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Intergenerational Transmission of Education and Mediating Channels: Evidence from Compulsory Schooling Reforms in Germany*

Abstract

This paper estimates the causal effect of an additional year of parents' schooling on their children's education, exploiting compulsory schooling reforms that were implemented in all West German states between 1946 and 1969. Although previous research indicates that these reforms had no effects on earnings or political behaviour, I find that an additional year of schooling women strongly affects their sons' education. Based on several datasets, numerous channels that might mediate the positive impact of mothers' education are investigated. Most importantly, individuals with more schooling value children's educational success as more important.

JEL Code: I20, J62.

Keywords: Intergenerational mobility, education, compulsory schooling reforms, SOEP.

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1 Introduction

Children of lower educated parents tend to end up being lower educated themselves.¹ This is an important observation since educational attainment is a strong predictor of labor market success and other life outcomes.² While parents' education has been shown to be the most important factor in explaining the education of their children (see the review by Haveman and Wolfe, 1995), correlation does not necessarily mean causation. Recent empirical studies investigate the extent to which these correlations reflect a causal effect of parents' education on that of their children. Several studies exploit reforms that increased the number of compulsory school years to obtain exogenous variation in parents' schooling, 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. (2010)). These compulsory schooling reforms are typically estimated to have strong positive earnings effects on the order of 10 to 15 percent for those individuals forced to stay in school longer (see, for example, Harmon et al. (2003) and Oreopoulos (2006)). In contrast, Pischke and von Wachter (2008) provide evidence of zero earnings returns to a similar reform in Germany, which extended compulsory schooling by one year, arguing that this finding is due to German students having 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 child-rearing abilities and/or increasing parents' valuation of education, which in turn improved their children's education.

This paper estimates the causal effect of parents' education on that of their children and investigates numerous transmission channels through which the intergenerational effect might work, exploiting changes in compulsory schooling laws in West Germany. After World War II, compulsory schooling was eight years; an additional compulsory ninth grade was introduced in basic schools, the least academic secondary school type, between 1946 and 1969. The reform was implemented in all West German states in different years, thus providing both regional and time-series variation in the amount of required schooling. This variation allows

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

²See Card (1999) for a literature review on the effects of education on earnings and 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).

us to identify the causal effects of an additional year of parents' schooling on 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 household survey that allows linking information on parents and their children. The reform effects on parents' labor market outcomes are estimated with the large Micro Census, which has the advantage that outcomes were reported while most parents were in their prime working age. The impact of more schooling on parents' evaluation of education is studied with the German General Social Survey (ALLBUS), which contains information on individuals' attitudes and behaviors. 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 IV 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 a strong positive effect of mothers' education on that of their sons, but no effect on their daughters' education. Fathers' education has no impact on their children's education. The empirical result that a mother's schooling is one factor that helps improve her son's educational attainment is confirmed by several robustness tests. Concerning transmission channels, the results suggest that individuals with more schooling more highly value children's education. Although this has previously been posited to be one important 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, the additional year of schooling has no effect on the probability of obtaining post-secondary vocational education, 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 a slight negative effect on fertility. Finally, descriptive evidence suggests that the impact of mothers' education differs for sons and daughters partly because lower educated mothers have higher educational aspirations for their sons than for their daughters.

Previous research provides strong evidence that the compulsory schooling reforms in Germany had no effects on earnings or political behavior. In the working paper version,

Pischke and von Wachter (2005) use three large datasets and present precisely estimated effects of the additional ninth grade on wages, with coefficients being close to zero. The authors find no evidence that this is due to labor market institutions or the apprenticeship training system in Germany. Instead, Pischke and von Wachter argue that the absence of any returns to compulsory schooling in Germany is likely due to the institutional features of the German school system: German students had already learned labor-market-relevant skills by the time of graduation from secondary school, whereas in other countries, marginal students were still learning these skills at age 14 or 15. Similarly, Siedler (2010) finds no effects of the additional compulsory school year on political behavior, such as individuals' political interest, participation in voting, and democratic values. This is again in contrast to studies from other countries, which estimate positive political returns to schooling in terms of voter participation and several forms of civic behavior (see Dee (2004) and Milligan et al. (2004)). Siedler discusses several potential explanations for the zero political returns to schooling in Germany and concludes, similar to Pischke and von Wachter (2008), that the fundamentals of democracy had already been learned at an earlier stage of schooling.

To investigate intergenerational schooling effects in Germany is interesting for three distinct reasons. First, as pointed out above, if we find intergenerational schooling effects in Germany, then effects on earnings can be ruled out as a transmission channel—in contrast to other countries with positive intergenerational effects and positive earnings returns. This implies that in Germany there must be other channels mediating 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 reform effects in Germany is that the structure of the school system was the same before and after the extension of compulsory schooling. 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 16.³ Therefore, the estimated intergenerational effects of the Scandinavian countries are difficult to interpret because they reflect the effects of both the compulsory schooling extension and delayed tracking.

The remainder of the paper is structured as follows. Section 2 provides an overview of the literature on the intergenerational transmission of education. Section 3 briefly describes the school system and the compulsory schooling reforms in West Germany. Section 4 describes the datasets. Section 5 presents results of the reform effects on parents' length of schooling and the intergenerational transmission of education, as well as robustness checks. Section 6 presents results on the transmission channels and Section 7 concludes.

2 Existing Literature on the Intergenerational Transmission of Education

Studies on the intergenerational transmission of education take one of three approaches in identifying the causal effects of parents' education on that of their children: natural experiments/instrumental variables, twins as parents, and adoptees.⁴ Several recent literature surveys summarize the findings: Black and Devereux (2011) review studies of the intergenerational effects of both parents' education and parents' earnings; Björklund and Salvanes (2011) and Holmlund et al. (2010) focus on the intergenerational effects of parents' education only. This section briefly summarizes the findings of those intergenerational schooling studies that exploit compulsory schooling reforms. Next, the main differences between this approach and the twin and adoption studies are set out, followed by a discussion of the extant evidence on possible transmission channels.

Most instrumental variables studies on intergenerational schooling effects exploit changes in compulsory schooling laws as a source of exogenous variation in parents' education. In Norway, for example, mandatory schooling was increased from seven to nine years in different municipalities in different years between 1959 and 1973. Black et al. (2005b) use this reform to instrument for parents' education and find a small, statistically significant causal relationship between a mother's and her son's education, but no causal relationship between mothers' and daughters' education and no effect of fathers' education. Similar

³The next section reviews the existing literature on the intergenerational transmission of education.

⁴The empirical research on intergenerational schooling transmission has its theoretical roots in the intergenerational income mobility models (see, for example, Becker and Tomes (1979, 1986) and Solon (1999)).

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., 2010). Based on an increase in the minimum school-leaving age (from 15 to 16) in Britain in the early 1970s, no significant effects of the father's education, but large positive effect of the mother's on her child's education were estimated (Chevalier, 2004). Exploiting the same reform, parents' education appears to have no effect on the probability that their child leaves school early (Chevalier et al., 2010). Galindo-Rueda (2003) uses the earlier minimum school-leaving age reform of 1947 in the United Kingdom and finds 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.⁵ Based on changes of the compulsory schooling laws in the United States, an increase in either parent's schooling by one year is estimated to reduce the child's probability of repeating a grade by 2 to 4 percentage points (Oreopoulos et al., 2006). In sum, studies based on compulsory schooling reforms in other countries with largely different school systems generally find that an exogenous increase in parents' education has a small positive effect on their children's education, with mothers' education being somewhat more important.⁶

Overall, IV studies suggest that mothers' education is more important than fathers' education for the child's school success; however, studies using identical twins or adoptees tend to find that fathers' education is more important (see the surveys cited above). Using adopted children is like an experiment where children are randomly assigned to parents, meaning that, arguably, parents and children share no inheritable characteristics. In this sort of study, it is thus not possible to ascribe an intergenerational correlation to genetic factors. Therefore, any observed correlation between adoptive parents' education and their adoptee's education must be caused by either parents' education or by other (unobserved) parental characteristics, such as parenting style and attitudes toward education. Therefore,

⁵A drawback of the U.K. studies is that the authors are unable to disentangle schooling effects 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 laws.

⁶Other instrumental variables studies use alternative identification strategies to obtain exogenous variation in parents' education. For example, Page (2006) exploits the World War II G.I. Bill and finds that a one-year increase in a father's education reduces the probability that his child repeats a grade in school by about 2 to 3 percentage points. Carneiro et al. (2007) use several instruments that affect the direct and opportunity costs of education in the United States and find 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) exploit a 1968 change in the qualification level required for university admission in France and find 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.

estimates in adoptee studies not only identify the effect of parents' education, but also capture any effects of parents' environment on the adoptee's education that are correlated with parents' education. Similar to using adoptees, the aim of studies using adult identical twins is to eliminate the genetic influences of one of the parents. One implicit assumption of the twins approach is that differences in the education of the twin parents is due to chance, which requires that twins differ only in terms of education but not in terms of other, potentially unobservable, characteristics that affect the child's education.

To better understand why different identification strategies lead to different results, Holmlund et al. (2010) apply all three identification strategies to the same Swedish dataset and find 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. The authors argue 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. The findings of Holmlund et al. (2010) imply that results are comparable only among studies that use similar identification strategies. Therefore, the findings of this paper are compared only to the results of the Scandinavian studies that are also based on reforms that increased compulsory schooling by one or two years and that also were implemented gradually over time.

Although transmission channels mediating the effect of parents' education on that of their children are considered an important issue, only very few intergenerational schooling studies investigate potential channels. Obvious candidates, apart from labor market effects, are assortative mating effects, that is, whether individuals with more schooling have better educated or wealthier partners, as well as the quantity-quality tradeoff, that is, whether individuals with more schooling prefer fewer, but better educated children.⁷ There is no evidence of assortative mating effects based on the compulsory schooling reform in Norway (Black et al., 2003); however, in the United States, mothers with more schooling seem to have better educated partners and delay childbearing (Carneiro et al., 2007). Neither study finds evidence that schooling affects the number of children. The study by Carneiro and coauthors 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

⁷See also Almond and Currie (2011) for a comprehensive literature review of the effects of early childhood influences and interventions on later life outcomes, such as educational attainment.

hours per year when mothers have more education, but, at the same time, do not appear to spend less time with their children. Overall, the authors find no effect on the probability that the child has low birth weight. Furthermore, Carneiro et al. investigate early childhood channels for children up to one-year old and find that mothers with more schooling are more likely not to smoke during pregnancy, breastfeed for a longer period, and work substantially more hours per year. Interestingly, they 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.⁸

In sum, existing studies provide some evidence on potential transmission channels, such as labor market effects and better home environments, but none of the extant work investigates the effects of more schooling on parents' attitudes toward education. This paper, therefore, makes a contribution by providing first evidence that more schooling can increase parents' valuation of children's education (see Section 6.5).

