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

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Abstract

This paper investigates the effect of education on fertility under inflexible labor market conditions. We exploit exogenous variation from a German compulsory schooling reform to deal with the endogeneity of education. By using data from two complementary data sets, we examine different fertility outcomes over the life cycle. In contrast to evidence for other developed countries, we find that increased education causally reduces completed fertility. This negative effect operates through a postponement of first births away from teenage years, and no catch-up later in life. We attribute these findings to the particularly high opportunity costs of child-rearing in Germany.

Keywords: fertility, education, childlessness, timing of births, educational reform
JEL: I21, J13, J24

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1. Introduction

Since the 1970s total fertility rates (TFR) have fallen substantially in most developed countries. The declining birth rates and increasing percentages of childless women accelerate population aging that has become a major demographic problem in the developed world. The causes of these negative fertility trends are not fully understood because various factors such as education, income, marital status, and labor market conditions are likely to influence fertility (D'Addio and D'Ercole, 2005).

Educational expansion is one of the much disputed determinants of low fertility. Early research has shown a significant negative correlation between women's educational attainment and fertility. However, because of potential reverse causality and selection on unobservable factors, a causal link is difficult to establish. More recent studies for several developed countries demonstrate that the relationship between education and fertility disappears or becomes even positive after accounting for the endogeneity of schooling (see, e.g., Monstad et al., 2008; Fort et al., 2011; McCrary and Royer, 2011). So far, however, the evidence for the effect of education on fertility is inconsistent and differs across countries.[1]

The controversy about the empirical evidence for the education-fertility-nexus draws attention to the importance of various institutional conditions such as labor market flexibility, child care availability, and a broad range of policies that vary across countries. This paper focuses on West Germany and thus studies an institutional setting marked by high wage penalties for motherhood, an inflexible labor market, and a very limited supply of public childcare (Charles and Luoh, 2003; Kreyenfeld, 2004). Low fertility is currently one of the major political issues in Germany and the debate continues about the best policies to mitigate the recent demographic changes. The fertility developments experienced by Germany over the past decades remain almost unprecedented. For example, the total fertility rate fell below the replacement level of 2.1 children per woman already in 1970 and has since decreased to 1.4 (Federal Bureau of Statistics, 2007; World Bank, 2012). In

[1]The causal studies for the U.S. and several European countries usually show that extended education leads to postponement of first births away from teenage motherhood (see, e.g., Black et al., 2008; Monstad et al., 2008; Silles, 2011). However, the evidence for the effect on completed fertility is mixed (see, e.g., Monstad et al., 2008; Fort et al., 2011; Braakmann, 2011). We give an overview of the existing evidence in Section 3.

2009, German women gave their first birth on average at age 30. This age at first birth is one of the highest among OECD countries (OECD, 2011). Furthermore, high numbers of childless women contribute to the low birth rates (OECD, 2011).

This paper traces the effect of education on fertility over the life cycle. We examine different outcomes: the number of children, the probability of remaining childless, and the timing of births. We use two complementary datasets, the German Mikrozensus and the German Socio-Economic Panel, to investigate fertility of West German women born between 1937 and 1961. To deal with potential omitted variable bias, we explore a reform that extended mandatory schooling from 8 to 9 years. In particular, our identification strategy takes advantage of exogenous variation in education from the staggered implementation of the reform in West German states. We identify the causal effect of education on fertility by applying an instrumental variables approach.

We find that increased education permanently reduces fertility in Germany. More specifically, one additional year of schooling ultimately reduces the number of children by more than 0.1, and increases the probability of childlessness by about 2-5 percentage points. These results contradict prior evidence for other developed countries (see, e.g., Monstad et al., 2008; Silles, 2011; Geruso et al., 2011). Our complementary analysis of the timing of births contributes to the understanding of underlying mechanisms and cross-country differences. We confirm the findings from other countries that increased education decreases the probability of teenage motherhood (see, e.g., Black et al., 2008; Monstad et al., 2008).[1] However, in contrast to women from other countries, German women do not compensate at later ages for the initial loss in births. Clearly, this lack of a catch-up effect translates to a negative effect of education on completed fertility. We argue that contradictory evidence for the effect of education on completed fertility may derive from different institutional conditions that affect women's opportunity costs over their life course. Our results pass usual specification tests and are robust to different sample restrictions.

The structure of this paper is as follows: Section 2 briefly introduces the German school system and the reform of compulsory schooling. Section 3

[1]There is a general agreement that the negative effect of education on teenage fertility may simply reflect the incompatibility of schooling and motherhood.

gives an overview of the literature, and derives our hypotheses. Section 4 introduces our empirical approach, and Section 5 describes the data. Section 6 gives our main results, and Section 7 provides the results of several specification and robustness tests. Section 8 concludes.

2. Institutional setting

The German secondary school system is a tripartite system that sorts students at the age of 10 into school tracks.[2] The three school tracks, basic school (*Hauptschule*), middle school (*Realschule*), and high school (*Gymnasium*), prepare for different careers. Basic school lasts until grades 8 or 9 and prepares for apprenticeship trainings. Middle school comprises 10 grades and prepares for apprenticeships and training in white collar jobs. High school certificate after 12 years (*Fachhochschulreife*) gives access to technical colleges (*Fachhochschule*), and after 13 years (*Abitur*) to universities.

The sorting of students into the different tracks depends on various criteria and differs by state.[3] 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 and Schneider, 2007).

Figure 1 gives the percentages of graduates from the three tracks in the birth cohorts 1932-1965. We observe lower percentages of basic school graduates and higher percentages of middle and high school graduates in the younger cohorts. The percentages of basic school graduates in the cohorts born before 1935 was over 80% and fell to less than 40% in the cohorts born after 1960. Accordingly, in the observed period the percentages of high school graduates increased from about 8% to about 23%. Figure 1 illustrates that educational expansion in Germany led to a sizeable shift of graduates away from basic schools.

[Figure 1 about here]

[2] 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.

[3] In some states parents decide about the track that their child attends after 4 years of primary school. In others primary school grades determine the eligible tracks. In most states sorting depends on both the recommendation of primary school teachers and the parents' choice (see KMK (2010a) for a description of transition processes to secondary school tracks, and KMK (2010b) for a detailed description of the school system).

The reform that we use for our identification strategy extended compulsory schooling by one year (from 8 to 9 years) between 1946 and 1969.[4] Petzold (1981) specifies two major goals of the reform. First, the reform aimed at improving vocational maturity, the physical and psychological development of children, and the quality of occupational choices because 14 year old pupils were considered to be too immature for the labor market. Second, the reform aimed at directing young workers away from manual to more intellectually demanding jobs. This argument was related to the high level of youth unemployment and a shortfall of apprenticeship training positions in the 1950s.

[Table 1 about here]

The German school system is governed at the level of the federal states. This decentralized organization lead to a staggered implementation of the 9th grade by state. Table 1 shows the year of 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 World War II, others such as Bavaria postponed the reform until the 1960s.[5] 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.[6]

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.

One of the most discussed channels is the labor market channel as proposed in the standard microeconomic model of fertility (Becker et al., 1960).

[4]Some studies already exploited the German reform of the extension of compulsory schooling. See, e.g., Pischke and von Wachter (2008), Brunello et al. (2009), Siedler (2010), Kemptner et al. (2011), Piopiunik (2011), Fort et al. (2011).

