Parental education and child's education: A natural experiment

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Abstract:

Is the intergeneration educational link due to nature or nurture? In order to separate the nature (genetic) and nurture (education) effects, researchers have relied on situations where two individuals are specifically affected by one or the other effects but not both; typically comparing twins' offspring or natural and adopted children. These studies estimate that maternal schooling has no effect on her offspring education.

In this paper, we propose an alternative strategy to identify the effect of parental education on their offspring schooling choices. Changes in the minimum school leaving age created a discontinuity in the education of parents due uniquely to their birth cohort. The effect of parental schooling is only identify for a group of parents with a distaste for education and may not reflect the social return that a policy increasing education for another group of parents may have. However, we reckon that this is the strength of this estimation strategy since the children of parents with a lower taste for schooling are likely to be the most at risk of not maximising their education potential. We find a strong positive effect of maternal education on their children schooling achievements.

Keyword: Education choice, intergenerational effect
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I Introduction

Parents and more generally the family environment have a strong influence on the behaviour and decisions taken by adolescents. There is a tradition for social scientists to study this intergenerational link from its effects on child development, health and various adult outcomes. Economists have mainly focused on the effect of parental background on income, social class or exit from poverty. Typically these studies have found a strong link between one's earnings and its father's, with the intergenerational correlation in earnings between father and son reaching between 0.40 and 0.50 in the US (Solon, 1999) and 0.60 in the UK (Dearden et al, 1997).

The mechanisms by which this intergenerational correlation in earnings is initiated are still subject to debate but education is a likely culprit. In most of the western world children brought up in less favourable conditions obtain less education, and tend to remain poor (Gregg and Machin,xx). The debate about the effect of parental background on educational choices is not new and resurfaces regularly, especially whenever free education is questioned. The arguments for the intergenerational education link usually focused on the liquidity constraints, as in Becker and Tomes (1986). This is the base of policies of financial support for the poorest, like the Education Maintenance Allowance in the UK where poorer pupils receive a weekly allowance conditional on staying in post compulsory schooling. However, Cameron and Heckman (1998) for the US or Chevalier and Lanot (2002) for the UK show that the effect of financial constraints on educational choice is less important than the effect of family background (mainly parental education). This would suggest that most cost-efficient interventions should be provided at an earlier stage of the child life[1].

The common view is that more educated parents provide a “better” environment helping their children to reach decisions that can be considered “better”. This assumption was the base of a World Bank programme on mother’s education with evidence that more educated mothers have healthier children[2]. In this paper, the focus is on one of the decisions taken by adolescents: staying in post compulsory schooling[3]. There is a wealth of evidence on the positive relationship between parental education, especially the mother’s, and offspring’s education[4]. The elasticity for intergenerational mobility in education ranges from 0.14 to 0.45 in the US (Mulligan, 1999) and 0.25 to 0.40 in the UK (Dearden et al., 1997). So it would seem that policies increasing education have a positive effect on the second generation, thus creating social returns to education. However, some recent studies have put a note of caution on these results.

Is the intergeneration educational link due to nature or nurture? Or to put in other terms, can the parental education be considered exogenous in a regression of intergenerational educational choice? Parents’ decision to invest in their own education was affected by their own observable and unobservable characteristics. For example, ability is positively associated with more schooling and ability is also partly genetically

[1] See Carneiro and Heckman (2003) for a review and comparison of various interventions targeting at closing the educational gap between rich and poor in the US.

[2] This relationship between mother’s education and children birth weight (a main predictor of child health) is found in the developing world (Behrman, 1997) but also in the US (Currie and Moretti, 2002).

[3]Choices made by teenagers have a long lasting effect on their labour market prospect or health. Since reducing the number of teenagers not investing in their education post compulsory schooling is one of the main targets of the UK government (Education Maintenance Allowance for example) this is the main outcome of interest for this paper but the effect of parental education on other outcomes such as smoking, teenage pregnancy and criminal activities could also be of interest.

[4] See Behrman (1997) for an extensive review of this literature, focusing mostly on the US and developing countries.

transmitted from parents to children[5], thus the link between parents' and offspring's schooling could be due to unobserved characteristics rather than a positive effect of education per se. In order to separate the nature and nurture (education) effects of parental education, researchers have relied on situations where two individuals are specifically affected by one or the other effect but not both.

A strategy to eliminate some of the nature effect is to compare the effect of parental education on siblings or between cousins. These strategies were implemented by Behrman and Wolfe (1984) and Rosenzweig and Wolpin (1994) respectively and do not allow to conclude on the relative effect of nature and nurture since the within family estimates are either below (Behrman and Wolfe) or above (Rosenzweig and Wolpin) the OLS estimates. [should I give more details about these papers?]

