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The causal effect of parents' schooling on children's schooling in Europe. A new IV approach.

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The causal effect of parents' schooling on children's schooling in Europe. A new IV approach.

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Abstract

This paper estimates the causal effect of parental education on children's education in 13 European countries, using representative data from the Survey of Health, Ageing and Retirement in Europe (SHARE). A novel instrumental variable approach is used to solve the endogeneity issue. We combine two instruments: parental birth order (indicator for being a first born) and Compulsory Schooling Laws (CSL). While CSL have been widely used in applied work, our contribution is to introduce parental birth order as instrument in the intergenerational mobility literature. We find that parental education has a positive, large and significant causal effect on children's education. This finding is robust to the instrument chosen (birth order, CSL, or both), to sample selection and to several robustness checks.

Keywords: intergenerational transmission, causality, birth order, education, Europe

JEL codes: J01, J6, I2, I24.

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[†]Any views expressed in this paper are those of the author and do not necessarily reflect those of the Bank of Italy.

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 educational persistence across generations. Therefore, the objective of this paper is to propose a new strategy to correct for the endogeneity issue.

Our contribution to the existing literature is twofold.

First, we introduce a new instrumental variable (IV) approach in the IGM literature: 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), we instrument parental education with parental birth order. Importantly, 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.[1] Then, we enrich the instrument set considering also the compulsory schooling laws (CSL) introduced in several European countries in different years, especially in the post-World War II period. 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 favour of the exogeneity of parental birth order as instrument.

The second contribution we make points toward external validity: we extend the

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

analysis of intergenerational causal effects in education to 13 European countries (Austria, Belgium, Czech Republic, Denmark, France, Germany, Greece, Italy, Netherlands, Poland, Spain, Sweden, Switzerland). In this way we overcome the limitation of most of the existing literature which relies on single-country analysis.

We draw on a new longitudinal household survey such as the Survey of Health, Ageing and Retirement in Europe (SHARE), which is quickly reaching popularity among researchers for many advantages it offers with respect to other data sources (see Section 4.1). For what concerns the analysis on CSL reforms, we supplement our data with external information, relying on Brunello et al. (2009) and Brunello et al. (2012).

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 those countries who have experienced changes in the compulsory schooling legislation (which reduces the sample size by two thirds), the causal effect increases to 0.6 using first born alone and to 0.541 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 *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 favour of the treatment homogeneity, although this conclusion must be taken with care. Finally, we run a series of specification tests and robustness checks aimed to exclude the possibility that our results are driven by a particular country or a particular characteristics of parent's family.

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, IV-2SLS regressions, the characterization of the compliers sub-population and robustness checks. Finally, Section 6 concludes.

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 at-

tempts 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),[2] adoptees (based on the idea that the genetic link between children and adoptive parents is absent),[3] 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.[4] 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 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 favour of the exogeneity of parental birth order. The economic rationale of using these two instruments is developed in sections 3.2 and 3.3.

Among others,[5] 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)[6] 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

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

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

[4]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.

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

[6]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.

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) analyses 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 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 and descriptions of the instruments

3.1 Empirical 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:

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

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)[7] and if we assume $\eta > 0$, then we have $\mathrm{Cov}(E_j^P, h_{ij}^{\mathrm{C}}) > 0$: in this case, the OLS estimator of β_1 is biased upward.

To estimate β_1 consistently 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 2SLS, where equations (2) and (3) are respectively the causal equation of interest and the first stage:

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

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

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 $\mathbf{Z}_{jtc} \equiv [\mathrm{FB}_{jtc}]$ or $\mathbf{Z}_{jtc} \equiv [\mathrm{FB}_{jtc},\ \mathrm{CSL}_{jtc}]$, 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

In the first part of the analysis we instrument parental education with parental birth order (using first born status). It is well established in the economic literature that first

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

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 families. 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 sibship size larger or equal to 4). Second, there might exist cultural preferences that tend to favour investment of resources and time towards first born children. 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 he/she does not have to share parental time with other siblings until a second child is born. Fourth (but somehow 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 behaviour 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, would be indeed an interest topic to explore but this goes beyond the scope of the paper. Since we are using a sample of parents aged more than 50 years old (with the median age be 64), we can only conjecture that cultural preferences towards the first born individuals are driving most first born educational advantage (our first stage effects that we will see in section 3.2). Whatever are the underlying mechanisms that link birth order to education, a necessary condition for our identification strategy is that our first-born instrument is exogenous.[8]

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 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

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

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 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 family 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.[9]

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

3.3 Second instrument: Compulsory Schooling Laws

As already stated in the previous section, we consider as a second instrument the number of mandatory schooling years induced by the introduction of CSL in 10 European countries in different years. The use of CSL as instrument for education is well established in the literature and has been used in different contexts (Angrist and Keueger 1991; Oreopoulos et al. 2006; Chevalier 2004; Brunello et al. 2009, etc), showing a positive and moderate effect of CSL on educational attainment in different countries.