[5]The timing of the reform at the local level varied also within the states because of shortfalls of teachers and a lack of infrastructure in rural areas (Leschinsky and Roeder, 1980).

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

The model assumes that education increases a woman's permanent wage, but the consequent effect on fertility is unclear. On the one hand, higher earnings raise 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, higher earnings should be positively related to fertility because families can afford more children (Becker et al., 1960). This income effect may be however weaker if parents with higher income prefer children of higher quality (Becker and Lewis, 1973). The more parents invest in each child, the fewer children they can afford.

Previous literature emphasizes also the role of several other channels. For example, under positive assortative mating, a woman's education is causally related to her partner's education (Behrman and Rosenzweig, 2002). An exogenous increase in a woman's education affects her permanent income through a spouse-related multiplier effect. Another causal mechanism works through the effect of education on women's knowledge about contraception or reproductive health (Grossman, 1972; Rosenzweig and Schultz, 1989). Education may also affect fertility through what has been termed 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 and Rønsen, 2005).

Although an extensive empirical literature documents a negative association between female education and fertility, a causal relationship is difficult to establish because of potential reverse causality and selection on unobservable factors. Some recent studies approach these problems by using exogenous variation from school entry policies (see, e.g., McCrary and 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. So far, however, the these studies offer mixed findings as we demonstrate in Table 2.

[Table 2 about here]

Table 2 shows that that studies for countries and population groups with higher levels of fertility generally find negative effects of education on fertility (see, e.g., Osili and Long (2008) for Nigeria, Lavy and Zablotsky (2011) for Arabs in Israel). Analyses using 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, Geruso et al. (2011) for U.K.). McCrary and 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) show contradictory evidence from mandatory schooling reforms in Europe.[7] Their results suggest that more education significantly increases the number of births.[8] The estimated effects may differ across studies because education affects fertility through different channels, but also because the importance of these channels may vary across countries, subpopulations, or levels of education.

As for Germany, the related literature on mothers' labor market outcomes generally agrees that the opportunity costs of child rearing are particularly high because the institutional framework hampers the compatibility of work and family life. For example, West German mothers experience low coverage of public childcare services (see, e.g., D'Addio and D'Ercole, 2005; Wrohlich, 2008). A considerable excess demand for subsidized childcare is related both to the highly regulated market for childcare (Evers et al., 2005) and to the high cost of private childcare alternatives (Wrohlich, 2008). Furthermore, a general skepticism about the quality of public childcare (Blau and Hagy, 1998; Spie and Tietze, 2002) and social attitudes against the employment of mothers (Hank et al., 2004; Lee et al., 2007) may exacerbate the problem of low coverage. In addition, compared to mothers from other developed countries, German mothers experience relatively high wage losses from child-related employment interruptions (Gangl and 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),

[7] Closest to ours is the study by Fort et al. (2011) who examine fertility using variation from compulsory schooling reforms in eight European countries. However, this study considers only the four German states that introduced the reform simultaneously in 1967.

[8] The authors attribute their finding to a positive effect of education on the stability of marriages.

or to work part time (see, e.g., Sainsbury, 1999; Geyer and Steiner, 2007; Dearing et al., 2007; Hanel and Riphahn, 2012).

Given the German institutional framework, we test the hypothesis that education reduces fertility not only temporary, but also permanently. Specifically, we expect that an extension of mandatory schooling generates a postponement of first births because education and childrearing are difficult to combine. Furthermore, we expect that additional education increases the probability of remaining childless and reduces the number of births because of increased opportunity costs of child rearing associated with an extra year of schooling.

4. Identification strategy

Our identification strategy considers the following equations:

$$y_i = \alpha \; educ_i + state_i'\theta + cohort_i'\xi + statetrend_i'\mu + z_i'\pi + \epsilon_i \qquad (1)$$

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

where the dependent variable y_i in (1) represents different measures for woman i's fertility outcomes. We consider three fertility outcomes: the number of children ever born, the probability of remaining childless, and the age-specific probability 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.[9] α, θ, ξ, μ, π, ϕ, β,γ, δ, λ represent coefficients to be estimated, ϵ_i and ν_i are random error terms.

Estimating equation (1) by OLS is likely to yield biased estimates for the impact of education on fertility because the error term ϵ_i may contain 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.

[9]If the data allows, we also control for women's family background, e.g., their own mothers' age of birth. State-specific cohort trends are used both in linear and squared forms.

We exploit exogenous changes in education from the schooling reform described in Section 2 to identify the causal effect of education on fertility outcomes. In particular, we exploit the cross-regional and cross-time variation in education from a staggered implementation in German states between 1958 and 1969.[10] In a two stage least squares approach we first estimate equation (2) that gives years of education of individual i ($educ_i$) as a function of an indicator for the reform status ($reform_i$). The reform indicator equals 1 if a woman was affected by the reform, and 0 otherwise. In the second step we estimate equation (1). The two equations use exactly the same control variables.

Our identification strategy fails if the timing of the reform is correlated with fertility in the federal states. To mitigate this concern we also include linear and squared state-specific cohort trends that should capture any smooth trends in fertility and schooling at the state level. We also include state fixed effects to control for state-specific differences such as religious affiliation or social norms, which may have also influenced the timing of the reform. Given that our estimation strategy relies on a long period, we use cohort fixed effects to account for any changes that took place over time such as introduction of oral contraceptives.[11]

Our identification strategy also fails if other fertility-relevant changes took place and affected the same birth cohorts as the schooling reform did. However, such changes are unlikely because the overall responsibility for family policies lies with the federal government, not with the states. Although the state governments enforce enacted laws or reforms and are free to extend them (Gerlach, 2010), any important family policy reform in the last 60 years such as the introduction of child benefits (*Kindergeld*) affected women in all states simultaneously.[12]

[10]Because of data limitations we exclude Hamburg and Schleswig-Holstein from the analysis. Section 5 gives more details on our data.

[11]We argue that our results are not driven by the set of states and the consequent set of cohorts. Sensitivity tests show that our findings qualitatively do not change if we exclude single states and thereby change the period of analysis. Detailed results available upon request.

[12]Similarly, the health care system is governed at the federal level, so the timing of the introduction of the contraceptive pill was not state-specific. This argument applies also to the reimbursement of oral contraceptives by health insurances. Some health insurances operate regionally (e.g., company health insurance funds), but the timing of first reimbursements did unlikely vary across health insurances. Nevertheless, we tested whether

5. Data

We use data from two complementary German surveys to examine the relationship between women's education and her fertility outcomes such as number of children, childlessness, and the age-specific probability of births.

Our primary data source is the German Microzensus (MZ). The MZ is an annual survey of a 1% random sample of German households.[13] We use the 2008 survey, which is the first survey providing information on the number of children ever born to female respondents.[14] The key advantage of the MZ is its large sample size and low unit and item non-response rates. However, the survey suffers from the lack of information on children's birth year, the state where an individual went to school, and parental background.

Our second data source is the German Socio-Economic Panel (SOEP). The SOEP is a longitudinal survey of private households, conducted annually since 1984 (Haisken-DeNew and Frick, 2005; Wagner et al., 2007). The data set overcomes three important shortcomings of the MZ. First, the SOEP contains retrospective biographical information on childbearing, thereby permitting our analysis of the timing of births over the life cycle. 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 SOEP provides 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 observations from all available survey years, 1984-2010, to obtain a sufficient number of women for the treatment and control groups. In particular, we consider only the first interview of a woman conducted after she has turned 40 years old.

