education differential among men in adult mortality is well established in the literature (e.g. Zajacova and Hummer, 2009), but the reason for this difference remains an open question, despite its importance for understanding the pathways through which education affects health. Schumacher and Vilpert (2011) argue that since older cohort women were less involved in the labor market, their material resources are less dependent on their level of education than on their husbands. For the same reason, low educated women are preserved by being employed in risky occupations or high education women cannot benefit from more cognitively demanding occupations.

1.2 Why birth order effects?

The relationship between birth order and education (and intelligence) has always captured the attention of both psychologists and economists. The most influential theories in both economics and psychology are based on resource dilution. The most popular economic model, the quantity-quality model (e.g. Becker and Lewis 1973), predicts a trade-off between child quantity and quality where quality is proxied by educational outcomes. Additional children in fact may reduce the monetary and non-monetary resources of the parents. This theory can be easily extended to include birth order effects since the first-born may receive a greater time endowment than subsequent children

who have to compete for parental attention (Birsdall 1991). In psychology the dilution model (Zanjonc 1976) arrives at a similar conclusion since families with many children have a relatively inferior intellectual climate due to the lower average age of the household. Biological factors may also matter. Later born might be associated with lower birth weight because mothers are older. On the other hand, life cycle effects may run in the opposite direction because younger parents are usually poorer and less experts and this can favorite later borns. Birth order effects may also arise due to cultural factors. Historically the first (male) born is the most important in many societies (Horton 1988) and more generally since the first born is the first to become economically independent, parents might invest more in him (her) if they need security for old age.

From an empirical point of view, many studies have estimated birth order effects but only few of them follow the rigorous data requirements described in Blake (1989) to avoid the confounding effects of family size, cohort and parental characteristics. Using a data set on the entire Norway population over an extended period of time, Black *et al.* (2005) show the presence of strong and homogenous birth order effects on education and earnings across cohorts, family structures (intact families or divorced) and educational groups (of the mother). These results are particularly important because they exclude the possibilities that these effects are driven by financial constraints. In a following study (Black *et al.* 2011), they control also for birth characteristics excluding the possibility that biological factors are responsible for these effects. Similar evidence against biological explanations can be found in the epidemiologic study of Kristensen and Bjerkedal (2007). Comparing the IQs of first-borns with those of second-borns whose older sibling (early) died, they show that what matter in explaining birth order effects is the social rank in the family and not birth order as such.

Summing up, the empirical evidence shows strong birth order effects but is not able to identify the precise channel through which birth order affects education. Since this paper use birth order information – namely whether the respondent is the first-born – as excluded instrument for education, it is fundamental that birth order has its effects on old age health and cognition only through education and not through other unobservable characteristics which may drive both educational choices and old age outcomes. As already mentioned in the introduction, the work of Black *et al.* (2005) is the only one that (credibly) uses birth order effects as instrument to estimate returns to education once controlled for all the observable potential confounders. Since they obtain an implied return to education of 5% for men and 7% for women – what we would expect the return

to education to be (see also Black *et al.* 2005b) – they argue that "*much of the birth order effect on earnings is likely working through education*". As largely explained in Section 3.2 this paper accounts for most of the potential confounders in the relationship between birth order and education and also implements a battery of tests to indirectly verify the exogeneity of the first-born variable as excluded instrument.

2 Data and Summary Statistics

This paper uses data from waves II and III (2006 and 2008) of the Survey of Health, Ageing and Retirement in Europe (SHARE), a multidisciplinary, cross-national bi-annual household panel survey. The target population consists of individuals aged 50 and over who speak the official language of the respective country and do not live abroad or in an institution, plus their spouses or partners irrespective of age. The topic areas covered in the main questionnaire include individual and household characteristics; physical and mental health, cognitive abilities and functioning, subjective psychological health and wellbeing, social participation and social support; housing, work, pensions, income and assets; and expectations for the future. The common questionnaire and interview mode, the effort devoted to the translation of the questionnaire into the respective national languages, and the standardization of fieldwork procedures and interviewing protocols are the most important design tools adopted to ensure cross-country comparability (Börsch-Supan and Jürges 2005).

The second and third waves cover 13 countries[5], representing different European regions, from Scandinavia (Denmark, Sweden) through Central Europe (Austria, Belgium, France, Germany, the Netherlands, Switzerland) and Mediterranean countries (Greece, Italy, Spain) to Eastern Europe (Poland and Czech Republic). As in Brunello *et al.* (2009), this paper uses data only from 8 countries that experienced a CS law change that affected the SHARE cohorts. Table 1 shows for each country the year of the considered reform, the pivotal cohort and the change in CS. The only differences form the reforms in Brunello *et al.* (2009) are the exclusion of Austria because of the absence of a refreshment sample in the second wave – which makes the sample small and highly selected – and the inclusion of Poland. Additional information is provided in Appendix A.

In addition to the topics covered in the first two waves, the third wave of SHARE, called SHARE-

[5]The countries now covered by SHARE are 22 but only 13 participated in both wave 2 and 3.

LIFE, has been implemented to collect the retrospective histories of the SHARE respondents in order to obtain information about the lives of respondents before the baseline year of the survey (2004). The use of retrospective questionnaires is a way to overcome the lack of large nationally representative cohort studies connecting the earliest years of life to later stages of the life-course. This is especially important, since numerous studies have demonstrated the importance of early life events for later life outcomes (see Currie 2009 for an extensive survey). In order to improve the respondents' recall ability SHARE orders the different interview modules according to what is usually most important for the respondent and thus remembered most accurately. Moreover, the interview is supported with a life grid – a computerized version of the life history interview that serves as a basis for the SHARELIFE interview. Basically, as the respondent answers, the information appears in a calendar for both the respondent and the interviewer to see, so that it is easy for the interviewer to link questions to personal events. For an analysis of the accuracy of the retrospective information in SHARELIFE, I refer to Havari and Mazzonna (2011).

The empirical analysis uses two main samples. In both of them I exclude immigrants and restrict attention to individuals aged 50–80 at the time of the second wave interview.

The first sample – which uses the CS law changes as source of identification – includes all respondents who were born within 10 years before and after the first cohort potentially affected in each country by the change in CS. Since CS laws constrain drop-outs from primary and secondary school and are unlikely to have affected people with very high educational attainment, I exclude the top 5% of the educational distribution, which correspond to people with more than a college degree[6]. In this way I can also increase the power of the instrument which is a relevant issue using this empirical strategy in a cross-country survey. This sample is further restricted when I use information on the individuals' occupation history because of the relevant fraction of women without any labor force attachment. The second sample – which uses birth information as source of identification – restricts the attention to those respondents who participated both in wave 2 and 3 of SHARE and declared to have had siblings. Additional restrictions are imposed to this sample when I jointly use the two instruments.

