TI 2006-109/3
Tinbergen Institute Discussion Paper

Parental Education and Child Health: Evidence from a Schooling Reform

Maarten Lindeboom
Ana Llena Nozal
Bas van der Klaauw

Vrije Universiteit Amsterdam, and Tinbergen Institute.

Tinbergen Institute
The Tinbergen Institute is the institute for economic research of the Erasmus Universiteit Rotterdam, Universiteit van Amsterdam, and Vrije Universiteit Amsterdam.

Tinbergen Institute Amsterdam
Roetersstraat 31
1018 WB Amsterdam
The Netherlands
Tel.: +31(0)20 551 3500
Fax: +31(0)20 551 3555

Tinbergen Institute Rotterdam
Burg. Oudlaan 50
3062 PA Rotterdam
The Netherlands
Tel.: +31(0)10 408 8900
Fax: +31(0)10 408 9031

Parental Education and Child Health: Evidence from a Schooling Reform

Maarten Lindeboom [a]

Ana Llena-Nozal [b]

Bas van der Klaauw [c]

December 2006

Abstract: This paper investigates the impact of parental education on child health outcomes. To identify the causal effect we explore exogenous variation in parental education induced by a schooling reform in 1947, which raised the minimum school leaving age in the UK. Findings based on data from the National Child Development Study suggest that postponing the school leaving age by one year had little effect on the health of their offspring. Schooling did however improve economic opportunities by reducing financial difficulties among households. We conclude from this that the effects of parental income on child health are at most modest.

Keywords: returns to education, intergenerational mobility, health, regression-discontinuity.

[a] Free University Amsterdam, Tinbergen Institute, HEB, IZA & Netspar.
[b] Free University Amsterdam & Tinbergen Institute.
[c] Free University Amsterdam, Tinbergen Institute & CEPR.
Correspondence: Department of Economics, Free University of Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, The Netherlands. E-mail: mlindeboom@econ.vu.nl, allenanozal@econ.vu.nl, bklaauw@econ.vu.nl.

1 Introduction

Studies have found that poor infant health persists into adulthood and that poor infant health contributes to the health income gradient found later in life (see Case, Fertig and Paxson, 2005; and the references cited therein). It is therefore important to examine which factors determine infant health and whether their effect is causal. In this paper we look at the effect of parental education on child health.

There are different channels through which parental education can affect their children's health. Education might have a direct impact on child health because it increases the ability to acquire and process information. This helps parents to make better health investments for themselves and their children and may result in better parenting in general. Alternatively, education can affect child health through indirect pathways. An increased level of education can give access to more skilled work with higher earnings and these resources could be used to invest in health and to cushion the impact of adverse health shocks (Case, Lubotsky and Paxson, 2002). In the presence of assortative mating, individuals with a higher level of education also marry partners with higher levels of education, which positively affect family income. Case, Lubotsky and Paxson (2002) find that parents' long run income is important for the child's health. Furthermore, attending school for a longer time could lead to a change in preferences by either lowering the discount rate or increasing risk-aversion (Cutler and Lleras-Muney, 2006). Finally, increased education can increase the opportunity cost of having children and change fertility choices or delay having children. However, McCrary and Royer (2006) do not find any effect of mother's education on fertility choices.

While all these channels are potential explanations to why parental education might induce better child health, parental education and child health can also be related in non-causal ways. Indeed, endowments that are transmitted across generations can cause a positive association between parental education and child health. To overcome such endogeneity problems it is necessary to find some exogenous variation in parental education. Recently the use of schooling reforms as a source of exogenous variation has become popular in labor and health economics. Most studies focus on the causal impact of education on earnings (e.g. Harmon and Walker, 1995; Meghir and Palme, 2005; Pischke and Von Wachter, 2005) or on the effect of parental income on the education of their children (e.g. Black, Devereux and Salvanes, 2005; Chevalier, Harmon, O'Sullivan and Walker, 2005; Holmlund, Lindahl and Plug, 2006; Oreopoulos, Page and Stevens, 2006). Only a few papers have examined the impact of education on health. Oreopoulos (2006) uses changes in the minimum school leaving ages in the UK and

Ireland and finds that an extra year of schooling increases earnings and improves self-assessed health when leaving school. Lleras-Muney (2005) uses variation across states in compulsory education laws and finds that an additional year of education lowers mortality. Using Danish panel data, Arendt (2005) finds inconclusive results of education on self-reported health and body mass index. He finds, however, that an increase in education reduces the probability that a person smokes. Currie and Moretti (2003) examine the impact of college openings on women's educational attainment and their infants' health. They find that maternal education does improve their offspring's health. Part of the effect is assigned to the increased use of prenatal care and reduced smoking. McCrary and Royer (2006) exploit discontinuities in school entry policies in California and Texas to assess the effect of education on fertility and infant health outcomes. They find that education does not affect observable inputs to infant health and has only small effects on infant health. Finally, Doyle, Harmon and Walker (2005) use a schooling reform and grandparental smoking behavior to instrument parental education and income and find no effect of parental income on the health of their offspring and weak effects of parental education. They conclude from this that the significant effects of parental income on child health as found in Case, Lubotsky and Paxson (2002) and Currie, Shields and Wheatley-Price (2006) is spurious.

In this paper, we use a schooling reform that took place in the United Kingdom in 1947. The reform raised the minimum school leaving age from 14 to 15. We show that the reform only affected the schooling decision of individuals at the lower end of the education distribution; the fraction of individuals leaving school at age 16 or later remained unaffected by the reform. More precisely, due to the reform about 50% of the individuals in a birth cohort raised their school leaving age from 14 to 15. We focus our empirical analyses mainly on those parents (fathers and mothers) leaving school at age 14 and 15.[1] This means that the estimated impact of parental education should be considered as local average treatment effects (see Imbens and Angrist, 1994). We show that restricting the data increases the impact of the reform on schooling compared to using individuals with all levels of schooling as is done in previous studies. Previous approaches in this literature (e.g. Chevalier, Harmon, O'Sullivan and Walker, 2005; Doyle, Harmon and Walker, 2005; Oreopoulos, 2006) mostly included all schooling levels in the analyses, thereby implicetly assuming that reforms at the lower end of the education distribution also affect school leaving ages of those at the higher end of the education distribution. In the absence of such effects on the higher end of the education distribution this might lead to a weak instruments problem that will bias the results.

[1] This is in line with the approach taken by Black, Devereux and Salvanes (2005).

We assess the causal effect of parental education on a wide range of child health variables. These variables include health measured at birth as well as health measured later in childhood. We discussed above that parental education might affect child health through different mechanisms. We therefore also examine whether parental education causally affects parental behavior, parental health and labor market outcomes. We find little effect of a direct causal relationship between parental education and child health. We also find that increased parental education reduces possible financial difficulties in the family. We therefore conclude that the effects of parental education and income on child health are at most modest.

The remainder of this paper is organized as follows. In Section 2 we describe the dataset, and in Section 3 we discuss the background of the 1947 reform. Section 4 presents the empirical specification. The results are presented in Section 5 and we close with a discussion and conclusion in section 6.

2 Data

The National Child Development Study is a longitudinal study of about 17,000 babies born in Great Britain in the week of 3-9 March 1958. The study started as the “Perinatal Mortality Survey” and surveyed the economic and obstetric factors associated with stillbirth and infant mortality. Since the first wave, cohort members have been traced on six other occasions to monitor their physical, educational and social circumstances. The interviews were carried out in 1965 (age 7), 1969 (age 11), 1974 (age 16), 1981 (age 23), 1991 (age 33) and 1999 (age 42). For the birth survey, information was gathered from the mother and medical records. For the surveys during childhood and adolescence, interviews were carried out with parents, teachers, and the school health service. The advantage of the National Child Development Study is that it contains information on both parents and children about education, health and other background characteristics.

