This paper is a draft submission to the

WIDER Development Conference

Human capital and growth

6-7 June 2016 Helsinki, Finland

Intergenerational Transmission of Education in Europe. A New Instrumental Variable Application. *

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Abstract

We estimate the causal effect of parental education on children's education using rich data from the Survey of Health, Ageing and Retirement in Europe (SHARE). We exploit two sources of variation: parental birth order and compulsory schooling laws (CSL) in Europe. While CSL have been widely used in the economic literature, our contribution is to consider parental birth order as an instrument for parental education. We find a positive and significant causal effect of parental education on children's education either using birth order alone or combining it with CSL: an increase of parental education by one year leads to an increase of children's schooling by about half a year. As an extension, we show that the two instruments target different compliant sub-populations , with CSL reforms affecting parents from a poor background and first born status affecting parents from a higher socio-economic status. Since the two instruments affect different subpopulations (based on compliers' characterization) and yet deliver IV estimates not statistically different from each other (as implied by the overidentifying test of restrictions), we interpret these findings as a piece of evidence in favor of homogeneous treatment effects.

Keywords: intergenerational transmission, education, birth order, causal effect, Europe, SHARE

JEL codes: J01, J6, I2, I24.

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†Any views expressed in this paper are those of the authors and do not reflect those of the Bank of Italy and European Commission.

1 Introduction

It is widely known that more educated parents raise more educated children, although it is difficult to identify the channels that contribute to this positive relationship and their relative importance (Becker and Tomes 1979; Black and Devereux 2011; D'Addio 2007). Inherited ability (nature) is a key driver in the transmission process of human capital, as it reflects the underlying genetic endowment that is passed over generations. On the other hand, children's probabilities of success in life are also influenced by the environment in which an individual grows up (nurture), shaped by parental education and the amount of time and resources invested on them.

Disentangling the nature channel from the nurture one would help solving not only an old controversy in the social sciences but also improve the design of more efficient policies in the future. If the nurture channel prevails, then a well targeted policy intervention (such as promoting the diffusion of student loans to mitigate family financial constraints) can lead to positive spillovers for successive generations. Vice-versa, if ability dominates, policies can still be useful in helping more disadvantaged children, but the achievements would come at a greater cost (Holmlund et al. 2011). Since parents not only invest time and financial resources on children but also transmit part of their unobserved ability, simply regressing children's education on parental education is likely to produce upward biased estimates of the parameter of interest, overstating persistence of education across generations. In this paper we account for the endogeneity issue proposing a new Instrumental Variable (IV) application.

Our contribution to the existing literature is twofold. First, we introduce parental birth order as an instrument for parental education, exploiting the fact that first born individuals are on average better educated than later born individuals (see sections 3.2 and 4.3 for details).[1] This strategy does not require exogenous shocks to be implemented (in the spirit of the quasi-experimental method), as information on parental birth order is available in most household surveys.[2] Second, we enrich the instrument by exploiting variation in compulsory schooling reforms (CSL) in different European countries and different years, which allow us to estimate an over-identified model.[3]

[1] Using birth order as an instrument for years of education is not new in the economic literature. Gary-Bobo et al. 2006 use individual birth order as an instrument for years of schooling when estimating the returns to schooling. Our paper differs in this respect. We impose a different exclusion restriction, namely that parental birth order does not have a direct impact on children's education.

[2] Such as the German Socio-Economic Panel, the Health and Retirement Survey, etc.

[3] We rely on Brunello et al. (2009) and Brunello et al. (2012) for information on institutional background.

Although the two instruments target two different sub-populations (namely, first born parents and parents exposed to the increase of the minimum schooling leaving age), results are similar using either one or the other instrument, or using both of them. Since exogeneity of CSL reforms is generally accepted, we interpret this result as evidence in favor of the exogeneity of parental birth order as instrument (the characterization of the compliant subpopulation associated to each instrument is described in Section5.2).

To put in practice this identification strategy we use rich data from the Survey of Health, Ageing and Retirement in Europe (SHARE), which collects information on individuals aged 50 or more in many European countries and offers at the same time detailed data on their offspring. We consider 13 European countries (Austria, Belgium, Czech Republic, Denmark, France, Germany, Greece, Italy, Netherlands, Poland, Spain, Sweden, Switzerland), which allow us overcome the limitation of relying on a single country (external validity). Two are the main advantages of SHARE: i) the cross-country comparability of educational attainment due to the common questionnaire and the standardization of fieldwork procedures in each country; ii) the possibility to exploit retrospective data on individuals' lives from age 10 until late adulthood (SHARELIFE).

We find that parental education has a positive and significant causal effect on children education. Using first born instrument only, a one-year increase in parental education leads to an increase of 0.429 years in children's schooling. When the sample is restricted to countries that experienced changes in the compulsory schooling legislation (which reduces the sample size by two thirds), the causal effect increases to 0.605 using first born alone and to 0.538 using as instruments both first born indicator and compulsory years of schooling. We acknowledge that the two instruments target two different sub-populations and we address this issue by characterizing the sub-population of the *compliers* with respect to some predetermined socio-economic variables. As expected we find that CSL reforms mostly affect individuals at the bottom of the education distribution while the same cannot be stated for first born *compliers*. Noteworthy, the causal effects are similar (in a statistical sense) in apparently different sub-populations: a piece of evidence in favor of the treatment homogeneity, although this conclusion must be taken with care. Finally, we estimate separate regressions for the responding parent being the mother or the father. The main finding is that mother's education seems to matter more than father's education, but differently from previous literature we also find strong effects for father's education.

Finally. we run a series of robustness checks addressing issues related to the validity of the instruments and model specification. Our baseline results remain intact.

The paper is organized as follows: Section 2 describes the relevant literature regarding intergenerational transmission of human capital in a causal framework. Section 3 outlines our identification strategy and provides background information on parental birth order and CSL. Section 4 describes our data, what are our most important variables and sample choices, and provides some descriptive statistics. Section 5 presents our results: OLS regressions, Two-Stage Least Squares regressions (2SLS), the characterization of the compliers sub-population and robustness checks. Section 6 concludes and discusses policy implications.

2 Background literature

The literature on intergenerational transmission of education from parents to children is very rich and has traditionally followed a descriptive approach, documenting education persistence across generations. However, in the last years there have been attempts to unveil the causal mechanisms (if any) underlying such associations (Black and Devereux 2011).

Empirical research accounts for endogeneity of parental education typically resorting to three different strategies: twins studies (where the difference in schooling between children of twin parents is regressed on the difference in schooling between the twin parents themselves, so unobserved components are removed within twins),[4] adoptees (based on the idea that the genetic link between adopted children and adoptive parents is absent),[5] or instrumental variables (which exploit plausible exogenous shocks that in principle should affect children's schooling only through the education of the parents, commonly called the exclusion restriction). None of these strategies is immune from pitfalls. Twins and adoptees strategies are usually based on relatively small samples and they often neglect relevant unobserved heterogeneity.[6] On the other hand, instruments typically used to identify the parameter of interest (such as CSL reform, or variation in college attendance rates) may be weak, capture cohort

[4]Examples of twins strategy are Rosenzweig and Wolpin (1994), Behrman and Rosenzweig (2002), Pronzato (2012).

[5]Examples of adoptees strategy are Sacerdote (2002, 2007), Björklund et al. (2006).

[6]For instance, the fact that identical twins may not have identical abilities or that, as in the adoptees case, the placement process can be related to the characteristics of the biological parents.

variation if the policy shock is implemented nationwide, or cannot even be available for the population of interest.

Our paper contributes to the IV strategy stream of literature. We instrument parental education with parental birth order alone (in the first part of the analysis) and with birth order and CSL reforms together (in the second part). We show that birth order as instrument is relevant (it is highly predictive of parental education, *ceteris paribus*) and delivers parameter estimates non statistically different from those obtained using the two instruments together: we interpret this fact as indirect evidence in favor of the exogeneity of parental birth order. The economic rationale for using these two instruments is developed in Sections 3.2 and 3.3.

Among others,[7] our paper is mostly related to Oreopoulos et al. (2006), Chevalier (2004), Black et al. (2005b), which exploit CSL reforms implemented after 1945 respectively in US, UK and Norway. The intuition behind these papers is that a given cohort of parents in a given county (or state, or municipality)[8] was forced to stay in school an extra year before they were legally allowed to drop out: this extra year of education does not depend on parental unobserved ability or motivation and can be exploited as a source of exogenous variation. Oreopoulos et al. (2006) find that increasing parental education significantly reduces children probability of grade repetition, and that mother's and father's effects have similar magnitude. Chevalier (2004) finds a positive effect of maternal education on the probability that a child receives a post-secondary degree; no significant effect is found for fathers. Black et al. (2005b) show no evidence of a causal relationship between parents' and children years of education; when restricting the sample to parents with less than 9 years of education (since the reform should not have impact on more educated parents), they find no effect for fathers and small effects for mothers.

Our paper is also related to two recent works by Mazzonna (2013) and Stella (2013), both of them using SHARE data. Mazzonna (2013) analyzes the causal effect of education on physical and mental health of the elderly population (SHARE respondents), instrumenting education with birth order and CSL reforms. The most important differences between our paper and Mazzonna (2013) are that: first, we focus on the intergenerational persistence of education and not on the education-health relationship; second, in our setting the statistical units are children of SHARE respondents and the

[7]Such as Carneiro et al. (2013), Maurin and McNally (2008).

[8]With the exception of Chevalier (2004), in which the 1972 CSL reform was applied nationwide in England and Wales, in the other papers cited above there is variation in CSL implementation both in time and in space.

endogenous variable is parental education, thus leading to a different exclusion restriction (while in Mazzonna 2013 both the dependent and independent variable is defined at the same individual level). Stella (2013) adopts the CSL strategy in an IV regression of child's on parental education. There are important differences between our paper and Stella (2013). First, we instrument parental education with birth order and CSL: this allows us to run an over-identification test of restrictions for the instruments exogeneity and - within a heterogeneous effect framework - to study the characteristics of the sub-populations who comply with our two instruments. Second, we consider up to 4 children for each parent while Stella's analysis is limited to *first born children* only: for the reasons explained in section 3.2 we think that first born individuals are different from later borns, so results obtained with a sample of first born children may not be generalized to other siblings.

3 Empirical strategy

3.1 The model

Our analysis assumes a linear model linking child's education to parents' education, unobserved child's ability and other observed socio-demographic characteristics. This model is standard in the economic literature when the dependent variable is given by years of education and seems to fit very well our data (see the descriptive analysis in section 4.3). The parameter of interest is the expected increase in children's education caused by a one-year increase in parental education, anything else being equal. Therefore, our interest is in estimating β_1 in the following equation:

$$E_{ij}^{C} = \beta_0 + \beta_1\, E_j^{P} + \eta\, h_{ij}^{C} + \epsilon_{ij}, \tag{1}$$

where E_{ij}^{C} denotes years of the education of child i in family j, E_j^{P} is parental education (note that this variable does not have the index i, as we can have more children for one parent), h_{ij}^{C} represents child's unobserved ability and ϵ_{ij} is a random component uncorrelated with any other variables included in the model. OLS estimation of (1) is flawed since children's ability is not observed. If innate ability is transmitted across generations (for example through a first order AR process)[9] and if we assume $\eta > 0$,

[9] If, for example, equation (1) was estimated on a sample of adopted children and adoptive parents, then: *(i)* child's and parent's innate ability would be orthogonal; *(ii)* child ability would be orthogonal

then we have $\text{Cov}(E_j^P, h_{ij}^C) > 0$: in this case, the OLS estimator of β_1 is biased upward.

To consistently estimate β_1 we rely on instrumental variables (IV). The underlying idea is to exploit exogenous variation in parents' education (that is, not related to parental ability) to identify the causal effect on children's education. The identifying assumption is that the instruments affect child's education only through parental education.