More directly, Behrman et Rosenzweig (2002) use pairs of twin parents in order to eliminate the nature effect of one of the parent (since twins have identical genome) and compare the educational choices of cousins. Sacerdote (2002) and Plug (2002) purge the nurture effect, relying on the difference between adopted and natural children. These studies report that controlling for ability and assortative mating (more educated women tend to marry more educated men), the positive effect of maternal education on children's education disappears[6].

Behrman and Rosenzweig (2002) find that assuming the exogeneity of parental education, mother's schooling increased her children's years of education by 13% while the effect of father's schooling was about twice as large. However, between twins

[5] Taking IQ as a measure of ability, the correlation in IQ between parents and natural children is 0.42 for children living with their parents and 0.22 for those brought apart (Feldman et al., 2000).

[6] Sacerdote uses the British National Child Development Survey while Berhman and Plug use idiosyncratic datasets: a register of Minnesota twins and a longitudinal survey of Wisconsin high-school leavers respectively.

estimate which eliminates the mother's unobservable characteristics (since monozygotic twins have identical genetic background) leads to a negative (but insignificant) effect of mother's education on her child attainment. This counter-intuitive result is consistent with a hypothesis that education alters the value of home time with more educated mothers switching from time intensive tasks to information intensive tasks, and the net effect on their children education appears to be negative. This study also contradicts the general view that mother's schooling has a larger effect than her husband's schooling on the achievement of their children. As well as the usual shortcomings of twin estimates (see Bound, 2000 for example), this identifying strategy only provides unbiased estimate of one parent (the one with a twin). Also Antonovics and Goldberger (2003 SOLE) demonstrate that the results do not hold after some minor recoding of the data.

Studies comparing adopted and natural children may also suffer from some bias, as they typically compare children in different families and therefore assume that adoptive and natural families provide identical environment or that adopted children are randomly allocated to families[7].

Finally, researchers have previously relied on sister in law schooling (Behrman and Taubman, 1985), grand parents schooling (Lillard and Willis, 1994) or local technological shocks (Behrman and al., 1999) to instrument mother's education. The first and third papers report IV estimates that are about twice as large as OLS while Lillard and Willis note, as expected, a reduction of the maternal education effect when it is instrumented. Doubts remain regarding the validity of the chosen instruments. In this

[7] This condition may not be sufficient to identify nature and nurture effects, since adopted and natural children may have different characteristics or treated differently in school or society (especially when of different race from their parents) or faced stigma to adoption. Additionally, adoptive family may provide a different environment to children (wealth, attention to child). As evidence of differences in the environment

paper, we propose an alternative strategy to identify the effect of parental education on their offspring schooling choices. In a perfect set-up, one would like to randomly allocate parental education to estimate its effects on children. This is obviously impossible, but the UK has provided a natural experiment that is fairly close in spirit to this set-up. Changes in the minimum school leaving age mean that the educational choice of parents was exogenously affected, at least for those whishing to leave school at the earliest age. Some parents would then have experienced an extra year of education than other parents similar to them on any other points but their birth year. This discontinuity can be exploited to identify the exogenous effect of parental education on their children's education. A similar strategy has recently been used by Black et al. (2003) using reform of the minimum school leaving age in Norway. The authors report that the effect of parental education on their children's educational achievement is greatly reduced, and with the exception of the mother-son relationship, become insignificant, when parental education is instrumented. Thus suggesting that parental education has no causal effect on children's education.

To summarise, this paper aims to determine whether a policy of increasing children's education would have some long-term benefit on the following generation. Findings of small/no direct effect of parental education on their children, as recently documented suggest little social returns to schooling. On the contrary, we find that estimates purged of the exogeneity of parental education are larger than those assuming exogeneity for mothers but that the paternal effects disappear. These results identify the effect of parental schooling for a group of parents with a distaste for education and may not reflect the social return that a policy increasing education for another group of parents

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of adopted and natural children, Maughan et al (1998) find that adoptees performed more positively than may have. However, we reckon that this is the strength of this estimation strategy since the children of parents with a lower taste for schooling are likely to be the most at risk of not maximising their education potential.

II Model of intergenerational spill-over

The conventional wisdoms are that parental education has a (1) positive effect on the education of their children and (2) the mother's education has a stronger effect than the father's. A simple model of the intergenerational education choice is presented in Behrman and Rosenzweig (2002). Following similar notations, we consider a linear reduced form equation describing the schooling choice (S_j^c) of the child in family j:

$$S_j^c = \beta_s^m S_j^m + \beta_E^m E_j^m + \beta_s^d S_j^d + \beta_E^d E_j^d + \varepsilon_j^c \qquad (1)$$

where subscripts c, m or d define a characteristic of the child, mother or father respectively. The schooling of the child is assumed to depend linearly on the schooling achievement of his parents (S) and their other characteristics (E). Due to intergenerational transmission of unobservable characteristics, we cannot assume that S_j^g is independent of ε_j^c where g stands for m or d. Behrman and Rosenzweig (2002) also assume that S_j^m is correlated with S_j^d and E_j^d due to assortative mating[8]. Thus, assuming the exogeneity of parental education leads to bias estimates of its effect on the child's education choice. The omitted variables are likely to be positively related with educational choices, so the estimates of parental education are biased upwards.