3 School System and Compulsory Schooling Reforms in West Germany

In Germany, children start school in the year after they turn 6. They attend 4 grades in primary school (*Grundschule*) and, at about age 10, students are allocated to one of three different secondary school types. The secondary school tracks differ by both duration and curriculum.⁹ Basic schools (*Hauptschule*) provide basic general education and lead to a certificate after grade 8 or 9. Middle schools (*Realschule*) provide a more extensive general education and cover grades 5 to 10. High schools (*Gymnasium*) offer the most academic curriculum and cover grades 5 to 13.¹⁰ Students who complete basic school typically enter an apprenticeship which combines part-time vocational school and firm-based training. The

⁸All cited effects of the Carneiro et al. (2007) study refer to white children.

⁹In some states, a fourth type of secondary school exists. Comprehensive schools (*Gesamtschule*) offer all lower and upper secondary education levels. Where comprehensive schools exist, only a minor fraction of students attends this school type. See Lohmar and Eckhardt (2010) for a more detailed description of the German school system.

¹⁰There is no perfect translation for the German school type *Gymnasium*. Sometimes the British term "grammar school" is used instead. Note that a German high school (*Gymnasium*) differs from a U.S. high school which is a comprehensive school that is attended by all students in upper secondary education, usually covering grades 9-12.

middle school leaving certificate qualifies students to enter an apprenticeship or school-based vocational training, or to attend a vocational school that provides a higher education entrance qualification. In particular, the technical school degree (*Fachhochschulreife*) can be acquired, and this 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 school performance in primary school which thus depends on the child's ability. In addition, primary school teachers give a school recommendation, which is largely based on the child's grades in the major subjects German and math, and parents might also have considerable influence on the secondary school choice of their child, depending on school laws which differ from state (*Bundesland*) to state.¹¹ The allocation procedure implies that basic school students tend to be the students with the lowest ability. Importantly, the secondary school track allocation after primary school tends to be rather permanent: Although students can theoretically switch between the different secondary school types, mobility is rather limited in practice, with downward mobility being more common than upward mobility (see, for example, Jürges and Schneider, 2007). This implies that the secondary school track decision at the end of primary school is a good predictor of final educational attainment.

In Germany, the decades after World War II witnessed a pronounced trend toward higher education: only 8 percent of the 1930 birth cohort graduated from high school; but 20 percent of the students born in 1960 obtained a high school degree (see Figure 1). 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; the respective share was less than 50 percent for students born in 1960.

Until the end of World War II, basic schools ended at eighth grade. Immediately after the war, the two northern states introduced a compulsory ninth grade: Hamburg in 1946 and Schleswig-Holstein in 1947, where the ninth grade was rooted in older traditions that had been interrupted by the Nazis.¹² During the first half of the 1950s, when youth unemployment was high, political parties, unions, and employer organizations in other states began to emphasize labor market rationales in their arguments for extending the amount of compulsory schooling (Petzold, 1981; Leschinsky and Roeder, 1980, p. 332ff.). During

¹¹The competence for all education policies in Germany lies with the individual states.

¹²West Berlin introduced a mandatory 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.

this period, many states introduced, on a temporary basis, a ninth compulsory school year. The temporary extension of compulsory schooling could be mandated by state education ministers in cities or communities suffering a severe shortage of apprenticeship positions and high youth unemployment.

Toward the end of the 1950s—when Saarland (1958) and Bremen (1959) introduced a compulsory ninth grade on a permanent basis—pedagogical and working-life arguments for a ninth compulsory school year became more prevalent. Pedagogical arguments involved children not being mature enough to enter the workforce, protecting children physically and psychologically from the working life, and the necessity of providing young people with more guidance in choosing an occupation. Working-life arguments emphasized the change in work type from manual to more intellectual, which required a more academic education. Lower Saxony introduced the ninth grade permanently in 1962, just before all states agreed on a ninth compulsory school year in the Hamburg Accord in 1964. Four states introduced the additional compulsory ninth year in basic schools in 1967; Bavaria did so in 1969, the last state to introduce this measure.^{13,14} Note that compulsory education does not necessarily end with secondary school for basic and middle school students, but can continue with vocational training, which is typically covered by attending a part- or full-time vocational school. Table 1 provides an overview of the years in which the compulsory ninth grade was officially introduced in each state and the corresponding birth cohorts first affected by the reform.¹⁵

Due to differential socioeconomic conditions and justifications, the focus of the additional ninth school year in basic schools varied from state to state. For example, North Rhine-Westphalia's focus was on general education, especially the consolidation of basic skills in numeracy, orthography, and language. Schleswig-Holstein, on the other hand, emphasized guidance in choosing an occupation, preparation for working life, and political education, while Bremen stressed a better foundation of general knowledge (Nordrhein-Westfalen, 1962). Overall, the aim of the additional ninth grade was to consolidate basic skills and knowledge, as well as provide students with occupational guidance (Leschinsky and Roeder, 1980, p. 333).

¹³The 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.

¹⁴All states except Hamburg and Bavaria used two short school years in 1966/1967 to shift the start of the school year from April to August (cf. Federal Statistical Office, 1967). These changes are taken into account when constructing the reform indicator. See Pischke (2007) for more detail about the short school years.

¹⁵Note that the introduction dates used in this study differ slightly from those reported by Pischke and von Wachter (2005). This difference leads to larger first-stage coefficients in this paper.

4 Data

The results are based on four datasets. The German Socio-Economic Panel Study (SOEP) is used to estimate the effects of parents' education on that of their children. The SOEP is a large annual household survey that has been conducted in West Germany since 1984 and is representative of the resident population. Participants answer detailed questionnaires covering such diverse topics as demographic characteristics, retrospective biographical information, and educational outcomes.¹⁶ The second dataset is the Micro Census, which provides the official representative statistics of the population and the labor market in Germany. This household survey is conducted on behalf of the German Federal Statistical Office and samples 1 percent of the German resident population. I use the waves from 1976, 1978, 1980, 1982, 1985, 1987, and 1989. Each wave contains about 300,000 individuals. The third dataset is the German General Social Survey (ALLBUS), a repeated biennial cross-section of the resident population, on attitudes, behaviors, and the social structure in Germany. The ALLBUS is representative of the German adult population with German nationality and currently covers the period from 1980 to 2008. It is the German equivalent of the U.S. General Social Survey (GSS).¹⁷ Finally, the Qualification and Career Survey (QaC) is used because it contains, in contrast to the other datasets, information on parents' years of schooling. The QaC is a repeated cross-section of employed German workers aged 15 to 65 collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) and 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 German Socio-Economic Panel (SOEP) is a longitudinal household survey that allows linking information on parents and their children. There are two ways this can be done. First, when both parents and their children participated in the SOEP survey, then information on parents is always self-reported, while information on the children is either reported by parents if the children are below age 16 at the time of the survey or is self-reported by children aged 16 and older. Note that the SOEP data structure allows linking adult children with their parents even if they live in different households. Second, when children participate in the SOEP survey, but their parents do not, then information

¹⁶All questionnaires, both in the original German and translated into English, are available at <http://panel.gsoep.de/soepinfo2009/>. For a detailed description of the SOEP, see Schupp and Wagner (2002), Haisken-DeNew and Frick (2003), and Wagner et al. (2007).

¹⁷Terwey and Baltzer (2009) provide detailed information on the ALLBUS surveys and document the variables available in the cumulated dataset covering the period 1980-2008.

on both child and parents is reported by the child. In the final estimation samples, about half the parents are SOEP participants.

Whether a parent was required to attend basic school for eight 8 or 9 years depends on the state in which he or she went to school and when the parent was born. Because the SOEP survey started collecting information on the state of school attendance only in 2001, this information is known for only 15 percent of the parents. For all other parents in the sample, current state of residence is used as a proxy for state of school attendance.¹⁸ Using year of birth, state of residence or state of last school attendance, and the information on the first affected birth cohort of Table 1, I impute whether an individual was subject to either the eight- or nine-year compulsory schooling law.¹⁹

German datasets generally do not contain information on the number of years of schooling or the highest grade attended, and this is also true of the SOEP. Therefore, it is not possible to obtain first-stage or IV results with the SOEP dataset.²⁰ Hence, it is mainly results from reduced-form regressions that are presented for the effect of parental education on child education. Because first-stage coefficients will be estimated with the Qualification and Career Survey (QaC), two-sample IV 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.

The child's final school degree is obviously of interest in a study investigating the effect of parents' education on that of their children. However, because this study exploits an education 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.²¹ Thus, the dependent variable in this study equals 1 if the child obtains either a middle school degree, a technical school degree, or a high school degree and equals 0 if the child obtains only a basic school degree or left school without a degree. In the estimation

¹⁸Measurement error in the state of school attendance variable should not be a major concern, since 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.

¹⁹Ideally, one would like to construct the binary reform indicator using parents' state of residence before the compulsory schooling 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. But 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. Similarly, selective mobility seems to be a minor problem for the Swedish case. Meghir and Palme (2005) report a low degree of mobility across municipalities and, using birth municipality as an instrument, find no strong evidence that mobility is a source of bias for their results.

²⁰Instead of directly using information on the number of years of schooling, one could use information on the year of secondary school graduation. However, this is not feasible with the SOEP dataset because year of secondary school graduation is available for only 15 percent of the parents in the final sample.

²¹The secular trend toward higher education levels is, of course, taken into account in the empirical analysis.

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. Current secondary school attendance is a good proxy for the final school degree since mobility between the different secondary school tracks is rare (Jürges and Schneider, 2007). Furthermore, an additional specification shows that results are robust when the sample excludes those children who have not yet completed schooling.²²

The SOEP estimation sample includes all individuals (children in the estimations) with at least one parent born during the period 1930-1960 and living in West Germany.²³ Parents who immigrated to Germany after World War II are excluded from the sample. Table 2 provides summary statistics separately 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 reform period 1930-1960 and received a secondary school degree in West Germany.

The Micro Census, the second dataset, contains information on education and labor market outcomes. The huge sample sizes allow a precise estimation of the effects of the additional school year on parents' post-secondary education and labor market outcomes, separately for females and males. A further advantage of the Micro Census over the SOEP dataset 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 not available at all in the case the parents are not SOEP participants themselves. Thus, measurement error in the labor market variables is likely to be a much smaller issue in the Micro Census data. The final Micro Census 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. As in the SOEP dataset, there is no information about years of schooling. Therefore, only reduced-form results will be presented.

A drawback of the Micro Census is that the household structure of the survey does not allow accurate linking of information on parents' and children's final educational at-

²²De Haan and Plug (2011) investigate different methods of dealing with samples in which not all children have finished schooling. Of the three methods for correcting censored observations that they look at, the authors find that the one that treats parental expectations as if they were realizations performs best. This method is similar to the strategy employed here, since parental expectation about the child's final school degree is probably strongly correlated with the type of secondary school the child currently attends.

²³These are exactly the same birth cohorts as in the studies by Pischke and von Wachter (2008) and Siedler (2010).

tainment. For example, children cannot be linked with their parents if the children live in another household. Furthermore, respondents do not provide information on their parents' educational attainment. Thus, parents' and children's school degree can be observed together for only a very short period, that is, between the child's secondary school graduation and the time he or she moves out of the parents' household. Therefore, unbiased estimates of the impact of parents' education on that of their children can be obtained if and only if the probability of observing a child in his or her parents' household is uncorrelated with the child's school degree. In other words, unbiased estimates require that the probability of the child moving out of the parents' household is uncorrelated with the school degree the child obtains. However, 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.