Accordingly, we estimate the following model by two-stages least squares (2SLS), where equations (2) and (3) are respectively the causal equation of interest and the first stage:

$$E_{ijtc}^C = \beta_0 + \beta_1 \, E_{jtc}^P + \mathbf{X}_{ijtc}{}'\beta_2 + f(t,c) + (\eta \, h_{ijtc}^C + \epsilon_{ijtc}) \tag{2}$$

$$E_{jtc}^P = \alpha_0 + \mathbf{Z}_{jtc}{}'\alpha_1 + \mathbf{X}_{ijtc}{}'\alpha_2 + f(t,c) + \xi_{jtc}. \tag{3}$$

In equations (2) and (3), E_{ijtc}^C denotes again years of the education of child i in family j born in year t in country c, E_{jtc}^P is parental education, h_{ijtc}^C is the omitted child ability, and ϵ_{ijtc} and ξ_{jtc} are random shocks affecting respectively child's and parent's education. In the regressions we include a set of socio-demographic controls for both generations $\mathbf{X}_{ijtc}$ and a set of country and cohort fixed effects $f(t,c)$. The matrix $\mathbf{Z}_{jtc}$ represents our set of instrumental variables: depending on the specifications we will use $\mathbf{Z}_{jtc} \equiv [\text{FB}_{jtc}]$ or $\mathbf{Z}_{jtc} \equiv [\text{FB}_{jtc}, \ \text{CSL}_{jtc}]$, where FB_{jtc} is a binary variable taking value 1 when the parent is first born and CSL_{jtc} measures the minimum years of completed education mandated by Compulsory Schooling Laws.

In the next two sections we provide more details on our instruments and discuss their validity.

3.2 First instrument: Parental birth order

It is well established in the economic literature that earlier born individuals tend to have more years of education than the later born (Becker and Lewis 1973, Black et al. 2005a, De Haan 2010). There are several possible (and non exclusive) reasons behind this fact. First, credit constrained families may run out of financial resources to support investment in human capital of later born children, especially in larger fam-

to parent's education and; *(iii)* its omission from equation (1) would not bias OLS estimates.

ilies. This last consideration highlights the importance of appropriately accounting for family size in the birth order analysis, since later born individuals are more likely to belong to larger families (e.g. a birth order of 4 implies a sibship size larger than or equal to 4). Second, there might exist cultural preferences that tend to favor investment of resources and time towards first born children or contrary on later born children. For instance, De Haan and Plug (2014) using data from Ecuador find that later born children have higher education compared to earlier borns. They argue that high poverty rates in Ecuador or in developing countries more generally could be an explanation for this opposite finding. Third, if the time spent with parents during early childhood is more valuable (in terms of future educational achievement) than the time spent when children are grown-up, then a first-born child is advantaged since she does not have to share parental time with other siblings until a second child is born. Price (2008), using data from the American Time Use Survey shows that a first-born child receives on average 20 to 30 more minutes of quality time compared to a second born child while controlling for family size and an extended number of characteristics. Fourth (but related to the third reason), as emphasized by Zajonc (1976) "confluence model", the average intellectual environment within the family declines as the number of children increases. In other words, a first child exploits the richest intellectual environment composed by two cognitive mature adults, a second child lives with two adults and one immature child and so on: the result is that later born children face the most diluted intellectual environment, with negative consequences for their education. Finally, there might be strategic parenting, according to which the upbringing of a first born child is stricter in order to deter bad behavior in later born children, and this strict upbringing contributes to first borns' higher education (Hotz and Pantano 2013).

Determining which of these channels prevails, and how their relative importance varies across countries and cohorts, is indeed an interesting topic to explore but this goes beyond the scope of the paper. Abstracting from the underlying mechanisms that link birth order to education, a necessary condition for our identification strategy is that the first-born instrument is exogenous.[10]

A key assumption is that the first-born instrument satisfies the exclusion restriction, that is to say that parental education is the only channel through which the first-born variable FB_{jtc} affects children's education E^C_{ijtc}. The exclusion restriction may be

[10]In our previous notation: $\text{Cov}(\text{FB}_{jtc}, \eta\, h^{\text{C}}_{ijtc} + \epsilon_{ijtc}) = 0$.

violated if first born parents have a better endowment of innate ability, which then is transmitted to their children via the genetic link. It is difficult though to imagine a credible reason why first born individuals should possess more innate ability than later born individuals, except maybe for the fact that later-born children are born to older mothers and are going to receive a presumably lower quality genetic endowment. The evidence on the relationship between birth order and intelligence, as measured by IQ or by *ah hoc* designed cognitive tests, is highly controversial (see discussions in Black et al. 2011 and in Kanazawa 2012). Most of this controversy involves whether birth order has genuine within-family effect (with earlier born being more intelligent then later born in the same family) or reflects spurious between-family association (with earlier born in small families being more intelligent than later born in large families). Rodgers et al. (2000) argue that almost all empirical studies have used cross-sectional data, and the negative association between birth order and intelligence found by those studies is a "methodological illusion". More recently Black et al. (2011) find large and significant birth order effects on IQ for a large sample of Norwegian young men, using both cross-sectional and within-family methods; however, the authors themselves state that such IQ gap cannot be ascribed to either genetic or biological differences resulting from different experiences *in utero*. Kanazawa (2012) studies the effect of birth order an a series of *ad hoc* designed cognitive tests for a cohort of British children at ages 7, 11 and 16. Differently from Black et al. (2011), he finds that the correlation between birth order and test scores is completely driven by sibship size and suggests that "birth order has no genuine causal effect on general intelligence". In light of the above discussion, we feel safe in considering parental birth order orthogonal to children's innate ability.

The exclusion restriction might also be violated if parental birth order is correlated to some other determinants of children's education, that go beyond father education. Although instrument exogeneity is untestable, we tackle this issue in two ways. First, evidence from the *J*-test of over-identifying restrictions never rejects the null hypothesis of instruments exogeneity: since CSL reforms arguably satisfy the exclusion restriction (see section 3.3), we interpret this evidence in favour of birth order exogeneity. Second, we can exclude one potential mechanism through which parental birth order may affect children's education, other than parental education. If first born parents are more likely to receive bequests from their parents, this can have a positive effect on children's education. Our data show that birth order is not associated with the probability of receiving a property house as bequest. Finally, Mazzonna (2013) shows that parental birth order does not affect biological characteristics (such as adult height) or psychological traits (such as the level of religiosity) that in our case may drive both parental and children education.[11]

3.3 Second instrument: Compulsory Schooling Laws

As stated previously, we use as a second instrument the number of mandatory schooling years induced by the introduction of education reforms in 10 European countries in different years. The use of CSL as instrument for years of education is well established in the economic literature and has been employed in different contexts (Angrist and Krueger 1991; Oreopoulos et al. 2006; Chevalier 2004; Brunello et al. 2009, etc), showing a positive correlation with years of education.

For the use of mandatory schooling years as instrument we rely mostly on Brunello et al. (2009), where they exploit plausibly exogenous variation in schooling induced by a series of reforms adopted in Europe in the post-World War II period. The key assumption in this type of literature is that mandatory schooling is assigned to each individual based on the date of birth [12] and shouldn't affect their future choices or observable characteristics. In our paper this translates into saying that mandatory schooling is assigned to parents based on their year of birth without influencing directly the future education of their children (exclusion restriction). The identification of CSL effects on parental education comes from the variation of the schooling reforms across countries and over cohorts. In a few cases reforms have been adopted in different years for different regions within a country (e.g Germany).

Despite the customary use of CSL in the literature there are might be suspects on the exogeneity assumption. One raised issue is that educational reforms could potentially improve school quality (e.g leading to spillover effects) which is in most cases an omitted variable. Brunello et al. (2013) develop a test to assess whether the identification strategy based on CSL allows to estimate the causal effect of education when school quality and other unobserved factors are omitted. They find that the internal validity of this instrument remains intact.

[11] In Mazzonna (2013) the focus is on potential drivers of educational choices and old age health outcomes for the same individual.

[12] The crucial information is given by the year in which the reform is passed and the cohort interested by mandatory schooling.

4 Data and descriptive statistics

4.1 The Survey of Health, Ageing and Retirement in Europe

Our study draws on the Survey of Health, Ageing and Retirement in Europe (SHARE), a multidisciplinary cross-country household panel survey, which collects detailed information on individuals aged 50 or more (plus their spouses independent of age), who speak the official language of the country in which they reside and do not live abroad or in an institution. SHARE is to be considered a representative sample of old-age Europeans and is conducted in many countries (13 of which form a longer panel), representing different areas of Europe: Northern Europe (Denmark and Sweden), Central Europe (Austria, Belgium, France, Germany, the Netherlands, Switzerland), Eastern Europe (Poland and Czech Republic), the Mediterranean countries (Greece, Italy, Spain), Ireland and Israel. Five waves of SHARE are currently available, where in the last two waves (4 and 5) new countries joined the project (Estonia, Hungary, Luxembourg, Portugal Slovenia). Greece left the project in wave 4, while Israel participated in wave 1 and has re-joined the project in wave 5.

SHARE collects detailed information at the household and individual level covering different areas of research such as: household economics, education, health, social security and income, financial investments, etc. An advantage of the survey is the cross-country comparability of educational attainment and many other variables due to the common questionnaire and the standardization of fieldwork procedures in each country (Börsch-Süpan and Jürges 2005). Further, it is designed to be harmonized with other surveys such as the Health and Retirement Study (HRS) in the US and the English Longitudinal Survey on Ageing (ELSA) in UK.

A second advantage of SHARE is the release of its third wave, named SHARELIFE, which recollects retrospectively information on life-histories of each individual from age 10 onwards considering among others housing transitions (from first residence), employment transitions (from first job), complete fertility histories, changes in health and health behaviour, and includes also a distinct module with detailed questions regarding childhood circumstances (when respondents were about 10 years old). This is important in the light of numerous findings that early life circumstances matter for long-term outcomes (Almond and Currie 2011). In SHARELIFE the modules of questions are arranged based on what is usually most important for the respondent and hence remembered most accurately (e.g starting from children, partners and then accommodation). Further, the interview is supported by a multidimensional life grid, a computerized version of the life-calendar interview - that allow respondent to view important events on a computer screen and at the same time allows the interviewer to link questions to parallel events. See Havari and Mazzonna (2011) for the analysis of recall bias problems in the SHARELIFE data, where they show that information related to the childhood period is very little affected by problems of recall bias or coloring.

In our analysis we will consider only countries that participated in both wave 2 and 3. This choice is motivated by the fact that important information on parents' family background (e.g birth order and family size) is collected by combining these two waves together.

4.2 Description of main variables

Parental and children education The main variables in our analysis are the educational attainment of parents and children. We measure education by the number of years spent in full time education. For parents we know both self-reported years of completed schooling and the highest degree obtained based on the International Standard Classification of Education (ISCED). For children instead, we know the highest degree obtained (using ISCED classification)which we convert into years of education by exploiting information on the education system of each country.[13] Since the questionnaire is common to all countries and the questions related to education attainment are already expressed in a comparable format (using ISCED classification), we expect errors to be of small size if any.

Parental birth order and sibship size. Information on parental birth order is gathered from wave 2. Each respondent is asked to provide his birth order, specifically whether he is the oldest child, the youngest one or somewhere in between. Further, he is asked to count the number of siblings still alive at the time of the interview. To better infer respondent's sibship size we also rely on questions contained in the childhood module (SHARELIFE). In particular, knowing the number of persons living in the house at the age of 10 and the presence of each family component (mother, father, etc) we compute the number of siblings at the age of 10. We combine information on sibship size from both wave 2 and SHARELIFE and in absence of information from

[13]SHARE provides the table of conversion from ISCED level into years of education in the release guide (see SHARE release guide wave 2, 2-5-0, 2011).

wave 2 we use siblings size defined at the age of 10.

