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Spillover Effects of Maternal Education on Child's Health and Health Behavior

Daniel Kemptner¹ and Jan Marcus² (DIW Berlin)

This study investigates the effects of maternal education on child's health and health behavior. We draw on a rich German panel data set (SOEP) containing information about three generations. This allows instrumenting maternal education by the number of her siblings while conditioning on grandparental characteristics. The instrumental variables approach has not yet been used in the intergenerational context and works for the sample sizes of common household panels. We find substantial effects on health behavior for adolescent daughters, but neither for adolescent sons nor for the health status of newborns. We show that possible concerns for the validity of the instrument are unlikely to compromise these results. We discuss mother's health behavior, assortative mating, household income, and child's schooling track as possible channels of the estimated effects. Maternal education seems to affect daughter's smoking behavior through the higher likelihood of the daughter pursuing a higher secondary schooling track.

JEL: C26, I12, J62

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1 Introduction

When analyzing returns to education, economists often focus on wages and income (see [Card \(1999\)](#) for an overview). More recently, research is also concentrating on the effect of education on non-market outcomes like health (see [Cutler & Lleras-Muney \(2008\)](#) and [Grossman \(2006\)](#) for overviews). Furthermore, researchers point to intergenerational spillover effects of education ([Black & Devereux \(2011\)](#) and [Currie \(2009\)](#) provide overviews). Quantifying such intergenerational links is not only relevant regarding optimal investments into education, but also relates to social mobility. The more that a child's outcomes are determined by its parents' education, the less that a society can be considered to be socially mobile.

Our paper investigates the effects of maternal education on child's health and health behavior in Germany. We consider both the effects on newborns and adolescents. Therefore, we look at various outcome variables: physical health, smoking behavior, overweight, and doing sports for adolescents; low birth weight and preterm birth for newborns. We apply an instrumental variables (IV) approach that has not yet been used in the intergenerational context. We instrument maternal education by the number of her siblings while conditioning on characteristics of her parents, the child's grandparents. For this purpose, we draw on a rich household survey, the German Socio-Economic Panel Study (SOEP), containing detailed information about *three* generations. We argue that, given the grandparents' characteristics, the number of the mother's siblings generates variation in maternal years of education that is exogenous regarding her child's health and health behavior. If grandparents are constrained in borrowing against the mother's future earnings, the number of her siblings affects household resources available for her educational investments.

Previous studies on the effects of parental education on child's health and health behavior in developed countries produced mixed evidence (see table 1). [Currie & Moretti \(2003\)](#) find maternal education reduces the risks of low birth weight and preterm birth. This finding is not corroborated by the IV-study of [McCrary & Royer \(2011\)](#). For teenage children, [Carneiro et al. \(2007\)](#) as well as [Lindeboom et al. \(2009\)](#) find no significant effects of parental education on the children's health status in their IV-analyses. Other studies for Germany analyze the intergenerational correlation of health ([Coneus & Spiess 2012](#)), as well as the correlation between parental education and child health ([Lamerz et al. 2005](#)). We add to the literature by applying an IV strategy that works for the sample size of common household panels, by considering a variety of outcomes for both

newborns and adolescents, and by investigating possible channels of the estimated effects. We focus only on mothers because the SOEP basically reports on the partner of the mother and not on the biological father.

For newborns, we find maternal education to be associated with a reduced probability of preterm maturity. Our IV approach, however, does not indicate significant effects on newborns. For adolescents, we find strong and significant effects on health-related behavior for daughters. An additional year of maternal education is estimated to reduce the daughter's probability of smoking by 7.4 percentage points and to increase the daughter's likelihood of doing sports at least once a week by 7.5 percentage points. We do not obtain significant effects on sons' health behavior. We do not find any effects on child's physical health and overweight.

We demonstrate the robustness of our IV estimates by sequentially introducing the control variables. The results are not substantially altered when we include controls for grandparents' education, grandparent's occupational prestige and the size of the area where the mother grew up. Also the results do not change when we control for some possibly "bad controls" (i.e. variables that are possibly consequences of maternal education) like mother's fertility, health and health behavior. Furthermore, the results are robust to only considering mothers with one, two or three siblings as well as to more flexible specifications of the first stage. We discuss mother's health behavior, assortative mating, household income, and child's schooling track as possible channels of the estimated effects. Our results do not suggest that mother's health behavior, assortative mating or household income explain the effects on adolescent daughters. When including child's schooling track as an additional control variable, the effect of maternal education on daughter's smoking behavior disappears. Hence, maternal education seems to affect daughter's smoking behavior through the higher likelihood of the daughter pursuing a higher secondary schooling track. Even though early tracking is a peculiarity of the German schooling system, the mechanism at work (school quality or peer group effects) may also be relevant for other countries.

The paper is structured as follows. Section 2 describes the data and presents descriptive statistics. Section 3 contains a detailed discussion of our empirical strategy. In section 4, we present both Probit and IV- Probit results and present sensitivity checks. Section 5 investigates channels of the estimated effects. Section 6 concludes with a discussion on the implications of our findings.

2 Data and descriptive statistics

2.1 Sample

In our analysis we make use of the rich data from the German Socio-Economic Panel Study (SOEP). The SOEP started in 1984 and annually collects information at the household and individual levels (see [Wagner et al. 2007](#)). In 2010 more than 10,000 households participated in this panel study.

The SOEP hosts several features that make it particularly attractive for the present analysis: Not only is it one of the largest and longest-running panel studies in the world, it also provides detailed health information on adolescents and newborns. Furthermore, due to the collection of additional biographical information of adult respondents, for children data on their parents and on their grandparents are available. We conduct our analysis for two different samples according to the child’s age when the information was collected: “newborns” (0-18 months) and “adolescents” (18-19 years). Both samples are pooled across survey years. The following section describes these samples and the child outcomes in more detail, before turning to variables at the mother’s and grandparents’ level.

The sample of newborns is based on the “mother and child questionnaire”, which the SOEP introduced in 2003. It is distributed to the mothers of children born in the year of the survey or the year before. Therefore, children born from 2002 to 2010 constitute the newborns sample. The adolescents sample consists of West German children born between 1983 and 1992, using data from when these children were around 18 years of age. Hence, the sample is pooled across survey years. For health related variables, we use data from the year when the adolescents answered the relevant questions on the individual adult questionnaire for the first time. Since some of the health variables are only included every two years, for some adolescents we use information from the year they turned 18 and for the rest we use information from the year they turned 19. In the regressions we control for these age differences through fixed birth year effects.

2.2 Outcome variables

We look at six different health outcomes, two in the newborns sample, four in the adolescents sample. All outcomes are binary variables and coded in such a way that “1” reflects less healthy outcomes. In the newborns sample, we consider two different health indicators: preterm birth and low birth weight. Preterm birth refers to the birth

of a child of less than 37 weeks gestational age. In developed countries, preterm birth is the major cause of infant mortality and neurological long-term morbidity ([Martius et al. 1998](#)). Another related health measure is the child’s birth weight. Babies with low birth weight have adverse health status later on in life - even when controlling for preterm maturity ([McIntire et al. 1999](#)). We define a child to be of low birth weight if its weight at birth is below 3000g.³ We only analyse biological children, and exclude twin babies from the newborns sample because their birth weight is lower in general ([Naeye 1964](#)).