Although sample sizes are limited, the German General Social Survey (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.²⁴ Analogous to the other datasets, the estimation 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.²⁵

In contrast to the other three datasets used in this paper, the Qualification and Career Survey (QaC) provides the year in which an individual graduated from secondary school, which allows computing the number of years of schooling. *Years of schooling*, the dependent variable of the first stage, is constructed as follows. Since 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 6.5.²⁶ As in the SOEP dataset, a dummy variable is constructed indicating how many years of compulsory schooling (eight or nine) an individual was subject to using information on year of birth, current state of residence, and the first affected birth cohort given in Table 1.²⁷ Again, the sample is restricted to individuals of German nationality born between 1930 and 1960 who live in West Germany.

²⁴This question has been explicitly asked in several, but not all, waves.

²⁵Descriptive 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.

²⁶Because children enter primary school in the year after they turn six, they are, on average, 6.5 years old when they start school.

²⁷Information on the state where an individual attended secondary school is not available in the QaC dataset. Also note that parents and children cannot be linked in the QaC survey.

5 Intergenerational Transmission of Education

This section presents evidence about the effects of the compulsory schooling reforms on two outcomes. First, it is shown that the formal introduction of the compulsory ninth grade actually increased the average number of years of schooling of the affected parental cohorts. Then results of the effects of parents' education on that of their children is presented, as well as several robustness checks. As described in the previous section, it is mainly reduced-form estimates, but also some two-sample IV results for the main specifications, that are presented. Furthermore, a falsification test provides evidence that the compulsory ninth grade introduction is a valid instrument for parents' schooling.

5.1 Impact of the Reforms on Parents' Length of Schooling

Before investigating the effect of parents' education on that of their children, it is important to know whether the official introduction of the compulsory ninth grade actually increased the number of years of parents' schooling. Only if the first-stage coefficient is positive and statistically different from zero can the compulsory schooling reforms in West Germany be exploited to estimate causal intergenerational schooling effects.

Administrative data reveal that the formal introduction of the compulsory ninth grade had a strong effect on ninth grade attendance. Figure 2 shows that the share of basic school students attending the ninth grade increased sharply in the year the compulsory ninth grade was officially introduced. The figure plots the number of basic school students attending the ninth grade as a fraction of the number of basic school students attending the eighth grade in the previous school year.²⁸ In the years before the ninth grade was made compulsory, only about 5 percent of basic school students attended a voluntary ninth grade. Immediately after the compulsory ninth grade was formally introduced, the share of basic school students attending a ninth grade increased to about 85 percent. The fact that not all students attended a ninth grade after it was officially made compulsory suggests that not all basic schools could provide the compulsory ninth grade immediately; instead, implementation of the additional grade was a process requiring several years. Because the fraction of ninth-grade students did not go from 0 percent to 100 percent at the time the

²⁸These administrative data are available for six of the 10 West German states. Because the introduction year of the compulsory ninth grade differs across states, it is aligned at $t = 0$ for each state. It is implicitly assumed that a student who attends a ninth grade also completes it. Furthermore, it is assumed that students do not move to another state between grade 8 and grade 9. This assumption seems plausible because students in grade 8 are on the verge of taking final exams.

compulsory ninth grade was formally introduced, the first-stage coefficient will be smaller than 1 in the subsample of basic school students.²⁹

The first-stage coefficient is obtained with the QaC dataset by regressing the constructed length of schooling variable on the binary reform indicator showing whether the individual was affected by the compulsory schooling reform. To arrive at a difference-in-differences estimate, the specifications include the maximal sets of year of birth dummies and state of residence dummies, the two variables that jointly determine the reform indicator. Table 3 shows that the first-stage coefficient is far below 1. Column (1) contains the full sample of 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 by the reform, that is, individuals with a basic school degree, the coefficient increases to 0.41, which is still far below 1.³⁰

Figure 3 illustrates the impact of the introduction of the compulsory ninth grade on the actual length of schooling using the QaC data. The graph plots the average length of primary plus secondary schooling of individuals with a basic school degree in the five years before and after the reform (across all states). It reveals that the average length of schooling was about 8.2 years before the reform and jumped to almost 8.7 years after the reform. Thus, while the difference is less than one year, there is a clear increase in the length of schooling when the compulsory ninth grade was officially introduced.

The administrative data shown in Figure 2 provide some explanation for why the first-stage coefficient for the subsample of basic school students is smaller than 1. In practice, introduction of the ninth grade was not implemented immediately in all basic schools, but required some time until each school offered the mandatory ninth grade. Furthermore, some basic school students attended a voluntary ninth grade before it was made compulsory. Nevertheless, Figure 2 suggests that the first-stage coefficient should be substantially larger than 0.41 for the subsample of individuals with a basic school degree. This discrepancy most likely arises because measurement error in the ninth grade introduction variable attenuates

²⁹Instead of using information on the number of students attending basic schools, Pischke and von Wachter (2005, p. 14) use administrative data on school leavers from the four states that introduced the ninth grade in 1967. Plotting the average number of grades attended by school leavers from 1963 to 1967, they find a similar, though somewhat less pronounced, pattern.

³⁰Pischke and von Wachter (2008) run an equivalent first-stage regression with the QaC dataset. For the subsample of basic track individuals, their coefficient on the ninth grade introduction is only 0.28 and thus substantially smaller than my coefficient. This difference is mainly due to the fact that Pischke and von Wachter use different reform introduction dates for some states. Furthermore, their sample differs somewhat from my sample; for example, they drop all individuals with missing wages.

the coefficient toward zero. There are two sources of measurement error. First, instead of using the state where the individual attended school, the current state of residence is used to construct the reform indicator. Second, the year of birth does not perfectly determine whether an individual was affected by the reform: some children might start primary school one year earlier or one year later than officially scheduled and some students repeat a grade. All these measurement errors can contribute to an attenuation bias of the reform indicator coefficient. Nevertheless, the F-statistic for the excluded instrument in Column (2) is higher than 100, indicating that the instrument is strong.

Since the introduction of the ninth grade was targeted only at basic school students, the compulsory schooling reforms should have had no impact on the length of schooling of individuals who obtained a degree from a middle school or a high school. Column (3) indicates that this is, indeed, the case: introduction of the ninth grade had no effect on the average length of schooling of those individuals who should not have been affected by the reform. This finding corroborates the validity of the instrument.³¹

5.2 Effect of Parents' Schooling on Their Children's Education

This section investigates the relationship between parents' schooling and their children's educational attainment. Before studying causal effects, simple correlations between parental and child education are presented first. Table 4 presents the correlations between parents' and children's years of schooling separately for all four possible child-parent-gender combinations. Since years of schooling is not available in the SOEP dataset, I impute this variable as the number of years of primary plus secondary schooling that is typically required for the secondary school degree obtained. For example, a high school degree typically requires 13 years of schooling.³² To account for the secular trend toward higher education, the maximal sets of dummies for both parents' and children's years of birth are included. Furthermore, state of residence dummies are included as controls to factor out permanent differences between states. Two patterns can be seen in Table 4: the correlations in years of schooling between parents and children are somewhat stronger for sons than for daughters and are also a bit stronger for mothers than for fathers. Differences are statistically significant at the

³¹All standard errors are calculated with the cluster-command in Stata, allowing for correlations of the errors within state and year of birth cells, the level of the reform indicator.

³²For simplicity, it is assumed for the OLS regressions that all individuals with a basic school degree have attended school for nine years.

5 percent level between mothers-daughters and fathers-daughters and between fathers-sons and fathers-daughters.³³

After having observed that parents' and their children's education are strongly positively correlated, the question arises whether this association is due to causal effects of parents' education. To identify causal effects, one needs an instrument that induces variation in parents' education that is independent of parents' ability and other parental characteristics that are correlated with both parents' and child's education. These characteristics might include, for example, parents' degree of patience, risk aversion, or valuation of education, all of which might affect educational choices and can be passed from parents to children. The exogenous variation in parents' length of schooling stems from the school reforms that increased compulsory schooling from eight to nine years between 1946 and 1969 in all West German states. Since the additional ninth grade was introduced in different states in different years, these reforms provide both time-series and cross-sectional variation in the amount of required schooling. This allows estimating the causal intergenerational education effect in a difference-in-differences framework.

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 children's education because the mother is typically the main provider of child care. Moreover, recent studies suggest that the intergenerational transmission of human capital might also differ according to the gender of the child.³⁴

Primarily results from reduced-form regressions are presented. This is the intention-to-treat effect (ITT) in the jargon of the potential outcome framework of Angrist et al. (1996). Specifically, the following basic linear probability model is estimated separately for the four parent-child gender pairs:

$$\begin{aligned} school^{Child} = & \beta_0 + \beta_1 Reform^{Parent} + \beta_2 (Year\ of\ Birth\ Dummies)^{Parent} + \\ & + \beta_3 (State\ of\ Residence\ Dummies)^{Parent} + \epsilon \end{aligned}$$

³³The correlation coefficients are about 0.5 and thus about twice as large as those for the Nordic countries. However, the results are hard to compare because the dependent variable here contains only the number of years of schooling typically required for the school degree, while *years of schooling* in the Scandinavian studies contains both schooling and post-schooling education, such as tertiary education. Furthermore, characteristics of the school systems, for example, age at first tracking, vary between countries.

³⁴Black et al. (2005b), for example, find positive causal education effects for mothers-sons and zero effects for mothers-daughters. Carneiro et al. (2007) find that effects of mothers' schooling on children's abilities in math and reading differ between boys and girls.

where the binary dependent variable $school^{Child}$ 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 a degree. $Reform^{Parent}$ is a binary indicator for whether the parent was affected by the introduction of the compulsory ninth grade. Because the reform indicator is based on the interaction between parent's state of residence and parent's year of birth, the maximal sets of parent's state of residence and parent's year of birth dummies are included to exploit within-state within-cohort variation in compulsory schooling for the identification of causal effects of parents' education on that of their children. The state fixed effects capture state-specific factors such as differences in socioeconomic levels or different attitudes toward education. The year fixed effects capture factors common to all states, such as the secular trend toward higher education.

Table 5 presents the reduced-form estimates for the full sample, which contains parents with any type of secondary school degree. The results of the basic specification are presented in the first column of each parent-child subsample. Only mothers' education seems to have a causal effect on sons' educational attainment; there is no evidence for an effect of mothers' education on daughters' education or of fathers' education on children's education, regardless of child's gender.

The basic specification assumes, however, that the timing of a state's introduction of the compulsory ninth grade was random. In particular, if there are factors that are correlated with both children's education and the timing of the reform, the coefficient on the reform indicator will not be estimated consistently. For example, the coefficient would be *overestimated* if states that introduced the compulsory ninth grade early also implemented other policies aimed at promoting higher educational attainment for children. In this case, part of the positive coefficient on the reform indicator would actually reflect these other policies and not the effect of parents' additional schooling. The coefficient would be *underestimated*, for example, if a state introduced the additional ninth year early because children's educational attainment were declining in that state.