Compulsory schooling laws. Information on CSL is based on the papers by Brunello et al. (2009) and Brunello et al. (2012). We restrict our sample to include the following countries: Austria, Belgium (only Flanders), Czech Republic, Denmark, France, Germany (only Western part), Italy, the Netherlands and Sweden (See Table 1). [14] We select one reform for each country (or Land in the case of Germany), and report respectively the year in which the reform was passed, the birth year of the pivotal cohort (first cohort hit by the reform) and the change in mandatory schooling years (whether it was raised from 8 years to 9 years, and so on).

There is a substantial variation across countries in the year the reform was adopted, whereas the change in mandatory schooling years is the same for most of the countries (from 8 to 9 years). This means that using CSL as instrument we identify the effect of a one year increase in schooling for parents at the ISCED 2 level. To avoid blurring the differences between the treated group (cohorts born after the pivotal cohort) and the control group (cohorts born before the pivotal cohort), we restrict the sample selecting parents born up to ten 10 years before or after the pivotal cohort. Furthermore, we restrict the sample to biological children (about 96% of the sample) that are aged 25 or more and have completed their education (see Section 4.3). [15]

Other control variables We also control for children's birth order and the sibship size as we do for parents. Information on children can be found in wave 2 in a specific module, where one of the parents (the family respondent), provides details for children's year of birth, gender, education, whether she is a biological child or not, marital status, etc.

4.3 Sample selection and descriptive statistics

In the regression analysis we use two samples, depending on the adopted set of instruments. When parental first born indicator is used alone, we consider the full sample. We select parents present in both wave 2 and 3 of SHARE and aged more than 50 years old (about 14.000 parents). For each respondent, we know the schooling level of up to four selected children. We end up with a sample of almost 32.000 children.

[14] For more details we remind to the paper by Brunello et al. (2009).

[15] In Section 5.5 we show that results are not affected if we restrict the sample to parents born 7 years before and after the pivotal cohort.

From now on, we refer to this sample as the *Full sample*. Figure 1 gives a graphical description of birth year for parents and children in the full sample.

When parental first born indicator and CSL reforms are used together, our sample is restricted to those countries that actually experienced a CSL reform: in this way we lose Greece, Poland, Spain and Switzerland. Moreover, to isolate the CSL reform effect, for each country we consider parents born up to 10 years before and 10 years after the first cohort potentially affected by the reform (so called pivotal cohort). With these restrictions we are left with about 6000 parents and 13000 children. For simplicity we refer to this sub-sample as the *CSL sample*.

Table 2 shows descriptive statistics for parents and children using both full and CSL sample. In the full sample parents have on average 10.6 years of education, 57% are female and the average age is 63. Furthermore, about 39% are first borns, have on average two children, and prevalently live with the spouse or partner (74%). The picture is similar in both samples, with the exception of average years of education which is slightly higher in the CSL sample (11.3 years). This could reflect the fact that some countries were excluded due to absence of strict CSL reforms. Children have on average 13 years of education in both samples, are equally shared among males and females and aged about 40. About 45% are first borns and 64% of them live with the spouse or partners.

Since we are interested in the relation between parental and children education, in Figure 2 we show the bivariate association between the two variables. Each point in the graphs represents the conditional sample mean of children education for any given level of parental education. These graphs provide support for the use of a linear model for our data.

5 Results

In this section we analyze the causal effect of parental schooling on children's schooling while accounting for a wide range of explanatory variables. We start the analysis discussing the ordinary least square (OLS) estimates of our main equation and interpreting them based on the literature of interest. We then provide an extensive analysis on the relevance of the two instruments, also exploring the characteristics of the compliant subpopulations. The purpose of this exercise is to say something about the nature of the causal effect of parents schooling on children's schooling. Differences in the compliant subpopulations can explain variability in the treatment effects from one instrument to another leaving space for extrapolation (Angrist and Pischke 2008) to other populations of interest, that is say something about external validity. If the compliant subpopulations associated to each instrument are different but yet the estimates are not statistically different from one another, one might be ready to use this evidence in support of homogenous treatment effects. This is one of the main objectives of this paper. At the end we run a series of specification checks to mitigate potential threats to the identification strategy employed.

5.1 OLS estimates

Since the sample size associated to each instrument is different[16], to make sensitive comparisons across estimation methods we report the OLS estimates for both the full sample and the CSL sample (Table 3). To make the comparison of estimates easier we employ one rich specification and apply it to different samples. We control for parental education, indicators for the dyads father-daughter, mother-daughter and mother-son (excluded category being father-son), aimed to capture fixed effect for parent and children gender.[17] As for children characteristics we control for birth order dummies (excluded category: being a first born child) and family size (the total number of children in the family).[18] Finally, we add country fixed effects, parental birth year fixed effects and child three-year cohort fixed effects. These fixed effects are meant to absorb time-invariant country characteristics and capture the historical evolution of years of schooling for both parent and children generations. Standard errors are clustered at the cohort and country level, to make OLS results comparable with the IV estimates in the next section. As expected we find a positive and significant relationship between parents' and children's years of education. An increase in parental education of one year leads to an increase in children's schooling of 0.25 of a year (about three months).[19] Interestingly, the estimated coefficients for parental education are the same across the two samples. In general, daughters are more educated than sons (irrespec-

[16]Full sample when using only first born as instrument; CSL sample when using compulsory schooling laws since we restrict the analysis to countries that have enacted important schooling reforms.

[17]We do so to keep the sample size as large as possible. At the end of the section we also discuss separate regressions for the mother and father.

[18]We also experimented using a series of dummies for number of children instead of using the continuous variable: estimated coefficients are very similar both for the parameter of interest and for the control variables.

[19]The estimates are consistent with the general findings in the literature. Intergenerational elasticities in education vary between 0.20 and 0.45 (Black et al. 2005b).

tively whether the respondent parent is male or female), and sons with respondent parent being the mother are more educated than sons with respondent parent being father.

The estimated coefficients of other control variables have the expected sign. Second born children, third born, fourth born, fifth or later born have respectively 0.32, 0.50, 0.82, 1.35 fewer years of education than first born children, suggesting that the advantage of earlier born individuals persists even in the most recent generations. Moreover, an increase in the number of children decreases the expected years of schooling of all children by 0.18 years of education (about 2.5 months). On the other hand coefficients on parental sibship size seem not to affect children's education. However, the OLS estimates should be interpreted with care and the endogeneity problem should be tackled accordingly to formulate policy recommendations. In the next sections we provide support to our identification strategy, discussing the advantages and potential threats to our approach.

5.2 First stage estimates: first born and CSL

We now discuss estimates from the first stage regression, which represents one of our core results. In the baseline regression we use the two instruments together which implies using the smallest sample (CSL sample), and restrict the analysis to countries that passed a reform and to cohorts born around the pivotal cohort. Furthermore, combining birth order with CSL allow us to say something about the exogeneity assumption related to first born indicator. At a second stage of the analysis we run our series of regressions using as instrument only the first born indicator.

Table 4 shows the estimates from the first stage where parental education is regressed on each instrument separately and then the two instruments together. As for the control variables we employ the same set as in Table 3. Clearly, children characteristics such as birth order and family size are not determinants of parental education but we include them to maintain comparison with the IV and the reduced form specifications. From the first column we see that being a first born parent increases parental schooling by about 0.5 (approximately 6 months of extra schooling) with respect to being a later born, everything else being equal. In the second column we regress parental education on CSL, that is the years of compulsory schooling depending on the country-cohort combination. One extra year spent in school due to the reform increases parental education by 0.20, about 2.5 months.[20] In the third specification we use the two instruments together. The coefficients of the first born indicator and CSL are statistically significant at a 1% level and their magnitude remains the same across different specifications.

In what aspects do these instruments differ from each another? To answer this question we enrich the analysis by showing, through a series of plots, which part of the education distribution is mostly affected by each instrument.

Figure 3 shows the empirical cumulative distribution function (CDF) for parental years of education for parents that were subject to compulsory schooling reforms and parents that were not (parents born before the pivotal cohorts). For simplicity, we create a binary indicator that takes value 1 if parents were born up to 10 years after the pivotal and value 0 if parents were born up to 10 years before the pivotal cohort. Similarly, Figure 4 and Figure 5 plot the distribution for parental education separately for parents who are first born or not first born, using both the CSL sample (respondents born within a window of 10 years before and after the pivotal cohort) and the full sample. These figures provide evidence that meet our expectations. CSL reforms seem to affect mainly parents at the bottom of the distribution: the gap in terms of years of schooling between the treated group (parents born after the pivotal cohort) and control group (born before the pivotal cohort) is higher for parents having less than 12 years of schooling. Instead the difference in years of schooling between first born and not-first born parents remains pretty stable across the whole distribution.

As a second extension we characterize the complier sub-populations, in order to learn as much as possible about the two instruments. The compliant sub-population associated to the CSL instrument is composed by parents who in absence of the reform wouldn't have taken further education. On the other hand, the sub-population related to the first born instrument is given by the pool of parents who wouldn't have taken further education had they not been first born.

Although the compliers cannot be listed from observed data, we can learn something about their characteristics by exploiting the Bayes theorem in the case where both the endogenous variable and the instrument are binary.[21] Therefore, we recode our endogenous variable (parental education) as a dummy taking value 1 if parent has more than 10 years of education.[22] Since the first-born variable is already binary, we

[20]The result from the first stage when using CSL is similar to that reported by Brunello et al. (2009).

[21]See Angrist (2004) and Angrist and Pischke (2008) for the methodology and Fort et al. (2011) for an application.

[22]We choose 10as a cut-off because the average years of schooling for parents lies between 10 years

recode CSL such that it takes value 1 if the individual belongs to the cohorts potentially affected by the reform and 0 otherwise. We characterize the sub-population of compliers according to the following set of pre-treatment binary variables that summarize parental family background when the parent was at the age of 10: *male*, (=1 if there were none or very few books in the house), *large household* (=1 if the number of people living with the respondent is higher than the country-cohort median), *room per capita* (=1 if the number of rooms per capita was smaller than the country-cohort median), *breadwinner main activity* (=1 if he/she was a blue collar or with an elementary occupation), *rural* (=1 if the household used to live in a rural area during childhood). The analysis for a sample of 6400 parents (CSL sample) is shown in Table 5, where we report the unconditional mean of the pre-treatment dummy X, the conditional mean for the compliers sub-population and the relative likelihood that a complier has $X = 1$ (respect to the whole sample).

With respect to the whole sample, CSL compliers are 25% more likely to be male, almost equally likely to have had fewer than 25 books at home when ten and to have lived in a larger family, 20% more likely to have had fewer rooms per capita and 28-33% more likely that their main breadwinner had an elementary occupation and they they lived in a rural area. All in all, this characterization confirms that CSL compliers have a poorer socio-economic background than the average parent in the sample. Tracing a precise picture of first born compliers is more difficult. With respect to the sample, they are 10% more likely to be male, 41% more likely to have had few rooms per capita, but they are about 30% less likely to have had few books at home, 15% less likely to have lived in a larger family and about 5% less likely to have lived in a rural area and that their main breadwinner had an elementary occupation.

Compliers' characterization shows that CSL compliers have a poorer socio-economic background compared to the overall sample, which presumably places them at the bottom of the education distribution. Differently, first born compliers have not a poorer background (if any, their socio-economic status is slightly higher than the average).[23]

and 11 years, depending on the sample used.

[23] A *caveat* applies here since we have not used the original variables but we recoded our endogenous variable and our CSL instrument in dummy indicators, in order to make the analysis feasible.