The main indicators of health at birth are birth weight and an indicator for whether the child experienced an illness in the first week of life. We exclude twins from our sample since their birth weight is not comparable with singletons. Illnesses at birth can be: incompatible Rh, severe jaundice, congenital malformation, convulsions (or cerebral irritation/cyanotic attacks), hypothermia, respiratory distress, infection, and pyloric stenosis. During later years in childhood and adolescence, parents are asked questions about their children’s record of illnesses, psychological problems, accidents and hospitalizations. A medical examination is performed by a physician who records the child’s specific medical problems. Using this information we develop several measures of child health. The first one is a measure of morbidity based on the number of conditions the child has experienced at ages 7, 11 and 16 (as reported by both parents and the physician)[2]. In addition, the survey contains information on the height and weight of the cohort members measured by a physician (and therefore less subject to measurement error than self-reports), which can be used to construct anthropometric indicators. Height-for-age-z-scores are built by comparing the height data with the distribution of height for a reference population, which is constructed by the US National Center for Health Statistics. Low height for age, or stunting, is an indicator of past growth failure and is associated with frequent or chronic illness, chronic inappropriate nutrition (insufficient energy intake and protein), and poverty. Height and weight are also used to construct the Body Mass Index, which is a measure for overweight and thinness. We use the height-for-age-z-scores and the Body Mass Index when the child was 7, 11 and 16.

We know the year of birth of the parents and the age at which they left full-time education. In each wave we have information on the mother's working status and on whether the family experienced financial difficulties. We choose not to use information on wages given the low response rate for this variable. The National Child Development Study records parental weight and height when the child is age 11. This information can be transformed to obtain the Body Mass Index. In addition, chronic conditions for the father and/or mother are recorded in all waves during childhood and adolescence. We use this information to construct a dummy for the presence of chronic conditions. Both can be used as measures for parental health. Finally, we have some information about fertility since the birth survey contains a measure of parity (the number of times the mother has given birth in 1958) and on the number of siblings the cohort member has at each age.

Table 1 shows sample statistics of parental and child variables for different levels of parental education. For this study, we focus on the sample of cohort members who have both their natural parents between 1965 and 1974. We observe that parents with more education have better socioeconomic and health outcomes. In particular, both more educated fathers and mothers have higher earnings and the prevalence of chronic conditions and obesity is lower among this group. Furthermore, all measures of child health are better for higher educated parents (lower probability of birth weight, illness at birth, serious conditions, stunting, and obesity). This shows the presence of the positive association between parental socioeconomic status and health that is also found in other studies.

[2] The conditions are categorized under 12 groups (see Power and Peckham, 1987).

3 Background of he 1947 reform and changes in schooling distribution

3.1 Description of the education reform

The Education Act of 1944 changed the education system for secondary schools in England and Wales. It introduced a tripartite system whereby secondary schools were divided into: grammar schools (academic track), secondary technical and secondary modern schools. Students were allocated on the basis of an exam known as the 11 plus. It also made secondary education free for all. The aims of the education reform were to "improve the future efficiency of the labor force, increase physical and mental adaptability, and prevent the mental and physical cramping caused by exposing children to monotonous occupations at an especially impressionable age" (Oreopoulos, 2006). In addition, the Act resulted in the raising of the minimum school-leaving age from 14 to 15 in April 1947. According to Galindo-Rueda (2003), the reform brought about an increase in the number of pupils that was largely concentrated among the secondary modern and technical schools where there were few entry requirements based on ability.

3.2 Distribution of schooling before and after the reform in the National Child Development Study data

The National Child Development Study includes parents born at different dates who are therefore affected differently by the reform. The first cohort of parents that is affected by the reform is born in 1934; they had to stay in school until the age of 15, compared to 14 for previous cohorts. Figure 1 shows the mean age of finishing school by year of birth for fathers and mothers. The mean age experiences a sharp raise in 1934, showing that the reform raised schooling age by on average 3 months for fathers and 4.5 months for mothers. Previous to the reform fathers' education reached a peak in 1930 and started to decline while mother's education declined later, in 1932. This is due to the fact that fathers tend to be older than mothers in our sample (see frequency of birth years in Table 2). In addition, after the original increase caused by the reform we observe a decrease in the mean age of schooling. Note that these are parents who had a child in 1958 and that less educated individuals are more likely to have children at young ages. This can lead to a sample where older individuals are more likely to have more education.

Figures 2 and 3 depict the percentages of parents leaving school at each age (stratified according to their year of birth). We see that prior to the reform more than 60% of the population left school at age 14 while between 10 and 20% (depending on the year and gender) left at age 15. Within two years after the reform, close to 70% of fathers and mothers left at age 15. The graphs show that the proportion leaving at age 16 and beyond remains similar before and after the implementation of the new minimum school leaving age. It therefore appears that the reform primarily affected those who would have left school earlier in absence of the reform. In 1934 only about 50% finished school at age 15 (55% for mothers), while 20% of mothers and 30% of fathers stayed until age 14 only. This is most likely due to partial implementation of the reform or to pupils turning 14 before the reform was fully passed. Since we do not have the exact date of birth we cannot check either hypothesis. Galindo-Rueda (2003) investigated whether behavioral responses to the reform varied according to observable characteristics. He found that mothers from smaller families and with skilled or semi-skilled parents were more likely to increase their schooling (the response was not heterogeneous for fathers).

We estimate the effect of the reform on the age at which fathers and mothers leave school. We capture the effect of the reform by a dummy for whether the individual was 14 on the year the reform was implemented and on the subsequent years it was in place. Since the reform might not fully affect the 1934 cohort like the later birth cohorts, we look at the effect of being born in 1934 and of being born in 1935 and afterwards. Additionally, for comparison purposes, we re-estimate the same model excluding those born in 1934. We perform the regressions for different birth year intervals and we also compare the effect on the entire education distribution (full sample) and only those finishing at ages 14 and 15 (restricted sample). The results are reported in Table 3 and show that the education reform had a higher impact on the restricted sample of lower educated individuals. For the restricted sample both the coefficients are higher and the standard errors are lower. For the full sample, the reform in 1947 increased the mother's education by 0.407 years. The increase for the lower educated (restricted) was 0.555 years. For males this difference was even bigger (the coefficient increased from 0.147 to 0.477). This indeed confirms that the reform mainly affected the educational choices of those individuals at the lower end of the educational distribution. Furthermore, there seems to be some sensitivity of the reform's impact to the sample of birth cohorts chosen. When looking at all education ages, it appears that the reform had a slightly larger effect for those born in 1934. The reverse is true for the sample of people leaving at ages 14 and 15: those born in 1935 and afterwards experienced a greater increase in education than those born in 1934. In addition, the effect of the reform slightly decreases as birth cohorts closer in time are taken into account.

4 Estimation methods

The schooling reform provides a natural experiment that can be used to identify the causal impact of parental schooling on a number of different outcome measures. Since close to the reform individuals are expected to be similar except for exposure to the reform, we can use regression-discontinuity techniques. The design is fuzzy as the school leaving age does not deterministically depend on exposure to the reform (e.g. Hahn, Todd and Van der Klaauw, 2001). Obviously prior to the reform some individuals left school at age 15 or later, but also after the reform still some individuals left school at age 14. Since exposure to the reform depends on the year of birth, the regression-discontinuity design suggests that we should compare individuals born close to 1934, which was the first birth cohort affected by the reform. In the fuzzy regression-discontinuity design parental education is instrumented by whether or not they were exposed to the reform. Our empirical model is summarized by the following three equations:

$$H = \beta_0 + \beta_1 E^f + \beta_2 E^m + \beta_3 S + \beta_4 P + \beta_5 R + \beta_6 A^f + \beta_7 A^m + \varepsilon \qquad (1)$$

$$E^f = \delta_0 + \delta_1 Y^f + \delta_2 S + \delta_3 P + \delta_4 R + \delta_5 A^f + \gamma \qquad (2)$$

$$E^m = \delta_0 + \delta_1 Y^m + \delta_2 S + \delta_3 P + \delta_4 R + \delta_5 A^m + \upsilon \qquad (3)$$

H represents child health, *E* is the age at which the father and mother finished school, *S* is the sex of the child, P is parity in 1958, *R* includes dummy variables for the region of residence, *A* includes the age of the father and the mother in 1958, and *Y* is a dummy for whether the individual was affected by the reform. The superscript *f* indicates that the variable relates to the father, while the superscript *m* relates to the mother.

