

The Effect of Education on Fertility: Evidence from a Compulsory Schooling Reform

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Abstract

This study analyzes the effect of education on the number of children, childlessness, and the timing of births. We use exogenous variation from a mandatory reform of compulsory schooling in West Germany to deal with the endogeneity of schooling. In contrast to studies for other developed countries, we find a significant negative effect of education on fertility. This negative effect seems to operate mainly through a postponement of first births and through suppressed childbearing among women in their 30s. We attribute these findings to the particularly high opportunity costs of child-rearing in Germany.

Key Words: Fertility, education, timing of births, childlessness, educational reform

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1 Introduction

Population aging challenges social security systems including health care systems and public pensions. The continuous decline in birth rates and the increasing share of childless women in the developed world accelerates recent demographic changes. Since the 1970s total fertility rates (TFR) have fallen substantially in various developed countries. Germany exhibits one of the fastest fertility declines worldwide. Remarkably, Germany's fertility rate has fallen below the replacement level of 2.1 children already in 1970 and has been continuously decreasing to 1.4 in 2009 (Federal Bureau of Statistics 2007). In 2009, German women reached first motherhood on average at age 30. This age at first birth is one of the highest among OECD countries (OECD 2011). Furthermore, the observed rise in the share of childless women additionally contributes to the drop in birth rates (OECD 2011).

The causes of these negative fertility trends are still not fully clear. Various factors such as female employment rates and educational attainment are correlated with total fertility (D'Addio & D'Ercole 2005). However, recent studies for developed countries do not support the hypothesis that increased education causally reduces fertility, because the negative education-fertility-relationship disappears or becomes even positive after controlling for the endogeneity of schooling (see, e.g., Monstad et al. 2008, Fort et al. 2011, McCrary & Royer 2011). Nonetheless, the existing evidence on the education-fertility-nexus for European countries is mixed. Different results across countries may reflect the importance of different channels through which education affects fertility (e.g., opportunity costs or assortative mating).

This study examines the causal effect of education on three fertility outcomes: number of children, probability of remaining childless, and timing of childbearing. We use two large datasets: the German Mikrozensus (MZ), and the German Socio-Economic Panel (SOEP) to analyze the fertility of West German women born between 1937 and 1961.

We exploit a reform that extended mandatory schooling from 8 to 9 years in West Germany. We take advantage of this exogenous variation in schooling to deal with potential omitted variable bias. In particular, our identification strategy draws on the exogenous variation in education resulting from the staggered implementation of the reform in the West German states. By applying an instrumental variable approach we identify the causal effect of education on fertility.

In contrast to previous studies for developed countries, we find a negative causal effect of education on fertility. We find that one additional year of schooling reduces the number of children on average by 0.2, and increases the probability of childlessness by about 5 percentage points. Our analysis of the timing of births suggests that education causally reduces the probability of teenage births. Such birth postponement away from teenage motherhood is consistent with earlier research (see, e.g., Black et al. 2008, Monstad et al. 2008) and may reflect the incompatibility of schooling and motherhood. In addition, we find that education decreases the probability of giving the first birth between ages 31 and 35. This effect may work either through higher opportunity costs or be a consequence of age-related changes in fecundity. We argue that in contrast to other countries the main driving factor behind the strong negative effect of education on completed fertility are substantial opportunity costs of child-rearing in Germany. Specifically, we focus on West Germany and thus study an institutional setting marked by high wage penalties for motherhood and very limited supply of public childcare (Charles & Luoh 2003, Kreyenfeld 2004). Our results are robust to a number of robustness checks (specification tests, sample restrictions).

This paper is structured as follows: section 2 briefly describes the German school system and the reform of compulsory schooling. Section 3 presents the theoretical considerations, summarizes existing evidence, and derives our hypotheses. In section 4 we introduce our empirical approach. Section 5 describes the data, section 6 presents the results, and section 7 shows robustness checks. We conclude in section 8.

2 Institutional setting

The German secondary school system is a tripartite system which sorts students into school tracks at the age of 10.¹ The three school tracks, i.e. basic school (*Hauptschule*), middle school (*Realschule*), and high school (*Gymnasium*), prepare for different careers. Basic school lasts 8 to 9 years and prepares for apprenticeship training. Middle school comprises 10 grades and prepares for apprenticeships and training in white collar jobs. Graduation from high school after

¹ In recent years some of the German states changed the school system to a two-track system. These changes do not affect the birth cohorts under study.

12 years (*Fachhochschulreife*) gives access to technical colleges (*Fachhochschule*). The school leaving certificate after 13 years (*Abitur*) qualifies for university study.

Figure 1 gives the shares of students graduating from the three tracks for birth cohorts 1932-1965. We observe decreasing shares of basic school graduates and increasing shares of high school and middle school graduates for the younger cohorts. The share of basic school graduates born before 1935 was over 80 percent and fell to below 40 percent in the cohorts after 1960. Accordingly, in the observed period the share of high school graduates increased from about 8 percent to about 23 percent. Thus, educational expansion in Germany led to a sizeable shift of graduates away from basic schools.

[Figure 1 about here]

The sorting of students into the different tracks depends on various criteria and differs by state.² Assignment to a particular track strongly affects subsequent careers because students rarely change tracks (Pischke 2007, Dustmann 2004). Switching to lower tracks is more frequent than switching to higher tracks (Jürges & Schneider 2007).

The reform that at the state level we use for our identification strategy extended compulsory schooling by one year (from 8 to 9 years) between 1946 and 1969.³ The motives for the reform were twofold: first, 14 years old pupils were considered to be too immature for the labor market. Thus, an extension of compulsory schooling aimed at improving occupational maturity, physical and psychological protection of children, and the quality of occupational choices (Petzold 1981). The second argument was related to high youth unemployment and a shortfall of apprenticeship training positions in the 1950s. Extended schooling aimed to direct youth from manual to more intellectually demanding jobs (Petzold 1981).

[Table 1 about here]

² In some states parents decide about the track their child attends after primary school. In others only primary school grades determine the eligible tracks. In most states sorting follows by the recommendation of primary school teachers and choice of the parents (see KMK (2010a) for a description of transition processes to secondary school tracks, and KMK (2010b) for a detailed description of the school system).

³ A number of studies has already exploited the German reform of the extension of compulsory schooling. See, e.g., Pischke & von Wachter (2008), Brunello et al. (2009), Siedler (2010), Kemptner et al. (2011), Piopiunik (2011), Fort et al. (2011).

The German school system is governed at the level of the federal states. This decentralized organisation lead to a staggered implementation of the 9th grade by state. [Table 1](#) gives the year of the implementation and the first affected birth cohort by state. While some states such as Hamburg and Schleswig-Holstein introduced the 9th grade shortly after the Second World War, others such as Bavaria postponed the reform until the 1960s.⁴ Some states implemented the reform simultaneously with the standardization of the starting date of the school year, i.e. states where school traditionally started in spring shifted the start to the fall.⁵

3 Literature and hypotheses

From a theoretical perspective, the predicted effect of an exogenous increase in a woman's education on her fertility is ambiguous because it depends on different substitution and income effects. Economists emphasize several causal channels by which education could affect fertility choices.

The standard microeconomic model of fertility stresses the role of the labor market channel (Becker et al. 1960). On the one hand, the education increases women's permanent wage stream and raises the opportunity costs of leaving the labor market to rear children (Becker 1965, Willis 1973). This substitution effect tilts women's optimal fertility choices towards fewer children. On the other hand, economic theory suggests that an increase in labor earnings should be positively related to fertility if families can afford more children (Becker et al. 1960). However, the strength of this income effect may be weakened if parents with higher income prefer children of higher quality (Becker & Lewis 1973). The more parents invest in each child, the fewer they can afford.

Other channels by which education could affect fertility comprises the mating market. Under positive assortative mating, a woman's education is causally related to her partner's education (Behrman & Rosenzweig 2002). Consequently, an exogenous increase in a woman's education affects her permanent income through a spouse-related multiplier effect. Another causal path-

⁴ The timing of the reform at the local level varied also within the states because of shortfalls of teachers and lack of infrastructure in rural areas (Leschinsky & Roeder 1980).

⁵ See Pischke (2007) for a detailed description of the long and short school years.

way emphasized by the literature works through the effect of education on women's knowledge of contraception or reproductive health (Grossman 1972, Rosenzweig & Schultz 1989).

Finally, education may affect childbearing through a pure "incarceration effect" because enrollment in the educational system itself may be incompatible with motherhood (see, e.g., Black et al. 2008). However, such birth postponement may be temporary and does not necessarily affect completed fertility (see, e.g., Lappegård & Rønsen 2005).

Although an extensive empirical literature documents the association of female education and childbearing, a causal relationship is difficult to establish because of potential reverse causality and selection on unobservable factors. Several recent studies address these challenges by using school entry policies (see, e.g., McCrary & Royer 2011) or changes in compulsory schooling laws (see, e.g., Black et al. 2008, Monstad et al. 2008, Silles 2011) as instruments for education. The evidence from these studies is mixed as we show in [Table 2](#).

