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Intergenerational transmission of human capital in Europe: evidence from SHARE

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Abstract

This paper extends the previous literature on the intergenerational transmission of human capital by exploiting variation in compulsory schooling reforms across nine European countries over the period 1920–1956. My empirical strategy follows an instrumental variable (IV) approach, instrumenting parental education with years of compulsory schooling. I find some evidence of a causal relationship between parents' and children's education. The magnitude of the estimated effect is large: an additional year of parental education raises the child's education by 0.44 of a year. I also find that maternal schooling is more important than paternal schooling for the academic performance of their offspring. The results are robust to several specification checks.

JEL codes: I20, J62

Keywords: Intergenerational transmission; Human capital; SHARE

1 Introduction

The notion that there is a positive association between the educational outcomes of the parents and their children is well documented. However, while there is a substantial consensus on this intergenerational correlation, less is known about the existence of a causal relationship underlying the transmission of education between generations (see, for instance, Björklund and Salvanes 2010; Black et al. 2005; Oreopoulos et al. 2006).

On the policy side, to the extent that policymakers are concerned about early school leavers, an analysis of the mechanisms through which education is passed on from parents to children is particularly relevant in light of reforms that extend the length of compulsory schooling. For example, if there is evidence that parental education is responsible for children's performance in school, then interventions that improve the educational attainment of less educated parents should lead to increased human capital among their children, thus reducing the degree of inequality of opportunity in education.

However, the primary concern is that intergenerational educational estimates might not adequately account for the correlation between parental schooling and some unobserved, inherited characteristics that might affect the academic achievement of their offspring. Such correlations would imply that the intergenerational transmission of education could be primarily driven by selection rather than reflecting a causal relationship running from a parent's to a child's education. To address this concern regarding endogeneity caused by omitted variables, the empirical literature has recently focused on three identification strategies: twin parents (Behrman and Rosenzweig 2002); adopted children (Björklund et al. 2006); Plug 2004; and instrumental variables (Black et al. 2005; Oreopoulos et al. 2006).

In my study, I employ this latter IV approach that obtains identification from compulsory schooling laws that influence the educational distribution of the parents without directly affecting the children. In particular, this study is strictly connected to the seminal paper by Black et al. (2005), which, using the Norwegian schooling reforms during the sixties and early seventies, finds no evidence of a causal impact of parental education on the next generation's education, with the exception of the weak impact of maternal schooling on educational attainment among sons. Similarly, Holmlund et al. (2011), applying this methodology to Sweden, obtain results in line with Black et al. (2005). However, these findings of limited effects of parental education in Norway and Sweden have not been supported by studies for other countries (see, for example, Oreopoulos et al. 2006 for the USA; Chevalier 2004 for the UK; and Maurin and McNally 2008 for France). Perhaps these contradictory results are related to the relatively low levels of inequality with respect to economic and educational outcomes in Scandinavian countries.

The contribution of this paper to the literature is twofold. To my knowledge, there are no studies that examine the causal effect of parental schooling on the human capital of their children by exploiting the variation provided by compulsory schooling laws over time and across European countries. Therefore, this paper adds to previous research by using this source of exogenous variation in parental schooling to disentangle the direction of causality. Another contribution of this paper is to shed new light on the different roles played by mothers and fathers in explaining the transmission of education to their sons and daughters. The findings from this multi-country analysis contribute to our understanding of how and why education is transmitted across generations by accounting for the effects of different institutional and cultural environments in Europe. A key element of my identification strategy is that it makes it possible to control for both country fixed effects, which account for time-invariant characteristics across countries, and birth cohort fixed effects for parents, which will capture any systematic difference in schooling outcomes across parental cohorts. To conduct this analysis, I draw data from the first two waves (2004 and 2006) of the Survey of Health, Ageing and Retirement in Europe (SHARE). This European dataset has three important features: first, it collects data on the current economic, health, and family conditions of over 30,000 individuals aged fifty and above in several European countries; second, it provides information on educational attainment for two family generations; and finally, as it is designed to be cross-nationally comparable, this dataset enables me to properly conduct a multi-country analysis. Furthermore, I use data on reforms of the minimum school leaving age by relying on recent studies (Brunello et al. 2009,2012; Garrouste 2010).

Based on these data, my main results demonstrate that: a) when omitting country-specific trends, there is some evidence of a causal relationship between parents' and children's education. The magnitude of the effect is large: an additional year of parental education induced by the reform generates 0.44 years of additional schooling for their children; b) when including country-specific trends, the estimated effects of parental education are no longer statistically significant. I argue that this lack of statistical significance can be explained by the fact that the addition of country-specific trends greatly reduces the first stage power of my instrument; c) the mother's schooling has a slightly stronger impact than that of her husband on the academic achievement of their offspring with or without country-specific trends. These findings are robust to a number of specification checks.

The remainder of the paper is organized as follows. The next section discusses the relevant literature on the intergenerational transmission of education. Section 3 presents a description of the data and illustrates the main features of European compulsory schooling reforms. Section 4 describes the empirical specification and identification strategy. The main results of the paper are presented in Section 5, and Section 6 provides robustness checks. I discuss the results in Section 7. Concluding remarks are provided in Section 8.

2 Literature review

Over the last decade, several empirical studies have attempted to shed some light on the causal mechanism that underlies the relationship between parents' and children's educational outcomes. These studies have proposed different strategies to identify exogenous variation in parental schooling. In the literature to date, there are three main research streams investigating the causal effect of parental education on their offspring's education. These streams differ in their choice of identification strategy. Below, I present a brief review of these studies and explain my contribution relative to the existing literature.[1]

The first strand of the literature examines the causal relationship between parental and children's education using data on pairs of identical twin parents to difference out not only family fixed effects but also unobserved factors due to the parents' genetics. One of the first studies, by Behrman and Rosenzweig (2002), compares the schooling of the children of twin mothers and twin fathers who were identical in all characteristics except their level of educational attainment. While Behrman and Rosenzweig's findings suggest a positive and large effect of the father's schooling but no effect from the mother's schooling, Antonovics and Goldberger (2005) questione the validity of these results by demonstrating their sensitivity to school coding schemes and sample selection rules.

The second stream of the literature estimates intergenerational schooling effects using samples of parents and their adopted children. Sacerdote (2002) and Plug (2004) compare adopted and natural children and conclude that environmental factors are important for the intergenerational transmission of education. However, these studies were severely limited by the paucity of data on the adopted children and a lack of information on the biological parents of adoptees. To overcome these issues, the literature has recently made use of large registry datasets of adopted children, which are available in the Nordic countries. In their study, Björklund et al. (2006) improve on the previous literature by employing a unique administrative dataset of Swedish adoptees that allowed them to examine the impact of both the adoptive and biological parents' years of schooling on the adopted child's years of schooling. They find both the adoptive and the biological parents' education to be important. Overall, studies on adopted children emphasize the importance of both genetic and environmental factors for a child's success in school.

Finally, there is a strand of the literature based on instrumental variables. This IV approach is the one I apply in this paper, and is closely related to the seminal paper by Black et al. (2005), which utilizes the Norwegian schooling reforms that occurred in different municipalities for the period 1959–1973. This study provides little evidence for the causal effects of parental education on children's attainment. Overall, the authors conclude that the father's schooling has no impact on children's educational attainment despite a positive, but small, intergenerational effect between mothers and their sons. Similar results were obtained in a more recent paper by Holmlund et al. (2011) applying the same strategy in Sweden. In contrast to these studies on Nordic countries, Oreopoulos et al. (2006), relying on variation in the school minimum age across states and time in the US, demonstrate that increasing the education of either parent has a negative and significant effect on the probability that a child repeats a year of school. This decline in grade repetition by children as a consequence of an increase in parental schooling is also found in France (Maurin and McNally 2008). Using changes in the mandatory schooling laws implemented in Britain during the seventies, Chevalier (2004) finds evidence of large, positive effects of maternal education on her child's education but no significant effects of fathers' education.