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 CSL in Europe in the post-World War II period, using prevalently the SHARE survey. The key assumption in this type of literature is that additional schooling is assigned to each individual based on the date of birth and independently from 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. 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) test this assumption using external data and find that the internal validity of this popular identification strategy is intact.

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 longitudinal sample), 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. Four waves of SHARE are currently available, where in the last one (released in November 2011) four new countries have joined the project (Estonia, Hungary, Portugal and Slovenia).

SHARE collects detailed information at the household and individual level covering

different topics such as: household economics, education, health, social security and income, financial investments, etc. An advantage of the survey is undoubtedly its cross-country comparability thanks 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. We remind to Havari and Mazzonna (2011) for the analysis of recall bias problems in the SHARELIFE data. In particular they show that information related to the childhood period is very little affected by problems of recall bias or coloring.

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. While for parents (SHARE respondents) we know both the years of completed schooling and the highest degree obtained (ISCED level), for children we know only the latter: for this reason we convert the ISCED level into years of schooling by exploiting information on the educational system of each country.[10] Since the questionnaire is common and the questions related to educational attainment are already expressed in a comparable format (using ISCED classification), we expect errors to be very limited.

Parental birth order and family size Information on parental birth order is gathered from wave 2. Each respondent is asked to provide his birth order, specifically whether

[10]SHARE provides details in the release guide (see SHARE release guide wave2, 2-5-0, 2011).

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

Compulsory schooling laws. Information on CSL is taken from 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).[11] As in previous papers we select one reform for each country to avoid blurring the differences between the pre-treatment and post-treatment cohorts. It is important to underline that generally the compulsory schooling laws have contributed to an increase of parental schooling by one extra year, affecting especially those at the ISCED 2 or 3 level depending on the country under consideration. To make the comparison between post-treated and pre-treated cohorts as credible as possible, we restrict our sample to parents born up to 10 years before/after the pivotal cohort, who have biological children aged 25 or more (see Section 4.3).

Other control variables We also control for children's birth order and 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 education, birth year, marital status etc. Further, we include an indicator for parent ever being unemployed during lifetime. This indicator is constructed looking at employment transitions of SHARE respondents (parents) in SHARELIFE.

4.3 Samples 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 (more than 16.000 parents). For each respondent, we know the schooling level of up to 4 selected children. We end up with a sample of almost 37.000 children, born before 1981. 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.

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

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 6400 parents and 13500 children. For simplicity we refer to this sub-sample as the *CSL sample*.

Table 2 shows descriptive statistics for parents and children in both the full and CSL samples. We learn that parents have on average 10.3 years of education, are female (57%, reflecting differential mortality at old ages), aged 66, first born (39%), with 2.39 children and prevalently live with the spouse or partner (69%). The picture is similar in both samples, with the exception that the average level of education is higher in the CSL sample (11.2 years), because some countries characterized by low levels of education (such as Spain and Greece) are excluded due to absence of CSL reforms. Children have on average 13 years of education in both samples, are equally shared among males and females, aged 40, first born (45%) and prevalently live with the spouse or partner (64%).

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 provides support for the use of a linear model for our data.

In the next figures we show graphically the intuition behind our first born instrument. The left panel of Figure 3 shows the "first stage" when using birth order, controlling for family size. In particular, first born parents have approximately 0.5 years of education more than non-first borns. This advantage is roughly constant for any level of sibship size, although it slightly declines in larger families. The right panel shows the "reduced form" effect. Children of first born parents have approximately 0.25 years of education more than children of non-first born parents: even in this case the gap is roughly constant controlling for sibship size and slightly reduces for larger family. From this simple analysis we would expect a 2SLS effect not far from 0.5.

5 Results

5.1 OLS results

Because the sample size associated to each instrument is different,[12] to make sensitive comparisons across estimation methods we report: the OLS results for both the full sample and the CSL sample (Table 3); IV results using only parental birth order as instrument for both samples as well (Tables 4 and 5); IV results using only CSL reforms as instrument for the CSL sample (Table 6); 2SLS results using both instruments for the CSL sample (Table 7); a synthetic view of results is given in Table 8.

We start discussing the OLS estimates for equation (2) in Table 3. Two different specifications are used. In the baseline regression (columns 1 and 3) we control for parental education, indicators for the dyads father-daughter, mother-daughter and mother-son (excluded category: father-son) aimed to capture fixed effect for parent's and children gender. We also add country fixed effects, parental birth year fixed effects and child 3-years cohort fixed effects: these fixed effects are meant to absorb time-invariant country characteristics and capture the historical evolution of education for both parents and children generations. In columns 2 and 4 we add a series of indicators for child's birth order (excluded category: first born) and a continuous variable for the total number of children in the family.[13] Since parental employment history could be relevant for children's achievement, we add an indicator for parent being ever unemployed. We cluster standard errors at the household level as we have multiple children for each parent.