[[Table 2](#) about here]

Studies for countries and population groups with higher fertility levels generally find negative effects of education on fertility (see, e.g., Osili & Long (2008) for Nigeria, Lavy & Zablotsky (2011) for Arabs in Israel). Analyses based on compulsory schooling reforms in the U.S. and several European countries usually suggest that increased education leads to a postponement of the first birth away from teenage motherhood (see, e.g., Black et al. (2008) for the U.S. and Norway, Silles (2011) for Great Britain and Northern Ireland). However, increased education does not necessarily affect completed fertility because women can catch up an initial reduction in births at later ages (see, e.g., Monstad et al. (2008) for Norway, Fort (2009) for Italy). McCrary & Royer (2011) explore school entry rules in two U.S. states (California and Texas) and do not find any causal effect of education on fertility, neither on the incidence of motherhood, nor on the timing of first births. Recent contributions by Fort et al. (2011) and Braakmann (2011) present contradictory evidence from mandatory schooling reforms in Europe.⁶ Their results suggest that more education significantly increases the number of births. Inarguably, the estimated effects may differ across studies because education affects fertility

⁶ The study closest to ours is Fort et al. (2011) which examines fertility in Europe using reforms to compulsory schooling from eight countries. However, this study considers only four German states which introduced the reform simultaneously in 1967.

through several different channels, and the intensity of these channels may vary across countries, subpopulations, or levels of education.

As for Germany, related literature on mothers' labor market outcomes indicates that opportunity costs of child rearing are particularly high because the institutional framework renders work and family life incompatible. West German mothers experience low coverage of public childcare services (D'Addio & D'Ercole 2005). Wrohlich (2008) finds a considerable excess demand for subsidized childcare in Germany. Persisting excess demand is related both to the highly regulated market for subsidized childcare (Evers et al. 2005) and to the high cost of private childcare alternatives (Wrohlich 2008). Also, skepticism about the quality of public childcare (Blau & Hagy 1998, Spieß & Tietze 2002), and social attitudes against the employment of mothers (Hank et al. 2004) may increase the cost of childrearing in Germany. In addition, compared to mothers from other developed countries, German mothers experience high wage losses associated with child-related employment interruptions (Gangl & Ziefle 2009). Cross-national comparisons of fiscal and parental leave policies conclude that the German institutional framework favors traditional "male bread-winner" families and encourages mothers to stay out of the labor market (at least temporarily), or to work part time (see, e.g., Sainsbury 1999, Geyer & Steiner 2007, Dearing et al. 2007, Hanel & Riphahn 2012).

Based on the above discussion, we test the hypothesis that education reduces fertility in Germany. Specifically, we expect that additional education raises the probability of remaining childless and reduces the number of births because the opportunity costs of child rearing increase. Furthermore, we expect that an extension of mandatory schooling generates a postponement of first births because the German education system and labor market are both incompatible with childrearing.

4 Identification strategy

Our identification strategy considers the following equations:

$$y_i = \alpha \text{educ}_i + \text{state}'_i \theta + \text{cohort}'_i \varphi + \text{statetrend}'_i \mu + z'_i \pi + \epsilon_i \quad (1)$$

$$educ_i = \phi reform_i + state_i' \beta + cohort_i' \gamma + statetrend_i' \delta + z_i' \lambda + \nu_i \quad (2)$$

where the dependent variable y_i represents different measures for women's fertility outcomes. We consider three fertility outcomes: the number of children ever born, the probability of remaining childlessness, and the timing of births. These outcomes are a function of years of education ($educ_i$), state fixed effects ($state_i$), cohort fixed effects ($cohort_i$), state-specific cohort trends ($statetrend_i$), and socio-demographic background variables z_i such as marital status and community size.⁷

Estimating equation 1 by OLS may yield biased estimates for α - the impact of education on fertility, because the error term ϵ_i may include unobserved characteristics such as family preferences or childhood experiences. These characteristics are correlated with both education and fertility. However, the direction of the bias in an OLS estimation of α is not clear: while, e.g., family orientation leads to a downward bias, childhood experiences may bias the coefficient upwards.

We draw on exogenous changes in education caused by a compulsory schooling reform to identify the causal effect of education on fertility outcomes. In particular, we exploit the cross-regional and cross-time variation in education that results from a staggered implementation in the German states within a 23-year period (1946-1969). In a two stage least squares approach we first estimate equation 2: years of education of individual i ($educ_i$) are a function of an indicator for the reform status ($reform_i$), which equals 1 if a woman was affected by the reform, and 0 otherwise. Furthermore, equation 2 includes the same control variables as equation 1: cohort fixed effects, state fixed effects, state-specific cohort trends, and socio-demographic variables z_i .

Our identification strategy fails if the timing of the reform is correlated with fertility in the federal states. To mitigate this concern we include linear and squared state-specific cohort trends in birth cohorts, which should capture any smooth trends in fertility and schooling at the state level. Moreover, we include cohort fixed effects to account for any cohort-specific effects such

⁷ If the data allows it, we also control for family background, e.g. mothers' age of birth. State-specific cohort trends (Interaction terms of state interactions and linear year of birth) are included linear and quadratic.

as the availability of oral contraceptives. Finally, state fixed effects should capture state-specific differences such as religious affiliation which may have also influenced the timing of the reform.

Our identification strategy also fails if other fertility relevant changes took place and affected the same birth cohorts as the schooling reform did. This is very unlikely since the overall responsibility for family politics lies with the federal government. The state governments enforce the enacted laws or reforms and are free to extend these reforms, but the constitution grounds that inhabitants of different states must have similar living conditions (Gerlach 2010). Thus, any important reform in the last 60 years such as the introduction of child benefits (*Kindergeld*) affected women in all states simultaneously.⁸

5 Data

We use data from two German surveys to examine the relationship between women's education and the three fertility outcomes (Number of children, childlessness, and the age-specific probability of births): the German Microzensus (MZ), and the German Socio-Economic Panel (SOEP).

Our primary data source is the MZ, an annual survey of an one percent random sample of the households in Germany.⁹ We use the 2008 survey, which is the first survey providing information on the number of children ever born to female respondents.¹⁰ The key advantage of the MZ is its large sample size and low unit and item non-response. The main shortcoming of the data is the lack of information on the timing of childbearing, the state where an individual went to school, and parental background.

The second data set used here is the SOEP, a longitudinal survey of private households, conducted annually since 1984 (Haisken-DeNew & Frick 2005). The SOEP overcomes several drawbacks of the MZ: first, it contains retrospective biography information on childbearing

⁸ Similarly, the health care system is governed at the federal level, so the timing of the introduction of the pill was not state-specific. This applies also to the reimbursement of oral contraceptives by the health insurances. Some health insurances operate only regionally (e.g., company health insurance funds), but to the best of our knowledge the timing of first reimbursements did not vary across health insurances. In addition, our results remain robust to exclusion of single states from the analysis which suggests that our results are not driven by a particular state.

⁹ The scientific use file includes 70 percent of the full data set.

¹⁰ Previous waves provide information on the number of children living in the household at the time of the interview.

and facilitates an analysis of the timing of childbearing. Second, the SOEP provides family background variables that may affect both education and fertility (e.g., the number of siblings, parental educational attainment, and parents' birth year). Finally, the data provide retrospective information on individuals' educational careers and the state where they went to school. The main shortcoming of the SOEP is its relatively small sample size. We use all available survey years, 1984-2010, to obtain a sufficient number of observations for the treatment and control group at the level of the federal state.

We impose similar sample restrictions on both MZ and SOEP data. [Table 3](#) presents the details on our data selection procedure.

[[Table 3](#) about here]

We select native German women¹¹ from eight out of ten (excluding Berlin) West German states. We exclude Schleswig-Holstein, and Hamburg from the analysis because the first birth cohorts affected by the reform were 1932 and 1931, respectively, and the MZ 2008 only reports the fertility of women born after 1933.¹² We extract those observations from the MZ, who were born up to 5 years before/after the first birth cohort affected by the reform as presented in [Figure 2](#). As the small sample size of the SOEP does not allow us to use a 5-year time window, we use a 7-year window instead. Ideally we would like to have direct information on the state where a woman went to school. However, the geographic location of the attended school is available only for a subsample in the SOEP. For the remaining observations we use the current state of residence as a proxy.¹³ We exclude women who graduated from a school in socialist East Germany from the sample.¹⁴

Neither of the two data sets reports the exact number of completed years of schooling. Instead, we observe the highest secondary school degree, and women's post-secondary education

¹¹ We omit women with migration background.

¹² In addition, the information on the dates of reform implementation in Schleswig-Holstein vary across different sources (compare Leschinsky & Roeder 1980, Pischke & von Wachter 2008).

¹³ A potential source for biased estimates may be regional mobility. If mobility is uncorrelated with the reform our results might be biased towards zero. If there was anticipation and thus selective migration, our results are biased upwards. We argue, in line with e.g., Pischke & von Wachter (2008), that regional mobility in Germany is so low (see, e.g., Harhoff & Kane 1997) that this should yield at most minor consequences for our estimates.