An important reason for including parity of the child and parental age is to reduce potential biases that might arise because the sample consists of families having a child born in 1958. It cannot be ruled out that the schooling reform affects fertility decisions such as the timing of childbearing and/or the number of children. We have checked the effect of the reform on parity in 1958 and on total fertility as observed in the 1974 survey and we did not find a significant effect of the reform in these regressions. Nevertheless, it is possible that the reform affects the decision to have any children at all or to delay childbearing. Furthermore, parents affected by the reform were born in later years than parents not affected by the reform. This implies that the parents affected by the reform were younger in 1958 when the child was born. We expect that controlling for parity and parental age reduces potential biases, but we cannot rule out that some biases remain. It has to be noted that the same criticism applies to the study by McCrary and Royer (2006) who condition on mothers having their first child before age 23.

This model will estimate the causal effects of parental education on a range of child health variables: the child's birth weight, whether the child had an illness at birth, the number of chronic conditions in later childhood, height-for-age-z-scores and Body Mass Index. The results of these analyses will be discussed in Subsection 5.1.

As mentioned earlier, the impact of parental education may act on child health through various channels. Firstly, it may be that higher educated parents have more knowledge about prenatal care and care-taking of children and therefore for example they smoke less during pregnancy or more often breastfeed their child. Secondly, it is possible that increased education may have a direct impact on parents' health and that better parental health is transmitted across generations. Thirdly, health benefits might come from increased earnings or changed labor supply choices (particularly for women). We will also examine whether there is a causal effect of education on parental outcome variables such as: maternal smoking, whether the child was breastfed, an indicator of a chronic condition for the father or mother, father's Body Mass Index, or mother's Body Mass Index, the work status of the mother and whether the family experienced financial difficulties. The results of these analyses will be discussed in Subsection 5.2.

Identification from the regression-discontinuity design assumes that the population affected by the reform and the population not affected by the reform differ only in exposure to the reform. In practice, this assumption is justified only if the sample consists of birth cohorts sufficiently close to 1934 in order to avoid other cohort and trend effects. Indeed, children born to older parents might face a different socioeconomic environment than those born to younger parents, which might affect the outcomes of interest. We estimate our model for different subsamples of birth cohorts. It is obvious that if we restrict the subsample to only a few birth cohorts, we have a relatively small sample size. On the other hand if we take a subsample with many birth cohorts, other cohort and trend effects might bias the estimated effects. When restricting to a subsample of particular birth cohorts, we include only families with both parents born in the included birth years. As mentioned in the previous subsection, in 1934 there might have been only partial compliance to the reform. Therefore, as instrumental variables in equations (2) and (3), we include separate dummy variables for being born in 1934 and for being born in 1935 or later. Furthermore, we construct subsamples from which we exclude families with parents born in 1934. As mentioned in the previous section, the reform only affected the behavior of those individuals for which the reform was binding. The fraction of individuals leaving school at age 16 or later did not change due to the reform. We estimate our model both for the full sample containing individuals with all levels of education and a restricted sample containing only individuals who left school at age 14 or 15. The interpretation of the coefficients β_1 and β_2 differs between both sample choices. In case we use the full sample, the coefficients describe homogenous effects of education. We have shown that the reform affected only individuals in the lower part of the educational distribution. This implies that if we use the full sample, the linear first stage regressions (2) and (3) are wrongly specified. If we use the restricted sample, the coefficients β_1 and β_2 should be interpreted as local effect of schooling, since these coefficient only measure educational effects for those parents persuaded to obtain one additional year of education due to the reform. Under the assumption that no individual will lower his/her level of education due to the reform (monotonicity assumption), our estimated effects should be interpreted within the local average treatment effect framework (Imbens and Angrist, 1994). In particular, this implies that our estimated effects are the educational effects for those individuals who due to the reform increased their school leaving age from 14 to 15. From the previous section we have seen that this is about 50% of a birth cohort. The results are nevertheless interesting from a policy point of view because they focus on those at the bottom of the education distribution, the same group that is often aimed at in public programs.

5 Results

5.1 Child health

The OLS estimation results for equation (1) are presented in Table 4. The table includes the effect of parental education on infant health at the time of birth (measured by birth weight and whether or not the child has an illness at birth) and at later ages in childhood (the number of conditions and height-for-age-z-scores and Body Mass Index at ages 7, 11 and 16). We present the results for different samples of birth cohorts and education groups. The OLS estimates show some significant associations between parental education and indicators for their offspring's health at birth. Higher birth weight is related to more parental education (either father or mother depending on the sample). The coefficient is also higher when focusing on the restricted sample with less educated parents. There is, on the other hand no effect of parental education on the probability of an illness at birth (the sample of less educated parents born in 1933-1935 being the exception).

For later childhood health, the full sample shows that there exists a positive association between parental schooling and child health when looking at anthropometric measures. Both maternal and paternal education levels are associated with higher height-for-age-z-scores for children. When we focus on fewer birth years around the year of the reform, we find only maternal education to be significantly associated with higher height-for-age-z-scores. Father's education is correlated with Body Mass Index; more years of schooling for the father are associated with lower Body Mass Index. For the full sample, we never find a significant association between either father's or mother's education and the number of conditions during later childhood. We find no significant association between parental education and the child's health measures between ages 7 to 16 for the sample of lower educated parents.

Table 5 presents the instrumental variables (IV) results. We instrument the age at which the parents left school by whether they were affected by the reform. Almost all results are statistically insignificant, suggesting that there is no causal effect of increased parental education. Compared to the OLS results, the lack of significance is not always caused by reduced parameter estimates. For example, for the number of conditions and for height-for age-z-scores, we quite often see that both the estimated coefficients and the standard error increases. For the sample of parents leaving school at age 14-15 we find only that father's education has a marginally significant effect on the probability of having an illness at birth. But this effect is only present in the subsample of the birth cohorts 1931-1937 and disappears in the other subsamples of birth cohorts.

Epidemiological and economic studies on the long run effects of poor infant health often find different results for boys and girls. For instance, Leon et al. (1998) find that the relationship between birth weight and death from ischaemic heart disease is significant for men and not for women. Similarly, Van den Berg. Lindeboom and Portrait (2006) find that being born in a recession increases mortality risk at later ages and that this effect is only significant for men. We therefore also performed separate IV analyses for boys and girls. This did not alter the results. In none of the analyses we found any significant effect of parental education on the infant's health.

In the economic literature intergenerational effects are most often estimated separately for fathers and mothers (Black, Devereux and Salvanes, 2005; Holmlund, Lindahl and Plug, 2006). The interpretation of the coefficients of education in separate regressions differs from those in our model where both father's and mother's education are included. In particular, when separate regressions are done for the father and mother, the estimated effects also include the effects of whom he/she marries (Behrman and Rosenzweig, 2002). Effects of assortative mating on education are thus included in the parameter estimate of the education coefficient when one performs separate regressions for both parents. In a model where the education of both parents is included one can interpret the results as the direct effects of each parents' education. However, more importantly, performing separate analyses for fathers and mothers can lead to inconsistent estimates in the case of assortative mating, even if one performs IV analyses. The main reasoning behind this is that if the father and mother are close in age, the reform is not a valid instrumental variable anymore. If one parent is affected by the reform, this increases the probability that also the partner is also affected by the reform. Therefore, the increased education of the partner does not only run via the educational level of the parent, but also via the reform. Since the educational level of the partner is not included as regressor, it is absorbed in the error term of the second stage. Assortative matching on age thus causes that the variables describing the reform are correlated with the second-stage error terms, which violates the validity condition for instrumental variables. Our data shows that the correlation between year of birth of the father and mother is 0.79. The correlation for exposure to the reform is 0.53, while the correlation in years of education is 0.57.

It is, however, interesting to see how the effects of education change if we do separate analyses for fathers and mothers. The results from IV estimation for mothers and fathers are presented in Table 6 and 7 respectively. Most effects for parental education are very small and not significant. For mothers, we only find in the 1933-1935 sample that more education reduces the height-for-age-z-score. For fathers we find similarly in the 1933-1935 sample a significant negative effect of education on the height-for-age-z-score.

5.2 Parental outcomes

We found little evidence for a causal impact of parental education on child heath. In the introduction we have specified a number of channels through which parental education could affect child health. In particular, we mentioned that parental education may affect child health indirectly via parental behavior, parental health and parental financial resources. By investigating the causal impact of education on these parental outcomes measures, we might be able to rule out whether these parental outcomes might affect child health. The underlying idea is that when parental education for example significantly increases parental financial resources, it is very unlikely that parental financial resources have a substantial impact on child health, given that we do not find any effect of parental education on child health. In Table 8 we show results from OLS estimation for the effect of parental education on parental outcomes. Table 9 presents the IV results.