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 0.27–0.25 of a year (about three months), depending on the specification.[14] Interestingly, the estimated coefficients for parental education are very similar across the two samples. In general, daughters are more educated than sons (irrespectively 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, 0.54, 0.87, 1.29 fewer years of education than first born children:[15] this evidence suggests that

[12] Full sample when using first born as instrument; CSL sample when using compulsory schooling laws, restricting the number of countries to those enacting reforms.

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

[14] Our 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).

[15] The magnitude and the significance of these last four estimates is little affected if the number of children

the advantage of earlier born individuals persists even in the most recent generations. Moreover, increasing the number of children in the house decreases the expected years of schooling of all children by 0.18 years of education; finally, education is reduced by 0.3 years if parent has ever experienced an unemployment spell in his/her labour history.

5.2 2SLS results

We now discuss estimates from the IV/2SLS regression, which represents the core of our results. We start considering only parental birth order as instrument. Table 4 shows the estimates from the first stage, where parental education is regressed on a dummy for parent being first born, a set of dummy variables capturing parental sibship size (where the excluded category is a parent with no siblings) and all the other controls included in the OLS regressions.[16]

Like in Figure 3, we notice a positive effect of parental birth order on educational achievement, even when the largest set of controls is added. Being a first born parent increases parental schooling by 0.415 of a year (approximately 5 months of extra schooling) with respect to a later born; when we use the CSL sample the first stage estimate increases to 0.48. First born instruments exhibit a high predictive power on parental education: even when adding the full set of controls, the F statistic on the excluded instrument is 38.4 in the full sample and 21.2 in the CSL sample. Differently from what we have seen in Table 3 for the children generation, females are less educated than males: the mother-daughter and mother-son coefficients are negative and significant. The detrimental effect of parental sibship size is large and significant when sibship size is at least 3 (so parent with two brothers/sisters) in the full sample, and at least 4 in the CSL sample.

The IV estimates using first born as instrument are reported in Table 5. The results point towards a positive causal effect of parent's schooling: a one-year increase leads children schooling to increase by 0.43 of a year (around 5 months) using the full sample and to 0.6 of a year (around 7 months) using the CSL sample. Being the first time (to our knowledge) that such an instrument is used in the intergenerational mobility literature, we cannot compare this estimate with existing studies. Moreover, most of the above cited papers focus on different dependent variables rather than years of education (such as the probability of attending post-secondary education or the probability to repeat a grade).[17]

is entered in a set of fourteen mutually exclusive dummies.

[16] Obviously, children characteristics are not *determinants* of parental education: these controls are included because they enter as exogenous variables in the IV regression, whose estimates are shown in Table 5.

[17] Also for this lack of comparability we enriched the instrument set with CSL, even at the cost of reducing the sample size from more than 35,000 to less than 13,000 children.

Notably, the 2SLS estimates are higher than the OLS estimates by a factor of 1.7 and 2.4 respectively in the full and CSL sample. Although this does not meet the *a priori* believes about the source of endogeneity, this evidence is quite common in the empirical applications. One common 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. A detailed analysis on the characteristics of the subgroup mostly affected by the instrument (birth order) is presented in Section 5.3.

Table 6 show results for IV regression when using as instrument CSL solely. Since we select only countries where some educational reforms took place and cohorts born 10 years before or after the pivotal cohort, we lose about two-thirds of the full sample. From now on, our discussion will be based on the CSL sample only.

In the first two columns we show results from the first stage (column 1 is the baseline and in column 2 we add other controls). An increase of 1 year in compulsory schooling increases parental education by 0.25.[18] The F-statistic is higher than 11, so the instrument should not be weak, although it is lower than the F-statistics when using first born.

With CSL, we find again a positive effect of parental schooling on children's schooling (columns 3 and 4), although in the last specification it is significant at a 10% level. The estimated marginal effect is 0.42 or 0.38 according to the specification.[19]

We are now ready to discuss our last set of estimates: Table 7 shows 2SLS regression results with both instruments used together. As in table Table 7, columns 1 and 2 report first stage estimates. Being a first born parent increases education by 0.47 years (slightly less than when first born was used alone; see column 4 in Table 4) and one extra year of compulsory schooling yields 0.217 years of actual schooling (slightly less than when CSL was used alone; see column 2 in Table 6). Using the two instruments together does not jeopardize their predictive power: both first stage coefficients are significant at the 1% level and the combined F statistic is 14.5. In columns 3 and 4 we report 2SLS estimates. All control variable coefficients have the expected sign. Parental family size is irrelevant to children education; being second born children, third, fourth and more than fifth reduces education by respectively 0.237, 0.421, 0.57 and 1.212 years; an extra child in the family reduces education of 0.1 years. The causal effect in this over-identified model is 0.541 years of education: the effect is large in magnitude and significant at 1% level.

