Intergenerational Transmission of Human Capital in Europe: Evidence from SHARE.

Luca Stella*

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 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 size of the estimated effect is large: an additional year of parental education will raise the child's education by 0.44 of a year. I also find that mothers' schooling is more important than fathers' schooling for the academic performance of their offspring. The results are robust to several specification checks.

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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 the causal relationship underlying the transmission of education between generations (see, for instance, Black, Devereux, and Salvanes, 2005; Oreopoulus, Page, and Stevens, 2006; Bjorklund and Salvanes, 2010).

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 the child's performance in school, then interventions that improve the educational attainment of the less educated parents should lead to increased human capital among their children, thus reducing the degree of inequality in opportunity in education.

However, the primary concern is that intergenerational educational estimates might not adequately account for the correlation of parental schooling with some unobserved inherited characteristics that might affect the academic achievement of their offspring. These correlations 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 (Plug, 2004; Björklund, Lindahl and Plug, 2006); and instrumental variables (Black, Devereux, and Salvanes, 2005; Oreopoulus, Page, and Stevens, 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, Lindahl and Plug (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 other studies for different countries (see, for example, Oreopoulus *et al.*, 2006 for the USA; Chevalier, 2004 for the UK; and Maurin *et al.*, 2008 for France). This is perhaps because the Scandinavian countries are characterized by relatively low levels of inequality with respect to economic and educational outcomes.

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 add 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 some recent studies (Brunello, Fort, and Weber, 2009; Brunello, Weber, and Weiss, 2012; Garrouste, 2010).

Based on these data, my main results demonstrate that: a) 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) the mother's schooling has a slightly stronger impact than that of her husband on the academic achievement of their offspring. 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, and proposed different strategies to identify exogenous variation in parental schooling. In the literature to date, there are three main streams investigating the causal effect of parental education on their offspring's education. These streams differ in the choice of the identification strategy. Below, I present a brief review of these studies and explain my contribution relative to the previous literature.[1]

The first stream 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 was conducted by Behrman and Rosenzweig (2002), who compare the schooling of children of twin mothers and twin fathers, identical in all characteristics with the exception of 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, 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 ed-

[1] A more detailed summary of the literature on each identification strategy may be found in Holmlund, Lindahl, and Plug (2008); Bjorklund and Salvanes (2010); and Black and Devereux (2010). In particular, Holmlund *et al.* (2008) argue that the conflicting results across these three literatures derive mostly from different identification strategies rather than from differences in the countries that have been studied.

ucation. 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 for adopted children available in the Nordic countries. In their study, Björklund, Lindahl and Plug (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 adoptive and biological parents' education to be important. Overall, these 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, Devereux and Salvanes (2005), which utilizes the Norwegian schooling reforms that occurred in different municipalities for the period 1959-1973, providing little evidence for the causal effects of parental education. Overall, they conclude that while there is a positive but small intergenerational educational effect between mothers and their sons, the father's schooling has no impact. Similar results were obtained for Sweden in a more recent paper by Holmlund, Lindahl and Plug (2008) applying the same strategy. 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, show that increasing the education of either parent has a negative and significant effect on the probability that a child repeats a year of school. Similarly, 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 in 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 performed a comparative analysis of the educational reforms undertaken at the country level. This observation strengthens my argument that using this variation in Europe is a novel contribution to the literature that can improve our understanding of how and why parental education may affect children's outcomes by accounting for the effects of different 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 related to 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), and contains detailed information on a broad set of variables: demographics, socio-economic char-

[2] Altogether, the countries covered by wave 1 and 2 are 15, but in this study I consider only a sub-group of nine countries for which I have information on the educational reforms in the period between 1920 and 1956. Consistent with Brunello *et al.* (2012), I cannot include Spain and Greece because the compulsory schooling laws occured too late to identify a treatment group.

acteristics, self-reported health as well as social and family networks. In this paper, I present evidence for nine countries, where I could compute some key educational variables. These countries range 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 next use data on reforms in the minimum school leaving age across the above-mentioned European countries, relying on recent works by Brunello *et al.* (2009), Brunello *et al.* (2012) and Garrouste (2010). As in Brunello *et al.* (2012), Table 1 presents an historical overview of educational reforms that affected cohorts of parents from the 1930s until the late 1960s: for each country it reports the year of the reform, the *pivotal cohort*, i.e., the first birth 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 finally the age at school entry. It is worth noticing that the countries selected in this study have extended the school leaving age by one year or longer, and that the Netherlands and the Czech Republic have experienced only a temporary reduction in years of compulsory schooling.[3] Strikingly, although Italy had a lower initial level of mandatory schooling (5 years), it made substantial progress during the postwar period (8 years).[4] 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]

[Table 1 - around here]

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

[4]This observation could be extended to other Mediterranean countries, such as Spain, which is not included in my sample, though.