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non-adopted children from similar families on childhood tests of reading, mathematics, and general ability.

III Data

To carry out this research, data on parental education and teenagers' decisions are needed. We rely on a UK dataset that typically collects information on adults but also include some information on the children in the household. As with most surveys, children aged 16 to 18 living at home are interviewed in the Family Resources Survey (FRS), thus parental information can be matched to the child's record. To achieve a reasonable sample size, we pooled seven cross sections from the FRS (1994-2000), which leads to a sample of 14,614 individuals aged between 16-18 at the time of the interview. Only teenagers living with their parents[9] are selected which represents 94% of the population of interest. However, the selection becomes more severe with older teenagers, whilst 98% of 16 years old are observed living with their parents, this proportion in down to 88% for the 18 years old. The proportion of teenager not studying full time is 30% for those living with their parents but 75% for those living on their own. The proportion of teenagers living without parent is also disproportionately female (70%) and 14% are teenage mothers whilst this proportion for the full sample is 18‰.

Our strategy to identify the effect of parental education and their children's schooling rely on the following "natural experiment". Individuals born before September 1957 could leave school at 15, while those born after this date, had to stay for an extra year of schooling. Compliance to the change in school leaving age was high. As seen in Figure 1, this policy change creates a discontinuity in the years of

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[8] More educated women tend to marry more educated men who potentially have a higher endowment. Behrman and Rosenzweig (2002) estimate that " a women of given endowments who increases her schooling by one year would attract a mate with 0.4 more years of schooling" (p328).

[9] We define parents as natural, adoptive, step or foster parent. The dataset does not distinguish between natural and adoptive parents. See data annex for the definition of the sample.

education attained at the parental generation. Children affected by the new school leaving age have on average completed half a year more schooling than those born just after the reform. This change in achievement exactly coincides with the introduction of the new school leaving age; note the jump for children born in September 1957, thus is likely to be due to the reform rather than other policies.

[Figure 1: here]

This discontinuity is used as an identification strategy. However, as the change in compulsory schooling mostly affect the education decision of pupils who wanted to leave school at the first opportunity, we only identify a Local Average Treatment Effect (Imbens and Angrist, 1994). The instrument only identify parents at the lowest tail of the endowment distribution, some having different level of education because of the policy change. If we believe in a signalling model of education, children not directly affected by the reform may also increase their schooling in order to maintain their signal. In a signaling model, our instrument would identify an Average Treatment Effect. However, Chevalier et al. (2003) shows that the change in school leaving age, did not lead to a large change in the distribution of post-16 schooling.

For the parental generation faced with minimum school leaving age of 15, about 40% of parents left at the first opportunity. In our selected sample, it appears that the change of school leaving age led to a reduction in attainment at higher level of education, but this is only an artefact due to the sample selection. As more educated parents tend to have children at an older age, the sample of parents born after September 1957 with children aged 16 to 18 is disproportionally less educated.

Since fathers tend to be older than mothers, only 8.5% of fathers experienced the minimum school leaving age of 16 whist 18% of mothers have done so.

[Table 1: here]

The relation of interest in this paper is the intergenerational education choice. However, as we concentrate on children living with their parents, we cannot study completed schooling for the second generation. Instead, we focus on staying in post compulsory school leaving age[10].

[Table 2: here]

Parents are separated by compulsory school leaving age and schooling achievement. For both parents and school leaving age group, a positive relationship between parental education and the child's decision to remain in post-compulsory schooling exists. For example, whilst 68% of children with a father in the SLA 15 cohort, whose father left school at 15, have had some post compulsory schooling, this proportion is 96% if their father went to university. Mother's education appears to have a similar effect than father's on the decision to remain in education post compulsory school leaving age. For most level of education, children with parents who faced a school leaving age of 16 are less likely to be in compulsory education. The difference is the largest for children whose parents left school at 16. This reflects that parents leaving school at 16 will on average be of lower ability after the reform than before. Additionally, the reduction of the parental influence may be due to age (and therefore income) effects.

As seen in Table 3, children with older parents are more likely to remain in education than those with younger parents whatever the level of education at the parental generation is.