We impose similar sample restrictions on both the MZ and SOEP data. Table 3 demonstrates the details of our sample selection procedure.

[Table 3 about here]

We select native German women[15] from eight out of ten (excluding Berlin)

our results are driven by particular states by excluding single states from the analysis. The results remained qualitatively unchanged. Detailed results are available upon request.

[13]The scientific use file is a 70% sample of the entire data set.

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

[15]We omit first and second generation immigrants.

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 reports fertility only for women born after 1933.[16] 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 illustrated in Figure 2. Because the small sample size of the SOEP does not allow us to use a 5-year time window around the first birth cohort affected by the reform, we use a 7-year window instead. Ideally we would need 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.[17] We also exclude from the sample women who graduated from a school in socialist East Germany.[18]

Neither of the two data sets report the exact number of completed years of schooling. Instead, we observe a woman's highest secondary school degree, post-secondary education, and training. We construct years of schooling by assigning the usual number of years taken for a particular educational route (see, e.g., Krueger and Pischke, 1995; Pischke and von Wachter, 2008). We proceed as follows: For the basic track graduates, we use the state, 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 two higher tracks, we use the standard duration for graduating from a particular track. Finally, for all individuals, we incorporate the information on post-secondary education and training, thereby calculating a measure of total number of years of education (Krueger and

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

[17]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 and von Wachter (2008), that regional mobility in Germany is so low (see, e.g., Harhoff and Kane, 1997) that this should yield at most minor consequences for our estimates.

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

Pischke, 1995).[19] We conclude our sample selection by omitting observations with less than 7 years of education and with missing values on education or fertility variables.[20]

Our final MZ sample contains 17,428 women born 1938-1959; the SOEP sample, 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 in the MZ 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 the treatment and control groups in the SOEP. Nevertheless, women affected by the reform were more likely to delay their first birth. Fertility variation by reform status may reflect differences in birth cohort and educational attainment. Our sample selection rules determine that women affected by the reform were born later. As expected, they also completed 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 percentages of women obtained 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, with small differences between women affected and not affected by the reform. Figure 4 shows that women who were subject to the reform postponed their first birth beyond the early 20s, and were more likely to have their first child at older ages compared to women who were not subject to the reform. Further, women affected by the reform had fewer children up to the age of 36, but then they fully caught up, so that the

[19]In Section 7 we show that our results are robust to a definition of the education variable. For example, we use the information on graduation year to calculate the actual time that a woman remained in educational system.

[20]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.

completed fertility at the end of the fertile years is nearly identical in both groups.

6. Results

6.1. The effect of the reform on education

We first graphically explore whether the formal introduction of the compulsory 9th grade affected education (first stage) and fertility (reduced form). Figure 5 plots the average number of years of education for the five birth cohorts before and after the reform using the MZ data.[21] The graph reveals a general increase in education for younger cohorts, but also a discrete jump of roughly 0.85 years for the first birth cohort affected by the reform. Figure 6 illustrates the reduced form analysis. We plot the average number of children for the five birth cohorts before and after the reform. The adjusted number of children is the residual from a regression of the number of children on the full set of our control variables.[22] The reduced form graph signals a negative jump in fertility for the first birth cohort affected by the schooling reform.

[Figure 5 about here]

We next estimate equation 2 using the MZ sample and show the first-stage estimation results in Table 5. Columns 1 and 2 report 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 on track choice. We show the coefficients obtained from two specifications: one comprises only linear state-specific trends in birth cohort, the other one also includes 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).[23]

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[21]Note that we constructed the education variable by combining the information on a woman's highest completed degree and the total number of years usually taken for this degree. We show the robustness of our results to an alternative measure for education in Section 7.

[22]These control variables are state fixed effects, year of birth fixed effects, linear and quadratic state-specific trends in year of birth, and marital status.

[23]Following Angrist and Pischke (2009), we report conservative inference results. We obtain smaller standard errors from alternative methods such as clustering by state as

[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 all specifications and indicates that the reform increased women's education on average by about 0.65-0.75 years (columns 1 and 2). The magnitudes of our estimates are larger than those reported by other studies that exploit the German school reform (see, e.g., Pischke and von Wachter, 2008; Siedler, 2010; Kemptner et al., 2011; Piopiunik, 2011). We argue that these differences result mainly from different sample restrictions. For example, Pischke and von Wachter (2008) report a first-stage estimate of 0.19, but the authors use a different data set, pool males and females, investigate only salaried workers in the age group 15 to 65, and calculate a different education variable.[24] In the previous version of their paper Pischke and von Wachter (2005) report a first-stage coefficient from the German Mikrozensus of 0.569. Using our data and model specification, we re-estimated the first stage in a pooled sample of men and women, and separately by gender. Not only we nearly replicated the coefficient reported by Pischke and von Wachter (2005), but we also found that the schooling reform affected women much strongly than men.[25]

Several sources of measurement error can contribute to an attenuation of the reform indicator coefficient: First, the implementation of the additional grade may not have been immediate and in practice required several years until each school offered the mandatory ninth grade (Leschinsky and Roeder, 1980). Second, the direct information on the state of school attendance is unavailable, 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 may have started primary school a year earlier or later than officially scheduled. However, we draw on previous studies (see, e.g., Pischke and von Wachter, 2008; Siedler, 2010; Kemptner et al., 2011; Piopiunik, 2011) and argue that measurement error in

proposed by Bertrand et al. (2004), or the two-way clustering suggested in Cameron and Miller (2011). Detailed results available upon request.

[24] Pischke and von Wachter (2008) construct a measure of education from the information about birth and graduation year. We demonstrate in Section 7 that our first-stage coefficients diminish if we use this alternative education variable.

[25] Our estimations yield a first-stage coefficient of 0.647 for women and 0.391 for men. Detailed replication results are available upon request.

the instrument does not bias subsequent estimates unless it is 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 are greater than 10 across specifications and confirm the strength of the instrument.

Our estimation strategy identifies the *local average treatment effect* (LATE) of education on fertility (see Imbens and Angrist, 1994). Compliers in this setting are all women who's education was causally affected by the extension of compulsory schooling, i.e., women who did not leave school after 8 years because of the reform, regardless of the attended track. This group of women is not necessarily representative for the overall population.

An important assumption for an unbiased estimate of the LATE is *monotonicity*, i.e., that all individuals respond to the reform implementation in the same way (Imbens and Angrist, 1994). In this particular case, we assume that all women affected by the schooling reform extended their education, and that no woman reduced 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 graduating from the basic track (columns 5 and 6 in Table 5). The coefficients are very small, negative and insignificant. Therefore, consistent with previous findings, we do not find evidence that the reform affected track choice (see, e.g., Pischke and von Wachter, 2008; Kemptner et al., 2011; Piopiunik, 2011).[26]

6.2. Effect of education on completed fertility

Table 6 reports our key results on the effect of education on the number of children and the probability of remaining childless estimated separately for the full sample (columns 1 and 2) and the subsample of basic track graduates (columns 3 and 4).

