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More Schooling, More Children: Compulsory Schooling Reforms and Fertility in Europe

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Abstract

We study the relationship between education and fertility, exploiting compulsory schooling reforms in Europe as source of exogenous variation in education. Using data from 8 European countries, we assess the causal effect of education on the number of biological kids and the incidence of childlessness. We find that more education causes a substantial decrease in childlessness and an increase in the average number of children per woman. Our findings are robust to a number of falsification checks and we can provide complementary empirical evidence on the mechanisms leading to these surprising results.

JEL Classification: I2, J13
Keywords: Education, Fertility, Causal effects

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1 Introduction

Conventional wisdom on fertility rates tells us that more education reduces fertility. Skirbekk (2008) provides a meta-study on the correlation of social status, wealth and education with fertility: while in previous centuries higher social status was positively correlated with the number of children, this relation shifted to a negative or neutral one in the last century. Only since the beginning of the 20th century, data on education became available: out of 528 samples, in more than 88 percent the higher educated group had lower average fertility. Whereas fertility generally dropped in most developed countries, the fertility gap between high and low educated women has not converged (Skirbekk, 2008, p. 160). The situation is similar for developing countries (Martin, 1995; Strauss and Thomas, 1995).

These correlations do not necessarily imply a causal relationship running from education to fertility; they may instead be due to reverse causation or third factor problems: early pregnancies might impede further education or school drop-outs might also have a personality prone to early motherhood. While in the surveys above no causal papers were included, available causal studies relying on compulsory schooling reforms do not show a clear picture: most studies show that more education is reducing teen-pregnancies whereas the effect on completed fertility is less clear.

Studying the impact of education on fertility is important to get a complete picture of the non-pecuniary effects of education (Oreopoulos and Salvanes, 2011). Moreover, socio-economic gradients in fertility patterns might have long-term impacts on the structure of society with wide-ranging consequences.

In this paper, we extend the analysis of education and fertility to a pan-European framework, combining data from two big panel surveys (Survey on Health, Ageing and Retirement in Europe and the English Longitudinal Study of Ageing) in which we can observe completed fertility patterns. We use compulsory school reforms over 30 years to instrument for years of education. Our main results show that more education increases fertility and reduces the percentage of childlessness among women. We explain our results by investigating the impact of education on the marriage market: women with higher education are more likely to be married, have more stable marriages and their partners have higher education as well.

2 Literature: Education and Fertility

There are several ways how economists think about the relationship between education and fertility. The first channel is labor supply (Becker, 1965). Education increases the earnings capacity, thus the opportunity costs of leaving the labor market to have and raise children. This substitution effect predicts a decrease in fertility. On the other hand, the income effect of higher permanent income would predict an increase in fertility. The strength of the income effect might be weakened by a quantity-quality trade-off in children (Becker and Lewis, 1973), i.e. due to higher income parents tend to invest more in the quality of their children, not the quantity.[1] Depending on the magnitudes of the substitution and income effect, both, a positive and negative relationship between education and fertility are possible. Next to labor supply, higher education will render females more attractive on the marriage market; it will increase their marriage chances and - due to assortative mating - will also boost the educational attainment and income of their potential partners (Behrman and Rosenzweig, 2002). These effects from the marriage market will tend to increase fertility. Moreover, education may improve information and decision making on contraceptive use (Thomas et al., 1991) and may increase female's bargaining power within a marriage. Finally, staying longer in school might, in principle, reduce the reproductive life of females, if fertility rates during formal education are lower.

Several recent studies investigated the relationship between education and fertility using compulsory schooling reforms to instrument for years of schooling. Most of

[1] Recent studies on female employment rates, unemployment and fertility (Adsera, 2005; Ahn and Mira, 2002; Dehejia and Lleras-Muney, 2004; Del Bono et al., 2011) question the preponderance of the substitution effect and find pro-cyclical fertility in more developed countries.

the papers that use European data, focus on a single country. Monstad, Propper and Salvanes (2008) studied completed fertility and timing of births in Norway, Fort (2009) looks at Italy and Braakmann (2011) at Britain. Monstad, Propper and Salvanes (2008) and Fort (2009) found evidence on postponement of childbearing away from the teenage years towards (first) births later in the women's reproductive life. These authors cannot detect significant effects on total fertility. Conversely, Braakmann (2011) detected a positive causal effect of education on the number of children a woman has.

For the U.S., three further studies present contradictory evidence: Leon (2004) uses compulsory schooling laws and shows that education causally reduces fertility. McCrary and Royer (2011), on the other hand, use age at school entry as an instrument and find basically no effect in two American states, California and Texas. Duflo, Dupas and Kremer (2010) argue that such an experiment is different from extending schooling because here children typically drop out at the same age, but some start schooling earlier. Therefore, school extension experiments might have impacted fertility differently due to the fact that young females are longer in school during teenage years.[2] Moreover, Amin and Behrman (2011) use the Minnesota Twin Registry. Their within-twin estimates show that more educated twins tend to have somewhat less children, but there is no effect on the probability to be childless. Note, that these twins have on average more than 13 years of schooling and, thus, are not comparable to compliers of compulsory schooling laws.

Black, Devereux and Salvanes (2008) investigated the direct effect of the change in mandatory schooling legislation in Norway and in the U.S. on the timing of births and found a reduction in teenage-births due to the increase in compulsory education.

[2]Causal studies for less developed countries (Nigeria, Kenya) or population groups with higher fertility levels (Arabs in Israel) generally find negative effects of education on fertility (Duflo et al., 2010; Lavy and Zablotsky, 2011; Osili and Long, 2008). The exception is Breierova and Duflo (2004), who use a large school expansion program in Indonesia and find no effects on total fertility, but some effects on teenage fertility suggesting that higher education leads to motherhood postponement. Similarly, Kirdar et al. (2011) found that the compulsory schooling reform in Turkey lead to motherhood postponement.

In short, while most studies across different countries suggest that education leads to motherhood postponement, the empirical evidence on the effect of education on total fertility is inconclusive: the results obtained vary substantially with some authors finding no significant relationships, while others found positive or negative effects.

There are few studies on the causal impact of education on the marriage market, which is one important route by which fertility effects of education could be channeled. Currie and Moretti (2003) use college openings in the U.S. to identify the causal impact of maternal education on marriage probabilities and find a positive impact. As the authors concentrate on child outcomes, they have only a sample of women with kids. Furthermore, their IV estimates are based on compliers that may be different to those affected by compulsory schooling reforms.

Leon (2004) uses compulsory schooling reforms in the U.S. and finds positive, although insignificant effects of education on marriage. Fort (2009) finds no effects on the timing of first marriages in Italy, whereas Lefgren and McIntyre (2006) - using U.S. Census data and instrumenting education by quarter of birth - find positive causal effects of females' education on husbands' earnings, but no effects on the probability of marriage. No significant effects on husbands' employment status and the probability of marriage are obtained by Braakmann (2011) for the UK.

In our study, we are using compulsory schooling reforms in Europe to instrument for years of education, a strategy which has been used by Brunello, Fort and Weber (2009) to investigate the returns to schoolign and Brunello, Fabbri and Fort (2012, forthcoming) to study the effect of schooling on obesity.

3 Empirical strategy

We use the plausibly exogenous variation in schooling induced by mandatory schooling reforms in 8 European countries to identify the causal effect of education on fertility. The use of school entry-age laws or minimum school leaving age laws as instruments for educational attainment was firstly introduced by Angrist and Krueger (1991) and is now widespread in the literature. As in previous studies, the key assumption we make to guarantee causal interpretation of our estimates is that, within each country, additional schooling was assigned to women only on the basis of their date of birth and thus independently of their future fertility choices.

As in previous studies exploiting educational reforms in Europe, we select reforms who affected the individuals' years of schooling at roughly the same education level, i.e. secondary education (either ISCED 2 or ISCED 3, depending on the specific country considered). To avoid blurring the difference between pre-treatment and post-treatment cohorts, we focus on one reform per country and design the sample to exclude the occurrence of other compulsory schooling reforms. Brunello et al. (2009) and Brunello et al. (2012, forthcoming) used samples symmetric around the pivotal cohort, i.e. the first cohort of individuals potentially affected by each reform, to include in the sample of analysis broadly the same number of treated and control units. Our baseline results are based on data from asymmetric windows around the pivotal cohort within each country instead. We show in Section 5.4 that these choices do not affect our point estimates but guarantee higher precision.