Taken together, these IV studies do not present a clear picture and reveal that, while there is a large set of estimates of intergenerational mobility from a wide range of different countries, the literature to date has not included a comparative analysis of educational reforms undertaken at the country level. This observation strengthens my claim that using this variation in European compulsory schooling laws is a novel contribution to the literature that can improve our understanding of how and why parental education affects children's outcomes by accounting for institutional and cultural factors across different European countries.

3 Data

The data used in this study are drawn from the first two waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), which took place in 2004 and 2006 in nine different European countries.[2] This survey interviews individuals aged fifty and above who speak the official language of each country, and do not live abroad or in an institution, plus their spouses or partners irrespective of age. The main advantage of this data source is the representativeness of the sample of elderly people in Europe because this survey is constructed to ensure comparability of the analysis across the different countries. Furthermore, this survey is harmonized with the U.S. Health and Retirement Study (HRS) and the English Longitudinal Study of Ageing (ELSA). The survey also contains detailed information on a broad set of variables: demographics, socio-economic characteristics, self-reported health as well as social and family networks. In this paper, I present evidence for nine countries, for which I could compute some key educational variables. These countries cover the various regions of continental Europe, ranging from Scandinavia (Sweden and Denmark) through Central Europe (Austria, Belgium, France, Germany and the Netherlands), and from the Mediterranean area (Italy) to Eastern Europe (Czech Republic).

I also employ data on reforms in the minimum school leaving age across the above-mentioned European countries, relying on recent works by Brunello et al. (2009,2012) and Garrouste (2010). As in Brunello et al. (2012), Table 1 presents a historical overview of the educational reforms that affected cohorts of parents from the 1930s until the late 1960s: for each country, the table reports the year of the reform,[3] the *pivotal cohort* (i.e., the year of birth of the first cohort affected by the reform), the change in the minimum school leaving age and in the years of compulsory schooling prescribed by the law, and the age at school entry. It is worth noticing that the countries selected for this

Table 1 Compulsory school reforms, by country

Country	Reform year	Pivotal cohort	Change in min. school leaving age	Years of comp. educ.	Age at school entry
Austria	1962/66	1951	14 to 15	8 to 9	6
Belgium (Flanders)	1953	1939	14 to 15	8 to 9	6
Czech Republic	1948	1934	14 to 15	8 to 9	6
	1953	1939	15 to 14	9 to 8	6
	1960	1947	14 to 15	8 to 9	6
Denmark	1958	1947	11 to 14	4 to 7	7
France	1936	1923	13 to 14	7 to 8	6
	1959/67	1953	14 to 16	8 to 10	6
Germany (Baden-Wuerttemberg)	1967	1953	14 to 15	8 to 9	6
Germany (Bayern)	1969	1955	14 to 15	8 to 9	6
Germany (Bremen)	1958	1943	14 to 15	8 to 9	6
Germany (Hamburg)	1949	1934	14 to 15	8 to 9	6
Germany (Hessen)	1967	1953	14 to 15	8 to 9	6
Germany (Niedersachsen)	1962	1947	14 to 15	8 to 9	6
Germany (Nordrhein-Westfalen)	1967	1953	14 to 15	8 to 9	6
Germany (Rheinland-Pfalz)	1967	1953	14 to 15	8 to 9	6
Germany (Saarland)	1964	1949	14 to 15	8 to 9	6
Germany (Schleswig-Holstein)	1956	1941	14 to 15	8 to 9	6
Italy	1963	1949	11 to 14	5 to 8	6
Netherlands	1942	1929	13 to 14	7 to 8	6
	1947	1933	14 to 13	8 to 7	6
	1950	1936	13 to 15	7 to 9	6
Sweden	1949	1936	13 to 14	6 to 7	7
	1962	1950	14 to 16	7 to 9	7

Notes: Source: Brunello et al. (2012). Notice that the year of the reform corresponds to the year when a certain reform was passed, which may not be equal to year of implementation. For example, the Austrian reform of 1962 was implemented in 1966; the French reform of 1959 was implemented in 1967. The pivotal cohort denotes the year of birth of the first cohort potentially affected by the reform.

study have extended the school leaving age by one year or more, and that the Netherlands and the Czech Republic experienced only a temporary reduction in the years of compulsory schooling.[4] Strikingly, although Italy had a lower initial level of mandatory schooling (5 years), it made substantial improvements during the postwar period (8 years). Note also that, as the schooling reforms in the West German states occurred at different points in time, Table 1 presents information on these reforms at the state level.[5]

The key variables of interest in this analysis are the educational attainment of parents and children. I measure educational attainment using years of schooling. One unusual feature of the dataset I employ is that it contains direct information on years of schooling for both parents and children. However, while for countries in the first wave the data on years of education are provided and are defined according to the ISCED-97 criteria,[6] for countries in the second wave there is information available on the country specific ISCED-97 codes but not on years of education. In my analysis, the Czech Republic is the only country included in the second wave that is not present in the first wave. I addressed this lack of information on the Czech Republic by taking advantage of the country specific conversion table that allowed me to recode the ISCED-97 codes into years of schooling.[7] It is also important to note that the measurement error due to misreporting could be magnified by the fact that children's educational achievement is reported by their parents.

To construct the sample of parents, I restrict attention to married or cohabiting individuals with at least one biological child, and, following Brunello et al. (2012), I focus on the cohorts of parents born from 1920 through 1956.[8] These cohorts were affected by the reforms of mandatory schooling that gradually came into effect across the European countries. By comparing the year of birth with the pivotal cohort, I am able to determine whether parents were exposed to the changes in schooling laws. For the analysis of this paper, it is worth stressing that I focus only on mothers and fathers who are the *family respondents*, i.e., the first member of the couple interviewed, who was entitled to respond to questions in the children's section on behalf of the couple. This implies that, while information on parental education was reported directly by both spouses, the data on the children's characteristics, such as years of schooling, were collected from the family respondents.[9] Therefore, parents who are not the family respondents are not considered in my sample of parents. I then link the demographic and educational characteristics of each child to the data for the corresponding family respondent to create an intergenerational dataset. Because the early cohorts of parents are likely to be affected by the consequences of World War II that might have forced them to interrupt or delay their academic careers, in the robustness analysis I also construct a postwar sample that includes the birth cohorts of parents born between 1935 and 1956, and show that the results are robust to excluding the prewar cohorts.

In this paper, I restrict attention to first born children.[10] The cohorts of interest were born between 1956 and 1980. This interval presents two advantages: first, it guarantees the absence of an overlap between parents and their offspring that could potentially undermine the exclusion restriction of the instrument; second, it allows me to consider sufficiently old children who were at least 24 years old at the time of the interview.[11] The distributions of the samples of parents and children across the countries are presented in Table 2.

