Routledge
Taylor & Francis Group

European Societies

ISSN: (Print) (Online) Journal homepage: https://www.tandfonline.com/loi/reus20

The effects of female education on child education: a prospective analysis

Michael Grätz

To cite this article: Michael Grätz (06 Nov 2023): The effects of female education on child education: a prospective analysis, European Societies, DOI: 10.1080/14616696.2023.2275591

To link to this article: https://doi.org/10.1080/14616696.2023.2275591

EUROPEAN SOCIETIES
https://doi.org/10.1080/14616696.2023.2275591

The effects of female education on child education: a prospective analysis

Michael Grätz

Swiss Centre of Expertise in Life Course Research LIVES, University of Lausanne, Swedish Institute for Social Research (SOFI), Stockholm University, Lausanne, Switzerland

ABSTRACT

This study estimates the effects of women's education on their offspring using quasi-experimental evidence from six educational reforms that increased the length of compulsory schooling in several European countries. The empirical analysis uses data from the Survey of Health, Ageing and Retirement in Europe and instrumental variable estimation to estimate the effects of female education on fertility and on children's education. This study provides the first analysis using quasi-experimental variation in education to estimate prospective models of intergenerational effects. These models start with a birth cohort and link information on their fertility and on their children's outcomes. These models account for the effect of female education on the probability that women have children when estimating the effect of female education. The direct effect of female education on children's educational attainment, i.e. the effect conditional on the birth of a child, is positive. In addition, higher female education increases fertility. Therefore, the probability that a woman has a child with a high educational attainment is increased when considering the effect of female education on fertility. Studies that estimate retrospective models of intergenerational effects using reforms in the length of compulsory schooling may underestimate the total effect of female on child education.

ARTICLE HISTORY Received 17 September 2022; Accepted 21 June 2023

EDITED BY Marta Dominguez-Folgueras

KEYWORDS Education; fertility; intergenerational mobility; Quasi-experiment

CONTACT Michael Grätz michael.gratz@unil.ch Swiss Centre of Expertise in Life Course Research LIVES, University of Lausanne Bâtiment Géopolis, Lausanne 1015, Switzerland

Supplemental data for this article can be accessed online at https://doi.org/10.1080/14616696.2023.2275591.

Introduction

A growing body of literature estimates prospective instead of retrospective models of the intergenerational transmission of advantage (Breen and Ermisch 2017; Breen *et al.* 2019; Hillmert 2013, 2015; Lawrence and Breen 2016; Maralani 2013; Mare 1997; Mare and Maralani 2006; Richter 2016; Skopek and Leopold 2020; Song and Mare 2015). Retrospective models of intergenerational mobility start with a sample of children to whom parental characteristics are added. In contrast, prospective models start with a sample of parents and add information on their children. Because of this sampling approach, prospective models of intergenerational mobility can consider the associations between female (and male) education and fertility when estimating the association between female (male) education and their offspring's education. Prospective models of intergenerational mobility differ from retrospective models of mobility if increasing the education of women (and men) not only affects the education of the children born to these women (and men) but also influences the probability of women (or men) having children and therefore being able to transmit education across generations. The birth of children is a necessary condition for any transmission of advantageous resources (e.g. education) to take place (Breen and Ermisch 2017; Breen *et al.* 2019; Duncan 1966; Lawrence and Breen 2016; Maralani 2013). Therefore, the effects of increasing education on fertility must be considered to obtain a complete picture of the process of the intergenerational transmission of advantage.

While such a perspective is theoretically appealing, the empirical evidence reported in the studies quoted above may not be causal due to the possibility of omitted variable bias. Estimates of the effects of parental resources on children may be confounded by unobserved variables, such as innate parental abilities and ambition. To take up a distinction made by Björklund and Jäntti (2020), the prospective studies available thus far have analyzed intergenerational mobility, but they have not analyzed intergenerational effects. The sociological studies employing prospective models of intergenerational mobility have not employed research designs that control for the influence of unobserved variables, which could lead to omitted variable bias.

There are many studies in economics that estimate intergenerational effects and therefore analyze the causal question, 'What would happen to the offspring outcome if parental income or education were changed by means of some intervention' (Björklund and Jäntti 2020, pp. 3–4) using a research design controlling for the influence of unobserved variables. These studies employ three main research designs to identify the causal effects of parental education on child education: (1) the comparison of (monozygotic) twins (e.g. Amin *et al.* 2015), (2) adoption studies (e.g. Björklund *et al.* 2006), and (3) the use of educational reforms to instrument parental education (e.g. Lundborg *et al.* 2014). These studies take a retrospective approach to estimate intergenerational effects, taking as a starting point a sample of children to which information on the characteristics of their parents is added. By this design, these studies condition on the birth of children.

The novel contribution of the present study is that it combines the literature on intergenerational effects in economics with the literature on prospective models of intergenerational mobility in sociology. I estimate the causal effects of female education on the next generation using a research design that controls for the influence of unobserved variables in a prospective framework. By modelling the effect of female education in one generation on the education of the next generation conditional and unconditional on the birth of a child, the contribution of childlessness to the intergenerational transmission of education is identified. This is done by making use of six policy reforms, which increased the length of compulsory schooling in Austria, the Czech Republic, Denmark, France, Italy, and the Netherlands in the middle of the twentieth century. This is the first study employing a quasi-experimental research design to estimate the effect of female education on child education in a prospective framework.[1]

The study also contributes substantively new insights to the literature on prospective models of the intergenerational transmission of education, as the reforms I look at increased a rather low level of female education. Previous research estimating prospective models of intergenerational mobility focused on increases in university education (e.g. Breen and Ermisch 2017; Breen *et al.* 2019; Lawrence and Breen 2016). Investigating the consequences of increasing female education at lower levels of education allows us to expand the focus of prospective models of intergenerational mobility and therefore has not only methodological but also substantive importance.

[1]The analysis is limited to the effects of female education because, as will be shown below, the reforms did not affect male education. Previous research has shown that the differences between prospective and retrospective models of educational mobility are larger for women than for men (Breen and Ermisch 2017; Skopek and Leopold 2020).

Prospective and retrospective approaches to estimating the effects of education in one generation on the next generation

The intergenerational effects literature estimates the causal effects of education in one generation on the education of the next generation (Björklund and Jäntti 2020). However, selecting a sample of children and, therefore, conditioning on the birth of children does not estimate the total causal effect of education in one generation on education in the next generation. Although the direct effect of education in one generation on child education conditional on the birth of a child is also an interesting quantity, for many questions, it is the total effect of education that we are interested in. For instance, if we think about the consequences of policies increasing education, the total effect of education must be estimated to tell us what the consequences of a policy raising education in one generation are for the educational attainment of the next generation (Breen and Ermisch 2017).

