
Intergenerational Transmission of Education and Mediating Channels: Evidence from a Compulsory Schooling Reform in Germany

Author(s): Marc Piopiunik

Source: *The Scandinavian Journal of Economics*, July 2014, Vol. 116, No. 3 (July 2014), pp. 878-907

Published by: Wiley on behalf of The Scandinavian Journal of Economics

Stable URL: <https://www.jstor.org/stable/43673665>

REFERENCES

Linked references are available on JSTOR for this article:

https://www.jstor.org/stable/43673665?seq=1&cid=pdf-reference#references_tab_contents

You may need to log in to JSTOR to access the linked references.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



JSTOR

Wiley and *The Scandinavian Journal of Economics* are collaborating with JSTOR to digitize, preserve and extend access to *The Scandinavian Journal of Economics*

Intergenerational Transmission of Education and Mediating Channels: Evidence from a Compulsory Schooling Reform in Germany

Marc Piopiunik*

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich,
DE-81679 Munich, Germany
piopiunik@ifo.de

Abstract

In this paper, I estimate the causal effect that an additional year of schooling for parents has on their children's education, by exploiting a compulsory schooling reform that was implemented in all West German states between 1946 and 1969. Although previous research indicates that the reform had no effect on earnings, I find that an additional year of schooling for women strongly affects the education of their sons. There is no effect for the other parent-child gender pairs. I investigate numerous channels that might mediate the positive effect of the education of mothers. Most importantly, I find that individuals with more schooling value their children's educational success as more important.

Keywords: Human capital; instrumental variables estimation; intergenerational mobility

JEL classification: I21; J13; J62

I. Introduction

Children of lower-educated parents tend to end up being lower educated themselves.¹ This is an important observation because educational attainment is a strong predictor of labor-market success and other outcomes in life.² While the education of parents has been shown to be the most

*I thank Francesco Cinnirella, Oliver Falck, Elke Lüdemann, Martin Schlotter, Guido Schwerdt, Till von Wachter, Joachim Winter, Ludger Woessmann, and two anonymous referees, as well as participants at the ESPE 2010 in Essen and at the EEA 2010 in Glasgow, for their valuable comments and suggestions.

¹ Hertz *et al.* (2007) report strong correlations between the schooling of parents and that of their children for many countries. For Germany, there is a significant and non-declining association between the education of parents and their children over the past decades (Heineck and Riphahn, 2009).

² See Card (1999) for a review of the literature on the effects of education on earnings and see Oreopoulos and Salvanes (2011) for an overview of non-pecuniary returns to schooling. In Germany, different secondary-school degrees translate into substantial wage differentials later in life (Dustmann, 2004).

important factor in explaining the education of their children (see the review by Haveman and Wolfe, 1995), correlation does not necessarily mean causation. Several of the studies that have investigated the causal effect of the education of parents have exploited reforms that have increased the number of compulsory school years, in order to obtain exogenous variation in the schooling received by parents, which is likely correlated with innate ability and other unobservable characteristics that affect the education of their children; see the literature reviews by Black and Devereux (2011), Björklund and Salvanes (2011), and Holmlund *et al.* (2011). These compulsory schooling reforms are typically estimated to have strong positive earnings effects of the order of 10–15 percent for those individuals forced to remain in school longer (e.g., Harmon *et al.*, 2003; Oreopoulos, 2006). In contrast, Pischke and von Wachter (2008) have provided strong evidence of zero earnings returns to a similar reform in Germany, which extended compulsory schooling by one year. They have argued that German students have already learned the basic academic skills that are ultimately rewarded in the labor market before the end of compulsory schooling. Although the additional year of compulsory schooling did not increase earnings in Germany, it still might have had other beneficial effects, such as improving the child-rearing abilities of individuals and/or increasing individuals' valuation of education, which in turn improved their children's education.

In this paper, I exploit changes in compulsory schooling laws in West Germany to estimate the causal effect of the education of parents on their children's education. I also investigate numerous transmission channels through which the intergenerational effect might work. After World War II, compulsory schooling was eight years; an additional compulsory ninth grade was introduced in basic schools (*Hauptschule*), the least academic secondary school type, in all West German states between 1946 and 1969. This reform provides both regional and time-series variation in the amount of compulsory schooling, which allows us to identify the causal effect that an additional year of schooling for parents has on their children's education in a difference-in-differences framework.

The empirical analysis is based on four datasets. The intergenerational transmission of education is investigated with the German Socio-Economic Panel Study (SOEP), a large annual household survey that is representative of the resident population in Germany, which allows us to link information on parents and their children. The reform effects on the labor-market outcomes for parents are estimated with the large Micro Census, which has the advantage that outcomes were reported while most parents were at their prime working age. The effect of more schooling on parents' valuation of education is studied using the German General Social Survey (ALLBUS), which contains information on the attitudes and behaviors of individuals. All results from these datasets come from reduced-form

regressions because the actual number of years of schooling is typically not observed in German datasets. As an exception, the Qualification and Career Survey (QaC) does contain information on the number of years of schooling. Therefore, two-sample instrumental variables (TSIV) estimates are computed for the main specifications of the intergenerational analysis by combining the reduced-form results of the SOEP with the first-stage results of the QaC.

The results suggest that the education of mothers has a strong positive effect on the education of their sons, but that it has no effect on the education of their daughters. The education of fathers has no effect on their children's education. The empirical result that the schooling of a mother is one factor that helps to improve her son's educational attainment is confirmed by several robustness checks. Concerning transmission channels, the results suggest that individuals with more schooling value their children's education more highly. Although this has previously been posited as one potential channel (Björklund and Salvanes, 2011), to the best of my knowledge, this is the first study that provides evidence that additional schooling raises parents' valuation of their children's education. In line with the findings of Pischke and von Wachter (2008), there is no evidence that labor-market channels mediate the positive effect of maternal education: that is, for women, the additional year of schooling has no effect on the probability of working, on the number of hours worked, or on earnings. Furthermore, there is no evidence for assortative mating effects and only weak evidence for a negative effect on fertility. Finally, descriptive evidence suggests that the effect of a mother's education might differ for sons and daughters, because lower-educated mothers have higher educational aspirations for their sons than for their daughters.

Previous research shows that the compulsory schooling reform in Germany had no effects on earnings or political behavior, but did have some positive effects on health outcomes. Pischke and von Wachter (2005) have presented precisely estimated effects of the additional ninth grade on wages, with coefficients being close to zero. They have found no evidence that this is a result of labor-market institutions or the apprenticeship training system in Germany. Instead, they have argued that the absence of any returns to the compulsory schooling reform in Germany is likely to be a result of the institutional features of the German school system; German students had already learned labor-market-relevant skills before the end of compulsory schooling, whereas in other countries, marginal students were still learning these skills at age 14 or 15. Similarly, Siedler (2010) has found no effects of the additional school year on political behavior (e.g., the political interest, participation in voting, and democratic values of individuals). This is again in contrast to studies from other countries, which estimate positive political returns to schooling (e.g., Dee, 2004; Milligan

et al., 2004). Similar to Pischke and von Wachter (2008), Siedler has concluded that the fundamentals of democracy had already been learned at an earlier stage of schooling. In contrast, Kemptner *et al.* (2011) have found strong negative effects of the additional school year on long-term illness for men (but no effects for women) and weak negative effects on weight problems for both sexes. They have provided evidence that the effect on long-term illness and work disability for men is partly mediated through the effect of schooling on the probability of working in white-collar occupations with better health conditions.

It is interesting to investigate the intergenerational transmission of education in Germany for three distinct reasons. First, as pointed out above, if we find intergenerational schooling effects in Germany, then earnings (i.e., income) can be ruled out as a transmission channel – in contrast to other countries with similar compulsory schooling reforms with positive intergenerational education effects and positive earnings effects. This implies that in Germany there must be other channels that mediate the effect of parents' education on that of their children. Second, the German school system differs from most school systems in other countries with similar compulsory schooling reforms where intergenerational schooling effects have been studied. Whereas the German school system is highly selective – that is, students are allocated to different secondary school types very early (at about age 10) – the school systems of other countries are much more comprehensive, with students often attending the same school until age 16 (OECD, 2004). Because the cognitive capabilities of children aged 10–15 are still developing, different tracking ages might lead to different intergenerational schooling effects. Third, an advantage of studying the intergenerational schooling effects in Germany is that the structure of the school system was exactly the same before and after the compulsory schooling reform. In contrast, this is not the case for the Scandinavian countries, for example, where school reforms not only extended the number of compulsory years of schooling, but also replaced the old school system with early tracking by a comprehensive and non-selective school system up to age 16. Therefore, the estimated intergenerational effects are rather difficult to interpret for the Scandinavian countries because they reflect the effect of extending compulsory schooling and of delaying tracking.