[Table 3: here]

IV results

As completed education is not observed for the second generation, the focus is on attending post compulsory education, which is observed for all children. The proportion of children attending post compulsory education were in 1998 73% and 66% for 16 years old females and males respectively (DfEE). In the selected sample and over the period 1994 to 2000, these probabilities are 81% and 73% respectively. As expected, focusing on children living with their parents leads to a sample of higher achievers. The discrepancy between the official staying on rate and our sample may also be due to mismeasurement, however, the gender gap in achievement is captured in the selected sample. The outcome of interest is a dichotomous variable hence we estimate a probit model, where the exogenous variables include dummies for the year and month the interview took place, region of residence and age and gender of the teenager. Initially, the population is further restricted to teenagers living with two parents. The identifying strategy to estimate the effect of parental education is based on the argument that the

[10] This dichotomous variable is defined as being currently in education or having left full time education

change in school leaving age identifies a local average treatment effect. However, due to possible signalling effect of education, we retain the full sample and not only teenagers whose parents left school at the minimum age.

First focusing on the effect of mother's education, measured as the age at which she left full time education, it is estimated that for the mean individual, increasing mother's schooling by one year, increases the probability of her children staying past compulsory education by 6.6 percentage points, from a base of 78%. However, this coefficient on the effect of mother's education on her children's education is biased upwards due to assortative mating. Introducing the father's education, as an exogenous variable, has the expected effect of reducing the mother's effect by nearly 50% to 4.3%. Additionally, the mother's and father's effects on their children's education are not significantly different.

As parental education is correlated with family income, teenagers in less educated household may be more likely to be financially constrained. Measures of dad's income are also added; these include dad weekly log pay, and dummies for missing pay, dad not working and dad self-employed. Despite being significant, the inclusion of dad's income has no significant effect on the parental education coefficients.

[Table 4: here]

In these three models, mother's education was treated as an exogenous variable, this assumption is now relaxed. Mother's education is instrumented by the compulsory school leaving age she faced as a teenager and in order to take care of a possible trend in

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after the age of 16.

educational attainment or other cohort specific shocks, the year of birth of the mother, as well as its interaction with the school leaving age dummy are added. In all specification, we can reject that mother's education is exogenous, and a test of joint significance of the instruments in the first steep validates the instruments. In the simplest specification, the marginal effect of the mother's schooling effect, estimated at the mean of the sample, increases three folds; increasing mother's education by one year, would increase the probability of her child attaining post-compulsory education by 18.3 percentage points. Rather than a reduction of the mother's effect, that would have been expected by removing the endogeneity of this variable, a large rise is observed. This result is an artefact of our identification strategy. The effect of mother's education is identify for mother's whose education decision was affected by the change in the school leaving age, i.e. women with a lower taste for education. Hence, our estimate is only valid for this population and can be seen as a local average treatment effect (LATE). Rather than being informative for the whole population, our estimates only identify the effect of mother's education for women with the lowest taste for education, hence it is not surprising that for this specific population, increasing the mother's education will have a larger impact on the decision of their children than for the whole population. However, since the children of mother's with the lowest taste for education are also the one most likely to be at risk of leaving school at the first opportunity, this estimate is of interest to assess the effect of a policy increasing school leaving age on the second generation.

When father's education and earnings are included in the IV model, both have a negative effect (significant for father's education) and the effect of mother's education reaches 32 percentage points. These results are almost the opposite of

Behrman and Rosenzweig (2002) results of negative effect of mother's education and positive effect of father's education.

In the second panel of Table 4, father's education is instrumented by the same set of instruments. The results are similar to those obtained when instrumenting mother's education. The IV estimates are 4 to 10 times as large as the marginal effects estimated by the probit models. Whilst in the probit models, the effect of father's education ranges from 3.9 to 5.7 percentage points, the IV estimates range from 21.4 to 59.7 percentage points. None of these models pass the test of over-identification (Hansen J test, to account for heteroskedasticity in the errors); hence, the results may be biased. Furthermore, since both parents' education can be considered endogenous, both should be instrumented. Results from such a specification are presented in the last panel of Table 4.

The model is estimated by GMM assuming a linear model rather than a probit model[11]. In contradiction with recent evidence, we find that mother's education has a large effect on the education decision of her child while the dad's effect is nil. The negative but insignificant effect of father's education on the schooling of his child is consistent with a model where more educated fathers have a higher value of their time in the labour market and spend less time with their family. This simple test is however not supported in the selected sample, where no relationship between father's education and father's hours of work can be found. The large positive effect of mother's education is conformed to the belief that mother's spend more time with their children than father's. This positive effect of mother's education solely due to

[11] In the simpler case when only one of the parental education is considered endogenous, the linear model leads to similar estimates of the marginal effects of parental education than the probit model.

nature, and thus policies increasing the education of one generation have positive social returns, at least for women.