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

The key variable of interest in this analysis is the educational attainment of parents and children. I measure educational attainment with years of schooling. One unusual feature of the dataset I use is that it contains direct information on years of schooling for both parents and children. However, while for the countries in the first wave data on years of education are provided and are defined according to the ISCED-97 criteria,[6] for the countries in the second wave there is information available on the country specific ISCED-97 codes but not directly on years of education. In my analysis, the Czech Republic is the only country that comes from the second wave and is not present in the first wave. I addressed this lack of information of 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 in particular.[8] Overall, these cohorts were affected by the reforms of mandatory schooling that came into effect gradually across the European countries, and by comparing their year of birth with the pivotal cohort I am able to determine whether parents were exposed to the 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 are en-

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

[7] The conversion table for Czech Republic, which is not present in the Release Guide 2.5.0 Waves 1 & 2, has been provided by the Country Team.

[8] The motivation for this upper bound is that the number of family respondents born after 1956 drops substantially. Moreover, the remaining respondents are made up almost exclusively by females.

titled to respond to questions in the children's section on behalf of the couple.[9] Therefore, fathers or mothers 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 not sensitive to excluding the prewar cohorts. The distributions of the full and postwar samples of parents across the countries are presented in Table 2 and 3.

[Table 2 - around here]

[Table 3 - around here]

Consistent with, among others, Black *et al.* (2005), I restrict attention to first born children.[10] The cohorts of interest are born between 1956 and 1980. The choice of 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 aged at least 24 at the time of the

[9]See the Release Guide 2.5.0 Waves 1 & 2 for more details. The family respondents can be arguably considered as a random sample.

[10]In SHARE, the questions on the children's education are asked to a maximum of four children.

interview.[11] Table 4 reports their distribution by country.

[Table 4 - around here]

After these restrictions, the final full sample of parents consists of 7,635 family respondents: 4,201 (55%) fathers and 3,434 (45%) mothers, while the final sample of children consists of 6,891 siblings:[12] 3,470 (50.4%) sons and 3,421 (49.6%) daughters. Finally, the post-WWII sample of parents contains 5,997 family respondents: 3,146 (52.5%) fathers and 2,851 (47.5%) mothers.[13] Summary statistics reported in Table 5 show, as expected, that fathers are slightly older, are more educated and have substantially higher earnings than their spouses. Particularly striking is that the second generation of children has a considerably higher level of schooling than their parents (13.17 versus 10.72 years of schooling). However, part of the positive association between parents' and childrens' education might reflect the positive correlation with unobserved ability.

[Table 5 - around here]

In Figure 1, I analyze differences across countries in the pattern of educational attainment between the cohorts of parents and children. The vertical and horizontal axes represent 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 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

[11] The interview took place in 2004 for all countries with the exception of Czech Republic where it occurred in 2006.

[12] Notice that this number is smaller than the sample size of the parents given the choice of the cohort interval for children.

[13] All these samples contain individuals for which information on education is not missing.

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.

[Figure 1 - around here]

4 Empirical Specification

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

$$Edu^c_{ihj} = \alpha + \beta Edu^p_{ihj} + \gamma X_{ihj} + \tau^p + \tau^c + \eta_j + \epsilon^p_{ihj} \tag{1}$$

where the superscripts c and p refer to child and parental characteristics, respectively. The dependent variable Edu^c_{ihj} denotes 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 years of schooling of the family respondent Edu^p_{ihj}. A key element of my approach is that I include 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 intervals), which capture any systematic difference in school outcomes across parental cohorts. In model (1), I also include birth cohort fixed effects for children τ^c (in 1-year intervals) to control for cohort trends in education[14] and

[14]One might argue that 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.

account for the possibility that some children might not have finished school at the time of the interview. 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 childrens' 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 doing so, I am able to capture the different impacts of maternal and paternal education on children's education. Formally, I estimate the following specification:

$$Edu^c_{ihj} = \alpha + \beta Edu^p_{ihj} + \lambda Edu^p_{ihj} * gender^p + \gamma X_{ihj} + \tau^p + \tau^c + \eta_j + \epsilon^p_{ihj} \quad (2)$$

where $gender^p$ 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 years of schooling of the parents. Consistent with the existing literature (see, for example, Black *et al.*, 2005; and Oreopoulus *et al.*, 2006), this identification strategy is recognized to be valid, as these changes in the 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 returns to schooling and Brunello *et al.* (2011) to investigate the effect of schooling on health. Formally, the instrument is constructed as follows:

$$Reform_j^p = \begin{cases} (ycs)^A & \text{if parental year of birth}>\text{pivotal cohort} \\ \\ (ycs)^B & \text{otherwise} \end{cases} \tag{3}$$

where ycs represents 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:

[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 presents some similarities with a differences-in-differences identification strategy.

$$Edu^p_{ihj} = \delta_0 + \delta_1 Reform^p_j + \pi X_{ihj} + \varphi^p + \varphi^c + \sigma_j + \upsilon_{ihj} \tag{4}$$

where Edu^p_{ihj} is instrumented with $Reform^p_j$, the compulsory years of schooling in the respective country and cohort.