The remainder of the paper is structured as follows. In Section II, I provide an overview of the literature on the intergenerational transmission of education. In Section III, I briefly describe the school system and the compulsory schooling reform in West Germany, and in Section IV, I describe the datasets. In Section V, I present the results of the reform effects on the length of schooling for parents and the intergenerational transmission of education. In Section IV, I report the results on the transmission channels and I conclude in Section VII.

II. Existing Literature on the Intergenerational Transmission of Education

Studies that aim to identify the causal effects of the education of parents on that of their children use one of the following three approaches: natural experiments, twins as parents, or adoptees.³ In this section, I summarize the main findings of studies that exploit compulsory schooling reforms, I very briefly address the main differences to twin and adoption studies, and I conclude with an overview of the existing evidence on transmission channels.

Most studies on intergenerational schooling effects that exploit natural experiments use changes in compulsory schooling laws as a source of exogenous variation in parents' education. In Norway, for example, compulsory schooling was increased from seven to nine years in different municipalities in different years between 1959 and 1973. Black *et al.* (2005b) have used this reform to instrument for parents' education and have found a small, statistically significant causal relationship between a mother's and her son's education, but no causal relationship between the education of mothers and daughters and no effect of the education of fathers. Similar intergenerational schooling effects are found for a very similar reform in Sweden, where compulsory schooling was also extended from seven to nine years between 1949 and the early 1960s (Holmlund *et al.*, 2011). Based on an increase in the minimum school-leaving age from 15 to 16 years in Britain in the early 1970s, Chevalier (2004) has estimated insignificant effects of fathers' education, but large positive effects of the education of mothers on their children's education. Exploiting the same reform, the education of parents appears to have no effect on the probability that their children leave school early (Chevalier *et al.*, 2010). Galindo-Rueda (2003) has used the earlier minimum school-leaving age reform of 1947 in the UK and has found only some evidence that an additional year of schooling for a father has a positive effect on his son's education, whereas a mother's schooling seems to have no effect on her children's education.⁴ Based on compulsory schooling reforms in the US, an increase in either

³ Several recent surveys have summarized and contrasted the findings of this literature. Black and Devereux (2011) have reviewed studies of the intergenerational effects of both the education and earnings of parents, while Björklund and Salvanes (2011) and Holmlund *et al.* (2011) have focused on the intergenerational effects of parents' education only.

⁴ A drawback of the UK studies is that the authors are unable to disentangle the effects resulting from the schooling reforms from cohort effects because identification is based on a nationwide one-time change in the minimum schooling law, such that there is no cross-sectional variation in the minimum schooling requirements.

parent's schooling by one year is estimated to reduce the child's probability of repeating a grade by 2–4 percentage points (Oreopoulos *et al.*, 2006).⁵

While IV studies based on compulsory schooling reforms generally find that parents' education has a small effect on their children's education and that mothers' education is more important than fathers' education, studies using identical twins or adoptees tend to find that fathers' education is more important (see surveys cited above). Holmlund *et al.* (2011) have applied all three identification strategies to the same Swedish dataset and have found that the estimated effect of parents' education on that of their children varies with the strategy, even when the strategies are applied to the same country, the same cohorts, and the same institutional context. They have argued that one reason for these differences might be because each identification strategy uses a different sample of parents coming from a different part of the educational distribution, with different samples leading to different results if the effect varies across educational groups. Another possible explanation is that different methods control differently for inherited abilities. Because the findings of Holmlund *et al.* (2011) imply that different identification strategies identify different effects, the results of this paper will be compared only to the findings of studies that exploit similar compulsory schooling reforms.

Although transmission channels mediating the effect of parents' education on that of their children are considered an important issue, only very few intergenerational education studies investigate potential channels. Obvious candidates, apart from labor-market effects, are assortative mating effects (i.e., individuals with more schooling having better-educated or wealthier partners), as well as the quantity–quality trade-off (i.e., individuals with more schooling preferring fewer, but better-educated children). There is no evidence of assortative mating effects based on the compulsory schooling reform in Norway (Black *et al.*, 2003). However, in the US, mothers with more schooling seem to have better-educated partners and delay childbearing (Carneiro *et al.*, 2013). Neither study finds evidence that schooling affects the number of children. The study by Carneiro *et al.*

⁵ Other IV studies use alternative identification strategies to obtain exogenous variation in parents' education. For example, Page (2006) has exploited the World War II G.I. Bill and has found that a one-year increase in a father's education reduces the probability that his child repeats a grade in school by about 2–3 percentage points. Carneiro *et al.* (2013) have used several instruments that affect the direct and opportunity costs of education in the US and have found positive effects of mothers' education on the cognitive abilities of children aged 7–8 and considerable positive effects on behavioral problems and grade repetition for children aged 12–14. Maurin and McNally (2008) have exploited a 1968 change in the qualification level required for university admission in France and have found that even children of higher-educated parents benefit from their parents having additional years of higher education, when it comes to the risk of having to repeat a grade.

is an exception in the intergenerational literature in that they study a variety of potential channels. They find that family income is higher and that mothers work more hours per year when mothers have more education, but, at the same time, do not appear to spend less time with their children. Overall, they find no effect on the probability that the child has low birth weight and, concerning early childhood channels for children up to one year old, find that mothers with more schooling are more likely not to smoke during pregnancy, to breastfeed for a longer period, and to work substantially more hours per year.⁶ Interestingly, Carneiro *et al.* also provide evidence that more schooling has an effect on home environment in terms of a higher probability that a musical instrument or computer is in the home and a higher probability that the child takes special lessons. More schooling also seems to increase the likelihood that a mother reads to the child at least three times a week (all effects refer to white children).

In summary, the existing intergenerational studies provide some evidence on potential transmission channels, such as labor-market effects and better home environments, but none of the work investigates the effects of more schooling on parents' attitudes toward education. This paper, to the best of my knowledge, is the first study to provide evidence that more schooling can increase parents' valuation of children's education (see Section VI).

III. School System and Compulsory Schooling Reform in West Germany

In Germany, children start school in the year after they turn 6 and first attend four grades in primary school (*Grundschule*). At about age 10, students are allocated to one of three different secondary school types, which differ by both duration and curriculum.⁷ Basic schools (*Hauptschule*) provide basic general education and lead to a certificate after grade 8 or 9. Middle schools (*Realschule*) provide more extensive general education and cover grades 5–10. High schools (*Gymnasium*) offer the most academic curriculum and cover grades 5–13.⁸ Students who complete basic school typically enter an apprenticeship, which combines part-time vocational school

⁶ See Almond and Currie (2011) for a review of the literature on the effects of early childhood influences and interventions on outcomes in later life, such as educational attainment.

⁷ In some states, an additional fourth school type – comprehensive schools (*Gesamtschule*) – offers all lower and upper secondary education levels. Where comprehensive schools exist, only a minor fraction of students attend this school type. See Lohmar and Eckhardt (2010) for a more detailed description of the German school system.

⁸ There is no perfect translation for the school type *Gymnasium*. Sometimes the British term “grammar school” is used. Note that a German high school (*Gymnasium*) differs from a US high school, which is a comprehensive school that is attended by all students in upper secondary education, usually covering grades 9–12.

and firm-based training. Following middle school, students enter an apprenticeship or school-based training, or attend a vocational school that provides a higher education entrance qualification, in particular the technical school degree (*Fachhochschulreife*), which qualifies students to attend a polytechnic (*Fachhochschule*). The high-school leaving certificate (*Abitur*) is a prerequisite for attending a university or other institutions of higher education.

Allocation to the secondary school track depends to a large extent on the child's academic performance in primary school, which thus depends on the child's ability. Primary school teachers typically give a school track recommendation, which is largely based on the child's grades in the major subjects, German and mathematics. Parents might also have considerable influence on the school choice of their child, depending on whether the recommendation is binding or not, which differs from state (*Bundesland*) to state. The allocation procedure implies that basic school students tend to be the students with the lowest ability. Importantly, the school track allocation after primary school tends to be rather permanent: although students can theoretically switch between school types, mobility is rather low in practice, with downward mobility being more common (Jürges and Schneider, 2007). This implies that the secondary school track decision at the end of primary school is a good predictor of a child's final educational attainment.