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

[19] Both the magnitude and the precision of estimates are comparable to those reported in Stella (2013).

This estimate is closer to the one found when using first born instrument alone (0.6): this reflects the fact that the first stage when using first born is stronger than the first stage when using CSL reforms. Columns 3 and 4 also report the statistic and the p-value of the J-test of over-identifying restrictions: in both specifications the null hypothesis that the instruments are correctly excluded from the main equation is not rejected. Assuming the exogeneity of CSL, this - coupled with the robustness checks in Section 5.4 - might be interpreted as evidence in favour of the first born exogeneity.

In Table 8 we summarize all our main results. We found that the OLS estimates of parental education to be around 0.25, a value in line with other empirical studies. The IV estimates using first born ranged between 0.415 and 0.6 (always significant at 1%) depending on specifications and sample used; in all cases, first stage were very strong. The IV estimates using CSL reforms are 0.375 (significant at 10%) or 0.421 (5%) depending on the specification; F-statistics on the excluded instruments were smaller than in the first born case but above the often used rule of thumb of 10. Finally, full 2SLS specifications delivered estimates of 0.541 and led to a non rejection of the J-test.

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 OLS. This question is addressed within a heterogeneous treatment effect model. A priori we expect the two instruments to affect different subgroups of the population. The CSL affects those who in absence of the reforms wouldn't have taken further education: this is represented by those in low socio-economic status. First born instrument affect those individuals who wouldn't have taken further education if they had not been first born: in principle this might happen not only at the bottom of the socio-economic distribution but even - and probably more - in the middle class. Some socio-demographic characteristics of the individuals affected by the instruments are studied in Section 5.3.

5.3 Heterogeneous treatment and compliers characterization

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, 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*).[20] Although the compliers cannot be listed from observed data, by exploiting Bayes theorem the compliers population can be easily char-

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

acterized when both the endogenous variable and the instrument are binary.[21] Therefore we recode our endogenous variable as a dummy taking value 1 if parent has more than 10 years of education. Regarding our instruments, first-born is already binary and CSL has been recoded such that it takes value 1 if the individual belongs to the cohorts potentially affected by the reform. We characterize the sub-population of compliers according to the following set of pre-treatment dummy variables, mostly related to parent's infancy: *books* (=1 if there were less than 25 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 9, where we report the unconditional mean of the pre-treatment dummy X, the conditional mean for the compliers' population and the relative likelihood (respect to the whole sample) that a complier has $X = 1$.

With respect to the whole sample, CSL compliers are equally likely to have had fewer than 25 books at home when ten, 11% more likely to have lived in a larger family, 19-20% more likely to have had fewer rooms per capita and have lived in a rural area and 27% more likely that their main breadwinner had an elementary occupation. 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 36% more likely to have had few rooms per capita, but they are 28% less likely to have had few books at home, 19% 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 for both instruments shows that CSL compliers have a socio-economic background poorer than average, which presumably places compliers parents' 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).[22] The fact that two such different instruments (targeting two qualitatively different sub-populations) deliver similar causal effects[23] can be interpreted as evidence in favour of a homogeneous treatment effect model.[24]

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

[22] A *caveat* applies here: remember that 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.

[23] At least in the sense of the J-test of over-identifying restrictions.

[24] Naturally this conclusion must be taken with care, as in principal *other* valid instruments may affect *other* sub-populations with *different* causal effects.

5.4 Robustness checks

A fundamental assumption behind our empirical strategy is that the first born instrument satisfies the exclusion restriction. For example, it could be that first born parents are more likely to inherit their parents' house: if this is the case, children's education will be affected through a channel which is distinct from that of parental education, thus violating the exclusion restriction. Instrument exogeneity is intrinsically untestable, but we can rule out this hypothesis looking at results from a linear probability model in Table 10: the dependent variable is a dummy taking value 1 if the parent has ever received a property house as a bequest. The relation is interesting *per se*: for example we see that this probability decreases as parental sibship size increases. The most important aspect is that first born does not affect the probability of receiving a property as a bequest.

A second concern is that first born results may be driven by a particular type of family size (for example, parent with one brother/sister). In Table 11 we run IV regressions by sibship size (2, 3, more than 4). We see that the IV estimates of parental education are quite consistent with each other: the only non significant estimate is for family size of 3 (so parent plus 2 siblings).

Since we are exploiting cross-country data, it could be that results are driven primarily by one specific country in the sample. We repeat our 2SLS regression with both instruments leaving one country out at each time. Results[25] look very robust: estimates for parental education range from 0.481*** (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 propose to use 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 rea-

[25] Available upon request.

sonably 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 preferred estimate suggest an intergenerational causal effect of 0.429 years of education for the full sample and 0.6 for the CSL sample (see below).