[6]Such a selection criterion also allows to reduce the noise in the data, since I notice that a non-negligible fraction of the respondents with more than 20 years of schooling report also a lower educational attainment (only high school or high school drop-outs).

2.1 Main Variables

SHARE contains several measures of mental and physical health. As usual in empirical research, self-rated health status (SRH) is used as a measure of the health status. Respondents are asked to rate their general health according to five possible categories (*excellent*, *very good*, *good*, *fair*, *poor*). In order to facilitate the interpretation of the results the variable is recoded as a dummy variable equal to 1 for those that report at least good health. For robustness checks also other health measures, such as chronic conditions and Euro-D scale of depression, have been considered. The SHARE cognitive function module contains measures of cognitive function based on simple tests of memory, verbal fluency and numerical ability. For comparability reasons I focus on the most common test in the literature, namely the memory test (Glymour *et al.* 2008; Banks and Mazzonna 2012).

The test of memory consists of verbal registration and recall of a list of 10 words (butter, arm, letter, queen, ticket, grass, corner, stone, book, stick). The respondent hears the complete list only once and the test is carried out two times, immediately after the encoding phase (immediate recall) and at the end of the cognitive function module (delayed recall). The scores of both tests correspond to the number of words that the respondent recalls. A general measure of memory is constructed by summing up the individual scores in the two tests. The resulting memory variable ranges between 0 and 20. To facilitate comparison, the memory score have been standardized. For robustness check also the numeracy test have been used.

The last outcome variable used in the paper is the individual's main occupation. It is recorded according to the first digit of the ISCO-88 code and, as in Case *et al.* (2009), occupations are divided into three groups that refer to their assumed skill level: 1 "low", 2 "medium" and 3 "high".

The main explanatory variable is education which is measured with the number of years spent in full time education. I also exploit the information in SHARE about birth order and household characteristics when the respondent was 10. In particular, I create a dummy variable equal to one if the respondent was the first-born ("*first*") and another set of dummy variables that controls for the number of siblings. Unfortunately the exact number of siblings is not asked in SHARE, but it is inferred from the number of people in the household when the respondent was 10 and by subtracting all the people in the household who are not siblings (biological or not) from another question of the SHARELIFE questionnaire. However, the fact that this variable can control only

for the number siblings that used to live with the respondent should better capture the effect of resource dilution – including financial or other parental inputs. To this end I also drop a small number of respondents, who declare to have had siblings but reported to not have had siblings in their household when aged 10. Further relevant information used to proxy for family background and childhood conditions at the age of 10 are: number of people per room, main breadwinner's occupation, number of books at home, self-reported childhood health, whether their parents used to smoke or (heavy) drink, whether they lived with their biological parents and a set of dummies to control for the area of residence during the childhood – big city (reference category), suburb, large town, small town and rural area. Finally, I use three variables that should be proxies for adult psychological traits only to implement indirect exogeneity tests of our second instrument, first-born. The first question asks the respondent the level of trust she has in other people in a scale between 0 and 10. The second question asks for the percentage chance of leaving an inheritance of more than 150000 Euro. The last one asks for the religious background in a scale between 1 and 6.

2.2 Descriptive statistics

To examine the effect of law changes on educational attainment I present a set of descriptive figures showing the discontinuity in education and old age outcomes in the proximity of the pivotal cohorts. Each point of each graph represents the sample mean of all cohorts in the different countries which are at the same distance from the pivotal cohorts. Since the data are particularly noisy I present two graphs, which cover 20 or 10 years respectively before and after the pivotal cohorts, in order to better evaluate the presence of discontinuities. Moreover, purely for descriptive purposes, the fitted lines are based on local polynomial fits on the two sides of the discontinuity. The vertical line denotes the pivotal cohorts for the law changes.

Figure 1 shows the effect of the reform on years of education in our sample which is restricted to those with a high school degree or less. The Figure shows a discrete jump of about 0.4 years of education at the cut-off point for both men and women. The estimated jump is similar to those estimated in Brunello et al. (2009) using different data and a larger set of countries.

The second set of descriptive figures shows the discontinuity in the old age outcome of interest. Both Figure 2 and 3 show the presence of an evident discontinuity in the memory score and self-rated health for men but not for women. In particular, in the case of memory the discontinuity in the men's test score at the pivotal cohorts is of about 0.4 more words recalled while for SRH an

increase in the number of people reporting good health is of about 6%.

The last descriptive analysis involves the other instrument used in this paper, the first-born variable. Table 2 reports means comparison between birth order groups – first-borns vs. later borns – for our main outcomes and other observable characteristics. The first set of columns shows these statistics unconditional while in the second set they are reported conditional on sibling size, namely by using the residuals from a regression of sibling size on these variables. The other observable characteristics are adult height, which has been standardized by subtracting the country mean, a set of childhood characteristics – namely childhood health, a dummy for father with a white collar occupation, number of books at home, whether the respondent grew up in a city, whether she grew up with her biological parents and sibling size – and three variables that are meant to proxy for adult psychological traits – the level of trust in the other, the chance of receiving an inheritance in the next 10 years and the level of religiosity.

The third column for each set of columns reports p-values from t-tests on the equality of means. Starting from the unconditional comparison, these tests reject the null of equality of means (at the 1 percent level) for all the dependent variables used in this paper – education, SRH, cognitive abilities and occupational level. For instance, first-borns got on average 0.67 years of schooling more than later borns. However, as evident from the table there is a high negative correlation between first-born and sibling size. As a consequence, it is possible that these differences are partially driven by different family characteristics since a larger family size is usually correlated with poorer socio-economic conditions. This is why also important observable characteristics, such as adult height, number of books at home, being grown in a city and the three proxies for adult psychological traits, are not balanced between the two groups. This means that without controlling for sibling size the exogeneity of the first-born variable is not credible.

In the conditional means comparisons, although the differences in all our dependent variables are still significant, they are definitely smaller than in the unconditional ones. For instance, in the case of education the difference between first and later borns decreases from 0.68 to 0.24 years of education. Such a result suggests that controlling for sibling size is fundamental to not mix up birth order effects with other unobservable family characteristics that are correlated with sibling size. Finally, all the other important observable characteristics are now well balanced between the two groups. The last finding is important because a different result would have cast doubt on the exogeneity of the first-born as excluded instrument, as discussed in Section 3.2. However, this

kind of comparison is purely descriptive, because it does not take account of cohort trends in such variables. Consequently, I will return to this test in the robustness check section, when I estimate full models with control variables.

3 Empirical Strategy

The main target of this paper is to estimate the long-run effect of education on old age outcomes. This is done by applying an instrumental variable approach that uses two different instruments: CS and birth order. Two different empirical specifications are employed to take into account different identification issues that arise as a consequence of the two different instruments. However, in Section 4.2 I will employ a common empirical specification in order to use these two instruments jointly and implement a test of overidentifying restrictions.