¹⁴ The MZ allows us to identify women with specific school degrees that could have been obtained in the former East German states only. In a subsample of the SOEP we can identify those who attended school in the former GDR.

and training. We construct years of schooling for Germany and assign the usual number of years taken for a particular educational route (see, e.g., Krueger & Pischke 1995, Pischke & von Wachter 2008). First, we use the state information, the year of birth, and information on the timing of the reform from [Table 1](#) to determine whether an individual should have graduated after 8 or 9 years in the basic track. For the other tracks, we use the standard duration for graduating in this particular track. Finally, we incorporate the information on post-secondary degree and training to calculate a measure of total number of years of education (Krueger & Pischke 1995). We conclude our sample selection by omitting observations with less than 7 years of education, missing values on education or fertility variables.¹⁵

Our final MZ sample includes 17,428 women born 1938-1959. The SOEP sample includes 2,649 women born 1937-1961. [Table 4](#) shows summary statistics for both data sources. The table splits women into those who were affected by the reform, and those who were not.

[[Table 4](#) about here]

In general, the MZ and SOEP samples show similar patterns for educational attainment and our control variables; however, fertility outcomes differ between the datasets. While the MZ data show that women who were subject to the reform have on average fewer children and are more likely to remain childless, we observe no fertility differences between treatment and control group in the SOEP sample. Nevertheless, women affected by the reform are more likely to delay their first birth. Fertility variation by reform status may reflect differences in birth cohort and educational attainment. By sample construction women affected by the reform are born later and, as expected, complete more years of education than women not affected by the reform. In both groups the number of years of education exceeds the mandated 8 or 9 years of schooling because notable shares of women obtain an additional degree or training.

[Figure 3](#) and [Figure 4](#) plot the raw data on fertility outcomes by reform status. [Figure 3](#) reveals the overall trends of declining fertility and increasing incidence of childlessness for younger cohorts, with small differences for women affected and not affected by the reform. [Figure 4](#) shows that women who are subject to the reform postpone their first birth beyond the

¹⁵ We exclude individuals with 7 years of education because this is inconsistent with 8 years (or 9 after the reform) of compulsory schooling. However, inclusion of this small subsample into the analysis does not affect our main results.

early 20s, and are more likely to bear their first child at older ages compared to women who are not subject to the reform. Overall, women affected by the reform exhibit a lower probability of childbearing up to the age 24 and a higher probability of childbearing starting from the late 20s compared to women not affected by the reform.

6 Results

6.1 Effect of education on the number of children and childlessness

We first explore whether the formal introduction of the compulsory 9th grade is a valid instrument for women's education. [Figure 5](#) plots the average number of years of education for the five birth cohorts before and after the reform using the MZ data. The graph reveals a general increase in education for younger cohorts, but indicates a discrete jump of roughly 0.85 years for the first birth cohort affected by the reform.

[[Figure 5](#) about here]

We estimate equation [2](#) using the MZ sample and show the first-stage estimation results in [Table 5](#). Columns 1 and 2 present the results obtained on the full sample, and columns 3 and 4 on the subsample of basic track graduates. Additionally, columns 5 and 6 give the effect of the reform implementation on track choice. We present the coefficients obtained from two specifications: one includes only linear state-specific trends in birth cohort, the other one also squared trends. In addition, all regressions include state of residence-fixed effects, year of birth-fixed effects, and indicators for marital status and municipality size. We estimate standard errors clustered at the state-year of birth level throughout to deal with potential serial correlation and heteroscedasticity (80 clusters).

[[Table 5](#) about here]

The first-stage estimation results in [Table 5](#) confirm the graphic impression: the first-stage coefficient of the reform indicator obtained on the full sample is significant across as specifications and indicates that the reform actually increased women's education on average by about

0.65-0.75 years (columns 1 and 2). The magnitudes of our estimates are slightly larger than those reported by other studies exploiting the German school reform (see, e.g., Pischke & von Wachter 2008, Siedler 2010, Kemptner et al. 2011, Piopiunik 2011). We argue that these differences result mainly from different sample restrictions.¹⁶ Consistent with previous studies, the first-stage coefficient is smaller than 1 in the full sample (columns 1 and 2). Several measurement errors can contribute to an attenuation of the reform indicator coefficient: first, the implementation of the additional grade could not have been immediate and in practice required several years until each school offered the mandatory ninth grade (Leschinsky & Roeder 1980). Second, we do not have direct information on the state of school attendance, and random migration may bias our estimates towards zero. Furthermore, the year of birth does not perfectly determine whether the reform affected an individual because some women might have started primary school earlier or later than officially scheduled. However, the measurement errors in the instrument do not bias subsequent estimates unless they are systematically correlated with both the reform introduction and fertility outcomes, which is rather unlikely. The F-statistics of significance tests of the reform dummy in the first stage is greater than 10 across specifications indicating the validity of the instrument.

A important assumption for a causal estimate of the local average treatment effect is *monotonicity* i.e. that all individuals respond to the reform implementation in the same way (Imbens & Angrist 1994). Thus, individuals extent education because of the reform, but do not reduce education (e.g., by attending basic track instead of middle track, or dropping-out). To test whether the reform affected track choice, we estimate the effect of the reform implementation on the probability of graduation from basic track (columns 5 and 6 in [Table 5](#)). The coefficients are very small, negative and insignificant. Thus, consistent with previous findings, we do not find evidence that the reform affected track choice (see, e.g., Pischke & von Wachter 2008, Kemptner et al. 2011, Piopiunik 2011).¹⁷

[Table 6](#) reports our key results on the effect of education on the number of children and the probability of remaining childless obtained on the full sample (columns 1 and 2) and the

¹⁶For example, Pischke & von Wachter (2008) analyze working females and males and drop all individuals with missing wages.

¹⁷Kemptner et al. (2011) conclude that also changes in the composition of students within tracks and therefore any peer effects should be very small, if track choice is not affected.

subsample of basic track graduates (columns 3 and 4). Note that the basic track subsample potentially includes a higher share of compliers (though we can not identify them directly) because the reform primarily affected basic track students.

[Table 6 about here]

We start by showing the estimates of standard OLS regressions. The results indicate that an additional year of education is negatively correlated with the number of children (Panel A, first row) and positively associated with the probability of remaining childless (Panel B, first row). Across specifications, samples, and fertility outcomes the OLS estimates are statistically significant at the 1% level. However, the OLS regressions do not account for possible selection into education. Since we are interested in a causal relationship we turn to the instrumental variables estimates.

The signs of the coefficients obtained by the instrumental variable (IV) approach are similar to those from the OLS regressions: education reduces the number of children (Panel A, second row) and increases the probability of never having children (Panel B, second row). However, the magnitude of the effect of education on fertility outcomes is substantially larger when controlling for possible endogeneity.¹⁸

Our main results are robust to changes in specification regarding the state-specific birth cohort trends. However, we argue that the specification including squared trends better addresses the concern that the introduction of the reform could be correlated with trends in education and fertility. The coefficient obtained from our preferred specification (column 2) suggests that on the margin, one year of additional education decreases fertility by 0.17 children, which is about one tenth of the mean number of children in the full sample (1.63 children). The rise in the probability of remaining childless amounts to 5.1 percentage points, which is large compared to the frequency of childlessness in our sample (18%).

¹⁸We obtain qualitatively similar results on the SOEP sample, but the estimates are imprecise because of the small sample size (see Table A.1 in the appendix). While using the SOEP we select a 7-year window to prevent inconsistency due to a small sample size. The signs of coefficients obtained using a 5-year window remain the same, but the standard errors increase as well. Also, the results for basic track graduates are similar to those obtained with the MZ data. Detailed results are available upon request.

6.2 Effect of education on the age-specific probability of childbearing

Table 7 presents the results for the timing analysis of births. Using the SOEP data we estimate the effect of education on the probability of giving birth at a given age. Again, all regressions control for state of residence, year of birth, linear and quadratic state-specific trends in year of birth, marital status, and municipality size.

[**Table 7** about here]

The OLS estimations suggest that an increase in education is related to a reduced probability of teenage motherhood and to an increase in fertility at the end of women's fertile period. The IV results yield different conclusions. The point coefficients suggest a negative effect of education on the probability of childbearing across all age groups. However, except for the births given at ages 25 and below we do not find any statistically significant effects. As for women aged 15-20 and 21-25, the coefficients are significant at the 5% and 1% levels, respectively, and of large magnitude: each year of additional education reduces the probability of teenage motherhood by 5.7 percentage points and the probability of motherhood at age 21-25 by 7.1 percentage points. These are considerable impacts relative to the incidence of childbearing at these ages in the full sample of 16% and 38%, respectively. The first stage F-statistic of 16.83 indicates that a weak instrument is not a concern.