When state-specific time trends are included as controls, identifying the effects of introducing the compulsory ninth grade are based on whether such a change in the law led to deviations from preexisting state-specific trends. However, it is not clear whether one should control for state-specific trends in a difference-in-differences analysis. On the one hand, the omission of state-specific trends will bias the coefficient on the reform indicator if there is a systematic relationship between the trend in (children's) educational attainment

and the introduction of the ninth grade. On the other hand, state-specific time trends may pick up the effects of the reform and not just preexisting trends (Wolfers, 2006). In this case, the state-specific trends mistakenly control for outcomes of the reform. When including state-specific trends, the sample should contain many pre-reform observations so as to identify the state-specific time trends before the reform. This is the case in the SOEP sample, which includes sufficient cohorts of parents that were not affected by the reform. *State-specific linear cohort trends*, that is, interactions of state dummies with a linear trend in parent's year of birth, are added as controls in the second specification of each parent-child subsample. Results change only very slightly when state-specific trends are added, indicating that the timing of the reform was not correlated with state-specific trends in children's education.

Because the general trend toward higher education also applies for the children in the sample, a child's educational attainment is expected to be higher the later she or he was born. To capture this secular educational trend, Column (3) of each parent-child subsample includes dummies for the child's year of birth. However, that control variable is problematic if parental schooling affects the timing of childbearing. In particular, mothers with more schooling might give birth later in the life course than mothers with less schooling. Because a child's year of birth is endogenous with respect to parents' schooling, it is unclear whether it should be controlled for. The third specification of each subsample in Table 5 shows that the estimated effect of the ninth grade introduction is insensitive to adding controls for child's year of birth, suggesting that the additional ninth grade did not greatly affect the timing of childbearing.

The results in Table 5 are based on samples that contain all parents even though compulsory schooling reforms increased the years of schooling only of parents with a basic school degree (see Table 3). However, the reforms might have changed the probability of obtaining a basic school degree. Therefore, one should check whether the introduction of the compulsory ninth grade had an impact on individuals' track choice. Since it typically takes 10 years to obtain a middle school certificate, introduction of the compulsory ninth grade in basic school reduced the amount of additional schooling between a middle school degree and a basic school degree from two school years to one school year. Thus, after the reform, basic school students had to invest in only one more year of schooling to obtain a middle school degree instead of a basic school degree. This might have lowered the share of individuals obtaining a basic school degree and increased the share of individuals obtaining a middle school degree.

Table 6, however, presents evidence that the ninth grade introduction did *not* affect track choice. Columns (1) and (2) show the zero effect of the ninth grade introduction based on the large Micro Census dataset. This finding is replicated by the smaller SOEP dataset (see Columns (3) and (4)), which is the main dataset for estimating the intergenerational schooling effects.

Because the compulsory schooling reforms did not affect track choice, one can estimate intergenerational schooling effects for the sample of parents with a basic school degree. Table 7 presents the results of the same linear probability models as in Table 5, but now restricted to parents with a basic school degree.³⁵ The positive effect of mothers' schooling is now much stronger than for the full sample: sons of mothers who were subject to a compulsory ninth grade are more likely to obtain a higher school degree than sons of mothers who were not subject to a compulsory ninth grade (see Columns (1) to (3)). As for the full sample, mothers' education seems to have no causal effect on their daughters' education. Again, fathers' schooling has no effect on their children's educational attainment. As in the specifications for the full sample, adding state-specific cohort trends and child's year of birth dummies as control variables changes the coefficient on the reform dummy only slightly.

Although IV estimates cannot be computed with the SOEP data, one can combine the reduced-form results with the first-stage results of the QaC dataset to construct two-sample instrumental variables estimates (TSIV; see Angrist and Krueger, 1992). The TSIV coefficient is obtained by dividing the reduced-form coefficient by the first-stage coefficient of Table 3. The standard error is derived using the delta method as in Pischke and von Wachter (2008, footnote 9). For the mothers-sons subsample, and controlling for state-specific cohort trends, the TSIV coefficient estimate is 0.33 ($= 0.136/0.408$), with a standard error of 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. Though the point estimate is quite large, note that the effect is not very precisely estimated. Nevertheless, one can rule out that the causal effect of mothers' schooling is zero.

To compare the magnitude of this effect with findings from similar studies, outcomes need to be made comparable. To convert the outcome variable "child obtains at least a middle school degree" into years of schooling, one must look at the distribution of educational attainment. For example, 56.5 percent of the sons of mothers with a basic school degree

³⁵To be precise, the subsamples *mothers-sons* and *mothers-daughters* contain only mothers who obtained a basic school degree. The spouses of those mothers might have obtained another school degree (similarly for the fathers-children subsamples).

obtain a middle school or higher school degree. Of those sons, 51.8 percent obtain a middle school degree, 15.9 percent a technical school degree, and 32.4 percent a high school degree. Thus, the average number of years of schooling for sons with at least a middle school degree is 11.3 years ($= 10 * 0.518 + 12 * 0.159 + 13 * 0.324$). Hence, the difference in the average number of years of schooling between sons with a basic school degree and sons with a higher school degree is 2.3 years. This means that an effect size of 33 percentage points is roughly equivalent to 0.76 more years of schooling ($= 0.33 * 2.3$). This effect is much larger than those found for similar school reforms in other countries where the intergenerational effect estimates are concentrated around 0.10 years of schooling (see, for example, Black et al. (2005b) for Norway and Holmlund et al. (2010) for Sweden). This discrepancy might be due to differences in the school systems. In general, German results are difficult to compare with the results from other countries, where all students often attend the same school until the 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 a teacher recommendation for a higher secondary school track.³⁶ Since obtaining a basic school degree takes only nine years (including primary school) and a high school degree typically requires 13 years, effects measured in terms of years of schooling can easily become quite large in Germany.

In sum, the results suggest that while parents' and children's education are strongly correlated, a causal relationship seems to exist only between mothers' and sons' education. In particular, fathers' schooling is likely to have no causal effect on their children's education. It is striking that the pattern of intergenerational education effects for Germany is remarkably similar to those found for other countries that experienced similar compulsory schooling reforms (see Holmlund et al. (2010), Table 1, for an overview of existing studies). A notable difference here from the findings of other studies is that the effect of mothers' schooling is much stronger than found for other countries. One possible explanation could be that the German school system is substantially different from systems in other countries. Early school tracking in Germany means that even small improvements in academic performance in primary school can lead to a more academic secondary school track.

³⁶Students in the state of Bavaria, for example, need a grade point average (GPA) of 2.33 or better in the major subjects 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).

5.3 Robustness Checks

This section first presents a falsification exercise that corroborates the notion that the results for basic school track parents can be interpreted causally. Furthermore, robustness tests show that the results are unaffected by excluding children who have not yet finished their schooling, and that the results are robust to excluding parental cohorts that obtained less education due to World War II.

The compulsory schooling reforms exploited in this study as a source of exogenous variation in parents' schooling are expected to have affected only those individuals attending a basic school. This view is supported by evidence that introduction of the compulsory ninth grade affected neither the length of schooling of individuals in higher school tracks (see Table 3) nor the probability of attending the basic school track (see Table 6). Therefore, the compulsory schooling reforms should also not have any impact on the education of children whose parents obtained either a middle school, a technical school, or a high school degree.

The results of this falsification test are presented in Table 8. The positive effect of the additional ninth grade in the mothers-sons sample for mothers with a higher school degree completely disappears. The coefficient is close to zero and even slightly negative. As expected, none of the coefficients on the ninth grade dummy is positive and statistically significant in the subsamples of parents with a higher school degree.³⁷ These findings corroborate the interpretation that the positive coefficient on the ninth grade dummy for mothers with a basic school degree does indeed capture a causal effect of mothers' education on that of their sons and does not just reflect other factors that are correlated with both children's education and the ninth grade introduction.

Another issue arises from the fact that in the sample 15.3 percent of the children who have parents with a basic school degree have not yet finished their schooling. For these children, the currently attended secondary school type is used as a proxy. Although in practice mobility between different secondary school tracks is rare, a concern could arise over whether the results are biased due to discrepancies between the type of secondary school attended and the final school degree obtained. For example, students who successfully completed basic school might attend a (vocational) school afterward and obtain a middle or high school degree. If the probability of obtaining another (higher) school degree after finishing secondary school is correlated with the reform status of the parents, the coefficient

³⁷Only two (negative) coefficients are marginally significant at the 10 percent level in the fathers-sons subsample.

on the reform dummy could be biased. To avoid biased coefficients due to uncompleted schooling, a robustness test excludes all children who were younger than 23 years at the time the information on the school degree was reported. This is based on the assumption that by age 23, almost all students have obtained their final secondary school degree.

Table 9 shows that the basic results do not change if children younger than 23 years are excluded from the sample. As before, maternal education has a positive and significant effect on the education of sons. Although the coefficient is somewhat smaller in the specification with state-specific cohort trends, the difference from the respective coefficient in Table 7 is only half a standard error. The coefficient on the ninth grade dummy is also positive in the fathers-sons sample, but statistically insignificant. Due to large standard errors, the coefficients for the mothers-sons and fathers-sons samples are not statistically different. Again, the results suggest that there is no intergenerational schooling effect of parents on daughters.

Research shows that Germans born in the 1930s, who were about 10 years old during World War II, received less education compared to earlier and later birth cohorts (Ichino and Winter-Ebmer, 2004). The authors also document that these cohorts experienced a sizeable earnings loss about 40 years after the war, which they attribute to the human capital loss caused by the war. Therefore, a further robustness check excludes all parents who were born between 1930 and 1939. Table 10 shows that the estimates remain qualitatively unchanged, indicating a causal effect only from mothers to sons. The coefficient on the ninth grade dummy in the father-daughter sample is again negative, but statistically insignificant.

In sum, the absence of any effect of the ninth grade introduction on the schooling of children whose parents should not have been affected by the reform supports the notion that the positive coefficient in the sample of mothers with a basic school degree can be interpreted causally. Furthermore, the results are robust to excluding children who have not yet finished their schooling and are robust to excluding parents who received less education due to World War II.

6 Transmission Channels

The results of the previous section indicate that mothers with more schooling tend to have sons with higher educational attainment. This section investigates potential transmission channels that might explain how parental education is passed on to the next generation.

Recent work points out that child health might be one important pathway between parental and child education (see Currie (2009) for an overview). There is also an expanding body of literature that attempts 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.³⁸ Furthermore, recent research points to parental investment as an important determinant of a child's cognitive and noncognitive development and, thus, for later educational and labor market success (see, for example Cunha and Heckman, 2008; Cinnirella et al., 2010).

Because the previous section revealed positive intergenerational effects for mothers' schooling only, the following investigations of transmission channels focus on women. First, it is determined whether women with more schooling have a higher probability of obtaining a vocational degree after secondary schooling. Then, it is investigated whether mothers with more schooling have other labor market outcomes that might be beneficial to their child's education. Another possible mechanism is assortative mating, that is, women with more schooling marry men with higher educational attainment, who, in turn, pass on their abilities to the next generation and may also foster the child's development through better parenting skills. Also, mothers with more schooling might decide to have fewer, but better educated, children. Another potentially important channel—and one, that to my knowledge, has not been investigated to date—is that schooling might raise parents' valuation of their children's education, which might induce them to put more pressure on their children to achieve better academic results. Finally, some descriptive evidence is presented on why mothers' education might be more beneficial to sons' than to daughters' education.³⁹

6.1 Post-Secondary Education

One transmission channel that might mediate maternal schooling is increased participation of women in post-secondary education, which might increase their cognitive abilities and/or their valuation of education. A woman with more schooling might be more likely to obtain a vocational degree after finishing secondary school if the additional schooling (i.e., the compulsory ninth grade) increased her cognitive abilities or educational aspirations. Because

³⁸See Almond and Currie (2011) for a review of this topic. Carneiro et al. (2007), for example, investigate several investments mothers can make when children are young. They find that mothers with more schooling read more to their sons aged 7-8 and that children of these mothers are more likely to own a computer at ages 12-14.