After World War II, education levels increased over time: only 8 percent of the 1930 birth cohort graduated from high school, but 20 percent of the students born in 1960 (own calculations based on QaC data). Over the same period, the share of students attending basic schools fell sharply: about 75 percent of the 1930 birth cohort graduated from basic schools, while the respective share was below 50 percent for students born in 1960. The trend toward higher education is taken into account in the regressions by including year of birth fixed effects.

Until the end of World War II, basic schools ended after grade 8. Hamburg (in 1946) and Schleswig-Holstein (in 1947) were the first states to introduce a compulsory ninth grade.⁹ Bavaria was the last state to implement this reform (in 1969), several years after all states agreed on a ninth

⁹ Table 1 in the working paper version of this paper (Piopiunik, 2011) reports the year when each state introduced the compulsory ninth grade and the first birth cohort affected. Note that the introduction dates used in this study differ slightly from those used by Pischke and von Wachter (2008), Siedler (2010), and Kemptner *et al.* (2011). These differences lead to a larger first-stage coefficient in this paper. Introduction dates differ for the following states: Hamburg, 1946 (1949 in the other studies); Schleswig-Holstein, 1947 (1956); Saarland, 1958 (1964); Bremen, 1959 (1958). West Berlin introduced a compulsory ninth grade in 1948. However, individuals living in Berlin are excluded from the analysis because it is hard to identify whether an individual attended school in West Berlin or East Berlin.

compulsory school year in the Hamburg Accord in 1964.¹⁰ The reasons for extending compulsory schooling changed over time (Petzold, 1981). After World War II, when youth unemployment was high, political parties, unions, and employer organizations emphasized labor-market rationales. Toward the end of the 1950s, pedagogical and working-life arguments became more prevalent: children are not mature enough to enter the workforce, they should be protected physically and psychologically from working life, and they should be provided with more guidance when choosing an occupation. Working-life arguments emphasized the change from rather manual to more intellectual occupations, which required a more academic education.¹¹

IV. Data

The results are based on four datasets. The SOEP is used to estimate (reduced-form) effects of parents' education on that of their children. The SOEP is a large annual household survey, conducted in West Germany since 1984 and representative of the resident population. Participants answer detailed questionnaires including demographic characteristics, retrospective biographical information, and educational outcomes.¹² The Micro Census is employed to estimate the reform effects on parents' labor-market outcomes, which are observed during their prime working age. This household survey, which samples 1 percent of the resident population, is conducted on behalf of the Federal Statistical Office to provide the official statistics of the population and the labor market in Germany. I use the waves from 1976, 1978, 1980, 1982, 1985, 1987, and 1989. Each wave contains about 300,000 individuals. The ALLBUS is a repeated biennial cross-section of the resident population with information on attitudes and behaviors on diverse topics, including questions on the valuation of children's education. The ALLBUS is representative of the adult population with German nationality and currently covers the period from 1980 to 2008.¹³ Finally, the QaC is used to estimate first-stage coefficients because it contains, in contrast to the other three datasets, information on parents' years of schooling. The QaC is a repeated cross-section of employed German workers aged 15–65, collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) and

¹⁰ The Hamburg Accord also allowed a voluntary tenth grade in basic schools, which enables students to obtain the school-leaving degree typically obtained at middle schools. However, the tenth grade in basic schools remained quantitatively negligible until the 1970s.

¹¹ See Pischke and von Wachter (2005) and Piopiunik (2011) for more details on the reform.

¹² All questionnaires, in German and English, are available at <http://panel.gsoep.de/soepinfo2009/>.

¹³ Terwey and Baltzer (2009) have provided detailed information on the ALLBUS surveys and have documented the variables available for the period 1980–2008. The ALLBUS is the equivalent of the US General Social Survey (GSS).

the Bundesinstitut für Berufsbildung (BIBB). All five waves from 1979, 1985/1986, 1991/1992, 1998/1999, and 2005/2006 are used, each containing about 25,000 workers.

The SOEP is a longitudinal household survey that allows us to link information on parents and their children in two ways. First, when both parents and children are SOEP participants, then information on parents is always self-reported, while information on children below age 16 is reported by the parents and is self-reported by children aged 16 and older. Adult children can be linked with their parents even if they live in different households. Second, when only the child participates in the SOEP, then information on both child and parents is, of course, reported by the child. In the estimation sample, about half of the parents are SOEP participants.

Whether a parent was legally required to attend basic school until grade 8 or 9 depends on the state in which he or she went to school and when he or she was born. Because the SOEP survey started collecting information on the state of school attendance only in 2001, this information is known for only about 15 percent of the parents in the sample. For all other parents, current state of residence is used as a (good) proxy for state of school attendance.¹⁴ The binary reform indicator of whether an individual was subject to eight or nine years of compulsory schooling is constructed based on the individual's year of birth, state of last school attendance/state of current residence, and the year when the compulsory ninth grade was introduced in that state.¹⁵

German datasets generally do not contain information on the number of years of schooling or the highest grade attended. Because this is also true for the SOEP, mainly reduced-form results are presented for the effect of parents' education on that of their children. Because first-stage coefficients can be estimated with the QaC dataset, TSIV results based on the reduced-form results of the SOEP and the first-stage results of the QaC will be reported for the main specifications.

¹⁴ Measurement error in the state of school attendance variable should not be a major concern, because cross-state mobility of individuals with a basic school degree is low in Germany. In the estimation sample, only 8 percent of the parents with a basic school degree who reported their state of last school attendance currently live in a different state than that in which they obtained their school degree.

¹⁵ Ideally, one would like to construct the reform indicator using parents' state of residence before the reform was made public because grandparents might have reacted to the reform by moving to another state. However, this information is not available in the dataset. However, because state mobility of lower-educated individuals is low in Germany, the reform most likely was not a motivating factor behind cross-state moves and therefore any measurement error is likely to be small and unlikely to be correlated with the reform. For the Swedish reform, for example, Meghir and Palme (2005) have shown that selective mobility seems to be a minor problem.

The child's final school degree is a natural outcome of interest in an intergenerational education study. However, because this study exploits a reform that affected parents who attended the lowest secondary school track (basic schools), a more interesting outcome is whether the child achieves a higher school degree than his or her parents. Thus, the dependent variable equals 1 if the child obtains either a middle school degree, a technical school degree, or a high school degree, and it equals 0 if the child obtains only a basic school degree (or leaves school without a degree). In the estimation sample, 15.3 percent of the children have not yet completed schooling. For those children, the currently attended secondary school type is used as the outcome, which is a good proxy for the final school degree because mobility between school tracks is limited (Jürges and Schneider, 2007). A robustness check (see Table A4 in the Online Appendix) shows that results are very similar when children who have not yet completed schooling are excluded from the sample.¹⁶

The SOEP estimation sample includes all individuals (children) with at least one parent born between 1930 and 1960 and living in West Germany.¹⁷ Parents who migrated to Germany after World War II are excluded from the sample. Table 1 provides summary statistics by gender and reform status of the parent. Note that children can show up in both the mothers–children sample and the fathers–children sample. In some cases, one parent is in the sample while the partner is not because only one of them was born during the period 1930–1960 and received a school degree in West Germany.

The Micro Census contains information on parents' education and labor-market outcomes. The advantage of the Micro Census over the SOEP, apart from huge sample sizes, is that parents self-reported their labor-market outcomes during their prime working age, while in the SOEP, parents' labor-market outcomes are either self-reported retrospectively or are not available at all if parents are not SOEP participants. Thus, the measurement error in the labor-market variables is likely to be much smaller in the Micro Census. The sample contains individuals of German nationality, living in the 10 West German states, who were born between 1930 and 1960. Geographic information is limited to the current state of residence. Because there is no information about years of schooling – as in the SOEP – only reduced-form results are presented.¹⁸

¹⁶ A further robustness check (Table A5 in the Online Appendix) shows that results are also robust when parental cohorts who obtained less education because of World War II are excluded from the sample.

¹⁷ These are the same cohorts as in Pischke and von Wachter (2008), Siedler (2010), and Kemptner *et al.* (2011).

¹⁸ It is difficult to link information on parents and their children using the Micro Census. Most importantly, children cannot be linked with their parents if they live in different households and Micro Census participants do not provide information on their parents'.