After these restrictions, the final full sample of parents consists of 6,184 family respondents: 3,308 (53.5%) fathers and 2,876 (46.5%) mothers, while the final sample of children consists of 6,184 siblings: 3,117 (50.4%) sons and 3,067 (49.6%) daughters.[12] The summary statistics reported in Table 3 indicate, as expected, that fathers are older and are slightly more educated than their spouses. Particularly striking is that the second generation of children has a considerably higher level of schooling than their parents (13.25 versus

Table 2 Sample of parents and children, by country

Sample	Parents (1920–1956)			Children (1956–1980)		
	Fathers	Mothers	Total	Sons	Daughters	Total
Austria	312	170	482	225	257	482
Belgium	454	275	729	363	366	729
Czech Republic	360	363	723	363	360	723
Denmark	174	146	320	155	165	320
France	372	268	640	332	308	640
Germany	339	333	672	346	326	672
Italy	464	487	951	495	456	951
Netherlands	464	460	924	465	459	924
Sweden	369	374	743	373	370	743
Total	3,308	2,876	6,184	3,117	3,067	6,184

Notes: All the samples contain individuals for whom information on education is not missing.

Table 3 Summary statistics, sample of Parents (1920–1956) and Children (1956–1980)

Variable	Observations	Mean	Std. Dev.
Children			
Age	6,184	35.89	6.50
Education	6,184	13.25	2.84
Female (%)	6,184	0.49	0.5
Mothers and Fathers together			
Age	6,184	61.85	7.19
Education	6,184	10.71	3.68
Household size	6,184	2.4	0.79
Fathers			
Age	3,308	63.03	7.39
Education	3,308	10.98	3.74
Mothers			
Age	2,876	60.50	6.69
Education	2,876	10.40	3.59

Notes: All the samples include individuals for whom information on education is not missing. Education is measured with years of schooling and is defined according to the ISCED-97 criteria.

10.71 years of schooling). However, part of the positive association between parents' and children's education might reflect the positive correlation with unobserved ability.

In Figure 1, I analyze differences in the pattern of educational attainment between the cohorts of parents and children across countries. To facilitate comparisons, I separate the countries into two groups: in one group, the Northern European countries (Sweden, Denmark and the Netherlands, see Figure 1a); and in the other, the Western (Austria, Germany, Belgium and France), Southern (Italy) and Eastern (Czech Republic) European countries (see Figure 1b).[13] The vertical and horizontal axes describe the average number of years of schooling and year of birth, respectively. The vertical dashed line marks the year 1956 to separate the two samples. As one could expect, in all countries there is a clear trend of rising levels of education, so that one might be concerned that it may be difficult to distinguish the effect of the reform from the secular trend. Ideally, to thoroughly address this issue, one would like to rely on a very large sample of parents born in the close vicinity of the schooling law. Unfortunately, the sample size of my dataset is too small to conduct this local analysis.

4 Empirical specification

Following Black et al. (2005) and Oreopoulos et al. (2006), I specify a model for the children's education in a multi-country framework as follows:

$$Edu^{c}_{ihj} = \alpha + \beta Edu^{p}_{ihj} + \gamma X_{ihj} + \tau^{p} + \tau^{c} + \eta_{j} + \epsilon^{p}_{ihj} \quad (1)$$

where the unit of observation i denotes the child-parent pair and the superscripts c and p refer to child and parental characteristics, respectively. The dependent variable Edu^{c}_{ihj} denotes the years of schooling of the offspring generation, observed for child i within household h residing in country j and is expressed as a linear function of parental education levels measured by the years of schooling of the family respondent Edu^{p}_{ihj}. A key element of my approach is the inclusion of both country fixed effects η_j that account for time-invariant, unobserved characteristics, such as institutional and cultural features, that are likely to vary by country, and birth cohort fixed effects for parents τ^{p} (in 1-year

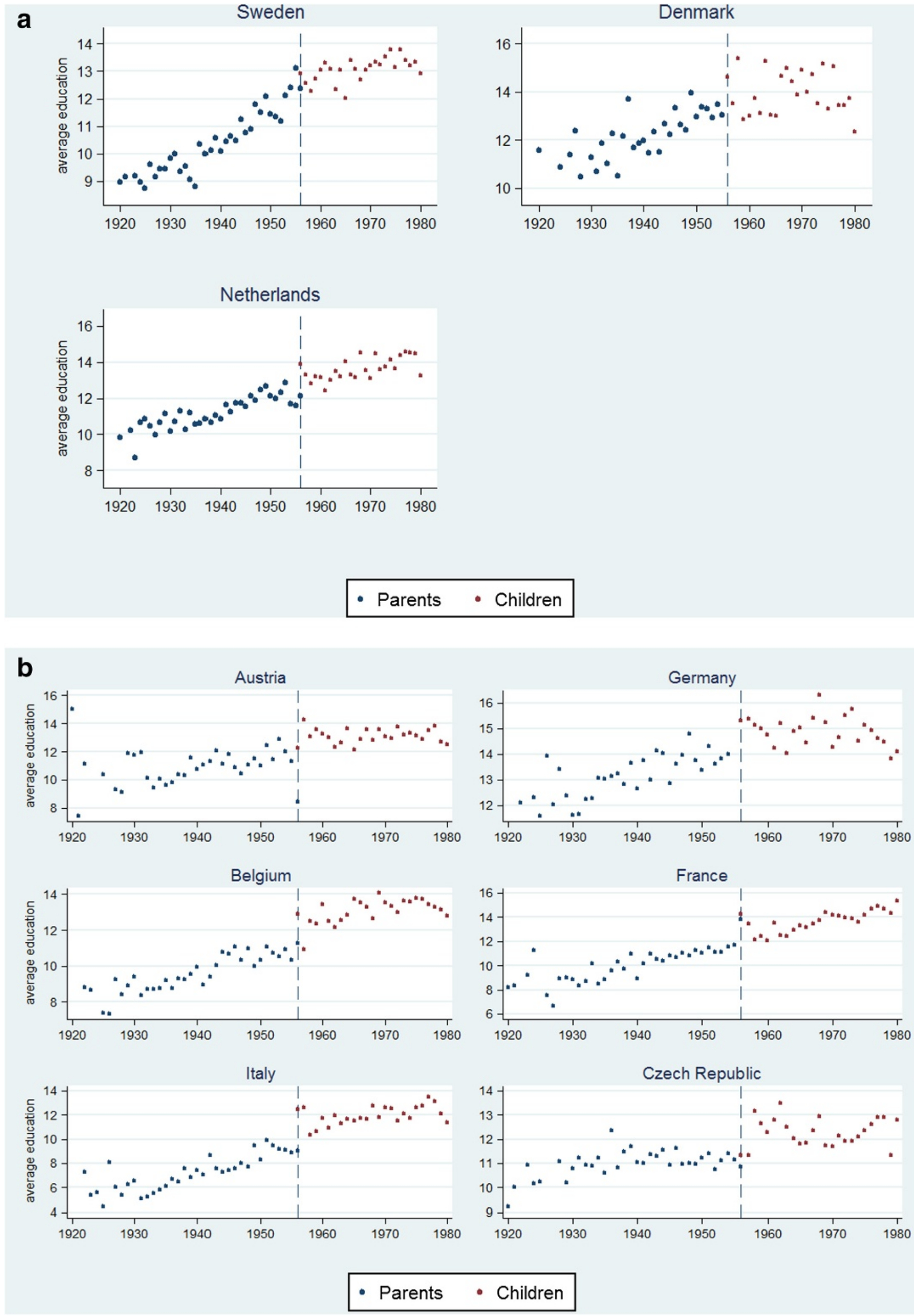

Figure 1 Trend in education of Parents and Children. (a) Northern Europe. **(b)** Western, Southern and Eastern Europe. *Notes:* The vertical and horizontal axes describe the average number of years of schooling and year of birth, respectively. The vertical dashed line marks the year 1956 to distinguish between the sample of parents (1920–1956) and children (1956–1980). Because in my sample there is only one Southern European country (Italy) and only one Eastern European country (Czech Republic), I put these two countries in a separate row together with the Western European countries.

intervals), which capture any systematic differences in school outcomes across parental birth cohorts. In model (1), I then include birth cohort fixed effects for children τ^c (in 1-year intervals) to control for cohort trends in education and account for the possibility that some children might not have finished school at the time of the interview.[14] In some specifications, I also control for country-specific quadratic trends in parental birth cohorts because the implementation of the schooling reforms might be correlated with country-level, unobserved, time-varying factors. Because many of the socio-economic characteristics of the parents tend to be endogenous, as they are themselves affected by the parent's education, I use a parsimonious specification: I add a set of individual socio-demographic characteristics X_{ihj}, including the children's gender and household size. Finally, ϵ_{ihj} represents an idiosyncratic error term. It is reasonable to believe that ϵ_{ihj} is correlated with the outcome variable because it embodies the unobserved factors of parents, including ability, which might affect the academic performance of the children.