Our instrumental variable is the number of mandatory schooling years given by law and we assume that each additional mandatory year of education exerted the same effect on the actual number of years of schooling in all the countries included in the study.[3] This variable exhibits variation over cohorts within each country and across countries for any given cohort. The variability over both cohorts and countries allows us to control for country specific fixed effects as well as cohort fixed effects, which we assume invariant across countries, while we capture the trends in fertility across cohorts with country-specific polynomials. We estimate equations (1)

[3] Brunello et al. (2009) discuss why this is a plausible assumption (see Table B.2 in the Technical Appendix).

and (2)

$$Y_{ick} = \beta_0 + \beta_1 Edu_{ick} + \beta_2 \mathbf{X}_{ick} + \beta_3 Country_c + \beta_4 Cohort_k + \beta_5 CTrend_{ck} + \epsilon_{ick} \quad (1)$$

$$Edu_{ick} = \alpha_0 + \alpha_1 Comp_{ck} + \alpha_2 \mathbf{X}_{ick} + \alpha_3 Country_c + \alpha_4 Cohort_k + \alpha_5 CTrend_{ck} + \nu_{ick} \quad (2)$$

where Y_{ick} is the dependent variable capturing fertility or marriage behavior of individual i in country c of birth cohort k; Edu_{ick} is the number of years in education; $\mathbf{X}_{ick}$ is a vector of some control variables[4]; $Country_c$ and $Cohort_k$ refer to country and cohort-fixed effects and $CTrend_{ck}$ captures country-specific linear or quadratic trends in cohorts. Since ϵ_{ick} might be correlated with education, we estimate equation (1) with 2SLS, instrumenting education with $Comp_{ck}$, the compulsory years of schooling in the respective country and cohort. Equation (2) is the first stage equation.

We are able to account for smooth trends in education and fertility using country-specific polynomial trends. These trends account for all the societal changes that either evolve slowly over time (like attitudes) or change at once (eg. the introduction of the pill or changes in divorce laws) but exert an influence on all women regardless of their cohort and age. Indeed, our identifying strategy relies on changes affecting cohorts differently before and after the change (i.e. the schooling reform) whereas other societal changes do never affect cohorts differently to a large extent and should be well captured by our country-specific polynomial time trends. Furthermore, our identifying assumptions become more plausible when the width of the window around the pivotal cohort is small, i.e. when the comparison between individuals assigned to the new mandatory schooling obligations and individuals not assigned to the new regulations is *local*. Thus, we replicate our estimates using individuals born up to 10 years before/after the pivotal cohort, up to 7 years and up to 5 years and find no substantial change in the results.

[4] An indicator for whether the individual is foreign born, whether there was a proxy respondent used for the interview and indicators for interview-years.

Table 1: Compulsory schooling reforms in Europe

Country	Reform	Schooling	Pivotal Cohort
Austria	1962/66	8 to 9	1951
Czech Republic	1948	8 to 9	1934
Denmark	1958	4 to 7	1947
England	1947	9 to 10	1933
France	1959/67	8 to 10	1953
Germany:			
Northrhine-Westphalia	1967	8 to 9	1953
Hesse	1967	8 to 9	1953
Rhineland-Palatinate	1967	8 to 9	1953
Baden-Wuerttemberg	1967	8 to 9	1953
Italy	1963	5 to 8	1949
Netherlands	1942	7 to 8	1929

Table 1 lists the countries and reforms we consider, presenting the change in years of education prescribed by the law and the *pivotal cohort*, i.e. the first cohort potentially affected by the reform. For a short description of each reform and the explanation of the choice of the pivotal cohort see the Appendix.[5]

With some exceptions, the reforms considered prescribed a 1-year increase in school-leaving age and in most countries, the reforms affected the educational attainment of individuals born after World War II.

3.1 Data

We pool data on women from the first two waves of the Survey on Health, Ageing and Retirement in Europe (SHARE) and the second wave (interviews in 2004/05) of the English Longitudinal Study of Ageing (ELSA).[6] As for SHARE, we use the

[5]We use different reforms than Brunello et al. (2009) and Brunello et al. (2012, forthcoming) for Denmark and the Netherlands due to data restrictions: we cannot include the most recent reforms because we do not observe treated individuals in our 50+ sample. As a result, while we are able to include the Czech Republic and England, we are forced to exclude some other countries (Belgium, Finland, Greece, Ireland, Portugal, Spain and Sweden).

[6]Previous studies using a similar strategy cover a slightly larger number of countries by using data from the first wave of SHARE in combination with other sources (European Community Household Panel, International Social Survey Program, German Socio Economic Panel). However,

second wave information for longitudinal individuals (interviews in 2006/07) and for those with missing information in wave 2, we use data from the interview in 2004/05 (wave 1). We also include records of individuals only interviewed in 2004/05 and for individuals only interviewed in 2006/07. The longitudinal individuals represent roughly 46 percent of the overall SHARE sample, nearly 36 percent are observed in wave 2 only and for 18 percent of records we use information from wave 1.[7]

We use only records of females aged 50 or above who were born in the country of residence or migrated before the age of 5 to ensure that they went to school in the host country at least at the early stages of their school career, i.e. when they were eligible for the changes induced by the reforms.[8] From this dataset, we extract women born up to 10 years before/after the pivotal cohort so that the final sample for the baseline regressions includes a total of 6,728 observations.

We measure education as years of education. As dependent variable we consider measures of completed fertility as well as whether the woman was ever married. It is important to highlight that we consider cohort measures of these phenomena and not period measures. Period measures of fertility are generally based on cross-section data and measure current fertility, giving up-to-date information on levels. However, most of these measures are affected by distortions due to changes in the timing of events (marriage, births), the so-called *tempo-effects*. As a consequence, the period-measures are quite misleading estimates of the long-run fertility of a given population. The cohort measures of fertility are mainly based on longitudinal or retrospective data. Their main advantage is that individuals belonging to the same cohort experience events (marriage, births) in the same socio-economic conditions (say, an economic boom or a recession period, a war, dramatic changes in laws,

those additional data sources would not allow us to measure cohort fertility in a consistent way across countries as SHARE and ELSA do.

[7]Note that sample attrition between the two waves of SHARE is no problem in our study because all individuals that appear at least once in the survey are included in our sample.

[8]We exclude records with missing information of key variables, i.e. no information on the level of education attained, no information on the number of children. We also exclude records of women whose age at birth of the first biological child was below 15 or above 45.

and so on); therefore those measures are not distorted by transient effects. As our measure for completed fertility of women we use the number of biological children. Our data are censored at 4 but we highlight that only a minority of women (4.75 percent) had more than 4 children in total (including non-biological ones), because the survey gives exact information only for the first 4 children. We control for this censoring in the section 5.3. The available retrospective information allows us to construct cohort measures of fertility for women who are aged at least 50, i.e. women who have completed their fertile lifespan.

Table 2 reports descriptive statistics on key variables in the sample used for the baseline estimations in the paper. The average number of biological children per woman in the sample is slightly below replacement level (i.e. 2), it is at replacement level for a few countries and its highest in the Netherlands (2.4 children per woman on average), where also the average age of the respondents is highest.[9] Since this variable is censored at 4, we report also the total number of children per woman, including step-children, adopted children, foster children and the children of the current spouses. This variable is slightly higher, 2.1 on average. The third column of the table shows the proportion of women without biological children, ranging from about 9 percent in Denmark to almost 18 percent in Germany. The average age of women at their first births is about 25, the average years of education around 11 and the average number of compulsory schooling years around 8. About 95% of the women in our sample are married at the time of the interview or have been married in the past and about 10% declare to be separated or divorced at the time of the interview.

Our measures of the number of children only refer to those children who are still alive at the time of the interview. This could potentially affect our identification strategy if children of women whose education is affected by the reform are more

[9]Note that due to our sampling windows (+10/-10 cohorts around the reforms) and the differences in the timing of the reforms, a comparison of variable means across countries is not meaningful.

likely to still be alive at the time of the interview. We postpone this discussion to Section 5.1.