Similarly, the first stage for model (2) can be written as:

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

Therefore, in equation (5) I need to use as an instrument not only the years of compulsory schooling but also the interaction between compulsory schooling and the gender of the parent. There are two points to note on this instrumental variables strategy. First, as 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] However, as pointed out 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.

[16]As for Germany, given that the instrument varies at the state level, clustering is 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 obtaining that the 2SLS standard errors are almost identical. Also, these standard errors do not change remarkably using the robust option without clustering.

[17] See Imbens and Angrist (1994).

5 Main Results

5.1 Association between the Schooling of Parents and their Children

Table 6 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 a 0.32 increase in the number of years of schooling for children. This coefficient is significant and robust to the inclusion of controls for parental birth cohort and socio-demographic characteristics (column 2), including the gender of the children and household size. When separately controlling for country fixed effects (column 3) and cohort fixed effects for children (column 4), I continue to find that parental education is positively and significantly associated with children's education, although the coefficients are slightly reduced to 0.3 and 0.29, respectively.

[Table 6 - around here]

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, equal to one if the family respondent is the mother. The estimates for the most general specification are reported in Table 7. Column 1 corresponds to column 4 of Table 6. 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 latter and paternal education, the coefficient on the interaction term is highly statistically significant. Interestingly, this positive sign appears to be 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 7, I find similar results when dividing the sample into sons (column 3) and daughters (column 4). Columns 3 and 4 also seem to suggest that the relationship between maternal schooling with the schooling outcomes of her sons is somewhat stronger.

[Table 7 - around here]

Overall, my OLS estimates confirm a strong positive intergenerational correlation in education even when country fixed effects are controlled for or 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 also 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 4 in Table 6), a one unit increase in the household size is associated with a 0.17 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 Table 8 and 12, 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. In Table 8, I notice that while 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) reduces the significance of the 2SLS estimate, but it is still maintained at the 10 percent threshold. With regard to the magnitudes, 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 extremely important to control for country fixed effects because countries with a higher level of education might want to anticipate or delay the implementation of schooling reforms. Birth cohort fixed effects for parents need also to be included to capture any systematic differences in school outcomes across parental cohorts. Since my identification strategy is based on the inclusion of these fixed effects to account for any selection on unobservables coming from individuals residing in a particular country and belonging to a given cohort, I choose the most general specification reported in column 4 of Table 8 as my preferred one. In this model, my results suggest that, once these sources of selection are accounted for, 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.

[Table 8 - around here]

Table 8 also reports the corresponding Angrist-Pischke F statistics of the first stage 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 around 7.5, which is below the cutoff value of 10 suggested by Bound, Jaeger, and Baker (1995) and Staiger and Stock (1997), thus raising concerns about a lack of power in my identification strategy.

To further investigate this issue, I 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 that provides the Anderson-Rubin (AR) statistic (Anderson and Rubin, 1949) and, as a reference, the standard Wald test for specifications 3 and 4 in Table 8.[18] As one could expect given the relatively low value of Angrist-Pischke statistics, the AR p-value and confidence intervals are larger than the nonrobust Wald counterparts, but the differences are limited, and most importantly the AR p-value is still on the border of statistical significance at approximately the 10 percent threshold. These results imply that, when also accounting for the presence of a weak instrument, the treatment effects of parental education remain marginally statistically significant.

[Table 9 - around here]

I next investigate the first stage estimates reported in Table 10. These estimates show that across all specifications the reform is strongly and positively correlated with the years of schooling of the parents, and that its t statistic is

[18]Since my model is just-identified, the conditional likelihood-ratio (CLR) test converges to the AR test, so there is no point in reporting both. In case 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 this can be found in Finlay *et al.* (2009).

above 2.7 even when controlling for country and cohort fixed effects. Quantitatively, in my preferred model (column 4) one additional year of compulsory education increases schooling by 0.21 years.

[Table 10 - around here]

For the above reasons, and in light of the Angrist and Pischke (2009) motto: "just-identified 2SLS is approximately unbiased", I conclude that the issue of weak instrument bias may be of somewhat less concern, and that there is some evidence of a causal effect of parental education on the educational attainment of their children. Table 11 summarizes the results for the preferred specification. The first column reports OLS estimates from a regression of the child's education on the education of the parents. In the second column, I show 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.

[Table 11 - around here]

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 exploring the role of fathers and mothers in explaining the transmission of human capital to sons and daughters. To do this, I proceed in two steps.