For adolescents, we construct a variable “overweight” indicating a body mass index (BMI) greater than 25. We code a binary variable “currently smoking” according to the question “Do you currently smoke, be it cigarettes, a pipe or cigars?” The SOEP started asking detailed health questions, including weight and smoking behavior, in even numbered years, starting in 2002.⁴ A variable on sport activities indicates whether an adolescent is *not* doing sports at least once a week.⁵ We generally use information on sport behavior from the year the adolescent turned 18. However, this variable was not collected in the survey years 2002, 2004, 2006, and 2010. For those who turn 18 in these years, we use the information about doing sports from the year they turned 19 - information gathered during the next wave. Apart from these three variables indicating health behaviors, we also look at a measure of general health status for adolescents. Our measure is based on the physical component summary scale (pcs) provided by the SOEP group, a weighted combination of the 12 items of the SF-12 module ([Andersen et al. 2007](#)). In order to facilitate comparison to the other outcomes we also dichotomize this outcome variable. Adolescents with physical scale values below the median of all adolescents are coded as having “poorer health”.⁶

³More commonly low birth weight is defined as a birth weight of less than 2500. However, we encounter the same problem as [Lindeboom et al. \(2009: 111\)](#): with this strict definition we only have a few observations with low birth weight. These observations might be affected by measurement error. Hence, we apply the same definition as Lindeboom and colleagues.

⁴The SOEP collects data on smoking behavior also in the years 1998, 1999 and 2001. These questions, though, differ in their phrasing. Therefore, we exclude information from these survey years.

⁵We also computed the regressions for a slightly different definition of this variable. The results differ only marginally, when we consider a person as being active who is doing sports at least once a *month*. These and other results not shown are available from the authors upon request.

⁶We also applied different thresholds for the definition of poorer health, used the metric instead of the dichotomized physical health measure and resorted to a different self-rated health item (“How would you describe your current health?”). All these redefinitions of the health status outcome do not change the results presented in section 4.

2.3 Parental and grandparental variables

At the parental level, we focus only on mothers because the SOEP collects data on the mother's partner, who may or may not be the biological father of the child. Relevant data for mothers include years of education, number of siblings and population of the area where the mother grew up until the age of 15. The SOEP constructs the years of education variable from the respondents' information about the obtained level of education and adds time for additional occupational training.⁷ For the numbers of siblings, we use the earliest available information about brothers and sisters collected in the survey.⁸ Since siblings might have died, this is the best approximation of the number of brothers and sisters when the mother went to school. The area where the mother grew up is a discrete variable with four categories according to the size of the hometown: countryside, small city, medium city and large city. All information about mothers is self-reported by the mothers.

At the level of the parents of the mother, we use data on educational levels and occupational prestige. For both, grandfathers and grandmothers, we construct dummy variables according to five educational levels: secondary school degree, intermediate/technical school degree, general university-entrance diploma, other degree and no school degree/no school attended. To measure occupational prestige we rely on the International Socio-Economic Index of Occupational Status (ISEI). The ISEI assigns scores to almost 300 different occupation categories "in such a way as to maximize the role of occupation as an intervening variable between education and income" (Ganzeboom et al. 1992: 2).⁹ Information on the grandparents is either contributed by the grandparents directly (less than 5 percent) - if they are SOEP participants - or by proxy via interviews of the mothers: All individuals with a valid personal interview in the SOEP are requested to

⁷If the variable years of education is missing for an individual in a given survey year, we use information from other survey years. Following Kemptner et al. (2011), we also employ a different measure of the years of education, in which we only considered years of primary and secondary schooling: 9 years for individuals without school degree and those with basic track degree, 10 years for those with intermediate track school or other degree, 12 years for technical school degree and 13 years for general university-entrance diploma. However, the Probit and IV-Probit results did not change qualitatively, only the size of the coefficient estimates increased.

⁸The SOEP collected this information in 1990, 1996, 2001, 2003 and 2006. We consider siblings inside and outside of the household.

⁹The ISEI score is derived from the occupational status of grandfather and grandmother. SOEP questions on the occupational status of grandfather and grandmother are formulated to reflect the situation when the mother was 16. For each pair of grandparents we make use of the highest ISEI score, which in most cases is the score of the grandfather. Missing values are imputed as described in the appendix.

answer the supplementary biography questionnaire with questions on their parents and their social origin. Missing values at the grandparental level are imputed as described in the appendix.

2.4 Descriptive statistics

Table 2 displays unweighted means and standard deviations for relevant variables at the maternal and child level for both of our samples. While the newborns sample consists of West German children born between 2002 and 2010 and excludes both adopted children and twins, the adolescents sample consists of West German children born between 1983 and 1992. Both samples do not include children whose mothers were educated in the German Democratic Republic. We make the estimation samples more homogeneous by restricting them to mothers with siblings (see subsection 3.2 for further discussion). Due to the construction of the two samples, the mothers of the adolescents come from earlier birth cohorts. Therefore, differences in mean years of education and number of siblings between the two samples can be explained by the increase in years of education and the decrease of family sizes over time. All our regression models include the mother’s year of birth to control for these time trends.

Figure 1 presents lines from non-parametric local constant estimations of the association between mother’s years of education and various child outcomes. For almost all outcome measures there is a monotonous relationship: Worse health behavior and poorer health of the child decrease almost linearly with the mother’s education. For instance, the chance of not doing sports at least once a week is around 60 % for children of poorly educated mothers, 50 % for children of mothers with about 10 years of education and 30 % for children of mothers with more than 15 years of education. The probability of preterm birth is almost twice as high for the least educated mothers compared to the best educated mothers. The increase in the overweight probability on the right tale of the education distribution is not statistically significant as the confidence bands indicate.

3 Empirical strategy

3.1 Main estimation equation

We estimate the effects of maternal years of education on binary child outcomes. For this purpose, we rely on single (Probit) and two-equation models (IV-Probit).¹⁰ All models are estimated with robust standard errors that are clustered by mothers, accounting for serial correlation between children of the same mother. Our single equation model linking child's outcome to maternal years of education is specified as follows:

$$H_c = 1 [\beta_0 + \beta_1 \cdot S_m + \beta_2 \cdot \mathbf{x} + \varepsilon_c > 0] \quad (1)$$

where H_c is child's outcome and S_m is maternal years of education. \mathbf{x} contains different sets of control variables that we gradually incorporate in section 4 to assess their impact. In the preferred specification, \mathbf{x} includes sets of fixed effects accounting for mother's birth cohort, the size of the area where the mother grew up, grandparents' level of education and occupational prestige, child's birth cohort, and child's sex.¹¹ ε_c is an idiosyncratic child specific error term that is normally and identically distributed. $1[\cdot]$ is an indicator function.

Estimating equation (1) as a single equation model will only produce consistent parameter estimates if maternal years of education, S_m , are uncorrelated with ε_c . Since maternal years of education are likely to be correlated with unobserved confounders, we expect the coefficient estimates to be biased in an unknown direction.