Education could affect child health through improved prenatal care, for instance because better educated parents have more knowledge of the adverse effects of maternal smoking on infant health. The OLS results in the upper part of Table 8 show that parental schooling (father's or mother's or both depending on the sample) is significantly associated with smoking during pregnancy and whether or not the mother breastfeeds the child. When we restrict the sample to those parents leaving school at age 14-15, the significant effect of parental education on pregnancy smoking disappears and only marginally significant effects of mother's education on breastfeeding remain. When we furthermore instrument parental education by the reform none of the effects remain significant (see Table 9). The increase in education due to the reform did not decrease mother's smoking during the pregnancy, nor did it increase breastfeeding.

The IV estimation results show no significant effect of education on any of the parental health variables (chronic illnesses and Body Mass Index of both the father and mother).[3] This is different from the OLS estimates. These OLS estimates indicate a negative association between education and having a chronic illness and education and Body Mass Index. This holds for fathers and mothers and for different samples.[4]

The OLS results for the full sample show that mother's education is positively associated with being at work. A higher education of the father is negatively related with employment status of the mother. When we restrict the sample to those with fewer years of education, we no longer find a significant association between education and mother's working status (except for the 1933-1935 birth years). The IV results for this variable are in general larger than the OLS results and in 2 of the 3 subsamples we find an effect of father's education on the mother's work status that is significant at 10%.

Table 8 shows that more education is associated with reduced chances of having financial difficulties. For the full sample this even holds for all cohort years. Table 8 also shows that the effect of the mother is generally larger than the effect of the father. The IV results show that more schooling for the mother is associated with a decrease in financial difficulties. This holds for the full sample and for the restricted sample. The estimates in the restricted sample are most often slightly smaller than the estimates in the full sample. Our result that more education causally leads to fewer financial difficulties is in line with the results of the vast literature on the returns to education. For example, Oreopoulos (2006) finds using the same education reform we

[3] Body Mass Index as a measure of health is non-linear since both low and high values reflect poor health. We have therefore experimented with a measure of parental obesity and being underweight and found no significant effects either.

[4] For the sample of individuals finishing school at 14 or 15 both the OLS and IV estimates show no association between education and paternal health (Body Mass Index, chronic illnesses). Only the subsample of those born in 1933-1935 shows some significant effects.

use large and significant earnings returns to education. It is generally found that more education leads to higher earnings and that the IV results are generally larger than the OLS results (see for instance the survey of Card, 1999).

The significant causal effect of education on parental income sheds some more light on the potential effect of income in determining child health. Given that parental education has a causal effect on financial resources but no direct effect on the child health, we can conclude that parental income can at most have a very modest effect on child health. For the population of parents affected by the reform we do not find any effect of education on own health or on parental care. Therefore, our results do not rule out that parental health and/or parental care are important for child health.

6 Discussion and Conclusion

We examined the intergenerational effects of education on child health. As in most of the empirical literature, our data shows a strong positive association between parental socioeconomic status and child health. To investigate the causality of the relationship, we have exploited exogenous variation in parental educational due to a schooling reform on the minimum school leaving age. We have shown that the schooling reform only affected the educational decisions of individuals at the lower end of the educational distribution. In particular, about 50% of all individuals in a birth cohort were affected. The education reform appears to have had a substantial positive effect on time in schooling. For males additional schooling can be as high as 0.6 years, for females this is 0.7 years. Our results provide little evidence of a direct causal effect of parental education on child health. There is however more robust evidence of the positive effect of increasing education on living standards since an extra year of schooling decreases the household's financial difficulties. Given the fact that education has a causal impact on financial resources but little impact on child health, this raises the question as to what extent parental income does influence offspring health outcomes. For the population that is affected by the reform we do not find any effect of education on parental health or on parental care. Therefore our results do not rule out that parental health and/or parental care are important for child health.

Our findings are line with finding from the literature on the intergenerational transmission of education. Black, Devereux and Salvanes (2003) use a change in the educational system in Norway to assess the causal effect of parental education on the child's education. They also do not find a causal effect from parental education. They conclude from their findings that the intergenerational correlation in education is due to family circumstances and/or inherited ability. This may also be the case for child health.

It is interesting to compare our findings to two studies on the intergenerational effects of education on child health. Currie and Moretti (2003) find significant improvement of infant health for women attending College. This seems to contrast our findings. However, they argue that the improvements in child health come from increases in prenatal care and reduced smoking due to the higher education of the mother. We did not find any changes in prenatal behavior or child care due to the increased schooling. Our results are completely in line with McCrary and Royer (2006). They exploit discontinuities in school entry policies. In their set up the discontinuities can lead to 0.14 to 0.25 fewer years of education for those born beyond the school entry date. This change is substantially smaller than the changes in our sample induced by the reform. They examine the effect of education for those mothers giving birth before the age of 23 and find limited returns to education. They argue that this is because they focus on a sample of low educated women at risk of dropping out of school (like in our sample). Alternatively, the differences in results between Currie and Moretti (1999) on the one hand and our study and McCrary and Royer (2006) on the other hand can be explained by the fact that the type of policy is different: our study focuses on a policy manipulating time of exit while Currie and Moretti (2003) look at a policy promoting College entrance.[5] The policies thus interfere at different margins of the parental educational distribution. One might take from combining the studies that positive intergenerational effects on child health appear when the parents reach a sufficiently high educational level. Besides most of those affected by the 1947 reform went into general secondary education and one could argue that because of this the value added of the additional year of schooling was very small. So, the quality of education rather than the quantity of education is important.

[5] McCrary and Royer (2006) is more similar to our study as they also consider low educated mothers and they focus on the time in school of these women.

References

Arendt, J.N. (2005), Does Education Cause Better Health? A Panel Data Analysis Using School Reforms for Identification, *Economics of Education Review* **24,** 149-160.

Behrman, J.R. and M.R. Rosenzweig (2002), Does Increasing women's Schooling Raise the Schooling of the Next Generation?, *American Economic Review* **92**, 323-334.

Black, S., P. Devereux and K. Salvanes (2005), Why the Apple Doesn't Fall Far: Understanding the Intergenerational Transmission of Education, *American Economic Review* **95,** 437-449.

Card, D. (1999), The Causal Effect of Education on Earnings, in O.C. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics, Volume 3A*, North-Holland.

Case, A., A. Fertig and C. Paxson (2005), The Lasting Impact on Childhood Health and Circumstance, *Journal of Health Economics* **24**, 365-389.

Case, A., M. Lubotsky and C. Paxson (2002), Economic Status and Health in Childhood: The Origins of the Gradient, *American Economic Review* **92,** 1308-1334.

Chevalier, A., C. Harmon, V. O'Sullivan and I. Walker (2005), The Impact of Parental Income and Education on the Schooling of their Children. *IZA Discussion Papers Series*, Discussion Paper 1496.

Currie, J. and E. Moretti (2003), Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings, *Quarterly Journal of Economics* **118,** 1495-1532.

Currie, A., M.A. Shields and S. Wheatley-Price (2006), Is the Child Health / Family Income Gradient Universal?, *Journal of Health Economics*, forthcoming.

Currie, J. and M. Stabile (2003), Socioeconomic Status and Child Health: Why Is the Relationship Stronger for Older Children?, *American Economic Review* **93,** 1813-1823.

Cutler, D.M. and A. Lleras-Muney (2006), Education and Health: Evaluating Theories and Evidence. *National Bureau Economic Research Working Paper Series*, Working Paper 12352.

Doyle, O., C. Harmon, I. Walker (2005), The Impact of Parental Income and Education on the Health of their Children, *IZA Discussion Paper Series*, Discussion Paper 1832.

Galindo-Rueda, F. (2003), The Intergenerational Effect of Parental Schooling: Evidence from the British 1947 School Leaving Reform, Centre for Economic Performance, mimeo.

Hahn, J., P. Todd and W. van der Klaauw (2001), Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design, *Econometrica* **69**, 201-209.

Harmon, C. and I. Walker (1995), Estimates of the Economic Return to Schooling for the United Kingdom, *American Economic Review* **85**, 1278-1296.