To distinguish between the intergenerational effects of mothers and fathers, in model (1) I also include the interaction between parental education and the gender dummy for the parents. By including this interaction, I am able to capture the differential impacts of maternal and paternal education on children's education. Formally, I estimate the following specification:

$$Edu^{c}_{ihj} = \alpha + \beta Edu^{p}_{ihj} + \lambda Edu^{p}_{ihj} * gender^{p}_{ihj} + \gamma X_{ihj} + \tau^{p} + \tau^{c} + \eta_{j} + \epsilon^{p}_{ihj} \tag{2}$$

where $gender^{p}_{ihj}$ is equal to one if the family respondent is the mother.

4.1 Identification strategy

I identify the causal effect of parental education on children's education using compulsory schooling laws over 30 years as an instrument for the number of years of parental schooling. A large body of economic literature (among others, Black et al. 2005; Oreopoulos et al. 2006) recognizes this identification strategy as valid because changes in compulsory schooling laws produce variation in parental education that is credibly exogenous and unlikely to be related to unobservable characteristics of the parents, such as ability, that might explain the different educational outcomes of their offspring.

In this study, I apply this IV strategy to a European framework by instrumenting parental education with the number of years of compulsory schooling determined by the law.[15] This multi-country approach has been employed by Brunello et al. (2009) to study the returns to schooling and Brunello et al. (2011) to investigate the effects of schooling on health. Formally, the instrument is constructed as follows:

$$Reform^{p}_{ij} = \begin{cases} (ycs)^{A} & if \ \left(parental\ year\ of\ birth\right)_{i} > (pivotal\ cohort)_{j} \\ (ycs)^{B} & otherwise \end{cases} \tag{3}$$

where the variable *ycs* represents the number of years of compulsory schooling, and the superscripts *B* and *A* denote before and after the educational reform, respectively. Therefore, I construct the instrument in such a way that it depends on three factors: the country *j* in which the reform took place, the parents' years of birth, and the first birth cohort affected by the reform (i.e., the pivotal cohort). I can then determine whether parents were exposed to the compulsory laws by comparing their years of birth with those of the pivotal cohort.

Model (1) is estimated using two stage least squares (2SLS), and the first stage regression is given by:

$$Edu^{p}_{ihj} = \delta_{0} + \delta_{1} Reform^{p}_{ij} + \pi X_{ihj} + \varphi^{p} + \varphi^{c} + \sigma_{j} + \upsilon_{ihj} \tag{4}$$

where Edu^p_{ihj} is instrumented with $Reform^p_{ij}$, the compulsory years of schooling in the respective country and cohort. Similarly, the first stage for model (2) can be written as:

$$Edu^p_{ihj} = \delta_0 + \delta_1 Reform^p_{ij} + \delta_2 Reform^p_{ij} * gender^p_{ihj} + \pi X_{ihj} + \varphi^p + \varphi^c + \sigma_j + \upsilon_{ihj} \quad (5)$$

Therefore, in equation (5) I employ not only the years of compulsory schooling but also the interaction between compulsory schooling and the gender of the parent as instruments. There are two points to note on this instrumental variables strategy. First, because it varies over parental cohorts and across countries, the instrument is affected by two potential sources of serial correlation: within country over parental cohorts and across countries for the same parental cohort. To mitigate this concern, I cluster all standard errors by the country and cohort of the parents, thus allowing for arbitrary dependence within country-cohort cells.[16] Second, the compulsory schooling reforms do not affect the entire population. Rather, these reforms influence only the least educated groups of parents. As a consequence, this identification strategy allows me to recover a Local Average Treatment Effect (LATE) instead of averages across the population (ATE).[17] As noted by Card (2001), these local effects are of interest because the groups of individuals captured by the LATE are those that are most likely to be affected by the mandatory schooling laws.

5 Main results

5.1 Association between the schooling of Parents and their Children

Table 4 presents the results from a simple ordinary least squares (OLS) estimation of model (1). In column 1, I report the coefficient of parental education without other controls: the OLS estimate suggests that a one year increase in the parents' years of schooling is associated with an increase in the number of years of schooling for children of 0.32 years. This coefficient is significant and robust to the inclusion of controls for parental

Table 4 Effects of Parents' education, naive OLS

Dependent variable: Child's education	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Parental education	0.325***	0.327***	0.305***	0.286***	0.286***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Female (child)		0.204***	0.217***	0.225***	0.225***
		(0.066)	(0.065)	(0.063)	(0.064)
Household size		-0.135***	-0.121***	-0.170***	-0.176***
		(0.042)	(0.043)	(0.043)	(0.043)
Socio-demographic controls	No	Yes	Yes	Yes	Yes
Cohort F.E. for parents	No	Yes	Yes	Yes	Yes
Country F.E.	No	No	Yes	Yes	Yes
Cohort F.E. for children	No	No	No	Yes	Yes
Country-specific quadratic trends	No	No	No	No	Yes
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.178	0.190	0.228	0.243	0.247
Mean of Dep. Var.	13.25				
Std. Dev. of Dep. Var.	2.84				

Notes: Birth cohort dummies for parents and children are in 1-year intervals. Country-specific quadratic cohort trends are computed by interacting parental birth cohort and its square with country dummies. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

birth cohort and socio-demographic characteristics (column 2) including the gender of the children and household size. When controlling separately for country fixed effects (column 3) and cohort fixed effects for children (column 4), parental education remains positively and significantly associated with children's education, although the coefficients are slightly reduced to 0.30 and 0.29, respectively. I then include a full set of country indicators interacted with a quadratic trend in the parents' year of birth (column 5). The results are virtually unchanged relative to the previous specification.

To allow for separate effects of maternal and paternal education, I estimate model (2), in which I include the interaction between parental education and a female dummy that takes value one if the family respondent is the mother. The estimates for the most general specification are reported in Table 5. Column 1 corresponds to column 5 of Table 4. The inclusion of the interaction term (see column 2) reduces the magnitude of the coefficient on parental education, but the OLS estimate remains positive and significant. While I find only a slightly stronger relationship between maternal education and children's outcomes than between the children's and paternal education, the coefficient on the interaction term is highly statistically significant. Interestingly, this positive sign is consistent with the view (see, for example, Black et al. 2005; Chevalier 2004, Chevalier et al. 2011) that mothers are likely to devote more time to child care than fathers. This finding is discussed later in the paper. Finally, in Table 5, I present similar results from dividing the sample into sons (column 3) and daughters (column 4).

Overall, my OLS estimates confirm a strong, positive intergenerational correlation in education even when country fixed effects are controlled for and the sample is divided into sons and daughters. However, this positive correlation could be explained by the role family background characteristics played in determining the children's level of educational attainment, or it might reflect genetic differences in ability that are transmitted to the children. In the next subsection, I attempt to establish whether this positive correlation has a causal interpretation.