First, adding an interaction term 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 (column 4 in Table 8) uses as instruments not only the years of compulsory schooling but also interaction term between compulsory schooling and the gender of the parent. From the 2SLS estimates, reported in Table 12, it can be noted that, when also controlling for the differential impacts of mothers and fathers (see column 2), the results remain substantially unchanged with respect to the direction, magnitude, significance and power of identification. Similar to the results from the OLS estimates, I find the coefficient on the interaction between years of education and parental gender to be highly statistically significant (at the 1% level) and positive, thus suggesting that mothers' education is somewhat more important than that of fathers.

[Table 12 - around here]

Second, in an attempt to disentangle the treatment effects of parental schooling on sons from those 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 12), respectively. When conducting the analysis on sons, I find the coefficient on parental education to be statistically significant and larger than the one 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 on 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 when discussing the results.

A potential explanation for the non-significant effects of parental education on daughters can be largely attributed to the weak first stage relationship between the reform and years of schooling of the parents when examining the sample of daughters (column 4 in Table 13): the t statistic for the reform is around 1.6 compared to around 3 for sons (column 3). Furthermore, the Angrist-Pischke first stage F 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.

[Table 13 - around here]

Regardless of the specification, I find the IV estimates to be higher than their OLS counterparts. While this result might appear to contradict intuition regarding omitted variable bias given the positive correlation between parental education with unobserved ability, it is consistent with several studies that use 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 on only 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 ratio of the IV estimate to the OLS estimate in the entire sample and in the sample of sons and daughters ranges between 1.5 and 2.4. Similar ratios have been found in Oreopoulus *et al.* (2006), Angrist and Krueger (1991) and Staiger and Stock (1997).

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

[20] A further explanation is that there might be some correlation between the instrument and the unobserved factors that affect child's outcome. However, previous studies using this variation have not been concerned about the validity of the instrument.

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. I start 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 tend to be positively selected on their health and other unobservable characteristics because these individuals are still alive and able to participate in the SHARE interviews. 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 accounts for the consequences of WWII that might have influenced the educational decisions of the early cohorts of parents 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 Table 14 show that the effect of parental education becomes slightly larger once the prewar cohorts are dropped, but displays an identical pattern: 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 daughters. In addition, consistent with my baseline specification in Table 12, I continue to find a more pronounced impact of maternal than paternal education. Therefore, the results are quite robust to excluding the prewar cohorts.

I further investigate the robustness of my results to the exclusion of the child's year of birth. As mentioned above, there is 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. As one can see in Table 15, I show that my 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 Oreopoulus *et al.* (2006), I conduct my analysis on the sample of the less educated parents who are most likely to be affected by the reforms of mandatory schooling. Therefore, I look at the subset of children whose parents have 11 or fewer years of education. The 2SLS estimates presented in Table 16 are similar in direction and significance to the benchmark specification. As expected, the sample size is greatly reduced, and the results are much less statistically precise. Contrary to my expectation, I find the estimated coefficients to be much larger in size: I am concerned that the small sample size is likely to bias my results, thus limiting this type of analysis. However, the first stage estimates (see Panel B of Table 16) show, as expected, that compulsory schooling laws are strongly correlated with lower levels of parental schooling, except for daughters.

A further sensitivity check is based on the inclusion of country-specific linear trends in parental cohorts to control for potential differences across countries that evolve over time, e.g., changes in society. The results are shown in Table 17. My IV estimates become larger and less precise than those that do not control for these trends, but are qualitatively similar: I find a positive although insignificant effect of parental education, and positive significant effects of the mothers' schooling, except on daughters. I suspect that the main reason for this lack of precision when including these additional controls is related to the limited amount of variation in my data.

Finally, I assess the robustness of my findings to the use of an alternative definition of the instrument. I construct a binary reform variable which is set to one in 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:

$$Treat_j^p = \begin{cases} 1 & \text{if parental year of birth} > \text{pivotal cohort} \\ \\ 0 & \text{otherwise} \end{cases} \tag{6}$$

where $Treat_j^p$ is now an indicator taking a value of one if the parent in a given country j belongs to a birth cohort that was exposed to the schooling reform and zero otherwise. This implies that the treated are the individuals born after the pivotal cohort. Importantly, in some countries more than one compulsory schooling law was implemented in my observation period: from two laws in Sweden and France to three laws in the Netherlands and Czech Republic. For this 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).[21] Therefore, the number of indicators corresponds to the number of the within country reforms. For the analysis in this study, it is important to note that the indicators are set to zero when an additional reform did not take place in a given country. One weakness of this binary instrument with respect to the previous instrument based on the years of compulsory schooling is that it does not adequately capture the magnitude of the reform: one reform raising the number of years of compulsory schooling by one year (as in Austria for example) is treated in the same manner as one increasing compulsory schooling by more than one year (as in Italy for example). In this setup, the first stage is given by:

[21] I exclude the third reform because it involves only Czech Republic and Netherlands. Therefore, identification would be given only by these two countries.

$$Edu^p_{ihj} = \delta_0 + \delta_1 Treat^p_{l,j} + \pi X_{ihj} + \varphi^p + \varphi^c + \sigma_j + \upsilon_{ihj} \text{ , } l = 1, 2 \quad (7)$$

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

The results are presented in Table 18. As expected, the magnitude of the effects of parental education is lower than that obtained in the benchmark specification (see Table 12), but, most importantly, the estimated coefficients remain unchanged with respect to the direction and significance in the full sample as well as in the samples of sons and daughters.