Unfortunately, the results in **Table 7** do not allow us to draw further detailed conclusions on the effects of education on the age-specific probability of childbearing. Thus, we ran the regressions separately for each single age-year. **Figure 6** summarizes the results obtained from 26 separate regressions.¹⁹

[**Figure 6** about here]

We plot the coefficients of education and 90% confidence intervals around these point estimates by age. In contrast to the results in **Table 7**, **Figure 6** reveals more heterogeneous effects of education on childbearing across the fertile ages. The driving forces behind this heterogeneity is not fully clear. We can only suggest what causes some of the dynamics in the effects.

¹⁹The sample for age 40 additionally includes a small number of births given after age 40.

Specifically, we observe positive and significant effects for ages 18, 21 and 27. These positive effects may reflect a catch-up effect after graduation from secondary school, apprenticeships training, and university, respectively, which usually take place at these ages. Increased childbearing at age 31 may reflect improved knowledge about birth-related complications associated with increasing age. The remaining coefficients generally indicate negative effects of education on fertility. In particular, the largest effects are those at ages 19-20, and shortly before age 31 and in the mid 30s. Overall, negative effects of education on childbearing at a given age seem to outweigh any catch-up effects in births.

Finally, we consider the timing of the first birth only. Analyzing the effect of education on the probability of first birth in a given age allows us to draw further conclusions on the heterogeneity in the effect of education on the age-specific probability of births in [Figure 6](#). We estimate the effect of education on the probability of the first birth at a given age conditional on not already having a child. Thus, women who already had a birth are excluded from the sample and the number of observations used in these estimations falls with increasing age from 2,649 to 551 in the last regression for age 40.²⁰ [Figure 7](#) summarizes the results obtained from these 26 separate regressions.²¹

[[Figure 7](#) about here]

Comparing the point coefficients in [Figures 6](#) and [7](#) we find similar patterns for young ages: negative effects of education on the probability of a first birth at ages 19-20 and an increase in the probability of giving the first birth at age 27. This may reflect a postponement of births from very young ages to the mid 20s. Also, we find large negative effects of education on the probability of the first birth at ages 32-35 though in the analysis on the first birth some of the coefficients are now insignificant likely due to reduced sample sizes. As for women at ages between 32 and 35, the large negative point estimates accompanied by zero effects for subsequent ages suggest that they are more likely to remain childless than to bear children late. [Table A.2](#) in the appendix shows that the coefficient obtained from a regression using the age group 31-35 is significant at the 10% level and of a considerable magnitude: each year

²⁰ The sample for age 40 additionally includes women who gave their first birth between ages 40 and 43.

²¹ We show estimation results for five age groups in [Table A.2](#) in the appendix.

of additional education reduces the probability of first birth at ages 31-35 by 15.4 percentage points. The F-statistic of 10.00 indicates that a weak instrument is not a concern.

Overall, our results suggest that the impact of increased education is to lower the number of children and to raise the probability of childlessness. The negative effect of education on fertility seems to operate mainly through a postponement of the first birth away from early births and reduced childbearing in the mid 30s. Our findings for Germany contrast with the existing evidence for the U.S. and several West European countries (see, e.g., Black et al. 2008, Silles 2011, Monstad et al. 2008). These studies usually find that more education leads to a delay of the first birth beyond teenage years and to a later catch-up for the initial loss in births. However, up to now there is no evidence for developed countries suggesting that education causally reduces completed fertility. While birth postponement away from teenage motherhood in response to extended education may reflect a pure incarceration effect, reduced childbearing in the mid 30s may work through higher opportunity costs. This mechanism is consistent with the previous evidence suggesting that German women experience higher wage penalties for motherhood compared to women in other European countries (Gangl & Ziefle 2009). Still, the question remains whether the reduced childbearing at older ages should be traced back to women's choice to remain childless, or to an age-related decline in fecundity.

7 Robustness

In this section we test whether our main results are robust to changes in the definition of the educational variable and different sample selection criteria.

Our baseline results use the imputed years of education as a measure for women's educational attainment. We use the typical duration of attaining a specific degree and add postsecondary years of education. This educational variable has some disadvantages. For example, it does not consider information about repeated or skipped classes.

To check the robustness of our results we use an alternative variable called the "length of education". To construct this variable we use self-reported information about the graduation

year and the assumption that all women entered primary school at age 6.²² Thus, “length of education” is given by the actual graduation year minus year of birth minus school entry age 6 ($graduation\ year - year\ of\ birth - 6$) and measures calendar years, not school years. To some extent this alternative variable captures skipped and repeated classes, and the short and long school years, which some states performed simultaneously to the extension of compulsory schooling. However, the “length of education” introduces noise because, for example, some women start school at age 7. Such noise may bias our first-stage results towards zero, and leads to higher coefficients at the second stage.

[Table 8 about here]

Table 8 gives the IV results for the effect of education on fertility obtained by using the variable “length of education”.²³ The results are robust to this change in the measure of educational attainment. First-stage coefficients are below 1 across all specifications which may be driven by measurement error, the short school years, or measurement error in the timing of the reform. The values of the first-stage F-statistics are lower than those presented in Table 6. In the full sample (columns 1 and 2) the significance of the results varies with the specification of the included trends in birth cohort, but the sign of the coefficient remains negative. Also, the estimation on the basic track graduates confirms the robustness of our basic results (columns 3 and 4).

To test whether our results depend on our sample selection criteria, we replicate the results using different symmetric and asymmetric windows of birth cohorts around the first cohort affected by the reform. In the main analysis we use a symmetric window. We follow Monstad et al. (2008) and Brunello et al. (2009) and argue that a symmetric approach guarantees similar sample sizes in treatment and control group by state and similar characteristics of the two groups. Furthermore, the estimation of the effect of education is on a more local level because

²²The major problem with this variable is potential measurement error in the self reported graduation year. Our sample size reduces to 12,657 observations if we use only reliable information from MZ. We classify information reliable if the “length of schooling” was up to two years longer or shorter than the standard duration needed to obtain a certain degree. The first-stage results are slightly affected by our definition of valid information. The wider the window the lower the first-stage coefficient and vice versa. The direction of the effect of education on the number of children and childlessness are robust. More detailed results are available upon request.

²³We are not able to carry out this robustness test on the SOEP sample because the information on graduation year is available for a very small number of observations.

a symmetric window excludes effects of other potential reforms and reduces the influence of unaccounted confounders (Brunello et al. 2009). The disadvantage of a symmetric window is a substantial reduction in sample size. An alternative approach is to use an asymmetric window, i.e., to include all individuals born between 1938 and 1959 (see, e.g., Pischke & von Wachter 2008, Piopiunik 2011). We next estimate the effect of education on completed fertility and childlessness using different definitions of the window around the reform: a 4 to 7-year symmetric window, and an asymmetric window.

[Table 9 about here]

Table 9 shows that the choice of the window width may affect the magnitude of the effect, but the effect of education on the number of children remains negative. The first stage F-statistic increases the wider the window and the larger the sample. The effect of education on fertility decreases the wider the window and the larger the sample. If we use a 7-year symmetric window the effect remains negative, but the coefficient is insignificant. Also, the effect of education on the probability of childlessness is robust to these changes in sample selection criteria.

We apply similar robustness checks for our main results on timing obtained with the SOEP sample and using a 7-year window. We repeat the regressions using alternative definition of the window around the first birth cohort affected by the reform. Table 10 gives the results on the probability of giving birth in a given age group obtained with both a 5-year and a 9-year window.

[Table 10 about here]

The table reveals that the sample size decreases to hardly 2,000 individuals if we use a 5-year window, however, the results remain qualitatively the same. The magnitude and significance of the coefficients changes, but the direction of the effects on giving birth at a particular age remains unaffected. The results obtained using a 9-year window confirm our baseline results presented in Table 7. The effect of education on giving birth at a given age is negative for almost all age groups except for women older than 35. Also, we find negative and significant effects of education on giving birth below age 25. Consistent with the results obtained with the MZ sample, the effects are smaller for a wider window.

8 Conclusions

We examine the effect of women's education on the three fertility outcomes number of children, probability of ultimate childlessness, and timing of births. We exploit a German schooling reform that extended compulsory education from 8 to 9 years, to identify the causal effect of one additional year of schooling on fertility. Our empirical approach takes advantage of the exogenous variation in the reform's timing across federal states and time.

We find that education generally increases the probability of remaining childless and reduces the number of children. In particular, one additional year of education raises the probability of childlessness by 5 percentage points. This effect is considerable compared to the average incidence of childlessness of 18 percent in the analyzed cohorts of women. The effect of education on the total number of children is also pronounced, as an additional year of education induces women to reduce the number of births by almost 0.2 children. This decrease accounts for about one tenth of average cohort-specific fertility. We show that our main findings are robust to changes in sample selection criteria and alternative variable definitions.

Consistent with causal evidence for other developed countries (see, e.g., Monstad et al. 2008, Grönqvist & Hall 2011) our findings on the timing of births suggest that longer education leads to a birth postponement away from teenage motherhood and from motherhood in the early 20s. Furthermore, additional education significantly suppresses the probability of giving birth at ages 31-35. It may seem implausible that the extension of compulsory schooling by one year still affects women's choices in their 30s, but we argue that decreased childbearing in this age group may reflect higher opportunity costs, inflexibility of the labor market, or age-related decline in fecundity.