Song and Mare (2015) distinguished between retrospective and prospective approaches to intergenerational mobility. Retrospective approaches start with a child generation and add parental characteristics. These approaches do not lead to a representative parental generation, as not all men and women in the parental generation have children (Duncan 1966; Lawrence and Breen 2016). This approach is most often taken by research on intergenerational educational mobility in sociology and other disciplines. It is also the approach usually taken by studies in economics that estimate the causal effect of parental education on child education (e.g. Amin *et al.* 2015; Behrman and Rosenzweig 2002; Björklund *et al.* 2006; Black *et al.* 2005; Chevalier 2004; Hægeland *et al.* 2010; Holmlund *et al.* 2011; Lundborg *et al.* 2014; Oreopoulos 2006; Oreopoulos *et al.* 2006; Piopiunik 2014; Plug 2004; Sacerdote 2002).

Contrary to retrospective approaches, prospective approaches to intergenerational mobility start with a birth cohort and follow the social reproduction of this birth cohort. By these means, prospective models consider the associations between education and fertility and estimate the association between female (or male) education and child education without conditioning on the birth of children (Breen and Ermisch 2017; Lawrence and Breen 2016; Maralani 2013). This difference is visualized by the box 'childbirth' in the directed acyclic graph (DAG) shown in Figure 1 (Lawrence and Breen 2016).

Figure 1 presents a DAG (Elwert [2013]) portraying the intergenerational transmission of advantage. Studies that analyze the effect of

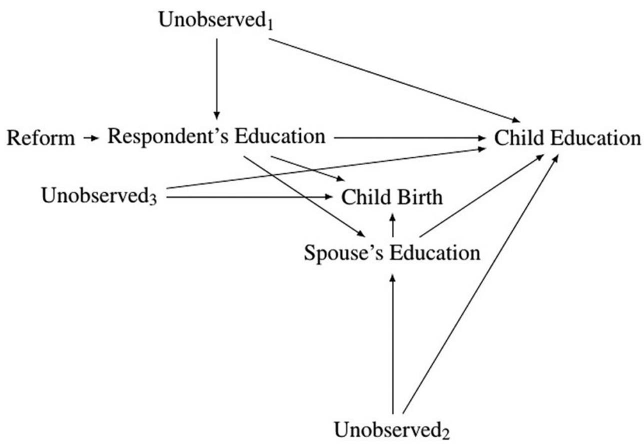

Figure 1. Directed Acyclic Graph (DAG) of the transmission of education to offspring.

respondents' education among a selected sample of respondents who have children necessarily condition on whether a respondent has a child. To estimate the total causal effect of the respondent's education on child education, it is necessary to estimate this effect without conditioning on the birth of a child. The total causal effect of female (or male) education on child education cannot be analyzed among a sample of mothers (fathers) but requires a representative sample of a generation of women (men).

Retrospective models of intergenerational mobility can be interpreted as estimating direct effects conditional on the birth of a child. The direct effect of maternal education on child education can be due to mechanisms such as increasing the level of education in the maternal generation leads to children growing up in an environment that is more beneficial for their educational development. In addition, increasing the level of maternal education can lead to higher maternal income and wealth; these resources can also be beneficial for the offspring's educational development.

Often, these direct effects, which are the result of conditioning on a post-treatment variable (here, the birth of a child), are not well defined. One way to define them is as controlled direct effects (Acharya *et al.* 2016). The controlled direct effect can be interpreted as the counterfactual of an intervention that would make every woman (or man) have a child. The identification of this controlled direct effect requires that there are no unobserved variables confounding the relationship between the mediator (having a child) and the outcome (child education). These variables are indicated by the box 'Unobserved$_3$' in Figure 1.

One further remark is necessary with respect to Figure 1, and this relates to the influence of the partner's resources. Several studies on the intergenerational transmission of education have argued that maternal and paternal resources should be included simultaneously in models estimating the intergenerational transmission of advantage (Acker 1973; Beller 2009; Bloome and Western 2011; Buis 2013; Jæger 2007; Hout 2018; Kalmijn 1994; Korupp *et al.* 2002; Mare 1981; Marks 2008; Mood 2017; Song and Mare 2017). According to these studies, omitting the spouse's resources from an analysis of the relationship between a respondent's socioeconomic resources (e.g. education, income, or occupation) and her or his child's socioeconomic resources leads to omitted variable bias. To lead to omitted variable bias, the spouse's resources must be confounding variables. It is, however, unclear whether the spouse's resources are indeed confounding and not mediating variables. They are only confounding variables if a respondent's resources do not affect the spouse's resources. However, assortative mating (Blossfeld 2009; Schwartz 2013) is likely to induce a relationship between a respondent's and her/his spouse's socioeconomic and educational resources. If that is the case, the resources of a respondent's spouse are mediating variables (Grätz 2022; Holmlund *et al.* 2011; Lawrence and Breen 2016). We are interested in estimating the total causal effect of respondents' education on child education. The DAG shows that spouse education is a variable lying on the causal path running from the respondent's education to the child's education.[2] Therefore, conditioning on spouse's education leads to overcontrol bias.[3]

As this is usually the case, our estimate of the causal effect of respondent's education on child education in Figure 1 is likely to suffer from omitted variable bias, as unobserved variables, such as parental abilities and the motivation of parents to foster the development of cognitive and noncognitive skills in their children, confound the relationship

[2]This is true even if the education of the spouse is completed before partners meet because a woman's (or a man's) level of education affects who they partner with.

[3]We could estimate the direct effect of the respondent's education on the child's education conditional on the spouse's education. However, this direct effect does not answer the research question of this study, which refers to the total causal effect ('What is the effect of female education in one generation on child education in the next generation?'). In addition, conditioning on spouse's education is likely to introduce endogenous selection bias (Elwert and Winship 2014; Grätz 2022; Lawrence and Breen 2016). The direct effect can be correctly identified only if we can condition on all confounding variables that affect the relationship between the spouse's education and the child's education. Many of these variables are likely to be unobserved. These variables are indicated by the box 'Unobserved$_2$' in Figure 1. Finally, even though the information on the partner is principally available in the data, the many missing values on the partner's education considerably reduce the sample size, making such an analysis in an IV framework very unreliable.

between the respondent's education and the child's education. These unobserved variables are indicated by the box 'Unobserved$_1$' in Figure 1.

The DAG also illustrates how the causal effect of the respondent's education on child education can be identified, namely, by using a variable to instrument the respondent's education (Angrist *et al.* 1996). The present study uses the length of compulsory schooling, which was changed by educational reforms in Austria, the Czech Republic, Denmark, France, Italy, and the Netherlands, as an instrumental variable.