Table 1. *Descriptive statistics of SOEP samples*

Variable	Mothers–children		Fathers–children	
	Non-reform (1)	Reform (2)	Non-reform (3)	Reform (4)
Parents				
School degree				
Basic school	0.76	0.47	0.70	0.47
Middle school	0.18	0.33	0.15	0.24
Technical school	0.00	0.03	0.02	0.07
High school	0.06	0.17	0.14	0.22
Year of birth	1939.5 (6.2)	1953.0 (7.0)	1939.1 (6.2)	1952.6 (7.1)
Number of parents	5,379	1,882	5,128	1,683
Children				
School degree				
Basic school or none	0.31	0.21	0.30	0.22
Middle school	0.32	0.32	0.32	0.34
Technical school	0.08	0.06	0.08	0.04
High school	0.29	0.41	0.30	0.39
Year of birth	1966.5 (7.6)	1981.2 (8.5)	1969.0 (8.5)	1983.4 (8.5)
Age ^a	37.6 (10.0)	26.2 (8.0)	35.6 (9.9)	25.0 (7.5)
Female	0.50	0.49	0.51	0.48
Number of children	6,492	2,832	6,398	2,599

Source: SOEP.

Notes: means reported; standard deviations of continuous variables in parentheses. The mothers–children samples contain mothers living in the 10 West German states who were born between 1930 and 1960, whereas their spouses can have been born in any year (the same is true for the fathers–children samples). Each parent has at least one child born in 1999 or earlier. Reform (non-reform) samples contain parents who went to school when schooling laws required nine (eight) years of compulsory schooling.

^aChild's age when he or she reported the school degree or type of secondary school attended.

Although sample sizes are limited, the ALLBUS is an interesting dataset because it contains information on attitudes toward raising children and on individuals' valuation of education. Furthermore, it is the only dataset with reliable information on the number of children a woman has borne. Similar

educational attainment. Thus, the educational attainment of parents and their children can be observed together for only a very short period between the child's school graduation and the time he or she moves out of the parental household. Unbiased estimates of the effect of parents' education on that of their children can therefore be obtained only if the probability that the child lives in his or her parents' household is uncorrelated with the child's school degree, which implies that the probability of the child moving out of the parents' household must be uncorrelated with the child's school degree. This is unlikely to be the case. Children who obtain a basic or middle school degree are about 16 years old when they finish school. Hence, they tend to leave their parents' home earlier than children who obtain a high school degree at about age 19. For these reasons, intergenerational education effects cannot be estimated (without bias) with the Micro Census.

to the other datasets, the sample contains only individuals of German nationality, living in the 10 West German states, who were born between 1930 and 1960. Again, geographic information is limited to the current state of residence and there is no information on years of schooling.¹⁹

In contrast to the other datasets, the QaC provides the year in which an individual graduated from secondary school, which allows us to compute the number of years of schooling. The dependent variable of the first stage, *years of schooling*, is constructed as follows. Because children enter primary school in the year after they turn 6, I compute the length of primary plus secondary schooling as secondary school graduation year minus year of birth minus 6.5 (approximate average age at school start). The reform indicator is constructed as in the SOEP dataset and the sample is also restricted to Germans born between 1930 and 1960 who live in West Germany.

V. Intergenerational Transmission of Education

In this section, first I show that the introduction of the compulsory ninth grade actually increased parents' years of schooling. Then, I present the effect of parents' education on their children's education. In addition to reduced-form estimates, TSIV results are computed for the main specifications.

Effect of the Reform on Parents' Length of Schooling

The effect of the introduction of the compulsory ninth grade on parents' years of schooling (first stage) is obtained with the QaC data by regressing the constructed length of schooling variable on the reform dummy indicating whether an individual was affected by the compulsory schooling reform. In the difference-in-differences estimations, all specifications include the maximal sets of year of birth and state of residence dummies, the two variables that jointly determine the reform indicator. Column 1 of Table 2 contains all individuals with degrees from any type of secondary school. The coefficient on the compulsory ninth grade is only 0.28, although 60 percent of the sample acquired a basic school degree. When the sample is restricted to those individuals who should have been affected

¹⁹ Descriptive statistics for the Micro Census and ALLBUS samples are not presented because results are based on several different samples. Descriptive statistics are available upon request from the author.

Table 2. *Effect of ninth grade introduction on length of schooling*

	Full sample (1)	Basic school track (2)	Middle and high school track (3)
Dummy for grade 9 in basic school	0.275*** (0.039)	0.408*** (0.036)	−0.070 (0.055)
Female	Yes	Yes	Yes
Quartic in age	Yes	Yes	Yes
State dummies	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
State-specific linear cohort trends	Yes	Yes	Yes
Individuals	66,942	39,912	27,030

Source: QaC.

Notes: Dependent variable: length of primary plus secondary schooling; imputed as graduation year minus year of birth minus 6.5. Ordinary least-squares regressions. Robust standard errors clustered at the state \times year of birth level in parentheses. Samples contain individuals of German nationality born between 1930 and 1960 living in the 10 West German states. ***, **, and * denote significance levels $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

by the reform (i.e., individuals with a basic school degree), the coefficient increases to 0.41, which is still far below 1 (Column 2).^{20,21}

There are at least three reasons why the first-stage coefficient is smaller than 1 for the subsample of individuals with a basic school degree. First, administrative data suggest that the ninth grade was not implemented immediately in all basic schools, but rather required some time until each basic school offered the new compulsory ninth grade (see figure 2 of Piopiunik, 2011). Second, some basic school students attended a voluntary ninth grade before it was made compulsory. Third, two sources of measurement error in the ninth grade introduction variable attenuate the coefficient: (i) because the state of school attendance is not observed, the current state of residence is used to construct the reform indicator; (ii) year of birth does not perfectly determine whether an individual was affected by the reform (i.e., some children might start primary school one year earlier or later than officially scheduled and some students might repeat a grade). Except for the measurement error in the state variable, all explanations suggest that the first stage can be “smoothed out” around the policy change

²⁰ Pischke and von Wachter (2008) run the same first-stage regressions using the QaC data. For the subsample of individuals with a basic school degree, their coefficient on the ninth grade introduction is only 0.28 and thus substantially smaller than my coefficient. This difference is mainly because Pischke and von Wachter (2008) have used different reform introduction dates for some states. Furthermore, their sample differs somewhat from my sample; for example, they drop all individuals with missing wages.

²¹ See, for example, Pischke and von Wachter (2005) and the working paper version of this paper (Piopiunik, 2011) for graphical illustrations of the first stage.

because these explanations become increasingly irrelevant for cohorts further away from the reform year. Additional regressions that omit more and more cohorts on each side of the reform year from the sample (not shown) do lead to increasingly larger first-stage coefficients, indicating that the arguments above are important explanations for the rather low first-stage coefficient.²² However, even though the first-stage coefficient is attenuated, the *F*-statistic for the excluded instrument in Column 2 is higher than 100, indicating a strong instrument.

Because the introduction of the compulsory ninth grade was targeted only at basic school students, it should have had no effect on individuals who obtained a degree from a middle school or a high school. Column 3 shows that this is indeed the case: the introduction of the ninth grade had no effect on the length of schooling of those individuals who should not have been affected by the reform. This finding corroborates the validity of the instrument.

Effect of Parents' Schooling on Their Children's Education

To estimate the causal effects of parents' education on their children's education, I exploit the compulsory school reform, implemented in all West German states between 1946 and 1969, which induced variation in parents' length of schooling that is uncorrelated with ability and other unobservable characteristics that might affect children's education.²³

The effect of parents' education on that of their children might differ both by the gender of the parent and by the gender of the child. *A priori*, one might suspect that maternal education is more important for the child's education because the mother is typically the main provider of child care. Moreover, recent studies suggest that the intergenerational transmission of education might also differ by the child's gender. Black *et al.* (2005b), for example, have found positive causal effects of mothers' education on sons, but not on daughters. Similarly, Carneiro *et al.* (2013) have found that the influence of mothers' schooling on children's abilities in mathematics and reading differs between boys and girls. Therefore, the reduced form is estimated with the following linear probability model for the four parent–

²² Omitting the cohort of the reform year and one cohort before and after the reform year increases the first-stage coefficient to 0.478; omitting even two (three) cohorts before and after the reform year raises the coefficient to 0.588 (0.604). In line with this finding, omitting the immediate cohorts around the reform year also increases the second-stage coefficient for the mothers–sons subsample (see Table A3 in the Online Appendix).