Furthermore, it is not surprising that in all specifications I do find a negative and statistically significant correlation between family size and children's schooling performance. In the more comprehensive specification (see column 5 in Table 4), a one unit increase in

Table 5 Effects of Parents' education on sons and daughters, naive OLS

Dependent variable: Child's education	**(1)**	**(2)**	**(3)**	**(4)**
Sample	**Overall**	**Overall**	**Sons**	**Daughters**
Parental education	0.286***	0.231***	0.230***	0.228***
	(0.011)	(0.013)	(0.020)	(0.017)
Parental educ*female (parent)		0.041***	0.044***	0.040***
		(0.006)	(0.009)	(0.008)
Household size	-0.176***	-0.172***	-0.154**	-0.176***
	(0.043)	(0.043)	(0.063)	(0.055)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and country-specific quadratic cohort trends (computed by interacting parental birth cohort and its square with country dummies). Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

the household size is associated with a 0.18 years decline in child education. This result appears to be in line with the notion that there might be a trade-off between child quantity and quality (Becker and Lewis 1973).

5.2 Causality between schooling of the Parents and their Children

In Panel A of Tables 6, 7 and 8, I present the two stage least squares (2SLS) estimates, which are the primary estimates of interest in this study. To instrument for parental education, I use years of compulsory schooling. Table 6 (Panel A) indicates that, in the first two specifications, the coefficient on parental education is strongly statistically significant (at the 1 percent level); adding country fixed effects (column 3) and cohort fixed effects for children (column 4) to the model reduces the significance of the 2SLS estimate, but it is maintained at the 10 percent threshold. The magnitude of the effect of parental education varies remarkably with the specification and becomes substantially larger when country fixed effects are added to the model (see column 3).

As emphasized by Holmlund et al. (2011), for the validity of the instrument to hold, it is important to control not only for country fixed effects but also for country-specific time trends to disentangle the identifying variation in parental education induced by the

Table 6 Effects of Parents' education, IV analysis

		Panel A: 2SLS			
Dependent variable:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Child's education					
Parental education	0.281***	0.367***	0.498**	0.437*	0.468
	(0.057)	(0.054)	(0.254)	(0.262)	(0.334)
Female (child)		0.206***	0.224***	0.229***	0.238***
		(0.066)	(0.066)	(0.065)	(0.069)
Household size		-0.114**	-0.123***	-0.150***	-0.159**
		(0.048)	(0.044)	(0.056)	(0.069)
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.175	0.188	0.179	0.214	0.202
Mean of Dep. Var.	13.25				
Std. Dev. of Dep. Var.	2.84				
First stage F statistic	42.99	38.23	8.58	7.47	1.63
		Panel B: First stage			
Dependent variable:	(1)	(2)	(3)	(4)	(5)
Parent's education					
Compulsory education	0.632***	0.604***	0.217***	0.207***	0.104
	(0.096)	(0.098)	(0.074)	(0.076)	(0.081)
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.062	0.081	0.221	0.258	0.262
For all panels:					
Socio-demographic controls	No	Yes	Yes	Yes	Yes
Cohort F.E. for parents	No	Yes	Yes	Yes	Yes
Country F.E.	No	No	Yes	Yes	Yes
Cohort F.E. for children	No	No	No	Yes	Yes
Country-specific quadratic trends	No	No	No	No	Yes

Notes: Birth cohort dummies for parents and children are in 1-year intervals. Country-specific quadratic cohort trends are computed by interacting parental birth cohort and its square with country dummies. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7 Effects of Parents' education on Sons and Daughters, IV w/o country-specific trends

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
Panel A: 2SLS				
Dep. Var.: Child's education				
Parental education	0.437*	0.462*	0.553*	0.410
	(0.262)	(0.269)	(0.300)	(0.573)
Parental educ*female (parent)		0.050***	0.058***	0.044*
		(0.013)	(0.017)	(0.023)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78
Angrist-Pischke first stage F statistic	7.47	6.99	7.56	2.00
Panel B: First stage				
Dep. Var.: Parent's education				
Compulsory education	0.207***	0.226***	0.301***	0.153
	(0.076)	(0.077)	(0.102)	(0.094)
Compulsory educ*female (parent)		-0.052***	-0.055***	-0.048***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics. The Angrist-Pischke first stage F statistic refers to the first stage regression of parental education; the first stage regression of parental education*female has much stronger power, thus the Angrist-Pischke first stage F statistic is omitted. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8 Effects of Parents' education on Sons and Daughters, IV with country-specific trends

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
Panel A: 2SLS				
Dep. Var.: Child's education				
Parental education	0.468	0.421	1.064	0.671**
	(0.334)	(0.272)	(0.973)	(0.334)
Parental educ*female (parent)		0.047***	0.079*	0.052***
		(0.013)	(0.043)	(0.015)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78
Angrist-Pischke first stage F statistic	1.63	8.64	1.39	4.04
Panel B: First stage				
Dep. Var.: Parent's Education				
Compulsory education	0.104	0.124	0.153	0.092
	(0.081)	(0.082)	(0.109)	(0.108)
Compulsory educ*female (parent)		-0.051***	-0.055***	-0.045***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals), socio-demographic characteristics and country-specific quadratic cohort trends (computed by interacting parental birth cohort and its square with country dummies). The Angrist-Pischke first stage F statistic refers to the first stage regression of parental education; the first stage regression of parental education*female has much stronger power, thus the Angist-Pischke first stage F-statistic is omitted. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

compulsory schooling reforms from the confounding factors that arise from country-level, upward trends in educational attainment. When I add country-specific quadratic trends in birth cohorts to the model (column 5), I find that the estimated effects of parental education are no longer statistically significant. While this lack of significance raises concerns about the ability of my analysis to properly distinguish between the compulsory schooling effects and the positive trends in average educational attainment of the parents, I argue below that this result can likely be explained by the fact that the inclusion of country-specific trends markedly reduces the first stage power of my instrument. Interestingly, this weak first stage relationship between the instrument and parental education when including country-specific trends has also been found in Oreopoulos et al. (2006), who point to the presence of contemporaneous trends of increasing both average educational attainment of the parents and years of compulsory schooling.

Thus, I investigate the first stage estimates reported in Panel B of Table 6. These estimates show that the reform is strongly and positively correlated with the number of years of parental schooling and that its t statistic is above 2.7 even when conditioning on country and cohort fixed effects. One notable exception, however, is the model that includes the country-specific trends (column 5), in which the first stage estimate is not statistically different from zero, with the t statistic of approximately 1.3. Panel A of Table 6 also reports the corresponding first stage F-test statistic for each specification that accounts for the clustering of the standard errors at the parents' country and cohort level. When subsequently including country fixed effects and cohort fixed effects for children (columns 3 and 4), this statistic falls to approximately 7.5, which is below the cutoff value of 10 suggested by Bound et al. (1995) and Staiger and Stock (1997), but this value is substantially higher than the first stage F-test statistic produced by the model that incorporates the country-specific trends (column 5). Because of the lack of power in my identification strategy after controlling for country-specific trends, I choose the specification that does not allow for country-specific trends (see column 4 of Table 6, Panel A) as my preferred one. In this model, my results reveal that parental education appears to have a large causal effect on children's education: I find that an additional year of parental education will raise a child's educational attainment by 0.44 of a year.