³⁹Although health might be an important channel, I am not able to investigate it due to missing information on children's health when they were young.

women are much less likely than men to complete a vocational training—which typically lasts between two and three years in Germany—this transmission channel most likely affects only women, not men.⁴⁰

Based on the large Micro Census, Columns (1) to (4) in Table 11 show that women with one year more of schooling are about 1.8 percentage points more likely to obtain a vocational training degree.⁴¹ Compared to the baseline probability that 57 percent of all women with a basic school degree acquire a vocational training degree, this amounts to a small effect of only 3.1 percent. Thus, the extension of compulsory schooling from eight to nine years induced only some women to participate in vocational training.⁴² In contrast to women, the vast majority of men obtain a vocational degree. Among men with a basic school degree who are at least 29 years old and who were born between 1930 and 1960, only 16.7 percent had not completed vocational training. Because almost all men with a basic school degree obtain a vocational degree, the extension of compulsory schooling from eight to nine years did not affect their probability of obtaining a vocational training degree (results not shown).

Since the additional ninth grade increased women’s likelihood of obtaining a vocational degree only marginally, it is not expected that including a dummy for whether the mother acquired a vocational degree will change the coefficient on the ninth grade introduction in the mothers-children samples. This presumption is confirmed in Columns (5) to (8) in Table 11, which are based on the same mother-child subsamples as employed in the previous section. Compared to the basic results for mothers with a basic school degree in Table 7, the coefficient on the ninth grade dummy remains unchanged in both the mothers-sons and the mothers-daughters samples. These results and the finding that the additional year of school induced only few women to acquire a vocational degree suggests that increased participation in post-secondary schooling is an unlikely explanation for the positive effect of mothers’ education on that of their sons.

⁴⁰Several potential transmission channels are investigated with the Micro Census because it is much larger than the SOEP. A small drawback of the Micro Census is that a woman can be identified as a mother only if her child lives in the same household. Therefore, results are presented for women who have married at some point in time, which is a good proxy for a woman being a mother since statistics based on the Micro Census show that almost 86 percent of all married women aged 30 to 40 have at least one child. Further regression results are based on a subsample of prime-aged women who live with at least one child.

⁴¹To ensure that all women have completed their vocational training, the sample is restricted to women aged 21 and older. Note that individuals typically finish basic school at age 16. Thus, most individuals have finished a two- or three-year vocational training before age 21. Results are very similar if younger women, for example, aged 21-28, are also excluded from the sample.

⁴²Almost all women with a basic school degree and a vocational degree (97 percent) received their vocational degree at a vocational school. Therefore, it is fruitless to differentiate between different types of vocational degrees.

6.2 Labor Market Outcomes

Another mechanism that might mediate the positive impact of maternal schooling is that more schooling leads to more resources that can be used to invest in children's education. The idea behind this argument is that parents with budget constraints have to trade off their own consumption against investments in their children (Becker and Tomes, 1986). Hence, wealth-constrained parents—which are likely parents with low education—may underinvest in their children's education, whereas parents who are not budget-constrained will not underinvest.

Due to changed preferences or higher cognitive abilities, women with more schooling might be more likely to participate in the labor market. Working (more hours), in turn, could have either a beneficial or a detrimental effect on the child's education. On the one hand, women who are working have less time for their children, for example, less time to help them with their homework or to engage in school-related activities.⁴³ On the other hand, women who work might further increase their cognitive abilities at the workplace or develop higher educational aspirations for their children because they are more likely to personally experience the importance of education for labor market success.

Table 12 indicates that mothers with more schooling have the same probability of being employed than mothers with less schooling (see Column (1)). All results in Table 12 are based on prime-age women (25-40 years old) who have a child, exactly the group of women who need to take care of their kindergarten- and/or school-age children. Conditional on being employed, women with more schooling also work about the same number of hours per week as women with less schooling (Column (2)). Thus, differential labor market participation cannot explain the positive effect of mothers' schooling on their sons' education.⁴⁴

Finally, even if more schooling affects neither the probability nor the intensity of labor market participation, it might affect earnings through increased productivity. To investigate the impact on earnings, the sample is restricted to women with regular employment who work at least 20 hours per week and whose predominant source of income comes from employment. Column (3) indicates that women with more schooling do not have higher labor market income. Similarly, there are no labor market effects for men: men with more schooling are not more likely to be employed, work more hours, or have higher earnings (results not

⁴³Interestingly, time use data indicate that more educated mothers spend more time with their children (Guryan et al., 2008).

⁴⁴In contrast, Carneiro et al. (2007), using other sources of exogenous variation in mothers' education, find for the United States that mothers with more schooling work more hours in the year the child is born than do mothers with less schooling.

shown). These findings are in line with Pischke and von Wachter (2008), who estimate the effect of the ninth grade introduction on labor market outcomes jointly for women and men.

In sum, extending compulsory schooling by one year did not result in increased labor market participation or higher earnings for women (or men). Thus, the positive causal effect of mothers' education on that of their sons cannot be attributed to changed labor market behavior or higher earnings that might make parents less credit-constrained.⁴⁵

6.3 Assortative Mating

Assortative mating, that is, the positive correlation between spouses' education, plays an important role in the low intergenerational income mobility in Germany, where permanent family income is highly persistent across generations (see Ermisch et al., 2006).⁴⁶ Yet, it is not clear whether the father's education should be included as a control variable in a model that regresses child's education on mother's education. Without controlling for the father's education, the estimated effect of the mother's schooling captures both the direct impact of the mother and the indirect impact of the father. Mothers with one more year of schooling, however, might have better educated partners (assortative mating). If this is the case, women with more schooling might have sons with higher educational attainment because their better educated partner might have genetically passed on superior ability and/or has had a non-genetic influence such as a stronger preference for education or better child-rearing skills. On the other hand, if the father's education is included in the model, the estimated effect captures the effect of an increase in mother's schooling on the education of her child, netting out any effects of the father's education.

Before analyzing the impact of assortative mating in the intergenerational model directly, I first investigate whether women with more schooling have better educated partners. For this analysis, the Micro Census sample is restricted to women with a basic school degree who cohabit with a male partner.⁴⁷ Based on both the Micro Census and the SOEP, the results in Table 13 suggest that there are no assortative mating effects among women with a

⁴⁵Schooling might not only affect earnings or employment probability, but might also lead to non-pecuniary benefits in the labor market, such as better working conditions, higher occupational prestige, and higher job satisfaction (Oreopoulos and Salvanes, 2011).

⁴⁶Ermisch et al. report that the correlation between partners' educational attainment is about 0.52 in Germany (among individuals with any type of school degree), with the two partners of almost 50 percent of German couples having the same educational level.

⁴⁷This restriction yields a sample of women, almost all of whom either are or have been married. While about 70 percent of the women in this sample have at least one child living in the same household, even 90 percent of women aged 35-40 have at least one child.

basic school degree. Columns (1) to (4) present coefficients and standard errors from ordered probit models with father’s school degree as a multi-valued outcome variable. All coefficients on the dummy indicating whether a woman was affected by the compulsory schooling reform are close to zero.⁴⁸ Although there are strong assortative mating effects when looking at all individuals (see Ermisch et al., 2006), the coefficients close to zero indicate that women with nine years of schooling do not have more educated partners than women with only eight years of schooling.⁴⁹

Finally, the male partner’s school degree is included in the specification to test whether his education changes the effect of a mother’s education on her child’s schooling. The same mothers-children samples are used as before and now include dummies for father’s school degree as additional control variables. Compared to the baseline results in Table 7, the positive coefficients for the mothers-sons subsample are only slightly smaller (half a standard error) when controlling for partner’s education (Columns (5) and (6)), a difference that is not statistically significant. The coefficients on the ninth grade are basically the same for the mothers-daughters subsample (Columns (7) and (8)).⁵⁰ In sum, assortative mating effects cannot explain the causal effect of mothers’ schooling on their sons’ education.

6.4 Fertility

The fertility decision is another transmission channel with potential to explain the effect of mothers’ education on that of their children. Theory suggests that family size may matter for children’s education if there is a quantity-quality tradeoff between the number of children and the investment per child due to budget constraints (Becker and Lewis, 1973; Becker and Tomes, 1976). Mothers with fewer children, *ceteris paribus*, have more time and more resources to invest in each child, for example, by attending cultural events together or helping with homework and other school-related projects. Empirical evidence does indeed show that women with more schooling have a preference for fewer children (see Jones and Tertilt, 2009).

Since the German General Social Survey (ALLBUS) is the only dataset that contains reliable information on the number of children a woman has borne, it is used to investigate the effect of an additional year of schooling on fertility. Table 14 presents results for both

⁴⁸Using ordinary least squares instead of ordered probit models yields very similar results.

⁴⁹Using a dummy variable indicating whether the partner achieved at least a middle school degree instead of the multi-valued school degree as the outcome also yields coefficients that are close to zero. Furthermore, women with more schooling are not more likely to have a male partner (either married or cohabiting).

⁵⁰The samples in Columns (5) to (8) are marginally smaller than the corresponding samples in Table 7 because the male partner’s school degree is missing for a few children.

women and men, separately for individuals with a basic school degree and individuals with a higher school degree. The samples include individuals born between 1930 and 1960 who live in West Germany and are age 40 or older, since these individuals are most likely past their child-rearing years.