4 Results

First, we present our baseline results of the causal impact of schooling on the number of biological kids and childlessness. In section 4.2, we discuss the external validity of our estimates and try to characterize the subpopulation of compliers. Furthermore, we discuss possible mechanisms and present additional estimates on potential channels for a transmission of educational impacts on fertility, such as marriage behavior or social status of respective partners.

4.1 Baseline results

We first look at the effect of the reforms on years of education (first stage) and the outcomes (reduced form parameters). The first stage and the effect of the reforms on the number of biological kids are shown graphically in Figure 1. In these graphs cohorts from different countries are normalized with the compulsory schooling reforms, showing cohorts before and after the event, respectively. The graph in the left panel shows the first stage: the reforms had an impact on years of education: mean years of schooling are higher for cohorts after the reforms. The reduced form graph (right panel) shows the (adjusted) number of biological kids for cohorts before and after the reforms.[10] The graph shows generally a decrease in fertility, but indicates a small positive jump at the pivotal cohort.

Table 3 shows the estimated coefficients of education on the number of biological kids and childlessness for three samples as well as for two different specifications of the country-specific trends in cohorts, a linear and a quadratic trend. Sample 10

[10]The adjusted number of biological kids is the residual from a regression of the number of biological kids on a set of control variables (foreign born, proxy interview, interview year, cohort, country and country-specific linear trends in cohorts).

Table 2: Descriptive statistics

	Number of children		Proportion	Age at	Education		Ever	Separated	Age	Obs
Country	biological[a]	all	childless[b]	first birth	individual	compulsory	married[c]	or divorced[c]		
Austria	1.8	1.9	14.8	23.3	10.4	8.3	90.3	16.0	58.9	425
Czech Republic	1.8	2.2	8.7	23.5	10.4	8.4	97.4	8.7	74.7	391
Denmark	2.0	2.2	9.6	24.1	11.9	5.7	94.7	14.3	58.9	968
England	1.9	2.1	15.8	25.5	10.7	9.6	96.0	10.0	70.5	2,399
France	2.0	2.1	10.2	24.7	12.0	8.6	92.5	15.2	56.6	816
Germany	1.6	1.7	17.7	25.2	13.2	8.2	96.3	13.5	56.6	350
Italy	1.9	2.0	11.5	24.9	8.2	6.1	94.9	3.0	59.3	1,109
Netherlands	2.4	2.7	12.6	27.0	9.2	7.4	94.0	5.2	78.2	270
All	1.9	2.1	13.0	24.9	10.7	8.0	94.9	10.4	64.4	6,728

NOTES: Sample includes one reform per country (see Table 1) and women born up to 10 years before or after the pivotal cohort.
[a] the variable is censored: we count up to four biological children; [b] this is the fraction of women with no biological children in the sample in percent; [c] the sample size is slightly smaller for these variables due to missing values in the marital status question and amounts to 6,718.

Effect of the reforms on

years of education (first stage) | number of biological children (reduced form)

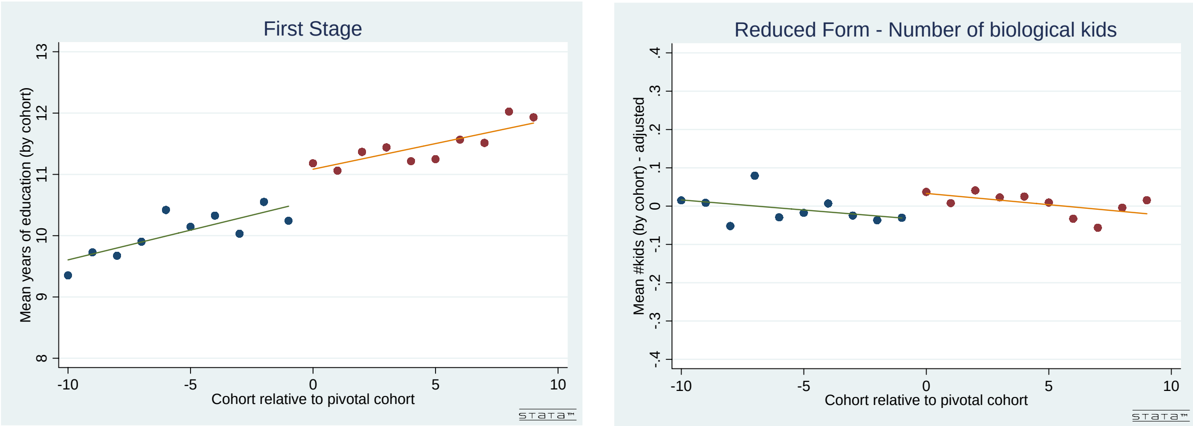

Figure 1: Effect of the reforms on education and the number of biological children

includes at maximum 10 cohorts before and 10 cohorts after the reform, sample 7 is restricted to 7 and sample 5 to 5 cohorts before and after.[11] Consistently across samples and specification, the coefficients of the OLS regressions show the same signs as comparable correlation studies: years of education are negatively correlated with the number of biological kids and positively correlated with childlessness.

Furthermore, Table 3 reports reduced form estimates and first stage results of our model. The first stage results show that the reforms actually had an effect on schooling, one more year of compulsory education increased schooling by about 0.2 – 0.3 years. The magnitudes of these coefficients are similar to other studies using compulsory schooling reforms in Europe (Brunello et al. (2009),Brunello et al. (2012, forthcoming)) . The F-statistics of the excluded instrument in the first stage ranges from about 18 to 25 in the specification with the linear country-specific trend, indicating that the instrument is sufficiently correlated with the endogenous variable. The specifications with the quadratic trends - where more variation in school attainment is filtered out - show smaller F-statistics, especially with sample 5. The reduced form estimates confirm the graphical inspection: one year of additional

[11] In some countries 10/7/5 cohorts before and after are not available because the reform was too early or too late for our sampling period or another reform was implemented.

compulsory schooling increases the average number of children by between 0.06 and 0.08 depending on the specification and causes a large reduction in childlessness (by between 1 and 4 percentage points depending on the specification); i.e. nearly up to 30 percent of the childlessness observed in our sample.

Two-stage least-squares estimates have the same signs as the reduced form leading to an unexpected and interesting result: when we instrument years of education with the number of compulsory schooling years, all coefficients change their signs, i.e. schooling increases fertility. One additional year of schooling raises the number of biological kids a women has by 0.2 – 0.3 and decreases childlessness by about 7.5 – 13 percentage-points.

As shown in Table 3, the main results are very robust across the different specifications - with respect to sampling and trend specifications.

4.2 Interpretation and Mechanisms

We observe a positive causal relationship between education and fertility. On average, one year of education increases the number of biological kids by about 0.27 and reduces childlessness by about 11 percentage-points. These coefficients are large in magnitude and amount to about 14 percent and 85 percent of the dependent variable. We interpret these results as Local Average Treatment Effects, i.e. the effect of education on fertility for those who changed their schooling attainment because they were affected by the reforms *(compliers)*. Since we are analyzing compulsory schooling reforms, our estimates might apply for those at the bottom of the education distribution. Figure 2 shows the distribution of years of education for our full sample three cohorts before and three cohort after the respective reforms. The graph shows that the reforms had the largest effects for those with few years of education.[12]

[12] Brunello et al. (2009) show that this is true using quantile regressions.