Holmlund, H., M. Lindahl and E. Plug (2006), Estimating Intergenerational Schooling Effects: A Comparison of Methods, Mimeo.

Imbens, G.W. and J.D. Angrist (1994), Identification and Estimation of Local Average Treatment Effects, *Econometrica* **62,** 467-475.

Leon, D.A., H.O. Lithell, D. Vågerö, I. Koupilová, R. Mohsen, L. Berglund, U-B. Lithell and P.M. McKeigne (1998), Reduced Fetal Growth Rate and Increased Risk of Death from Ischaemic Heart Disease: Cohort Study of 15 000 Swedish Men and Women Born 1915-29. *British Medical Journal* **317**, 241-245.

Lleras-Muney, A. (2005), The Relationship Between Education and Adult Mortality in the United States, *Review of Economic Studies* **72**, 189-221.

McCrary, J. and H. Royer (2006), The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth, University of Michigan, Working Paper.

Meghir, C. and M. Palme (2005), Educational Reform, Ability and Parental Background, *American Economic Review* **95,** 414-424.

Oreopoulos, P. (2006), Estimating Average and Local Average Treatment Effects of Education When Compulsory Schooling Laws Really Matter, *American Economic Review* **96,** 152-175.

Oreopoulos, P., M.E. Page and A.H. Stevens (2006), The Intergenerational Effects of Compulsory Schooling, *Journal of Labor Economics* **24**, 729-760.

Pischke, J.-S. and T. von Wachter (2005) Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation, *National Bureau Economic Research Working Paper Series*, Working Paper 11414.

Power, C. and C. Peckham (1987), Childhood Morbidity and Adult Ill-Health, *National child Development Study User Support Group*, Working Paper No. 37.

Van den Berg, G.J., M. Lindeboom and F. Portrait (2006), Economic Conditions Early In Life and Individual Mortality, *American Economic Review* **96**, 290-302.

Table 1: Parental and child variables by level of parental schooling

	Fathers			Mothers		
	14	15	16+	14	15	16+
Financial difficulties in the family (Avg over 1965, 1969, 1974)	9.56%	9.75%	3.09%	10.57%	9.79%	3.86%
Mother works (Avg over 1965, 1969, 1974)	53.23%	59.52%	48.96%	57.85%	59.39%	53.53%
Father chronic conditions (Avg over 1969, 1974)	8.26%	4.78%	4.03%	8.62%	5.63%	4.52%
Mother chronic conditions (Avg over 1969, 1974)	6.19%	5.64%	4.24%	6.68%	5.41%	4.26%
Father obese in 1974	5.01%	3.41%	3.49%	5.05%	3.69%	3.86%
Mother obese in 1974	8.08%	5.67%	2.68%	7.87%	6.54%	3.24%
Maternal smoking during pregnancy	36.20%	31.63%	24.57%	37.71%	33.42%	21.81%
Breastfeeding	64.98%	71.36%	76.47%	63.19%	72.36%	75.54%
Child birth weight in kg	3.34	3.31	3.39	3.35	3.30	3.39
Child illness at birth	3.03%	2.23%	2.41%	3.19%	2.63%	2.13%
Child number of conditions (Avg over 1965, 1969, 1974)	2.17	2.16	2.07	2.15	2.22	2.10
Child stunt (Avg over 1965, 1969, 1974)	2.68%	2.69%	1.03%	2.58%	2.85%	1.12%
Child obese (Avg over 1965, 1969, 1974)	4.42%	3.28%	3.09%	4.67%	3.27%	3.10%

Table 2: Distribution of parents schooling by year of birth

	Fathers			Mothers		
	Mean	SD	Freq.	Mean	SD	Freq.
1927	14,96	2,11	1644	14,81	1,74	1254
1928	14,94	1,93	1947	14,83	1,64	1557
1929	14,94	2,00	2019	14,84	1,67	1905
1930	15,03	2,03	2133	14,86	1,62	1857
1931	14,99	1,92	1989	14,92	1,71	2316
1932	14,86	1,62	1977	14,96	1,71	2040
1933	14,79	1,65	1785	14,82	1,39	2055
1934	15,09	1,35	1500	15,24	1,29	2019
1935	15,06	0,94	1305	15,25	1,04	1986
1936	15,14	1,14	966	15,17	0,98	1860
1937	15,15	1,08	588	15,19	0,87	1608
1938	15,01	0,73	330	15,12	0,68	1245
1939	15,03	0,74	174	15,09	0,65	744

Table 3: Effect of the reform of school leaving age

	Father		Mother	
	Full sample	Restricted sample	Full sample	Restricted sample
All years				
Born in 1934	0.147 (0.064)**	0.477 (0.024)**	0.407 (0.053)**	0.555 (0.020)**
Born in 1935 and afterwards	0.145 (0.036)**	0.671 (0.013)**	0.323 (0.025)**	0.708 (0.008)**
Observations	11072	8389	11274	8593
1930-1938				
Born in 1934	0.176 (0.070)**	0.443 (0.026)**	0.355 (0.058)**	0.573 (0.021)**
Born in 1935 and afterwards	0.182 (0.047)**	0.628 (0.015)**	0.292 (0.036)**	0.721 (0.011)**
Observations	4186	3342	5669	4350
1931-1937				
Born in 1934	0.218 (0.072)**	0.425 (0.026)**	0.347 (0.061)**	0.570 (0.022)**
Born in 1935 and afterwards	0.235 (0.052)**	0.613 (0.017)**	0.299 (0.042)**	0.704 (0.013)**
Observations	3365	2806	4625	3527
1933-1935				
Born in 1934	0.297 (0.090)**	0.383 (0.031)**	0.424 (0.072)**	0.552 (0.026)**
Born in 1935 and afterwards	0.266 (0.081)**	0.544 (0.029)**	0.423 (0.066)**	0.644 (0.024)**
Observations	1530	1258	2024	1508
1930-1938 excluding 1934				
Born in 1935 and afterwards	0.182 (0.047)**	0.628 (0.015)**	0.292 (0.036)**	0.721 (0.011)**
Observations	3686	2924	4996	3854

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level

Table 4: Parents education and child's health- OLS

	Full sample					Parents finishing at age 14-15				
	Birth weight	Illness at birth	Number of conditions	Height-for age-Z scores	Body Mass Index	Birth weight	Illness at birth	Number of conditions	Height-for age-Z scores	Body Mass Index
1930-1938										
Father	0.007	0.000	0.000	0.028	-0.040	0.084	0.008	-0.110	0.073	0.049
	(0.006)	(0.002)	(0.015)	(0.013)**	(0.026)	(0.026)**	(0.008)	(0.069)	(0.054)	(0.109)
Mother	0.020	-0.001	-0.014	0.039	-0.002	-0.035	-0.008	-0.011	-0.062	-0.085
	(0.008)**	(0.003)	(0.021)	(0.016)**	(0.034)	(0.029)	(0.009)	(0.075)	(0.057)	(0.119)
P-value joint	0.000	0.951	0.725	0.000	0.150	0.006	0.515	0.238	0.314	0.752
Observations	3331	3459	8186	7921	7921	2287	2381	5609	5415	5415
1931-1937										
Father	0.005	-0.003	-0.009	0.026	-0.085	0.080	0.005	-0.116	0.046	-0.035
	(0.007)	(0.002)	(0.018)	(0.015)*	(0.029)**	(0.030)**	(0.010)	(0.085)	(0.062)	(0.131)
Mother	0.018	0.001	-0.021	0.041	0.029	-0.015	-0.001	-0.021	-0.037	-0.117
	(0.010)*	(0.003)	(0.025)	(0.019)**	(0.041)	(0.033)	(0.010)	(0.091)	(0.066)	(0.144)
P-value joint	0.023	0.496	0.367	0.000	0.008	0.028	0.834	0.304	0.726	0.625
Observations	2345	2434	5740	5543	5543	1606	1669	3928	3786	3786
1933-1935										
Father	0.014	0.009	-0.057	0.018	-0.171	0.088	-0.200	-0.231	-0.029	-0.357
	(0.017)	(0.006)	(0.043)	(0.027)	(0.058)**	(0.055)	(0.100)*	(0.142)	(0.105)	(0.243)
Mother	0.013	-0.008	0.001	0.080	0.165	-0.109	-0.021	-0.077	-0.048	-0.355
	(0.019)	(0.007)	(0.054)	(0.034)**	(0.080)**	(0.058)*	(0.119)	(0.154)	(0.112)	(0.276)
P-value joint	0.396	0.311	0.344	0.008	0.011	0.109	0.099	0.133	0.812	0.027
Observations	543	561	1321	1288	1288	372	2365	900	868	868
1930-1938, excluding 1934										
Father	-0.000	0.000	0.017	0.023	-0.058	0.099	0.010	-0.023	0.047	0.082
	(0.007)	(0.002)	(0.017)	(0.015)	(0.028)**	(0.032)**	(0.010)	(0.084)	(0.066)	(0.128)
Mother	0.028	-0.002	-0.024	0.047	0.006	-0.002	-0.011	-0.063	-0.062	-0.092
	(0.009)	(0.003)	(0.022)	(0.018)**	(0.039)	(0.004)	(0.011)	(0.091)	(0.068)	(0.141)
P-value joint	0.002	0.785	0.487	0.000	0.042	0.006	0.483	0.697	0.599	0.719
Observations	2532	2612	6221	6032	6032	1746	1816	4282	4151	4151