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 with both instruments shows that the coefficient of intergenerational persistence amounts to 0.541 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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Figures and Tables

Figures

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

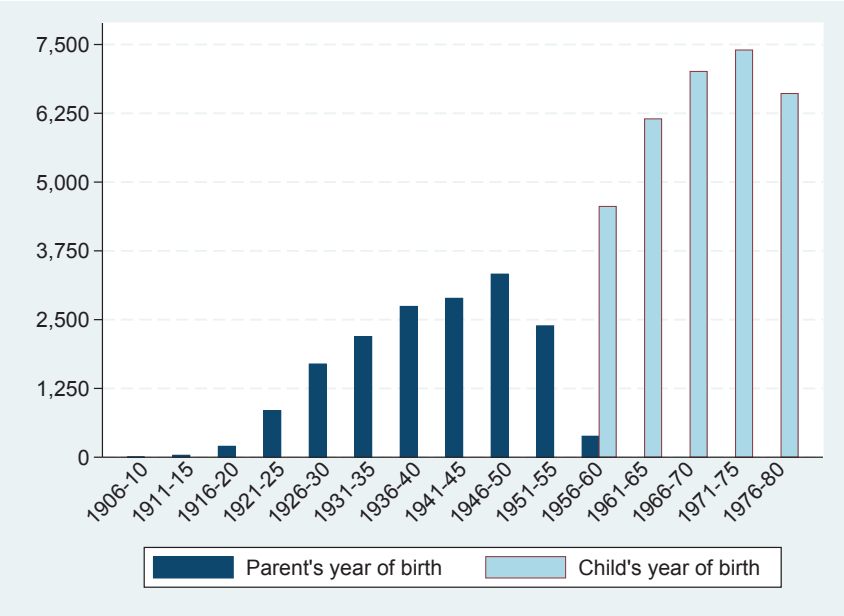


Figure 2 – Bivariate association between parental and children education

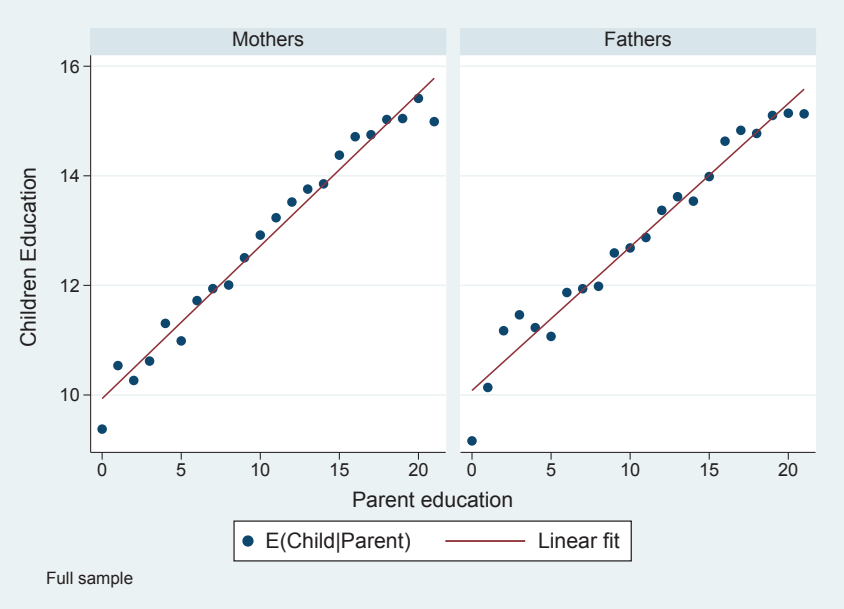
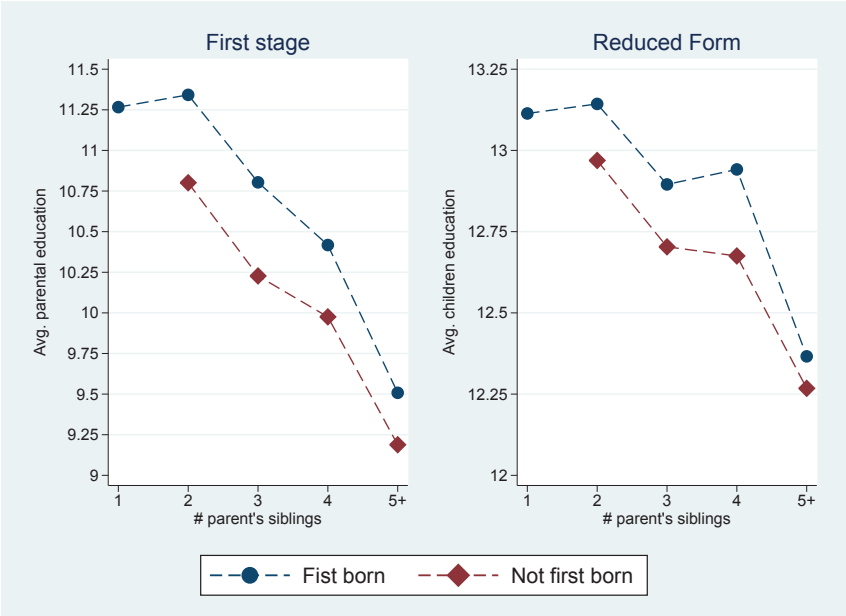


Figure 3 – Birth order: first stage and reduced form



Tables

Table 1 – CSL 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 (Lands):			
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

See: Brunello, Weber and Weiss 2012; Brunello, Fort, Weber, 2009.