3.1 Compulsory schooling as instrument

The first empirical strategy estimates the causal effect of education on old age outcomes by exploiting the geographical and time exogenous variation in schooling generated by changes in CS. As already stated in the introduction, this identification strategy has been first used by Brunello *et al.* (2009) in a cross-country study that estimates the return to education. Following that study, in each country I construct a pre- and post-treatment sample composed of the respondents born within 10 years before and after the pivotal cohorts[7]. Specifically, I estimate the following two equations:

$$Y_{ick} = \beta_0 + \beta_1 E_{ick} + X'_{ick}\beta_2 + \delta_k + \delta_c + f(c,k) + u_{ick} \tag{1}$$

$$E_{ick} = \alpha_0 + \alpha_1 Z_{ck} + X'_{ick}\alpha_2 + \gamma_k + \gamma_c + g(c,k) + \nu_{ick} \tag{2}$$

where Y_{ick} are the old age outcome of interest (health or cognition) and E_{ick} the educational level of individual i, of cohort k, in country c; Z_{ck} is equal to the number of years of CS; the vector X contains other exogenous controls, such as sex and whether the respondent was surveyed in the second wave refreshment sample. The specification controls also for cohort and country fixed effects, δ_k, δ_c, γ_k and γ_c, and for country-specific polynomial trends in birth cohort, $f(c,k)$ and $g(c,k)$. The last set of controls is meant to take into account country-specific trends in health

[7]For robustness checks I will consider also smaller windows around the cutoff point

and education that improved over time and across cohorts (Lochner 2011). For instance, they might capture country specific historical improvements in nutrition and vaccination that might be otherwise correlated with the changes in CS (Matzunder 2008). However, the inclusion of the last set of regressors comes at the cost of an increasing level of collinearity with our instrument, which varies only across cohorts and countries. For these reason I also try alternative specification that substitute the cohort fixed effect, with a 3 years cohort fixed effect or a parametric quadratic control in age.

To verify the sensitivity of the results to the bandwidth selection – within 10 years on the two side of the pivotal cohorts – I report in the appendix the results using smaller bandwidth (7 or 5 years). Finally, to account for sex heterogeneity the model is estimated separated by sex.

3.2 Birth order as instrument

The second estimation strategy uses birth order information – namely whether the respondent was the first-born – as source of identification. As already largely discussed in the introduction, different from the previous source of identification, the validity of the instrument is more questionable. In particular, some concerns may arise about the exogeneity of this variable as excluded instrument if birth order has direct effects on old age outcomes – in addition to those through schooling – or if it is correlated with the same unobservables (e.g. latent ability) that lead to the endogeneity of education.

Following Blake (1989), each regression conditions on sibling size effects because the probability of being the oldest decreases with sibling size. In addition, as in equation (2) I control for cohort effects and country specific trends in birth cohorts and – using information from SHARELIFE – I control also for a large set of controls that proxy for parental and housing characteristics when the respondent was 10 (see Section 2.1). The last set of controls is meant to control for period effects and other parental differences that may arise with parents at different periods of their childbearing. As robustness check, I implement estimates of the first-born effects separated by sibling size (see the discussion in Section 4.3).

Two different samples are used. The first sample includes all respondents to both wave 2 and 3 who declared to have had siblings. The second sample restricts the first sample to those respondents born 10 years before and after the CS reforms in order to use the two instruments jointly.

Finally, two different types of tests are performed to verify the validity of our identification

strategy, namely the exogeneity of the birth order information as excluded instrument. Firstly, I use this first-born instrument jointly with CS and perform a classic J-test of overidentifying restrictions. Assuming the exogeneity of CS, this test might be considered as a test on the exogeneity of the first-born variable.

Secondly, in Section 4.3 I test whether similar individuals in terms of observable characteristics – except for the birth order – have different childhood health, adult height and adult psychological traits. This test is meant to verify whether birth order is somewhat correlated with those unobservables that lead to the classic endogeneity of education problem, such as genetic and biological characteristics which are important determinants of childhood health, adult height and psychological traits and which might drive both education and adult health. The test does not reject the null hypothesis of zero effect of first-born on these variables.

4 Estimation Results

In this section I estimate the effects of one extra year of schooling on old age outcomes using two different instruments. Section 4.1 shows the results using variation within and between countries in CS as main identification strategy. Section 4.2 shows the effect of one extra year of schooling using birth order as instrument. Finally robustness checks are provided in Section 4.3.

4.1 Compulsory Schooling

Table 3 shows the estimated effect of years of CS on years of schooling pooled and separated by sex. To explore the sensitivity of the results I show the estimations on four different specifications within 10 years before and after the pivotal cohorts. As already mentioned in Section 3.1 each regression includes controls for sex, a dummy for the refreshment sample and country dummies. The four specifications differ only in how they account for the historical improvement in health and education over time. The last row for each empirical specification reports the F-test on the excluded instrument - the years of CS. Standard errors are robust to heteroskedasticity. In the first column, which includes a full set of cohort dummies, the estimated effect of one additional year of CS is slightly less than 0.3 years of schooling for both sexes, a result consistent with the effect estimated in Brunello *et al.* (2009). As expected, once in the second column I add country specific linear trends in birth cohort the effect of CS slightly decreases in size but the standard

errors increase because on an increasing level of collinearity between the large set of controls and the the instrument that varies only at country and cohort level[8]. For this reason, the value of the F-test drops in size even below the Staiger and Stock (1997) nominal value of 10. For this reason in the third and the fourth column I try to avoid the collinearity problem by substituting the cohort fixed effects with a quadratic control in age and with 3 years cohort groups fixed effects. The results using the quadratic control in age are in line with the estimates in the first two columns but they still show lack of power, while when I use the 3 years cohort groups control I obtain slightly larger but more precise estimates. All in all, different from Lleras Muney (2005), the results are robust to the inclusion of country specific trends in birth cohorts which implies only an increase in the standard errors, without seriously affecting the point estimates.

The next step is to evaluate the effect of the exogenous increase in education on the old age outcomes of interest as before pooled and by sex. Table 11 reports 2SLS estimates of the effects of education on memory and SRH. Starting from the pooled estimates, the table shows that the extra year of schooling significantly increases the old age memory of the affected cohorts by about 0.2 standard deviations, while for SRH the effect is statistically significant only in the last column with point estimates always around 3-5 percentage points increase in the probability of reporting good health.