Our findings for Germany are quantitatively and qualitatively important, because there is no previous causal evidence for developed countries suggesting that education restricts motherhood, and reduces family size.²⁴ Our findings sharply contrast with the recent evidence for other European countries by Fort et al. (2011) and Braakmann (2011) who conclude that more education significantly increases the number of births. We interpret these different patterns for

²⁴The existing causal studies for the U.S. and several European countries usually suggests that extended education leads to postponement of first birth away from teenage motherhood, but does not affect completed fertility (see, e.g., Black et al. 2008, Monstad et al. 2008, Silles 2011).

Germany mainly as the result of high opportunity costs of childrearing, compared to other countries. Specifically, our results are consistent with previous evidence suggesting that German mothers experience the highest wage penalty for motherhood in the Western world (Gangl & Ziefle 2009). Gangl & Ziefle (2009) find that the high permanent wage losses seem to be related to statistical discrimination against mothers in the German labor market. Several features of the institutional and cultural environment in Germany hamper the compatibility of family and work, and thus reinforce employers' discrimination against mothers. Most striking is the highly inflexible system of public childcare which might account for the persisting low public childcare provision (D'Addio & D'Ercole 2005, Wrohlich 2008). Furthermore, extensive parental leave regulations and the tax system providing a "housewife bonus" favor traditional family types and reinforce attitudes in favor of mothers' non-involvement in the labor force (Kreyenfeld 2004).

The evidence on the effect of education on fertility varies across countries. These differences may reflect institutional differences that affect women's opportunity costs of childbearing such as childcare provision rates, family policies or flexibility of the labor market. As for Germany, our results may be a consequence of institutional setting, labor market conditions, or social norms. We leave the exploration of cross-country differences in the effect of education on fertility for future research.

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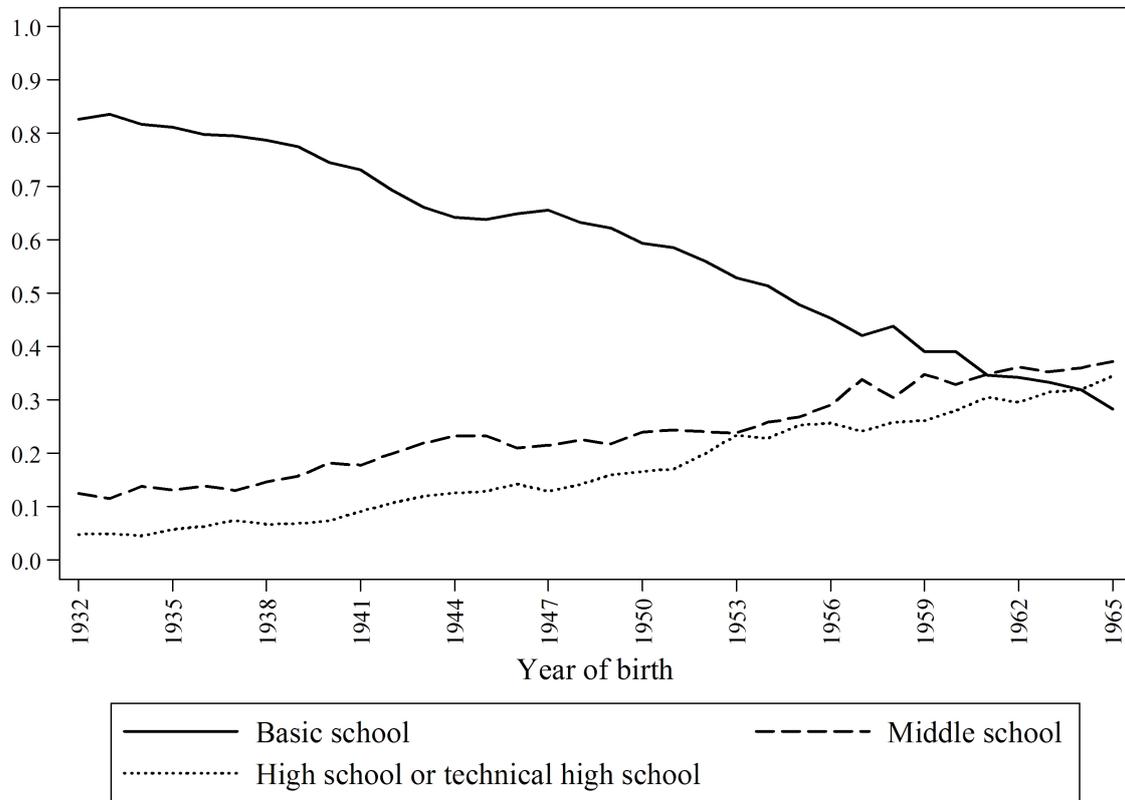
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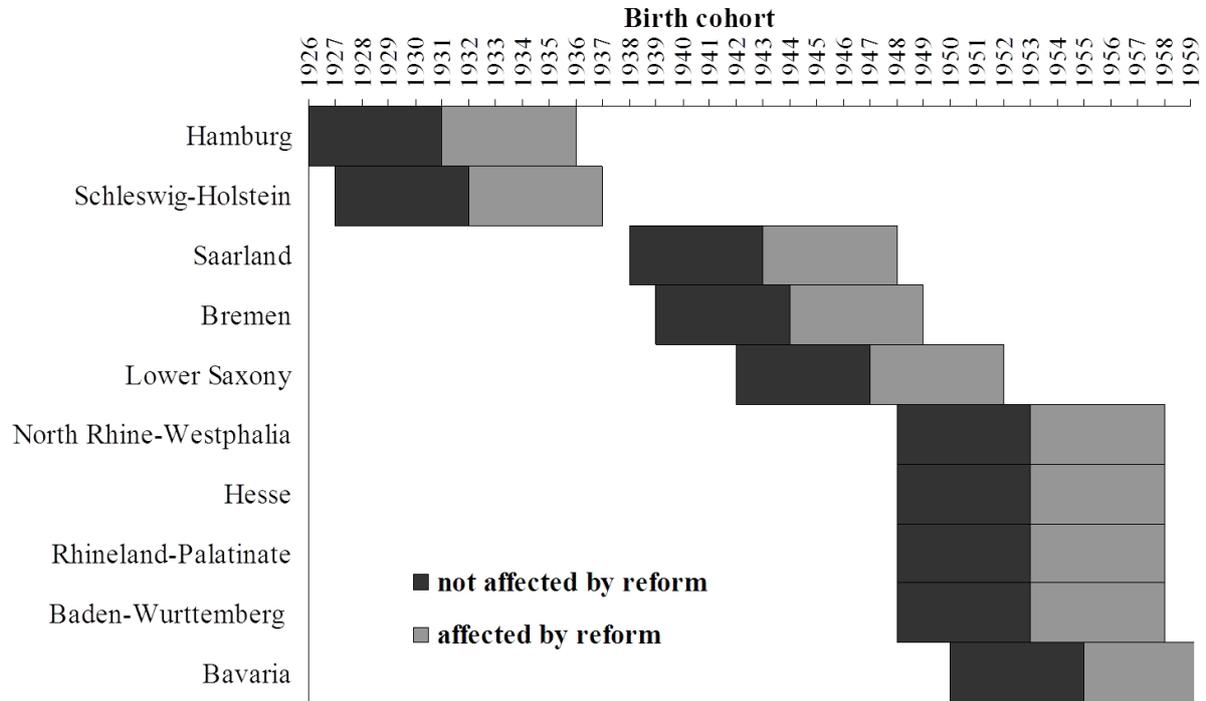
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Figure 1: Cohort share of graduates by school degree and birth year



Source: German Mikrozensus (MZ) 2008; own calculations.