5.3 Reduced form estimates

Table 6 shows the estimates from the reduced form regressions where children's years of schooling is regressed on the instruments maintaining the same set of controls as in the previous tables. Three specifications are used, where we consider one instrument at a time and then combine them together. From the first column we see that being a first born parent is associated with an increase in children's schooling of about 4 months (0.27 years). The control variables have the expected sign. Children's birth order and family size are strongly correlated with children's years of schooling. Later born children have on average lower educational attainment compared to the earlier borns. This is an interesting pattern which reinforces the evidence already shown for the parents, namely a strong negative correlation between birth order and years of schooling. We also control for family size (number of children in the house) and we see that having one more child leads to a disadvantage in terms of schooling of about 0.15 years. In the second column we report the estimates when using the CSL instrument solely. One extra year spent in compulsory education for the parent, implies an increase of 0.083 years of schooling for the children. The coefficient is significant at the 10% level albeit weaker when compared to the results from the first column. Finally, in the third column we include both instruments. The estimated coefficients for the first born indicator and CSL are respectively 0.275 and 0.081, a signal that the first born indicator has a stronger predictive power on children's schooling compared to CSL.

Figure 6 summarizes the findings discussed so far for the first born instrument. The left panel shows the results from the first stage, that is the effect of birth order on parental schooling, while controlling for parental sibship size. Overall, a first born parent has approximately 0.5 more years of education than a non-first born parent. This advantage remains for any level of family size. The right panel shows the results from the reduced form specification. Children of first born parents have approximately 0.25 more years of education than children of non-first born parents: this advantage is again significant and stable at any level of family size. An interesting result emerges from this table. Not only birth order exerts a strong influence on someone's schooling but it has predictive power for the educational attainment of the next generation. In the last part of the paper we run a series of robustness checks that tend to shed light on this pattern.

5.4 2SLS estimates

The IV estimates using different instruments are reported in Table 7. We restrict the analysis to the CSL sample and use three specifications (which differ by the combinations of instruments used) as before in order to compare the IV estimates with the first stage and reduced form estimates. At a later stage of the analysis we show the results using the full sample where the only instrument available is the first born indicator.

Table 7 documents a positive causal effect of parents' schooling on children's schooling independently from the instrument chosen. When using only first born (column 1), one-year increase in parent schooling leads to an increase in children's schooling of 0.6 of a year (around 7 months). Being the first to use parental birth order as an instrument in the intergenerational mobility literature, we cannot compare this result with existing studies. Moreover, most of the related papers focus on different outcomes such as the probability of attending post-secondary education or the probability to repeat a grade rather than years of schooling. Lack of comparability with previous studies motivated the decision to enrich the instrument set using CSL, at the cost of reducing the sample size (losing more than 40% of the observations). Thus, in the second column we estimate the same model replacing the first born indicator with CSL. We find a positive causal effect of parent schooling of 0.4 years (about 5 months); this effect is significant at the 10% level. Such result is in line with what has been found in the literature so far. Comparing the first and second column of Table 7 we notice that the IV estimates are larger in magnitude when using first born compared to CSL and both of them are larger than OLS estimates. Although these results do not meet the *a priori* beliefs about the source of endogeneity, this evidence is quite common in the empirical applications. One possible explanation is that the IV corrects both for classical measurement error (which biases downwards the OLS estimates) and for the endogeneity of the dependent variable, and that the first effect prevails on the second one. Another explanation (in the context of IGM literature see discussion in Oreopoulos et al. 2006) suggests that IV can be larger than OLS because the IV estimates approximate the average effect for a small group of the population (the one targeted by the instrument), whereas the OLS estimates provide an average effect among everyone. From Table 5 we saw that an instrument such as CSL hits substantially parents raised in a family of poor socio-economic background, that in absence of the reform would have dropped out of school. The first born instrument on the other hand seems to affect a different subpopulation of parents (not necessarily the poorest ones), and has a stronger first stage compared to CSL (F-statistics is about 20.8 for first born and 7.2 for CSL).

It is interesting from a methodological point of view to combine two different instruments (first born shows a stronger first stage whilst CSL receives stronger support regarding the exogeneity assumption), that have bite in different parts of the distribution, to estimate a policy relevant parameter such as the intergenerational effect of education. In the third column we use the two instruments together, which does not jeopardize their predictive power: the combined F statistic is 14.5 so the instruments are relevant. The causal effect in this over-identified model is 0.538 years of education: the effect is large in magnitude and significant at 1% level. This estimate is closer to the one found when using first born instrument alone (0.605): this reflects the fact that the first stage when using first born is stronger than the first stage when using CSL reforms. All control variable coefficients have the expected sign and change very little across different specifications. From the third column we see that parental family size is irrelevant to children education; being a second born child, third, fourth and more than fifth reduces education by respectively 0.240, 0.425, 0.574 and 1.2 years; an extra child in the family reduces education of 0.1 years.

To our knowledge, this is the first paper to achieve overidentification in the intergenerational mobility context and run an overidentifying test of restrictions.[24] Columns 3 report the statistic and the p-value of the *J*-test of over-identifying restrictions: the null hypothesis that the instruments are correctly excluded from the main equation is not rejected. Assuming the exogeneity of CSL, such result - coupled with the robustness checks in Section 5.5 - might be interpreted as evidence in favour of the first born exogeneity.

Where does the difference between the two IV estimates (with first born delivering higher estimates than CSL) come from? Within a homogeneous treatment effect framework it just reflects sample variability (if both instruments are valid), whereas if we allow the treatment effect to vary across individuals it may be the case that the two instruments target two different sub-populations and thus deliver two different IV estimates. In this section we interpret our estimates as Local Average Treatment Effect (LATE), i.e. the effect on children education for those parents who changed their education in response to one or the other instrument (so called *compliers*).[25]

One further question needs to be explored further: why the two IV estimates are different (although not as much as to reject the *J*-test) and why they are larger than

[24] The model is estimated via 2SLS, so the test coincides with the Sargan test.

[25] Imbens and Angrist (1994), Angrist and Pischke (2008).

OLS. This question can be addressed within a heterogeneous treatment effect model. The fact that two different instruments that seem to target two qualitatively different sub-populations (Table 5) deliver similar causal effects [26] can be interpreted as evidence in favor of a homogeneous treatment effect model. This result puts the overidentifying test of restrictions at service of the external validity.[27]

As explained throughout the analysis, using the two instruments together implies a substantial reduction in the sample size which could affect the results. We now report all the estimates (first stage, reduced form and 2SLS), when using as instrument only first born (sample size consists of about 30500 children). Table 8 summarizes the results. From the first column we see that the estimates from the first stage are comparable with what we showed in Table 4. Being a first born parent can guarantee an increase in schooling in the range of 6 months (0.429 years). What seems to be different are the results from the reduced form and first stage (column 2 and 3). As for the reduced form regression we notice a decrease in the coefficient of first born indicator from 0.277 in Table 6 to 0.180. Results are strong and statistically different from 0 at the 1% level. The IV estimate is equal to 0.419, still higher than OLS but significantly lower compared to the one showed in Table 7 (about 0.6 years of schooling). Interestingly, this coefficient is very close to the one estimated when using CSL as an instrument for parental schooling (about 0.387). Such result indirectly provides support for the test of the overidentifying restrictions (the difference in the magnitude between the two instruments could be just a result of sample variability).

5.5 Robustness checks

We now propose a series of robustness and sensitivity checks that aim to complete the main analysis addressing issues related to the validity of the instruments and sample specification.

Gender differences. We now report separate regressions for the responding parent being either the mother or the father making a clear distinction in the transmission of education. Table 9 and Table 10 show the results when using the two instruments together (CSL sample) or when using only first born (full sample). In both cases we ob-

[26] At least in the sense of the overidentifying test of restrictions.

[27] Naturally this conclusion must be taken with care, as in principal other valid instruments may affect other sub-populations with different causal effects. See Imbens and Angrist (1994) and Angrist and Pischke (2008) for further details.

serve a strong and positive causal effect of parental education on children's education for the mother and the father, even though the results differ in terms of magnitude. When combining the two instruments (Table 9), an extra year spent in school for the mother translates in about 8 months of schooling for the children (0.729 years). As for the father, we find an effect of about 4 months of schooling for every year of education (0.3 years). These results are coherent with what has been found in the literature so far: mother's education seems to matter most (Holmlund et al. 2011). In addition, we do get strong IV estimates also for fathers' education.

We get similar insights from Table 10, except for the fact that now IV estimates are slightly smaller in magnitude (about 0.672 years for the mother and 0.25 years for the father).

Birth order and inheritance. A fundamental assumption behind our empirical strategy is that the first born instrument satisfies the exclusion restriction. We discuss some indirect tests that aim to rule out possible channels that could violate the exclusion restriction. A first thing that comes in mind is that first born parents could be more likely to receive inheritances from their parents: if this is the case, it could also have an effect on children's education, thus violating the assumption.

Instrument exogeneity is intrinsically untestable, but one could provide some evidence on the effect of birth order on the probability of receiving inheritances. In Table 6 we show estimates from a linear probability model where the outcome variable is a binary indicator which takes value 1 i the parent has ever received a house as bequest (columns 1-2) or have bought/built it with some help from the family (columns 3-4). For each outcome we use two specifications: the full sample in columns 1 and 3 and CSL sample in columns 2 and 4. We notice that being a first born parent seems not to have a significant effect on the probability of inheriting a house or building it with some help from the family.

Birth order and risk preferences. Another potential threat to the exclusion restriction could be related to risk taking behavior. A common discussion in the sociological literature is that later born children could make more risky choices compared to first born children, to make-up for the disadvantage of being a later born. Although it is not possible to fully test such assumption we provide some evidence on the relationship between parental birth order and risk aversion. We use a subjective measure of risk aversion based on the following question asked to parents[28]: "Which of the following statements on the card comes closest to the amount of financial risk that you are willing to take when you save or make investments? i) Take substantial financial risks expecting to earn substantial returns; ii) Take above average financial risks expecting to earn above average returns; iii) Take average financial risks expecting to earn average returns; iv) Not willing to take any financial risks. We create a binary indicator that takes value one if the parent is willing to take either substantial financial risks or above average financial risks and 0 otherwise. Results are reported in Table 6. In the first two columns we show the estimates when considering a first born dummy, whereas in the last two columns we include dummies for being a middle or last born parent (reference category being a first born). Interestingly, conditional on education and other characteristics, birth order seems not to have an effect on risk preferences. This result seems to support the exclusion restriction: even if parental risk preferences were to have an effect on children education it is very likely that such affect passes through the education of the parents.

IV estimates separately by sibship size. Another plausible concern is that first born results may be driven by a particular type of family. We know for instance that birth order and family size are correlated, so controlling for sibship size may not be sufficient. In Table 13 we show the IV estimates considering all parents that have at least one sibling (the reference category being sibship size = 2). We see that the IV estimates of parental education are consistent: the only non significant estimate arises for a family size equal to 3 (that is the parent plus two siblings).

Are first borns different? Due to data limitations we cannot use the complete birth order of parents and clearly distinguish a first born from a second born, third born and so on. If we expect first borns to be special compared to later borns (coherent with the idea of a role model), then we should also expect that second borns, third borns, etc. should have similar education. This hypothesis cannot be tested using data for parents but but we fill this gap using data for children, as we know precisely their birth order and sibship size. In Figure 7 we show the residuals from a regression of children's education on country fixed effects, birth year fixed effects and the age of the parent (mother or father) at birth, separately by birth order and sibship size. The descriptive

[28]This question is asked to the parent (SHARE respondent) who is the financial respondent in the household. If both parents are present in the household they may decide to answer questions about their finances separately or choose one representative of the couple).

evidence rejects the hypothesis that first borns are special. It is clear from the figure that educational attainment decreases monotonically with birth order, keeping fixed sibship size.

Model specification. We conclude this session with two sensitivity checks on our empirical model. First, we re-run the 2SLS regressions excluding only-child parents (then the reference category for sibship size would be parents having one sibling). This choice does not seem to affect our results (Table 14). The coefficients are statistically significant and comparable in terms of magnitude with those shown in Table 7.