The previous estimates can be considered as the total effect of parental education, including direct but also indirect impact, since increasing parental education would also affect other determinants of the child's decision such as parental income or labour force participation. Including father's income and a dummy for mother's labour force participation would lead to an estimate of the direct effect of parental education if these variables could be considered exogenous. As expected, the estimates on parental education are reduced (in absolute terms) but the previous conclusions remain unchanged. Increasing the mother's education by one year, increases the probability of her child attending post-compulsory schooling by 20 percentage points. Having a working mother reduces this probability by 5 percentage points and counter-intuitively, after accounting for dad's education, the paternal income has a negative effect on the decision to remain in school post-compulsory age. As paternal income and mother's participation to the labour market are endogenous to education, our favoured model remains the base one.

To check the robustness of these results, we also estimate the simpler model separately for the three age groups in our data (Table 5). The concern is mostly on the 16 years old group, where possibly measurement error affects our recording of the post-compulsory decision. Assuming the exogeneity of parental education, the estimates obtained at age 16 are significantly reduced compared to those for children aged 17 and 18, which is consistent with a possible measurement error bias for the younger cohort. When instrumenting parental education, the estimates at age 16 are also different from those obtained for the other two age groups. While, as for the older pupils, father's education is not significantly different from 0, at age 16, it would appear that mother's education effect on her child's education decision would also be insignificant. The coefficient in less than half of the one estimated for the older pupils. The results for the 17 and 18 years old are rather similar with an extra year of mother's education increasing the probability of attending post-compulsory schooling by at least 32% percentage points whilst the father's education has no significant effect.

[Table 5: here]

As parental effect may be gender specific, the preferred model is run separately for sons and daughters. The assumption is that father's education has more impact on their sons and mother's on their daughters. Note that such a gender separation in the effect of parental education is not compatible with a pure genetic model. In the probit models, there is no evidence that mother's schooling has a stronger impact on daughters than on sons, nor that father's schooling has a larger effect on the decision of sons (Table 6). However, when instrumenting parental education, these results are dramatically changed. For girls, parental education has no significant effect on the decision to remain in school post compulsory age. For boys, a strong and significant effect of mother's education is estimated while the father's effect is negative but insignificant. Contrary to a priori beliefs, it appears that mother's education impacts more on her son's decision than on her daughter's. This could be because male's participation to post compulsory education is lower than female's.

[Table 6: here]

V Further results

In this section, various robustness tests are conducted. The first test is concerned with trend effects and therefore the validity of the instrument, while the second series deals with some of the simplifying assumptions that are implied in the base model.

Education achievement has been increasing at the parental generation thus it would be possible that the change in school leaving age has no identifying power and that only the trend matters. To test that our results are not driven solely by the positive trend in parental education, we reduce the sample to a sample of parents born around the reform and drop the trend. Our instrument for parental education becomes solely whether or not affected by change in school leaving age. Results with windows of 5 and 2 years around the reform are reported in Table 7. Compared to the full population, parents born around the reform are younger; for example, with the 5 years window, 2/3rd of fathers are born before the left bound of the window but only 1% are trimmed by the upper bound. In the exogenous case, a year of parental education increases the likelihood of attending post compulsory education by 1 to 2 percentage points above the full sample. When the assumption of exogeneity of the parental education is relaxed, estimates for the two windows are contrasted. Using a 5 years window, the estimated effect of mother's education is similar as the one obtained with the full sample, while father's effect is this time positive but still insignificant. With a smaller window, estimates get extremely imprecise and are not significantly different from zero. However, the first stage reveals that change in school leaving age is a valid instrument for parental education. All in all, it appears that the results obtained with the full sample are not solely due to a trend in parental education and that our identifying strategy is valid.

[Table 7: around here]

Due to asortative mating, Behrman and Rosenzweig (2002) highlight that education affects the choice of partners (xxx more details). This point has so far been neglected but as a test of the bias involved we compare our results for children leaving with both parents with those leaving with single parents. In the exogenous model, single parent's education has twice as much effect on the child's probability of attending some post-compulsory education than for married parents. This is consistent with a model where single parents spend proportionally more time with their children. However, when the endogeneity of parental education is acknowledged, the estimate of the effect of maternal education reaches 24 percentage points, similar to the estimates obtained for married mothers. Paternal education effect is not different to the one estimated in the exogenous model. These results broadly confirm those obtained for the population of children living with both parents; maternal education has a large positive effect on the decision to attend post-compulsory schooling whilst paternal education has no significant effect. For mother's education, the estimates are almost identical which suggests that asortative mating bias is not an issue.