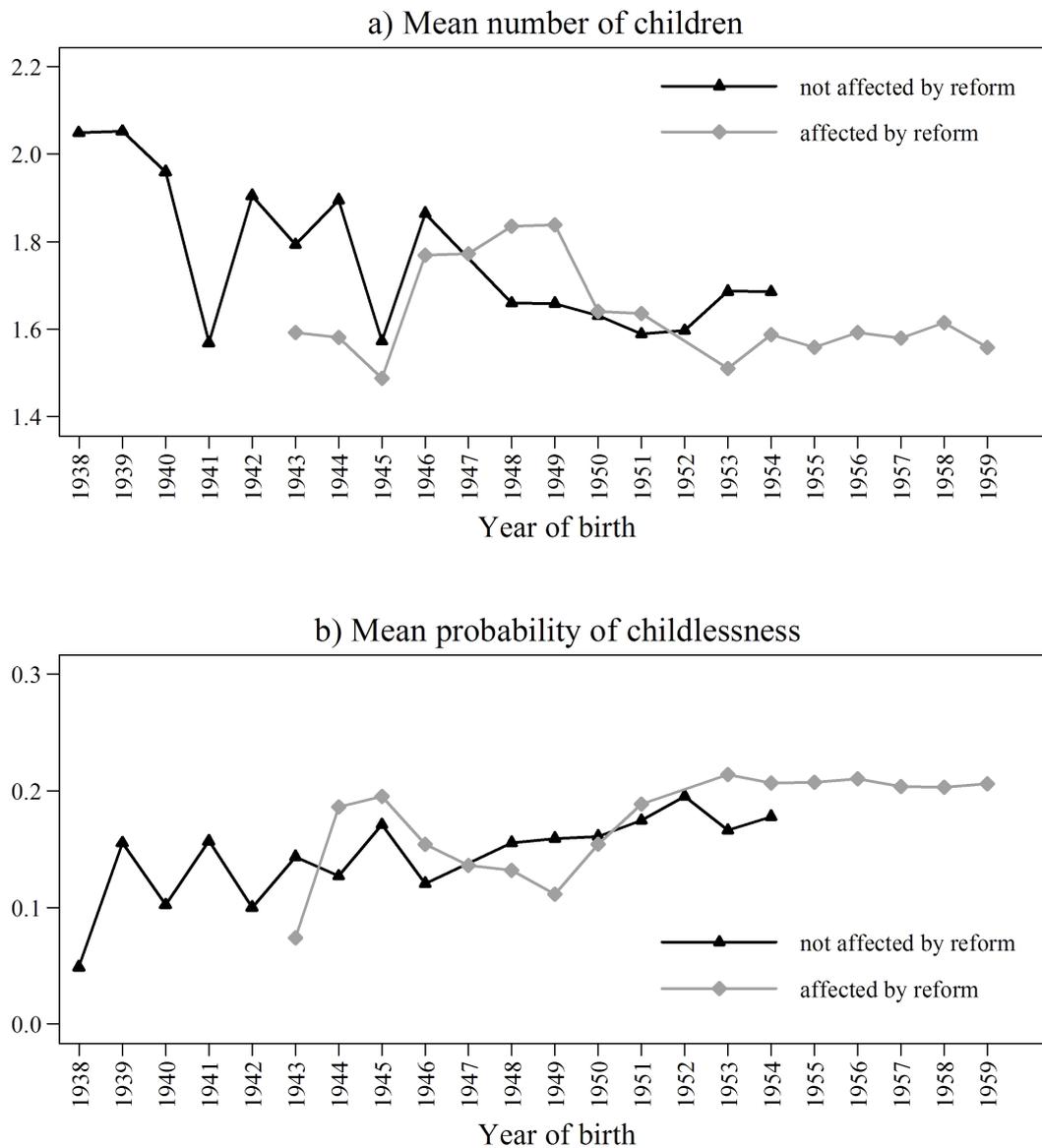
Figure 2: Analyzed birth cohorts by federal state and reform status



Note: We exclude Schleswig-Holstein and Hamburg from the analysis because the MZ 2008 reports fertility only for women born later than 1933.

Source: Leschinsky & Roeder (1980); own illustration.

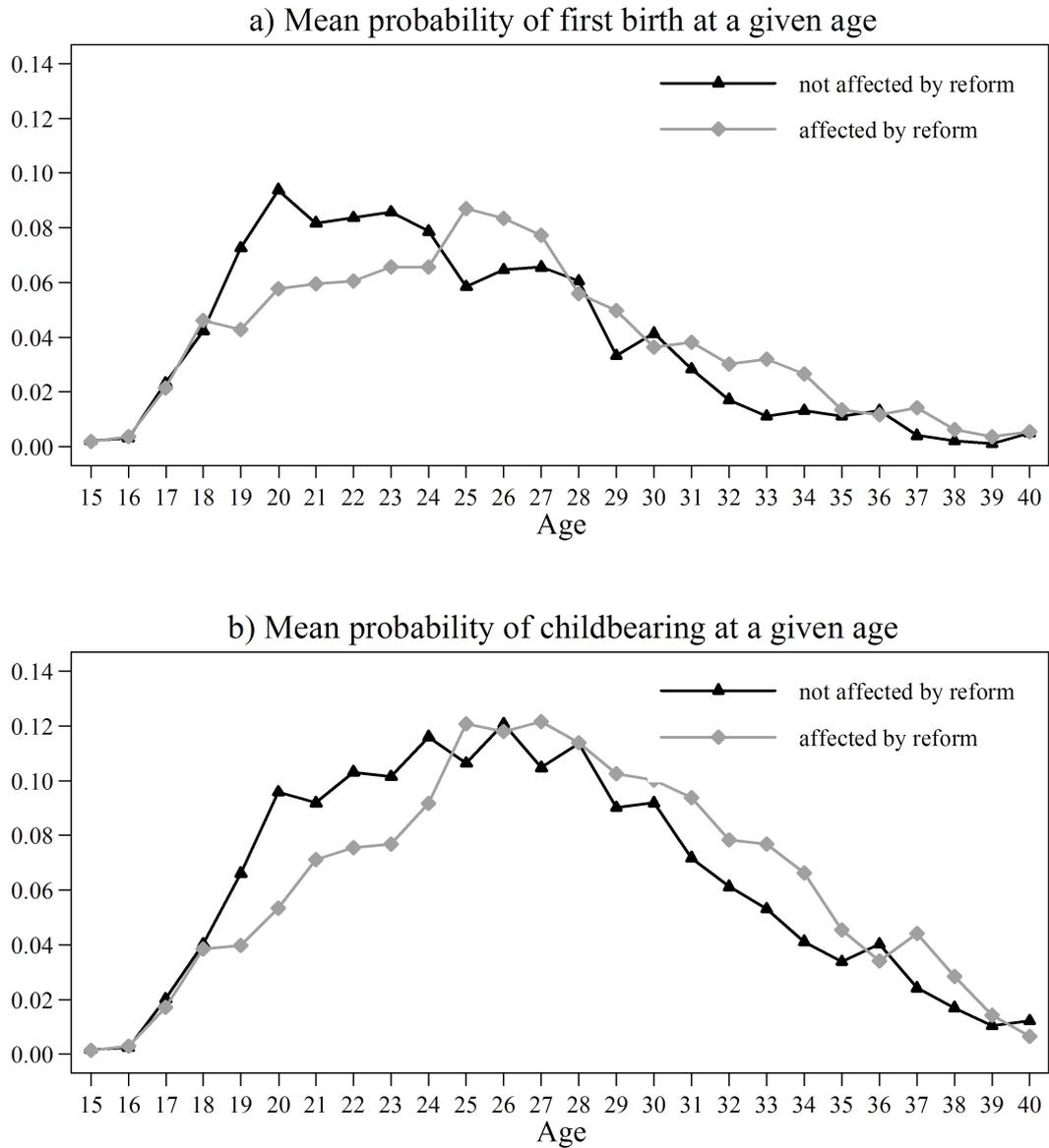
Figure 3: Fertility outcomes by birth cohort and reform status



Note: The plots present raw data.

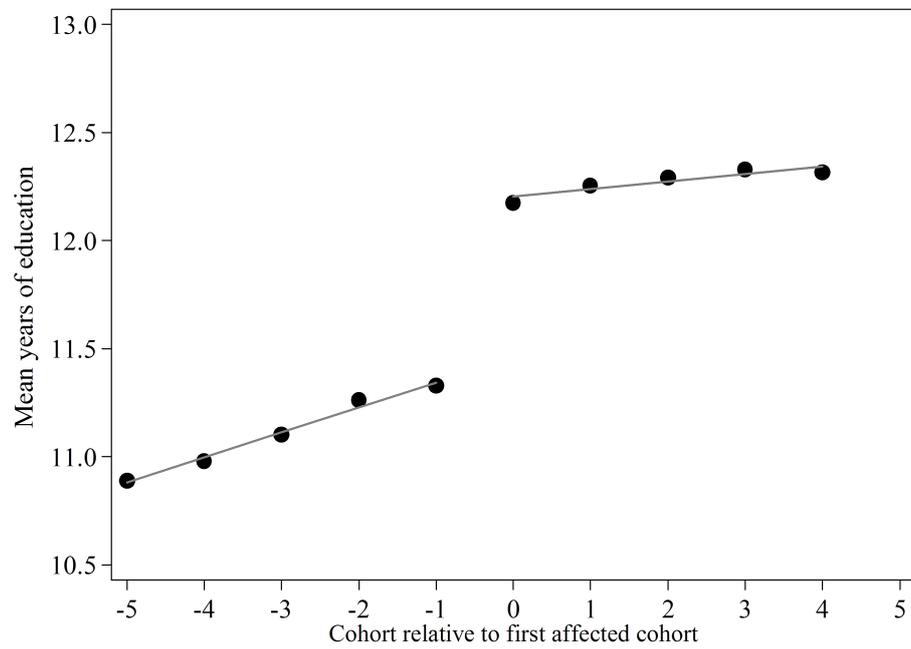
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 4: Timing of births by age and reform status



Note: The plots present raw data.
 Source: SOEP 1984-2010; own calculations.