The results suggest that lower educated women tend to have fewer children if they received more schooling (Columns (1) and (2)). The coefficient is not statistically significant in the specification without controls for state-specific cohort trends, but does become larger in magnitude and significant when state-specific cohort trends are added. Although sample sizes are not large, the coefficients in both specifications are in the same direction. Again, to obtain a two-sample IV estimate, the reduced-form coefficient must be divided by the first-stage coefficient, which yields an effect size between 0.46 and 0.83 fewer children for one additional year of schooling (albeit with sizeable standard errors). This is a non-negligible effect since the average number of children is 2.0 and the standard deviation is 1.3 children among women with a basic school degree age 40 or older. For men with a basic school degree, schooling has no effect on fertility. As expected, there is no evidence that the ninth grade introduction affected the number of children of individuals with a higher school degree. The coefficient is zero for women in the specification with state-specific cohort trends (Column (4)); however, it is slightly positive, but statistically insignificant, for men (Column (8)).⁵¹

Even though the results indicate that women with more schooling have fewer children, it is not clear whether this explains the positive effect of mothers' schooling because the evidence on the causal effect of child quantity on child quality is mixed. For example, Rosenzweig and Wolpin (1980) use multiple births as an exogenous increase in family size in India and show that, on average, larger families have children with lower education. On the other hand, Angrist et al. (2005) also use multiple births and a dummy for same-sex sibling pairs in Israeli families with two or more children to instrument for the number of children and find that an exogenous increase in family size at second and higher births seems to have little effect on the educational attainment of first- and second-born children. Similarly, Black et al. (2005a) use Norwegian administrative data and the multiple birth instrument and find no effects on child's schooling or earnings. Given the ambiguous evidence on the causal effect

⁵¹Unfortunately, information on mothers' age at first birth is available only in very few ALLBUS waves. Thus, estimates of the effects on mother age at first birth are not presented. If anything, the point estimates suggest that mothers with more schooling give birth slightly earlier in the life course than their less-schooled peers. However, standard errors are quite large for these smaller subsamples, such that these results must be viewed with caution. In contrast, Carneiro et al. (2007) find that women with more schooling delay childbearing, but they find no effect on the number of children.

of the number of children on children's education, it is not clear whether smaller family size helped increase the educational attainment of sons in Germany. Due to missing information, it is not possible to control for family size directly in the SOEP dataset.⁵²

6.5 Parents' Valuation of Children's Education

Schooling may also change individuals' preferences and attitudes, for example, by providing more information on the benefits of education.⁵³ Björklund and Salvanes (2011) state 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 means that even if more schooling does not increase the (child-rearing) abilities of parents, more schooling might help improve the educational success of their children if more schooling raises their awareness that success in school is important for the child's future. This view implies that in addition to increased human capital, it is the parents' awareness or valuation of the child's education that matters for intergenerational education effects.

In 1980 and 1982, the German General Social Survey (ALLBUS) participants were asked to indicate the importance of several traits and abilities that can be fostered by appropriate parenting. Specifically, participants were asked: "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 range from 1 (not important) to 7 (very important). The two traits most interesting for this study concern *good school performance* and *studiousness* of the child.⁵⁴

The results in Table 15 suggest that the introduction of the compulsory ninth grade led to individuals more highly valuing good school performance by their child. This result is robust to including state-specific cohort trends (see Columns (1) and (2)). All specifications also include state of residence, year of birth, and survey year dummies, as well as a quartic

⁵²Research shows that the birth order of children also affects educational outcomes, with the first-born children completing more years of schooling on average (see, for example, Blake, 1989; Black et al., 2005a). Although birth order is not observed for most children in the SOEP, it is unlikely that birth order is systematically correlated with parents' reform status and, thus, ignoring birth order is unlikely to bias the coefficient of interest.

⁵³Using the U.S. General Social Survey, Oreopoulos and Salvanes (2011), for example, present evidence that individuals with more schooling are more patient.

⁵⁴The other traits include, for example, the importance of manners, sympathy for other people, ability to respond positively to criticism, and self-confidence.

in age and a male dummy.⁵⁵ The magnitude of the reduced-form coefficient is economically significant since 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. The respective coefficients are slightly negative, but statistically insignificant.

In addition to the positive effect of the additional school year on the valuation of children's school performance, more schooling seems also to have increased the importance to an individual that his or her child is studious (Columns (5) and (6)). Although standard errors are somewhat large and 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 Carneiro et al. (2007), who find that in the United States, mothers with more schooling are more likely to believe that their seven-year-old child will later attend college.

In sum, more schooling seems to increase individuals' valuation of aspects likely to be important for their children's educational success: good school performance and studiousness. As suggested in other work, this change in parents' valuation might be one important channel through which mothers' education affects their sons' education.⁵⁶

6.6 Differential Effects of Mothers' Schooling on Sons' and Daughters' Education

The finding that a mother's schooling affects the education of a son but not that of a daughter is puzzling. 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? The finding that a mother's education has a stronger effect on the

⁵⁵Because sample sizes are limited, women and men are pooled in all specifications.

⁵⁶Note that good school performance and studiousness, respectively, might have different effects on educational attainment, depending on the educational stage at which the child improves his or her academic performance. If the child performs better in primary school, he or she is more likely to attend a higher secondary school track. If the child improves academic performance in secondary school, it might result either only in better grades, or the better school grades might increase the probability that the child switches to a more academic secondary school (although this is not very likely given the low upward mobility; see Jürges and Schneider (2007)).

schooling of her son than of her daughter is not unique to Germany, but is also reported for Norway (Black et al., 2005b).⁵⁷

To shed some light on the gender-specific findings for Germany, I look at individuals' aspirations for their child's education. The 1986 survey of the German General Social Survey (ALLBUS) contains information on individuals' educational aspirations for both daughters and sons. The relevant question is: "Suppose you had a 10-year-old daughter (son). Which type of degree should she (he) obtain in your opinion?" The answer categories include both school and college degrees: 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 degree from a polytechnic (*Fachhochschule*) or university (5). As before, the samples are restricted to individuals born between 1930 and 1960 who live in West Germany.⁵⁸

Table 16 shows that women with a basic school degree are more likely to have higher educational aspirations for sons than for daughters compared to other individuals. Column (1) reveals that 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. The positive but 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 or compared to men. Looking at 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 with a basic school degree (Column (3)); and 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)). Note, however, that the coefficients are statistically insignificant due to the small sample sizes.

Descriptive statistics reveal that the higher educational preference in regard to sons, as compared to that desired for daughters, is most pronounced among women with a basic school degree. While 27.3 percent of these women desire a higher educational attainment for their

⁵⁷In contrast, Chevalier (2004) finds that the effect of maternal education is stronger for daughters and that paternal education matters for sons' education only, suggesting that role models by same-sex parents might explain this pattern.

⁵⁸Because 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, about 80 percent of the women and 70 percent of the men born between 1930 and 1960 have children in the survey year 1986. Thus, a large majority of the 1986 survey participants are parents.

son than for their daughter (70.6 percent have equal aspirations for son and daughter), only 16.6 percent of women with a higher school degree desire a higher educational attainment for their son than for their daughter (82.0 percent have equal aspirations). Thus, the preference for son's relative to daughter's educational success is considerably stronger among individuals with a basic school degree.⁵⁹ As mentioned above, Carneiro et al. (2007) find for the United States that the more highly educated the mother of a child aged 7-8, the more likely she is to believe that her child will go to college. Interestingly, and consistent with the gender-specific patterns detected in this study, this effect is larger for sons than for daughters (though not statistically different). Furthermore, the authors find that the higher the mother's cognitive ability (as measured by her score on the Armed Forces Qualification Test), the larger this effect.

In sum, this descriptive evidence may partially explain why mothers' schooling has differential effects on sons' and daughters' education. That is, if the additional year of school increased women's child-rearing skills and if women (due, perhaps, to the paternalistic culture they were raised in) have higher educational aspirations for their sons compared to their daughters, then the mothers' increased child-rearing skills will benefit their sons more than their daughters. Whether this explanation is true of other countries will depend on mothers' relative preference for sons' versus daughters' educational attainment in the respective society.

7 Conclusion

This study estimates 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 in basic schools from eight to nine years that was implemented in all West German states at various times between 1946 and 1969. Even though these compulsory schooling reforms seem to have had no effects on earnings or political behavior (see Pischke and von Wachter (2008) and Siedler (2010)), I find that an additional year of schooling of women strongly affects 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

⁵⁹In separate regressions (not shown), the effect of the ninth grade introduction on educational aspirations is small and insignificant, meaning that the additional year of school did not affect aspirations concerning the preference for son's versus daughter's education.

countries, but the effect of maternal schooling on sons' education appears to be much stronger in Germany. This could be because the German school system is significantly different than the systems in other countries. Specifically, and most importantly, the rigid and early school tracking process in Germany that occurs when children are about age 10 (see OECD, 2004) might result in stronger intergenerational schooling effects because a slight improvement in the child's school performance in primary school can easily lead to his or her attendance at a more academic secondary school, and thus to higher educational attainment.

As to transmission channels, there is evidence for one interesting mechanism that might partially mediate the strong positive effect of mothers' education: parents with more schooling more highly value good school performance by their children 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 classically discussed channels, such as better labor market outcomes (in line with the findings of Pischke and von Wachter (2008)) or assortative mating, explain the strong effect of maternal schooling. Although results suggest a negative effect of schooling on fertility, it is not clear whether this channel can explain the intergenerational transmission of education because the dataset does not allow directly controlling for family size. Consistent with the differential effects of mothers' education on that of their sons and daughters, descriptive evidence indicates that lower educated women have higher aspirations for their sons than for their daughters in the matter of education. If more schooling increases women's (unobserved) child-rearing abilities, then sons are more likely to benefit from these additional abilities than are daughters.

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

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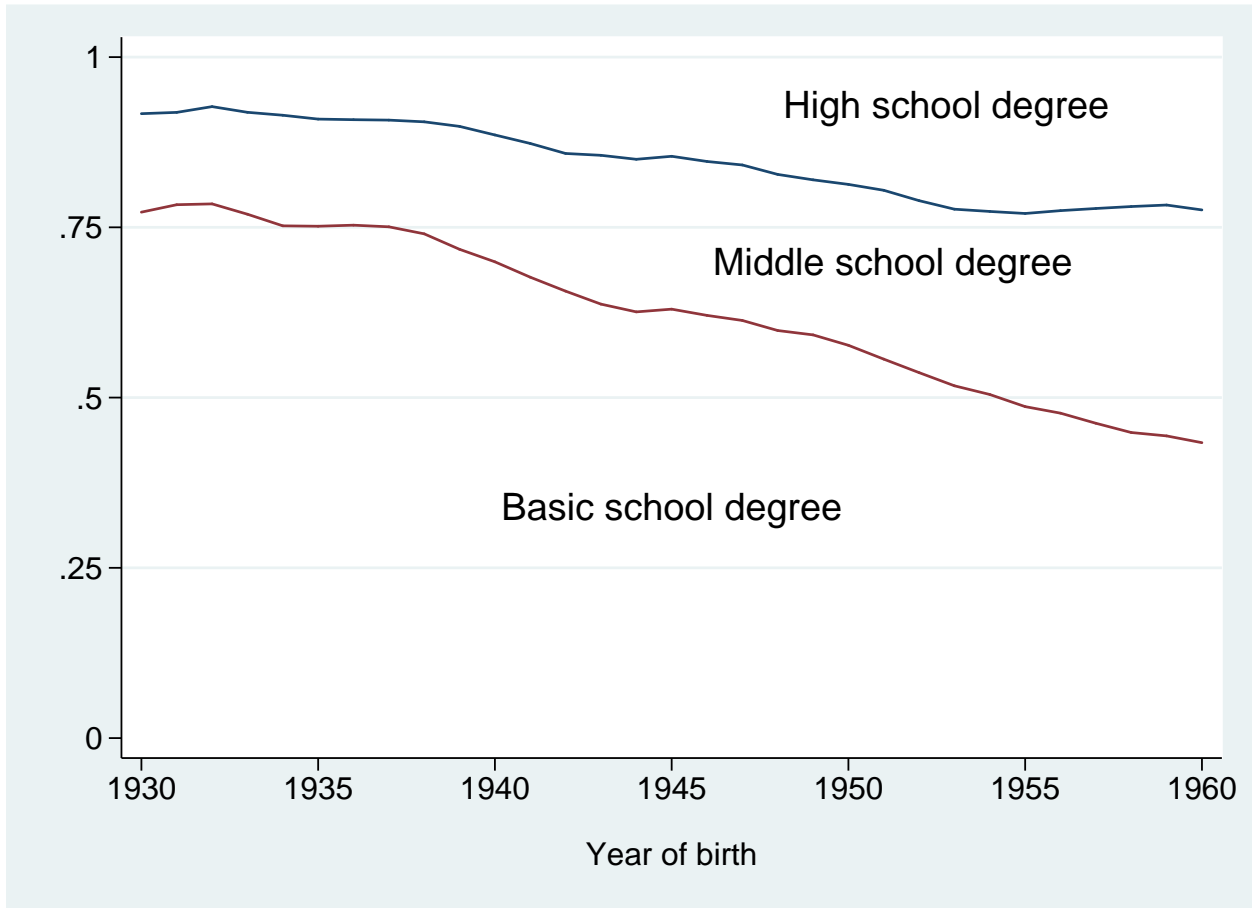
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Figures and Tables