7 Discussion

In this section, I discuss my empirical findings. In particular, I focus on two issues: a) the role of mothers; b) whether it is plausible to constrain the coefficient of intergenerational transmission of education to be the same across countries.

7.1 Do Mothers matter more?

In this study, I found that maternal education is more important than paternal education for the academic achievement of their children. While this finding appears to be in line with the established IV literature on the intergenerational transmission of human capital (Black *et al.*, 2005; Chevalier, 2004; Chevalier *et al.,* 2010), 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.* (2010) emphasize that these 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 or taking them outside. As noted by Black *et al.* (2003), this stronger incidence of maternal education could also be attributed to other mechanisms, such as positive assortative mating or the quantity/quality trade-off.

On the other hand, it also true that, because educated mothers are more likely to work, they should have less time to stay at home, and therefore less time to devote to child care. However, Carneiro *et al.* (2011) criticize this idea, arguing that more educated mothers do not spend less time with their children partly because they have fewer children or less leisure time. Their conclusion is that the increase in employment of more educated mothers does not have negative effects on children.

7.2 What explains cross-country differences?

In this paper, I assumed that the coefficient of intergenerational mobility is the same across different countries. This leads to the question of whether this is a legitimate assumption. To allow the coefficient to vary by country, I include in my model (1) a full set of interactions between parental years of schooling and the country dummies instrumented by the interactions between compulsory schooling and the country dummies. I then test the joint significance of this array of country specific slopes in years of education. In unreported results, I show that I do not reject the null of identical slope coefficients across countries.[22] This finding supports the hypothesis that my sample is poolable across countries, and that the main effect is driven by the coefficient on parental education constrained to be equal across countries.

Nevertheless, in Table 19 I run a separate OLS regression for each country.[23]

[22] The results are available upon request.
[23] All specifications include socio-demographic controls as well as cohort fixed effects for parents and children.

The magnitude of the estimated coefficient ranges between 0.15 and 0.35 of a year. Not surprisingly, while in Sweden I find the smallest impact of parental education (0.15 years),[24] in Italy and the Czech Republic the intergenerational effects are much larger in size, and Central European countries present point estimates that vary inbetween. An exception to this pattern, however, is provided by Denmark. It is difficult to explain why this Scandinavian country has such a large point estimate (0.36 years). Here, I am concerned that years of education might have some issues regarding how the ISCED coding was conducted in this country. This problem seems to also be present when considering the distribution of the sample of parents and children: in Denmark, the number of individuals for which information on education is not missing is considerably smaller than the other countries. Therefore, information on educational attainment may not be particularly representative for the Danish population.

Finally, in Table 19, one can also notice that, consistent with what was found previously, in each country maternal schooling is more strongly correlated with children's education, with the exception of Denmark. These maternal effects seem to be particularly important for France and Italy.

8 Conclusion

In this paper, I used the changes in compulsory schooling laws in Europe over the period 1920-1956 to study the effect of parental education on the schooling performance of their children. My estimates from a sample of nine different European countries suggest that there is some evidence of a causal relationship between parental and children's education. The magnitude of the effect is large:

[24]It is reassuring that my OLS estimate is very similar to the estimate obtained by Holmlund *et al.* (2011) for Sweden.

an additional year of parental education induced by the reform generates 0.44 years of additional schooling for the children. Furthermore, I find evidence that the mother's schooling has a stronger impact than her husband's in determining the educational success of their offspring. The findings of this paper reveal that increasing the education of parents has positive effects on the educational outcomes for the next generation, as family background characteristics have a substantial impact on the intergenerational transmission process.

Overall, these results demonstrate the effectiveness of educational interventions in improving intergenerational outcomes in education, as well as the importance of the role of the mothers in determining the transmission of educational attainment to children. They also suggest that supporting the education of mothers may represent an important avenue for educational policies.