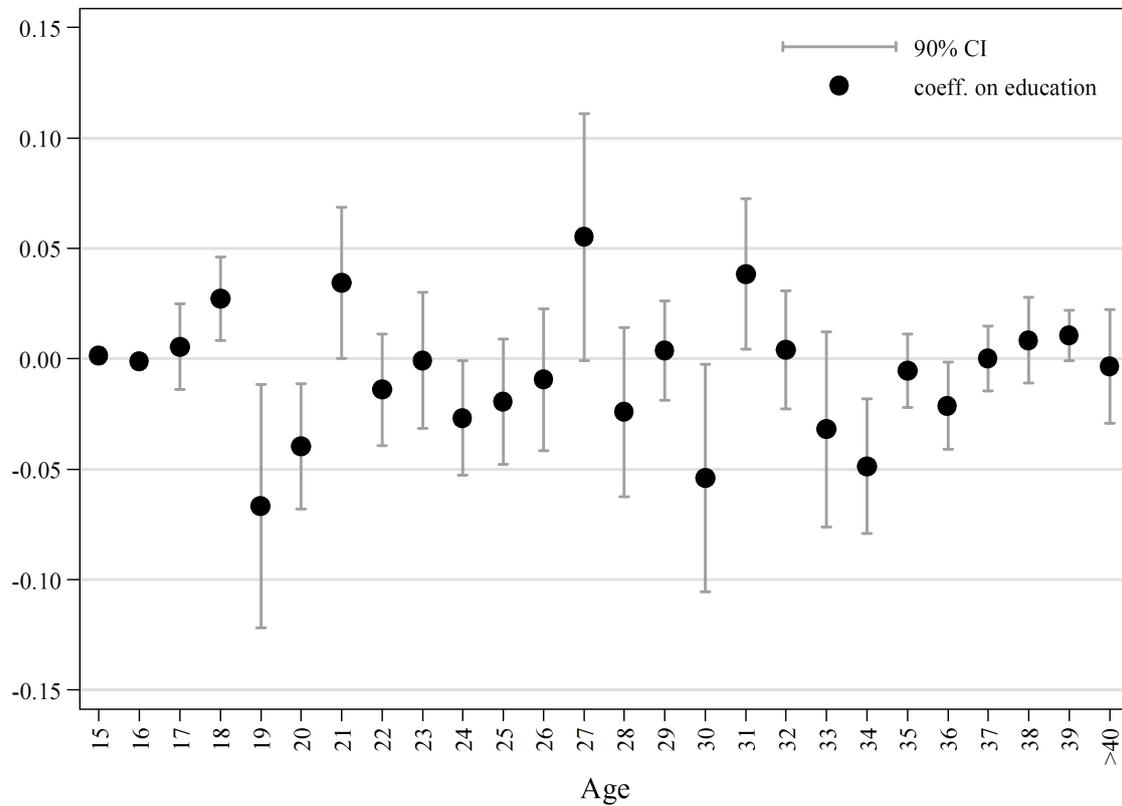
Figure 5: First stage: effect of the reform on years of education



Note: The plot presents raw data.

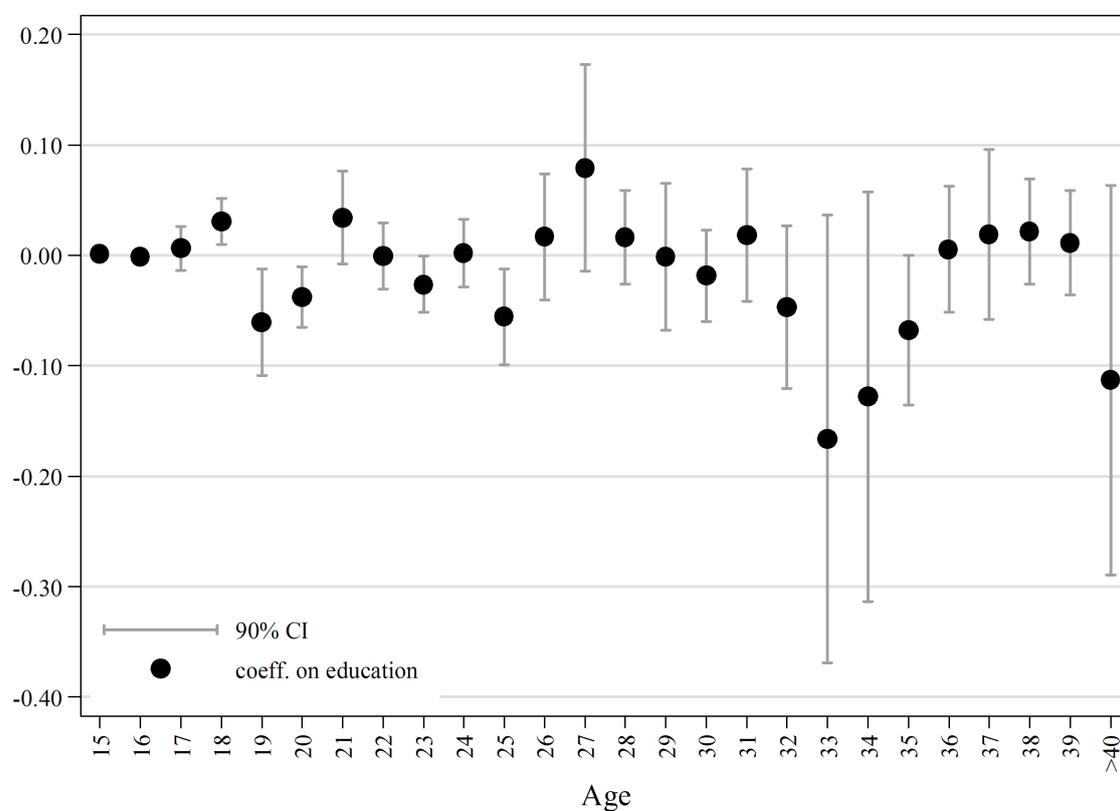
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 6: The effect of education on the age-specific probability of childbearing



Note: Dependent variable is probability of childbearing at a given age. Each dot presents a coefficient on education obtained from a separate linear regression. Range spikes are 90% confidence intervals around the point estimate. All regressions include state of residence-fixed effects, year of birth-fixed effects, indicators for marital status, mothers' age at birth, and municipality size.
 Source: SOEP 1984-2010; own calculations.

Figure 7: The effect of education on the age-specific probability of first childbearing



Note: Dependent variable is probability of childbearing at a given age, conditioned on not already having a child. Each dot presents a coefficient on education obtained from a separate linear regression. Range spikes are 90% confidence intervals around the point estimate. All regressions include state of residence-fixed effects, year of birth-fixed effects, indicators for marital status, mothers' age at birth, and municipality size. Source: SOEP 1984-2010; own calculations.

Table 1: Introduction of the 9th grade in compulsory schooling

Federal state	First school year with compulsory 9 years	First birth cohort with compulsory 9 years
Hamburg	1946	1931
Schleswig-Holstein	1947	1932
Saarland	1958	1943
Bremen	1959	1944
Lower Saxony	1962	1947
North Rhine-Westphalia	1967	1953
Hesse	1967	1953
Rhineland-Palatinate	1967	1953
Baden-Wuerttemberg	1967	1953
Bavaria	1969	1955

Note: Date of the introduction of the 9th grade in secondary schooling in West Germany and the first affected birth cohort.

Source: Leschinsky & Roeder (1980).

Table 2: Empirical evidence on the effect of education on fertility

Dependent variable	Number of children	Age of first birth	Country
Osili & Long (2008)	negative		Nigeria
Lavy & Zablotsky (2011)	negative		Arabs in Israel
Fort (2009)	no effect		Italy
Monstad et al. (2008)	no effect	positive	Norway
Grönqvist & Hall (2011)	no effect	positive	Sweden
McCrary & Royer (2011)	no effect	no effect	U.S.
Fort et al. (2011)	positive		Europe
Braakmann (2011)	positive		Great Britain
Black et al. (2008)		positive	U.S. and Norway
Silles (2011)		positive	Great Britain and Northern Ireland

Note: Listed studies analyze the effect of education on the number of children and/or the effect of education on the timing of the first birth. All studies address the endogeneity of schooling e.g., by using school reforms as an instrument for education.

Table 3: Data selection

	Number of observations	
	MZ	SOEP
Women from eight West German states		
born 5-years before/after the first affected birth cohort	20,054	-
born 7-years before/after the first affected birth cohort	-	2,708
Information on state of education available	-	1,328
Excluded because education in former East German states	251	13
Excluded because education < 7 years or missing	263	45
Excluded because missing fertility information	2,112	1
Sample size	17,428	2,649

Source: German Mikrozensus (MZ) 2008 and SOEP 1984-2010; own calculations.