Previous studies estimating the effects of parental education on child education

Previous studies in economics used reforms in the length of compulsory schooling to instrument parental education in a retrospective framework (Black *et al.* 2005; Chevalier 2004; Holmlund *et al.* 2011; Lundborg *et al.* 2014; Oreopoulos 2006; Oreopoulos *et al.* 2006; Piopiunik 2014). Some of these studies compared the direct effects of male and female education, conditional on the birth of a child, on child education. For instance, Lundborg *et al.* (2014) found, using a reform that increased the length of compulsory schooling in different regions in Sweden between 1949 and 1969, a positive direct effect of women's education on their sons' cognitive skills at age 18 but no direct effect of male education on any of these outcomes. Using a reform in England and Wales in 1972, Chevalier (2004) found a positive direct effect of both mothers' and fathers' education on children's educational attainment.

This literature is particularly relevant because there is also a related literature employing these reforms in the length of compulsory schooling to estimate the effects of female education on fertility. Cygan-Rehm and Maeder (2013) used the compulsory school leaving age reform in Germany to estimate the effects of female education on fertility and found that increasing female education reduced fertility in this context. Fort *et al.* (2016) compared the effects of various reforms in the length of compulsory schooling in different European countries. They found, similar to Cygan-Rehm and Maeder (2013) an increasing female education to reduce fertility in England. However, the opposite was found for the other countries that Fort *et al.* (2016) analyzed (Austria, Denmark, France, Italy, and the Netherlands): In these countries, increasing female education due to the reforms led to an increase in fertility.[4] With respect to Canada, DeCicca and Krashinsky (2023) found increasing female education to increase the probability that a woman had a child but lowered the completed fertility rate.

There are two alternative approaches used in economics to estimate the effects of fathers' and mothers' education on child education. First, some studies used twin-fixed effects models (Amin *et al.* 2015; Behrman and Rosenzweig 2002; Ermisch and Pronzato 2011; Hægeland *et al.* 2010; Holmlund *et al.* 2011; Pronzato 2012). The twin-fixed effects approach is based on a strong assumption: differences between twins in educational attainment must be random. If twins select themselves into education, this approach leads to biased estimates of the effects of fathers' and mothers' education on child education. In addition, twins are a selected group, and it is unclear whether estimates based on twins generalize to the whole population (Bound and Solon 1999). Behrman and Rosenzweig (2002) found, using data on monozygotic (MZ) twins from Minnesota, a positive effect of fathers' education but a negative effect of mothers' education on children's education. Holmlund *et al.* (2011) found similar results using data on MZ and dizygotic (DZ) twins in Sweden. Pronzato (2012) found stronger effects of fathers' education than of mothers' education using MZ and DZ twin data from Norway. Hægeland *et al.* (2010) obtained, using data on MZ and DZ twins, similar findings using test scores as an outcome in Norway. Employing data on MZ twins from Sweden, Amin *et al.* (2015) found an equally strong positive effect of both paternal and maternal education on child education.

Second, studies have employed data linking adopted children to the characteristics of the parents who adopted them (Björklund *et al.* 2006; Plug 2004; Sacerdote 2002). This approach identifies a causal effect of parental education on child education under the assumption that the allocation of children to parents is random. Sacerdote (2002) found the education of the mother to positively affect the education of the adopted child. Plug (2004) found positive effects of fathers' education on adopted children's education but no effects of mothers' education in the United States. Using data from Sweden, Björklund *et al.* (2006) found both fathers' and mothers' education to have equally strong effects on adopted children's education.

Most of these studies employed a retrospective approach and did not study the social reproduction of a birth cohort. Even the studies that

[4]As in the analysis reported in the present study, Fort *et al.* (2016) analyzed a pooled sample of these countries and did not report separate results differentiating between Austria, Denmark, France, Italy, and the Netherlands.

employed a prospective sample (e.g. Ermisch and Pronzato 2011) did not estimate the effect of female education on fertility but estimated intergenerational effects among women (and/or men) who had children. Therefore, these studies conditioned on the birth of children and did not estimate the total effect of female (or male) education on child education.

Analytic strategy

To estimate the causal effect of female education on fertility and the education of the next generation, I employ instrumental variable estimation (Angrist *et al.* 1996). The causal effect of female education is identified using the length of compulsory schooling stipulated by the law as an instrumental variable (Schneeweis *et al.* 2014). The DAG reported in Figure 1 provides a graphical representation of how a causal effect is identified using an instrumental variable. Using the length of compulsory schooling to instrument education controls for unobserved and observed variables that confound the effects of female education on fertility and child education.

The length of compulsory schooling stipulated by law varies across both birth cohorts and countries, as indicated by Table 1. Formally, I estimate, in the first stage, the following models with respondents i from country c and birth cohort b:

$$E_{ibc} = \alpha + \beta_1 \mathrm{Comp}_{bc} + \beta_2 \mathrm{Country}_c + \beta_3 \mathrm{Cohort}_b + \beta_4 \mathrm{Trend}_{bc} + \varepsilon_{ibc} \tag{1}$$

The equation includes the respondent's (female) education E and the length of compulsory schooling as stipulated by the law Comp_{bc}. The estimation controls for fixed effects at the country and cohort levels as well as for country-specific linear time trends Trend_{bc}. These controls aim at

Table 1. Overview over the reforms in the length of compulsory schooling used to instrument respondent's education.

Country (year of the reform)	Year of birth of the first cohort affected by the reform	Change in the length of compulsory schooling	Years of birth of the pre-reform cohort	Year of birth of the post-reform cohort
Austria (1962)	1952	8–9 years	1943–1951	1953–1961
Czech Republic (1960)	1947	8–9 years	1938–1946	1948–1956
Denmark (1972)	1957	7–9 years	1948–1956	1958–1966
France (1959)	1953	8–10 years	1944–1952	1954–1962
Italy (1962)	1949	5–8 years	1940–1948	1950–1958
Netherlands (1971/1975)	1957	8–10 years	1948–1956	1958–1966

isolating the effects of the increase in education due to the reforms in the length of compulsory schooling from variation across countries and over cohorts unrelated to the change in the law (see Schneeweis *et al.* 2014 for a similar approach).

The second stage equation is as follows:

$$Y_{ibc} = \gamma + \delta_1 \hat{E}_{ibc} + \delta_2 \text{Country}_c + \delta_3 \text{Cohort}_b + \delta_4 \text{Trend}_{bc} + \varepsilon_{ibc} \quad (2)$$

which is estimated using 2SLS with $\hat{E}$ being instrumented by the continuous variable length of compulsory schooling Comp. Y indicates the three outcomes of the analysis: (1) the education of the child conditional on the birth of a child, (2) childlessness, and (3) the educational attainment of the child without conditioning on the birth of a child.

The interpretation of the IV estimates rests on three untestable assumptions: (1) the exclusion restriction, (2) monotonicity, and (3) the stable unit treatment value assumption (SUTVA). In the following, I discuss the plausibility of these assumptions. First, the exclusion restriction requires that the only path through which the reforms in the length of compulsory schooling affect child education is by altering female education. This assumption is likely to hold, as it is hard to imagine how educational reforms should affect women's fertility in a direct way, which is not mediated by education.