Table 3: Baseline results

	Sample 10		Sample 7		Sample 5	
	l-trend	q-trend	l-trend	q-trend	l-trend	q-trend
A: # biological kids						
OLS	-0.033	-0.033	-0.033	-0.033	-0.031	-0.032
	(0.005)***	(0.005)***	(0.005)***	(0.005)***	(0.006)***	(0.006)***
2SLS	0.205	0.284	0.312	0.294	0.188	0.311
	(0.075)***	(0.095)***	(0.111)***	(0.134)**	(0.064)***	(0.058)***
Reduced Form	0.064	0.078	0.083	0.068	0.064	0.056
	(0.018)***	(0.019)***	(0.021)***	(0.021)***	(0.021)***	(0.024)**
B: Childlessness						
OLS	0.007	0.007	0.007	0.007	0.006	0.006
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
2SLS	-0.075	-0.127	-0.137	-0.121	-0.090	-0.137
	(0.025)***	(0.039)***	(0.039)***	(0.051)**	(0.025)***	(0.024)***
Reduced Form	-0.023	-0.035	-0.036	-0.028	-0.031	-0.012
	(0.006)***	(0.007)***	(0.007)***	(0.009)***	(0.008)***	(0.010)
First Stage	0.312	0.274	0.265	0.230	0.341	0.180
	(0.065)***	(0.070)***	(0.063)***	(0.087)***	(0.068)***	(0.111)
F-Statistics	23.34	15.20	17.92	6.93	24.95	2.63
Observations	6,728	6,728	5,118	5,118	3,923	3,923

NOTES: Each coefficient represents a separate linear regression. Country-fixed effects, cohort-fixed effects, country-specific trends in birth cohorts (linear and quadratic), indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

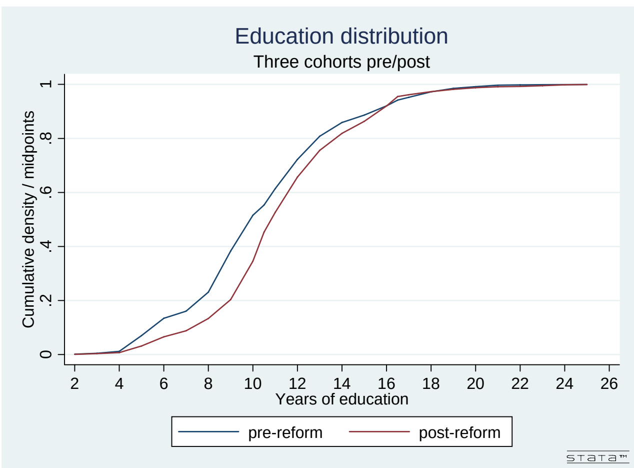

Figure 2: Education distribution before and after

Though it is not possible to identify compliers using observed data, since they are defined by means of counterfactual outcomes, we can characterize the population of compliers with respect to some interesting pre-treatment variables, as first suggested by Angrist (2004). The compliers population can be easily characterized by exploiting Bayes theorem (see Angrist (2004) for the details) when both the treatment (education) and the instrument (compulsory schooling) are binary variables. The extension of the result to continuous or discrete variables is not trivial, thus, we re-coded our treatment and instrument as binary.[13] Both surveys, SHARE and ELSA, include retrospective information about the respondent's histories. We select pre-treatment variables that are similarly reported in the two surveys and can be considered as proxies for family attitudes and/or parental background, namely: (i) a binary indicator of whether the individual had few books (between 0 and 10) at home when aged 10; (ii) a binary indicator taking the value 1 if the individual has more (alive) siblings with respect to the country median (nearly 2 in almost all countries), 0 otherwise and (iii) a binary indicator taking the value 1 if the individual used to live in a large household, i.e. an household with more persons with respect to the country median in the sample, when aged 10.

We find that, with respect to the sample average, compliers tend to be: (i) 60 percent more likely to have had few books at home when aged 10; (ii) 97 percent more likely to have an above median number of siblings alive and iii) 86 percent more likely to come from large (i.e. above median) households. We interpret these results as suggestive evidence that compliers tend to have a poorer background and be more family oriented with respect to the average individual in the sample.

[13]The treatment is a binary indicator taking the value 1 if the individual's actual years of education are equal or exceed the post-reform number of mandatory schooling years and 0 otherwise. The instrument is a binary indicator taking the value 1 for post-reform cohorts and 0 otherwise. For this exercise, we consider only countries for which the new mandatory schooling prescribed a one-year increase, so that the instrument coefficient has the same interpretation in all countries. The first stage on this sub-sample is smaller compared to our baseline results, but still statistically significant at 10 percent level.

If the causal effect of education on fertility is positive, why are those variables negatively correlated in OLS regressions? One explanation is, that the OLS results are biased downwards because of an omitted variables bias. Assume the true econometric model to be

$$\mathit{Fertility}_{ick} = \gamma_0 + \gamma_1 \mathit{Edu}_{ick} + \gamma_2 \mathit{Family}_{ick} + \ldots + \epsilon_{ick}, \tag{3}$$

with *Family* capturing positive general attitudes towards the family or preferences for having children ($\gamma_1 > 0$ and $\gamma_2 > 0$). This variable will be positively related to fertility, but might be negatively related to years of education ($COV(\mathit{Edu}, \mathit{Family}) < 0$) because women often have to decide between being family or career-oriented. If this variable is omitted from the regression and sufficiently correlated with education, the OLS coefficient on education will be biased downwards.[14]

As described above, one possible channel why education may influence fertility is marriage behavior. We investigate whether education is related to the probability and the stability of marriage.

Panel A of Table 4 shows the OLS and the 2SLS coefficients on marriage behavior. The OLS model exhibits that education is negatively correlated with an indicator variable of ever being married and positively related to being separated or divorced. When taking care of the endogeneity of education again using compulsory schooling laws, all coefficients change their signs. One additional year of education increases the likelihood that a women got married by 6 percentage-points on average (6.3 percent). The 2SLS estimates on separation/divorce are less precisely estimated in the smaller samples but show similar magnitudes. One year of education decreases the likelihood of separation/divorce by 5 percentage-points (50 percent). Both results

[14] Normalize family orientation between 0 (no family orientation) and 1 (highest family orientation). If $\gamma_2 = 1$, then women with the highest level family-orientation have one child more than those with the lowest level family-orientation. In that case, a slope coefficient of 0.247 from the regression of family orientation on years of schooling (in the sample 10 model with linear trend) would explain the difference between the OLS and the IV model.

Table 4: Mechanisms

	Sample 10		Sample 7		Sample 5	
	l-trend	q-trend	l-trend	q-trend	l-trend	q-trend
A: Marriage outcomes						
Ever married						
OLS	-0.005	-0.004	-0.005	-0.005	-0.004	-0.004
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
2SLS	0.037	0.062	0.054	0.082	0.057	0.086
	(0.017)**	(0.026)**	(0.022)**	(0.041)**	(0.020)***	(0.018)***
Separated/divorced						
OLS	0.003	0.003	0.002	0.002	0.002	0.002
	(0.001)**	(0.001)*	(0.002)	(0.002)	(0.002)	(0.002)
2SLS	-0.053	-0.056	-0.057	-0.077	-0.030	-0.041
	(0.028)*	(0.027)**	(0.030)*	(0.055)	(0.022)	(0.016)**
Observations	6,718	6,718	5,108	5,108	3,916	3,916
B: Quality of partner						
Years of education of partner						
OLS	0.612	0.613	0.611	0.610	0.605	0.603
	(0.020)***	(0.020)***	(0.022)***	(0.022)***	(0.025)***	(0.025)***
2SLS	0.532	0.821	0.613	0.629	0.648	0.594
	(0.257)**	(0.330)**	(0.370)*	(0.360)*	(0.260)**	(0.432)
Observations	3,705	3,705	2,784	2,784	2,123	2,123

NOTES: Each coefficient represents a separate linear regression. Country-fixed effects, cohort-fixed effects, country-specific trends in birth cohorts (linear and quadratic), indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

are in line with our results on fertility, i.e. education improves marriage outcomes, which in turn may increase fertility.

Next to its effect on the likelihood of marriage, education might improve the quality of the husband. Panel B in Table 4 presents an analysis of this channel, based on a restricted sample of females with cohabiting partners.[15] The OLS and 2SLS coefficients of the impact of female education on the years of education of their partners are very similar and amount to about 0.6, indicating a high degree of assortative mating. With respect to fertility, the education of the partner should

[15] Other than the females' sample, this sample also includes couples whose partner is below the age of 50. However, all individuals in this sample are at least 40 years old.

increase household income and fertility. Note that these conditional effects on the quality of the partner for those who do have a partner are lower bounds to the unconditional effects of increased education on the probability for all women to have a highly-educated partner, because also the probabilities to get and stay married are higher for those women with higher education. The effects of education on marital outcomes and the quality of a potential partner are very consistent across the board: more education means a higher probability to live with a partner; a partner with higher education, as well.