[Table 6 about here]

The OLS results indicate that an additional year of education is associated with a lower number of children (Panel A, first row) and a higher probability of remaining childless (Panel B, first row). The magnitude of

[26]Kemptner et al. (2011) conclude that also changes in the composition of students within tracks because of the reform should be very small if the track choice is not affected.

the coefficients is comparable to results of other studies. The OLS estimates are statistically significant at the 1% level across specifications, samples, and fertility outcomes. However, the OLS regressions do not account for possible selection into education and may therefore yield biased results. The direction of the bias is not clear. Because 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 after controlling for possible endogeneity.[27]

Our main results are robust to changes in specification of state-specific birth cohort trends. However, we argue that squared trends better mitigate the concern that the introduction of the reform could be correlated with trends in education and fertility. The coefficient obtained from our preferred specification (Panel A, column 2) suggests that on the margin, one year of additional education increases the probability of remaining childless by 5.1 percentage points. However, the effect of education on childlessness is smaller and not statistically significant in the basic track subsample (Panel A, column 4).[28] The magnitude of the effect on the number of children

[27] We 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). For the SOEP sample we select a 7-year window to prevent inconsistency from a small sample size. The signs of coefficients obtained using a 5-year window remain the same, but the standard errors are larger. Furthermore, the results for basic track graduates are similar to those obtained with the MZ data. Detailed results are available upon request.

[28] The differences between the full sample and the basic track subsample may be a result of other channels (e.g. health related knowledge, assortative mating, or opportunity costs) through which the reform affected fertility. On the one hand, we would expect to observe a more pronounced effect in the basic track subsample because basic track was directly affected by the reform. On the other, also women attending middle or academic track may have been compliers and thus be affected by the reform (e.g, if decided against drop-out). Monstad et al. (2008) argue that a schooling reform might have affected women such that they chose a different "life track" as they would have without the reform implementation. Furthermore, Lang and Kropp (1986) show that under the signaling and screening hypothesis, compulsory attendance laws affect all educational groups. However, because of noisy IV estimates the differences in the effects between the basic track sample and the full sample are not significant.

also varies between the full sample and the basic track subsample (Panel B, columns 2 and 4). However, the coefficient is negative and significant throughout, and demonstrates that an additional year of education reduces fertility by at least 0.1 children.[29]

6.3. Effect of education on fertility over the life cycle

Table 7 gives the results for the analysis of the timing of the first birth. Using the SOEP data we estimate the effect of education on the probability of giving first birth at a given age, conditional on not already having a child. For example, women who gave their first birth between age 15 and 20 are omitted in the estimation for the age group 21-25, and the number of observations falls from 2,649 to 2,219. All regressions control for state of residence, year of birth, linear and quadratic state-specific trends in year of birth, marital status, age of the mother at the woman's birth, and municipality size.

[Table 7 about here]

The OLS estimations suggest that an increase in education is related to a reduced probability of first birth below the age of 30 and increased probability of first birth at the end of women's fertile period. However, the IV results yield different conclusions. The point coefficients suggest a positive effect of education on the probability of first birth only for the age group 26-30, but the coefficient is statistically insignificant. For the remaining age groups the coefficients translate to negative effects of increased education. As for women aged 15-20 the coefficient is significant at the 5% level and of a large magnitude: each additional year of education reduces the probability of first motherhood as a teenager by 5.7 percentage points. This is a considerable impact relative to the incidence of teenage childbearing in the full sample of 16%. The first-stage F-statistic of 16.83 indicates that a weak instrument is not a concern. Furthermore, we find a large and significant effect of education on the probability of the first birth at ages 31-35: a one-year increase in education decreases the probability of first motherhood at ages 30-34 by 15.4 percentage points. The incidence of giving birth in this age group is roughly

[29]These results reflect a combined effect of different channels though which extended education affects completed fertility, e.g., increased human capital, an age effect associated with a longer stay in school, or assortative mating. Unfortunately, we can not disentangle here these different mechanisms.

9% in the full sample. This large negative effect is accompanied by a zero effect for the subsequent age group and suggests that women in their early 30s are more likely to remain childless than to bear children later in life.

For completeness, we incorporate the information on the timing of the subsequent births and estimate the effect of education on cumulative fertility at a given age. This cumulative fertility is the number of children born up to a specific age and we estimate the effect separately for one-year age intervals between 15 and 45 by 2SLS. All of these 31 regressions control for state of residence, year of birth, linear and quadratic state-specific trends in year of birth, marital status, and municipality size. Figure 7 summarizes the results obtained from these linear regressions.

[Figure 7 about here]

We plot the coefficients on education and 90% confidence intervals around these point estimates by age. The IV estimates give the effect of education on the number of children born up to a given age. Figure 7 reveals more heterogeneity in the effects of education on teenage childbearing than Table 7. Specifically, at age 18, we observe a positive coefficient, which could indicate a catch-up effect after graduation from school. Women may potentially compensate for the earlier loss in births from "incarceration" by extended schooling, but the effect at age 18 is statistically indistinguishable from zero. Figure 7 generally confirms fertility reduction in teenage years. The remaining coefficients indicate a negative effect of education on cumulative fertility throughout. Therefore, the effect of education on completed fertility over the life cycle is negative. In contrast to the findings for the U.K. by Geruso et al. (2011) who find an upward trend and a catch-up effect in births, our point estimates follow a downward trend, i.e., in Germany the fertility losses from increased education become more severe with increasing age. Although for several age-years after the teenage years the coefficients are insignificant and small, thereby implying some catch-up effects, for age 33 and later the effect of education on cumulative fertility is clearly negative, thereby rejecting a complete catch-up. The effect at age 45 is identical with the effect on the total number of children from Section 6.2 and confirms that additional education leads to a permanent reduction in completed fertility.

Overall, our results suggest that the impact of increased education is to lower the number of children and to raise the probability of childlessness. Our findings for Germany contradict the 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 compensation for the initial loss in births. So far, however, there is no evidence that education causally reduces completed fertility in a developed country. While birth postponement away from early motherhood in response to extended education may reflect a pure "incarceration" effect, reduced childbearing in the 30s must work through higher opportunity costs. This mechanism is consistent with the previous evidence demonstrating that German women experience higher wage penalties for motherhood compared to women in other European countries (Gangl and Ziefle, 2009). Still, the question remains whether the reduced childbearing at older ages is driven by women's choice to remain childless, or by 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 education variable and in 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 define an alternative variable - "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.[30] The "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. This alternative variable captures skipped and repeated classes, and the short

[30]The 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 as reliable if the "length of schooling" was not more than up to two years longer or shorter than the standard duration of 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.

and long school years, which some states performed simultaneously to the extension of compulsory schooling (Pischke, 2007). 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".[31] Overall, our main results qualitatively do not change, but there are some differences. For example, the first-stage coefficients are below 1 across all specifications. These result may be driven by measurement error in the alternative education variable, the short school years, or measurement error in the timing of the reform. Furthermore, the values of the first-stage F-statistics are lower than those shown in Table 6. Finally, in the full sample (columns 1 and 2) the significance of the results varies with the specification of the used trends in birth cohort, but the sign of the coefficient remains negative.