Second, the monotonicity assumption requires that women only increase (and not decrease) their educational attainment due to the reforms. This assumption is also likely to be fulfilled, as it seems unlikely that any woman would have reduced her educational attainment due to the reforms. Therefore, it is likely that the population consists only of always-takers, i.e. women who would have attained a higher level of education independent of the reform, never-takers, i.e. women who would have left school early both before and after the reform, and compliers, i.e. women who were positively affected by the reform in their educational attainment. The monotonicity assumption only applies if there are heterogeneous effects (which is likely to be the case).

Even under the monotonicity assumption, the estimates of the IV models can only be interpreted as local average treatment effects (LATEs) or, to be more precise, in the specific case of the models employed here with a continuous variable being instrumented, as the average causal response (ACR) (Angrist and Pischke 2009). The ACR is different from the average treatment effect (ATE), as only respondents who are affected by the instrument (the educational reforms) contribute to the estimate of the ACR. Contrary to the IV estimates, the OLS estimates provide an estimate of the ATE, although the OLS estimate is likely to be biased. Given that the IV strategy is implemented by combining data on several countries, the analytical strategy is also based on the assumption that the population of compliers does not vary across countries.

The ACR interpretation of the instrumental variable estimates requires an additional assumption, namely, that there is no further heterogeneity in the relationship between the instrument and the treatment within the group of those affected by the instrument, i.e. the compliers (Breen and Ermisch 2021; Heckman *et al.* 2006). Such heterogeneity is problematic only if the treatment effects are heterogeneous, but this is likely to be the case. If there are heterogeneous treatment effects and there is heterogeneity in the instrument-treatment relationship, the ACR interpretation becomes problematic (Breen and Ermisch 2021). In particular, the ACR interpretation in the present analysis requires that there be no such heterogeneity across countries, as the IV estimate is obtained by pooling data on different countries.

Third, the SUTVA requires that the change in the value of education of one respondent be unaffected by the change in education for other respondents (Angrist *et al.* 1996). This assumption is fulfilled if women are not affected in their decision to remain in education by the effects of the reform on other women. A possible violation of the SUTVA would occur if women who were not affected by the reform decided to stay longer in education to be better able to compete with the after-reform cohorts in the education system and in the partner market. While such a violation of the SUTVA seems rather unlikely, it cannot be completely ruled out. The exact timing of the implementations of the reforms in the specific countries was unforeseeable, and when the reforms were implemented, older cohorts had already left the education systems in the specific countries. This makes the SUTVA likely to hold.

The research design of this study relies on variation across countries (and time) to identify the effect of female education. Consequently, it ignores that the effects of female education could vary across countries. Whilst cross-country variation can potentially be important, I would still argue that the present analysis allows us to obtain a baseline estimate of the effects of female education. Future research can exploit variation across countries, but such an approach will require another research design and data set. In the SHARE, the number of respondents per country is too small to identify the country-specific effects of female education in an IV framework.

Data and variables

Data

I employ data on Austria, the Czech Republic, Denmark, France, Italy, and the Netherlands from the Survey of Health, Ageing and Retirement in Europe (SHARE) (Börsch-Supan *et al.* 2013). The SHARE samples respondents aged 50 and older as well as their partners.[5] I use pooled data from all seven waves of the SHARE currently available (Börsch-Supan 2019). The last wave of the survey (wave 7) was collected in 2017. Even though the SHARE is a panel data set, I employ only one observation per individual using the most recent information available for each variable. I limit the sample to respondents who were born in the country in which they were interviewed and who were at least 40 years old at the last wave at which they participated. The first sample selection criterion ensures that the respondents could have been affected by the reforms in the length of compulsory schooling. The second criterion ensures that they have completed their fertility.[6]

The unit of analysis is the women (and, for one part of the analysis, the men) in the pre- and post-reform cohorts. These women can have several children in the data. However, I look only at the highest educational attainment of any of these children, as explained in detail in the next section.

The programming code with which to replicate all analyses is available under https://osf.io/j4rga/.

Variables

Years of schooling (respondent)

I measure respondents', i.e. women's (and men's), educational attainment via years of schooling. Two sources of information are used to construct this variable: (1) respondents' self-reported years of schooling and (2) respondents' self-reported education classified into ISCED1997 categories. I use information from both sources, giving priority to the self-reported years of schooling. Only for the respondents with missing information on self-reported years of schooling do I assign the typical

[5]There is a possibility of bias introduced by mortality. While I cannot account empirically for this bias, I believe it be minimized by using a data set that is specifically targeted at sampling the older population (instead of using data that are representative for the whole adult population).

[6]The same and/or similar reforms to the ones I look at have been studied by previous studies, which estimated, for instance, the effects of education on cognitive skills in adulthood (Schneeweis *et al.* 2014), earnings (Brunello *et al.* 2009; Grenet 2013), fertility (Fort *et al.* 2016), mortality (Gathmann *et al.* 2015), and political attitudes (Cavaille and Marshall 2019).

years of schooling based on the ISCED category of their highest educational qualification. This procedure follows Schneeweis *et al.* (2014). The resulting variable is a continuous indicator of educational attainment, with higher values indicating more education.

Length of compulsory schooling

The reforms affected the length of compulsory schooling. I assign to each birth cohort in each country the length of compulsory schooling as indicated by the law effective at the time they went to school. Table 1 gives an overview of the reforms, the length of compulsory schooling, and the assignment of pre- and post-reform cohorts in every country. More details on the reforms are provided in the *Online Supplement*.

In every country, I limit the pre- and the post-reform cohorts to nine birth years. The fewer birth years are included in a cohort, the more likely the identifying assumptions of my research design, which were discussed in the 'Analytic Strategy' section, are met. However, the consideration of too few birth years in a cohort leads to small sample sizes and a weak first stage in the IV models, which will result in biased estimates (Bound *et al.* 1995; Staiger and Stock 1997). In all countries, I drop the first birth year affected by the reform from the analysis sample because this birth year may have been only partially affected by the reform.

Gender

In Table 3 below, I demonstrate that only female education (but not male education) was affected by the reforms I look at. Therefore, the main analysis is limited to women.

Child upper secondary education

I measure child education using a dummy variable that is coded as one if a respondent has at least one child with upper secondary education (level 3 of the ISCED1997 educational classification and higher) and zero if all her children have less education.[7] This variable looks at the child with the highest educational attainment. The models using this outcome variable measure the effect of female education on child education conditional on the birth of a child. I choose upper secondary education as an outcome

[7]A limitation of the data is that the SHARE only collects information on a maximum of four respondents per woman. It seems unlikely that a woman would have four children with a low level of education who are included in the data and a fifth child with a high level of education who is not included in the data. However, if such cases arise in a sufficiently large number, the estimates reported in this article could be biased.

because it is related to the level of education that was affected in the parental generation. In addition, using a measure of child education requires restricting the sample to children who are old enough to have completed their education (Breen and Ermisch 2017; Skopek and Leopold 2020). When analyzing children's educational outcomes, I limit the sample to women with children who were born in 1998 and earlier. This ensures that the children were old enough to have completed upper secondary education in case they stayed in education until this level. Given that the last wave of the data was collected in 2017, the youngest children were 19 years old when data on their attainment of upper secondary education were collected. For that reason, it is not possible to use tertiary education as an outcome because children have not completed tertiary education at age 19.