[Table 8: here]

As in Plug (2002) or Sacerdote (2002) we wish to separate between natural and other children, in order to eliminate the nurture component in the intergenerational correlation of educational choice but the data only records the following three status on the relationship between child and parents: (1) natural or adoptee, (2) step child, (3) foster child. Only 795 (resp. 268) fathers (mothers) were non-natural, leading to a rather small sample of children leaving with a least one non-natural parent, mostly a step-father. To reduce the selection bias, we drop the 36 children living with foster parents. In the exogenous model, children with step-parents benefit more from the schooling of their parents, this is especially the case for mother's education. This surprising result would be consistent with a model where the remaining natural parent spends more time with their children than never-divorced parents, maybe to compensate for the lower attention of the step-parent. However, when instrumenting parental education, results for children living with both natural parents and other children are not significantly different. For mother the point estimates are both around 20 percentage points whilst for father, the estimate in the non-natural sample is positive but insignificant. We cannot conclude that the parental education effects were significant smaller for non-natural children compared to natural ones which is consistent with our identification strategy based on an exogenous change in parental education.

[Table 9 here]

The estimated effects of maternal education may appear rather large at first. For example the education maintenance allowance experiment has increased participation of 16 to 18 years old by 8 percentage points in the treated areas [12](DfES, 2002). Sixty-three percents of the pupils receiving support registered in short vocational course rather than follow the academic track, and 10% dropped out by the end of the first year. Assuming that all vocational courses last two years and that half of the students on the academic track eventually graduate from university (5 years) whilst the remaining stop after A-levels (2 years), we compute the effect of increasing parental education on the years of education completed. In such a scenario, assuming that individuals not directly affected by the change in parental education do not change their education decision[13], the 25 percentage points increase in post-compulsory education will be equivalent to an average increase of 0.6 years of education for the whole population. Berhman and Rosenzweig (2002) estimate that one year of paternal education increases on average education attained by the child by 0.34 to 0.56. In this light, our estimates of the effect of parental education on child schooling achievement are in line with the rest of the literature.

VI Conclusions

As in other studies, we initially find that parental education has a significant effect on the decision to stay in school after 16; increasing parental education by one year, increases the probability of staying on by 4 to 6 percentage points. To eliminate the endogeneity of the parental education variables, we use changes in compulsory school

[12] EMA is an experiment currently conducted in England where children aged 16 to 18 receive a means-tested financial support of up to £40/week if staying in post-compulsory education.

[13] In a signaling model, individuals who originally wanted to leave school at age t, may decide to remain in school longer if the schooling attained by individuals at lower end of the schooling distribution is increased. Chevalier et al. (2003) show that these ripples are limited.

leaving age as an instrument. This identification strategy estimates a local average treatment effect, since only parents who whished to leave school at 15, and therefore have either a lower taste for education, lower ability or were financially constrained, are affected by the instrument. The IV estimates is therefore not directly comparable to the logit estimates initially reported. Instrumenting both parental education, leads to estimates of mother's effect on the decision to remain in post-compulsory education reaching at least 20 percentage points while father's education has no significant effect. These estimates are consistent for different age group and children brought up by lone parents. The maternal effect on educational choice may seem large, but it should be reminded here that these estimates represent the effect of increasing the parental education for parents with "a lower taste for education". Previous studies have shown that the private returns to education for these parents were also substantial (Harmon and Walker, 1995).

These results are of interest, since the children that benefited the most from the change in compulsory school-leaving age at the parental generation are those that were more at risk of leaving school at the earliest opportunity. Increasing education has positive effects at the next generation. These long-term effects should be taken into account when estimates of the social rate of returns to education are formulated. These effects are rather large and could be compared to those obtained from the EMA experiment.

Additionally, we do find evidence that the effect of parental education is gender specific. Using our favoured model, parental education has no significant effect on the education decision of daughters whilst mother's education has a large positive effect on her son's decision.

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Appendix 1: Sample selection in FRS (1994-2000)

14,614	age 16-18
13,741	Living with at least one parent
13,516	Parents older than 15 when kid born or less than 55

Table 1: Parental schooling distribution by compulsory school leaving age

	Father		Mother	
Age left school	SLA15	SLA16	SLA15	SLA16
15	43.15		38.61	
16	25.16	83.86	27.48	81.75
17	7.15	5.27	10.00	8.42
18	6.85	5.83	8.38	6.33
19-21	8.11	3.59	10.33	2.29
22-25	9.57	1.45	5.21	0.71
Obs	9590	892	10396	2268

Table 2: Intergenerational schooling choices

	Proportion in post-compulsory schooling Father		Proportion in post-compulsory schooling Mother	
Age parent left school	SLA 15	SLA 16	SLA15	SLA16
15	67.69		66.72	
16	79.98	66.04	78.44	66.94
17	87.17	87.23	86.63	81.15
18	90.41	86.54	92.65	88.39
19-21	96.01	87.5	97.11	92.31
22-25	98.03	92.31	97.59	100
Observations	9590	892	10396	2268

Table 3: Parental cohort and child's education

Parental cohort	% child in education			
	Dad	Observation	Mother	Observation
Born 48-52	80.19	3584	80.62	3988
Born 53-57	74.66	2514	76.56	4173
Born 58-62	69.51	869	69.97	2128
Born 63-67	71.43	105	67.71	288
Total	76.78	7072	76.52	10577