3.2 Instrumental variables approach

The endogeneity of S_m can be dealt with by instrumenting S_m with Z_m , where Z_m must meet the following two conditions:

$$\begin{array}{ll} E[\varepsilon_c | Z_m] = 0 & \text{(validity)} \\ E[S_m | Z_m, \mathbf{x}] \neq E[S_m | \mathbf{x}] & \text{(relevance)} \end{array}$$

¹⁰In the section on robustness checks, we also present results from a two-stage least squares model (2SLS). Being more robust regarding the distributional assumptions of the error terms but less efficient, the estimated effects differ only marginally.

¹¹By controlling for both mother's birth cohort and child's year of birth, we indirectly control for the mother's age at birth. Since the mother's age at child birth is a choice variable and possibly correlated with maternal education, we also run the models without this control variable. The results are insensitive to this modification. These estimates are available upon request from the authors.

Z_m is a valid instrument if it affects the child’s outcome only through mother’s years of education, given the other covariates. Z_m is a relevant instrument if the explanatory power of Z_m with respect to S_m is sufficiently large, given the other covariates. Various instruments for education are proposed in the literature on returns to education (see [Card \(1999\)](#) and [Grossman \(2006\)](#) for overviews). A first wave of IV studies relies on family characteristics as instruments, such as parents’ income and parents’ schooling. While these instruments are strongly associated with education, the validity assumption seems questionable. A second wave of IV studies uses variations in educational policies and other natural experiments. This second wave of IV studies faces less criticism regarding the validity assumption. However, the association with education is often weak and, hence, weak instrument problems may arise. Researchers frequently draw on huge sample sizes to mitigate this problem. A drawback of huge data sets is that these often do not include detailed outcome measures. Another problem with policy changes and other natural experiments is that they only affect certain cohorts.

We do not rely on policy changes but instead use the number of mother’s siblings as an instrument for maternal education while conditioning on characteristics of the grandparents. These grandparental characteristics include variables describing the grandparents’ level of education and occupational prestige as well as the area where the mother grew up. This identification strategy works also for cohorts unaffected by policy changes and for the limited sample sizes of common household panels. This instrument was suggested before (e.g. [Sander 1995](#)). We improve the approach by conditioning on characteristics of the grandparents. There is an obvious concern regarding the validity of the instrument. Fertility is higher in the countryside and negatively correlated with social status, i.e. mothers with siblings are more likely to live in the countryside and to have parents with lower social status. Therefore, we condition on the grandparent’s level of education, the grandparent’s occupational prestige and the size of the area where the mother grew up. We gradually incorporate these variables to assert their influence.

Consistency of our estimates rests on the assumption that the instrument identifies exogenous variation in the endogenous education variable, given the other covariates. We deal with possible concerns regarding the validity of the instrument by including controls for maternal health, health behavior and fertility in one specification. However, this is not our preferred specification because the additional control variables might be consequences of maternal education themselves.

The number of mother’s siblings should also be a relevant instrument because the resources available for educational investments per child depend substantially on the

number of children in the household. This assumes that parents are constrained in borrowing against their children’s future earnings. A significant effect of the number of mother’s siblings on her education in the first stage points to such a borrowing constraint of the grandparents. Even though there are no schooling fees and very low or no tuition fees at public educational institutions in Germany, investments into children’s education involve forgone earnings for both the parents and the children. Parents’ time constraints and limited housing space may impose pressure upon the children to make their own living instead of spending more time on educational investments.

Figure 2 contains a graph showing the average years of education for mothers with different numbers of siblings as well as the share of mothers with this number of siblings.¹² The graph shows that most children in our sample are born to mothers who grew up with four or fewer siblings (about 85 %). Furthermore, the graph pictures a clear negative relationship between maternal education and the number of her siblings. However, mothers without siblings seem to be special having on average less education than mothers with one or two siblings. This does not correspond with the argument that more siblings introduce resource constraints leading to lower educational achievements. [Black et al. \(2005\)](#) also find this only child particularity for the US, which disappears when they consider the subsample of intact families. Hence, it may be that parental divorces exert negative effects on the only children’s educational achievement. The only child particularity could lead to a non-linear relationship between mother’s years of education and the number of her siblings or even have long-lasting effects on the grandchildren. For this reason, and in order to make the estimation sample more homogeneous, we restrict the sample to mothers with siblings. In subsection 4.3, we present a robustness check for the inclusion of children of mothers without siblings. It turns out that our findings are insensitive to this modification of the sample.

3.3 Implementation

We implement the IV strategy by estimating the following two-equation model using the method of maximum likelihood:

$$S_m = \gamma_0 + \gamma_1 \cdot Z_m + \boldsymbol{\gamma_2} \cdot \mathbf{x} + \mu_m \quad (2)$$

$$H_c = 1 [\beta_0 + \beta_1 \cdot S_m + \boldsymbol{\beta_2} \cdot \mathbf{x} + \varepsilon_c > 0] \quad (3)$$

¹²The graph displays the numbers for the sample of adolescents with non-missing smoking information. Similar pictures emerge for the other samples.

μ_m and ε_c are assumed to be bivariate normally and identically distributed with mean zero and to be independent of the instrument Z_m (see [Wooldridge \(2002: 472\)](#) for further details). Under the assumptions of instrument validity and relevance, joint estimation of equations (2) and (3) as an IV-Probit model produces consistent parameter estimates. The coefficients of the first stage can be directly interpreted as marginal effects. Since the parameters of a Probit model cannot be given this interpretation, we compute average marginal effects and apply the delta method when calculating standard errors. In subsection 4.3, we present a robustness check for the distributional assumptions of the model.

4 Results

Table 3 contains the first stage results of the IV-Probit model. The second stage results of the IV-Probit model and the findings from the single equation Probit model are presented in table 4 for the newborns and table 5 for the adolescents. As has been discussed above, we estimate several specifications and sequentially introduce the control variables. This demonstrates the robustness of our estimates. Note that for the IV-Probit model specification 6 is our preferred specification because it conditions on a rich set of grandparental characteristics, but does not include potentially “bad controls” like mother’s health, health behavior, and fertility. In the following subsections, we first discuss the effect of mother’s siblings on her educational achievement (first stage of the IV-Probit model), then we discuss the findings on the effects of maternal education on the child’s outcomes (Probit model and second stage of the IV-Probit model), and subsequently we present some additional sensitivity checks.

4.1 The effect of siblings on years of education

Table 3 presents the first stage coefficients of the IV estimation. The small differences in the first stage coefficient estimates stem from different sample sizes for the outcome measures. The estimated effects of the number of mother’s siblings on her educational attainment are highly significant in all specifications. This indicates that the number of siblings is a relevant instrument for maternal education. The association is slightly stronger for adolescent daughters than for adolescent sons, which is presumably due to

sampling variation.¹³ Conditioning on characteristics of the grandparents' household reduces the gradient between the number of siblings and educational achievement of the mothers. In the preferred specification 6, an additional sibling is predicted to decrease the years of education by 0.23-0.28 years for the female adolescents and the newborns samples and by about 0.15 years for the male adolescents sample. We are interested in the F-statistics testing the assumption that the number of mother's siblings does not affect her educational achievement, given the other covariates. For specification 6, all our F-statistics for the pooled newborns sample are above 24. Furthermore, all the F-statistics are above 37 for the sample of female adolescents and above 11 for the sample of male adolescents.

Thus, the estimation strategy seems not to suffer from a weak instruments problem. The estimated significant effects in the first stage point to financial constraints of the grandparents when investing in their daughter's education.