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Tables and Figures

Figure 1: Trend in Education of Parents and Children, by Country

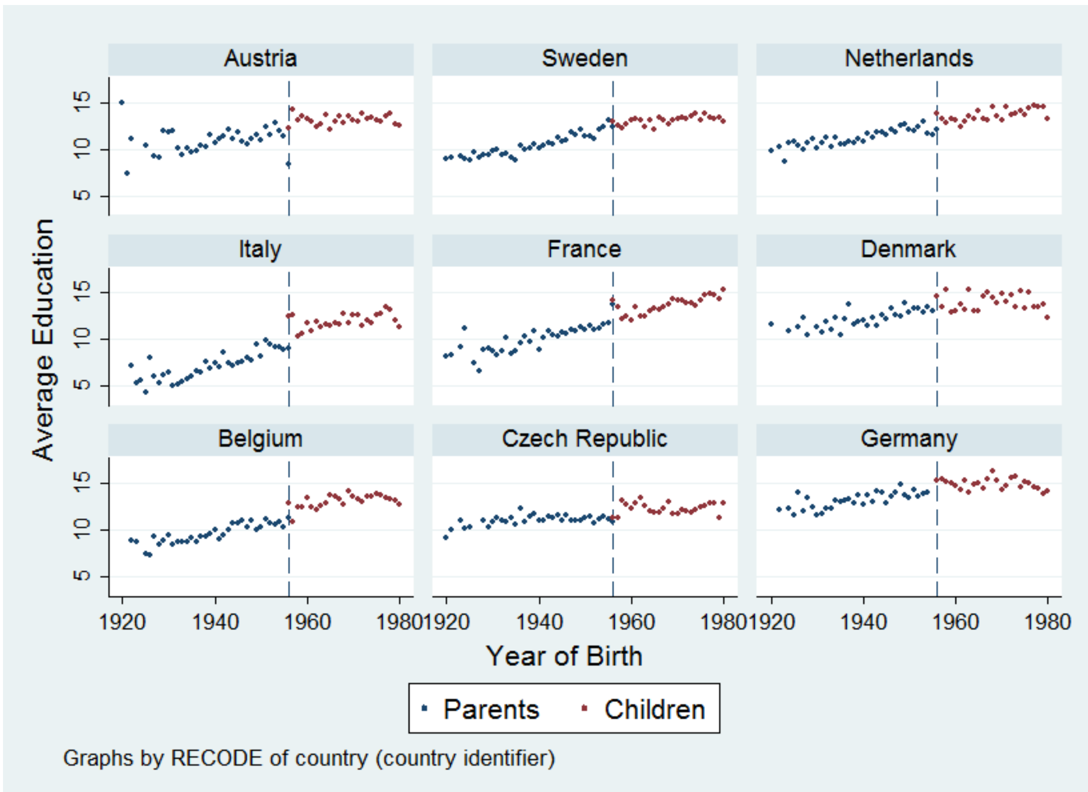

Table 1: Compulsory School Reforms, by Country

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, Weber and Weiss (2012).

Table 2: Full Sample of Parents (1920-1956), by Country and Gender

Country	Fathers	Mothers	Total
Austria	375	203	578
Belgium	566	317	883
Czech Republic	454	432	886
Denmark	250	189	439
France	478	318	796
Germany	431	396	827
Italy	558	566	1,124
Netherlands	609	540	1,149
Sweden	480	473	953
Total	4,201	3,434	7,635

Table 3: Post-WWII Sample of Parents (1935-1956), by Country and Gender

Country	Fathers	Mothers	Total
Austria	290	170	460
Belgium	415	260	675
Czech Republic	356	387	743
Denmark	194	143	337
France	355	268	623
Germany	326	326	652
Italy	409	470	879
Netherlands	456	457	913
Sweden	345	370	715
Total	3,146	2,851	5,997

Table 4: Sample of Children (1956-1980), by Country and Gender

Country	Sons	Daughters	Total
Austria	225	257	482
Belgium	363	366	729
Czech Republic	363	360	723
Denmark	155	165	320
France	332	308	640
Germany	346	326	672
Italy	495	456	951
Netherlands	465	459	924
Sweden	373	370	743
Total	3,117	3,067	6,184

Table 5: Summary Statistics, Sample of Parents and Children

Variable	Observations	Mean	Std. Dev.
First Child			
Age	6,184	35.88	6.49
Education	6,184	13.25	2.84
Female (%)	6,184	0.49	0.5
Mothers and Fathers together			
Age	7,635	62.56	8.56
Education	7,635	10.72	3.74
Earnings	3,803	27,842	35,204
Fathers			
Age	4,201	63.49	8.79
Education	4,201	11.04	3.79
Earnings	2,292	33,205	40,027
Mothers			
Age	3,434	61.42	8.11
Education	3,434	10.34	3.65
Earnings	1,511	19,706	24,077

Notes: All the samples contain individuals for which information on education is not missing. Education is measured with years of schooling and is defined according to the ISCED-97 criteria. Earnings are expressed in euros, even for the observations belonging to the non-euro countries, and do not distinguish between employment and self-employment.