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 and follow Monstad et al. (2008) and Brunello et al. (2009) who 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 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 use all individuals born between 1938 and 1959 (see, e.g., Pischke and von Wachter, 2008; Piopiunik, 2011). We next estimate the effect of education on completed fertility and childlessness using different cohort windows around the reform: a 4 to 7-year symmetric window and an asymmetric window.

[Table 9 about here]

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

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. We observe a consistent pattern that 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. Table 9 exemplifies the argumentation from the previous literature that smaller windows allow for a more local identification.

We apply similar robustness checks for our main results on the timing of first births obtained with the SOEP sample. We repeat the regressions using alternative definition of the window around the first birth cohort affected by the reform. Table 10 reports the results on the probability of giving first 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 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 the first birth at a particular age remains unaffected. The results obtained using a 9-year window confirm our baseline results shown in Table 7. The effect of education on giving a first birth at a given age is negative for age group 15-20. Consistent with the results obtained with the MZ sample, the effects are smaller for a wider window if we compare the 9-year and 7-year window.

8. Conclusions

We examine the effect of women's education on fertility and track this effect over the life cycle. 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 the number of children, probability of ultimate childlessness, and timing of births. Our empirical approach takes advantage of the exogenous variation in the reform's timing across federal states and time.

Consistent with causal evidence for other developed countries (see, e.g., Monstad et al., 2008; Grönqvist and Hall, 2011), we show that extended education leads to a birth postponement away from early motherhood. However, we also find that additional education significantly lowers fertility later in life. In contrast to previous studies for developed countries, we find a negative effect of education on completed fertility.[32] In particular, one additional year of education raises the probability of childlessness by at least 2 percentage points. This effect is considerable compared to the average incidence of childlessness of almost 20% 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 reduces the number of births per woman by more than 0.1 children. This decrease accounts for about 6% of average cohort-specific fertility. We show that our main findings are robust to changes in sample selection criteria and alternative definition of the education variable.

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 Germany mainly as the result of high opportunity costs of childrearing, compared to other countries. Specifically, our results are consistent with previous evidence that German mothers experience the highest wage penalty for motherhood in the Western world (Gangl and Ziefle, 2009). Gangl and Ziefle (2009) find that the high permanent wage losses appear to be related to statistical discrimination against mothers in the German labor market.

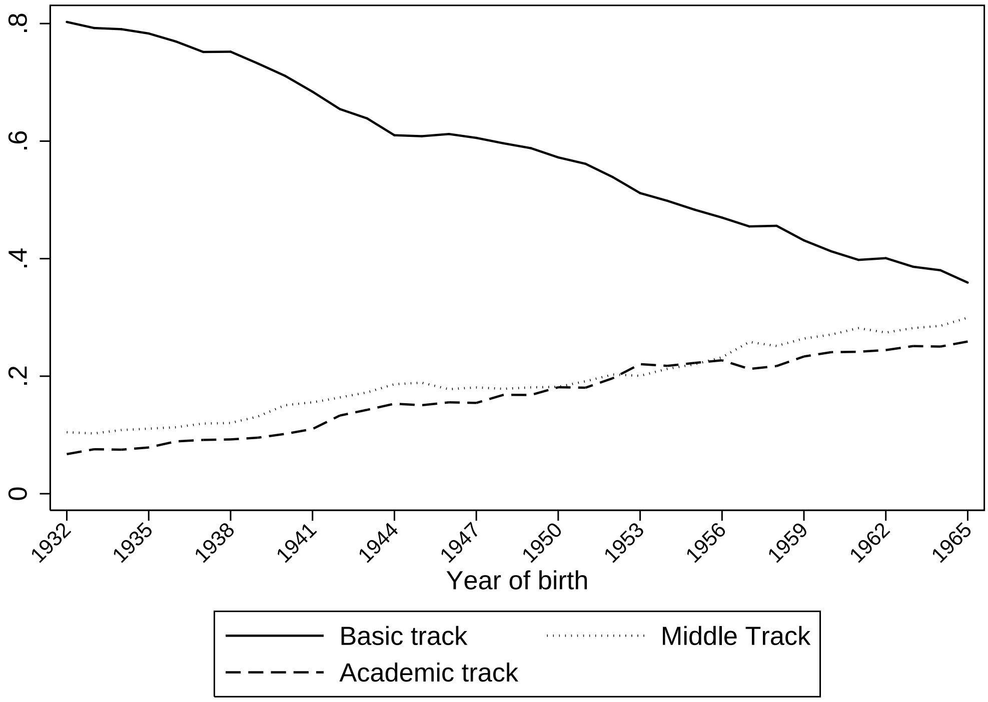
Several features of the institutional and cultural environment in Germany impede the compatibility of family and work, and may therefore reinforce employers' discrimination against mothers. Most striking is the highly inflexible system of childcare that translates to a considerable excess demand for subsidized childcare (D'Addio and 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 (Gornick et al., 1998; Kreyenfeld, 2004).

Our explanation of the negative effect of education on fertility may appear inconsistent with the findings of Pischke and von Wachter (2008) who determine no returns to schooling from the same reform. Their study, however, pays little attention to potential heterogeneous effects for men and woman

[32]The 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 decrease completed fertility (see, e.g., Black et al., 2008; Monstad et al., 2008; Silles, 2011).

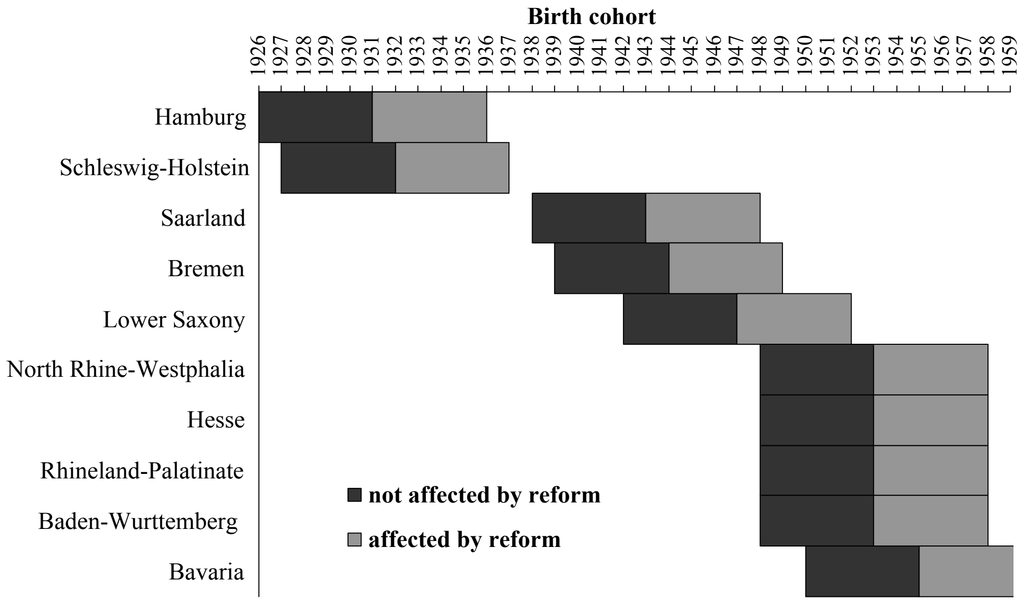
and we find that the reform affected women and men differently. Other previous studies for Germany suggest higher returns to education for women than for men (see, e.g., Lauer and Steiner, 2001; Boockmann and Steiner, 2006). We leave the investigation of the exact effect on earnings for future research.