Proper identification of the causal effect hinges on two conditions: first, as in all studies using compulsory schooling reforms, we rule out general equilibrium effects (Stable Unit Treatment Value Assumption, see discussion in Angrist et al. (1996)). In particular, we assume that the compulsory schooling reforms did not change the general availability of more educated partners for women affected by compulsory schooling reforms with respect to women not affected. Typically, females are younger that their partners. For instance, in the estimation sample 10, in 77% of the couples the woman is younger than the man; the mean age difference between husband/partner and wife is 2.6 years. As the mean schooling effect of an increase in compulsory schooling is rather modest for both men and women and for many women close to the threshold year, potential (elder) partners are not treated, general equilibrium issues seem to be less severe.

Secondly, we need to assume that the reforms affected female fertility only through female education and did not affect fertility directly. This assumption may be violated if women marry men of the same age – which would imply the same treatment status – **and** male education has a direct effect on female fertility. In what follows, we present complementary empirical evidence and discuss arguments that rule out such direct effects of the education of husbands or partners on fertility.

In our set-up, we can propose an internal validity test under mild conditions. The test is motivated by previous work by Oreopoulos et al. (2006). The authors assess the causal effect of parental education on children's education instrumenting parental education (the sum of father's and mother's education) with compulsory schooling reforms. Following their approach, we can use the subsample of females where we have information about the husband to investigate the total effect of parental education on fertility of the couple. Note that, as discussed above, estimates obtained in the subsample of couples have to be interpreted as a lower bound of the true effect because the effects of education on marriage probability and assortative mating are not included. Moreover, without additional assumptions, such estimates are not directly comparable to the ones reported above because they investigate a different causal parameter: the effect of parental education versus the effect of female education. The main advantage of looking at the causal effect of parental education on couples' fertility is that the estimates of this parameter are internally valid even if male education does directly affect fertility.

In this subsample of couples, we can also estimate the effect of female education on couples' fertility. Again, this would be a lower bound of the true effect, excluding the indirect effect of female education through marriage/divorce and assortative mating. However, this estimate is not internally valid if male education directly affects couples' fertility.

The parental education IV estimator is internally valid if the reforms do not affect couples' fertility directly. The female education IV estimator is internally valid if the reforms do not affect couples' fertility directly and male education does not affect couples' fertility directly. We can compare the estimates delivered by the two estimators in a sensible way under the assumption that a) the reforms do not affect fertility directly in either case and b) father's and mother's education have the same causal effect of education on couples' fertility, on average.

We require the assumption that compulsory schooling reforms do not affect fertility directly – over and above their impact on education – when we compare the causal effects of parental education with those of female education. Assuming that father's and mother's education have the same effect on couples' fertility and that both strategies are internally valid, the two estimators are both consistent and should deliver similar point estimates. In other words, by comparing the point estimate for the causal effect of female education with that of parental education on couples' fertility we can test if male education affects fertility directly. If this would be the case, the two estimates would be significantly different, because only the estimate of the effect of parental education is internally valid.

Results of such tests are reported in Table 5. In Panel A we show the estimates of the causal effect of average parental education on fertility when we instrument education with the average compulsory schooling years.[16] We interpret these estimates as Local Average Treatment Effect. Since this interpretation implies that the parameter we can identify is instrument-dependent, we also present estimates where we only use female compulsory schooling laws as instruments for parental education (Panel B); the point estimates are not statistically different to Panel A, but less precise. Panel C reports corresponding estimates for the causal effect of female education on couples fertility. Again, all point estimates have the right sign, are fairly close to and not different from those when we use parental education. This corroborates our claim that male education does not affect fertility directly and supports the causal interpretation of the estimates reported in previous sections. [17] Note that, in our application, the focus on the effect of female education on fertility rather than parental education on couples' fertility has the advantage of including the effect of education on fertility that operates through assortative mating. Indeed,

[16] We also used compulsory schooling years of the woman and man in the couples separately as instruments, with exactly the same results. This suggests that the assumption that the effect of compulsory schooling laws on average parental education is the same for laws affecting the man and laws affecting the woman is valid. Notably, when we use the gender-specific instrument, each instrumental variable attracts a positive and significant coefficient of almost the same magnitude than the one reported in Table 3 in the paper. Results are available on request.

[17]As Oreopoulos et al. (2006), we do not include each parent's education separately. Indeed, as they noted "when we include each parent's education separately in the same regression, the standard error estimates that are produced are too large to be able to discern differences between the effects of mother's and father's education".

this indirect effect of female education on fertility would be neglected by focusing on a sample of couples only.

Table 5: Couples' fertility

	# biological kids			Childlessness		
	Sample 10	Sample 7	Sample 5	Sample 10	Sample 7	Sample 5
A: Couples - average education, instrument: both						
2SLS						
average years of education	0.151	0.155	0.158	-0.056	-0.079	-0.083
	(0.088)*	(0.113)	(0.113)	(0.027)**	(0.038)**	(0.039)**
First Stage						
average years of compulsory education	0.644	0.594	0.693	0.644	0.594	0.693
	(0.150)***	(0.181)***	(0.211)***	(0.150)***	(0.181)***	(0.211)***
F-Statistics	18.52	10.84	10.77	18.52	10.84	10.77
B: Couples - average education, instrument: females only						
2SLS						
average years of education	0.091	0.152	0.115	-0.024	-0.063	-0.036
	(0.107)	(0.172)	(0.119)	(0.035)	(0.060)	(0.039)
First Stage						
years of compulsory education female	0.347	0.268	0.424	0.347	0.268	0.424
	(0.107)***	(0.123)**	(0.142)***	(0.107)***	(0.123)**	(0.142)***
F-Statistics	10.49	4.76	8.95	10.49	4.76	8.95
C: Females - couples' sample						
2SLS						
years of education of female	0.067	0.157	0.128	-0.024	-0.076	-0.048
	(0.091)	(0.165)	(0.121)	(0.031)	(0.061)	(0.041)
First Stage						
years of compulsory education female	0.418	0.309	0.470	0.418	0.309	0.470
	(0.079)***	(0.084)***	(0.095)***	(0.079)***	(0.084)***	(0.095)***
F-Statistics	27.79	13.47	24.41	27.79	13.47	24.41
Observations	3,710	2,787	2,125	3,710	2,787	2,125

NOTES: Each coefficient represents a separate linear regression. Country-fixed effects, cohort-fixed effects and country-specific linear trends in birth cohorts of both partners (panels A-B) / females (panel C) as well as indicators for interview year, foreign born and proxy interview included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country*cohort-female*cohort-male (panel A-B), country*cohort-female (panel C). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

5 Sensitivity analysis

This section presents several sensitivity checks and falsification tests. We will show that our estimates are not confounded with any selection biases. In 5.1, we deal with the potential confounder of selective mortality of the respondents themselves and their children. Furthermore, section 5.2 present the robustness of our estimates to placebo reforms. We relax the assumptions on the functional form of the relationship between education and fertility by applying Count-data and Tobit models in 5.3 and finally, we investigate the robustness of our estimates with respect to the selected reforms, countries and samples (5.4).

5.1 Fertility and mortality

One potential confounder may be selective mortality of the respondents themselves and their children. We start with discussing this issue for the respondents.

The older cohorts in our sample may be positively selected with respect to their health, since these individuals are still alive and able to participate in the SHARE and ELSA interviews. One concern is that these individuals might be selected with respect to fertility as well. If mortality is related to fertility in the way that childless women and women with fewer biological kids live longer, our estimates might reflect these patterns. This would mean that in our "control" group (older cohorts with fewer years of compulsory education) the less fertile women might be over-represented.

One big advantage of our estimation strategy is that we are able to control for cohort-fixed effects. A large part of a potential mortality-related selectivity should thus already be eliminated. However, to eliminate any further biases, we pursue three different strategies: (i) we review the literature on the relationship between fertility and mortality, (ii) we restrict our analysis to younger cohorts and (iii) we

Table 6: Selective mortality

	Baseline (see Table 3)	Recent cohorts	Life-expectancy control	Life-expectancy weight
# biological kids	0.205	0.236	0.202	0.254
	(0.075)***	(0.089)***	(0.075)***	(0.094)***
Childlessness	-0.075	-0.095	-0.073	-0.098
	(0.025)***	(0.031)***	(0.025)***	(0.033)***
Observations	6,728	3,518	6,728	6,728

NOTES: Each coefficient represents a separate 2SLS linear regression based on Sample 10. Recent cohorts are those born 1940–56, the Czech-Republic, England and the Netherlands are dropped from this regression. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

estimate our models by controlling for differences in the life-expectancy of individuals born in different years and countries.