²³ Ordinary least-squares estimates are presented in Table A1 in the Online Appendix.

child gender pairs separately:

$$\text{school}^{\text{Child}} = \beta_0 + \beta_1 \times \text{Reform}^{\text{Parent}} + \beta_2 \times (\text{Year of Birth Dummies})^{\text{Parent}} + \beta_3 \times (\text{State of Residence Dummies})^{\text{Parent}} + \epsilon. \quad (1)$$

Here, $\text{school}^{\text{Child}}$ equals 1 if the child obtained a middle school or higher school degree and equals 0 if the child obtained a basic school degree or left school without any degree. $\text{Reform}^{\text{Parent}}$ is a binary indicator for whether the parent was subject to the compulsory ninth grade. To exploit within-state within-cohort variation in the length of compulsory schooling, the maximal sets of parent's state of residence and parent's year of birth dummies are included. The state fixed effects capture state-specific factors such as differences in socioeconomic levels or in attitudes toward education, while year of birth fixed effects capture factors that are common to all states, such as the secular trend toward higher education.²⁴

Table 3 presents the reduced-form estimates for the full sample of parents with any type of school degree. Only the additional school year for mothers seems to affect their sons' educational attainment; there is no evidence that mothers' additional schooling affects their daughters' education or that fathers' schooling has any effect. The basic specification (the first column of each parent-child subsample) assumes that the timing of a state's introduction of the compulsory ninth grade was random. However, if there are factors that are correlated with both the timing of the reform and children's education, the effect of parents' education will not be estimated consistently. The coefficient would be underestimated, for example, if a state introduced the compulsory ninth grade because the level of educational attainment was declining in that state. However, it is not clear whether one should control for state-specific trends in a

²⁴ Standard errors in all regressions are calculated with the cluster-command in STATA, allowing for correlations of errors within state \times year of birth cells, the level of the reform indicator. Underestimating standard errors – a common concern in difference-in-differences models (see Bertrand *et al.*, 2004) – seems to be a minor issue here because additional regressions show that errors are not positively serially correlated within states. Because I want to emphasize the significant effect of mothers' education on their sons' education, this analysis is based on specification 2 of Table 4 (the results are quite similar for specifications 1 and 3). I compute the mean of the residuals of specification 2 for each state \times year of (mother's) birth cell and regress this mean residual on the lagged mean residual in the same state. First, I run this regression for each state separately. The coefficients on the lagged mean residual vary between -0.299 and $+0.201$ (all coefficients are statistically insignificant; coefficients for five states are even negative). To estimate the overall serial correlation of errors within states, I then run the same regression on the sample pooling all states (adding state fixed effects). The coefficient on the lagged mean residual is virtually zero (coefficient = -0.005 ; standard error = 0.069). These results indicate that errors are not too small because of positive serial correlation, and that clustering standard errors at the state \times year level seems therefore appropriate.

Table 3. *Reduced-form results: full sample*

	Mothers–sons			Mothers–daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for grade 9 in basic school	0.075** (0.037)	0.093** (0.039)	0.078** (0.039)	0.032 (0.034)	0.002 (0.036)	0.003 (0.035)
State-specific linear cohort trends	No	Yes	Yes	No	Yes	Yes
Year of birth dummies (child)	No	No	Yes	No	No	Yes
Number of children	4,647	4,647	4,647	4,677	4,677	4,677
	Fathers–sons			Fathers–daughters		
	(7)	(8)	(9)	(10)	(11)	(12)
Dummy for grade 9 in basic school	−0.014 (0.029)	−0.012 (0.034)	−0.010 (0.034)	0.004 (0.031)	−0.017 (0.034)	−0.021 (0.031)
State-specific linear cohort trends	No	Yes	Yes	No	Yes	Yes
Year of birth dummies (child)	No	No	Yes	No	No	Yes
Number of children	4,486	4,486	4,486	4,511	4,511	4,511

Source: SOEP.
Notes: Dependent variable: child obtains at least a middle school degree (equals 1 for middle, technical, or high school degree and equals 0 for a basic school degree or left school without a degree). Ordinary least-squares regressions. Robust standard errors clustered at the state \times parent's year of birth level in parentheses. State-specific linear cohort trends are interactions of state dummies with a linear trend in parent's year of birth. All regressions include the maximal sets of parent's year of birth dummies and state of residence dummies. Samples contain parents born between 1930 and 1960, living in the 10 West German states. ***, **, and * denote significance levels $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

difference-in-differences approach because time trends might pick up the effects of the reform and not just pre-existing trends (Wolfers, 2006). Adding state-specific linear cohort trends (i.e., interactions of state dummies with a linear trend in parent's year of birth) changes the results only very slightly. To capture the secular trend toward higher educational attainment, fixed effects for the child's year of birth are added. This control might be problematic, however, if parental schooling affects the timing of childbearing (e.g., mothers with more schooling giving birth later). Table 3 shows that the results are insensitive to adding a child's year of birth, suggesting that the introduction of the compulsory ninth grade did not greatly affect the timing of childbearing.²⁵

Even though the compulsory schooling reform affected only the years of schooling of parents with a basic school degree (see Table 2), the reform might in the first place have changed the probability that an individual obtained a basic school degree. This is possible because the introduction of the compulsory ninth grade in basic school reduced the amount of additional schooling between a middle school degree (typically requiring 10 years)

²⁵ Additional regressions with a child's year of birth as the dependent variable (not shown) also provide evidence that the reform did not affect the timing of childbearing.

Table 4. *Reduced-form and TSIV results: only parents with basic school degree*

	Mothers–sons			Mothers–daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for grade 9 in basic school	0.138** (0.057)	0.136** (0.060)	0.136** (0.062)	0.049 (0.044)	0.010 (0.044)	0.015 (0.043)
State-specific linear cohort trends	No	Yes	Yes	No	Yes	Yes
Year of birth dummies (child)	No	No	Yes	No	No	Yes
Number of children	2,981	2,981	2,981	3,108	3,108	3,108
Two-sample IV estimates						
Dummy for grade 9 in basic school	0.338** (0.144)	0.333** (0.150)	0.335** (0.155)	0.121 (0.109)	0.025 (0.108)	0.037 (0.106)
	Fathers–sons			Fathers–daughters		
	(7)	(8)	(9)	(10)	(11)	(12)
Dummy for grade 9 in basic school	0.034 (0.049)	0.020 (0.052)	0.023 (0.056)	−0.017 (0.051)	−0.060 (0.047)	−0.048 (0.041)
State-specific linear cohort trends	No	Yes	Yes	No	Yes	Yes
Year of birth dummies (child)	No	No	Yes	No	No	Yes
Number of children	2,755	2,755	2,755	2,799	2,799	2,799
Two-sample IV estimates						
Dummy for grade 9 in basic school	0.084 (0.119)	0.050 (0.127)	0.056 (0.139)	−0.041 (0.126)	−0.147 (0.114)	−0.117 (0.099)

Source: SOEP; also QaC for computation of TSIV estimates.

Notes: See notes of Table 3. For the computation of the TSIV estimates, see the main text. ***, **, and * denote significance levels $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

and a basic school degree from two school years to one school year. This might have induced some individuals to acquire a more academic middle school degree instead of a basic school degree. However, Pischke and von Wachter (2008, Table 1) have precisely estimated that the introduction of the compulsory ninth grade did not affect the school track choice (based on the large Micro Census and the QaC). Using the SOEP dataset, I also find that the compulsory ninth grade introduction did not affect the share of individuals acquiring a basic school degree (results available upon request).

Because the reform did not affect the probability of obtaining a basic school degree, it is possible to estimate the intergenerational education effects only for the subsample of parents with a basic school degree. Restricting the estimation sample to this subpopulation,²⁶ the positive effect of mothers' schooling on their sons' education becomes substantially stronger (Table 4). As for the full sample, mothers' schooling seems to have no causal effect on their daughters' education and fathers' schooling has no

²⁶ To be precise, the mothers–sons and mothers–daughters samples contain only mothers with a basic school degree. The spouses of these mothers might have obtained another school degree (similarly for the samples with fathers).

effect on their children's educational attainment.²⁷ As before, adding state-specific cohort trends and a child's year of birth changes the coefficient only slightly.²⁸

Although IV estimates cannot be obtained using the SOEP data, it is possible to combine the reduced-form results of the SOEP with the first-stage results of the QaC data to construct TSIV estimates (see Angrist and Krueger, 1992). The TSIV coefficient is obtained by dividing the reduced-form coefficient by the respective first-stage coefficient. The standard error is derived using the delta method, as in Pischke and von Wachter (2008, their footnote 9). Using the specification that controls for state-specific cohort trends, the TSIV coefficient estimate is 0.33 ($= 0.136/0.408$) for the effect of mothers' schooling on sons' education (with standard error 0.15). This is a strong effect: *ceteris paribus*, sons are 33 percentage points more likely to obtain a middle school or higher school degree if their mother acquired one more year of schooling. Although the point estimate is quite large, note that the effect is not very precisely estimated. Nevertheless, it is possible to rule out that the effect of mothers' schooling is zero.