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



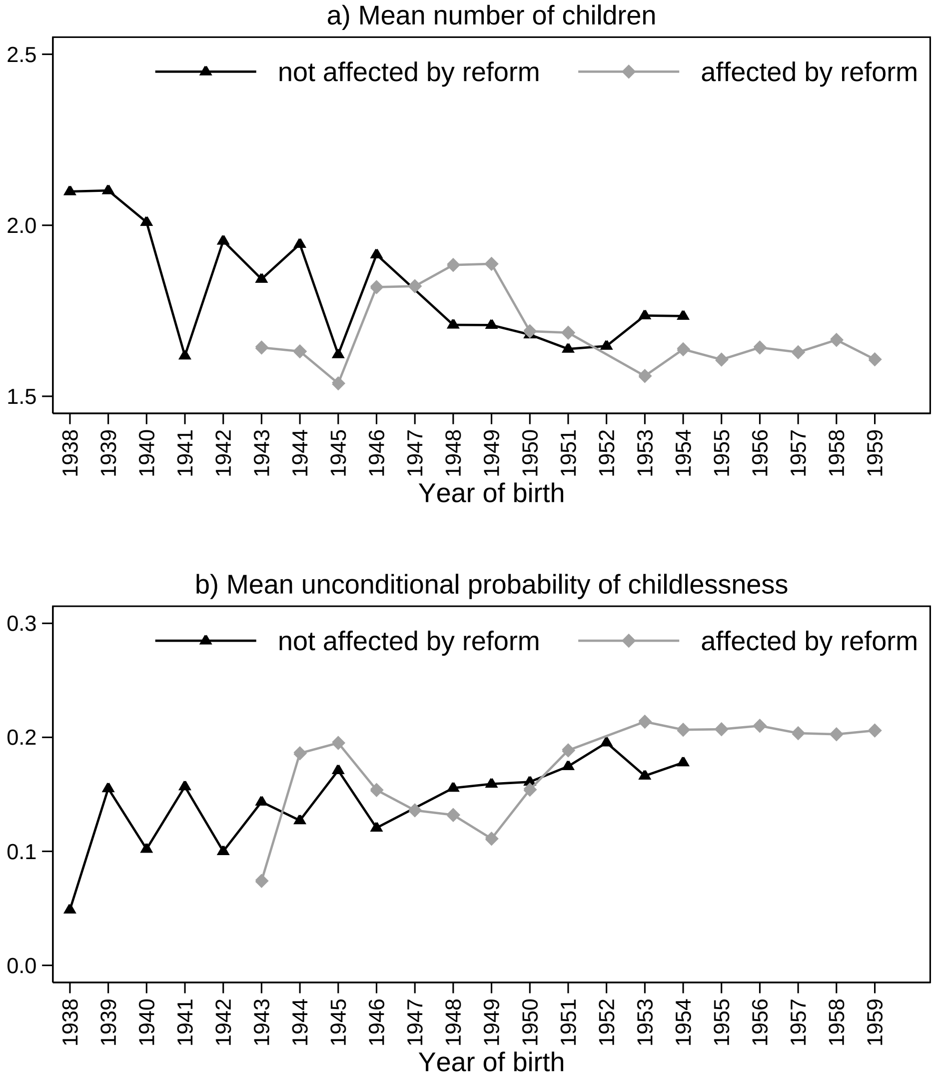
Source: German Mikrozensus (MZ) 2008; own calculations.

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



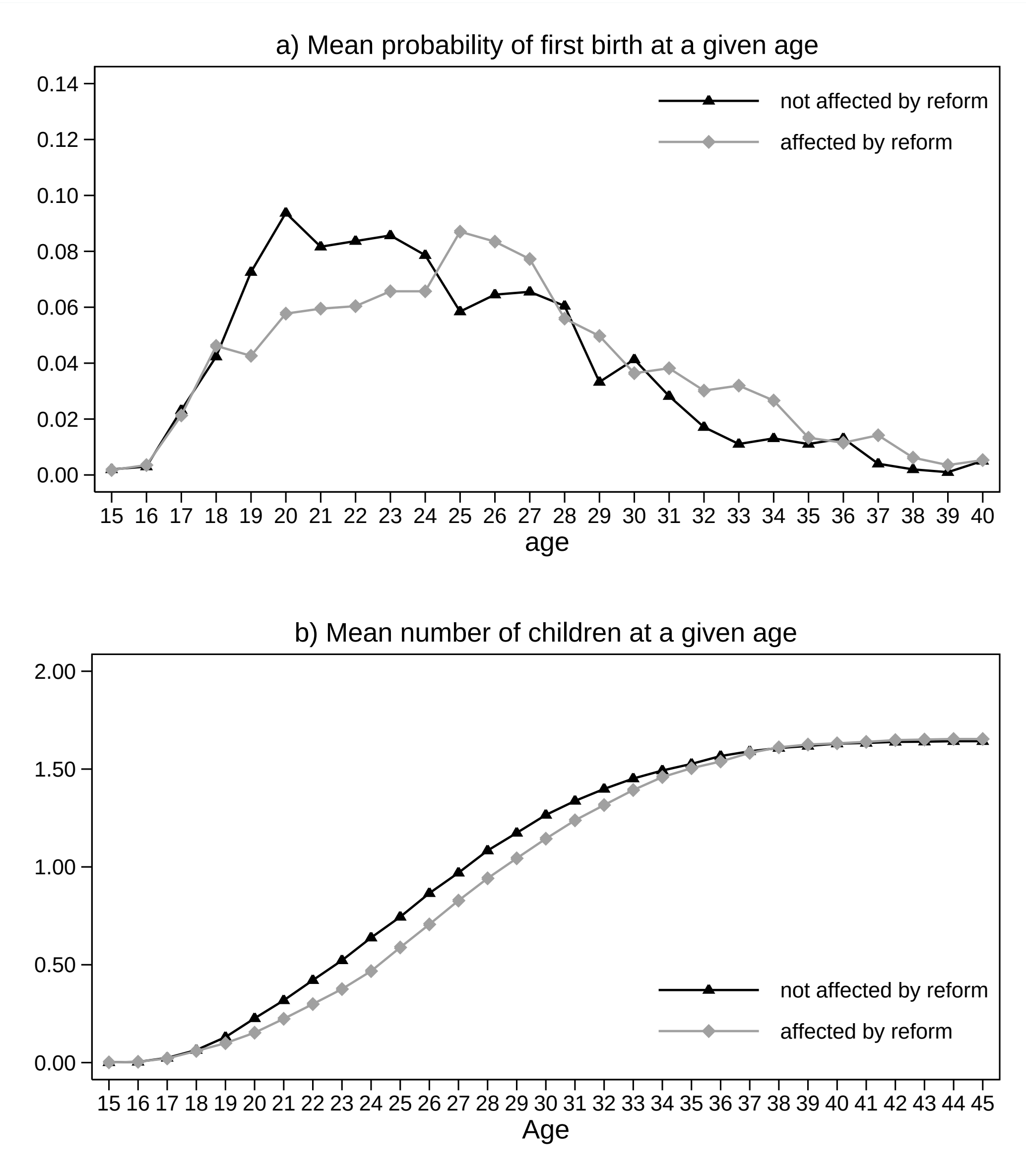
Note: We exclude Schleswig-Holstein and Hamburg from the analysis because the MZ 2008 reports fertility only for women born after 1933.
Source: Leschinsky and Roeder (1980); own illustration.