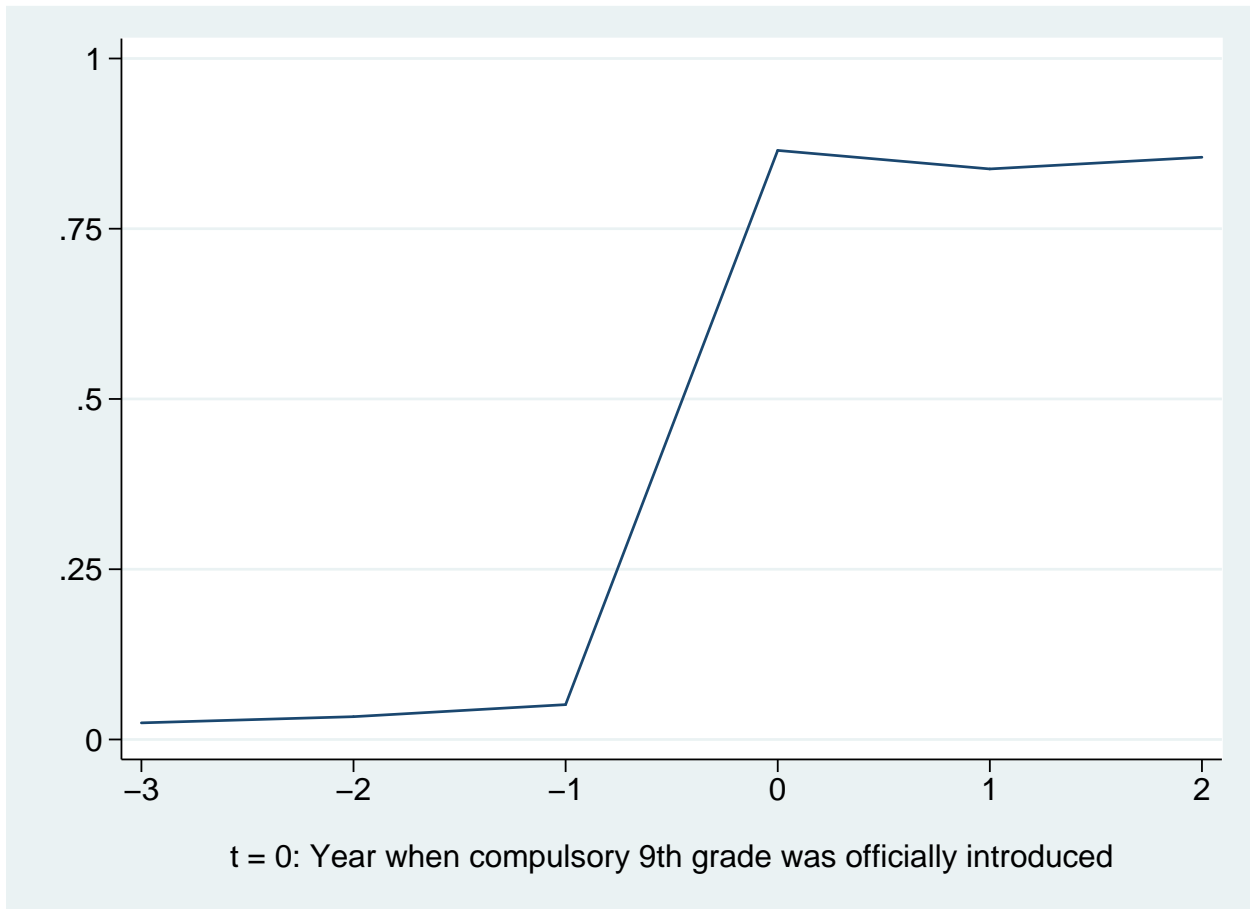
Figure 1
Trend toward Higher Education after World War II



Notes: figure plots shares of individuals with degrees from basic, middle, and high school, respectively. *High school degree* contains individuals with either a high school degree or a technical school degree. Shares are three-year moving averages.

Source: Qualification and Career Survey (QaC); own calculations.

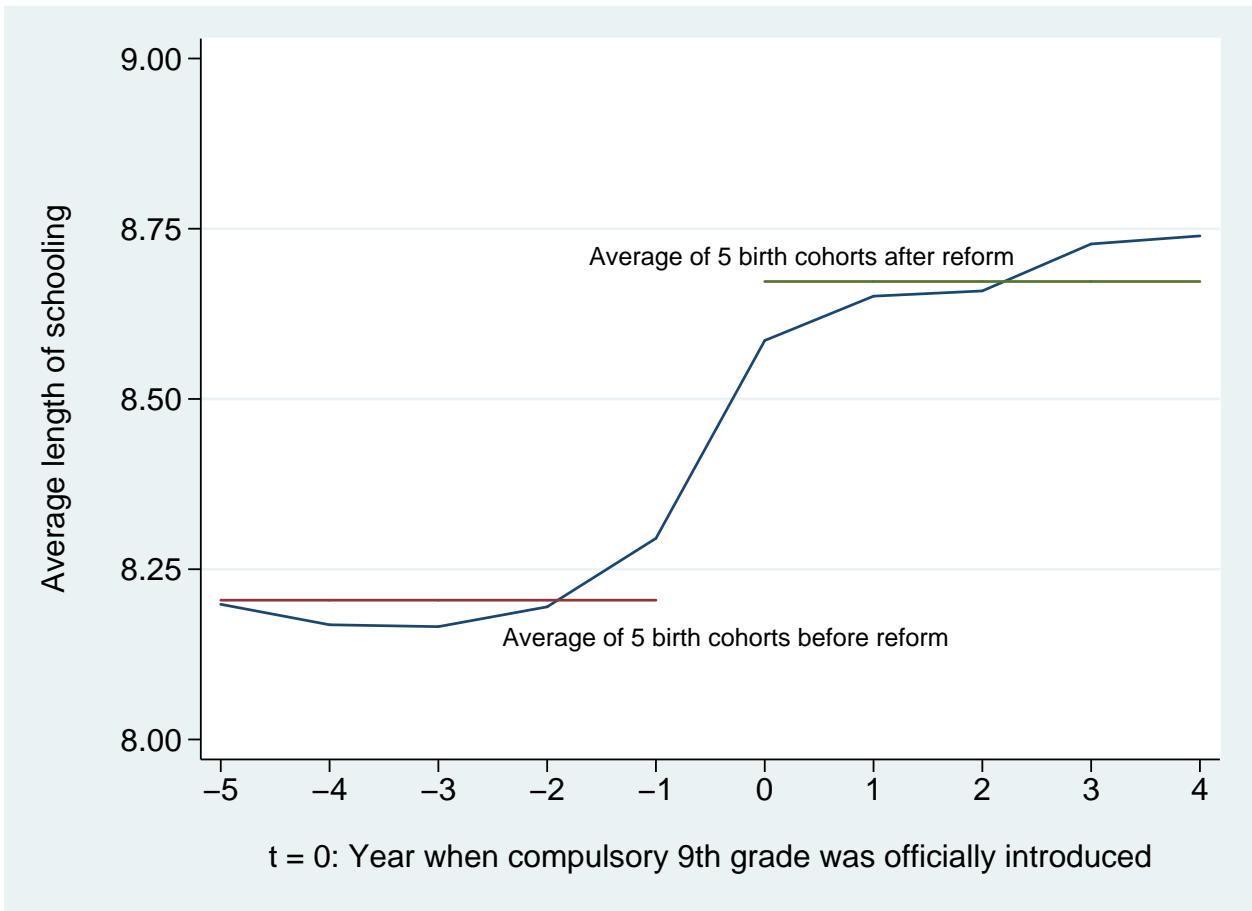
Figure 2
Share of Basic School Students Attending the 9th Grade



Notes: shares equal the number of basic school students attending the ninth grade in school year t divided by the number of basic school students attending the eighth grade in school year $t - 1$. Figure plots student-population-weighted average shares of the states Baden-Württemberg, Bavaria, Bremen, North Rhine-Westphalia, Lower Saxony, and Rhineland-Palatinate. The year of the official ninth grade introduction is aligned at $t = 0$ for all states.

Source: Federal Statistical Office (1957) and respective volumes of the following years.

Figure 3
Length of Schooling of Individuals with Basic School Degree



Notes: length of schooling equals length of primary plus secondary schooling; imputed as graduation year minus year of birth minus 6.5. Figure plots average length of schooling for individuals of German nationality with a basic school degree who were born between 1930 and 1960 and live in West Germany.

Source: Qualification and Career Survey (QaC); own calculations.

Table 1
Introduction Dates of Compulsory 9th Grade in Basic Schools

State	First year with compulsory 9th grade	First birth cohort with compulsory 9th grade
Hamburg	1946	1931
Schleswig-Holstein	1947	1932
Saarland	1958	1943
Bremen	1959	1944
Lower Saxony	1962	1947
North Rhine-Westphalia	1967	1953
Hessen	1967	1953
Rhineland-Palatinate	1967	1953
Baden-Württemberg	1967	1953
Bavaria	1969	1955

Notes: in Hessen, the compulsory ninth grade was gradually introduced between 1963 and 1967. The respective birth cohorts are excluded from the analysis.

Source: Leschinsky and Roeder (1980), p.332ff.; Secretariat of the Standing Conference of the Ministers of Education (Kultusministerkonferenz).

Table 2
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)
Reform status	0	1	0	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

Notes: means reported for all variables; standard deviations of continuous variables in parentheses. *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 (analogously *Fathers-Children* samples). Each parent has at least one child born in 1999 or earlier. *Reform* samples contain individuals who went to school when schooling laws mandated nine years of compulsory schooling. Individuals in the *non-Reform* samples were required to attend only eight years of school. ^a Child's age in the survey year in which the child reported his or her school degree or type of secondary school attended, respectively.
Data: German Socio-Economic Panel (SOEP).

Table 3
Effect of 9th Grade Introduction on Length of Schooling

	Full sample	Basic school track	Middle & high school track
	(1)	(2)	(3)
Dummy for 9th grade 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

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. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Data: Qualification and Career Survey (QaC).