The literature on the relation between the number of children a wife has born and mortality is unclear; there are some papers showing correlation but no causal studies. Studies for previous centuries find a positive correlation between parity and mortality (Doblhammer and Oeppen (2003) looking at English peers starting from 1500 onwards as well as Smith et al. (2002) using Utah couples from 1860-1899). This might be due to medical risks directly related to childbirth. Studies using more recent data are inconclusive: while Hank (2010) finds no effect for Germany, Hurt, Ronsmans and Thomas (2006) in a meta-study find generally no relation between parity and mortality, if ever mortality risk is highest for women without children and those with more than four children.[18]

In Table 6 we present several regressions that take care of a potential selective mortality bias. The first column replicates the baseline 2SLS results for Sample 10 (with the linear country-specific trend in cohorts). The coefficients in column 2 are based on a restricted sample of younger cohorts, those born 1940-1956.[19] For this

[18] See also Doblhammer (2000) and Grundy and Tomassini (2005).

[19] This restriction also takes care of the argument, that our sample persons might be hampered by wartime effects: for these women, schooling started after World War 2.

sample we had to exclude countries with early reforms (the Czech-Republic, England and the Netherlands). We argue that these cohorts are younger and selectivity on the basis of mortality differences is less severe. If our baseline results of a positive effect of education on fertility were driven by a selectivity bias, the estimates for recent cohorts should be significantly smaller than the baseline results. The estimated coefficients show that this is not the case; on the contrary: the numerical coefficients are somewhat higher.

For a further test, we collected data on life-expectancy at birth from the Human Mortality & Human Life-Table Databases.[20] While younger cohorts in our sample are generally aged below their life-expectancy, the older cohorts are above. In column 3, we added this variable to our regression. The coefficients do not change. Column 4 presents 2SLS estimates of a weighted regression, with $weight = 1/(age - life\text{-}expectancy)$ if $age > life\text{-}expectancy$, 1 otherwise, i.e. individuals that are aged above their life-expectancy get less weight in the regression. The 2SLS coefficients are, again, very similar to the baseline results.

All results presented in Table 6 are not sensitive to the specification (linear or quadratic trend) and the sampling window. Overall, the analysis suggests that the results are not driven by selective mortality of the respondents.

As described above, we only observe the children of the respondents if they are still alive at the time of the interview. The older cohorts in our sample might have had more children who are not alive anymore and therefore not counted in the dependent variable. Thus, we have a measurement error problem, with the measurement error being very likely to be correlated with explanatory variables, the cohorts and most importantly our instrument, years of compulsory schooling. This problem is very similar to the selective mortality of the respondents themselves and

[20]The databases are provided by the Max Planck Institute for Demographic Research (www.demogr.mpg.de). The information is missing for some cohorts in Austria and Germany. We linearly predicted the life-expectancy for these cohorts. We use period-measures of life-expectancy at birth since cohort measures of life-expectancy at birth are currently not available for the cohorts we consider.

the same sensitivity analysis apply. If our results would stem from selective mortality of the children of the respondents, the magnitude of the coefficients would get smaller if only recent cohorts are used for the analysis (for whom the measurement error should be smaller) or if life-expectancy is accounted for. As Table 6 shows, this is not the case. Furthermore, the average age at first birth of women in our sample is nearly 25 and their age at the time of the interview is 65 on average. Thus, their oldest child should only be aged 40 at the time of the interviews.

However, this sensitivity analysis does not apply if education reduces child mortality – compared to general mortality at older ages. Education might not influence fertility behavior as such but might increase the probability that the child is still alive at the time of the interview. More educated women might behave more healthy during pregnancy or invest more in their children's health due to better knowledge or income effects. For the U.S., there are two studies focusing on the effect of maternal education on infant health. While McCrary and Royer (2011) use age at school entry as instrument for maternal education and find no significant effects, Currie and Moretti (2003) use college openings and find significant effects on birth weight, the incidence of a premature birth and prenatal care. Comparing the results of these two studies suggests that education has a positive effect on the health of the child only at higher levels of education.

If education reduces child mortality, the results obtained above may stem from these effects rather than from labor or marriage market effects as discussed in Section 4.2. There is one feature of the SHARE and ELSA data that helps us to reject this hypothesis. At the time of the third wave (2008/09 in SHARE and 2007 in ELSA), retrospective life-interviews have been conducted and the respondents were asked: Have you had another (ever had) a biological child - even one who only lived for a short time? Using this information we construct an indicator variable of whether a person never had a biological child.

Table 7: Selective mortality of children

	Sample 10	Sample 7	Sample 5
A: All individuals			
Childless at time of interview	-0.075	-0.137	-0.090
	(0.025)***	(0.039)***	(0.025)***
Observations	6,728	5,118	3,923
B: Individuals with retrospective interview			
Childless at time of interview	-0.077	-0.140	-0.114
	(0.035)**	(0.069)**	(0.058)**
Mean	[0.124]	[0.119]	[0.115]
Never had biological kids	-0.081	-0.142	-0.126
	(0.038)**	(0.072)*	(0.065)*
Mean	[0.115]	[0.111]	[0.106]
First Stage	0.284	0.213	0.232
	(0.087)***	(0.090)**	(0.101)**
F-Statistics	10.510	5.551	5.241
Observations	4,470	3,379	2,596

Notes: Each coefficient represents a separate linear regression. Country-fixed effects, cohort-fixed effects, country-specific trends in birth cohorts (linear), indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

Table 7 presents the results of this sensitivity check. Panel A shows the baseline estimates for childless (at the time of the interview). Since only about 66% of our sample respondents were still in the panel at the time of the third wave, we replicate our estimates on childlessness for those individuals who took the retrospective interview (Panel B). While the estimates are very stable, the first stage turns out to be weaker for the smaller samples. The estimates, furthermore, are very stable when children that are not alive anymore at the time of the interview are taken into account. We interpret these results as evidence that potential child mortality is not biasing our basic results, which turn out to be very robust, indeed.

5.2 Placebo treatments

As compulsory schooling reforms affect cohorts differently we might be concerned that our school reform variables pick up some unspecified time trend in the countries. To test for this, we are using a placebo reform exercise. Similar to Black et al.

Table 8: Placebo treatments

	Reduced Form (see Table 3)	Reduced Form +3yrs in future	Reduced Form +5yrs in future
# biological kids			
Compulsory schooling reform	0.064	0.065	0.060
	(0.018)***	(0.018)***	(0.019)***
Placebo reform		0.004	-0.010
		(0.017)	(0.025)
Childlessness			
Compulsory schooling reform	-0.023	-0.023	-0.024
	(0.005)***	(0.005)***	(0.005)***
Placebo reform		0.005	-0.000
		(0.007)	(0.010)

NOTES: Each column and panel represents a separate regression based on Sample 10. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). The number of observations in all specifications is 6,728. ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

(2008), we introduce a placebo treatment where we add a hypothetical compulsory schooling reform for each of our countries, either three or five years in the future. This placebo reform should not have any impact on fertility. If we find an impact, our results might be driven by other unobserved mechanisms (like selective mortality or time trends). As the placebo reform should have no impact on attended years of schooling, we can only use the reduced form estimates to test for a placebo effect.

Table 8 shows the reduced form estimates for the number of biological kids and childlessness (again for sample 10 with linear time trends). In both panels, the reduced form of the baseline model is given in column 1. In columns 2 and 3, the results of the placebo tests are given. Adding placebo schooling reforms three years in future (column 2) and five years in future (column 3) does not alter the reduced form estimates of the original reforms. Furthermore, none of the future laws has any impact on fertility. The same results are obtained with sample 7 and with the quadratic specification of the time trends.[21]

[21]Note that we have to include the real compulsory schooling reforms in the regressions as well, as for some cohorts placebo and real reform overlap.