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level. For each interval, both the mother and the father are born within those years. Regressions are performed for children living with their natural parents and include sex of child, parity, regional dummies, and parental age. The results for the number of conditions, height-for age-Z scores and Body Mass Index are based on observations when the child was 7, 11 and 16 years old. We control for the age of the child and the estimation includes clustered standard errors. Disaggregated analyses are available upon request.

Table 5: Parents education and child's health – IV

	full sample					Parents finishing at age 14-15				
	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index
1930-1938										
Father	0.094	0.002	0.134	0.091	-0.301	0.049	-0.018	-0.066	-0.058	-0.458
	(0.091)	(0.027)	(0.209)	(0.151)	(0.327)	(0.099)	(0.031)	(0.241)	(0.190)	(0.391)
Mother	-0.121	0.000	0.116	-0.059	-0.175	-0.145	-0.005	0.058	-0.145	-0.382
	(0.078)	(0.023)	(0.195)	(0.142)	(0.313)	(0.075)*	(0.023)	(0.184)	(0.139)	(0.296)
P-value joint	0.253	0.997	0.556	0.810	0.460	0.152	0.810	0.929	0.519	0.165
Observations	3331	3459	8186	7921	7921	2287	2381	5609	5415	5415
1931-1937										
Father	0.087	-0.017	0.183	0.024	-0.285	0.172	-0.073	-0.036	-0.018	-0.070
	(0.137)	(0.040)	(0.353)	(0.257)	(0.580)	(0.138)	(0.043)*	(0.349)	(0.272)	(0.572)
Mother	-0.105	0.006	0.241	-0.231	-0.418	-0.045	0.009	0.128	-0.214	-0.482
	(0.127)	(0.036)	(0.320)	(0.234)	(0.483)	(0.097)	(0.030)	(0.245)	(0.186)	(0.388)
P-value joint	0.533	0.885	0.655	0.609	0.625	0.459	0.241	0.870	0.471	0.411
Observations	2345	2434	5740	5543	5543	1606	1669	3928	3786	3786
1933-1935										
Father	-0.025	-0.012	0.055	-0.056	-0.301	0.024	-0.011	0.102	-0.388	-0.832
	(0.105)	(0.035)	(0.278)	(0.162)	(0.454)	(0.121)	(0.039)	(0.305)	(0.243)	(0.574)
Mother	-0.240	-0.054	-0.525	0.105	-0.095	-0.098	-0.030	-0.363	-0.062	1.380
	(0.187)	(0.060)	(0.568)	(0.381)	(0.822)	(0.109)	(0.035)	(0.294)	(0.216)	(1.121)
P-value joint	0.437	0.652	0.564	0.872	0.791	0.656	0.554	0.457	0.107	0.284
Observations	543	561	1321	1288	1288	372	386	900	868	868
1930-1938, excluding 1934										
Father	0.183	-0.006	0.161	-0.037	-0.011	0.094	-0.013	-0.049	-0.125	-0.014
	(0.178)	(0.046)	(0.330)	(0.258)	(0.525)	(0.120)	(0.038)	(0.286)	(0.234)	(0.455)
Mother	-0.201	0.035	0.059	-0.132	-0.497	-0.153	0.031	0.026	-0.316	-0.567
	(0.142)	(0.037)	(0.305)	(0.226)	(0.467)	(0.097)	(0.030)	(0.230)	(0.174)	(0.360)
P-value joint	0.362	0.544	0.688	0.668	0.396	0.262	0.595	0.982	0.132	0.277
Observations	2532	2629	6221	6032	6032	1746	1816	4282	4151	4151

Robust standard errors in parentheses; * significant at 10% level; ** Significant at 5% level. For each interval, both the mother and the father are born within those years. The regressions are performed for those children with their natural parents. Extra controls as in Table 4.

Table 6:Separate analyses: Mother's education and child's health IV

	full sample					finishing at age 14-15				
	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index
1930-1938										
Mother	-0.063	-0.009	0.041	0.063	-0.201	-0.094	-0.005	-0.007	-0.061	-0.395
	(0.071)	(0.022)	(0.162)	(0.127)	(0.278)	(0.057)	(0.019)	(0.143)	(0.107)	(0.231)
Observations	5337	5515	13043	12618	12618	4094	4229	9952	9601	9601
1931-1937										
Mother	-0.029	-0.009	0.010	0.096	-0.125	-0.057	-0.008	-0.005	-0.004	-0.374
	(0.073)	(0.023)	(0.164)	(0.130)	(0.281)	(0.067)	(0.022)	(0.167)	(0.126)	(0.269)
Observations	4342	4496	10625	10277	10277	3313	3426	8054	7761	7761
1933-1935										
Mother	-0.107	-0.020	0.083	-0.093	-0.206	-0.109	-0.015	0.053	-0.161	-0.266
	(0.067)	(0.020)	(0.150)	(0.120)	(0.260)	(0.047)**	(0.016)	(0.118)	(0.088)*	(0.188)
Observations	1908	1971	4678	4531	4531	1426	1466	3469	3335	3335
1930-1938, excluding 1934										
Mother	-0.073	0.011	-0.022	0.059	-0.329	-0.101	0.006	-0.010	-0.060	-0.423
	(0.103)	(0.031)	(0.225)	(0.175)	(0.392)	(0.065)	(0.022)	(0.164)	(0.121)	(0.262)
Observations	4707	4861	11460	11075	11075	3627	3747	8795	8480	8480

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level.
The regressions are performed for those children with their natural parents. Extra controls as in Table 4.

Table 7: Separate analyses: Father's education and child's health IV

	full sample					finishing at age 14-15				
	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index	Birth weight	Illness at birth	Number of conditions	Height-for age-Z-scores	Body Mass Index
1930-1938										
Father	0.034	-0.011	0.108	-0.043	-0.393	-0.003	-0.010	0.053	-0.135	-0.424
	(0.090)	(0.028)	(0.224)	(0.177)	(0.376)	(0.084)	(0.027)	(0.216)	(0.175)	(0.356)
Observations	3944	4093	9614	9291	9291	3141	3266	7650	7392	7392
1931-1937										
Father	0.016	-0.035	0.219	0.011	-0.321	-0.026	-0.029	0.077	-0.247	-0.505
	(0.112)	(0.036)	(0.319)	(0.239)	(0.514)	(0.105)	(0.033)	(0.279)	(0.230)	(0.464)
Observations	3167	3286	7692	7423	7423	2543	2645	6193	5973	5973
1933-1935										
Father	-0.019	-0.010	0.422	-0.181	-0.445	-0.009	-0.017	0.150	-0.237	-0.286
	(0.104)	(0.033)	(0.355)	(0.236)	(0.494)	(0.063)	(0.019)	(0.165)	(0.130)*	(0.256)
Observations	1444	1496	3475	3362	3362	1182	1227	2837	2735	2735
1930-1938, excluding 1934										
Father	0.056	-0.015	-0.057	-0.118	-0.308	0.034	-0.011	-0.089	-0.161	-0.239
	(0.101)	(0.031)	(0.243)	(0.209)	(0.421)	(0.091)	(0.029)	(0.229)	(0.186)	(0.375)
Observations	3468	3601	8479	8219	8219	2764	2874	6751	6553	6553

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level.
The regressions are performed for those children with their natural parents. Extra controls as in Table 4.