4.2 The effect of maternal education on child outcomes

4.2.1 Newborns

In the Probit models, the average marginal effects indicate a significant association between maternal education and the likelihood of preterm birth (see specification 1-3 in table 4). When conditioning on grandparental characteristics, we find no evidence for an effect of maternal education on low birth weight.¹⁴ Note that the grandparental characteristics capture some potential confounders of maternal education being related to the family background. The average marginal effect on preterm birth matches the smoothed bivariate regression line from figure 1. Controlling for characteristics of the grandparents' household, an additional year of maternal education is associated with a reduction in the probability of preterm maturity by 1.3 percentage points (specification 3).

Although insignificant for most specifications, in the IV model the estimated effect on preterm maturity increases in size compared to the findings of the single equation Probit model (2.3 vs. 1.3 percentage points reduction in risk). It might be that our sample size is too small to detect an existing effect. For the US, [Currie & Moretti \(2003\)](#) report a significant reduction of 1 percentage point in the probability of preterm birth for each

¹³None of the effect differences between mothers of daughters and mothers of sons is significant at the 5 % level.

¹⁴We also obtain small and insignificant effects when using birth weight as outcome in 2SLS regressions.

year of maternal education (see table 1 for an overview of the results and the designs of previous IV-studies on the effect of maternal education on child's health in developed countries).

Average marginal effects on low birth weight are close to zero and the standard errors are relatively large. This finding is in line with [Lindeboom et al. \(2009\)](#). Making use of a change in compulsory schooling in Britain in 1947, they find no effect of mother's education on birth weight and low birth weight. With the same policy change, [Chevalier & O'Sullivan \(2007\)](#) estimate an increase of 74 gram birth weight for every additional year of mother's education but do not find a significant reduction in the probability of low birth weight. Also [McCrary & Royer \(2011\)](#) and [Carneiro et al. \(2007\)](#) find no effect on low birth weight. Only [Currie & Moretti \(2003\)](#) estimate a significant reduction of 1 percentage point in the probability of low birth weight for an additional year of maternal education.

4.2.2 Adolescents

The Probit specifications in table 5 indicate that one more year of mother's education is associated with a decrease in the adolescent's probability of being a smoker by about 2 percentage points, given the grandparents' characteristics. There is no significant association between years of maternal education and an adolescent's probability of being overweight at age 18/19. However, there seems to be a strong relationship with the child's likelihood of not doing sports at least once per week. The estimates suggest that each additional year of maternal education is associated with a decrease in the probability of not doing sports regularly by 3 percentage points for sons and 3.4 percentage points for daughters. The results do not suggest an association of maternal education with an adolescent's physical health.

Turning to the results of the IV-Probit model (specifications 4-6 in table 5), we find large and significant effects of maternal years of education on daughter's smoking and sport behavior in all IV specifications. These effects do not disappear when we include the grandparents' level of education, their occupational prestige and the size of the area the mother grew up as additional controls. The coefficients tend to be even slightly larger. In our preferred specification 6, the probability of the daughter doing sports regularly is increased by 7.4 percentage points per year of maternal education. In addition, one additional year of maternal education decreases the likelihood of the daughter being a smoker at age 18/19 by 7.5 percentage points. We do not find any significant effects on

son's health and health behavior in general.¹⁵ Confirming the finding from the Probit specification, there is also no significant effect on overweight or physical health for girls. Loureiro et al. (2010) also find that mothers are only influential with respect to the smoking behavior of their daughters but not for their sons. These gender differences are in line with the idea of gender-specific parental role-models and the finding that children identify stronger with the same-sex parent (Starrels 1994). Furthermore, this supports the theory of more productive parenting by the same-sex parent (Gugl & Welling 2011).

Comparing the significant effects of maternal education on child's outcomes with the estimates from the Probit models, the estimated effects from the IV-Probit models appear to be larger. This is in line with the majority of findings in the literature on returns to education (see Card 1999). Currie & Moretti (2003) and Carneiro et al. (2007) also find larger effects when instrumenting maternal education. Three factors might be responsible for this finding. First, measurement error in maternal education attenuates the Probit estimates. Second, unobserved variables that are negatively correlated with maternal education but positively with better child outcomes might result in downward biased estimates. Third, in the presence of effect heterogeneity, IV approaches may not identify the average effect for the overall population but rather local average effects for the so-called compliers, i.e. mothers who obtain fewer [more] years of education because they have more [less] siblings.

4.3 Sensitivity checks

Table 6 presents five sensitivity checks for the adolescents samples.¹⁶ These additional specifications are estimated to show that some possible concerns regarding the instrument's validity, the sample characteristics, the functional form assumptions, or the distributional assumptions of the error terms are unlikely to compromise our results. Specification 7 includes additional controls' for mother's health, health behavior and fertility. Since mother's health and health behavior are potentially affected by the number of her siblings, we include the four health-related outcome variables - at the mother's level - as additional control variables, i.e. we include binary variables for the mother's overweight, smoking, sport behavior and bad health status. A further concern for the instrument's validity relates to mother's fertility. Grandparents' fertility could affect mother's fertility and lead to financial constraints that have a direct impact on child outcomes. Indeed,

¹⁵Although the effect on sport activity is rather large and only borderline insignificant.

¹⁶We do not show sensitivity analyses for newborns because similar to the results in table 4 the effects are insignificant in all sensitivity analyses.

we find that the number of mother’s siblings explains 4 % of the variation in the number of mother’s own children. We address this concern by including fixed effects for the number of mother’s children in our IV model.¹⁷ However, this is not our preferred specification as the additional control variables might be inherent consequences of maternal education and, hence, bad control variables. The estimated effects of specification 7 differ only marginally from the estimates of our preferred specification 6.

Specification 8 allows for full flexibility with respect to the functional relationship between the number of siblings and maternal education. In this specification we instrument maternal education by a set of dichotomous variables that indicates the number of siblings. Categories of the number of siblings are 1, 2, 3, 4, 5 and 6 and more siblings. Specification 9 includes children of mothers without siblings to show that our findings do not hinge on this sample restriction.

Another concern regarding our instrument is that the fertility decision of parents may be affected by heterogeneous preferences for child quality if parents take into account the resource constraints of the household (“quantity-quality trade-off”; see [Becker et al. 1960](#); [Becker & Lewis 1973](#)). In principle, conditioning on the grandparental characteristics should account for this heterogeneity to the degree that the preferences for child quality are correlated with education or occupational prestige. The concern, however, is that conditioning on these variables might not be enough. In specification 10, we estimate our model for a more homogeneous sample that includes only mothers with one to three siblings. This sensitivity check relies on the assumption that parents of these mothers are presumably more similar than in the full sample. We find it reassuring that our results are not sensitive to this sample restriction.

Lastly, we check the sensitivity of our findings regarding the distributional assumptions of the IV-Probit model (specification 11). We estimate a two-stage least squares model (2SLS), which - unlike the IV-Probit model - also produces consistent parameter estimates in the presence of heteroscedasticity and non-normally distributed errors.