Consistently with the graphical evidence, when I allow for sex heterogeneity the table shows that for men point estimates are larger than pooled estimates for both memory and SRH, while for women they are not significantly different from zero. Several considerations arise about men's results. In the case of memory the estimated effect is consistent with the results in Glymour *et al.* (2008) who used a similar estimation strategy exploiting changes in CS across US states, but lower than the half standard deviation estimated in Banks and Mazzonna (2012). Such a difference may be due not only to the different estimation strategy (IV vs. RD) but also to the peculiarity of the 1947 English reform that affected about 50% of the population of 14 years-olds. For SRH, point estimates are between 10 and 14 percentage points. This result is somewhat comparable with some studies reviewed by Lochner (2011).

To sum up, the results stress the importance of taking sex heterogeneity into account since the results from pooled estimates might be misleading in particular for SRH. As already stressed in the introduction and in Section 1.1 if labor market related factors are among the most important

[8]This is verified by computing the variance inflation factor (VIF)

mediating factors in the education gradient in health, the different labor force attachment between men and women might explain part of the sex heterogenety in that gradient.

Consistent with that hypothesis Table 5 shows the effect of the extra year of schooling due to CS on the respondent's last occupational level. The number of observations is smaller than before, in particular for women because of their low labor force attachment[9]. For such reason the power of the instrument strongly decreases, in particular in the second columns, and point estimates are somewhat noisy. However, for both men and women the estimates are always positive and in most cases statistically significant at least at the 10 percent level. Such result is consistent with the hypothesis that CS law changes in Europe affect the individual's occupation with positive spillovers on cognitive abilities and health.

4.2 Birth order

This section estimates the causal effect of education using first-born as additional instrument. Given the results in the previous section I show only the results by sex.

I start by showing in Table 6 the estimated effect of being first-born on years of schooling. The baseline estimates in the first column – which include controls for sex, birth cohorts and country dummies and country specific linear trends in birth cohort – indicate that the first-born effect is positive and significant. Being first-born is associated with .73 more years of schooling for men and more than a half year for women. However, as I include a control for sibling size the coefficient strongly decreases in size by roughly 30% for men and 40% for women. Such a result confirms that not accounting for sibling size upward biases the birth order effects estimates. More generally, the estimated effect of the first-born is comparable with the effect estimated in Black *et al.* (2005)[10]. The third column adds controls for childhood conditions when the respondent was 10 to control for period effects and initial endowment passed from parents to children. However, although these controls are extremely relevant – they dramatically increase the R^2 by over 40% – they do not markedly decrease the size of the birth order coefficients. The sex heterogeneity seems to support the hypothesis of cultural reasons behind the first-born story, according to which parents invest

[9]Despite the reduction in women sample size the number of respondents still is larger than the number of women with a continuous labor force history. In fact, many respondents report their last occupation even if it was for a short period of time.

[10]They control for a full set of birth order dummies and the estimated effects range between 0.3 and 0.5 additional years of education for the first-born

more in the first male born. This hypothesis is also supported by a separated analysis, not reported here, in which I find that for older cohorts of women the effect is close to zero.

In the last two columns, in order to use jointly the two instruments, the sample is restricted to those cohorts born 10 years before and after the pivotal cohorts and I do not include the country specific trends in birth cohort in order to increase the of power to the CS instrument and implement credible overidentifying restrictions tests in the next tables. In particular the third column shows the estimates using only the first-born instrument, while the last one displays the result when using both instruments. The results show that both instruments are positive and statistically significant. More generally, it is worth to stress that in this sample the effect of CS is slightly larger than the effect estimated before, in particular for men. The most reasonable explanation is that the exclusion of only child respondents leads to a sample of respondents from larger and poorer families shifting the education distribution towards the left where the CS reforms had the largest effects.

Table 7 and 8 show the 2SLS estimates of the effect of education on all old age outcomes for men and women. Given the importance of sibling size all estimates already include this control. As before the last two columns restrict the attention on the CS cohorts.

Starting from men, the effect of one extra year of education is positive for all old age outcomes, but in the case of memory the effects are definitely smaller than those estimated in Table 11. However, this is only the consequence of the sample selection since when in the last two columns the effect is estimated in the common sample, point estimates are larger and very similar using one or two instruments. Consequently, the results from the J-tests of overindentifying restrictions never reject at conventional levels the hypothesis that the excluded instruments are correctly excluded from the main equation.

The evidence is somewhat different for women. This difference is particularly remarkable in the case of SRH where estimates are now statistically significant with a large drop in the estimated effect from .14 to .05 when I use both instruments. For this reason in this case the J-test does reject the exogeneity hypothesis. For memory, in the first two columns, point estimates are very similar to those of men, but in the common (restricted) sample the lack of power of the first born instrument does not allow to have the precise estimates.

To sum up, the 2SLS estimates of education differ only for women in the case of SRH. It is hard to believe that the concerns about the endogeneity of the first-born variable as instrument explain why the results differ only for women. More reasonably, if the occupational channel is one

of the more relevant for explaining the educational gradient, the fact that the two instruments affect different populations with different labor force attachment might explain the puzzle. In particular, women affected by CS might show a lower labor force participation than those affected by fist-born instrument.

To examine that hypothesis Table 9 shows results from regressions of the first stage effect of CS and first-born by sex and educational groups (High school drop-outs vs. high school and college graduate). The results clearly show that CS mainly affects respondents at the lower end of the education distribution, while first-born does affect both groups or, as for men, mainly those with higher education attainment. The last result is consistent with the evidence in Black *et al.* (2005) that shows homogeneous birth order effects across several dimensions (see Section 1.2).

At the same time the third row shows the labor force attachment of the four groups. The Table clearly displays that women with low education definitely show a lower labor force attachment, roughly 52%, than women with high education, 80% and men who always have an history of high labor force attachment, roughly 90% . Therefore if labor force participation is a relevant source of heterogeneity in the effect of education, instrumental variable estimates that use CS law changes as instrument can recover only local average treatment effects.

4.3 Robustness checks

As already mentioned in Section 3.2, this section starts by showing the results from an indirect test of exogeneity on first-born to verify whether the birth order is correlated with genetic or biological characteristics that may drive both educational choices and old age outcomes. I perform this test on adult height and self-reported childhood health. Although adult height is not a birth characteristic it is strongly correlated with height at the age of 3 and has been used in the literature as a marker for the genetic endowment and the health environment that an adult experienced in early life (Case and Paxson 2009). Table 10 shows that being first-born does not significantly affect both variables once I control for sibling size. In addition, in the last three columns I test whether first-borns are different in some adult psychological traits employing the proxies used also in Section 2.2. The main idea is to see whether birth order is correlated with some non-cognitive skills that may drive both education and adult health. It is worth noticing that since I use proxies for the adult psychological traits a correlation with birth order does not undoubtedly imply an endogeneity problem because it is still possible that this correlation is due to the indirect effect of education. However, when

I control for sibling size, the results reject any significant relationship and so also indirect effects through education.