Table .8: Parental education on parental variables –OLS results

	Maternal smoking during pregnancy	Breastfeeding	Father Illness	Mother Illness	Body Mass Index Father	Body Mass Index Mother	Mother work	Financial difficulties
All education years								
1930-1938								
Father	-0.012	0.022	-0.008	-0.004	-0.104	-0.179	-0.018	-0.009
	(0.006)**	(0.006)**	(0.002)**	(0.002)*	(0.043)**	(0.054)**	(0.005)**	(0.002)**
Mother	-0.025	0.018	0.000	-0.001	-0.077	-0.105	0.013	-0.008
	(0.008)**	(0.008)**	(0.003)	(0.003)	(0.054)	(0.067)	(0.006)**	(0.002)**
P-value	0.000	0.000	0.000	0.018	0.000	0.000	0.003	0.000
Observations	3459	3121	5966	5966	2849	2849	8947	8906
1933-1935								
Father	-0.027	0.029	-0.018	-0.006	-0.115	-0.188	-0.007	-0.008
	(0.016)*	(0.016)*	(0.006)**	(0.007)	(0.114)	(0.134)	(0.015)	(0.004)*
Mother	-0.001	0.012	0.002	-0.001	-0.156	-0.084	0.025	-0.014
	(0.019)	(0.018)	(0.007)	(0.008)	(0.132)	(0.159)	(0.014)*	(0.005)**
P-value	0.184	0.053	0.002	0.484	0.113	0.175	0.181	0.000
Observations	561	495	970	970	463	463	1449	1446
1930-1938 except 1934								
Father	-0.008	0.020	-0.008	-0.003	-0.116	-0.222	-0.019	-0.008
	(0.007)	(0.007)**	(0.002)**	(0.002)	(0.049)**	(0.061)**	(0.006)**	(0.002)**
Mother	-0.031	0.023	0.001	0.000	-0.040	-0.075	0.016	-0.009
	(0.009)**	(0.008)**	(0.003)	(0.003)	(0.061)	(0.076)	(0.007)**	(0.002)**
P-value	0.000	0.000	0.000	0.164	0.002	0.000	0.006	0.000
Observations	2629	2373	4529	4529	2159	2159	6794	6761
Left school at 14-15								
1930-1938								
Father	-0.031	-0.009	-0.013	0.011	-0.159	0.002	-0.015	-0.019
	(0.025)	(0.026)	(0.010)	(0.012)	(0.181)	(0.234)	(0.020)	(0.010))*
Mother	0.032	0.058	0.010	-0.012	-0.114	-0.281	0.012	-0.038
	(0.027)	(0.028)**	(0.011)	(0.013)	(0.197)	(0.254)	(0.022)	(0.011)**
P-value	0.333	0.109	0.405	0.518	0.453	0.507	0.714	0.000
Observations	2381	2158	4098	4098	1951	1951	6168	6139
1933-1935								
Father	0.078	0.020	-0.065	-0.021	-0.293	-0.770	0.094	-0.001
	(0.052)	(0.052)	(0.026)**	(0.025)	(0.390)	(0.459)*	(0.039)**	(0.021)
Mother	0.006	0.091	0.057	0.021	-0.701	-0.368	-0.033	-0.025
	(0.055)	(0.056)	(0.026)**	(0.026)	(0.419)*	(0.494)	(0.042)	(0.021)
P-value	0.257	0.156	0.025	0.635	0.08	0.08	0.061	0.447
Observations	386	338	667	667	315	315	998	996
1930-1938 except 1934								
Father	-0.005	0.013	0.001	0.016	-0.101	0.041	-0.032	-0.021
	(0.030)	(0.031)	(0.012)	(0.015)	(0.222)	(0.288)	(0.024)	(0.011)*
Mother	-0.020	0.069	0.004	-0.021	-0.017	-0.099	0.021	-0.050
	(0.033)	(0.034)**	(0.013)	(0.015)	(0.243)	(0.315)	(0.027)	(0.012)**
P-value	0.785	0.065	0.945	0.324	0.875	0.951	0.398	0.000
Observations	1816	1646	3129	3129	1492	1492	4708	4685

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level. For each interval, both the mother and the father are born within those years. The regressions are performed for those children with their natural parents. Extra controls include parental age.

Table 9: Parental education on parental variables –IV results

	Maternal smoking during pregnancy	Breastfeeding	Father Illness	Mother Illness	Body Mass Index Father	Body Mass Index Mother	Mother work	Financial difficulties
All education years								
1930-1938								
Father	-0.054	-0.080	-0.01 8	0.029	0.277	0.148	-0.041	-0.008
	(0.076)	(0.076)	(0.028)	(0.028)	(0.511)	(0.644)	(0.055)	(0.029)
Mother	0.055	0.049	0.030	-0.021	0.139	0.469	-0.020	-0.068
	(0.068)	(0.073)	(0.027)	(0.029)	(0.469)	(0.593)	(0.056)	(0.029)**
P-value	0.648	0.537	0.522	0.517	0.759	0.638	0.630	0.034
Observations	3459	3121	5966	5966	2849	2849	8947	8906
1933-1935								
Father	-0.033	0.000	-0.028	0.053	-0.173	-0.526	0.067	-0.016
	(0.082)	(0.084)	(0.038)	(0.037)	(0.617)	(0.692)	(0.071)	(0.032)
Mother	0.072	0.041	0.024	0.038	-1.211	-0.203	-0.097	0.000
	(0.145)	(0.160)	(0.078)	(0.080)	(1.555)	(1.744)	(0.154)	(0.063)
P-value	0.795	0.966	0.652	0.331	0.732	0.749	0.403	0.872
Observations	561	495	970	970	463	463	1449	1446
1930-1938 except 1934								
Father	0.095	-0.083	-0.052	0.039	0.576	-0.069	0.051	-0.017
	(0.142)	(0.129)	(0.049)	(0.049)	(0.996)	(1.209)	(0.106)	(0.049)
Mother	-0.042	0.181	0.053	-0.045	0.204	0.837	-0.182	-0.083
	(0.118)	(0.121)	(0.045)	(0.045)	(0.865)	(1.049)	(0.095)*	(0.044)*
P-value	0.785	0.278	0.482	0.607	0.533	0.543	0.095	0.018
Observations	2529	2373	4529	4529	2227	2227	6794	6761
Left school at 14-15								
1930-1938								
Father	0.051	-0.161	-0.023	0.041	0.476	0.489	0.055	-0.020
	(0.094)	(0.092)	(0.038)	(0.038)	(0.645)	(0.830)	(0.069)	(0.036)
Mother	0.113	0.036	0.042	0.001	0.188	0.263	-0.024	-0.052
	(0.071)	(0.070)	(0.027)	(0.028)	(0.466)	(0.600)	(0.052)	(0.027)*
P-value	0.188	0.216	0.279	0.551	0.655	0.703	0.688	0.111
Observations	2381	2158	4098	4098	1951	1951	6168	6139
1933-1935								
Father	-0.083	-0.145	-0.056	0.072	0.131	-0.805	0.147	-0.022
	(0.113)	(0.114)	(0.050)	(0.054)	(0.844)	(0.991)	(0.084)*	(0.041)
Mother	0.157	0.021	0.060	0.061	-0.432	-0.427	-0.024	-0.004
	(0.102)	(0.102)	(0.043)	(0.037)*	(0.769)	(0.902)	(0.079)	(0.038)
P-value	0.300	0.430	0.307	0.014	0.850	0.468	0.209	0.839
Observations	386	338	667	667	315	315	998	996
1930-1938 except 1934								
Father	0.150	-0.067	-0.045	0.019	0.650	0.203	0.135	-0.041
	(0.110)	(0.107)	(0.044)	(0.044)	(0.790)	(1.015)	(0.083)*	(0.042)
Mother	0.082	0.066	0.039	-0.014	0.794	0.785	-0.097	-0.078
	(0.090)	(0.087)	(0.035)	(0.034)	(0.609)	(0.782)	(0.067)	(0.033)**
P-value	0.181	0.664	0.403	0.866	0.225	0.552	0.136	0.022
Observations	1816	1646	3129	3129	1534	1534	4708	4685

Robust standard errors in parentheses; * significant at 10% level; ** significant at 5% level. For each interval, both the mother and the father are born within those years. The regressions are performed for those children with their natural parents. Extra controls include parental age.