Having a child

I construct a dummy variable that is coded as one if a respondent has a child and as zero if a respondent has no children. The information I use to build this variable is based on the most recent interview for each respondent. Given that I consider only women aged 40 and older when this variable is measured, I measure the completed fertility of these women in practically all cases.

Having a child with upper secondary education

This variable is different from the 'child upper secondary education' variable, as it also includes respondents who do not have any children. The dummy variable is coded as one if a respondent has a child with upper secondary education and as zero if a respondent has no child with upper secondary education. The latter can be due to two processes: (1) a respondent not having a child and (2) all children of the respondent having a lower level of education than upper secondary education. The models using this outcome variable estimate the effect of female education on child education in the prospective approach. Additionally, when analyzing this outcome, the sample is restricted to women with children born in 1998 and earlier to ensure that they have completed their education. As women and men who have no children are included in the analysis of this outcome, the only respondents who are dropped from the sample used to analyze this outcome are women and men who have at least one child, but no child born in 1998 or earlier.

Descriptive statistics on all variables used in the analysis are reported in Table 2. I report descriptive statistics on the pooled

Table 2. Descriptive statistics, only women.

	Sample Pooled Across Countries	Austria	Czech Republic	Denmark	France	Italy	Netherlands
Years of schooling (respondent)	11.089	9.378	11.716	13.777	12.113	8.621	11.945
	(3.638)	(3.819)	(2.631)	(2.849)	(3.238)	(3.833)	(2.796)
Highest educated child upper secondary education (conditional on having a child)	0.883	0.965	0.842	0.906	0.947	0.801	0.880
	(0.321)	(0.185)	(0.364)	(0.291)	(0.223)	(0.399)	(0.325)
Having a child	0.890	0.861	0.953	0.902	0.891	0.831	0.878
	(0.313)	(0.347)	(0.211)	(0.298)	(0.312)	(0.375)	(0.327)
Having a child with upper secondary education	0.786	0.830	0.803	0.817	0.844	0.665	0.773
	(0.410)	(0.376)	(0.398)	(0.387)	(0.363)	(0.472)	(0.419)
N	7,696	1,212	1,890	865	1,303	1,458	968

Notes: The table reports the means and in brackets the standard deviations.
Source: SHARE, Waves 1–7.

sample, which combines the information on respondents from all six countries and on each of the six countries included in the analysis separately.

As recommended in the literature on instrumental variables, I use the OLS regression estimator for both continuous and binary outcomes (therefore, I estimate Linear Probability Models [LPMs] in the latter case) (Angrist and Pischke 2009).

Results

The effects of the reforms in the length of compulsory schooling on female and male educational attainment

Table 3 presents models that estimate the effects of the reforms in the length of compulsory schooling on respondents' educational attainment, measured through years of schooling. Model 1 is estimated on a sample that combines male and female respondents. Model 2 reports the estimates for female respondents, while Model 3 is limited to men.

The major result of this analysis is that only women were affected by the reforms in the length of compulsory schooling. According to Model 2, each additional year of compulsory schooling increased education among women by, on average, 0.204 years of schooling. For men, the estimate in the model with control variables is positive but statistically insignificant (Model 3). While it cannot be ruled out that the reform had small effects on men as well, the reforms cannot be used to

Table 3. OLS estimates of regression models predicting respondent's years of schooling.

	(1) All	(2) Women	(3) Men
Years of compulsory schooling	0.160*	0.204*	0.082
	(0.052)	(0.046)	(0.073)
Respondent is male	0.477†		
	(0.214)		
N	13,431	7,696	5,735

Notes: Standard errors in parentheses.
Source: SHARE, Waves 1–7.
† $p < .10$; * $p < 0.05$ (two-tailed tests).

instrument education among men. The following analysis is therefore limited to women.[8]

The effects of female education on fertility and child education

In Table 4, I present both OLS and IV estimates of the effects of female education on fertility and on child education. Panel A in Table 4 reports LPMs predicting the associations between female education and a child having upper secondary education in the sample of women with at least one child (Model 1), between female education and the probability of having a child (Models 2 and 3), and between female education and the probability of having a child with upper secondary education (Model 4). These models are the benchmark to which the instrumental variable estimates, which are reported in Panel B in Table 4, are compared. The sample sizes differ across these models because the models are estimated on different samples. Model 1 includes only women with children who were born in 1998 and earlier. If the age restriction were not imposed, we would obtain biased estimates, as we would measure the educational attainment of children who are still in school (Breen and Ermisch 2017; Skopek and Leopold 2020). Model 2 includes all women, those who do have children (independent of their children's year of birth) and those who do not have any children. Finally, Models 3 and 4 include women who do not have any children and women who have children born in 1998 and earlier. Again, the conditioning on children's age is necessary to correctly measure the children's final educational attainment.

[8]I did run IV models instrumenting male education. As expected, these models led to both F-statistics below 2 and a weak instrument problem (Bound *et al.* 1995; Staiger and Stock 1997). Therefore, I do not report these results.

Table 4. Linear probability models estimating the effects of women's years of schooling on outcomes.

	(1) Child has Upper Secondary Education (Conditional on Having a Child)	(2) Having a Child	(3) Having a Child, excluding children born after 1999[a]	(4) Having a Child with Upper Secondary Education (Compared to Having No Child *or* Child having Less Education)[a]
Panel A: OLS estimates				
Years of schooling (women)	0.015*	−0.003*	−0.003*	0.011*
	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: IV estimates				
Years of schooling (women)	0.094	0.082*	0.082*	0.159*
	(0.077)	(0.020)	(0.019)	(0.078)
N	6,826	7,696	7,675	7,675
F-statistic of the first stage	21.760	19.933	19.958	19.958

Notes: Standard errors in parentheses. All models control for country and cohort fixed effects as well as country-specific time trends (controls not shown). Standard errors are clustered at the country level.
[a]The sample size of these models is smaller than of Model 2 because the outcome of Models 3 and 4 is only measured for children who have been before 1999 and had therefore the time to complete their education.
Source: SHARE, Waves 1–7.
† $p < .10$; * $p < 0.05$ (two-tailed tests).