To compare the magnitude of this effect with the effect sizes of similar studies, the dependent variables need to be made as comparable as possible. To convert the outcome "child obtains at least a middle school degree" into years of schooling, it is necessary to consider the distribution of children's school degrees. Of the sons of mothers with a basic school degree, 56.5 percent obtain a middle school or higher school degree. Of those sons, 51.8 percent obtain a middle school degree, 15.9 percent obtain a technical school degree, and 32.4 percent obtain a high school degree. Thus, the average years of schooling for sons with at least a middle school degree is 11.3 years ($= 10 \times 0.518 + 12 \times 0.159 + 13 \times 0.324$). Among the group of sons who obtain at most a basic school degree, 4.6 percent are dropouts, for whom years of schooling is commonly assumed to be seven years. Therefore, the average years of schooling for this group is 8.9 years ($= 7 \times 0.046 + 9 \times 0.954$). Hence, the difference in average years of schooling between sons with at most basic school degree and sons with a higher school degree is 2.4 years. This means that an effect size

²⁷ The effect of mothers' education might be stronger than that of fathers if women were more strongly affected by the compulsory schooling reform than men. This explanation can be ruled out, however, because separate first-stage regressions by gender show that women (coefficient = 0.42) and men (0.40) were very similarly affected by the reform. This indicates that the effects of parents' education differ at the child outcome stage.

²⁸ A falsification test corroborates the notion that the coefficients for parents with a basic school degree can be interpreted causally because coefficients are close to zero and statistically insignificant in the subsample of mothers with higher school degrees (see Table A2 in the Online Appendix). In the Online Appendix, I also discuss the potentially confounding influence of the short school years in Germany.

of 33 percentage points is equivalent to 0.79 more years of schooling ($= 0.33 \times 2.4$).²⁹ This effect is much larger than those found for similar school reforms in other countries, where the effect of mothers' education on sons' education is concentrated around 0.10 years of schooling; see, for example, Black *et al.* (2005b) for Norway and Holmlund *et al.* (2011) for Sweden. This discrepancy might be the result of differences in the school system. In general, German results are difficult to compare with the results from other countries, where all students often attend the same school until age 15. In particular, the early school tracking in Germany might lead to stronger intergenerational effects because a slight improvement in the child's academic performance in primary school can easily lead to the attendance of a higher secondary school track.³⁰ Because a basic school degree typically requires only nine years and a high school degree 13 years, effects measured in terms of years of schooling can easily become quite large in Germany.

In summary, the results suggest that mothers' additional schooling affects their sons' education, whereas fathers' schooling seems to have no effect on their children's education. Strikingly, the pattern (but not the effect sizes) of the intergenerational education effects in Germany is quite similar to those found for other countries that experienced similar compulsory schooling reforms (see Holmlund *et al.*, 2011, Table 1, for an overview of existing studies). The effect of mothers' schooling on sons' education is stronger in Germany, which might be because of differences in the school systems, in particular the early tracking in Germany.

VI. Transmission Channels

In this section, I investigate several potential transmission channels through which parents' education might affect their children's education. Because only mothers' schooling seems to have a causal effect, the analyses focus on women. First, I analyze traditionally studied channels, such as labor-market outcomes, assortative mating, and fertility. Second, I study for the first time – to the best of my knowledge – another potentially important channel: the effect of schooling on parents' valuation of their children's education. Finally, I present descriptive evidence regarding why mothers'

²⁹ The identical effect size is also obtained with a TSIV estimate, where the reduced form directly uses sons' years of schooling as a dependent variable.

³⁰ For example, students in the state of Bavaria need a grade point average (GPA) of 2.33 or better in the major subjects, German and mathematics, at the end of primary school to be allowed to attend a high school. If a student's GPA is slightly worse than 2.33, he or she can attend a middle school. However, if the GPA is worse than 2.66, the student is only allowed to attend a basic school. GPA differences of 0.33 are quite small compared to the entire grade scale, which ranges from 1.0 (very good) to 6.0 (failed).

education might be more beneficial to their sons' education than to their daughters' education.³¹

Traditional Channels

The results for the traditionally studied transmission channels suggest that these channels cannot explain the positive effect of mothers' schooling on their sons' education.³²

Labor-market outcomes affect the resources (both time and money) that parents can invest in their children's education. Using the large Micro Census, the results reveal that women with an additional year of schooling have the same probability of being employed as women with less schooling. Conditional on being employed, women with more schooling also work about the same number of hours per week as women with less schooling. Restricting the sample to women with regular employment who work at least 20 hours per week and whose predominant source of income comes from employment, the results indicate that women with more schooling do not have a higher labor-market income. Similarly, there are no labor-market effects for men, which is in line with Pischke and von Wachter (2008) who have estimated the effect of the ninth grade introduction on labor-market outcomes jointly for women and men.

Women with more schooling might have better-educated partners (assortative mating). In this case, women with more schooling might have better-educated sons because their better-educated partner might have genetically passed on superior ability and/or has had a non-genetic influence, such as better child-rearing skills. Restricting the sample to women with a basic school degree in both the Micro Census and the SOEP, regression results indicate that women with nine years of schooling do not have better-educated partners than women with only eight years of schooling. Also, women with more schooling are not more likely to have a male partner (either married or cohabiting). Finally, including dummies for father's school degree as an additional control variable in the specifications of Table 4 reduces the positive effects in the mothers-sons sample only slightly.

³¹ Recent work points out that child health might be one important pathway between parental and child education (see Currie, 2009, for an overview). Unfortunately, because of missing information on children's health, the SOEP does not allow an investigation of this channel. Recent studies have also attempted to identify the types of parental resources and inputs that are beneficial to children's development and to identify at which stage of the child's development parents' investments are most important (for a review, see Almond and Currie, 2011). Although the SOEP contains some information on such child outcomes, reliable effects on these outcomes are hard to obtain because sample sizes are rather small as a result of many missing observations.

³² The results tables of this section are available upon request from the author.

This indicates that assortative mating cannot explain the effect of mothers' schooling on their sons' education.

Fertility is another potential channel that might explain the effect of mothers' education. Mothers with fewer children have more time and more resources to invest in each child, for example, by helping with homework and other school-related projects. Using the ALLBUS – the only dataset that contains reliable information on the number of children a woman has borne – there is some evidence to suggest that women who were subject to the compulsory ninth grade have slightly fewer children. The respective coefficient is not statistically significant in the specification without controls for state-specific cohort trends, but does become somewhat larger in magnitude (-0.3) and statistically significant when these controls are added. Even though the results suggest that women with more schooling have fewer children, it is not clear whether this partly explains the positive effect of mothers' schooling because existing evidence on the causal effect of child quantity on child quality is rather ambiguous (e.g., Rosenzweig and Wolpin, 1980; Black *et al.*, 2005a). Unfortunately, it is not possible to control for family size directly in the SOEP because of missing information.

Parents' Valuation of Children's Education

Besides the potential effects on the traditionally investigated channels, additional schooling might also change individuals' preferences and attitudes.³³ Björklund and Salvanes (2011, p. 215) have stated that “the fact that parents with higher education may be more aware of the value of the pecuniary and nonpecuniary advantages of education may mean that they put more pressure on their children to achieve more, or that they simply provide this necessary information to their children.” This view implies that even if additional schooling does not increase parents' (child-rearing) abilities, it might help to improve their children's educational success through raising parents' awareness that success in school is important for the child's future. Therefore, parents' awareness or valuation of the child's education might partially explain intergenerational education effects.

In 1980 and 1982, the ALLBUS asked their participants to indicate the importance of several traits and abilities that can be fostered by appropriate parenting. Specifically, participants were asked the following question. “How important do you rate these educational goals when thinking about the education of, for example, a 10-year-old child?” The seven possible answer categories ranges from 1 (not important) to 7 (very important). The

³³ Using the US GSS, for example, Oreopoulos and Salvanes (2011) have presented evidence that individuals with more schooling are more patient.

Table 5. *Effect on parents' valuation of children's education*

	Individuals with basic school degree		Individuals with higher school degree	
	(1)	(2)	(3)	(4)
Dependent variable: good school performance				
Dummy for grade 9 in basic school	0.416*** (0.148)	0.530*** (0.162)	−0.117 (0.176)	−0.137 (0.197)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	1,363	1,363	960	960
Dependent variable: studiousness				
Dummy for grade 9 in basic school	0.270** (0.135)	0.237* (0.133)	0.011 (0.204)	−0.023 (0.227)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	1,363	1,363	956	956

Source: ALLBUS, waves 1980 and 1982.
Notes: Dependent variables: good school performance and studiousness of children, respectively, as goals of parental education (ranging from 1 = not important, to 7 = very important). In 1980 and 1982, ALLBUS participants were asked to indicate the importance of several traits and abilities that can be fostered by parental education. Specifically, participants were asked the following question. “How important do you rate these educational goals, thinking about the education of, for example, a 10-year-old child?” Ordinary least-squares regressions. Robust standard errors clustered at the state × year of birth level in parentheses. Samples include individuals born between 1930 and 1960 living in West Germany. All specifications include the maximal sets of year of birth dummies, state of residence dummies, survey year dummies, a quartic in age, and a male dummy. ***, **, and * denote significance levels $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

two traits most interesting for this study concern *good school performance* and *studiousness* of the child.