In all these additional specifications, most of the estimated effects change only marginally. The effects of maternal education on sport and smoking behavior of daughters are significant in all sensitivity tests. The effect of maternal education on sons not doing sports regularly becomes significant for specification 8 and specification 11. When only considering mothers with one, two or three children, a positive effect of mothers on the son’s probability of being overweight emerges (specification 10). This is likely to be due to a weak instruments problem that arises for sons when using only the restricted

¹⁷More specifically, we include dummy variables for 1, 2, 3 and 4 or more children.

sample. The first stage F-statistics for this specification is about 5 for sons, and about 10 for daughters. We conclude that our main findings, the effects on daughters' smoking and sport behavior, are not sensitive regarding sample characteristics, functional form assumptions or distributional assumptions of the error terms.

Another issue is that there may be non-linearities in the effects of maternal years of education on child's outcomes, although the graphs in figure 1 suggest an approximately linear relationship. We try to detect non-linearities in the effects by using years of education and years of education squared as endogenous regressors in our IV-models. Accordingly, we instrumented the two endogenous regressors with the number of mother's siblings and its square. The estimated coefficients on the years of education squared were highly insignificant. Non-linearities in the effects of maternal years of education are either irrelevant or too small to be detected with our estimation approach and the given sample size. In any case, our estimates can be interpreted as the *average* effect of one more year of maternal education.

5 Channels

This section discusses possible channels of the estimated effects that could drive the relationship between maternal education and adolescents' outcomes. In order to investigate potential channels, we follow the strategy by [Oreopoulos et al. \(2008\)](#). When investigating intergenerational effects of father's displacement, [Oreopoulos et al. \(2008\)](#) analyze potential channels by a) including the channels as (potentially endogenous) additional controls and by b) investigating the effect on the channels (as outcomes).

We consider mother's health behavior, assortative mating, household income, and child's schooling track as potential channels. Table 6 contains the results of alternative specifications that include these potential channels as additional controls. The results from these specifications must be interpreted carefully because these additional controls are likely to be endogenous and may also bias the estimated effects of maternal education. Furthermore, table 7 presents estimates of the direct effects of maternal education on the potential channels using our IV approach.

Mother's health behavior may explain the estimated effects of maternal education on daughter's health behavior if the mother operates as role model. We look at the same health measures that we also consider for adolescents. Indeed, we find substantial effects of maternal years of education on mother's own health behavior, but not on physical

health, using our IV approach (see table 7).¹⁸ However, the effects of maternal education on daughter's health behavior remain unchanged when controlling for mother's health behavior (smoking, overweight, and no sports) in the IV model (see table 6, specification 7).

Assortative mating may explain to some degree the effects of maternal education on child outcomes. In our data we find a correlation coefficient of 0.66 between maternal years of education and her partner's years of education. Furthermore, our IV approach predicts that one more year of maternal education increases partner's years of education by 0.84 years (table 7). Thus, the estimated effects on daughter's health behavior may work through the partner's education. We focus on the mother's partner because the SOEP does not report on the biological father, just on the mother's current partner. In specification 12 (table 6), we estimate effects of maternal education on the child's outcomes, including the partner's years of education as an additional control variable. The magnitude of the effects on the probabilities of the daughter being a smoker and of the daughter doing sports regularly change only marginally, but the effects become insignificant. This may be due to the substantial loss in precision.

The effects of maternal education on the daughter's health behavior may also work through a higher household income. Household income is measured by the logarithm of a five years average of household post-government income. The results in table 7 show that one more year of maternal education leads to an increase in household income of 14 per cent. To some extent this also accounts for assortative mating (Jepsen 2005) because the mothers' partners are the principle earners in the majority of the households. When estimating a specification that includes the logarithm of household income as an additional control variable (table 6, specification 13), the estimated effects change only marginally.

Next, we investigate the child's schooling as potential channel. Usually after four years of primary school, the German school system selects children into one of three tracks: basic track (*Hauptschule*), intermediate track (*Realschule*), or academic track (*Gymnasium*). Pupils can only obtain the *Abitur* from academic track schools. The *Abitur* is the diploma usually required for enrolling into a German university. The IV estimates in table 7 suggest that one more year of maternal education increases the likelihood of the child pursuing the academic schooling track by about 10 percentage points. However, we do not find evidence for significant differences between sons and

¹⁸We do not find evidence that these effects differ substantially between mothers of daughters and mothers of sons.

daughters.¹⁹ Specification 14 (table 6) includes as additional control a binary variable indicating whether the adolescent attends an academic track school. As a result, the effect of maternal education on daughter’s smoking behavior disappears while the effect on daughters doing sports regularly remains unchanged. Thus, maternal education seems to affect the daughter’s smoking behavior by affecting schooling track. However, we cannot distinguish whether this is due to school quality (better understanding the risks of smoking, increased valuation of the future; see [Fletcher & Frisvold 2012](#)) or due to peer group effects (lower share of smokers).

6 Summary and discussion

Our paper investigates the effects of maternal education on child’s health and health behavior in Germany. Using a rich survey panel data set (SOEP), we analyze the effects on a wide range of outcomes for newborn and adolescent children. We estimate both Probit and IV-Probit models.

For newborns, we find a significant negative association between maternal education and the probability of preterm maturity. Although the effect on preterm maturity increases in size when estimating the IV-Probit model, the effect turns insignificant. It may be that the size of our newborns sample is not large enough to detect existing effects with the IV approach. We find no evidence for an effect of maternal education on low birth weight.

For adolescents, the IV approach suggests strong and significant effects on health-related behavior for daughters. One more year of maternal education is estimated to reduce the daughter’s probability of smoking at age 18/19 by 7.4 percentage points and to increase the daughter’s likelihood of doing sports at least once a week by 7.5 percentage points. However, we do not obtain significant effects of maternal education on sons’ health behavior. For both sexes, we do not find any effects on child’s physical health or overweight.

In line with previous research (e.g. [Carneiro et al. 2007](#); [Currie & Moretti 2003](#)), the significant estimates from the IV-Probit model exceed the corresponding estimates from the single equation Probit model. This may be attributed to three different reasons: measurement error in maternal education, unobserved variables leading to downward biased estimates in the Probit model, or the identification of local effects in the presence

¹⁹[Piopiunik \(2011\)](#) instrumenting maternal education by changes in compulsory schooling legislation finds significant effects of maternal education on sons’ but not on daughters’ education. This paper analyzes the effect of maternal education at the lower tail of the education distribution, while we investigate effects over the whole distribution of maternal education.

of effect heterogeneity when applying an IV-approach.

For our identification strategy, we do not rely on policy changes like previous studies. Instead, we present an IV approach that also works for cohorts unaffected by policy changes and for the limited sample sizes of common household panels. We argue that the mother's number of siblings is a valid instrument when conditioning on grandparental characteristics. Concerning the relevance of the instrument, we find all respective first stage F-statistics to exceed the critical value of 10. The estimation strategy seems not to suffer from a weak instruments problem. Regarding the validity of our instrument, we demonstrate the robustness of our IV estimates by sequentially introducing grandparental characteristics. The results are not substantially altered, when we include controls for grandparents' education, grandparent's occupational prestige and the size of the area where the mother grew up. Also the results do not change, when we control for further variables (like mother's health, health behavior, and fertility) that are possibly consequences of maternal education. Furthermore, the results are robust to only considering mothers with one, two or three siblings as well as to functional form assumptions of the first stage or distributional assumptions of the error terms.