The models in Panel A lead to three main findings. First, Model 1 shows that there is, as expected, a positive association between female education and child education conditional on the birth of a child. This finding reproduces results from the vast literature estimating associations between parental education and child education in samples limited to women with children, i.e. mothers. Second, Models 2 and 3 show that there is a negative association between female education and the probability of a woman having a child. This finding is in line with previous research (Breen and Ermisch 2017; Skirbekk 2008). The fact that there is no difference between Models 2 and 3 suggests that conditioning on children born in 1998 and earlier does not introduce sample selection bias. Finally, Model 4 reflects the combined effects of these processes. Because the positive association between respondent's education and child education conditional on the birth of a child and the negative association between respondent's education and the probability of having a child go into different directions, the association between female education and child education is reduced if we do not condition on the birth of a child. This finding supports the view that there can be differences between an analysis that does condition on the birth of a child (Model 1) and an analysis that estimates the total effect of female education on child education by not conditioning on the birth of a child (Model 4). The direction of this difference is also in line with previous studies estimating associations between female (and male) education, fertility, and child education (Breen and Ermisch 2017; Lawrence and Breen 2016).

The models reported in Panel B in Table 4 test whether this finding also holds if reforms in the length of compulsory schooling are used to instrument female education. There are three main findings. First, Model 1 shows that female education does lead to higher child education conditional on the birth of a child. In other words, the direct effect of female education, conditional on the birth of a child, is nine percentage points. The estimate is, however, statistically not significant. Nevertheless, the finding of a positive direct effect is in line with previous research using reforms in the length of compulsory schooling to estimate this direct effect (see the literature review).

Second, Models 2 and 3 show that higher female education increases the likelihood that a woman has a child. The effect of female education on fertility is statistically significant at the five percent level. Each additional year of schooling increases the probability that a woman has a child by 8.2 percentage points. These findings are at odds with the associations reported in Panel A. However, these estimates are in line with those reported by Fort *et al.* (2016), who found higher female education to reduce childlessness in a sample of women from countries similar to the ones analyzed in the present study (as discussed in the literature review above). The size of their estimate (nine percentage points) is also very close to that in my analysis. The estimates in Model 2 and Model 3 are similar, demonstrating that limiting the analysis to children born in 1998 and earlier does not introduce sample selection bias.

Finally, Model 4 estimates the total effect of female education on child education, i.e. the combination of the effect of female education on child education conditional on the birth of a child (Model 2) and the effect of female education on fertility (Models 2 and 3). According to Model 4, there is a positive effect of female education on child education. Each additional year of schooling of a woman results in a sixteen-percentage point increase in the probability of her having a child with upper secondary education.[9] This estimate is statistically significant. Because of the positive effect of female education on fertility, the total effect of female education on child education is nearly twice as large as the direct effect reported in Model 1. Therefore, I conclude that the total effect of

[9]Most reforms changed the length of compulsory schooling by two years, and the maximum change was three years (in Italy). Therefore, the IV models should not be interpreted as changing years of schooling by more than three years.

female education on child education is larger in an analysis that does not condition on the birth of a child (Model 4) than in an analysis that does condition on the birth of a child (Model 1).

The difference between the OLS and IV estimates could be due to two reasons. One possibility is that the OLS estimates are biased by unobserved variables, such as innate abilities and educational aspirations. The OLS estimates are downwardly biased because they are smaller than the corresponding IV estimates. For instance, the OLS estimate connecting female education to the probability of having a child is negative; the corresponding IV estimate is positive. A possible confounding unobserved variable could be educational aspirations. If women with high educational aspirations are more likely to select a high education and are less likely to have a child, this can induce a negative association between female education and the probability that a woman has a child. The IV corrects for this bias because it isolates the effect of female education from the influence of unobserved variables such as educational aspirations. The second possibility is that the estimates differ because the OLS estimates estimate the ATE, although possibly with bias, while the IV estimates estimate the ACR.

Table S1 in the *Online Supplemen*t reports the reduced form estimates regressing the outcome on the instrument. The findings of these models are fully in line with the 2SLS results reported in Panel B in Table 4.

Discussion and conclusion

The present study used reforms in the length of compulsory schooling to estimate the effects of female education on fertility and child education. Numerous studies in economics have used educational reforms to identify the causal effect of parental education on child education. However, these studies conditioned on the birth of children and therefore did not estimate the total effect of female (or male) education on child education. In contrast, my study started with a generation of women and examined their educational reproduction by analyzing the effects of female education on fertility and on child education.

The results of my study suggest that previous research has underestimated the effects of female education on child education through conditioning on childbirth. In fact, among the women included in my sample and with respect to the specific level of education affected by the reforms in the length of compulsory schooling, more female education increased the probability that a woman had a child (in line with

Fort *et al.* 2016). Considering this effect results in higher intergenerational effects of female education on child education.

It is important to interpret this finding within the specific context in which it is observed. The IV estimates are specific to the educational transition under investigation, i.e. the change in the length of compulsory schooling brought about by the policy change. The effect of female education is identified only for women who achieved a higher level of education due to the reforms in the length of compulsory schooling. This may be a selected group of women. In the causal analysis literature, this group is called the compliers, and the IV estimate of the causal effect is (in the case of a continuous outcome, such as the one used in the present analysis) an ACR. The estimated causal effect is specific (local) to the educational transition affected by the reform (Oreopoulos 2006). This means that at other levels of education, female education could have a different effect on child education.[10]

The second finding of my study refers to the difference between models that did and models that did not condition on childbirth, as increasing female education did increase the probability that a woman had a child, and therefore, taking this factor into account did change the effect of women's education on the education of their offspring. The total effect of female education on child education, without conditioning on the birth of a child, is larger than the direct effect conditional on childbirth. This large positive effect is a combination of a positive effect of female education on child education conditional on the birth of a child (the controlled direct effect) and a positive effect of female education on the probability that a woman becomes a mother.

This finding points in the opposite direction to the results of previous prospective studies on intergenerational mobility (Breen and Ermisch 2017; Lawrence and Breen 2016).[11] These differences can be due to several reasons. First, I used IV estimation, a method that controls for the impact on unobserved variables, such as parental characteristics, while previous studies employing a prospective approach relied on a

[10]Reforms in the length of compulsory schooling affect a rather low level of education, as they affect only women who leave education early. Estimates based on twins can refer to the whole schooling distribution. Twins, however, are a selected group and are not representative of the general population. Parents who adopt children are also likely to be a selective subpopulation. As a consequence, neither the sample of children with twin parents nor the sample of adopted children may be representative for the population, while estimates obtained using reforms in the length of compulsory schooling refer to a specific level of education. Differences in results across different types of methods could be due to these differences.