The results in Table 5 suggest that the additional ninth grade led individuals with a basic school degree to value good school performance by their child more highly. This result is robust to including state-specific cohort trends (see Columns 1 and 2). Because sample sizes are limited, women and men are pooled in all specifications. The magnitude of the reduced-form coefficient (about 0.5) is economically significant, given that the trait *good school performance* has a standard deviation of 1.2 among individuals with a basic school degree. As expected, there is no evidence that the additional ninth grade affected the valuation of children's school performance among individuals with a higher school degree.

In addition to the positive effect on the valuation of children's school performance, the additional school year seems also to have increased the importance to an individual that his or her child is studious (Columns 5 and 6). Although the coefficients are smaller than those for the trait *good school performance*, the results are in the same direction: parents with more schooling attach a higher importance to their children working hard. Again, there is no effect on the valuation of children's studiousness among individuals with a middle or high school degree. These results are in line

with those of Carneiro *et al.* (2013) who have found that in the US mothers with more schooling are more likely to believe that their seven-year-old child will later attend college.

In summary, more schooling seems to increase individuals' valuation of aspects likely to be important for their children's educational success: good school performance and studiousness. This change in parents' valuation might be one channel through which mothers' education affects their sons' education.

Differential Effects of Mothers' Schooling on the Education of Their Sons and Daughters

The finding that mothers' schooling affects the education of their sons but not that of their daughters is puzzling (a finding similarly reported for Norway; see Black *et al.*, 2005b). Assume, for a moment, that the additional year of schooling increased the child-rearing abilities of women: why does a son benefit from a more able mother but a daughter does not?

To shed some light on the gender-specific intergenerational education effects, I compare parents' aspirations for their sons' and daughters' education. The 1986 survey of the ALLBUS contains information on individuals' educational aspirations for both daughters and sons.³⁴ The two relevant questions are the following. "Suppose you had a 10-year-old daughter. Which type of degree should she obtain in your opinion? And what about a 10-year-old son. Which type of degree should he obtain in your opinion?" The answer categories include both school and college degrees: (1) basic school degree; (2) middle school degree; (3) certificate to study only particular subjects at a higher education institution (*fachgebundene Hochschulreife*); (4) general high school degree (*Abitur*); (5) degree from a polytechnic (*Fachhochschule*) or university.

Table 6 indicates that women with a basic school degree are more likely to have higher educational aspirations for sons than for daughters compared to other individuals. Individuals with a basic school degree are about 7 percentage points more likely to have a higher educational aspiration for their son than for their daughter (Column 1). The positive, though statistically insignificant, coefficient on the interaction term in Column 2 suggests that women with a basic school degree are more likely to have higher educational aspirations for a son versus a daughter compared to women with a higher school degree and compared to men with the same

³⁴ Because the 1986 survey contains no information on whether the respondent actually has a child, the 1984 survey and the birth cohorts two years earlier (1928–1958) are used to approximate the fraction of individuals having children in the 1986 survey. Using the 1984 survey data as a proxy, a large majority of women (about 80 percent) and men (70 percent) in the 1986 survey are parents.

Table 6. *Aspirations for the education of sons and daughters*

	All individuals		Individuals with basic school degree	Females
	(1)	(2)	(3)	(4)
Basic school degree	0.069*** (0.022)	0.050* (0.028)		0.094*** (0.033)
Female		0.007 (0.030)	0.047 (0.030)	
Female × basic school degree		0.037 (0.041)		
Year of birth dummies	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes
Individuals	1,604	1,604	900	839

Source: ALLBUS, 1986 survey.
Notes: Dependent variable: equals 1 if individual has higher educational aspiration for a son than for a daughter; 0 otherwise. Coefficients and standard errors (clustered at state × year of birth level) from linear probability models reported. Samples include individuals born between 1930 and 1960, living in West Germany. ALLBUS participants answered the following two questions in the 1986 survey. “Suppose you had a 10-year-old daughter. Which type of degree should she obtain in your opinion?” The same question was repeated for a son. The answer categories are as follows: basic school degree (1), middle school degree (2), certificate to study only particular subjects at a higher education institution (*fachgebundene Hochschulreife*) (3), general high school degree (*Abitur*) (4), and a degree from a polytechnic (*Fachhochschule*) or university (5). ***, **, and * denote significance levels $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

degree. Looking at the two subsamples separately yields the same findings. Among individuals with a basic school degree, women seem to have a stronger preference for sons’ education compared to men (Column 3); among women, those with a basic school degree have a much stronger preference for sons’ education compared to women with a higher school degree (Column 4).³⁵

For developing countries, there is abundant evidence that parents prefer sons over girls, which even affects fertility behavior (e.g., Sen, 1990). Similar to my findings, a survey among parents in India provides direct evidence on stronger educational aspirations for sons compared to girls (no difference between mothers and fathers; see Beaman *et al.*, 2012). For developed countries, there is less evidence on gender preferences. For example, Dahl and Moretti (2008) have studied the effect of having a daughter on family structure and fertility behavior in the US and have concluded that parents favor boys over girls. In line with my results, Wentzel (1998) has surveyed 363 parents (81 percent female) from one public elementary school in the US and has found that they have stronger educational aspirations for

³⁵ In separate regressions (not shown), the effect of the compulsory ninth grade introduction on parents’ educational aspirations is small and insignificant, implying that the additional year of schooling did not affect parents’ aspirations concerning the preference for son’s versus daughter’s education.

sons than for daughters.³⁶ Corroborating my findings, a recent representative survey among young people in Germany (aged 16–29) reveals that lower-educated individuals (pooling girls and boys) are more likely to have traditional gender views than higher-educated individuals (e.g., the man should be the principal earner in the family; see Table 4.14 of Gille *et al.*, 2006). If men are supposed to be the main earners, then boys' education might also be considered more important than that of girls (not asked in the survey).

In summary, differential educational aspirations might partially explain why mothers' schooling has differential effects on sons' and daughters' education. If the additional year of schooling increased women's child-rearing skills and if women (perhaps because of the paternalistic culture they were raised in) have higher educational aspirations for their sons compared to their daughters, then mothers' increased child-rearing skills will benefit their sons more than their daughters.

VII. Conclusion

In this study, I have estimated the causal effect of an additional year of schooling for lower-educated individuals on their children's education. The exogenous variation in parents' schooling stems from the extension of compulsory schooling from eight to nine years implemented in all West German states between 1946 and 1969. Even though this reform seems to have had no effect on earnings (Pischke and von Wachter, 2008) or political behavior (Siedler, 2010) – but some effect on health outcomes (Kemptner *et al.*, 2011) – I find that an additional year of schooling for women improves their sons' education. In contrast, mothers' schooling has no effect on their daughters' education and fathers' schooling has no effect on the education of their children. These results are similar to findings for Scandinavian countries, but the effect of maternal schooling on sons' education appears to be considerably stronger in Germany. This could be because the German school system is significantly different from the systems in other countries. Most importantly, the rigid and early school tracking in Germany at about age 10 might result in stronger intergenerational schooling effects, because a slight improvement of the child's performance in primary school can easily lead to the attendance of a higher secondary school track, and thus to a higher educational degree.

³⁶ Jacob (2010, Appendix A) contains an extensive list of studies on parental expectations and aspirations for their children's educational attainment. However, except for Wentzel (1998), none of these studies has looked at parents' educational aspirations for sons and daughters separately.

Concerning transmission channels, there is evidence for one interesting mechanism that might partially mediate the strong positive effect of mothers' education: parents with more schooling value good school performance by their children more highly and are also more likely to believe that it is important for children to be studious. In contrast, there is no evidence that any of the traditionally discussed channels, such as better labor-market outcomes (in line with the findings of Pischke and von Wachter, 2008) or assortative mating, can explain the effect of mothers' schooling. Consistent with the differential effects of mothers' education on that of their sons and daughters, descriptive evidence shows that lower-educated women have higher educational aspirations for their sons than for their daughters. If more schooling increases women's (unobserved) child-rearing abilities, then sons – rather than daughters – are more likely to benefit from these additional abilities.