Table 9: Poisson regression models results

	No censoring			Right censoring (4)		
	(1)	(2)	(3)	(4)	(5)	(6)
	edu	edu^a	resid^a	edu	edu^a	resid^a
Coefficient	-0.018***	0.125**	-0.142**	-0.020***	0.132**	-0.152**
	[-0.02,-0.01]	[0.02,0.35]	[-0.37,-0.04]	[-0.03,-0.01]	[0.01,0.39]	[-0.41,-0.03]
APE^b	-0.034***	0.182	-0.208*	-0.033***	0.171	-0.197*
	[-0.04,-0.02]	[0.00,0.56]	[-0.59,0.00]	[-0.04,-0.02]	[0.00,0.52]	[-0.55,0.00]

NOTES: 95 percent confidence intervals (CI) are in brackets. Each column and panel represents a separate regression based on Sample 10. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions.***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.
[a] Average estimates over 500 bootstrap replications.
[b] APE stands for Average Partial Effect on the average number of children at mean values of covariates in the sample. Columns (1) and (4): education treated as exogenous. Columns (2) and (5): education treated as endogenous. CI in columns (1) and (4) are based on standard errors estimated by Delta-method and normal approximation. CI in columns (2), (3) and (5), (6) are based on the estimator's empirical c.d.f. .

5.3 Functional form

In previous sections, we presented results based on the estimation of linear regression models. However our data present two characteristics that may be relevant for the choice of the regression model: first, the number of children in a family takes only non-negative integer values, so that count data regression models would be more appropriate choices; second, our data on the number of biological children are (right) censored at four, thus we should consider regression models that allow for censoring. This section is devoted to present evidence on the robustness of our results to different modeling choices. We consider in turn: (i) Poisson regression models (estimated by maximum likelihood); Poisson regression models that allow for right censoring (Raciboski (2011)); tobit regression models and discuss results in turn.

Table 9 reports results of Poisson regression models estimated by maximum likelihood. The left panel presents the coefficient estimates and average partial effects on the average number of children for a simple Poisson regression model, while the right panel presents estimates of a model that allows for right-censoring at four. In Columns (1) and (4) education is treated as exogenous: the average partial effects can be compared with OLS marginal effects in the first column of Table 3.

In columns (2) and (5) education is endogenous (compare with the 2SLS results in the first column of Table 3). In Poisson regression models, instrumental variable estimation is based on a control function approach. In practice, we proceed in two steps. In the first step, we generate the residuals from the first stage regression, i.e. the regression of years of education on years of compulsory schooling. In the second step, the generated residual is added as a regressor in the outcome equation. This allows to isolate - in the outcome equation - the variation in education that is exogenous, i.e. driven only by compulsory schooling reforms. Table 9 reports also the coefficient of the generated regressor: rejecting the null hypothesis that the coefficient of the residual is zero can be interpreted as evidence of endogeneity. Since the outcome equation in the second step includes generated regressors, we use bootstrap with 500 replications and base our confidence intervals on the resulting empirical cumulative distribution function of the estimator.

As in previous sections, when we do not take endogeneity of education into account we find a negative relationship between years of schooling and the number of children, with the magnitude of this correlation being essentially the same s the one delivered by OLS regressions. When we isolate the exogenous variation in years of education driven by compulsory schooling laws, the sign of the relationship is reversed: the average partial effect on the average number of children is around 0.18, very similar to our 2SLS estimates albeit less precise (Columns (1) and (2)). The same holds when we allow for censoring (see columns (4) and (5) in Table 9). In addition, the null hypothesis that the residual coefficient is zero is always rejected, pointing to endogeneity of education in the fertility equation.

Since the distribution of the number of births is approximately normal (see Figure 3 in the Appendix), we also estimate Tobit regression models by maximum likelihood (Table 10). By estimating a Tobit model, we model jointly the decision on whether to enter motherhood and the decision on the actual number of children, allowing

Table 10: Tobit regression models results

	Right censoring (4)		Right censoring (4) & Corner solution (0)	
	(1)	(2)	(3)	(4)
Coefficient	-0.037	0.217	-0.043	0.274
	(0.005)***	(0.082)***	(0.006)***	(0.098)***
	Average Partial Effects			
APE[a] on				
$Prob[Y=0]$	0.003	-0.026	0.005	-0.036
	(0.001)***	(0.011)***	(0.001)***	(0.013)***
$E[Y\|Y>0]$	-0.029	0.157	-0.031	0.180
	(0.004)***	(0.058)***	(0.004)***	(0.063)***
$E[Y\|1<Y<4]$	-0.015	0.064	-0.014	0.062
	(0.002)***	(0.023)***	(0.002)***	(0.021)***

NOTES: Each column and panel represents a separate regression based on Sample 10. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions. Cluster-robust standard errors are in parentheses. ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level. Columns (1) and (3): education treated as exogenous. Columns (2) and (4): education treated as endogenous. [a] APE stands for Average Partial Effect at mean values of covariates in the sample.

for correlation between these choices.[22] We allow alternatively for right censoring (columns (1) and (2) in Table 10) and for right censoring and corner solutions at 0 (columns (3) and (4) in Table 10). Using the estimates, we assess the average partial effect of education on the probability to be childless and on the average number of children for women who decide to: (i) have at least one child; (ii) have more than 1 but not more than 4 children. Columns (1) and (3) refer to the estimation results when education is treated as exogenous while in Columns (2) and (4) education is treated as endogenous. We confirm previous results in terms of direction of the effects: the association between education and fertility is negative while the causal effect is positive, i.e. education increases fertility and reduces childlessness. While the magnitude of the effect on the average number of children, conditional on entering motherhood, is similar to those estimated using linear regression models, the magnitude of the effect on childlessness is smaller, around 50 percent lower than

[22]This comes at the expense of imposing the same coefficient on education in the equation determining the two choices, as in standard Tobit models. Consider that it is difficult to think about an instrument for education for the motherhood equation than can be excluded from the equation for the number of children, once the woman enters motherhood.

the one previously estimated, which might be due to the restriction imposing equal coefficients in the Tobit model.

Overall, our results are robust to the choice of the regression model in terms of direction of the effects on completed fertility and childlessness and also with respect to the magnitude of the effect on the average number of children. This may be due to the fact that the amount of censoring is very small (less than 5 percent of the sample), and that the distribution of the number of births is approximately normal.

5.4 Further robustness

Last but not least, we show that our results are robust to the selection of samples, the choices of reforms and the countries we are analyzing. As described above, our samples are not necessarily symmetric around the pivotal cohort, since in some countries 10, 7 or 5 cohorts before and after the reform are not always available. In some countries, the reform was too early or too late for our sampling period or another reform was implemented early on. Table 11 shows the 2SLS estimates when we restrict our samples to symmetric windows around the reforms. The results are very robust to that.

In some countries, more than one compulsory schooling reforms were implemented in our observation period. Table 11 shows the 2SLS estimates when we use all those reforms for our analysis, again the results are very robust.

A further sensitivity check is based on the selection of countries we are using. Table 12 presents ours results, when we drop one country at a time from the sample. Again, the results are very robust.