Investigating possible channels of the estimated effects, our findings do not suggest that mother's health behavior, assortative mating, or household income explain the effects on daughter's health behavior. However, when including the child's schooling track as an additional control variable in our IV approach, the effect of maternal education on daughter's smoking behavior disappears. Thus, maternal education seems to affect child's health behavior by affecting schooling track. Even though early tracking is a particularity of the German schooling system, the mechanism at work (school quality or peer group effects) may also be relevant in other countries.

Public policy should take into account intergenerational links when thinking about optimal educational investments. There are persistent gains to be realized by increasing female education.

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A Appendix

Some variables on the mothers' and grandparents' level are affected by missing values; e.g. about 29 % of the children in the newborns sample have missing information on either the area where the mother grew up, grandparents' ISEI score, or grandfather's and grandmother's educational level. Omitting these cases will produce inefficient estimates, even if they are missing completely at random (MCAR; see [Rubin 1976](#)). The estimates will be biased if the information is not MCAR but only missing at random (MAR). Under MAR the missingness depends on other observed variables, e.g. if mothers with fewer years of education know less about their parents.

Due to these effectiveness and unbiasedness considerations, we impute four variables relevant for our analysis: grandfather's and grandmother's educational level, grandparents' ISEI score and the area where the mother grew up. In case the information is missing, for all variables we first copy information provided by the mother's siblings. We impute missing values in the size of mother's area randomly conditional on the size of the mother's district of residence when she was interviewed in the SOEP for the first time.

The other three variables are jointly imputed in four steps as follows. First, the educational levels of the grandparents are preliminarily imputed: If the level of education is missing for only one grandparent the information of the other grandparent is used. If the level of vocational training is available, the mode of level of education for each vocational training category is imputed. Second, we run a regression of the highest ISEI score of the grandparents (in most cases the grandfather's) score on sets of dummies for the grandfather's levels of vocational training and education, as well as dummies for the grandmother's levels of education and vocational training, dummies for the job position of the grandfather, controls for the birth decade of the grandfather and for each explanatory variable a dummy for missing values. These variables explain about 2/3 of the variance in grandparents' ISEI score. We exclude observations with missing information on all explanatory variables and do not impute any values for them.

Third, according to the regression results we predict values for those with missing information on the grandparents' ISEI score. We then add a random term drawn from the distribution of the regression residuals to maintain the variance of the dependent variable and to mimic the uncertainty of the imputation. [Little & Rubin \(2002: 60\)](#) refer to this procedure as stochastic regression imputation. Fourth, by means of multinomial logit models we regress the grandparents' educational level on the imputed grandparents' ISEI score, dummies for own vocational training levels and partner's education level. We use the predicted level of education for all those with missing information, including those with preliminarily imputed educational levels. In summary, we impute the grandfather's education for 7.5 % [7.0 %] of the adolescents [newborns], the grandmother's education for 6 % [5.5 %], the ISEI score for 44 % [16.5 %] and the size of the area the mother grew up for 1.5 % [9.8 %].

Table 1: Overview of previous IV-studies on the effect of maternal education on child's health

Author	Country	Dataset	Sample size	Instrument	Main findings
Currie & Moretti (2003)	USA	Vital Statistics	> 670,000	college openings	preterm birth: $\hat{\beta}_1 = -0.01$
Carneiro et al. (2007)	USA	Nativity records			low birth weight (LBW): $\hat{\beta}_1 = -0.01$
		National Longitudinal	> 3,600	variation in mother's	no effects on overweight (age 7-8, 12-14)
		Survey of Youth 1979	> 8,000	schooling costs	no effects on LBW
Chevalier & O'Sullivan (2007)	UK	National Child	> 8,000	compulsory schooling	no effect on LBW
		Development Study		reform	birth weight: $\hat{\beta}_1 = 0.74g$
Lindeboom et al. (2009)	UK	National Child	> 11,000	compulsory schooling	no effects on birth weight, LBW
		Development Study		reform	and overweight (at ages 7, 11 and 16)
McCrary & Royer (2011)	USA	natality data	> 320,000	age-at-school-entry	no effects on LBW and
		for TX and CA		policies	preterm birth

Note: The table presents an overview of previous IV-studies on the effect of maternal education on child's health in developed countries. Specifically, the table provides information about country, data set, sample size, applied instrument and main findings of the studies that analyze similar outcomes. $\hat{\beta}_1$ refers to the estimated effect of one more year of maternal education, see equation (3).

Table 2: Descriptive statistics

Variable	Adolescents sample		Newborns sample	
	Mean	SD	Mean	SD
Mothers				
Years of education	11.9	2.7	12.8	2.8
Year of birth	1959.9	5.4	1974.5	5.9
Number of siblings	2.6	1.9	2.1	1.7
Children				
Year of birth	1987.5	2.8	2005.1	2.3
Birth weight			3353.4	558.3
Preterm birth			16.2	36.9
Low birth weight			19.5	39.7
Currently smoking (%)	27.6	44.7		
Overweight (%)	17.8	38.2		
No sport (%)	44.8	49.7		
Poorer health (%)	47.4	49.9		
<i>N</i>	1741		977	

Note: Unweighted means and standard deviations for key variables of the sample of 18/19 year olds (adolescents) and of 0-18 month olds (newborns) as well as their mothers.

Table 3: First stage - the effect of number of siblings on years of education

Sample	Obs.	(4)	(5)	(6)
Newborns				
(Preterm birth)	962	-0.413*** (0.069) [35.77]	-0.316*** (0.057) [31.07]	-0.282*** (0.057) [24.58]
(Low birth weight)	977	-0.402*** (0.067) [36.06]	-0.311*** (0.055) [32.10]	-0.280*** (0.055) [25.72]
Adolescent daughters				
(Smoker)	867	-0.391*** (0.041) [92.76]	-0.253*** (0.038) [45.34]	-0.237*** (0.037) [40.70]
(Overweight)	859	-0.390*** (0.042) [86.68]	-0.250*** (0.039) [41.82]	-0.233*** (0.038) [37.39]
(No sport)	793	-0.378*** (0.042) [81.18]	-0.260*** (0.039) [45.21]	-0.249*** (0.039) [41.69]
(Poorer health)	843	-0.393*** (0.042) [88.87]	-0.249*** (0.038) [42.76]	-0.234*** (0.038) [38.85]
Adolescent sons				
(Smoker)	874	-0.297*** (0.041) [51.73]	-0.170*** (0.039) [18.91]	-0.149*** (0.039) [14.62]
(Overweight)	851	-0.291*** (0.041) [49.32]	-0.158*** (0.039) [16.19]	-0.137*** (0.039) [12.34]
(No sport)	805	-0.349*** (0.045) [60.77]	-0.211*** (0.042) [24.59]	-0.182*** (0.042) [18.60]
(Poorer health)	836	-0.291*** (0.042) [48.46]	-0.159*** (0.040) [15.93]	-0.136*** (0.040) [11.77]
GP education			Y	Y
GP status				Y

Note: First stage results. Marginal effects of the number of mother's siblings on her years of education, robust standard errors (in parentheses) and F-statistics (in brackets) separately for mothers of newborns, adolescent daughters and adolescent sons. Variables on the left hand side describe the sample. All regressions include controls for the child's and the mother's year of birth. Specifications 2 and 5 include additional fixed effects for the area the mother grew up and for the educational levels of the mother's parents, while specifications 3 and 6 control additionally for the grandparents' occupational prestige (ISEI score). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 4: The effect of maternal education on newborns' outcomes