Table 4: Parent's education and child's probability of post compulsory schooling
Children living with both parents

	Instrumenting mother's education					
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.066	0.183	0.043	0.326	0.041	0.321
	(0.003)	(0.015)	(0.003)	(0.033)	(0.003)	(0.037)
Father's schooling			0.038	-0.100	0.034	-0.097
			(0.003)	(0.016)	(0.003)	(0.017)
Father's income[A]					0.077	-0.015
					(0.009)	(0.015)
Excluded Instrument (F test)[B]		94.6		19.6		14.88
Endogeneity Test[C]		84.6		64.2		49.86
Hansen J: (χ^2)[D]		2.34		3.35		5.50

	Instrumenting Father's education					Instrumenting parents' education[1]		
	Probit	IV	IV	Probit	IV	IV	Probit	IV
Mother's schooling			-0.363	0.042	-0.382	0.245	0.040	0.202
			(0.110)	(0.003)	(0.119)	(0.069)	(0.003)	(0.060)
Father's schooling	0.057	0.214	0.576	0.039	0.597	-0.048	0.034	0.001
	(0.003)	(0.027)	(0.156)	(0.003)	(0.167)	(0.051)	(0.003)	(0.048)
Father's income[A]							0.076	-0.093
							(0.009)	(0.038)
Mother work				0.021	0.122		0.014	-0.053
				(0.009)	(0.038)		(0.009)	(0.027)
Excluded Instrument (F test)[B]		22.48	3.22		2.99	Dad: 39.2 Mum: 31.6		Dad: 21.3 Mum 19.8
Exogeneity Test [C]		74.28	63.34		75.91	110.6		94.5
Hansen J: (χ^2)[D]		0.54	0.93		0.57	5.32		6.48

Note: The model is estimated by for individuals living with both parents (9949 observations). The first step is a linear regression, while the second step is estimated by probit. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA

[A]: Dad weekly log pay. Also include dummy for missing pay, dad self employed (pay not reported) and dad not working.

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage instrument regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that each of the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

[1]: A linear model is estimated by GMM. In the cases where a single variable was instrumented, this model lead to similar results as those presented.

Table 5: Parent's education and child's probability of post compulsory schooling Children living with both parents, by age of child

	Age 16		Age17		Age 18	
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.027	0.115	0.049	0.319	0.060	0.383
	(0.004)	(0.090)	(0.005)	(0.140)	(0.007)	(0.128)
Father's schooling	0.028	0.002	0.051	-0.099	0.037	-0.046
	(0.004)	(0.074)	(0.005)	(0.118)	(0.006)	(0.071)
Excluded Instrument (F test)[B]		Dad: 16.1 Mum: 13.5		Dad: 16.2 Mum:15.9		Dad:7.80 Mum: 3.86
Endogeneity Test[C]		23.6		42.9		23.2
Hansen J: $(\chi^2)^D$		3.28		3.55		4.74
Observations	3658	3658	3425	3425	2866	2866

Note: The model is estimated for individuals living with both parents by GMM imposing linearity of the model. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage instrument regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that each of the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Table 6: Parent's education and child's probability of post compulsory schooling Children living with both parents, by gender

	Women		Men	
	Probit	IV	Probit	IV
Mother's schooling	0.039	0.116	0.046	0.290
	(0.004)	(0.069)	(0.005)	(0.106)
Father's schooling	0.030	0.047	0.047	-0.096
	(0.003)	(0.050)	(0.004)	(0.082)
Excluded Instrument		Dad: 17.0		Dad: 22.75
(F test)[B]		Mum: 13.8		Mum: 18.66
Endogeneity Test[C]		59.8		48.0
Hansen J: $(\chi^2)^D$		5.13		8.92
Observations	4828	4828	5121	5121

Note: The model is estimated for individuals living with both parents by GMM imposing linearity of the model. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage instrument regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that each of the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Table 7: Parent's education and child's probability of post compulsory schooling Children living with both parents – Size of the window around reform

Window: At least one parent born 5, 2 years before or after SLA	5 years around the reform		2 years around the reform	
	Probit	IV	Probit	IV
Mother's schooling	0.061	0.285	0.070	0.071
	(0.008)	(0.099)	(0.018)	(0.079)
Father's schooling	0.051	0.081	0.056	0.014
	(0.007)	(0.069)	(0.017)	(0.109)
Excluded Instrument		Dad: 12.5		Dad: 4.68
(F test)[B]		Mum: 7.8		Mum: 11.57
Endogeneity Test[C]		27.8		0.428
Hansen J: $(\chi^2)^D$		N.A.		N.A
Observations	2884		1568	

Note: The model is estimated for individuals living with both parents by GMM imposing linearity of the model. The instrument includes a dummy for minimum school leaving age.