[11]Although the DAG in Figure 1 is nonparametric, the estimated models are linear. The direction of bias follows from the linear parametrization.

selection of observed variables. Second, previous studies provided estimates of the ATE, while my IV approach identifies the ACR. This ACR is identified at a low level of education, which was affected by the reforms. Previous prospective models estimated intergenerational mobility at the level of university education. Third, previous research mostly looked at the United States and the United Kingdom, while I focus on several European countries. This may matter, as Fort *et al.* (2016) found in their IV analysis of educational reforms that England was an outlier for which they found a negative effect of higher female education on fertility, which is in line with findings by Breen and Ermisch (2017). Future research can follow up on these findings by explicitly modelling and estimating cross-country variation in prospective models of intergenerational effects. Another possibility for future research would be to implement the research design employed in this study by using a reform that was implemented at different time points in different regions within the same country.

Another further important extension of the work conducted here would be to include the effects of female education on family size. The present analysis was, following Breen and Ermisch (2017) and Breen *et al.* (2019), limited to the integration of the effects of female education on childlessness in models of intergenerational effects. The reforms could affect not only the probability of a woman having a child but also the number of children she has. While the present analysis did focus on childlessness, further extensions, which need to develop a new methodological approach and use another data source (the SHARE does not include information on all children), should include the effects of female education on completed fertility. However, Lawrence and Breen (2016) found no variation across family size in their prospective models of intergenerational mobility in the United States.

Acknowledgments

I thank Xi Song and my colleagues at the LNU group at SOFI, Stockholm University as well as participants at presentations at Humboldt University, CREST Paris, the Swiss Sociology of Education, the Research Network 21 of the European Sociological Associations, and the Annual Meeting of the American Sociological Association in 2021 for comments on an earlier version of this manuscript. The SHARE data collection has been primarily funded by the European Commission through FP5 (QLK6-CT-2001-00360), FP6 (SHARE-I3: RII-CT-2006-062193, COMPARE: CIT5-CT-2005-028857, SHARELIFE: CIT4-CT-2006-028812) and FP7 (SHARE-PREP: N°211909, SHARE-LEAP: N°227822, SHARE M4: N°261982). Additional funding from the German Ministry of Education and Research, the Max Planck Society for the Advancement of

Science, the U.S. National Institute on Aging (U01_AG09740-13S2, P01_AG005842, P01_AG08291, P30_AG12815, R21_AG025169, Y1-AG-4553-01, IAG_BSR06-11, OGHA_04-064, HHSN271201300071C) and from various national funding sources is gratefully acknowledged (see www.share-project.org).

Disclosure statement

No potential conflict of interest was reported by the author.

Funding

This work was supported by the Schweizerischer Nationalfonds zur Förderung der Wissenschaftlichen Forschung (SNSF) under grant agreement PZ00P1_180128 and by the Forskningsrådet om Hälsa, Arbetsliv och Välfärd (Forte) [grant number 2016-07099].

ORCID

Michael Grätz http://orcid.org/0000-0001-7920-1021

References

Acharya, A., Blackwell, M. and Sen, M. (2016) 'Explaining causal findings without bias: detecting and assessing direct effects', *American Political Science Review* 110: 512–29.

Acker, J. (1973) 'Women and social stratification: a case of intellectual sexism', *American Journal of Sociology* 78: 936–45.

Amin, V., Lundborg, P. and Rooth, D.-O. (2015) 'The intergenerational transmission of schooling: are mothers really less important than fathers?', *Economics of Education Review* 47: 100–17.

Angrist, J. D., Imbens, G. W. and Rubin, D. B. (1996) 'Identification of causal effects using instrumental variables', *Journal of the American Statistical Association* 91: 444–55.

Angrist, J. and Pischke, J.-S. (2009) *Mostly Harmless Econometrics*, Princeton, NJ: Princeton University Press.

Behrman, J. R. and Rosenzweig, M. R. (2002) 'Does increasing women's schooling raise the schooling of the next generation?' *American Economic Review* 92: 323–34.

Beller, E. (2009) 'Bringing intergenerational social mobility research into the twenty-first century: why mothers matter', *American Sociological Review* 74: 507–28.

Björklund, A. and Jäntti, M. (2020) 'Intergenerational mobility, intergenerational effects, sibling correlations, and equality of opportunity: a comparison of four approaches', *Research in Social Stratification and Mobility* 70: 100455. doi:10.1016/j.rssm.2019.100455.

Björklund, A., Lindahl, M. and Plug, E. (2006) 'The origins of intergenerational associations: lessons from Swedish adoption data', *Quarterly Journal of Economics* 121: 999–1028.

Black, S. E., Devereux, P. J. and Salvanes, K. G. (2005) 'Why the apple doesn't fall far: understanding intergenerational transmission of human capital', *American Economic Review* 95: 437–99.

Bloome, D. and Western, B. (2011) 'Cohort change and racial differences in educational and income mobility', *Social Forces* 90: 375–95.

Blossfeld, H.-P. (2009) 'Educational assortative marriage in comparative perspective', *Annual Review of Sociology* 35: 513–30.

Börsch-Supan, A. (2019) *Survey of Health, Ageing and Retirement in Europe (SHARE) waves 1 to 7*. Release version: 7.0.0. SHARE-ERIC. Data set. doi:10.6103/SHARE.w1-7.700.

Börsch-Supan, A., Brandt, M., Hunkler, C., Kneip, T., Korbmacher, J., Malter, F., Schaan, B., Stuck, S. and Zuber, S. (2013) 'Data resource profile: the Survey of Health, Ageing and Retirement in Europe (SHARE)', *International Journal of Epidemiology* 42: 992–1001.

Bound, J., Jaeger, D. A. and Baker, R. (1995) 'Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak', *Journal of the American Statistical Association* 90: 443–50.

Bound, J. and Solon, G. (1999) 'Double trouble: on the value of twins-based estimation of the return to schooling', *Economics of Education Review* 18: 169–82.

Breen, R. and Ermisch, J. (2017) 'Educational reproduction in Great Britain: a prospective approach', *European Sociological Review* 33: 590–603.

Breen, R. and Ermisch, J. (2021) 'Instrumental variable estimation in demographic studies: the LATE interpretation of the IV estimator with heterogeneous effects'. *SocArXiv*. doi:10.31219/osf.io/vx9m7.

Breen, R., Ermisch, J. and Helske, S. (2019) 'Educational reproduction in Europe: a descriptive account', *Demographic Research* 41: 1373–400.

Brunello, G., Fort, M. and Weber, G. (2009) 'Changes in compulsory schooling, education and the distribution of wages in Europe', *The Economic Journal* 119(536): 516–39.

Buis, M. L. (2013) 'The composition of family background: the influence of the economic and cultural resources of both parents on the offspring's educational attainment in the Netherlands between 1939 and 1991', *European Sociological Review* 29: 593–602.

Cavaille, C. and Marshall, J. (2019) 'Education and anti-immigration attitudes: evidence from Compulsory schooling reforms across Western Europe', *American Political Science Review* 113(1): 254–63.

Chevalier, A. (2004) 'Parental education and child's education: a natural experiment'. *IZA Discussion Paper*, No.1153.

Cygan-Rehm, K. and Maeder, M. (2013). 'The effect of education on fertility: evidence from a compulsory schooling reform', *Labour Economics* 25: 35–48.