I also perform a number of weak-instrument robust tests that allow me to conduct inference that has the correct size even in the presence of weak instruments. The results of this set of tests are presented in Table 9, which provides the Anderson-Rubin (AR) statistic (Anderson and Rubin 1949) and, as a reference, the standard Wald test for specifications 3 and 4 in Panel A.[18] As one could expect given the relatively low value of the first stage regression F-test statistic, the AR p-value and confidence intervals are larger than the non-robust Wald counterparts, but the differences are limited. Most importantly, the AR p-value is still on the border of statistical significance at approximately the 10 percent

Table 9 Weak-instrument robust tests for models (3) and (4) in Table 6 - Panel A

Endogenous variable: Parents' education	**(3)**		**(4)**	
	p-value	95% C. Set	p-value	95% C. Set
Anderson-Rubin	0.069	[-0.054, 1.292]	0.134	[-0.216, 1.235]
Wald	0.049	[0, 0.996]	0.095	[-0.076, 0.951]

Notes: Wald test is not robust to weak instruments.

threshold. These results imply that, even when accounting for the presence of a weak instrument, the treatment effects of parental education remain marginally statistically significant.

For the above reasons, and in light of the Angrist and Pischke (2009) claim that "just-identified 2SLS is approximately unbiased", I conclude that in my preferred specification (see Table 6, Panel A, column 4) the issue of weak instrument bias may be of less concern, and that there is some evidence of a causal effect of parental education on the educational attainment of their children. Table 10 summarizes the results of my favorite model. The first column reports the OLS estimates from a regression of the child's education on the education of the parents. In the second column, I display the reduced form coefficient from a regression of the child's education on the instrument. In the third column, I present the first stage estimate from a regression of parents' education on the instrument. In the last column, I present the 2SLS estimate, where years of compulsory schooling are used as an instrument for parents' education. This latter estimate is simply the reduced form estimate divided by the first stage estimate.

While the main goal of this study is the analysis of the effect of parental education on the second generation's education, another contribution is the exploration of the different roles fathers and mothers play in explaining the transmission of human capital to their sons and daughters. To conduct this analysis, I proceed in two steps.

First, by adding an interaction between the gender of the parent and parental education to the model (see model (2)), I am able to partially extend the analysis by allowing for different effects of maternal and paternal education. This means that my preferred model (see Table 6, Panel A, column 4) uses as instruments not only the years of compulsory schooling but also interaction term between compulsory schooling and the gender of the parent. The 2SLS estimates, reported in Panel A of Table 7, suggest that when controlling for the differential impacts of mothers and fathers (see column 2), the results remain substantially unchanged with respect to the direction, magnitude and significance. Consistent with the results of the OLS estimates, I find that the coefficient on the interaction between years of education and parental gender is highly statistically significant (at the 1% level) and positive, thus suggesting that maternal education is somewhat more important than paternal.

Table 10 Effects of parental education in the preferred model

	(1)	(2)	(3)	(4)
	OLS	Reduced-form	First stage	IV
Dependent variable:	Child's education	Child's education	Parental education	Child's education
Parental education	0.286***			0.437*
	(0.011)			(0.262)
Compulsory education		0.091	0.207***	
		(0.061)	(0.076)	
Observations	6,184	6,184	6,184	6,184
R^2	0.243	0.141	0.258	0.214
First stage F statistic	7.47			
Anderson-Rubin test p-value	0.134			

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals), and socio-demographic characteristics. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Second, in an attempt to disentangle the treatment effects of parental schooling on sons from the effects on daughters, I separately consider samples of male and female children. The results for sons and daughters are presented in columns 3 and 4 (Table 7, Panel A), respectively. When conducting the analysis for sons, the coefficient on parental education is statistically significant and larger than the coefficient generated by the full sample (0.55 versus 0.46 years), although the effect is less precisely estimated given the smaller sample size. On the contrary, when examining the sample of daughters, the 2SLS estimate for parental education falls to approximately 0.41 and is not statistically different from zero. In columns 3 and 4, I also find evidence that maternal education seems to matter more than paternal education in determining the educational success of their offspring. I explain these findings in the discussion of the results.

The non-significant effects of parental education on daughters can be largely attributed to the weak first stage relationship between the reform and number of years of parental schooling (see Table 7, Panel B, column 4): the t statistic for the reform is approximately 1.6 for daughters compared to approximately 3 for sons (column 3). Furthermore, the Angrist-Pischke first stage F-test statistic is approximately 2 for daughters compared to approximately 7.6 for sons.[19] The first stage estimates also reveal that the reform had a stronger impact on fathers.

In Table 8, I repeat the analysis in Table 7 using the country-specific quadratic trends: the coefficient on parental education is statistically significant, although very noisy, only for the sample of daughters. As expected, in the first stage regression (see Table 8, Panel B) the reform shows no evidence of being correlated with parental schooling. Interestingly, the coefficient on the interaction term between the gender of the parent and parental education remains statistically different from zero across all specifications, thus supporting the basic finding that mothers have a significant stronger effect than fathers on the academic outcomes of their offspring.

Regardless of the model, I find that the IV estimates are larger than their OLS counterparts. While this result might appear to contradict intuition regarding omitted variable bias given the positive correlation between parental education and unobserved ability, it is consistent with several studies that employ mandatory schooling reforms as instrument. Part of this difference can be attributed to two explanations (Card 2001). First, because there might be important measurement errors in the self-reported schooling of the parents, the resulting downward bias could be significantly larger than the upward omitted variable bias. Second, as mentioned previously, this IV strategy captures the effect of reforms only on the part of the population that is induced to obtain additional schooling by the educational reforms. Therefore, the treatment effect of parental education for this subset of compliers is likely to be above the average marginal effect for the entire population.[20] The ratios of the IV estimates to the OLS estimates for the entire sample and sub-samples of sons and daughters range from 1.5 to 2.4. Similar ratios have been found in Oreopoulos et al. (2006), Angrist and Krueger (1991) and Staiger and Stock (1997).

6 Robustness checks

In this section, I perform a variety of robustness checks to test how the results change when I modify the sample or use a different instrument (see Tables 11 and 12).

I begin by investigating whether my estimates are sensitive to WWII. The major concern here is that, despite the inclusion of cohort fixed effects, the older cohorts of parents

Table 11 Robustness checks, 2SLS estimates

Dep. Var.: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: Post-WWII sample of parents (1935-1956)				
Parental education	0.470*	0.496*	0.558*	0.513
	(0.259)	(0.266)	(0.293)	(0.599)
Parental educ*female (parent)		0.049***	0.056***	0.046***
		(0.010)	(0.014)	(0.017)
Observations	5,247	5,247	2,639	2,608
Panel B: w/o cohort F.E. for children				
Parental education	0.498**	0.518*	0.603**	0.405
	(0.254)	(0.266)	(0.279)	(0.642)
Parental educ*female (parent)		0.042**	0.057***	0.026
		(0.020)	(0.022)	(0.047)
Observations	6,184	6,184	3,117	3,067
Panel C: Parent's years of schooling<11				
Parental education	0.980**	0.982**	0.614	1.479
	(0.492)	(0.495)	(0.603)	(1.112)
Parental educ*female (parent)		0.048***	0.057***	0.048*
		(0.015)	(0.020)	(0.026)
Observations	2,829	2,829	1,407	1,422
Panel D: Narrow windows around the pivotal cohorts (+/- 6 years)				
Parental education	0.813***	0.658***	0.896***	0.651*
	(0.147)	(0.188)	(0.138)	(0.391)
Parental educ*female (parent)		0.049***	0.054***	0.052***
		(0.010)	(0.012)	(0.015)
Observations	2,804	2,804	1,382	1,422
Panel E: Binary instrument				
Parental education	0.334**	0.360**	0.418**	0.302
	(0.153)	(0.146)	(0.190)	(0.242)
Parental educ*female (parent)		0.039***	0.043***	0.036***
		(0.008)	(0.012)	(0.011)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics. In Panel D, for the countries with more than one reform, I consider only the most recent reform. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

tend to be positively selected on their health and other unobservable factors because these individuals are still alive and able to participate in the SHARE surveys. While SHARE data do not allow for the elimination of survivor bias and the identification of a sample entirely unaffected by WWII, I can construct a postwar sample that takes into account the consequences of WWII that might have influenced the educational decisions of the early cohorts of parents by leading them to interrupt or postpone their academic careers. This postwar sample contains the younger cohorts of parents born during the 1935–1956 period. The 2SLS estimates reported in Panel A show that the effect of parental education is slightly larger once the prewar cohorts are dropped, but this model displays an identical pattern relative to the baseline specification (see Table 7, Panel A): the estimate increases from 0.49 to 0.55 years once I move from the full sample to the sample of sons and then decreases to 0.51 years and becomes insignificant when I consider the sample of