Second, since we estimated a model with country fixed effects, it could be that results are driven by a specific country in the sample. We repeat our IV regression with both instruments leaving one country out at each time. Results[29] look very robust: estimates for parental education range from 0.481*** years (leaving Denmark out) to 0.613 *** (leaving Netherlands out). All in all, we are confident that our estimates are robust to the specification used and to the sample choice.

6 Conclusions

It is well known that parental and children's schooling achievements are correlated, but there is no consensus whether association describes a genuine causal effect or merely reflects the fact that more able parents tend to raise more able children. This *nurture versus nature* debate has important public policy implications: policies aimed at increasing the schooling level of the most disadvantaged individuals will have very different long-term effects depending on whether environmental factors (nurture) or unobserved characteristics (nature) prevail.

In this paper we introduce parental birth order as an instrument for parental education in the IGM literature. The main advantage of this strategy is that it does not require external or exogenous shocks to identify the causal effect for the population of interest. Birth order has a strong predictive power for parental education and it is reasonably uncorrelated to unobserved children's innate ability. We apply this estimation strategy to a large and representative sample of old age parents taken from the Survey of Health, Ageing and Retirement in Europe (SHARE). We find that parental education has a positive and significant causal effect on children education: our base-

[29] Available upon request.

line estimates suggest an intergenerational causal effect of 0.429 years of education for the full sample and about 0.6 years for the CSL sample.

In the second part of the analysis we moved away from the just-identified model: we enrich the instrument set with information on Compulsory Schooling Laws (CSL) reforms that have been introduced in several European countries after World War II. These reforms increased the minimum schooling leaving age for the same cohorts, thus requiring parents to stay an extra year in school. However, because reforms have taken place in fewer countries, we had to reduce our sample when using CSL compared to first born. The 2SLS estimate using both instruments shows that the coefficient of intergenerational persistence amounts to 0.538 and it is significant at the 1% level; the J-test of over-identifying restriction never lead us to reject the null hypothesis that the instruments are correctly excluded from the equation of interest.

The evidence presented in this paper suggests that parental education causally affects children's education. These results are robust to different sample selections and to the choice of the instrument set (first born alone, CSL alone, both of them).

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Tables and Figures

Table 1: Compulsory schooling reforms in Europe.

Country	Reform year	Pivotal cohort	Change in CS
Austria	1962	1947	8 to 9
Belgium (Flanders)	1953	1939	8 to 9
Czech Republic	1948	1934	8 to 9
Denmark	1958	1947	4 to 7
France	1972	1953	8 to 10
Germany (Lender):			
Baden-Wurttemberg	1967	1953	8 to 9
Bayer	1969	1955	8 to 9
Bremen	1958	1943	8 to 9
Hamburg	1959	1934	8 to 9
Hessen	1967	1953	8 to 9
Nieder-Sachsen	1962	1947	8 to 9
Nordrhein-Westphalen	1967	1953	8 to 9
Rheinald-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	1950	1936	7 to 8
Sweden	1962	1950	8 to 9

Notes: This table reports information on the compulsory schooling laws used in the paper. Column 1 reports the year in which the reform was adopted in each country or region within a country. Column 2 reports the year of birth of the first cohort interested by the reform. Column 3 shows the change in mandatory schooling years (e.g from 8 years to 9 years, etc.). We refer to Brunello, Weber and Weiss 2012 and to Brunello, Fort, Weber 2009 for further details.

Table 2: Descriptive statistics for parents and children.

Variable	*Full sample* Mean	s.d.	*CSL sample* Mean	s.d.
Parents				
Years of education	10.65	4.10	11.32	3.85
Male	0.45	0.50	0.45	0.50
Age	63.56	7.50	62.32	6.10
First	0.39	0.49	0.40	0.49
Mid	0.34	0.47	0.34	0.47
Last	0.27	0.44	0.27	0.44
Siblings	3.57	1.98	3.59	2.06
Nr. of children	2.35	1.08	2.35	1.09
Mother alive	0.23	0.42	0.25	0.43
Father alive	0.08	0.27	0.09	0.29
Living with spouse or partner	0.74	0.44	0.76	0.43
Divorced	0.10	0.29	0.11	0.31
Widowed	0.15	0.36	0.12	0.33
Pivotal cohort			1945.10	6.54
Number of countries	13		9	
N	13950	5979		
Children				
Years of education	12.85	3.15	13.04	3.04
Male	0.51	0.50	0.51	0.50
Age	37.30	6.73	36.44	6.20
Biological	0.97	0.17	0.96	0.20
First born	0.44	0.50	0.47	0.50
Second born	0.35	0.48	0.35	0.48
Third born	0.14	0.35	0.13	0.33
Fourth born	0.05	0.22	0.04	0.20
Fifth or more	0.02	0.15	0.02	0.13
Never married	0.30	0.46	0.31	0.46
Living with spouse	0.62	0.48	0.61	0.49
Divorced	0.06	0.24	0.06	0.24
Widowed	0.01	0.07	0.00	0.06
N	31684		12848	

Notes: This table reports descriptive statistics (Mean and s.d.) for parents (top panel) and children (bottom panel), by sample type. Full sample refers to our working sample, where information on parental birth order is complete. CSL sample is the sample we obtain by restricting the analysis to countries that implemented a sharp schooling reform.

Table 3: OLS estimates of the effect of parental years of education on children's years of education.

	Full sample	CSL sample
Parent education	0.247***	0.247***
	(0.008)	(0.013)
Mother-daughter	0.512***	0.570***
	(0.056)	(0.089)
Father-daughter	0.234***	0.296***
	(0.052)	(0.080)
Mother-son	0.311***	0.387***
	(0.055)	(0.084)
Parent sibship size=2	0.053	0.033
	(0.072)	(0.107)
Parent sibship size=3	-0.024	-0.033
	(0.075)	(0.115)
Parent sibship size≥4	-0.098	-0.139
	(0.072)	(0.111)
Child birth order=2	-0.316***	-0.377***
	(0.036)	(0.054)
Child birth order=3	-0.503***	-0.686***
	(0.058)	(0.088)
Child birth order=4	-0.822***	-0.992***
	(0.089)	(0.148)
Child birth order≥5	-1.350***	-1.707***
	(0.162)	(0.280)
Nr. of children	-0.188***	-0.129***
	(0.022)	(0.037)
N	30606	12260
R2-adj	.206	.184

Notes: Column 1 reports OLS estimates for the full sample and Column 2 for the CSL sample. Full sample refers to our working sample, where information on parental birth order is complete. CSL sample is the sample we obtain by restricting the analysis to countries that implemented a sharp schooling reform. All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: First stage estimates of parental education using only FB, CSL or both instruments.

	(1) FB	(2) CSL	(3) Both
FB parent	0.457***		0.453***
	(0.100)		(0.100)
CSL		0.216***	0.211***
		(0.080)	(0.080)
Mother-daughter	-0.364***	-0.382***	-0.370***
	(0.132)	(0.133)	(0.132)
Father-daughter	0.066	0.063	0.060
	(0.107)	(0.108)	(0.107)
Mother-son	-0.396***	-0.416***	-0.406***
	(0.128)	(0.130)	(0.129)
Parent sibship size=2	0.136	-0.092	0.138
	(0.185)	(0.181)	(0.185)
Parent sibship size=3	0.080	-0.219	0.082
	(0.199)	(0.187)	(0.198)
Parent sibship size$\geq$ 4	-0.782***	-1.127***	-0.777***
	(0.203)	(0.191)	(0.202)
Child birth order=2	-0.474***	-0.474***	-0.473***
	(0.053)	(0.053)	(0.053)
Child birth order=3	-0.912***	-0.910***	-0.910***
	(0.102)	(0.103)	(0.103)
Child birth order=4	-1.442***	-1.442***	-1.444***
	(0.167)	(0.168)	(0.167)
Child birth order$\geq$5	-1.495***	-1.484***	-1.484***
	(0.322)	(0.325)	(0.322)
Nr. of children	-0.109**	-0.107*	-0.108*
	(0.055)	(0.055)	(0.055)
N	12166	12166	12166

Notes: Columns 1-3 report first stage estimates when the set of instruments is given by FB, CSL or both. All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Characterization of the compliers.

Exogenous variable (X)	N	$Pr(X = 1)$	$Pr(X = 1 \mid \text{compliers})$	$\frac{Pr(X=1 \mid \text{compliers})}{Pr(X=1)}$
			IV = First born	
Male	5929	0.445	0.488	1.096
Few books	4257	0.597	0.417	0.698
Large household	4266	0.387	0.328	0.847
Rooms per capita (low)	4248	0.464	0.654	1.410
Breadwinner low SES	4051	0.497	0.478	0.961
Rural area	4290	0.439	0.414	0.944
			IV = CSL	
Male	5966	0.446	0.559	1.253
Few books	4270	0.597	0.621	1.040
Large household	4278	0.388	0.408	1.054
Rooms per capita (low)	4260	0.464	0.562	1.212
Breadwinner low SES	4062	0.497	0.664	1.336
Rural area	4303	0.438	0.562	1.282

Notes: The treatment variable is given by an indicator that takes value 1 if parental education is higher than 10 years and 0 otherwise.

Table 6: Reduced form estimates of the effect of the instruments on children's years of education (only FB, CSL or both).

	(1) FB	(2) CSL	(3) Both
FB parent	0.277 ***		0.275 ***
	(0.079)		(0.079)
CSL		0.083 *	0.081
		(0.050)	(0.050)
Mother-daughter	0.488 ***	0.478 ***	0.486 ***
	(0.094)	(0.094)	(0.094)
Father-daughter	0.303 ***	0.303 ***	0.301 ***
	(0.082)	(0.082)	(0.082)
Mother-son	0.291 ***	0.281 ***	0.287 ***
	(0.090)	(0.089)	(0.089)
Parent sibship size=2	0.151	0.012	0.152
	(0.125)	(0.119)	(0.125)
Parent sibship size=3	0.087	-0.095	0.088
	(0.132)	(0.123)	(0.132)
Parent sibship size≥4	-0.216 *	-0.428 ***	-0.215 *
	(0.130)	(0.120)	(0.129)
Child birth order=2	-0.495 ***	-0.495 ***	-0.495 ***
	(0.057)	(0.057)	(0.057)
Child birth order=3	-0.916 ***	-0.915 ***	-0.915 ***
	(0.092)	(0.092)	(0.092)
Child birth order=4	-1.350 ***	-1.350 ***	-1.351 ***
	(0.160)	(0.161)	(0.160)
Child birth order≥5	-2.062 ***	-2.058 ***	-2.058 ***
	(0.315)	(0.317)	(0.315)
Nr of children	-0.152 ***	-0.152 ***	-0.152 ***
	(0.038)	(0.038)	(0.038)
N	12166	12166	12166

Notes: Columns 1-3 report reduced form estimates (the effect of the instrument on children's years of education), when the set of instruments is given by FB parent (column 1), CSL (column 2) and the two instruments together (column 3). All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: IV/2SLS estimates of the effect of parental education on children's education using only FB, CSL or both instruments.

