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ABSTRACT

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Is the intergenerational educational link due to nature or nurture? In order to answer this dilemma, this paper identifies the effect of parental education on their offspring's schooling attainment using a discontinuity in the parental educational attainment. The discontinuity stems from changes in the minimum school leaving age legislation which took place in the Seventies in Britain. This strategy identifies the effect of parental schooling only for parents with a lower taste for education and may not reflect the general social returns of parental education. However, since policies are more likely to target children at risk of not maximising their educational potential, the estimates are of interest.
Contrary to recent evidence, we find a positive effect of both parents education on their children's schooling achievements when focusing on natural parents only. Step parents have no or a negative impact on children's education. In most cases, the endogeneity of parental education is rejected. These estimates suggest substantial social returns to education for same-sex parent. The estimates are robust to the introduction of additional controls for income, labour force participation, fertility and neighbourhood quality, indicating that the effect of parental education is direct.

JEL Classification: I20, J62

Keywords: educational choice, intergenerational effect

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Parental education and child's education: A natural experiment

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I Introduction

Parents and the family environment influence the behaviour and decisions taken by adolescents. There is a tradition for social scientists to study this intergenerational link and 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 the earnings of the father and his offspring. For example, the intergenerational correlation in earnings between father and son reaches between 0.40 and 0.50 in the US (Solon, 1999) and 0.60 in the UK (Dearden et al, 1997).

The mechanism of this intergenerational correlation in earnings is still subject to debate, but education is a likely culprit. 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). The common view is that more educated parents provide an environment, which improves their children's opportunities and decision processes. This assumption was, for example, the base of World Bank programmes to improve female education with evidence that more educated mothers have healthier children[1]. There is also a wealth of evidence on the positive relationship between parental education, especially mother's education, and offspring's education[2]. Policies increasing education appear to have a positive effect on the second generation.

Whilst intergenerational correlation is education has been largely documented, the current debate is on the causality of this link. This knowledge has important

[1] 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, 2003).

consequences for designing policies reducing educational inequality. This is an important issue, especially in Britain where the recent governments have targeted breaking the cycle of poverty between generations and reducing the proportion of children leaving school at 16. Interventions on the parental generation will generate social returns on the second generation only if the intergenerational educational link is causal, due to nurture, rather than just reflecting a nature (selection) effect. Finding small or no direct effect of parental education on their children would advocate policies targeted directly at the second generation children.

The next section reviews the literature. The identification strategy, which relies on a change in the school leaving age legislation at the parental generation to estimate the causal effect of parental education, is presented in the third section. The British data used for the analysis are presented in section IV, as well as some preliminary analysis of the intergenerational link in education. Section V reports the results of the base models as well as tests of the validity of the identifying strategy. The robustness of the results is assessed in section VI. This is followed by concluding remarks.

To summarise, when focusing on natural children, the intergenerational link in education is causal. The social returns to education on the second generation double when assuming the endogeneity of parental education. However, 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 may have. Nevertheless, 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. For natural children, intergenerational link is only

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

found with the same-sex parent. Non natural parents have no or a detrimental effect on the educational attainment of children, and severely reduce the estimates when included in the sample. We provide some evidence that the parental effect is direct and not due to an income effect, reduced fertility or neighbourhood characteristics.

II Previous literature

The correlation in education between generations has three possible channels: liquidity constraint, causal or nature. As private returns to education are large, less educated parents are more likely to face liquidity constraint preventing their children from fulfilling their schooling potential (Becker and Tomes, 1986). This is the basis 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[3]. 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). Thus, the most cost-efficient interventions are those provided at an earlier stage of the child's life or even at the parental generation[4].

Parents' decision to invest in their own education was affected by their own observable and unobservable characteristics. Some of these characteristics may be correlated with parenting skills whist others are genetically transmitted from parents to

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countries.

[3] See DfES (2002) et al. (2003) for an evaluation of the EMA experiment.

[4] 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. Blanden and Gregg (2004) also provide a review, including UK evidence, on family income effects through out the life of the child.

children[5], thus generating a correlation between parents' and offspring's schooling. In order to identify the causal effect of parental education, researchers have relied on three identifying strategies: twin parents, adopted children or instrumental variables.