Table 12 Robustness checks, first stage estimates

Dep. Var.: Parent's education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: Post-WWII sample of parents (1935-1956)				
Compulsory education	0.228***	0.241***	0.321***	0.151
	(0.082)	(0.083)	(0.104)	(0.102)
Compulsory educ*female (parent)		-0.034***	-0.039**	-0.029
		(0.013)	(0.017)	(0.018)
Observations	5,247	5,247	2,639	2,608
Panel B: w/o cohort F.E. for children				
Compulsory education	0.217***	0.226***	0.301***	0.153
	(0.074)	(0.077)	(0.102)	(0.094)
Compulsory educ*female (parent)		-0.053***	-0.055***	-0.048***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067
Panel C: Parent's years of schooling<11				
Compulsory education	0.145***	0.148***	0.166**	0.110
	(0.047)	(0.047)	(0.071)	(0.079)
Compulsory educ*female (parent)		-0.007	-0.004	-0.008
		(0.008)	(0.011)	(0.011)
Observations	2,829	2,829	1,407	1,422
Panel D: Narrow windows around the pivotal cohorts (+/- 6 years)				
Compulsory education	0.222**	0.236**	0.306**	0.173
	(0.094)	(0.096)	(0.124)	(0.129)
Compulsory educ*female (parent)		-0.034**	-0.040*	-0.028
		(0.016)	(0.022)	(0.025)
Observations	2,804	2,804	1,382	1,422
Panel E: Binary instrument				
First reform	0.429***	0.477***	0.701***	0.242
	(0.160)	(0.169)	(0.225)	(0.231)
Second reform	0.620***	0.799***	0.853**	0.710**
	(0.204)	(0.228)	(0.343)	(0.307)
First reform*female (parent)		-0.107	-0.178	-0.030
		(0.129)	(0.163)	(0.174)
Second reform*female (parent)		-0.473*	-0.476	-0.407
		(0.273)	(0.453)	(0.564)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals), and socio-demographic characteristics. In Panel D, for the countries with more than one reform, I consider only the most recent reform. Standard errors clustered at the parents' country and cohort level are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

daughters. The more pronounced impact of maternal education on children's schooling persists, consistent with the baseline specification. Therefore, the results are quite robust to excluding the prewar cohorts.

I also investigate the robustness of my results to the exclusion of the child's year of birth. There might be a concern that the year of the child's birth is an endogenous decision because it may be affected by the level of parental education. In Panel B, I show that the coefficients of interest are very similar to the main specification with regard to the direction, magnitude and significance, with the only difference being that the mother's schooling no longer has an impact on daughters.

As a third check, following Black et al. (2005) and Oreopoulos et al. (2006), I conduct the analysis on the sample of the less educated parents who are most likely to be affected by reforms in mandatory schooling. Therefore, I examine the subset of children whose parents have 11 or fewer years of education. The 2SLS estimates presented in Panel C are similar in direction and significance to the benchmark specification, but the results are much less statistically precise. The lack of precision of the estimates is largely due to the small sample size. Contrary to my expectation, I find the estimated coefficients to be much larger in magnitude: the reduced number of observations is likely to bias my results, thus limiting this type of analysis. The first stage estimates (see Table 12, Panel C) indicate, as expected, that compulsory schooling laws are strongly correlated with lower levels of parental schooling, except for daughters.

While the small sample size severely limits the possibility of using cross-country school reforms as a regression discontinuity, one can imagine taking a narrow window of parental birth cohorts around the pivotal cohorts to correct for the impact of any long-run trends across birth cohorts. To do this, I restrict the sample to children with their parents born six years before or after the change in the laws.[21] The results reported in Panel D are consistent with my baseline model: I find evidence of a causal impact, although larger in magnitude, of parental education and a larger impact of maternal education on children's schooling.

As a final check, I assess the sensitivity of my estimates to the use of an alternative definition of the instrument. I construct a binary reform variable which is set to one for a given country for the post-reforms cohorts of parents, i.e., if parental year of birth exceeds the pivotal cohort. This allows me to distinguish between the treated and untreated cohorts of parents. Formally:

$$Treat_{ij}^{p} = \begin{cases} 1 & if\ \left(parentalyearofbirth\right)_i > (pivotal\ cohort)_j \\ \\ 0 & otherwise \end{cases} \tag{6}$$

where the variable $Treat_{ij}^{p}$ is now an indicator that takes value 1 if parent i who resides in country j belongs to a birth cohort that was exposed to the schooling reform. This implies that the treated individuals are born after the pivotal cohort. Importantly, some countries implemented more than one compulsory schooling law during my observation period: two laws were implemented in Sweden and France and three laws were implemented in the Netherlands and the Czech Republic. For the group of countries with more than one reform, I construct a treatment dummy for each additional reform using the same procedure as defined in (6).[22] Therefore, the number of indicators corresponds to the number of within country reforms. For the analysis in this study, it is important to note that the indicators are set to zero when additional reforms did not take place in a given country. One weakness of this binary instrument compared to the previous instrument based on the years of compulsory schooling is that it does not adequately capture the magnitude of the reform: a reform raising the number of years of compulsory schooling by one year (Austria, for example) is treated in the same manner as a reform increasing compulsory schooling by more than one year (Italy, for example). In this setup, the first stage is given by:

$$Edu_{ihj}^{p} = \delta_0 + \delta_1 Treat_{ij,l}^{p} + \pi X_{ihj} + \varphi^{p} + \varphi^{c} + \sigma_j + \upsilon_{ihj},\ l = 1, 2 \tag{7}$$

as mentioned above, $Treat^{p}_{ij,l}$ is a binary variable that equals 1 if the parent i in country j was affected by the $l-th$ educational reform and 0 otherwise.

The results are presented in Panel E. As expected, the magnitude of the effect of parental education is smaller than in the benchmark specification (see Table 7, Panel A), but, most importantly, the estimated coefficients remain unchanged with respect to the direction and significance in the full sample as well as in the sub-samples of sons and daughters.

7 Discussion

In this study, I found that maternal education is more important than paternal education for the academic achievement of children. While this finding is consistent with the established IV literature on the intergenerational transmission of human capital (Black et al. 2005; Chevalier 2004; Chevalier et al. 2011), the mechanisms through which a mother's education may affect her child's education are not entirely clear. In their studies, Chevalier (2004) and Chevalier et al. (2011) emphasize that the stronger effects of maternal education can be largely explained by the role of the mother as the main provider of childcare within the family. For example, mothers tend to spend more time breastfeeding, reading to their children, helping them with homework, and taking them outside. As noted by Black et al. (2003), this stronger effect of maternal education could also be attributed to other mechanisms such as positive assortative mating or the quantity/quality trade-off. However, because educated mothers are more likely to work, they may also have less time to stay at home and less time to devote to child care. However, Carneiro et al. (2013) counter that more educated mothers do not spend less time with their children partly because they have fewer children or less leisure time. They conclude that increased employment among more educated mothers does not have negative effects on children.