	(1) FB	(2) CSL	(3) Both
Parent education	0.605***	0.387*	0.538***
	(0.184)	(0.213)	(0.144)
Mother-daughter	0.708***	0.626***	0.683***
	(0.119)	(0.128)	(0.112)
Father-daughter	0.263***	0.278***	0.268***
	(0.094)	(0.083)	(0.089)
Mother-son	0.531***	0.442***	0.504***
	(0.123)	(0.131)	(0.112)
Parent sibship size=2	0.069	0.048	0.063
	(0.124)	(0.110)	(0.118)
Parent sibship size=3	0.039	-0.010	0.024
	(0.143)	(0.127)	(0.133)
Parent sibship size≥4	0.257	0.008	0.180
	(0.241)	(0.262)	(0.196)
Child birth order=2	-0.208*	-0.312***	-0.240**
	(0.107)	(0.118)	(0.093)
Child birth order=3	-0.364*	-0.563***	-0.425***
	(0.196)	(0.210)	(0.162)
Child birth order=4	-0.477	-0.792**	-0.574**
	(0.322)	(0.348)	(0.274)
Child birth order≥5	-1.157***	-1.484***	-1.257***
	(0.398)	(0.400)	(0.348)
Nr of children	-0.086*	-0.110**	-0.094**
	(0.046)	(0.046)	(0.044)
N	12166	12166	12166
F-excluded	20.8	7.2	12.7
J-test			.8202
p-val			.3651

Notes: Columns 1-3 report show 2SLS estimates using as instruments: FB parent (column 1), CSL (column 2) and the two instruments together (column 3). All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: First stage, reduced form and IV/2SLS estimates when using only FB.

	(1) FS	(2) RF	(3) IV
FB parent	0.429 ***	0.180 ***	
	(0.072)	(0.049)	
Parent education			0.395 ***
			(0.106)
Mother-daughter	-0.628 ***	0.356 ***	
	(0.078)	(0.058)	
Father-daughter	-0.006	0.231 ***	
	(0.065)	(0.052)	
Mother-son	-0.685 ***	0.140 **	
	(0.077)	(0.057)	
Parent sibship size=2	0.008	0.094	0.082
	(0.124)	(0.085)	(0.081)
Parent sibship size=3	-0.318 **	-0.056	0.066
	(0.130)	(0.088)	(0.102)
Parent sibship size≥4	-1.014 ***	-0.291 ***	0.103
	(0.127)	(0.088)	(0.162)
Child birth order=2	-0.392 ***	-0.414 ***	-0.214 ***
	(0.031)	(0.035)	(0.060)
Child birth order=3	-0.823 ***	-0.703 ***	-0.280 **
	(0.064)	(0.061)	(0.119)
Child birth order=4	-1.231 ***	-1.117 ***	-0.488 ***
	(0.104)	(0.096)	(0.181)
Child birth order≥5	-1.441 ***	-1.714 ***	-0.930 ***
	(0.186)	(0.166)	(0.247)
Nr of children	-0.137 ***	-0.220 ***	-0.185 ***
	(0.032)	(0.024)	(0.025)
Female child			0.212 ***
			(0.034)
Constant	9.751 ***	14.423 ***	8.507 ***
	(0.464)	(0.373)	(1.077)
N	30474	30474	30474

Notes: This table shows the first stage (column 1), reduced form (column 2) and 2SLS estimates (column 3) when we use parental first born (FB) as the only instrument. All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: First stage, reduced form and IV/2SLS estimates for mother and father using both instruments.

	Mother			(Father)		
	(1)	(2)	(3)	(4)	(5)	(6)
	FS	RF	IV	FS	RF	IV
FB parent	0.252 *	0.360 ***		0.665 ***	0.161	
	(0.138)	(0.096)		(0.172)	(0.118)	
CSL	0.196 **	0.033		0.225 *	0.136 *	
	(0.097)	(0.068)		(0.116)	(0.076)	
Parent education			0.725 **			0.310 **
			(0.289)			(0.149)
Parent sibship size=2	0.021	0.406 **	0.297	0.308	-0.210	-0.286
	(0.247)	(0.170)	(0.184)	(0.302)	(0.190)	(0.176)
Parent sibship size=3	-0.082	0.252	0.189	0.340	-0.137	-0.218
	(0.267)	(0.171)	(0.202)	(0.337)	(0.201)	(0.178)
Parent sibship size≥4	-0.913 ***	0.034	0.556	-0.526 *	-0.539 ***	-0.348
	(0.265)	(0.165)	(0.350)	(0.317)	(0.189)	(0.239)
Child birth order=2	-0.580 ***	-0.575 ***	-0.157	-0.331 ***	-0.380 ***	-0.277 ***
	(0.062)	(0.071)	(0.182)	(0.084)	(0.087)	(0.101)
Child birth order=3	-1.188 ***	-1.129 ***	-0.271	-0.567 ***	-0.606 ***	-0.433 **
	(0.132)	(0.119)	(0.353)	(0.158)	(0.157)	(0.171)
Child birth order=4	-1.548 ***	-1.372 ***	-0.257	-1.312 ***	-1.361 ***	-0.958 ***
	(0.212)	(0.184)	(0.487)	(0.282)	(0.293)	(0.355)
Child birth order≥5	-1.697 ***	-2.062 ***	-0.829	-1.045 **	-2.003 ***	-1.683 ***
	(0.398)	(0.406)	(0.603)	(0.465)	(0.424)	(0.401)
Nr of children	-0.087	-0.123 **	-0.058	-0.108	-0.182 ***	-0.149 ***
	(0.075)	(0.048)	(0.070)	(0.074)	(0.056)	(0.053)
Female-child	0.024	0.200 ***	0.184 **	0.047	0.306 ***	0.293 ***
	(0.080)	(0.073)	(0.092)	(0.107)	(0.082)	(0.081)
Constant	9.740 ***	15.390 ***	5.317 **	7.043 ***	14.470 ***	8.793 ***
	(0.725)	(0.460)	(2.660)	(0.711)	(0.528)	(1.352)
N	6904	6904	6904	5262	5262	5262

Notes: This table shows the first stage (column 1), reduced form (column 2) and 2SLS estimates (column 3) when using both instruments. Estimates are reported separately for mother (columns 1-3) and father (columns 4-6). All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: First stage, reduced form and IV/2SLS estimates for mother and father when using only FB.

	Mother			Father		
	(1)	(2)	(3)	(4)	(5)	(6)
	FS	RF	IV	FS	RF	IV
FB parent	0.300 ***	0.201 ***		0.577 ***	0.140 *	
	(0.090)	(0.065)		(0.114)	(0.075)	
Parent education			0.672 ***			0.242 **
			(0.238)			(0.121)
Female-child	0.057	0.215 ***	0.177 ***	-0.010	0.232 ***	0.234 ***
	(0.050)	(0.046)	(0.050)	(0.065)	(0.052)	(0.049)
Parent sibship size=2	0.027	0.206 *	0.188	0.017	-0.038	-0.042
	(0.158)	(0.115)	(0.122)	(0.196)	(0.126)	(0.117)
Parent sibship size=3	-0.313 *	0.012	0.222	-0.286	-0.136	-0.067
	(0.163)	(0.118)	(0.171)	(0.208)	(0.132)	(0.141)
Parent sibship size≥4	-0.986 ***	-0.214 *	0.449	-1.001 ***	-0.380 ***	-0.138
	(0.160)	(0.119)	(0.310)	(0.203)	(0.131)	(0.206)
Child birth order=2	-0.433 ***	-0.465 ***	-0.173	-0.347 ***	-0.347 ***	-0.263 ***
	(0.039)	(0.047)	(0.114)	(0.049)	(0.052)	(0.065)
Child birth order=3	-0.905 ***	-0.756 ***	-0.148	-0.707 ***	-0.626 ***	-0.455 ***
	(0.081)	(0.083)	(0.231)	(0.103)	(0.089)	(0.117)
Child birth order=4	-1.338 ***	-1.180 ***	-0.280	-1.022 ***	-1.009 ***	-0.761 ***
	(0.132)	(0.131)	(0.345)	(0.166)	(0.145)	(0.183)
Child birth order≥5	-1.409 ***	-1.743 ***	-0.796 **	-1.303 ***	-1.640 ***	-1.324 ***
	(0.238)	(0.224)	(0.405)	(0.299)	(0.244)	(0.280)
Nr of children	-0.155 ***	-0.228 ***	-0.124 **	-0.113 **	-0.215 ***	-0.187 ***
	(0.041)	(0.033)	(0.051)	(0.049)	(0.033)	(0.033)
Constant	10.477 ***	14.237 ***	5.825 **	7.884 ***	13.196 ***	9.731 ***
	(2.285)	(1.462)	(2.380)	(0.801)	(0.709)	(1.262)
N	17083	17083	17083	13391	13391	13391
R2-adj	.341	.141	.0364	.279	.124	.201

Notes: This table shows the first stage (column 1), reduced form (column 2) and 2SLS estimates (column 3) when using FB only. Estimates are reported separately for mother (columns 1-3) and father (columns 4-6). All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered on country and parent cohort, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Birth order and the probability of receiving a house as bequest or buying it with some help from family.

	(1) Bequest	(2) Bequest	(3) Family help	(4) Family help
FB parent	-0.004	-0.009	-0.005	-0.003
	(0.008)	(0.010)	(0.008)	(0.012)
Parent sibship size=2	-0.023	-0.034	-0.009	-0.011
	(0.017)	(0.024)	(0.016)	(0.025)
Parent sibship size=3	-0.039**	-0.053**	-0.027*	-0.061**
	(0.017)	(0.023)	(0.016)	(0.025)
Parent sibship size=4	-0.047***	-0.054**	-0.039**	-0.059**
	(0.017)	(0.023)	(0.016)	(0.025)
Female	0.026***	0.005	0.019***	0.013
	(0.008)	(0.010)	(0.007)	(0.011)
Constant	0.285	0.094*	0.246	0.696*
	(0.180)	(0.052)	(0.178)	(0.367)
N	7057	3110	7057	3110
R2-adj	.0578	.0822	.0289	.0278

Notes: This table shows the OLS estimates of being first born on the probability of receiving a house as bequest or with some help from family (linear probability model). In columns 1 and 3 we use the full sample and in columns 2 and 4 the CSL sample. All specifications account for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered at the household level are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Birth order and parental risk preferences.

	Risk: substantial/above the average			
	(1)	(2)	(3)	(4)
FB parent	0.006	0.009		
	(0.005)	(0.009)		
Middle born parent			-0.007	-0.012
			(0.005)	(0.009)
Last born parent			0.000	0.001
			(0.007)	(0.013)
Years of education	0.005***	0.006***	0.005***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.027***	-0.033***	-0.027***	-0.034***
	(0.004)	(0.008)	(0.004)	(0.008)
Parent sibship size=2	0.010	0.033**	0.010	0.035**
	(0.008)	(0.014)	(0.008)	(0.014)
Parent sibship size=3	0.021**	0.028*	0.020**	0.026*
	(0.009)	(0.014)	(0.009)	(0.014)
Parent sibship size≥4	0.009	0.016	0.009	0.015
	(0.008)	(0.014)	(0.008)	(0.014)
Constant	-0.067**	-0.145***	0.023	-0.006
	(0.032)	(0.033)	(0.025)	(0.049)
N	12304	5169	12304	5169
R2-adj	.145	.182	.145	.183

Notes: This table shows the effect of being first born on risk taking behavior (willingness to take risks). In columns 1 and 3 we use the full sample and in columns 2 and 4 the CSL sample. All specifications control for country fixed effects, parent and child cohort fixed effect. Robust standard errors, clustered at the household level are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: IV/2SLS estimates by parental sibship size.

	(1) Sibship=2	(2) Sibship=3	(3) Sibship≥4
Parent education	0.431 **	0.326	0.447 **
	(0.191)	(0.206)	(0.180)
Mother-daughter	0.741 ***	0.453 ***	0.677 ***
	(0.153)	(0.165)	(0.144)
Father-daughter	0.255 ***	0.156	0.273 ***
	(0.098)	(0.103)	(0.079)
Mother-son	0.447 **	0.277	0.464 ***
	(0.174)	(0.180)	(0.137)
Child birth order= 2	-0.245 **	-0.260 ***	-0.243 ***
	(0.122)	(0.100)	(0.082)
Child birth order= 3	-0.328	-0.313	-0.376 **
	(0.231)	(0.192)	(0.163)
Child birth order=4	-0.532	-0.672 **	-0.595 ***
	(0.373)	(0.341)	(0.227)
Child birth order≥	-0.960 *	-1.147 **	-1.121 ***
	(0.536)	(0.513)	(0.297)
Nr of children	-0.208 ***	-0.195 ***	-0.132 **
	(0.043)	(0.046)	(0.051)
Constant	8.104 ***	9.170 ***	7.649 ***
	(1.919)	(2.200)	(1.600)
N	7093	7634	13210
R2-adj	.139	.193	.173
F-excluded	12.1	8.8	14.5

Notes: In each regression we control for country FE, parent's birth year FE, child's birth year FE. Standard errors clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: 2SLS estimates excluding parents who are only child.