Table 4
OLS Results: Relationship Between Children's and Parents' Schooling

	Mothers-Sons (1)	Mothers-Daughters (2)	Fathers-Sons (3)	Fathers-Daughters (4)
Parent's length of schooling	0.542*** (0.026)	0.498*** (0.026)	0.518*** (0.018)	0.448*** (0.020)
State dummies	Yes	Yes	Yes	Yes
Year of birth dummies (parent)	Yes	Yes	Yes	Yes
Year of birth dummies (child)	Yes	Yes	Yes	Yes
Number of children	3,563	3,660	3,139	3,260

Notes: Dependent variable: child's length of schooling; imputed as number of years of primary plus secondary schooling typically required for the secondary school degree obtained: no school degree = 7 years; basic school degree = 9 years; middle school degree = 10 years; technical school degree = 12 years; high school degree = 13 years. Parents' length of schooling is constructed in the same manner. Ordinary least squares regressions. Robust standard errors clustered at the state \times parent's year of birth level in parentheses. Samples contain parents born between 1930 and 1960, living in the 10 West German states. Samples include only children who reported their school degree when age 22 or older. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Data: German Socio-Economic Panel (SOEP).

Table 5
Reduced-Form Results: Full Sample

	Mothers-Sons			Mothers-Daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for 9th grade 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 9th grade 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

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. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: German Socio-Economic Panel (SOEP).

Table 6
Effect of 9th Grade Introduction on Probability of Obtaining Basic School Degree

	Micro Census		SOEP	
	(1)	(2)	(3)	(4)
Dummy for 9th grade in basic school	-0.009 (0.006)	0.004 (0.005)	-0.021 (0.024)	-0.027 (0.026)
Female	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes	Yes
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	1,108,637	1,108,637	18,321	18,321

Notes: Dependent variable: individual obtains a basic school degree (= 1) or a higher school degree (= 0). Ordinary least squares regressions. Robust standard errors clustered at the state \times year of birth level in parentheses. Samples contain individuals born between 1930 and 1960, living in the 10 West German states. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: Micro Census and German Socio-Economic Panel (SOEP).

Table 7
Reduced-Form and Two-Sample IV Results: Only Parents with Basic School Degree

	Mothers-Sons			Mothers-Daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for 9th grade 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 9th grade 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 9th grade 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 9th grade 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)

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 with basic school degree born between 1930 and 1960, living in the 10 West German states. See text for the computation of the two-sample instrumental variables estimates. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: German Socio-Economic Panel (SOEP) and Qualification and Career Survey (QaC).

Table 8
Falsification Test: Only Parents with Higher School Degree

	Mothers-Sons			Mothers-Daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for 9th grade in basic school	-0.027 (0.035)	-0.018 (0.043)	-0.007 (0.046)	0.014 (0.027)	-0.003 (0.029)	-0.006 (0.029)
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	1,666	1,666	1,666	1,569	1,569	1,569
<hr/>						
	Fathers-Sons			Fathers-Daughters		
	(7)	(8)	(9)	(10)	(11)	(12)
Dummy for 9th grade in basic school	-0.063* (0.035)	-0.068* (0.040)	-0.062 (0.039)	0.019 (0.034)	0.009 (0.039)	0.004 (0.040)
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	1,731	1,731	1,731	1,712	1,712	1,712

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 with middle, technical, or high school degree born between 1930 and 1960, living in the 10 West German states. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Data: German Socio-Economic Panel (SOEP).

Table 9
Robustness Check: Only Children with Completed Schooling

	Mothers-Sons		Mothers-Daughters		Fathers-Sons		Fathers-Daughters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy for 9th grade in basic school	0.151*** (0.054)	0.106* (0.060)	0.054 (0.053)	-0.022 (0.052)	0.064 (0.067)	0.051 (0.083)	0.022 (0.057)	-0.039 (0.049)
State-specific linear cohort trends	No	Yes	No	Yes	No	Yes	No	Yes
Number of children	2,451	2,451	2,564	2,564	2,081	2,081	2,165	2,165

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 with basic school degree born between 1930 and 1960, living in the 10 West German states. Children were at least 23 years old when reporting their school degree. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: German Socio-Economic Panel (SOEP).

Table 10
Robustness Check: Excluding Parents Born during the 1930s

	Mothers-Sons		Mothers-Daughters		Fathers-Sons		Fathers-Daughters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy for 9th grade in basic school	0.124*	0.172***	0.039	0.034	0.025	0.034	-0.049	-0.060
	(0.064)	(0.062)	(0.045)	(0.044)	(0.053)	(0.056)	(0.053)	(0.058)
State-specific linear cohort trends	No	Yes	No	Yes	No	Yes	No	Yes
Number of children	1,622	1,622	1,657	1,657	1,452	1,452	1,405	1,405

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 with basic school degree born between 1940 and 1960, living in the 10 West German states. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Data: German Socio-Economic Panel (SOEP).

Table 11
Effect on Women's Post-Secondary Education

	Dependent variable: Woman obtained vocational degree			
	Married women		Micro Census	
	(1)	(2)	(3)	(4)
Dummy for 9th grade in basic school	0.027*** (0.008)	0.018** (0.007)	0.029*** (0.010)	0.019* (0.010)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	310,298	310,298	218,395	218,395

	Dependent variable: Child obtained at least middle school degree			
	Mothers-Sons		Mothers-Daughters	
	(5)	(6)	(7)	(8)
Dummy for 9th grade in basic school	0.148*** (0.057)	0.146** (0.060)	0.038 (0.045)	0.013 (0.045)
Mother has vocational degree	0.214*** (0.019)	0.215*** (0.019)	0.170*** (0.020)	0.170*** (0.020)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	2,872	2,872	2,986	2,986

Notes: Dependent variable: woman obtained a vocational degree (equals 1 if woman obtained any type of vocational degree; 0 otherwise); 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 year of birth level in parentheses. Samples contain women with a basic school degree born between 1930 and 1960, living in West Germany, who are at least 21 years old at the time of the survey. Models (1) and (2) are restricted to women who have married at some point in time (an indicator for being a mother); Models (3) and (4) contain women aged 21-50 who have a child in their household. Specifications (1)-(4) include the maximal sets of year of birth dummies, state of residence dummies, survey year dummies, and a quartic in age. Specifications (5)-(8) include the maximal sets of mothers' year of birth and state of residence dummies. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: Micro Census and German Socio-Economic Panel (SOEP).

Table 12
Effect on Women's Labor Market Outcomes

	Dependent variable:		
	Employed	Working hours per week	Monthly earnings
	(1)	(2)	(3)
Dummy for 9th grade in basic school	0.006 (0.007)	-0.535 (0.417)	-32.005* (16.818)
State-specific linear cohort trends	Yes	Yes	Yes
Individuals	121,418	50,773	31,233

Notes: Dependent variables: employed equals 1 if the woman has regular or occasional employment, 0 otherwise; working hours per week refers to actual hours worked during the survey week; monthly earnings is based on monthly net income, reported in brackets and comprising all types of income. Each individual is assigned the respective bracket midpoint. Ordinary least squares regressions. Robust standard errors clustered at the state \times year of birth level in parentheses. Samples include prime-age women (25-40 years old) with a basic school degree who were born between 1930 and 1960 and who live with a child in the household in West Germany. In addition, Models (2) and (3) contain only women who are employed, and Model (3) contains only women whose main income source is labor market income and who usually work at least 20 hours per week. All specifications include the maximal sets of year of birth dummies, state of residence dummies, survey year dummies, and a quartic in age. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: Micro Census.

Table 13
Effect on Assortative Mating

	Dependent variable: Father's school degree			
	Micro Census		SOEP	
	(1)	(2)	(3)	(4)
Dummy for 9th grade in basic school	-0.008 (0.021)	-0.008 (0.023)	0.080 (0.104)	-0.001 (0.111)
State-specific linear cohort trends	No	Yes	No	Yes
Number of women	276,361	276,361	4,813	4,813
	Dependent variable: Child obtains at least middle school degree			
	Mothers-Sons		Mothers-Daughters	
	(5)	(6)	(7)	(8)
Dummy for 9th grade in basic school	0.099* (0.058)	0.103* (0.061)	0.047 (0.042)	0.015 (0.041)
Father's education	Yes	Yes	Yes	Yes
State-specific linear cohort trends	No	Yes	No	Yes
Number of children	2,901	2,901	3,035	3,035

Notes: Dependent variables: father's school degree: 1 = basic school degree, 2 = middle school degree, 3 = technical school degree, 4 = high school degree; child obtains at least middle school degree (equals 1 for middle, technical, or high school degree and equals 0 for basic school degree or left school without degree). Specifications (1) and (2) contain women with a basic school degree born between 1930 and 1960, living in West Germany, who have a male partner. Specifications (3)-(8) contain mothers with basic school degree born between 1930 and 1960. Columns (1)-(4) present coefficients and standard errors from ordered probit models. Columns (5)-(8) are estimated with OLS. All specifications include the maximal sets of mother's year of birth dummies and state of residence dummies. Robust standard errors clustered at the state \times women's year of birth level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: Micro Census and German Socio-Economic Panel (SOEP).

Table 14
Effect on Fertility

	Women with basic school degree		Women with higher school degree	
	(1)	(2)	(3)	(4)
Dummy for 9th grade in basic school	-0.185 (0.136)	-0.332** (0.159)	0.076 (0.121)	-0.000 (0.144)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	2,404	2,404	1,572	1,572

	Men with basic school Degree		Men with higher school Degree	
	(5)	(6)	(7)	(8)
Dummy for 9th grade in basic school	-0.140 (0.129)	0.040 (0.122)	0.052 (0.134)	0.073 (0.162)
State-specific linear cohort trends	No	Yes	No	Yes
Individuals	2,237	2,237	1,736	1,736

Notes: Dependent variable: number of children. Ordinary least squares regressions. Robust standard errors clustered at the state \times year of birth level in parentheses. Samples contain individuals born between 1930 and 1960 who are at least 40 years old at the time of the survey. Specifications include the maximal sets of year of birth dummies, state of residence dummies, survey year dummies, and a quartic in age. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: German General Social Survey (ALLBUS), waves 1980, 1982, 1984, 1991, 1992, 2000, 2002, 2004, 2006, and 2008.

Table 15
Effect on Parents' Valuation of Children's Education

	Dependent variable: Good school performance			
	Individuals with basic school degree		Individuals with higher school degree	
	(1)	(2)	(3)	(4)
Dummy for 9th grade 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			
	Individuals with basic school degree		Individuals with higher school degree	
	(5)	(6)	(7)	(8)
Dummy for 9th grade 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

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: "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 \times 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. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data: German General Social Survey (ALLBUS), waves 1980 and 1982.

Table 16
Aspirations for Daughters' and Sons' Education

	All individuals		Individuals with	Females
	(1)	(2)	basic school degree	(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 x 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

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 x year of birth level) from ordered probit models reported. Samples include individuals born between 1930 and 1960, living in West Germany. ALLBUS participants answered two questions in the 1986 survey: "Suppose you had a 10-year-old daughter. Which type of degree should she obtain in your opinion?" and the same question concerning 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) a degree from a polytechnic (*Fachhochschule*) or university (5). Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Data: German General Social Survey (ALLBUS), 1986 survey.

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