To verify the robustness of the result using CS as instrument to the bandwidth selection in the Appendix B I show the results using 5 or 7 years around the pivotal cohort. The results are consistent with those reported so far.

Other robustness checks are performed and the results – available on request – are in accordance with those reported so far. As suggested by Blake (1989), I run separated estimates of the first-born effect by sibling size. In order to verify the robustness of our CS reform instrument, I estimate the same models in Tables 11 by excluding each time one of the countries in this study. In addition I consider smaller bandwidth around the the pivotal cohort (7 and 5 years). Finally, I consider other health and cognitive outcomes in addition to SRH and Memory – such as Numeracy, chronic conditions and the Euro-D depression scale. The results for Numeracy are consistent with Memory despite the effect on men using CS as instrument is smaller in size. For the other health outcomes the point estimates are consistent with those presented so far, but often standard errors in particular with CS as instrument are larger.

5 Conclusions

In this paper I use data from two waves of the Survey of Health, Ageing and Retirement in Europe (SHARE) to estimate the causal effect of education on old age cognitive abilities and health. The estimation strategy involves the use of two different instruments, namely changes in CS laws across 8 different European countries and birth order information, namely a first-born dummy.

The results show that education does causally affect men's old age health and cognitive abilities, a result that has always been controversial in the literature on the broader effects of education. Less evident are the positive spillovers for women, at least when using CS as instrument. If the effect of education on health works also through the labor market, that difference might be explained by the lower women's labor force attachment. To this end, I present evidence of an effect of education also on the occupational level of the affected individuals that is consistent with that hypothesis. From a methodological prospective, that hypothesis is also in accordance with the fact that the two instruments affect different populations with different labor force participation and, as a consequence, they might recover different parameters (LATE vs. ATE).

Let me make a final remark on the use of first-born as instrument. This variable has been used to give power to our identification strategy and robustness to our findings, fully aware of the endogeneity concerns that, however, do not seems to seriously affect the identification strategy of the paper. Even though it is not the main target of the paper, the results from this paper might also help to understand why birth order affects educational outcomes. As in previous literature (Black et al. 2011, Kristensen and Bjerkedal, 2007), I do not neither find evidence of biological explanations – no effects on childhood health and height – nor of financial constraints because of larger effects on the upper end of the education distribution. More likely cultural influences seem to be a reasonable explanation given the larger effect I found for men and for older cohorts.

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Table 1: Compulsory school reforms

Country	Reform implementation	Pivotal cohort	Change in CS
Czech Republic	1948	1934	8 to 9
Denmark*	1958	1944	7 to 9
France	1967	1953	8 to 10
Germany			
– Baden-Württemberg	1967	1953	8 to 9
– Bayern	1969	1955	8 to 9
– Bremen	1958	1943	8 to 9
– Hamburg	1959	1934	8 to 9
– Hessen	1967	1953	8 to 9
– Niedersachsen	1962	1947	8 to 9
– Nordrhein-Westfhalen	1967	1953	8 to 9
– Rheinland-Pfalz	1967	1953	8 to 9
– Saarland	1964	1949	8 to 9
– Schleswig-Holstein	1956	1941	8 to 9
Italy	1963	1949	5 to 8
Netherlands	1942	1936	7 to 8
Poland	1966	1953	7 to 8
Sweden	1962	1950	8 to 9

*The Denmark reform of 1958 did not formally increase the CS but eliminated barriers to access to the 8th and 9th grade for children from villages.

Table 2: Mean comparisons between first-born and higher birth order (the p-value is derived from a t-test on equality of means).

	Unconditional			Conditional on sibling size		
First-born:	No	Yes	$p-$value	No	Yes	$p-$value
Years of education	10.30	10.97	0.00	10.44	10.68	0.00
Memory	8.89	9.21	0.00	8.96	9.06	0.17
SRH(good)	0.63	0.68	0.00	0.63	0.67	0.00
Occupation*	2.19	2.31	0.00	2.21	2.27	0.00
Height	0.16	0.44	**0.05**	0.24	0.28	0.78
Childhood background:						
Child health	2.11	2.09	0.56	2.11	2.09	0.57
White collar father	0.07	0.08	0.30	0.07	0.07	0.88
Number of books	-0.05	0.01	**0.01**	-0.04	-0.03	0.72
Grown in a city	0.12	0.15	**0.00**	0.12	0.14	0.11
Miss school	0.12	0.12	0.79	0.12	0.12	0.60
Biological father	0.91	0.91	0.83	0.91	0.92	0.20
Biological mother	0.97	0.97	0.36	0.97	0.97	0.12
Sibling size	4.09	3.16	**0.00**			
Adult psychological traits:						
Trust in others	5.74	5.85	**0.05**	5.76	5.81	0.33
Chance of inheritance	21.55	24.25	**0.04**	22.54	22.59	0.97
Religious	4.04	3.81	**0.00**	3.97	3.94	0.41
N	5864	2740		5864	2740	

The first three columns are unconditional mean comparisons. The second two are conditional on sibling size.
*In the case of the occupational level the number of observations is smaller because of people who never worked.

Table 3: First stage: Effect of compulsory school reforms on education

Pooled				
CSL	0.288 ***	0.259 ***	0.251 ***	0.324 ***
	(0.059)	(0.073)	(0.068)	(0.066)
R^2	0.254	0.256	0.255	0.255
F-test*	23.85	12.57	45.34	24.44
N	10098	10098	10098	10098
Men				
CSL	0.288 ***	0.274 **	0.250 **	0.341 ***
	(0.086)	(0.107)	(0.100)	(0.094)
R^2	0.253	0.256	0.250	0.252
F-test*	11.38	6.57	6.32	13.03
N	4494	4494	4494	4494
Women				
CSL	0.281 ***	0.228 **	0.231 **	0.292 ***
	(0.082)	(0.101)	(0.093)	(0.091)
R^2	0.256	0.260	0.260	0.259
F-test*	11.91	6.13	6.23	11.21
N	5604	5604	5604	5604
Controls:				
Country f.e.	X	X	X	X
Cohort f.e.	X	X		
Quadratic in age			X	
3-years cohort f.e.				X
Country linear trends		X	X	X

*F-test on the excluded instruments.

Standard errors are robust to heteroskedasticity.