DeCicca, P. and Krashinsky, H. (2023) 'The effect of education on overall fertility', *Journal of Population Economics* 36: 471–503.

Duncan, O. D. (1966) 'Methodological issues in the analysis of social mobility', in N. J. Smelser and S. M. Lipset (eds.), *Social Structure and Mobility in Economic Development*, Chicago: Aldine, pp. 51–97.

Elwert, F. (2013) 'Graphical causal models', in S. L. Morgan (ed.), *Handbook of Causal Analysis for Social Research*, Dordrecht: Springer, pp. 245–73.

Elwert, F. and Winship, C. (2014) 'Endogenous selection bias: the problem of conditioning on a collider variable', *Annual Review of Sociology* 40: 31–53.
Ermisch, J. and Pronzato, C. (2011) 'Causal effects of parents' education on children's education', in T. Smeeding, R. Erikson and M. Jäntti (eds.), *Persistence, Privilege, and Parenting*, New York: Russel Sage Foundation, pp. 237–60.
Fort, M., Schneeweis, N. and Winter-Ebmer, R. (2016) 'Is education always reducing fertility? Evidence from compulsory schooling reforms', *The Economic Journal* 126: 1823–55.
Gathmann, C., Jürgens, H. and Reinhold, S. (2015) 'Compulsory schooling reforms, education and mortality in twentieth century Europe', *Social Science & Medicine* 127: 74–82.
Grätz, M. (2022) 'When less conditioning provides better estimates: overcontrol and endogenous selection biases in research on intergenerational mobility', *Quality & Quantity* 56: 3769–93. doi:10.1007/s11135-021-01310-8.
Grenet, J. (2013) 'Is extending compulsory schooling alone enough to raise earnings? Evidence from French and British compulsory schooling laws*', *The Scandinavian Journal of Economics* 115: 176–210.
Heckman, J. J., Urzua, S. and Vytlacil, E. (2006) 'Understanding instrumental variables in models with essential heterogeneity', *Review of Economics and Statistics* 88: 389–432.
Hægeland, T., Kirkebøen, L. J., Raaum, O. and Salvanes, K. G. (2010) 'Why children of college graduates outperform their schoolmates: a study of cousins and adoptees'. *IZA Discussion Paper*, No. 5369.
Hillmert, S. (2013) 'Analysing intergenerational transmissions: from social mobility to social reproduction', *Comparative Social Research* 30: 131–57.
Hillmert, S. (2015) 'Changing structures of opportunity: a life-course perspective on social mobility and reproduction', *European Sociological Review* 31: 184–96.
Holmlund, H., Lindahl, M. and Plug, E. (2011) 'The causal effect of parents' schooling on children's schooling: a comparison of estimation methods', *Journal of Economic Literature* 49: 615–51.
Hout, M. (2018) 'Americans' occupational status reflects the status of both of their parents', *Proceedings of the National Academy of Sciences* 115: 9527–32.
Jæger, M. M. (2007) 'Educational mobility across three generations: the changing impact of parental social class, economic, cultural and social capital', *European Societies* 9: 527–50.
Kalmijn, M. (1994) 'Mother's occupational status and children's schooling', *American Sociological Review* 59: 257–75.
Korupp, S. E., Ganzeboom, H. B. and van der Lippe, T. (2002) 'Do mothers matter? A comparison of models of the influence of mothers' and fathers' educational and occupational status on children's educational attainment', *Quality and Quantity* 36: 17–42.
Lawrence, M. and Breen, R. (2016) 'And their children after them? The effect of college on educational reproduction', *American Journal of Sociology* 122: 532–72.
Lundborg, P., Nilsson, A. and Rooth, D.-O. (2014) 'Parental education and offspring outcomes: evidence from the Swedish compulsory school reform', *American Economic Journal: Applied Economics* 6: 253–78.

Maralani, V. (2013) 'The demography of social mobility: Black-White differences in the process of educational reproduction', *American Journal of Sociology* 118: 1509–58.
Mare, R. D. (1981) 'Change and stability in educational stratification', *American Sociological Review* 46: 72–97.
Mare, R. D. (1997) 'Differential fertility, intergenerational educational mobility, and racial inequality', *Social Science Research* 26: 263–91.
Mare, R. D. and Maralani, V. (2006) 'The intergenerational effects of changes in women's educational attainments', *American Sociological Review* 71: 542–64.
Marks, G. N. (2008) 'Are father's or mother's socioeconomic characteristics more important influences on student performance? recent international evidence', *Social Indicators Research* 85: 293–309.
Mood, C. (2017) 'More than money: social class, income, and the intergenerational persistence of advantage', *Sociological Science* 4: 263–84.
Oreopoulos, P. (2006) 'Estimating average and local average treatment effects of education when compulsory schooling laws really matter', *American Economic Review* 96: 152–75.
Oreopoulos, P., Page, M. E. and Stevens, A. H. (2006) 'The intergenerational effects of compulsory schooling', *Journal of Labor Economics* 24: 729–60.
Piopiunik, M. (2014) 'Intergenerational transmission of education and mediating channels: evidence from a compulsory schooling reform in Germany', *The Scandinavian Journal of Economics* 116: 878–907.
Plug, E. (2004) 'Estimating the effect of mother's schooling on children's schooling using a sample of adoptees', *American Economic Review* 94: 358–68.
Pronzato, C. (2012) 'An examination of paternal and maternal intergenerational transmission of schooling', *Journal of Population Economics* 25: 591–608.
Richter, André. 2016. 'Essays on the intergenerational transmission of disadvantage: the role of prenatal health and fertility', Doctoral dissertation, Swedish Institute for Social Research (SOFI), Stockholm University.
Sacerdote, B. (2002) 'The role of nature versus nurture in determining economic outcomes', *American Economic Review* 92: 344–48.
Schneeweis, N., Skirbekk, V. and Winter-Ebmer, R. (2014) 'Does education improve cognitive performance four decades after school completion?', *Demography* 51: 619–43.
Schwartz, C. R. (2013) 'Trends and variation in assortative mating: causes and consequences', *Annual Review of Sociology* 39: 451–70.
Skirbekk, V. (2008) 'Fertility trends by social status', *Demographic Research* 18: 145–80.
Skopek, J. and Leopold, T. (2020) 'Educational reproduction in Germany: a prospective study based on retrospective data', *Demography* 57: 1241–70.
Song, X. and Mare, R. D. (2015) 'Prospective versus retrospective approaches to the study of intergenerational social mobility', *Sociological Methods & Research* 44: 555–84.
Song, X. and Mare, R. D. (2017) 'Short-term and long-term educational mobility of families: a two-sex approach', *Demography* 54: 145–73.
Staiger, D. and Stock, J. H. (1997) 'Instrumental variables regression with weak instruments', *Econometrica* 65: 557–86.