Table 11: Sensitivity to samples and reforms

	Symmetric windows			All reforms		
	Sample 10	Sample 7	Sample 5	Sample 10	Sample 7	Sample 5
# biological kids	0.199	0.270	0.197	0.145	0.214	0.214
	(0.096)**	(0.118)**	(0.077)**	(0.063)**	(0.097)**	(0.097)**
Childlessness	-0.118	-0.143	-0.102	-0.070	-0.127	-0.098
	(0.038)***	(0.047)***	(0.031)***	(0.021)***	(0.033)***	(0.024)***
Observations	5,784	4,731	3,830	8,733	6,683	5,206

NOTES: Each coefficient represents a separate 2SLS regression. All reforms include additional reforms in the Czech-Republic (1953/1960), France (1936) and the Netherlands (1947/1950). Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

Table 12: Sensitivity to countries

	One reform per country			All reforms		
	# biological kids	Childlessness	Obs	# biological kids	Childlessness	Obs
w/o AUT	0.205	-0.073	6,303	0.141	-0.069	8,308
	(0.079)***	(0.026)***		(0.067)**	(0.022)***	
w/o CZE	0.197	-0.077	6,337	0.146	-0.085	7,335
	(0.074)***	(0.026)***		(0.074)**	(0.027)***	
w/o DNK	0.306	-0.092	5,760	0.202	-0.085	7,765
	(0.112)***	(0.035)***		(0.074)***	(0.027)***	
w/o ENG	0.222	-0.089	4,329	0.173	-0.074	6,334
	(0.083)***	(0.029)***		(0.060)***	(0.022)***	
w/o FRA	0.206	-0.089	5,912	0.092	-0.071	7,531
	(0.087)**	(0.029)***		(0.070)	(0.022)***	
w/o GER	0.180	-0.067	6,378	0.121	-0.063	8,383
	(0.069)***	(0.023)***		(0.061)**	(0.020)***	
w/o ITA	0.195	-0.069	5,619	0.105	-0.066	7,624
	(0.153)	(0.056)		(0.097)	(0.036)*	
w/o NLD	0.224	-0.082	6,458	0.199	-0.066	7,851
	(0.081)***	(0.027)***		(0.066)***	(0.022)***	

NOTES: Each coefficient represents a separate 2SLS regression based on sample 10. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and proxy interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

6 Concluding remarks

We study the effects of education on fertility for women in 8 European countries using exogenous variation in education brought along by mandatory schooling reforms in the 1930s-60s. Contrary to conventional wisdom, we show that more schooling can lead, in fact, to higher fertility. Using our instrumental variables estimates, we find that one additional year of schooling increases the number of children by 0.2 - 0.3[23], whereas the probability to remain childless falls by 7.5 - 13 percentage-points with somewhat smaller numbers in the Tobit specification. A number of falsification and robustness tests, like placebo reforms, tests for functional form or selective mortality in the sample strongly corroborates these findings.

What are the mechanisms behind these results? We argue that compulsory schooling reforms target a specific group in the population: those at the lower end of the educational distribution. For these women the income effect of increased schooling may outweigh the substitution effect on the labor market and thus higher education will result in higher fertility rates.[24]

Moreover, we find that compliers to compulsory schooling reforms are more likely to have grown up in larger and also poorer families relative to the average woman in the sample. Growing up in a large family might generate a positive attitude towards family and having kids.

Next to these labor market effects, the marriage market may also play a role in shaping fertility. We present evidence that additional schooling leads to i) a higher probability to get married, ii) a lower divorce/separation rate and iii) a potential partner who is better educated and, thus, more able to access financial resources

[23] Braakmann (2011) finds an effect of 0.27 for Britain using the same source of exogenous variation in education.

[24] Using a slightly different set of countries and reforms than we use in this study, Brunello, Fort and Weber (2009) find that education increases earnings of both, males and females, with the largest effects for females and at the lower part of the income distribution for both genders. The causal effect of one additional year of education amounts ranges between 4% and 7% and for males and between 5% and 10% for females for each additional year of schooling.

himself. We discuss the internal validity of our results in detail and provide complementary evidence on the absence of a direct effect of male education on fertility.

Research on non-monetary effects of education has expanded enormously in recent years (Oreopoulos and Salvanes, 2011). Assessing the impact on fertility – in particular at the lower end of the educational distribution – is an important part of it. Our research shows that previously assumed socio-economic gradients may be invalid; a fact, which may have long-lasting impacts on the structure of society in the future with wide-ranging consequences.

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A Appendix: Additional Figures

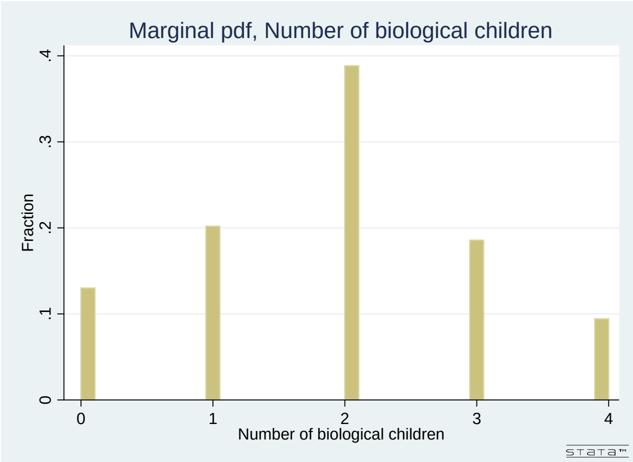

Figure 3: Distribution of the number of children in the sample.

B Appendix: Educational Reforms in Europe

In this section, we briefly describe the compulsory schooling reforms we are using in this study. The choice of reforms differs somewhat from Brunello et al. (2012, forthcoming) and Brunello et al. (2009) because the individuals in our data-set are of age 50 or older at the time of the interviews in 2004-2007. Thus, we are not able to consider more recent reforms in this study. For further details on educational reforms in Europe see Fort (2006).

Austria In 1962 a federal act was passed that increased compulsory schooling from 8 to 9 years. The law came into effect on September 1, 1966. Pupils who were 14 years old (or younger) at that time had to attend school for an additional year. Since compulsory education starts at the age of 6 and the cut-off date for school-entry is September 1, (mostly) individuals born between September and December 1951 were the first ones affected by the reform. Thus, the pivotal cohort is 1951.

Czech Republic In the 20^{th} century, compulsory education was reformed several times. In 1948 compulsory schooling was increased from 8 to 9 years (age 6 to 15). It was reduced to 8 in 1953 and increased to 9 again in 1960. Two further changes took place in 1979 and 1990. We consider only the first reform in 1948 for our main analysis. However, for a robustness test, we add the reforms in 1953 and 1960. The pivotal cohorts are 1934 (for the first reform), 1939 (for the second) and 1947 for the reform in 1960. See Garrouste (2010) for more information on compulsory schooling reforms in the Czech Republic.

Denmark In 1958 compulsory education was increased by 3 years, from 4 to 7. In 1971 compulsory schooling was further increased by 2 years, from 7 to 9. Education started at age 7, thus pupils who were 11 years old (or younger) in 1958 were potentially affected by the first reform, i.e. children born in 1947 and after. Since our data only cover individuals 50+ in 2004/2006, we only consider the first reform for this study.

England Two major compulsory schooling reforms were implemented in the UK in 1947 and 1973. The first reform increased the minimum school leaving age from 14 to 15, the second reform from 15 to 16. Since the school-entry age is 5 in the UK, compulsory schooling was increased from 9 to 10 years in 1947 and from 10 to 11 years in 1973. Pupils who were 14 years old (or younger) in 1947 were affected by the first reform, i.e. cohorts born in 1933 and after. Due to the sampling frame of ELSA (individuals 50+), we only consider the first reform in this study.

France Two education reforms were implemented in France. In 1936, compulsory schooling was increased from 7 to 8 years (age 13 to 14) and in 1959 from 8 to 10 years (age 14 to 16). After a long transition period, the second reform came into effect in 1967. The first reform affected pupils born 1923 (and after) and the second reform pupils born 1953 (and after). For our main analysis we only consider the second reform, however for the robustness check, we add the 1936 reform.

Germany In the former Federal Republic of Germany compulsory schooling was increased from 8 to 9 years, gradually among the German states, starting from the reform 1949 in Hamburg to 1969 in Bavaria. Due to the small sample size in several German states, we only consider 4 German states: Baden-Wuerttemberg, Hesse, Northrhine-Westphalia and Rhineland-Palatinate. In these states the education reform was implemented in 1967. The first cohort potentially affected by this reform is the cohort born in 1953.

Italy In 1963 junior high school became mandatory in Italy, which increased compulsory years of schooling by 3 years (from 5 to 8 years). The first cohort potentially affected by this reform is the cohort born in 1949.

Netherlands The Netherlands experienced many changes in compulsory education in the last century. In this paper, we consider three education reforms: in 1942, in 1947 and in 1950. Within the first reform compulsory schooling was increased from 7 to 8 years, the second reform led to a decrease to 7 years and the third reform increased schooling again by 2 years, from 7 to 9. Accordingly, we choose the cohorts born in 1929, 1933 and 1936 as pivotal cohorts. We have chosen the first reform for our main analysis and added the second and third as a robustness check. See Levin and Plug (1999) for more details on these reforms.