Outcome	Obs.	Probit			IV-Probit		
		(1)	(2)	(3)	(4)	(5)	(6)
Preterm birth	962	-0.020*** (0.005)	-0.014** (0.006)	-0.013** (0.006)	-0.037** (0.016)	-0.022 (0.022)	-0.023 (0.025)
Low birth weight	977	-0.011** (0.005)	-0.007 (0.006)	-0.005 (0.006)	0.005 (0.020)	0.014 (0.025)	0.021 (0.028)
GP education			Y	Y		Y	Y
GP status				Y		Y	

Note: Average marginal effects of maternal education and robust standard errors (in parentheses) for Probit (1-3) and second stage of IV-Probit (4-6) models for the sample of newborns. All regressions include controls for the child's and the mother's year of birth as well as for the child's sex. Specifications 2 and 5 include additional fixed effects for the area the mother grew up and for the educational levels of the mother's parents, while specifications 3 and 6 control additionally for the grandparents' occupational prestige (ISEI score). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 5: The effect of maternal education on adolescents' outcomes

Outcome	Obs.	Probit			IV-Probit		
		(1)	(2)	(3)	(4)	(5)	(6)
Daughters							
Smoker	867	-0.017*** (0.006)	-0.020*** (0.007)	-0.018** (0.007)	-0.046*** (0.016)	-0.074*** (0.022)	-0.075*** (0.024)
Overweight	859	-0.008* (0.005)	-0.009 (0.006)	-0.006 (0.006)	0.017 (0.017)	0.029 (0.027)	0.031 (0.029)
No sport	793	-0.040*** (0.006)	-0.038*** (0.008)	-0.034*** (0.008)	-0.072*** (0.016)	-0.076*** (0.026)	-0.074*** (0.028)
Poorer health	843	-0.010 (0.007)	-0.008 (0.008)	-0.006 (0.008)	-0.009 (0.023)	-0.010 (0.039)	-0.009 (0.042)
Sons							
Smoker	874	-0.015** (0.006)	-0.018*** (0.007)	-0.019*** (0.007)	0.002 (0.027)	0.013 (0.049)	0.015 (0.056)
Overweight	851	-0.008 (0.006)	-0.005 (0.007)	-0.004 (0.007)	-0.020 (0.025)	-0.020 (0.048)	-0.010 (0.057)
No sport	805	-0.026*** (0.007)	-0.030*** (0.008)	-0.030*** (0.008)	-0.031 (0.024)	-0.052 (0.038)	-0.056 (0.043)
Poorer health	836	0.000 (0.007)	0.001 (0.008)	0.001 (0.008)	0.005 (0.031)	0.021 (0.058)	0.024 (0.067)
GP education			Y	Y	Y	Y	Y
GP status				Y			Y

Note: Average marginal effects of maternal education and robust standard errors (in parentheses) for Probit (1-3) and second stage of IV-Probit (4-6) models separately for mothers of boys/girls. All regressions include controls for the child's and the mother's year of birth. Specifications 2 and 5 include additional fixed effects for the area the mother grew up and for the educational levels of the mother's parents, while specifications 3 and 6 control additionally for the grandparents' occupational prestige (ISEI score). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 6: The effect of maternal education on adolescents' outcomes - robustness & channels

Outcome	Robustness					Channels		
	[add. controls] (7)	[dummies] (8)	[+only child] (9)	[1-3 siblings] (10)	[2SLS] (11)	[mating] (12)	[income] (13)	[ch. educ.] (14)
Daughters								
Smoker	-0.070 * * (0.029)	-0.061 * * (0.030)	-0.093*** (0.023)	-0.110*** (0.035)	-0.062* (0.034)	-0.064 (0.040)	-0.083*** (0.030)	-0.019 (0.040)
Overweight	0.041 (0.034)	-0.002 (0.032)	0.059* (0.031)	0.049 (0.068)	-0.001 (0.028)	0.065 (0.042)	0.052 (0.035)	0.046 (0.037)
No sport	-0.083*** (0.029)	-0.073 * * (0.036)	-0.070 * * (0.035)	-0.085* (0.048)	-0.071* (0.037)	-0.072 (0.048)	-0.074 * * (0.037)	-0.077 * * (0.032)
Poorer health	-0.017 (0.049)	-0.015 (0.039)	-0.017 (0.050)	-0.004 (0.093)	-0.016 (0.040)	-0.044 (0.055)	-0.009 (0.054)	-0.009 (0.053)
Sons								
Smoker	0.017 (0.081)	0.031 (0.049)	-0.057 (0.052)	0.025 (0.094)	0.034 (0.050)	0.066 (0.107)	0.037 (0.067)	0.000 (0.075)
Overweight	-0.001 (0.072)	-0.007 (0.057)	-0.065 (0.048)	0.090 * * (0.041)	-0.010 (0.046)	0.002 (0.113)	-0.000 (0.078)	-0.033 (0.073)
No sport	-0.020 (0.079)	-0.092*** (0.032)	-0.010 (0.060)	0.045 (0.097)	-0.101 * * (0.050)	-0.050 (0.128)	-0.052 (0.059)	-0.058 (0.058)
Poorer health	0.039 (0.081)	-0.008 (0.061)	0.007 (0.076)	-0.052 (0.084)	-0.006 (0.053)	0.015 (0.140)	0.026 (0.093)	0.022 (0.091)

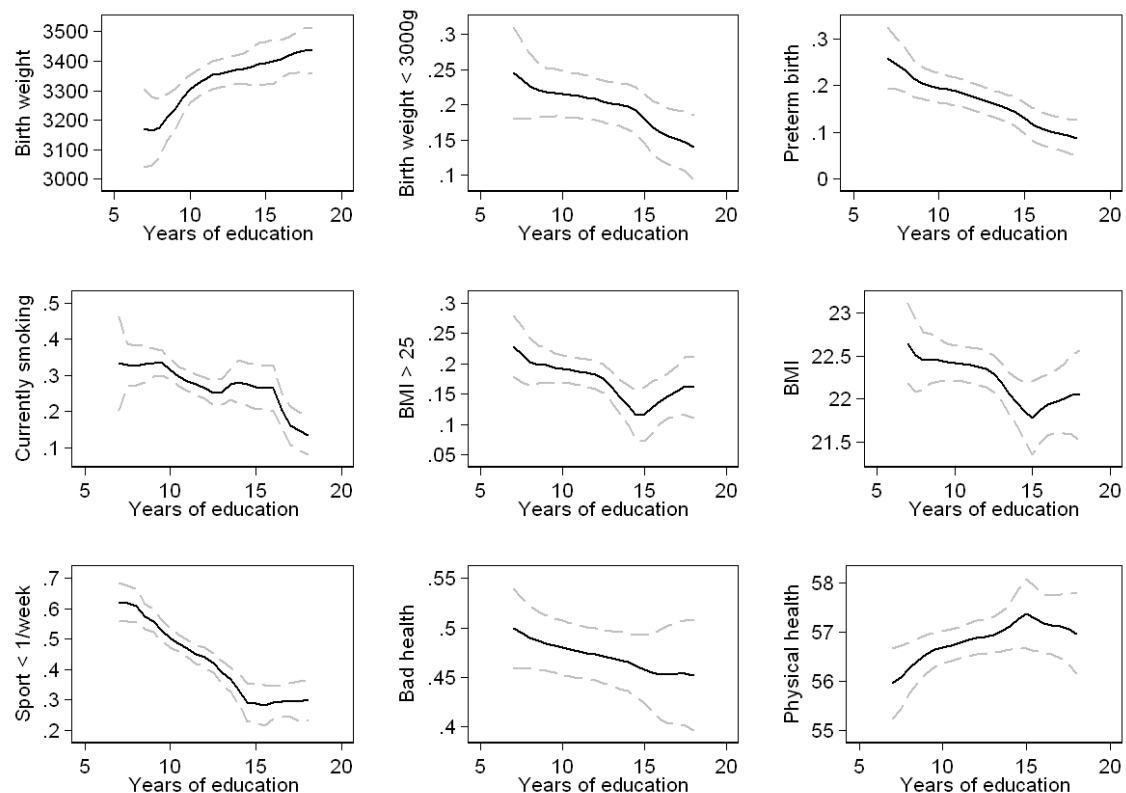
Note: See specification 6 in table 5. Specification 7 includes additional controls for maternal health, health behavior and fertility. Specification 8 rests upon dummies for 1, 2, 3, 4, 5 and 6+ siblings as instruments. Specification 9 includes also mothers without siblings, while specification 10 only considers mothers with 1, 2 or 3 siblings. Specification 11 is estimated with two-stage least squares. Specification 12 controls for the years of education of the mother's partner, specification 13 includes the logarithm of a five years average of household income and specification 14 controls for whether the child is on an academic track school. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 7: The effect of maternal education on potential channels