Table 4: 2SLS estimates of the effects of one extra year of schooling on old age health and cognition

Pooled				
Memory	0.204 ***	0.195 **	0.183 **	0.180 ***
	(0.059)	(0.079)	(0.074)	(0.055)
SRH (good)	0.034	0.038	0.051	0.055 **
	(0.029)	(0.038)	(0.038)	(0.028)
Men				
Memory	0.280 ***	0.272 **	0.285 **	0.248 ***
	(0.106)	(0.137)	(0.142)	(0.090)
SRH (good)	0.119 **	0.127 *	0.143 **	0.104 **
	(0.051)	(0.066)	(0.072)	(0.044)
Women				
Memory	0.140 *	0.123	0.102	0.119
	(0.073)	(0.105)	(0.095)	(0.075)
SRH (good)	-0.033	-0.048	-0.033	0.010
	(0.044)	(0.069)	(0.060)	(0.043)
Controls:				
Country f.e.	X	X	X	X
Cohort f.e.	X	X		
Quadratic in age			X	
3-years cohort f.e.				X
Country linear trends		X	X	X

Standard errors are robust to heteroskedasticity.

Table 5: 2SLS estimates of the effect of one extra year of schooling on the occupational level

Pooled				
Occupation	0.114 **	0.258 ***	0.261 ***	0.178 ***
	(0.057)	(0.100)	(0.095)	(0.058)
N	9278	9278	9278	9278
F-test*	15.00	7.40	30.02	11.03
Men				
Occupation	0.118	0.278 **	0.255 *	0.176 **
	(0.086)	(0.130)	(0.131)	(0.083)
N	4318	4318	4318	4318
F-test*	9.03	5.03	4.82	10.01
Women				
Occupation	0.098	0.244	0.284 *	0.189 **
	(0.078)	(0.169)	(0.154)	(0.087)
N	4960	4960	4960	4960
F-test*	9.98	2.59	3.52	8.50
Controls:				
Country f.e.	X	X	X	X
Cohort f.e.	X	X		
Quadratic in age			X	
3-years cohort f.e.				X
Country linear trends		X	X	X

Since the occupation history of the respondent is asked in wave 3 the sample is smaller because of attrition. Standard errors are robust to heteroskedasticity.

*F-test on the excluded instruments.

Table 6: Effect of first-born on education

Men	Age 50-80			Within 10 years CSL	
First-born	0.728 ***	0.512 ***	0.440 ***	0.499 ***	0.501 ***
	(0.110)	(0.113)	(0.104)	(0.123)	(0.123)
CSL					0.327 ***
R^2	0.262	0.272	0.401	0.290	0.294
F-test	43.70	20.55	18.05	14.14	13.12
N	4902	4902	4902	2584	2584
Women					
First-born	0.519 ***	0.316 ***	0.291 ***	0.284 ***	0.288 ***
	(0.092)	(0.093)	(0.084)	(0.107)	(0.107)
CSL					0.305 ***
					(0.097)
R^2	0.299	0.309	0.441	0.270	0.272
F-test	31.97	11.42	12.09	8.07	8.57
N	5955	5955	5955	3358	3358
Controls:					
Country linear trends	X	X	X		
Sibling size		X	X	X	X
Childhood conditions*			X	X	X
Always included: Cohort and country f.e.					

The last two columns restrict the sample to respondents born 10 years before and after the pivotal cohorts as in Table 3. Standard errors are robust to heteroskedasticity.

*Childhood conditions include self-reported childhood health and household characteristics when the respondent was 10: people per room, number of books at home, breadwinner's occupational level, whether parents smoke or drink and dummies for the area or residence.

Table 7: 2SLS estimates of the effects of one extra year of schooling on old age outcomes: men

2SLS	Age 50-80		Within 10 years CSL	
Memory	0.088 *	0.083	0.147 **	0.181 ***
	(0.048)	(0.056)	(0.070)	(0.058)
J-test ($p-$value)				0.410
SRH (good)	0.072 **	0.080 **	0.122 ***	0.115 ***
	(0.029)	(0.034)	(0.045)	(0.035)
J-test ($p-$value)				0.775
Occupation	0.243 ***	0.253 ***	0.263 ***	0.245 ***
	(0.055)	(0.065)	(0.080)	(0.058)
J-test ($p-$value)				0.561
Controls:				
Country linear trends	X	X		
Sibling size	X	X	X	X
Childhood conditions*		X	X	X
Always included: Cohort and country f.e.				

The last two columns restrict the sample to respondents born 10 years before and after the pivotal cohorts as in Table 3. Standard errors are robust to heteroskedasticity.

Table 8: 2SLS estimates of the effects of one extra year of schooling on old age outcomes: women

2SLS	Age 50-80		Within 10 years CSL	
Memory	0.115	0.115	0.145	0.144 **
	(0.075)	(0.079)	(0.106)	(0.065)
J-test ($p-$value)				0.380
SRH (good)	0.132 **	0.140 **	0.146 **	0.056
	(0.052)	(0.056)	(0.076)	(0.037)
J-test ($p-$value)				0.034
Occupation	0.268 ***	0.275 ***	0.299 *	0.183 **
	(0.093)	(0.102)	(0.151)	(0.079)
J-test ($p-$value)				0.252
Controls:				
Country linear trends	X	X		
Sibling size	X	X	X	X
Childhood conditions*		X	X	X
Always included: Cohort and country f.e.				

The last two columns restrict the sample to respondents born 10 years before and after the pivotal cohorts as in Table 3. Standard errors are robust to heteroskedasticity.

Table 9: Populations affected by the two instruments

	Men		Women	
Education:	HS dropouts	HS or college	HS dropouts	HS or college
CSL	0.410 **	0.174	0.280 **	-0.044
	(0.178)	(0.143)	(0.116)	(0.128)
first-born	0.108	0.589 ***	0.305 ***	0.333 **
	(0.152)	(0.152)	(0.118)	(0.137)
Labor force attachment*	89.18%	92.80 %	51.90%	80.70 %
N	997	1816	1574	1935

Each regression includes country dummies, sibling size dummies, 3-years cohort dummies and country specific linear trends in age.

*Current or previous labor marker attachment of the four groups. It considers the percentage of respondents who are employed in wave 2 or who declare to have worked at least up to the age of 50.

Table 10: Indirect exogeneity tests: OLS regression of first-born on height and childhood health

	Men			Women		
Height	0.457 **	0.294	0.227	0.059	-0.112	-0.117
Childhood health	0.022	0.028	-0.026	0.024	0.014	0.014
Chance inheritance	3.731 *	-0.073	0.207	3.538 *	1.836	2.386
Trust	0.071	0.061	0.051	0.126 *	0.046	0.046
Religion	-0.222 ***	-0.062	-0.053	- 0.190 ***	-0.010	-0.003
Controls:						
Sibling size		X	X		X	X
Childhood controls			X			X

Each regression includes also controls for sex, country and cohort dummies and a country-specific linear and quadratic trend in birth order. Standard errors are robust to heteroskedasticity.