Table 6: Effects of Parents' Education, Naive OLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)
parental education	0.325***	0.327***	0.305***	0.286***
	(0.011)	(0.011)	(0.011)	(0.011)
female (child)		0.204***	0.217***	0.225***
		(0.066)	(0.065)	(0.063)
household size		-0.135***	-0.121***	-0.170***
		(0.042)	(0.043)	(0.043)
Socio-demographic characteristics		Yes	Yes	Yes
Cohort F.E. for parents		Yes	Yes	Yes
Country F.E.			Yes	Yes
Cohort F.E. for children				Yes
Observations	6,184	6,184	6,184	6,184
R^2	0.178	0.190	0.228	0.243
Mean of Dependent Variable	13.25			
Std. Dev.	2.84			

Notes: Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. Birth cohort dummies for parents and children are in 1-year intervals. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of Parents' Education, Naive OLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
parental education	0.286***	0.231***	0.231***	0.228***
	(0.011)	(0.013)	(0.019)	(0.016)
parental educ*female (parent)		0.041***	0.043***	0.040***
		(0.006)	(0.009)	(0.008)
household size	-0.170***	-0.167***	-0.152**	-0.164***
	(0.043)	(0.043)	(0.063)	(0.055)
Observations	6,184	6,184	3,117	3,067
Mean of Dependent Variable	13.25	13.25	13.14	13.34
Std. Dev.	2.84	2.84	2.89	2.78

Notes: All specifications include controls for country dummies and birth cohort dummies for parents and children (in 1-year intervals). Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Effects of Parents' Education, 2SLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)
parental education	0.281***	0.367***	0.498**	0.437*
	(0.057)	(0.054)	(0.254)	(0.262)
female (child)		0.206***	0.224***	0.229***
		(0.066)	(0.066)	(0.065)
household size		-0.114**	-0.123***	-0.150***
		(0.048)	(0.044)	(0.056)
Socio-demographic characteristics		Yes	Yes	Yes
Cohort F.E. for parents		Yes	Yes	Yes
Country F.E.			Yes	Yes
Cohort F.E. for children				Yes
Observations	6,184	6,184	6,184	6,184
R^2	0.175	0.188	0.179	0.214
Mean of Dependent Variable	13.25			
Std. Dev.	2.84			
Angrist-Pischke First Stage F (1,503)	42.99	38.23	8.58	7.47

Notes: Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. Birth cohort dummies for parents and children are in 1-year intervals. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Weak-Instrument Robust Tests for models (3) and (4) in Table 8

Endogenous Variable: Parent's 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.

Table 10: First Stage

Dependent Variable: Parental Education	(1)	(2)	(3)	(4)
compulsory education	0.629***	0.601***	0.224***	0.214***
	(0.096)	(0.098)	(0.075)	(0.077)
Socio-demographic characteristics		Yes	Yes	Yes
Cohort F.E. for parents		Yes	Yes	Yes
Country F.E.			Yes	Yes
Cohort F.E. for children				Yes
Observations	6,229	6,229	6,229	6,229
R^2	0.061	0.080	0.221	0.258

Notes: Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. Birth cohort dummies for parents and children are in 1-year intervals. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Effects of parental education in the preferred model

	(1) OLS	(2) Reduced-Form	(3) First Stage	(4) 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		0.096	0.214***	
		(0.061)	(0.077)	
Observations	6,184	6,219	6,229	6,184
R^2	0.243	0.141	0.258	0.214
Angrist-Pischke First-Stage F	7.47			
Anderson-Rubin test p-value	0.134			

Notes: All specifications include controls for country dummies and birth cohort dummies for parents and children (in 1-year intervals). Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Effects of Parents' Education, 2SLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
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 Dependent Variable	13.25	13.25	13.14	13.34
Std. Dev.	2.84	2.84	2.89	2.78
Angrist-Pischke First Stage F	7.47	6.99	7.56	2.00

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. The Angrist-Pischke first stage F refers to the first regression of parental education; the regression of education*female has much stronger power, thus the AP first stage F is omitted. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 13: First stage

Dependent Variable: Parental Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
compulsory education	0.214***	0.232***	0.306***	0.158*
	(0.077)	(0.078)	(0.103)	(0.095)
compulsory educ*female (parent)		-0.052***	-0.056***	-0.045***
		(0.012)	(0.016)	(0.017)
Observations	6,229	6,229	3,133	3,096

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. *** $p<0.01$, ** $p<0.05$,* $p<0.1$.

Table 14: 2SLS, Post-WWII Sample

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
Panel A: 2SLS				
Outcome: child's education				
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
Angrist-Pischke First Stage F	7.82	7.49	8.66	1.83
Panel B: First Stage				
Outcome: parental education				
compulsory education	0.236***	0.248***	0.333***	0.150
	(0.082)	(0.084)	(0.105)	(0.102)
compulsory educ*female (parent)		-0.033***	-0.040**	-0.027
		(0.012)	(0.017)	(0.017)
Observations	5,285	5,285	2,651	2,634

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1935 to 1956. The Angrist-Pischke first stage F refers to the first regression of parental education; the regression education*female has much stronger power, thus the AP first stage F is omitted. *** p<0.01, ** p<0.05, * p<0.1.