	(1) FB	(2) CSL	(3) Both
Parent education	0.593***	0.469**	0.568***
	(0.179)	(0.230)	(0.149)
Parent sibship size= 3	-0.177	-0.052	-0.144
	(0.195)	(0.251)	(0.166)
Parent sibship size= 4	-0.218	-0.092	-0.184
	(0.182)	(0.223)	(0.155)
Mother-daughter	0.747***	0.701***	0.736***
	(0.115)	(0.132)	(0.113)
Father-daughter	0.257***	0.269***	0.255***
	(0.094)	(0.087)	(0.091)
Mother-son	0.527***	0.481***	0.517***
	(0.118)	(0.128)	(0.113)
Child birth order=2	-0.205*	-0.267**	-0.219**
	(0.107)	(0.131)	(0.098)
Child birth order=3	-0.291	-0.406*	-0.318*
	(0.203)	(0.243)	(0.176)
Child birth order=4	-0.411	-0.612	-0.457
	(0.339)	(0.406)	(0.300)
Child birth order≥5 or more	-0.969**	-1.227***	-1.021***
	(0.426)	(0.449)	(0.381)
Nr of children	-0.123***	-0.133***	-0.124***
	(0.045)	(0.046)	(0.045)
Constant	6.561***	7.555***	6.768***
	(1.431)	(1.855)	(1.218)
N	11107	11140	11081
R2-adj	.044	.125	.0637

Notes: In each regression we control for country FE, parent's birth year FE, child's birth year FE. Standard errors clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: IV/2SLS estimates, CSL sample (7 years before/after the pivotal cohort).

	(1)	(2)	(3)
	FB	CSL	Both
Parent education	0.501 **	0.570 *	0.526 ***
	(0.207)	(0.305)	(0.178)
Parent sibship size=2	0.120	0.120	0.116
	(0.140)	(0.142)	(0.139)
Parent sibship size=3	0.108	0.148	0.115
	(0.167)	(0.189)	(0.165)
Parent sibship size=4	0.240	0.323	0.259
	(0.292)	(0.397)	(0.254)
Mother-daughter	0.787 ***	0.810 ***	0.799 ***
	(0.131)	(0.174)	(0.128)
Father-daughter	0.337 ***	0.343 ***	0.333 ***
	(0.105)	(0.109)	(0.105)
Mother-son	0.529 ***	0.556 ***	0.540 ***
	(0.140)	(0.171)	(0.134)
Child birth order=2	-0.268 **	-0.235	-0.259 **
	(0.125)	(0.173)	(0.114)
Child birth order=3	-0.513 **	-0.444	-0.497 **
	(0.223)	(0.329)	(0.201)
Child birth order=4	-0.581	-0.490	-0.557 *
	(0.359)	(0.478)	(0.324)
Child birth order≥5 or more	-1.341 ***	-1.242 **	-1.318 ***
	(0.509)	(0.599)	(0.474)
Nr. of children	-0.112 **	-0.107 *	-0.108 **
	(0.053)	(0.063)	(0.053)
Constant	7.079 ***	6.441 **	6.862 ***
	(1.988)	(2.993)	(1.716)
N	9076	9108	9056
R2-adj	.1	.0502	.0849

Notes: In each regression we control for country FE, parent's birth year FE, child's birth year FE. Standard errors clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Sample selection: parents and children in SHARE.

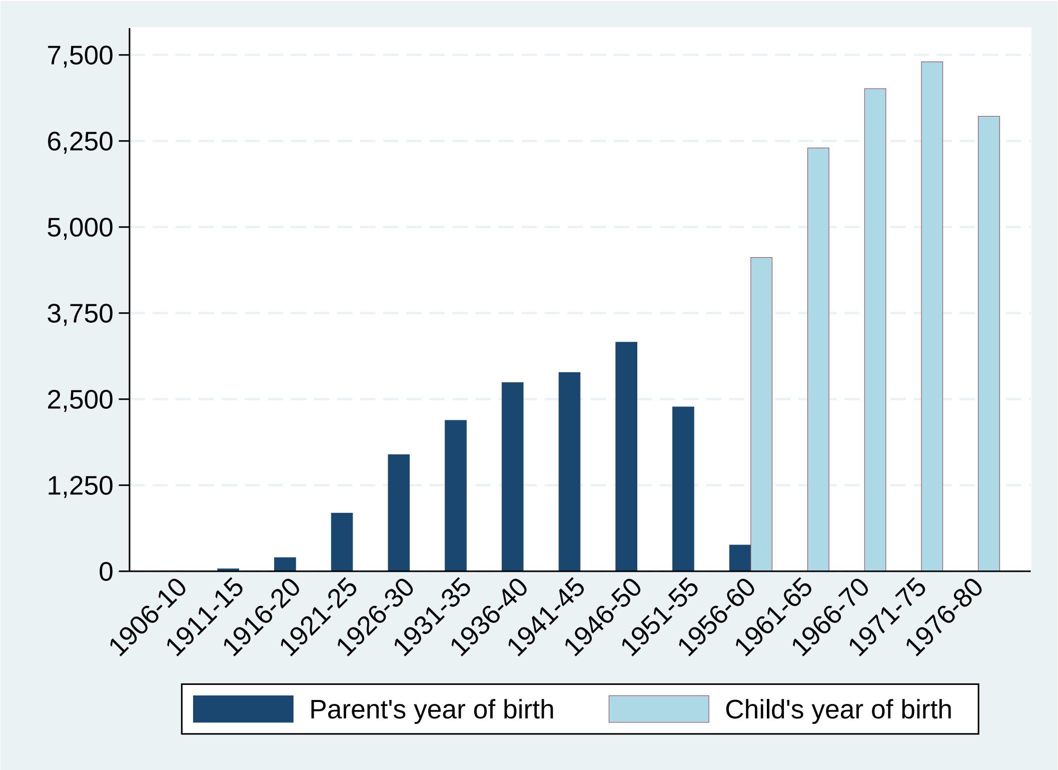


Figure 2: Bivariate association between parental and children years of education.

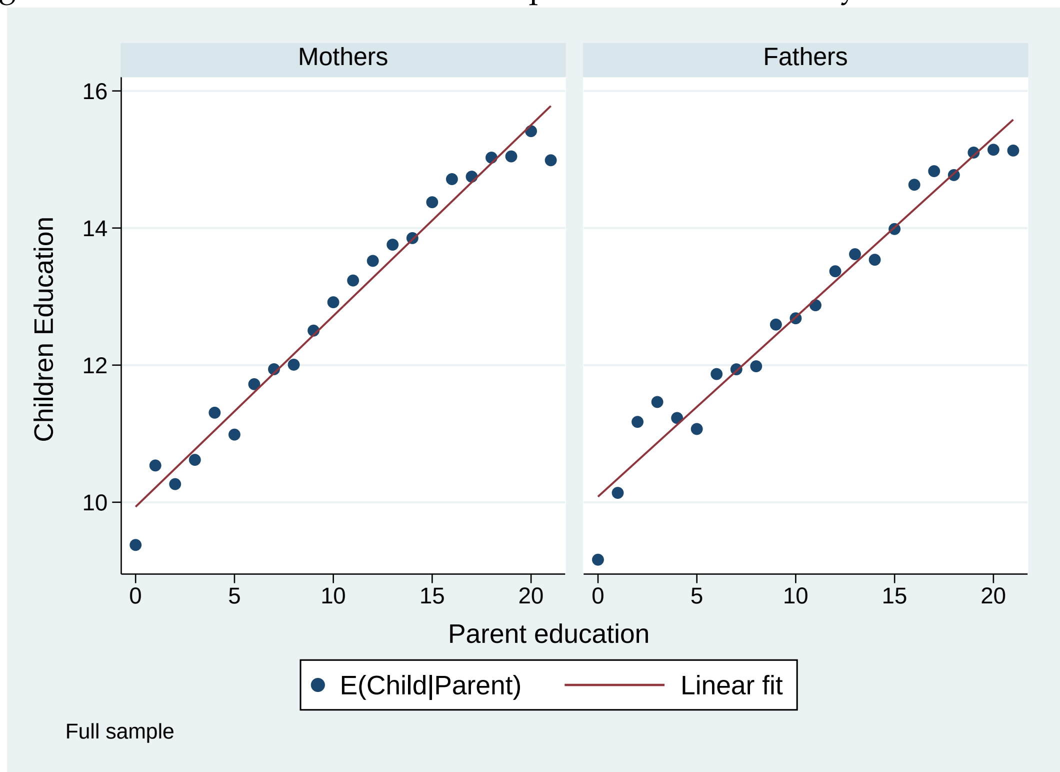
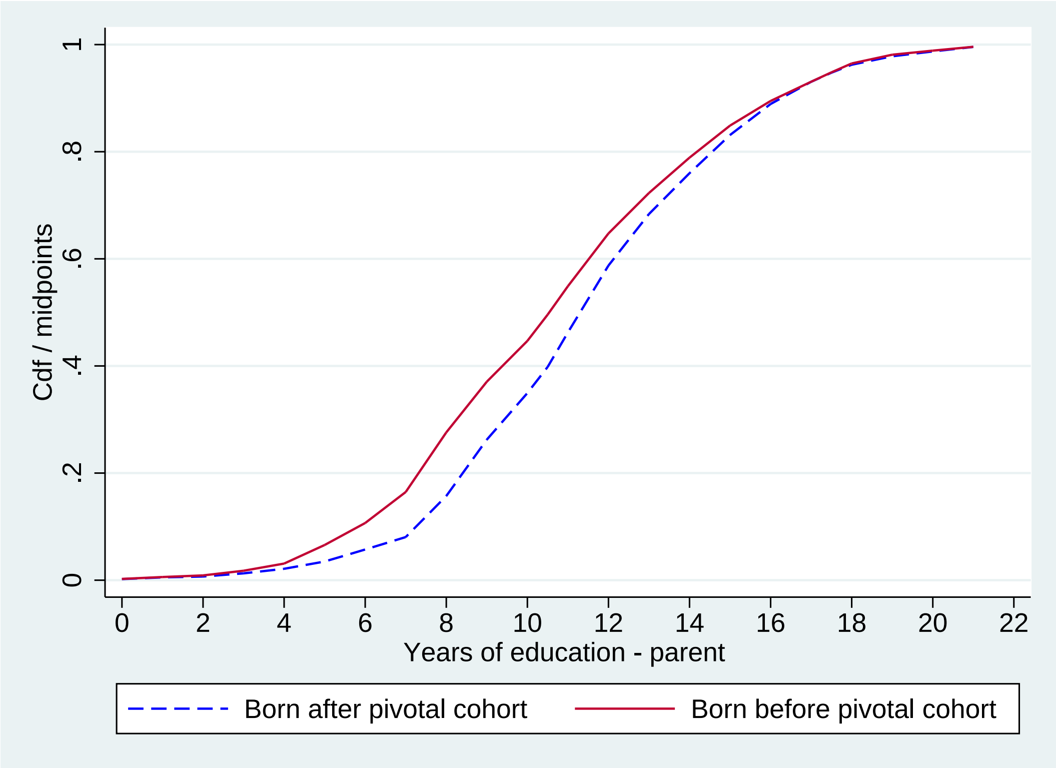
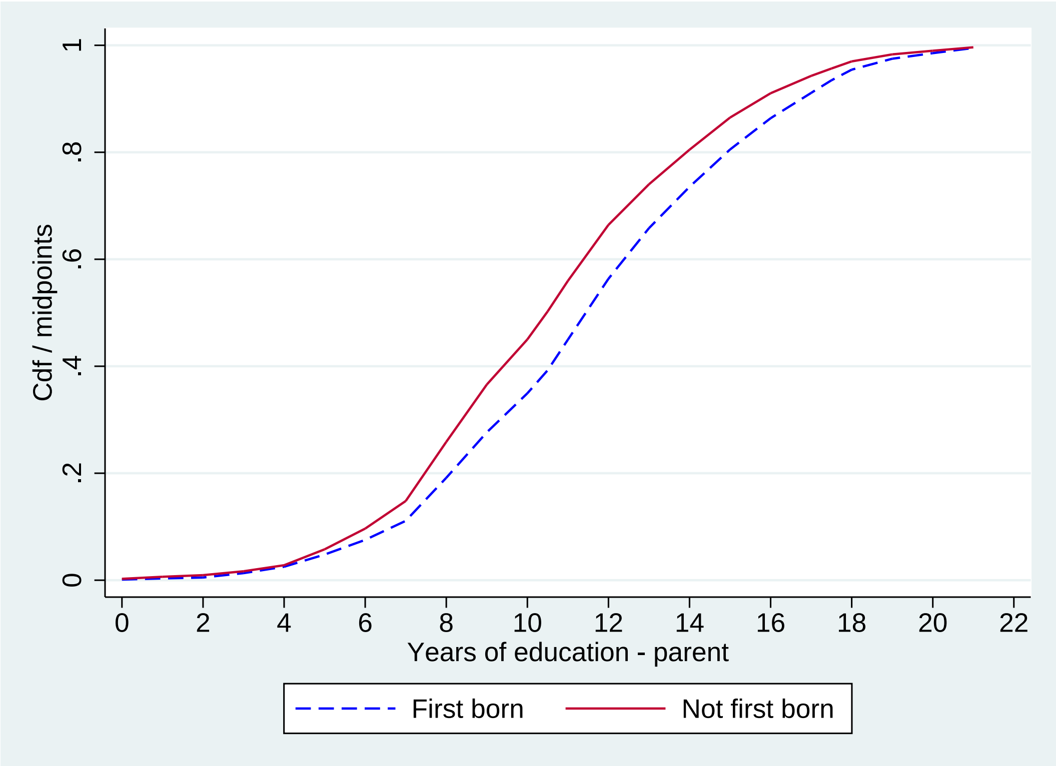