The transmission channels of intergenerational schooling effects are worth a great deal more investigation. In addition to classic mechanisms, such as labor-market effects, future studies might more closely investigate the potential effects on parents' educational valuations and how additional education changes parents' child-rearing activities, such as reading, attending cultural events with their children, and the like. This could provide a deeper understanding of why children of better-educated parents achieve high education levels themselves.

Supporting Information

The following supporting information can be found in the online version of this article at the publisher's web site.

Online Appendix

References

- Almond, D. and Currie, J. (2011), Human Capital Development before Age Five, in O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, Vol. 4, North-Holland, Amsterdam, 1315–1486.
- Angrist, J. and Krueger, A. (1992), The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples, *Journal of the American Statistical Association* 87, 328–336.
- Beaman, L., Duflo, E., Pande, R., and Topalova, P. (2012), Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India, *Science* 335, 582–586.
- Bertrand, M., Duflo, E., and Mullainathan, S. (2004), How Much Should We Trust Differences-in-Differences Estimates?, *Quarterly Journal of Economics* 119, 249–275.

- Björklund, A. and Salvanes, K. (2011), Education and Family Background: Mechanisms and Policies, in E. Hanushek, S. Machin, and L. Woessmann (eds.), *Handbook of the Economics of Education*, Vol. 3, North-Holland, Amsterdam, 201–247.
- Black, S. and Devereux, P. (2011), Recent Developments in Intergenerational Mobility, in O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, Vol. 4, North-Holland, Amsterdam, 1487–1541.
- Black, S., Devereux, P., and Salvanes, K. (2003), Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital, NBER Working Paper 10066.
- Black, S., Devereux, P., and Salvanes, K. (2005a), The More the Merrier? The Effect of Family Size and Birth Order on Children's Education, *Quarterly Journal of Economics* 120, 669–700.
- Black, S., Devereux, P., and Salvanes, K. (2005b), Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital, *American Economic Review* 95 (1), 437–449.
- Card, D. (1999), The Causal Effect of Education on Earnings, in O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, Vol. 3A, North-Holland, Amsterdam, 1801–1863.
- Carneiro, P., Meghir, C., and Parey, M. (2013), Maternal Education, Home Environments, and the Development of Children and Adolescents, *Journal of the European Economic Association* 11, 123–160.
- Chevalier, A. (2004), Parental Education and Child's Education: A Natural Experiment, IZA Discussion Paper 1153.
- Chevalier, A., Harmon, C., O'Sullivan, V., and Walker, I. (2010), The Impact of Parental Income and Education on the Schooling of their Children, Working Paper 201032, Geary Institute, University College Dublin.
- Currie, J. (2009), Healthy, Wealthy and Wise: Socioeconomic Status, Poor Health in Childhood and Human Capital Development, *Journal of Economic Literature* 47, 87–122.
- Dahl, G. and Moretti, E. (2008), The Demand for Sons, *Review of Economic Studies* 75, 1085–1120.
- Dee, T. (2004), Are There Civic Returns to Education?, *Journal of Public Economics* 88, 1697–1720.
- Dustmann, C. (2004), Parental Background, Secondary School Track Choice, and Wages, *Oxford Economic Papers* 56, 209–230.
- Galindo-Rueda, F. (2003), The Intergenerational Effect of Parental Schooling: Evidence from the British 1947 School Leaving Age Reform, Unpublished, London School of Economics.
- Gille, M., Sardei-Biermann, S., Gaiser, W., and de Rijke, J. (2006), *Jugendliche und junge Erwachsene in Deutschland: Lebensverhältnisse, Werte und gesellschaftliche Beteiligung 12- bis 29-Jähriger (Adolescents and Young Adults in Germany: Living Conditions, Values, and Civic Participation of 12–19 Year Olds)*, VS Verlag für Sozialwissenschaften, Wiesbaden.
- Harmon, C., Oosterbeek, H., and Walker, I. (2003), The Returns to Education: Microeconomics, *Journal of Economic Surveys* 17, 115–156.
- Haveman, R. and Wolfe, B. (1995), The Determinants of Children's Attainments: A Review of Methods and Findings, *Journal of Economic Literature* 33, 1829–1878.
- Heineck, G. and Riphahn, R. (2009), Intergenerational Transmission of Educational Attainment in Germany: The Last Five Decades, *Jahrbücher für Nationalökonomie und Statistik* 229, 36–60.
- Hertz, T., Jayasunderay, T., Piraino, P., Selcuk, S., Smithy, N., and Verashchagina, A. (2007), The Inheritance of Educational Inequality: International Comparisons and Fifty-Year Trends, *B.E. Journal of Economic Analysis and Policy* 7, Article 10.

- 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, 615–651.
- Jacob, M. (2010), Parental Expectations and Aspirations for their Children's Educational Attainment: An Examination of the College-Going Mindset among Parents, PhD Dissertation, University of Minnesota.
- Jürges, H. and Schneider, K. (2007), What Can Go Wrong Will Go Wrong: Birthday Effects and Early Tracking in the German School System, MEA Discussion Paper 138.
- Kempton, D., Jürges, H., and Reinhold, S. (2011), Changes in Compulsory Schooling and the Causal Effect of Education on Health: Evidence from Germany, *Journal of Health Economics* 30, 340–354.
- Lohmar, B. and Eckhardt, T. (2010), The Education System in the Federal Republic of Germany 2008, Secretariat of the Standing Conference of the Ministers of Education and Cultural Affairs, Bonn.
- Maurin, E. and McNally, S. (2008), Vive la Révolution! Long Term Returns of 1968 to the Angry Students, *Journal of Labor Economics* 26, 1–33.
- Meghir, C. and Palme, M. (2005), Educational Reform, Ability, and Family Background, *American Economic Review* 95 (1), 414–424.
- Milligan, K., Moretti, E., and Oreopoulos, P. (2004), Does Education Improve Citizenship? Evidence from the United States and the United Kingdom, *Journal of Public Economics* 88, 1667–1695.
- OECD (2004), *Learning for Tomorrow's World: First Results from PISA 2003*, OECD, Paris.
- Oreopoulos, P. (2006), Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter, *American Economic Review* 96 (1), 152–175.
- Oreopoulos, P. and Salvanes, K. (2011), Priceless: The Nonpecuniary Benefits of Schooling, *Journal of Economic Perspectives* 25, 159–184.
- Oreopoulos, P., Page, M., and Stevens, A. (2006), The Intergenerational Effects of Compulsory Schooling, *Journal of Labor Economics* 24, 729–760.
- Page, M. (2006), Father's Education and Children's Human Capital: Evidence from the World War II GI Bill, University of California, Davis Working Paper 06-33.
- Petzold, H. J. (1981), *Schulzeitverlängerung: Parkplatz oder Bildungschance? Die Funktion des 9 und 10 Schuljahres (Extending the Amount of Schooling: Parking Space or Educational Opportunity? The Function of the 9th and 10th School Years)*, pad extra Buchverlag, Bensheim.
- Piopiunik, M. (2011), Intergenerational Transmission of Education and Mediating Channels: Evidence from Compulsory Schooling Reforms in Germany, Ifo Working Paper 107.
- Pischke, J. S. and von Wachter, T. (2005), Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation, NBER Working Paper 11414.
- Pischke, J. S. and von Wachter, T. (2008), Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation, *Review of Economics and Statistics* 90, 592–598.
- Rosenzweig, M. and Wolpin, K. (1980), Testing the Quantity–Quality Fertility Model: The Use of Twins as a Natural Experiment, *Econometrica* 48, 227–240.
- Sen, A. (1990), More Than 100 Million Women Are Missing, *New York Review of Books* 37, 61–66.
- Siedler, T. (2010), Schooling and Citizenship in a Young Democracy: Evidence from Postwar Germany, *Scandinavian Journal of Economics* 112, 315–338.
- Terwey, M. and Baltzer, S. (2009), *Datenhandbuch ALLBUS 1980–2008 (Data Manual ALLBUS 1980–2008)*, ZA-No. 4570, GESIS, Cologne and Mannheim.

- Wentzel, K. (1998), Parents' Aspirations for Children's Educational Attainments: Relations to Parental Beliefs and Social Address Variables, *Merrill-Palmer Quarterly* 44, 20–37.
- Wolfers, J. (2006), Did Unilateral Divorce Raise Divorce Rates? A Reconciliation and New Results, *American Economic Review* 96 (5), 1802–1820.

First version submitted October 2011;
final version received January 2013.

.