Table 15: 2SLS, without cohort F.E. for children

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
Panel A: 2SLS				
Outcome: child's education				
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
Angrist-Pischke First Stage F	8.57	7.64	10.28	1.44
Panel B: First Stage				
Outcome: parental education				
compulsory education	0.224***	0.255***	0.350***	0.171*
	(0.075)	(0.077)	(0.099)	(0.101)
compulsory educ*female (parent)		-0.091***	-0.091***	-0.091***
		(0.012)	(0.016)	(0.016)
Observations	6,229	6,229	3,133	3,096

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. The Angrist-Pischke first stage F refers to the first regression of parental education, the regression education*female has much stronger power, thus the AP first stage F is omitted. *** p<0.01, ** p<0.05, * p<0.1.

Table 16: 2SLS, Parent's years of schooling<11

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
		Panel A: 2SLS		
Outcome: child's education				
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
Angrist-Pischke First Stage F	9.58	9.53	5.52	1.82
		Panel B: First Stage		
Outcome: parental education				
compulsory education	0.141***	0.144***	0.158**	0.113
	(0.047)	(0.047)	(0.071)	(0.079)
compulsory educ*female (parent)		-0.005	-0.005	-0.005
		(0.008)	(0.011)	(0.012)
Observations	2,851	2,851	1,413	1,438

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. The Angrist-Pischke first stage F refers to the first regression of parental education; the regression of education*female has much stronger power, thus the AP first stage F is omitted. *** p<0.01, ** p<0.05, * p<0.1.

Table 17: 2SLS, linear country trend

Sample	(1) Overall	(2) Overall	(3) Sons	(4) Daughters
Panel A: 2SLS				
Outcome: child's education				
parental education	0.794	0.887	1.064	0.909
	(0.743)	(0.779)	(0.975)	(1.455)
parental educ*female (parent)		0.065**	0.079*	0.060
		(0.032)	(0.043)	(0.051)
Observations	6,184	6,184	3,117	3,067
Angrist-Pischke First Stage F	1.56	6.99	1.38	0.41
Panel B: First Stage				
Outcome: parental education				
compulsory education	0.110	0.129	0.152	0.100
	(0.082)	(0.083)	(0.110)	(0.108)
compulsory educ*female (parent)		-0.050***	-0.056***	-0.043**
		(0.012)	(0.016)	(0.017)
Observations	6,229	6,229	3,133	3,096

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. The Angrist-Pischke first stage F refers to the first regression of parental education; the regression of education*female has much stronger power, thus the AP first stage F is omitted. *** p<0.01, ** p<0.05, * p<0.1.

Table 18: 2SLS, Binary Instrument

Sample	(1) Overall	(2) Sons	(3) Daughters
Panel A: 2SLS			
Outcome: child's education			
parental education	0.334**	0.454**	0.140
	(0.153)	(0.203)	(0.265)
Observations	6,184	3,117	3,067
Angrist-Pischke First Stage F	8.29	7.34	3.49
Panel B: First Stage			
Outcome: parental education			
first reform	0.431***	0.615***	0.237
	(0.162)	(0.211)	(0.216)
second reform	0.616***	0.689**	0.538**
	(0.206)	(0.284)	(0.225)
Observations	6,229	3,133	3,096

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 parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. *** p<0.01, ** p<0.05, * p<0.1.

Table 19: Naive OLS, by Country

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	Austria	Belgium	Czech Republic	Denmark	France	Germany	Italy	Netherlands	Sweden
parental education	0.188***	0.324***	0.353***	0.356***	0.222***	0.246***	0.316***	0.260***	0.152***
	(0.030)	(0.031)	(0.050)	(0.060)	(0.027)	(0.033)	(0.030)	(0.025)	(0.021)
parental educ*female (parent)	0.056**	0.023	0.036***	0.005	0.074**	0.034***	0.061**	0.050***	0.026**
	(0.022)	(0.023)	(0.012)	(0.035)	(0.029)	(0.013)	(0.026)	(0.012)	(0.010)
Observations	482	729	723	320	640	672	951	924	743
R^2	0.261	0.257	0.232	0.293	0.243	0.231	0.263	0.244	0.174
Mean of Dependent Variable	13.11	13.15	12.27	14.07	13.79	14.92	11.99	13.68	13.06
Std. Dev.	2.51	2.86	2.48	2.90	3.15	2.29	3.31	2.60	1.85

Notes: All specifications include controls for birth cohort dummies for parents and children (in 1-year intervals), and socio-demographic characteristics. Standard errors clustered at the parent's country and parent's cohort level are reported in parentheses. The sample of parents covers the cohorts born from 1920 to 1956. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.