Outcome	Obs.	Probit/OLS		IV-Probit/2SLS	
		(1)	(2)	(3)	(4)
				(5)	(6)
Mothers' health					
Mother smokes	1794	-0.029*** (0.004)	-0.036*** (0.005)	-0.036*** (0.005)	-0.051** (0.025)
Mother overweight	1798	-0.027*** (0.005)	-0.021*** (0.005)	-0.021*** (0.005)	-0.077*** (0.009)
Mother no sport	1970	-0.039*** (0.004)	-0.040*** (0.004)	-0.039*** (0.004)	-0.064*** (0.011)
Mother poorer health	1798	-0.027*** (0.004)	-0.025*** (0.005)	-0.026*** (0.005)	-0.074*** (0.020)
				-0.001 (0.031)	-0.000 (0.035)
Household outcomes					
Partner's education (<i>in years</i>)	1456	0.651*** (0.022)	0.579*** (0.028)	0.573*** (0.029)	0.865*** (0.081)
HH income (<i>log</i>)	2087	0.072*** (0.003)	0.068*** (0.004)	0.065*** (0.004)	0.108*** (0.013)
				0.138*** (0.022)	0.140*** (0.025)
Adolescents' schooling					
Daughters					
Academic track	952	0.072*** (0.005)	0.066*** (0.006)	0.065*** (0.006)	0.094*** (0.009)
Sons					
Academic track	974	0.074*** (0.004)	0.067*** (0.005)	0.065*** (0.005)	0.091*** (0.013)
				0.099*** (0.017)	0.100*** (0.019)
				0.090*** (0.026)	0.089*** (0.032)

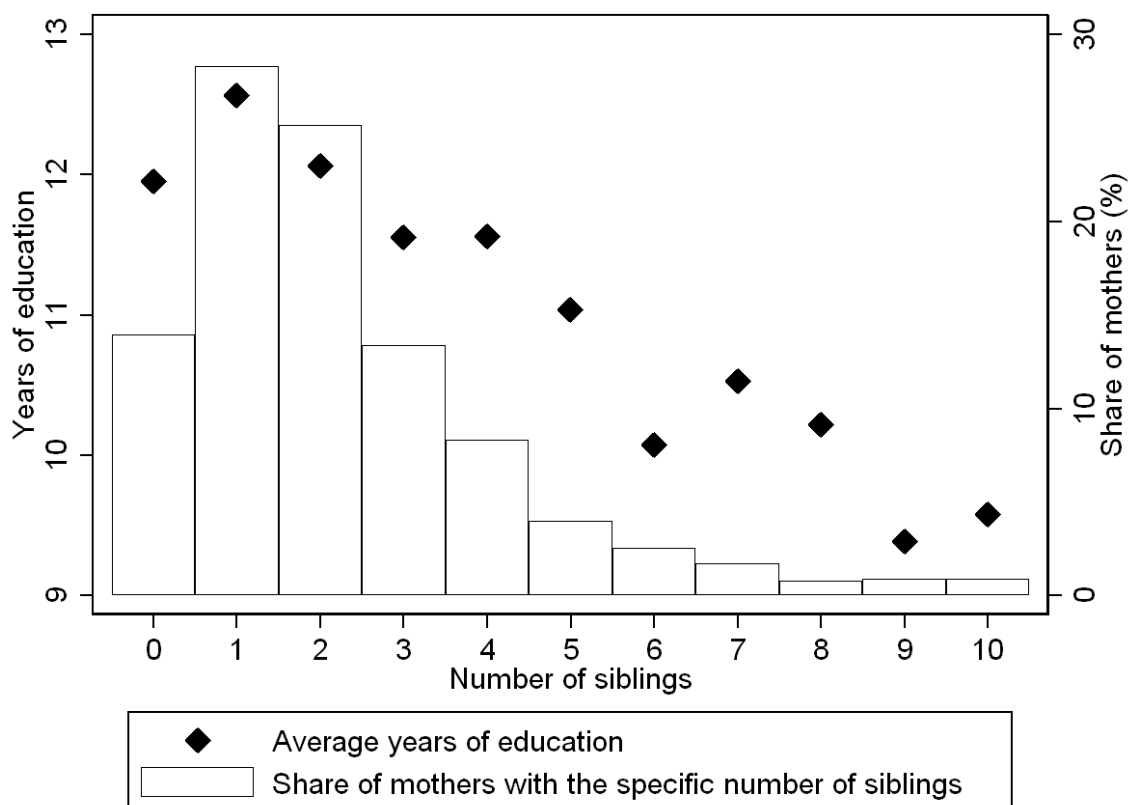
Note: Average marginal effects of maternal education and robust standard errors (in parentheses) for Probit/OLS (1-3) and second stage of IV-Probit/two-stage least squares (2SLS) models. All regressions include controls for the child's and the mother's year of birth. Specifications 2 and 5 include additional fixed effects for the area the mother grew up and for the educational levels of the mother's parents, while specifications 3 and 6 control additionally for the grandparents' occupational prestige (ISEI score). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 1: Bivariate relationships between maternal education and child's outcomes



Note: The bivariate relationship between maternal education and various child outcomes. The lines picture regression lines from non-parametric local constant estimators as well as the 95% confidence bands. The local constant estimators rely on a plugin estimator of the asymptotically optimal constant bandwidth (see [Fan & Gijbels 1996](#); [StataCorp 2009](#)) and an Epanechnikov kernel.

Figure 2: Number of siblings and years of education



Note: Average years of education for mothers with different numbers of siblings as well as the share of mothers with that number of siblings.