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage instrument regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that each of the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Table 8: Parent's education and child's probability of post compulsory schooling: Single parents

	Single mother		Single father	
	Probit	IV	Probit	IV
Mother's schooling	0.082 (0.006)	0.243 (0.056)		
Father's schooling			0.071 (0.014)	0.078 (0.140)
Excluded Instrument (F test)[B]		9.84		0.65
Endogeneity Test[C]		28.19		0.001
Hansen J: (χ^2)[D]		2.35		0.05
Observations	2715	2715	533	533

Note: The model is estimated for individuals living with one parent only by GMM imposing linearity of the model. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from the first-stage regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Table 9: Parent's education and child's probability of post compulsory schooling: Natural and step-children

	Natural		Step-parents	
	Probit	IV	Probit	IV
Mother's schooling	0.041	0.213	0.067	0.144
	(0.003)	(0.091)	(0.014)	(0.083)
Father's schooling	0.038	-0.052	0.046	0.099
	(0.003)	(0.070)	(0.011)	(0.059)
Excluded Instrument		Dad: 37.3		Dad: 2.72
(F test)[B]		Mum: 31.2		Mum: 2.19
Endogeneity Test[C]		67.82		7.85
Hansen J: $(\chi^2)^D$		11.23		4.58
Observations	9044	9044	869	869

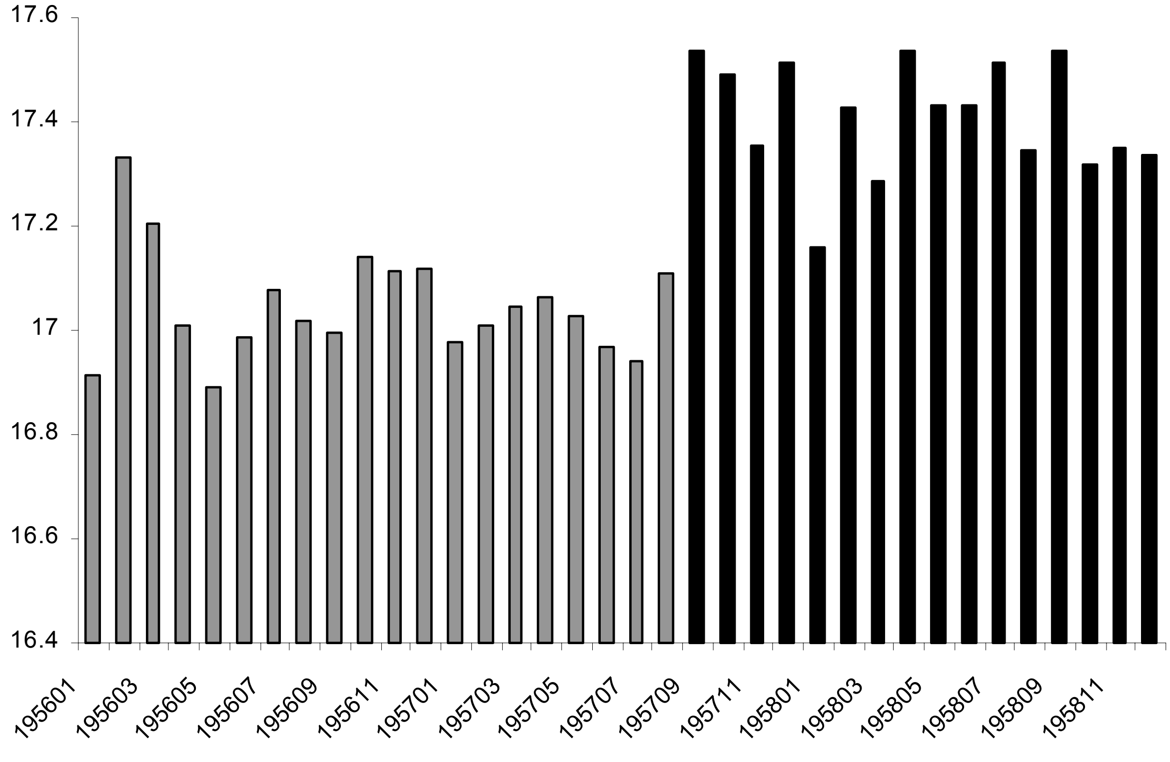
Note: The model is estimated for individuals living with both parents by GMM imposing linearity of the model. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA. For 637 cases, the step parent is a step father. Results based on this subpopulation are not different of those presented for all step parents.

[B]: Test of the joint significance of the instrument in a first stage regression

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage instrument regression are included in a probit model. Estimation of the model gives rise to a test for the joint hypothesis that each of the coefficients on the residual series are zero.

[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Figure 1: Years of schooling by birth cohort: Jan 1956- Dec 1958



Note: Source: LFS 1993-2001