Table 2 – Descriptive statistics

	Full sample		*CSL sample*	
Variable	Mean	S.D.	Mean	S.D.
Parents				
Years of education	10.32	4.14	11.25	3.81
Male	0.43	0.50	0.44	0.50
Age	66.03	9.29	63.14	6.94
First born	0.39	0.49	0.40	0.49
Mid born	0.35	0.48	0.33	0.47
Last Born	0.26	0.44	0.27	0.44
Number of siblings (incl. respondent)	3.53	1.97	3.56	2.06
Number of children	2.39	1.14	2.36	1.11
Mother alive	0.19	0.40	0.24	0.43
Father alive	0.07	0.25	0.09	0.28
Living with spouse or partner	0.69	0.46	0.73	0.44
Divorced	0.09	0.28	0.11	0.31
Widowed	0.21	0.41	0.15	0.36
Ever unemployed	0.06	0.24	0.07	0.25
Pivotal cohort			1944.5	6.77
Number of countries	13		9	
Number of observations	16698		6404	
Children				
Years of education	12.73	3.22	13.00	3.03
Male	0.51	0.50	0.51	0.50
Age	39.61	8.65	37.20	7.06
Biological	0.97	0.17	0.96	0.20
First born	0.45	0.50	0.47	0.50
Second born	0.34	0.47	0.34	0.48
Third born	0.14	0.34	0.13	0.33
Fourth born	0.05	0.21	0.04	0.20
Later the fifth born	0.02	0.15	0.02	0.12
Never married	0.27	0.44	0.30	0.46
Living with spouse or partner	0.64	0.48	0.62	0.49
Divorced	0.07	0.25	0.07	0.25
Widowed	0.01	0.09	0.00	0.07
Number of countries	13		9	
Number of observations	36903		13503	

Table 3 – OLS estimates of children education using two samples.

	Full sample		CSL sample	
	(1)	(2)	(3)	(4)
Parent education	0.270 ***	0.251 ***	0.269 ***	0.251 ***
	(0.006)	(0.006)	(0.010)	(0.010)
Mother-daughter	0.378 ***	0.453 ***	0.479 ***	0.560 ***
	(0.052)	(0.052)	(0.082)	(0.082)
Father-daughter	0.201 ***	0.201 ***	0.259 ***	0.272 ***
	(0.047)	(0.047)	(0.076)	(0.076)
Mother-son	0.233 ***	0.306 ***	0.276 ***	0.370 ***
	(0.052)	(0.051)	(0.081)	(0.082)
Child birth order: 2nd		-0.322 ***		-0.375 ***
		(0.032)		(0.051)
Child birth order: 3rd		-0.542 ***		-0.676 ***
		(0.055)		(0.091)
Child birth order: 4th		-0.875 ***		-0.987 ***
		(0.087)		(0.153)
Child birth order: 5th or later		-1.294 ***		-1.649 ***
		(0.140)		(0.265)
Nr of children		-0.181 ***		-0.142 ***
		(0.020)		(0.034)
Parent ever unemployed		-0.293 ***		-0.314 **
		(0.081)		(0.123)
N	35793	35793	12928	12928
R2-adj	.194	.212	.167	.185

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE. Standard errors clustered at household level.

Table 4 – First stage estimates. Instrument: parent is first born

	Full sample		CSL sample	
	(1)	(2)	(3)	(4)
Parent is first born	0.410 ***	0.417 ***	0.484 ***	0.482 ***
	(0.068)	(0.067)	(0.106)	(0.105)
Mother-daughter	-0.809 ***	-0.700 ***	-0.501 ***	-0.388 ***
	(0.073)	(0.073)	(0.113)	(0.114)
Father-daughter	-0.021	-0.019	0.047	0.060
	(0.061)	(0.061)	(0.095)	(0.095)
Mother-son	-0.871 ***	-0.761 ***	-0.553 ***	-0.424 ***
	(0.073)	(0.073)	(0.113)	(0.113)
Parent sibship size =2	-0.068	-0.017	0.083	0.097
	(0.119)	(0.117)	(0.183)	(0.182)
Parent sibship size =3	-0.396 ***	-0.296 **	0.064	0.114
	(0.124)	(0.123)	(0.192)	(0.191)
Parent sibship size ≥ 4	-1.159 ***	-0.981 ***	-0.921 ***	-0.773 ***
	(0.121)	(0.121)	(0.189)	(0.190)
Child-related controls		✓		✓
Constant	5.981 ***	7.354 ***	5.669 ***	4.360 ***
	(0.875)	(0.634)	(1.255)	(1.259)
N	35525	35525	12823	12823

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE.