Figure 3: Fertility outcomes by birth cohort and reform status



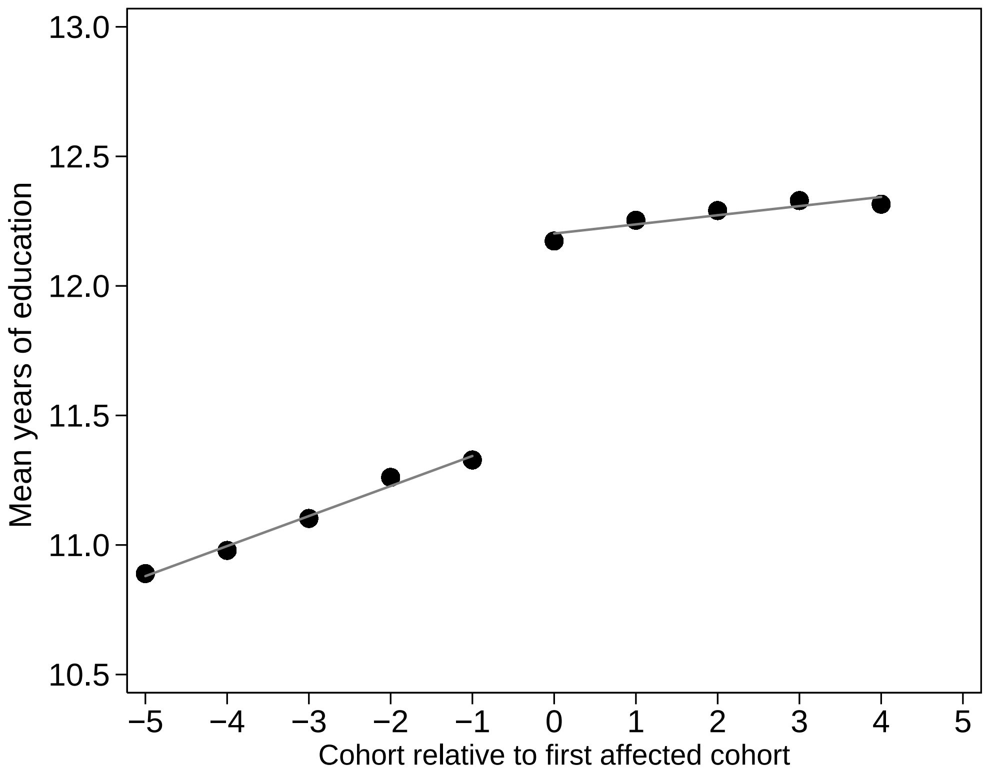
Note: The plots show unweighted raw data.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 4: Timing of births by age and reform status



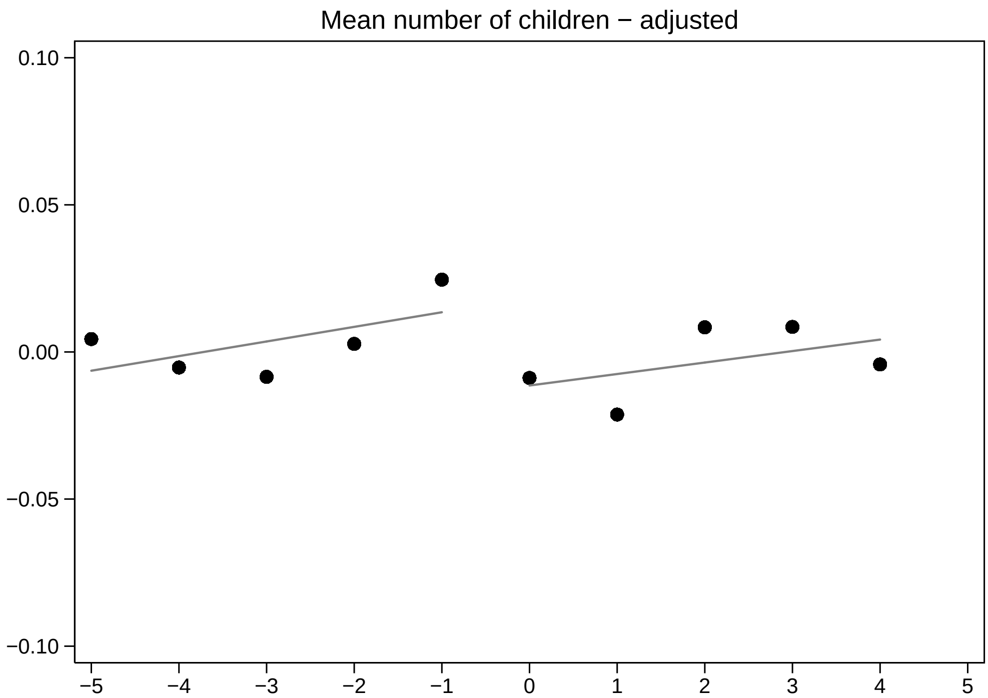
Note: The plots show unweighted raw data.
Source: SOEP 1984-2010; own calculations.

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



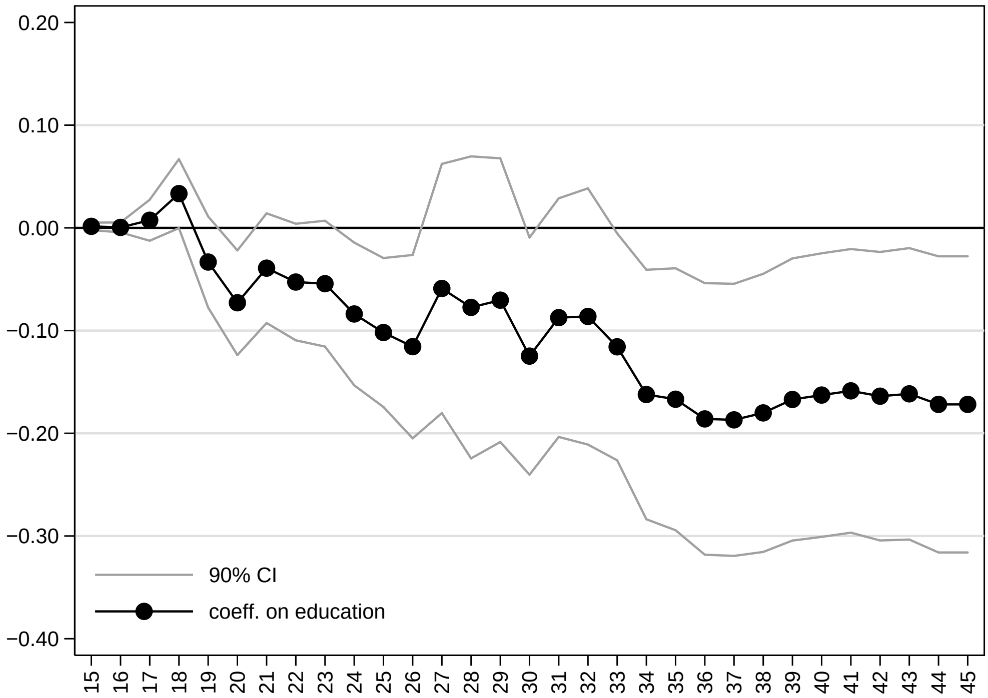
Note: The plot shows unweighted raw data.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 6: Reduced form: effect of the reform on adjusted number of children



Note: The adjusted number of children is the residual from a regression of the number of children on the full set of our control variables. These control variables are state fixed effects, year of birth fixed effects, linear and quadratic state-specific trends in year of birth, and marital status.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 7: Effect of education on fertility over the life cycle



Note: Dependent variable is the number of children at a given age. Each dot shows a coefficient on education obtained from a separate linear regression. Grey lines are 90% confidence intervals around the point estimate. All regressions also 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. 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-Wurttemberg	1967	1953
Bavaria	1969	1955

Note: Year of the introduction of the 9th grade in secondary schooling in West Germany and the first affected birth cohort.
Source: Leschinsky and Roeder (1980).