Whether it is plausible to assume that the intergenerational mobility coefficient is the same across different countries remains unexplored. To account for cross-country heterogeneity in parents' education, I add to model (1) a full set of interactions between parental years of schooling and the country dummies instrumented by the corresponding interactions between years of compulsory schooling and the country indicators. I then test the joint significance of this array of country-specific slopes in parental years of education and demonstrate that I do not reject the null hypothesis that the treatment effect of parental schooling is the same for all countries.[23] An alternative strategy to allow for the maximum level of heterogeneity at the country level would be to estimate separate models for each country. However, the number of observations in each country is too small to identify the treatment effects of parental education.[24] Overall, the evidence presented above suggests that the IV strategy on the pooled sample with common coefficients on all the variables is most appropriate for the data used in the present investigation.

8 Conclusion

An important component of human capital can be assessed by the extent of individuals' academic careers, measured by the number of years of education. When considering policies that improve the educational outcomes of new generations, a key question concerns the causal role of parents' education in influencing the educational outcomes of their offspring, that is, the intergenerational transmission of human capital. Does the increased education of parents cause higher levels of education of their children? Or, is the observed correlation between parents' and children's levels of education naive and due to unobserved covariates, such as innate ability? Are there differences in the education effects of mothers versus fathers, on daughters versus sons?

Although a large literature has attempted to answer these questions, the evidence remains largely mixed. In this paper, I employ the changes in compulsory schooling laws in Europe over the period 1920–1956 to explore the effect of parental education on the schooling performance of their children. In my preferred model, I do find some evidence of a causal relationship between parental and children's education. Specifically, I find that an additional year of parental education induced by the reform generates 0.44 years of additional schooling for their children. Furthermore, I provide evidence that the mother's schooling has a stronger impact than her husband's in determining the educational success of their offspring. This latter result is robust to the inclusion of the country-specific trends.

The findings of this paper reveal that increasing the education of less educated parents might have beneficial effects not only on the targeted generation but also on the educational outcomes of the next generation because family background characteristics affect the process of intergenerational transmission of human capital. These results highlight the long-term effectiveness of compulsory schooling laws in improving intergenerational outcomes in education. A mother's stronger influence over children's education suggests that supporting the education of mothers may represent an important avenue for educational policies.

Endnotes

[1] A more detailed summary of the literature on each identification strategy may be found in Holmlund et al. (2011); Björklund and Salvanes (2010); and Black and Devereux (2010). In particular, Holmlund et al. (2011) argue that the conflicting results across these three literatures arise mostly from the different identification strategies rather than from differences in the countries that have been studied.

[2] Altogether, 15 countries are covered by the first and the second waves, but I consider only a sub-group of 9 countries for which I have information on educational reforms during the period between 1920 and 1956. Following Brunello et al. (2012), I exclude Spain and Greece because the compulsory schooling laws occurred too late to identify a treatment group.

[3] The listed year corresponds to the year when a certain reform was passed, which may not be equal to year of implementation (e.g., the Austrian reform of 1962 was implemented in 1966; the French reform of 1959 was implemented in 1967).

[4] More details on the educational reforms in the Netherlands can be found in van Kippersluis et al. (2011) and Brunello et al. (2012).

[5] Pischke and von Wachter (2008) for more information on the educational reforms in the West German states.

[6] See http://www.unesco.org/education/information/nfsunesco/doc/isced_1997.htm for details on ISCED coding.

[7] The conversion table for the Czech Republic, which is not present in the Release Guide 2.5.0 for waves 1 and 2, was provided by the SHARE Country Team for the Czech Republic.

[8] After 1956, there is a substantial drop in the number of family respondents. The reason is that SHARE interviews people who are 50+. Therefore, for the 2006 wave, the people targeted by SHARE were born in 1956 or before.

[9] The family respondents answer the questions of the children's section. The couple's first person interviewed is the family respondent in the coverscreen. Because the family respondents are selected exclusively on the basis of the chronological order of interviews

per couple, the sample of parents can be arguably considered as a random sample. For further details, please see the Release Guide 2.5.0 for waves 1 and 2.

[10] In SHARE, questions about children's education are asked for a maximum of four children. Table A3 in Additional file 1: Appendix A displays the cross-country distribution of first-born and later-born children and Additional file 1: Table A4 reports the descriptive statistics. Importantly, in Additional file 1: Tables A5 and A6 I show that the main results remain substantially unchanged when including all children, both first-born and later-born children, thereby making my results relevant beyond the first-born children.

[11] The first SHARE interview took place in 2004 for all countries with the exception of the Czech Republic, which was surveyed in 2006. Table A7 in Additional file 1: Appendix A reports the descriptive statistics for children born after 1980 that are excluded from the analysis. As expected, the sample size is greatly reduced because there is a small fraction of parents that had their children after 1980.

[12] All of these samples contain individuals for whom information on educational attainment is not missing. Table A1 in Additional file 1: Appendix A reports the number of observations that are lost due to missing data on parents' and children's years of schooling for each country. Overall, it is reassuring to notice that the total number of missing values is relatively very low (87 individuals). Additional file 1: Table A2 reports the distribution of the postwar sample of parents across the countries.

[13] Because my sample includes only one Southern European country (Italy) and only one Eastern European country (Czech Republic), I included these two countries in a separate row with the Western European countries.

[14] One might argue that the birth year of the child is a potentially endogenous variable because parents can choose the timing of birth. However, in the robustness checks I show that the main results hold even when excluding cohort fixed effects for children.

[15] The fact that the identification of the effects of the reforms is made possible through differences in the timing of the changes in these laws across countries suggests some similarities with a differences-in-differences design.

[16] As for Germany, given that the instrument varies at the state level, clustering occurs at the level of the West German states. However, to account for potential correlation across West German states, I also cluster at the level of Germany finding that the 2SLS standard errors are virtually identical. Additionally, these standard errors do not change remarkably using the robust option without clustering.

[17] See Imbens and Angrist (1994).

[18] Because my model is just-identified, the conditional likelihood-ratio (CLR) test converges to the AR test, so there is no need to report both. In cases where the IV model contains more than one instrumental variable, additional weak-instrument robust tests, such as the LM test, are presented. Notice that these tests can only be applied to a model with one endogenous variable. A discussion of these issues can be found in Finlay et al. (2009).

[19] The Angrist-Pischke first stage F-test statistic refers to the first stage regression of parental education. The first stage regression of the interaction parental education*female has a much stronger power. Therefore, its Angrist-Pischke first stage F is omitted.

[20] An additional explanation is that there might be some correlation between the instrument and the unobserved factors that affect a child's outcome. However, previous studies using this variation have not questioned the exclusion restriction of the instrument.

[21] For the countries with more than one reform, I consider only the most recent reform.

[22] The third reform will not be used for causal interpretation because its identification would come only from the Czech Republic and the Netherlands.

[23] The results are available from the author upon request.

[24] The results are available from the author upon request. This country-specific analysis is usually not conducted in the economic literature using SHARE data (see, for example, Alessie et al. 2013; Brunello et al. 2009,2011,2012). Furthermore, the inclusion of only one country in the Mediterranean area (Italy) and one in Eastern Europe (Czech Republic) makes it difficult to produce separate estimates by European regions.

Additional file

Additional file 1: Appendix A: Supplemental Tables.

Competing interests
The IZA Journal of European Labor Studies is committed to the IZA Guiding Principles of Research Integrity. The author declares that he has observed these principles.

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