Fig. 1: Effect of the reforms on years of education

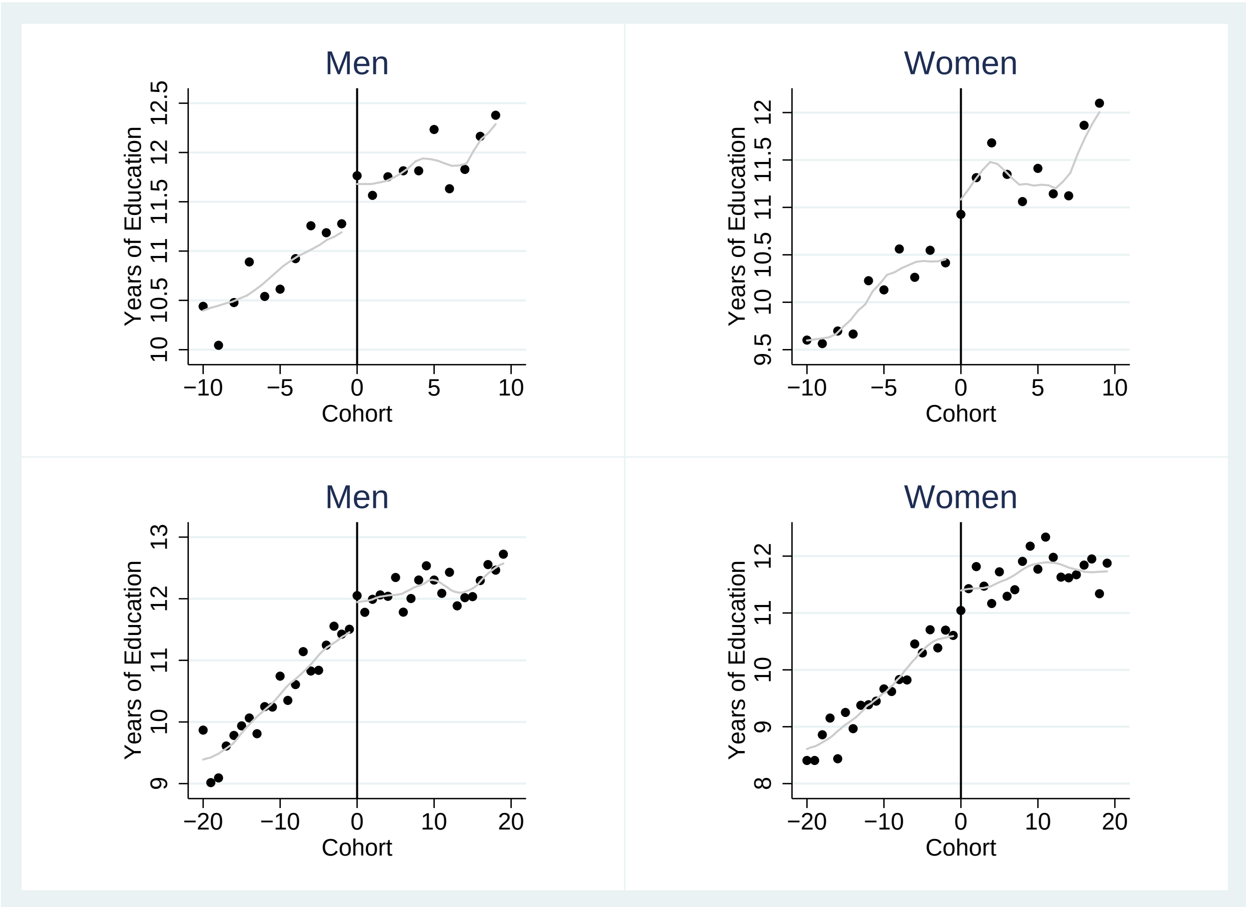


Fig. 2: Effect of the reforms on cognitive abilities

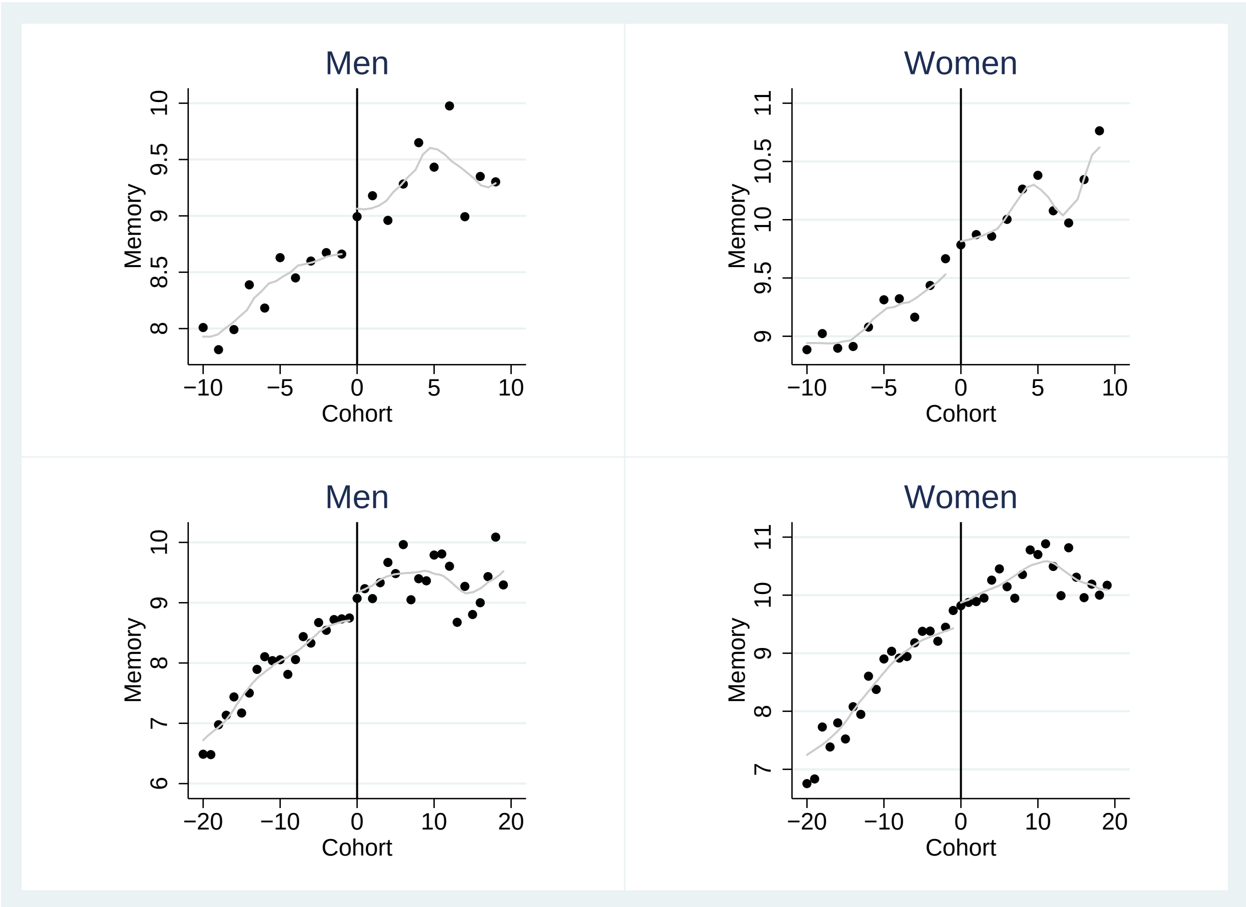
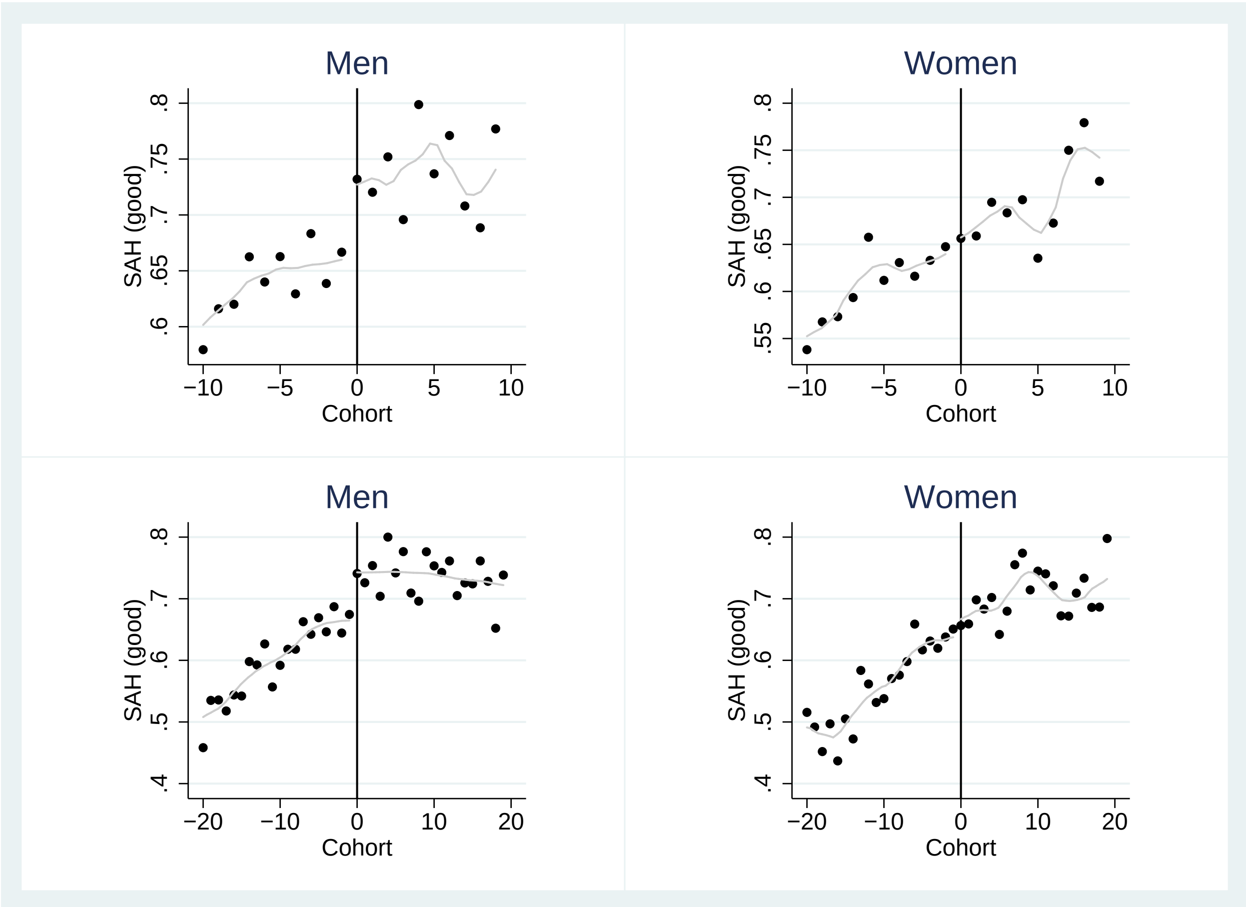


Fig. 3: Effect of the reforms on health



A Compulsory school reforms

This paper bases its analysis on compulsory schooling reforms implemented after World War II that affected the cohorts in the SHARE sample of the following nine European countries : Austria, Czech Republic, Denmark, France, Germany, Italy, Netherlands, Poland and Sweden.

Most of the information for each reform is based on Fort (2006) who made an extensive survey on European school systems and Brunello *et al.* (2009) who used such information to estimate returns to schooling. Moreover, I cross-checked the information with national sources. Different from Brunello *et al.* (2009) I follow Arendt (2005) and Bingley *et al.* (2009) who show that the 1958 CS reform in Denmark did not increase formally the CS but eliminated barriers to access to the 8th and 9th grade for children from villages. Finally, based on Jung-Miklaszewska (2000) I add information on the 1961 CS reform in Poland implemented only in 1966.