Table 4: Sample means by reform status

Variable	MZ		SOEP	
	not affected	affected	not affected	affected
Year of birth	1949.46 (3.12)	1954.53 (3.08)	1948.54 (3.36)	1955.80 (3.09)
Number of children	1.66 (1.12)	1.59 (1.16)	1.64 (1.16)	1.66 (1.20)
Childless	0.16 (0.37)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)
Probability of giving first birth at age				
15-20	-	-	0.11 (0.32)	0.09 (0.29)
21-25	-	-	0.34 (0.47)	0.25 (0.43)
26-30	-	-	0.23 (0.42)	0.28 (0.45)
31-35	-	-	0.09 (0.28)	0.13 (0.34)
>35	-	-	0.03 (0.18)	0.05 (0.21)
Probability of giving birth at age				
15-20	-	-	0.11 (0.32)	0.09 (0.29)
21-25	-	-	0.41 (0.49)	0.30 (0.46)
26-30	-	-	0.44 (0.50)	0.45 (0.50)
31-35	-	-	0.27 (0.44)	0.33 (0.47)
>35	-	-	0.12 (0.33)	0.16 (0.37)
Years of education	11.11 (2.91)	12.28 (2.77)	11.63 (3.23)	12.90 (2.98)
Attended (0/1) basic track: 8th or 9th grade	0.63 (0.48)	0.51 (0.50)	0.56 (0.50)	0.41 (0.49)
Number of observations	8,399	9,029	1,242	1,407

Note: Standard deviations in parentheses. Samples are not weighted.

Source: German Mikrozensus (MZ) 2008 and SOEP 1984-2010; own calculations.

Table 5: First stage results and the effect on track choice

	Dependent variable					
	Years of schooling				Probability attend basic track	
	Full sample		Basic track		Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy	0.744 *** (0.080)	0.647 *** (0.132)	1.040 *** (0.042)	1.090 *** (0.053)	-0.027 (0.020)	-0.009 (0.026)
F-Statistic	85.56	23.89	607.54	414.90	1.46	0.12
State-specific trends in year of birth						
Linear	✓	✓	✓	✓	✓	✓
Squared		✓		✓		✓
Observations	17,428		9,918		17,428	

Notes: Each coefficient represents a separate linear regression. The F-Statistic gives the result of a significance test of the reform dummy in corresponding regressions. All regressions include state of residence-fixed effects, year of birth-fixed effects, state-specific trends (linear and quadratic) in year of birth, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: German Mikrozensus (MZ) 2008; own calculations.

Table 6: Baseline results: effect of education on fertility

	Full sample		Basic track	
	(1)	(2)	(3)	(4)
Panel A: number of children				
OLS	-0.020 *** (0.003)	-0.020 *** (0.003)	-0.134 *** (0.012)	-0.133 *** 0.012
IV	-0.146 *** (0.035)	-0.172 *** (0.050)	-0.117 *** (0.043)	-0.101 * (0.057)
Panel B: childlessness				
OLS	0.010 *** (0.001)	0.010 *** (0.001)	0.016 *** (0.003)	0.015 *** (0.004)
IV	0.060 *** (0.013)	0.051 *** (0.018)	0.050 *** (0.012)	0.020 (0.015)
State-specific trends in year of birth				
Linear	✓	✓	✓	✓
Squared		✓		✓
Observations	17,428		9,918	

Notes: Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, state-specific trends (linear and quadratic) in year of birth, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: German Mikrozensus (MZ) 2008; own calculations.

Table 7: Baseline results: the effect of education on the age-specific probability of childbearing

Age at birth	15-20	21-25	26-30	31-35	>35
OLS	-0.028 *** (0.002)	-0.039 *** (0.003)	0.001 (0.003)	0.029 *** (0.003)	0.015 *** (0.003)
IV	-0.057 ** (0.022)	-0.071 *** (0.027)	-0.006 (0.024)	-0.030 (0.045)	-0.017 (0.022)
First stage results					
Reform dummy			1.638*** (0.399)		
First stage F-statistic			16.83		
Observations			2,649		

Note: Probability of childbearing at a given age. Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, squared state-specific trends in year of birth, indicators for marital status, municipality size, and mothers' age at the first birth. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: SOEP 1984-2010; own calculations.

Table 8: The effect of education on childlessness and the number of children - alternative definition of the education variable

	Full sample		Basic track	
	(1)	(2)	(3)	(4)
Panel A: number of children				
OLS	-0.025 *** (0.003)	-0.024 *** (0.003)	-0.071 *** (0.008)	-0.071 *** (0.008)
IV	-0.489 *** (0.160)	-0.910 (0.564)	-0.395 * (0.204)	-0.323 (0.203)
Panel B: childlessness				
OLS	0.010 *** (0.001)	0.010 *** (0.001)	0.010 *** (0.002)	0.010 *** (0.002)
IV	0.144 ** (0.059)	0.066 (0.048)	0.167 *** (0.058)	0.270 (0.183)
First stage results				
Reform dummy	0.305 *** (0.304)	0.165 (0.127)	0.428 *** (0.102)	0.457 *** (0.094)
First stage F-statistic	9.86	1.68	19.70	20.50
State-specific trends in year of birth				
Linear	✓	✓	✓	✓
Squared		✓		✓
Observations	12,657		7,686	

Notes: Each coefficient represents a separate linear IV regression. Education variable is “length of education” defined as *graduation year - year of birth - 6*. All regressions include state of residence-fixed effects, year of birth-fixed effects, state-specific trends (linear and quadratic) in year of birth, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: German Mikrozensus (MZ) 2008; own calculations.

Table 9: The effect of education on childlessness and the number of children - alternative selection of analyzed cohorts

Window	4-year	5-year	Symmetric 6-year	7-year	Asymmetric 1938-1959
Number of Children					
IV	-0.187 *** (0.058)	-0.172 *** (0.050)	-0.144 *** (0.052)	-0.071 (0.058)	-0.019 (0.057)
Childlessness					
IV	0.034 * (0.020)	0.020 (0.015)	0.033 ** (0.015)	0.029 (0.018)	0.011 (0.018)
First stage results					
Reform dummy	0.708 *** (0.192)	0.647 *** (0.132)	0.725 *** (0.115)	0.672 *** (0.092)	0.740 *** (0.076)
First stage F-statistic	13.65	23.89	39.81	53.53	94.61
Observations	13,897	17,428	21,053	24,620	38310

Notes: Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, state-specific trends (linear and quadratic) in year of birth, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: German Mikrozensus (MZ) 2008; own calculations.

Table 10: The effect of education on timing of childbearing - alternative selection of analyzed cohorts

Age at birth	15-20	21-25	26-30	31-35	>35
5-year window					
IV	-0.029 (0.019)	-0.041 (0.027)	0.015 (0.026)	-0.015 (0.054)	-0.001 (0.019)
First stage results					
Reform dummy			1.839*** (0.475)		
First stage F-statistic			15.09		
Observations			1,911		
9-year window					
IV	-0.057* (0.031)	-0.070** (0.034)	-0.038 (0.031)	-0.027 (0.044)	0.021 (0.021)
First stage results					
Reform dummy			1.330*** (0.359)		
First stage F-statistic			13.66		
Observations			3,403		

Note: Probability of childbearing at a given age. Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, squared state-specific trends in year of birth, indicators for marital status, municipality size, and mothers' age at the first birth. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: SOEP 1984-2010; own calculations.

A Appendix

Table A.1: Effect of education on fertility - SOEP results

	7-year window		5-year window	
Panel A: number of children				
OLS	-0.004 (0.008)	-0.004 (0.008)	-0.007 (0.009)	-0.006 (0.009)
IV	-0.074 (0.099)	-0.154 (0.095)	-0.097 (0.096)	-0.036 (0.108)
Panel B: childlessness				
OLS	0.003 (0.003)	0.003 (0.003)	0.005 (0.003)	0.005 (0.003)
IV	0.009 (0.030)	0.047 ** (0.024)	0.026 (0.024)	0.004 (0.024)
First stage results				
Reform dummy	1.123 *** (0.299)	1.638 *** (0.369)	1.498 *** (0.359)	1.828 *** (0.445)
First stage F-statistic	14,05	19,68	17,38	16,88
State-specific trends in year of birth				
Linear	✓	✓	✓	✓
Squared		✓		✓
Observations	2,649		1,911	

Notes: Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, indicators for marital status, municipality size, and mothers' age at the first birth. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: SOEP 1984-2010; own calculations.

Table A.2: The effect of education on the timing of the first birth

Age at first birth	15-20	21-25	26-30	31-35	>35
OLS	-0.028*** (0.002)	-0.036*** (0.003)	-0.008** (0.003)	0.022*** (0.005)	0.009* (0.005)
IV	-0.057** (0.022)	-0.052 (0.034)	0.048 (0.045)	-0.154* (0.079)	-0.045 (0.081)
First-stage results					
Reform dummy	1.638*** (0.399)	1.396*** (0.407)	1.451*** (0.385)	1.458*** (0.461)	0.989* (0.539)
First stage F-statistic	16.83	11.76	14.20	10.00	3.38
Observations	2,649	2,219	1,453	849	611

Note: Probability of giving the first birth at a given age, conditioned on not already having a child. Each coefficient represents a separate linear regression. All regressions include state of residence-fixed effects, year of birth-fixed effects, squared state-specific trends in year of birth, indicators for marital status, municipality size, and mothers' age at the first birth. Standard errors in parentheses are adjusted for clusters at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.

Source: SOEP 1984-2010; own calculations.