Standard errors clustered at household level.

Table 5 – IV results. Instrument: parent is first born

	Full sample		CSL sample	
	(1)	(2)	(3)	(4)
Parent education	0.415***	0.429***	0.601***	0.599***
	(0.113)	(0.111)	(0.162)	(0.162)
Mother-daughter	0.501***	0.580***	0.657***	0.705***
	(0.106)	(0.094)	(0.124)	(0.113)
Father-daughter	0.206***	0.206***	0.239***	0.245***
	(0.048)	(0.048)	(0.085)	(0.085)
Mother-son	0.361***	0.442***	0.459***	0.516***
	(0.112)	(0.100)	(0.127)	(0.114)
Parent sibship size =2	0.070	0.110	0.113	0.120
	(0.082)	(0.080)	(0.126)	(0.125)
Parent sibship size =3	0.018	0.099	0.028	0.055
	(0.108)	(0.100)	(0.133)	(0.130)
Parent sibship size ≥ 4	-0.003	0.148	0.202	0.283
	(0.183)	(0.162)	(0.241)	(0.220)
Child birth order: 2nd		-0.257***		-0.210**
		(0.050)		(0.091)
Child birth order: 3rd		-0.402***		-0.367**
		(0.101)		(0.169)
Child birth order: 4th		-0.660***		-0.482*
		(0.154)		(0.280)
Child birth order: 5th or more		-1.046***		-1.119***
		(0.212)		(0.360)
Nr of children		-0.148***		-0.093**
		(0.027)		(0.042)
Parent ever unemployed		-0.195**		-0.106
		(0.099)		(0.170)
Constant	7.924***	8.199***	5.718***	5.773***
	(1.091)	(1.117)	(1.817)	(1.809)
N	35525	35525	12823	12823
F-excluded	36.4	38.4	21	21.2

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE. Standard errors clustered at household level.

Table 6 – First stage and IV results. Instrument: Compulsory education (CSL)

	(1)	(2)	(3)	(4)
	First stage	First stage	2SLS	2SLS
Comp. schooling	0.263***	0.249***		
	(0.076)	(0.075)		
Parent education			0.421**	0.375*
			(0.199)	(0.206)
Mother-daughter	-0.529***	-0.411***	0.556***	0.609***
	(0.115)	(0.115)	(0.131)	(0.115)
Father-daughter	0.045	0.060	0.247***	0.260***
	(0.095)	(0.095)	(0.079)	(0.078)
Mother-son	-0.557***	-0.422***	0.356***	0.419***
	(0.114)	(0.114)	(0.137)	(0.118)
Child birth order: 2nd		-0.478***		-0.316***
		(0.045)		(0.111)
Child birth order: 3rd		-0.923***		-0.565***
		(0.097)		(0.213)
Child birth order: 4th		-1.447***		-0.811**
		(0.162)		(0.341)
Child birth order: 5th		-1.374***		-1.484***
		(0.307)		(0.395)
Nr of children		-0.163***		-0.121**
		(0.046)		(0.048)
Parent ever unemployed		-0.630***		-0.235
		(0.182)		(0.177)
Constant	4.185***	4.591***	7.540***	8.078***
	(0.865)	(0.849)	(1.906)	(1.980)
N	12894	12894	12894	12894
F-excluded			11.8	11

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE.

Standard errors clustered at household level.

Table 7 – First stage and IV/2SLS estimates. Instruments: first born and CSL

	(1)	(2)	(3)	(4)
	First stage	First stage	2SLS	2SLS
Parent education			0.552 ***	0.541 ***
			(0.135)	(0.136)
Parent is first born	0.474 ***	0.471 ***		
	(0.106)	(0.105)		
Comp. schooling	0.225 ***	0.217 ***		
	(0.075)	(0.074)		
Mother-daughter	-0.506 ***	-0.392 ***	0.629 ***	0.680 ***
	(0.113)	(0.114)	(0.112)	(0.104)
Father-daughter	0.049	0.062	0.235 ***	0.242 ***
	(0.095)	(0.094)	(0.083)	(0.083)
Mother-son	-0.562 ***	-0.432 ***	0.428 ***	0.487 ***
	(0.112)	(0.113)	(0.115)	(0.105)
Parent sibship size =2	0.097	0.110	0.102	0.108
	(0.183)	(0.183)	(0.121)	(0.120)
Parent sibship size =3	0.057	0.105	0.017	0.043
	(0.192)	(0.191)	(0.127)	(0.124)
Parent sibship size ≥ 4	-0.908 ***	-0.763 ***	0.130	0.209
	(0.189)	(0.190)	(0.209)	(0.193)
Child birth order: 2nd		-0.458 ***		-0.237 ***
		(0.045)		(0.081)
Child birth order: 3rd		-0.867 ***		-0.421 ***
		(0.096)		(0.151)
Child birth order: 4th		-1.407 ***		-0.570 **
		(0.162)		(0.251)
Child birth order: 5th or more		-1.434 ***		-1.212 ***
		(0.292)		(0.337)
Nr of children		-0.110 **		-0.099 **
		(0.046)		(0.040)
Parent ever unemployed		-0.583 ***		-0.140
		(0.180)		(0.157)
Constant	5.008 ***	3.824 ***	6.242 ***	6.386 ***
	(1.100)	(1.111)	(1.564)	(1.558)
N	12789	12789	12789	12789
F-excluded			14.7	14.5
J-test			.492	.686
p-val			.483	.407