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

Dependent variable	Number of children	Age of first birth	Country
Osili and Long (2008)	negative		Nigeria
Lavy and Zablotsky (2011)	negative		Arabs in Israel
Fort (2009)	no effect		Italy
Monstad et al. (2008)	no effect	positive	Norway
Grönqvist and Hall (2011)	no effect	positive	Sweden
McCrary and 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 not affected	MZ affected	SOEP not affected	SOEP affected
Year of birth	1949.46	1954.53	1948.54	1955.80
	(3.12)	(3.08)	(3.36)	(3.09)
Number of children	1.66	1.59	1.64	1.66
	(1.12)	(1.16)	(1.16)	(1.20)
Childless	0.16	0.20	0.20	0.20
	(0.37)	(0.40)	(0.40)	(0.40)
Probability of giving first birth at age				
15-20	-	-	0.19	0.14
	-	-	(0.39)	(0.35)
21-25	-	-	0.31	0.27
	-	-	(0.46)	(0.44)
26-30	-	-	0.21	0.24
	-	-	(0.41)	(0.43)
31-35	-	-	0.06	0.11
	-	-	(0.25)	(0.32)
>35	-	-	0.02	0.04
	-	-	(0.15)	(0.19)
Years of education	11.11	12.28	11.63	12.90
	(2.91)	(2.77)	(3.23)	(2.98)
Attended (0/1) basic track: 8th or 9th grade	0.63	0.51	0.56	0.41
	(0.48)	(0.50)	(0.50)	(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 education				Attends basic track	
	Full sample		Basic track		Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy	0.744 ***	0.647 ***	1.040 ***	1.090 ***	-0.027	-0.009
	(0.080)	(0.132)	(0.042)	(0.053)	(0.020)	(0.026)
F-Statistic	85.56	23.89	607.54	414.90	1.46	0.12
State-specific trends in year of birth						
Linear	YES	YES	YES	YES	YES	YES
Squared		YES		YES		YES
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 also 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: the effect of education on completed fertility

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

Notes: Each coefficient represents a separate linear regression. All regressions also 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 first birth

Age at first birth	15-20	21-25	26-30	31-35	>35
OLS	-0.028 ***	-0.036 ***	-0.008 **	0.022 ***	0.009 *
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
IV	-0.057 **	-0.052	0.048	-0.154 *	-0.045
	(0.022)	(0.034)	(0.045)	(0.079)	(0.081)
First-stage results					
Reform dummy	1.638 ***	1.396 ***	1.451 ***	1.458 ***	0.989 *
	(0.399)	(0.407)	(0.385)	(0.461)	(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 also 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 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 ***	-0.024 ***	-0.071 ***	-0.071 ***
	(0.003)	(0.003)	(0.008)	(0.008)
IV	-0.489 ***	-0.910	-0.395 *	-0.323
	(0.160)	(0.564)	(0.204)	(0.203)
Panel B: Childlessness				
OLS	0.010 ***	0.010 ***	0.010 ***	0.010 ***
	(0.001)	(0.001)	(0.002)	(0.002)
IV	0.167 **	0.207	0.144 ***	0.066
	(0.058)	(0.183)	(0.059)	(0.048)
First-stage results				
Reform dummy	0.305 ***	0.165	0.428 ***	0.457 ***
	(0.304)	(0.127)	(0.102)	(0.094)
First-stage F-statistic	9.86	1.68	20.50	19.70
State-specific trends in year of birth				
Linear	YES	YES	YES	YES
Squared		YES		YES
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 also 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	Symmetric				Asymmetric
	4-year	5-year	6-year	7-year	1938-1959
Panel A: Number of children					
IV	-0.187 ***	-0.172 ***	-0.144 ***	-0.071	-0.019
	(0.058)	(0.050)	(0.052)	(0.058)	(0.057)
Panel B: Childlessness					
IV	0.047 **	0.051 ***	0.051 ***	0.050 ***	0.011
	(0.022)	(0.018)	(0.016)	(0.019)	(0.018)
First-stage results					
Reform dummy	0.708 ***	0.647 ***	0.725 ***	0.672 ***	0.740 ***
	(0.192)	(0.132)	(0.115)	(0.092)	(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	38,310

Notes: Each coefficient represents a separate linear regression. All regressions also 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 the age-specific probability of first birth - alternative selection of analyzed cohorts

Age at birth	15-20	21-25	26-30	31-35	> 35
Panel A: 5-year window					
IV	-0.029	-0.026	0.052	-0.078	0.040
	(0.019)	(0.036)	(0.033)	(0.048)	(0.032)
First-stage results					
First-stage F-Statistic	15.01	14.58	34.57	33.77	11.84
Observations	1,911	1,599	1,044	606	444
Panel B: 9-year window					
IV	-0.055 **	-0.041	0.012	-0.018	-0.019
	(0.026)	(0.039)	(0.040)	(0.062)	(0.052)
First-stage results					
First-stage F-Statistic	20.35	12.21	20.18	12.07	5.96
Observations	3,403	2,872	1,878	1,090	785

Note: Probability of childbearing at a given age. Each coefficient represents a separate linear regression. All regressions also 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 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.

Appendix

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

	7-year window		5-year window	
Panel A: number of children				
OLS	-0.015 *	-0.015 *	-0.018 **	-0.018 **
	(0.008)	(0.008)	(0.009)	(0.009)
IV	-0.074	-0.172 *	-0.112	-0.064
	(0.099)	(0.088)	(0.077)	(0.095)
Panel B: childlessness				
OLS	0.007 **	0.007 **	0.009 ***	0.009 ***
	(0.003)	(0.003)	(0.003)	(0.003)
IV	0.012	0.053 ***	0.032 **	0.014
	(0.022)	(0.019)	(0.015)	(0.016)
First-stage results				
Reform dummy	1.123 ***	1.638 ***	1.534 ***	1.839 ***
	(0.300)	(0.399)	(0.401)	(0.475)
First-stage F-statistic	14.053	14.053	14.635	15.013
State-specific trends in year of birth				
Linear	YES	YES	YES	YES
Squared		YES		YES
Observations	2,649		1,911	

Notes: Each coefficient represents a separate linear regression. All regressions also include state of residence-fixed effects, year of birth-fixed effects, indicators for marital status, municipality size, and mothers' age at 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.

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