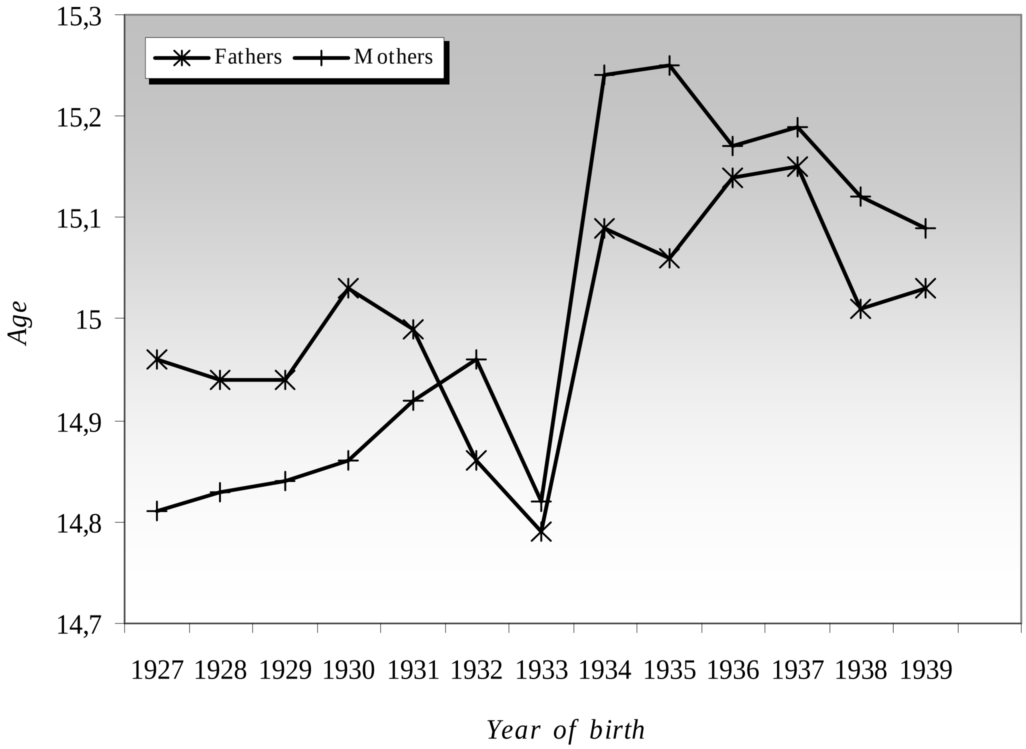

Figure 1: Mean age of finishing schooling by birth year

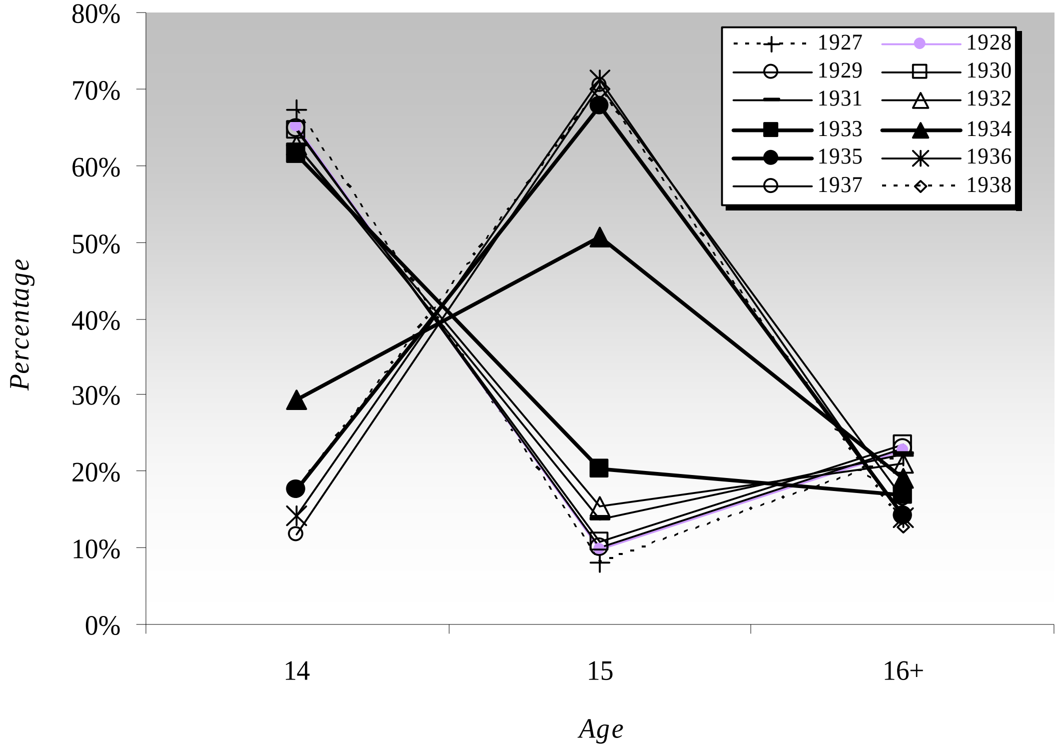

Figure 2: Age finishing school by year of birth (fathers)

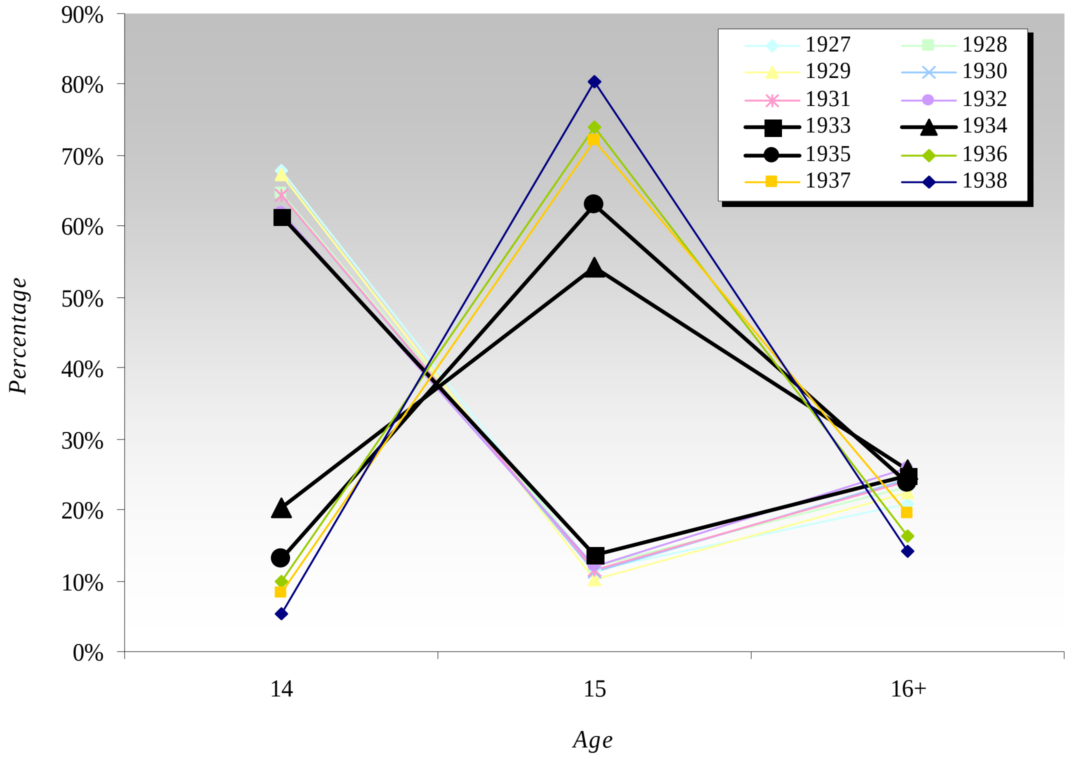

Figure 3: Age finishing school by year of birth (mothers)