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE.

Standard errors clustered at household level.

Table 8 – Synthesis of estimates: OLS and IV-2SLS. Full and CSL sample.

	OLS		IV (first born)		IV (CSL)	2SLS (First born, CSL)
	Full sample	CSL sample	Full sample	CSL sample	CSL sample	CSL sample
Parent education (second stage)	0.251***	0.251***	0.429***	0.599***	0.375*	0.541***
First born (first stage)			0.417***	0.482***		0.471***
CSL (first stage)					0.249***	0.217***
F-excluded			38.4	21.2	11	14.5
J-test statistic						0.686
J-test p-value						0.407
N	35793	12928	35525	12823	12894	12789

Table 9 – Compliers characterization. CSL sample.

Exogenous variable (X)	N	$Pr(X=1)$	$Pr(X=1\|\text{compliers})$	$\frac{Pr(X=1\|\text{compliers})}{Pr(X=1)}$
			IV = CSL	
Books (low)	4540	0.601	0.608	1.011
Large household	4548	0.389	0.431	1.109
Rooms per capita (low)	4529	0.458	0.546	1.193
Breadwinner (low SES)	4314	0.503	0.637	1.266
Rural	4576	0.442	0.529	1.196
			IV = First born	
Books (low)	4525	0.601	0.435	0.723
Large household	4534	0.389	0.314	0.808
Rooms per capita (low)	4515	0.458	0.623	1.360
Breadwinner (low SES)	4301	0.504	0.484	0.962
Rural	4561	0.443	0.411	0.928

Note: variables are described in page 17.

Table 10 – Robustness check (I): birth order and probability of bequest (LPM)

	(1)	(2)
	House bequest	House bequest
	Full sample	CSL sample
Parent is first born	-0.003	-0.009
	(0.008)	(0.010)
Parent sibship size =2	-0.028 *	-0.036
	(0.016)	(0.023)
Parent sibship size =3	-0.047 ***	-0.058 **
	(0.016)	(0.023)
Parent sibship size ≥ 4	-0.055 ***	-0.058 **
	(0.016)	(0.023)
Nr of children	0.002	0.009 **
	(0.003)	(0.005)
Parent ever unemployed	-0.029 ***	-0.021 *
	(0.011)	(0.012)
living with spouse/partner	0.009 **	0.012 **
	(0.004)	(0.006)
Constant	0.136 **	0.455
	(0.054)	(0.367)
N	8200	3236
R2-adj	.0601	.0902

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE.

Standard errors clustered at household level.

Table 11 – Robustness check (II): IV regression by parental family size. Full sample.

	Sibship = 2	Sibship = 3	Sibship ≥ 4
Parent education	0.416 **	0.298	0.529 ***
	(0.174)	(0.227)	(0.186)
Mother-daughter	0.719 ***	0.320 *	0.712 ***
	(0.173)	(0.176)	(0.154)
Father-daughter	0.234 **	0.082	0.280 ***
	(0.092)	(0.096)	(0.079)
Mother-son	0.488 **	0.205	0.552 ***
	(0.190)	(0.204)	(0.146)
Child birth order: 2nd	-0.241 **	-0.239 **	-0.262 ***
	(0.101)	(0.098)	(0.077)
Child birth order: 3rd	-0.440 **	-0.322 *	-0.392 **
	(0.193)	(0.188)	(0.163)
Child birth order: 4th	-0.673 **	-0.606 *	-0.619 ***
	(0.310)	(0.340)	(0.225)
Child birth order: 5th	-0.940 **	-1.139 **	-0.998 ***
	(0.464)	(0.473)	(0.302)
Nr of children	-0.188 ***	-0.196 ***	-0.101 **
	(0.042)	(0.044)	(0.049)
Parent ever unemployed	-0.157	-0.089	-0.194
	(0.188)	(0.188)	(0.163)
Constant	8.576 ***	10.310 ***	6.885 ***
	(1.640)	(2.191)	(1.671)
N	8466	9100	14931
R2-adj	.16	.204	.141
F-excluded	15.2	7.4	15.8

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In each regression we control for country FE, parent's birth year FE, child's birth year FE.

Standard errors clustered at household level.

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