B Sensitivity to bandwidth selection

Table 11: 2SLS estimates of the effects of one extra year of schooling on old age health and cognition

Within 7 years before and after the pivotal cohorts				
Men				
Memory	0.242 **	0.231 *	0.239 *	0.257 ***
	(0.099)	(0.132)	(0.139)	(0.095)
SRH (good)	0.130 **	0.163 **	0.155 *	0.124 **
	(0.054)	(0.081)	(0.082)	(0.050)
Women				
Memory	0.121 *	0.118	0.084	0.093
	(0.072)	(0.138)	(0.132)	(0.093)
SRH (good)	-0.008	-0.023	0.028	0.046
	(0.042)	(0.088)	(0.078)	(0.054)
Within 5 years before and after the pivotal cohorts				
Men				
Memory	0.161 *	0.299	0.279 *	0.358 **
	(0.096)	(0.196)	(0.162)	(0.154)
SRH (good)	0.123 **	0.087	0.134	0.186 **
	(0.057)	(0.088)	(0.090)	(0.086)
Women				
Memory	0.083	0.084	0.121	0.131
	(0.079)	(0.104)	(0.115)	(0.096)
SRH (good)	-0.037	-0.112	-0.004	0.021
	(0.049)	(0.089)	(0.070)	(0.057)
Controls:				
Country f.e.	X	X	X	X
Cohort f.e.	X	X		
Quadratic in age			X	
3-years cohort f.e.				X
Country linear trends		X	X	X

Standard errors are robust to heteroskedasticity.