Figure 3: Distribution of parental years of education by CSL status (10 years before/after the pivotal cohort).



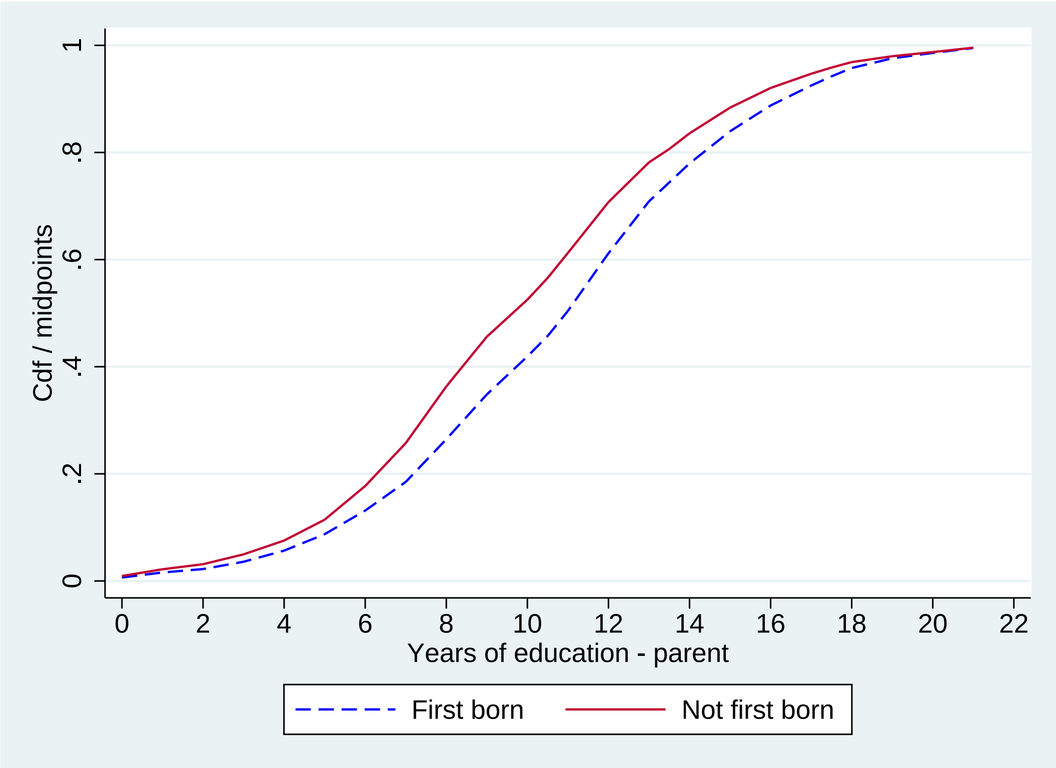
Notes: This figure reports the cumulative distribution function of parental years of education by their treatment status (born 10 years before or after the pivotal cohort). We use the restricted sample (CSL sample) and consider cohorts born up to 10 years before and 10 years after the pivotal cohort.

Figure 4: Distribution of parental years of education by First born status (10 years before/after the pivotal cohort).



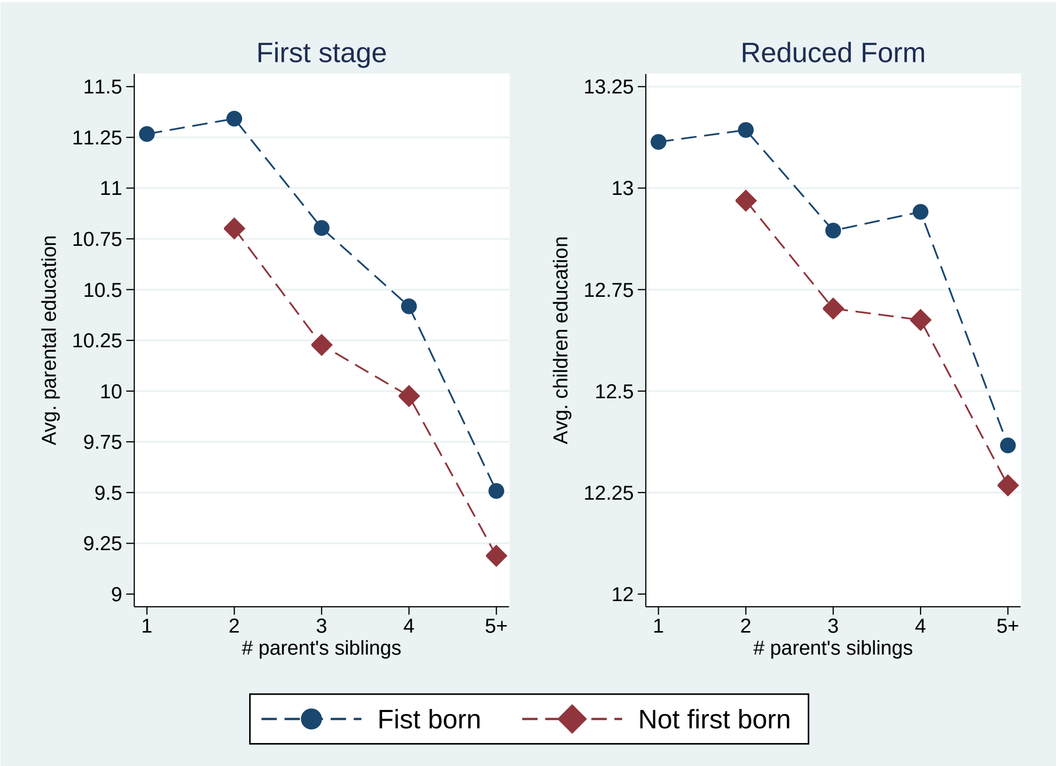
Notes: This figure reports the cumulative distribution function of parental years of education by first born status. We use the restricted sample (CSL sample) and include cohorts born up 10 years before and 10 years after the pivotal cohort.

Figure 5: Distribution of parents' years of education by First born status (Full sample).



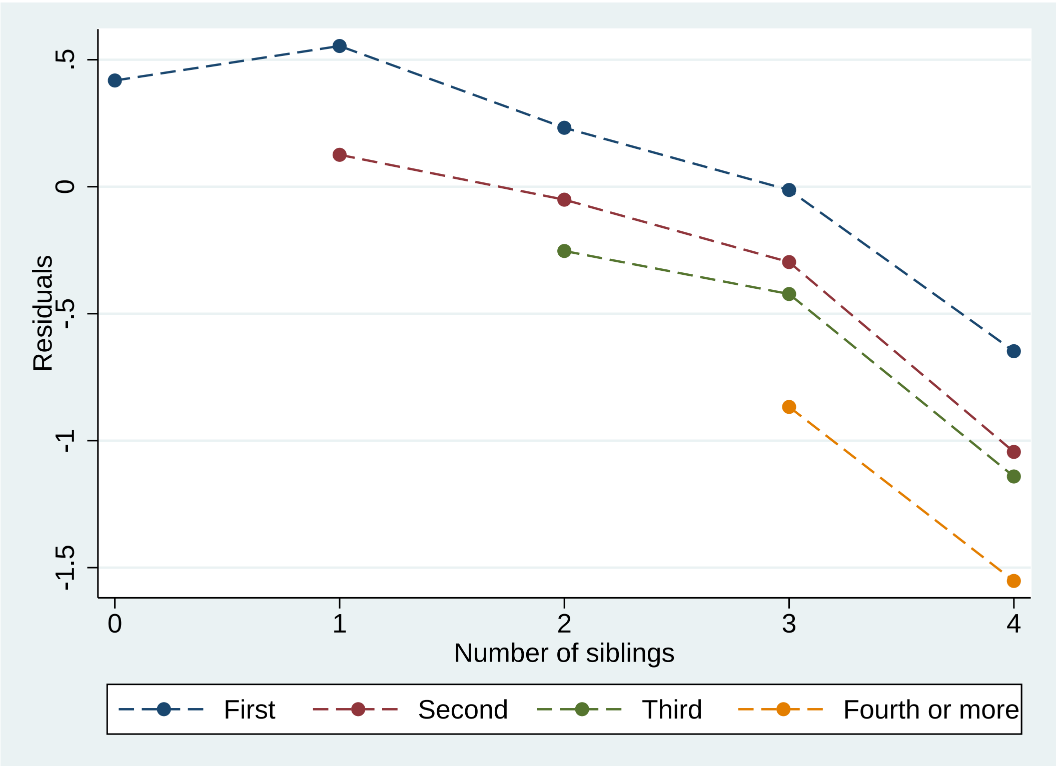
Notes: This figure reports the cumulative distribution function of parents' years of education by first born status. We use the full sample.

Figure 6: First born instrument: first stage and reduced form estimates.



Notes: This figure reports results from first stage and reduced form for first born instrument. The left panel shows the average years of education of parents by their first born status and sibship size. The right panel shows the average years of education of children by parents' first born status and sibship size.

Figure 7: Children education by birth order and sibship size.



Notes: We report the residuals from a regression of children's years of education on country fixed effects, cohort fixed effects and parent's age at birth, separately by birth order of the child (first born, second born, third born, fourth born or more) and number of siblings of children's generation (0 siblings, 1 sibling, 2 siblings, 3 siblings, 4 siblings or more).