As a direct way to control for unobservable genetic effects, Behrman and Rosenzweig (2002) use pairs of twin parents and compare the educational choices of their respective children. Assuming the exogeneity of parental education, each year of maternal schooling increases her children's years of education by 0.13 years while the effect of paternal schooling is about twice as large. However, between cousins estimates, which eliminates the mother's unobservable characteristics (since monozygotic twins have identical genetic background), lead to a negative (but insignificant) effect of mother's education on her child's educational attainment. This counter-intuitive result implies that more educated mothers switch from time-intensive tasks to information-intensive tasks, the net effect on their children's education being negative. This study 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 and Solon, 1999 for example), this identifying strategy only provides unbiased estimate for one parent (the one with a twin) even after accounting for assortative mating. Furthermore, Antonovics and Goldberger (2003) demonstrate that the results are sensitive to the selection of children aged 18 and above rather than 10, and with completed schooling.

Sacerdote (2002) and Plug (2004) compare adopted and natural children to estimate the causal effect of parental education. These studies report that mother's education had an insignificant effect on the educational attainment of adopted children, whilst the

[5] For example, the intergenerational correlation in IQ is 0.42 for children living with their parents and 0.22

paternal effect remains significant (0.20 year for each year of paternal education) even after accounting for family income[6]. Comparing natural and adopted children allows to net-out the genetic effect. Estimates based on adopted children may be biased, as they typically compare children in different families and therefore assume that adoptive and natural families provide an identical environment. Furthermore, they assume that adopted children are randomly allocated to families, and that children are randomly given for adoption[7]. Bjorklund et al. (2004), using a register of Swedish adoptees, test that the various biases due to the non-randomness of adoptions do not affect the conclusions and state that half of the parental schooling effects disappears when controlling for the genetic background. In their simpler model, each year of adoptive maternal education adds 0.05 years of schooling to the child or increases the likelihood of attending university by 6 percentage points. Paternal effects are about 40% higher.

These identifying strategies do not eliminate the non-genetic endogeneity that stems from unobservable characteristics in educational choice that are also correlated with parenting skills. In this paper, an alternative strategy to identify the effect of parental education on their offspring's schooling choices is used. One would like to randomly allocate parental education to estimate its effects on children. This is obviously impossible, but natural experiments that are fairly close to this set-up exist. Changes in the minimum school leaving age (SLA) mean that the educational choice of parents was exogenously affected, at least for those wishing to leave school at the first opportunity.

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for those brought apart (Feldman et al., 2000).

[6] Sacerdote uses the British National Child Development Survey while Plug uses a longitudinal survey of Wisconsin high-school leavers respectively.

[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

Some parents experienced an extra year of education compared to parents born just before the reform. This discontinuity can be exploited to identify the exogenous effect of parental education on their children's education.

Black et al. (2003) rely on a change in SLA in Norway during the Sixties. They report that the effect of parental education on children's educational achievement is greatly reduced and, with the exception of the mother-son relationship (0.17), become insignificant when parental education is assumed endogenous (this assumption is not tested). The rather small effects of parental education in Norway may also be specific to the rather egalitarian and homogeneous society characteristic of Nordic countries (see for example Bjorklund et al, 2002). Oreopoulos et al. (2003) relies on variation in SLA across states and time in the US, using aggregate data from the Census. Rather than the upward biased expected from omitted variable bias, the parental effect on grade repetition for children aged 7 to 15 doubles when instrumented, which is consistent with a local average treatment effect interpretation. However, their estimates may be biased due to measurement error in the independent variable affecting about 20% of the observations (Hausman, 2001) and aggregation of the data (Hanushek et al., 1996). None of these papers account for assortative mating as they estimate the effect of each parent separately[8] biasing the estimates upwards. Additionally, they do not control whether the child lives with her natural parents which impacts on parental altruism (Case et al., 2000).

This paper aims to determine whether the effect of parental education on their children's schooling attainment is causal by using a change in SLA in Britain which took

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of adopted and natural children, Maughan et al (1998) find that adoptees performed more positively than non-adopted children from similar families on childhood tests of reading, mathematics, and general ability.

[8] Oreopoulos et al.(2003) provide a table with estimates for both parents jointly. Whilst they report point estimates that are similar to those obtained in separate equations, it is unclear whether this rule out that their estimates are biased upward due to assortative mating, since they are obtained with aggregate data.

place in the Seventies. The variation in the timing of the policy is limited to a discrepancy between England and Wales, and Scotland where the policy was introduced three years later. To eliminate potential trend effects, our favoured model includes quadratic in birth cohort for both parents, and uses as instrument not only the reform but also interactions between the reform dummy on these quadratic functions.

III Model of intergenerational spill-over

The conventional wisdom is that parental education has a positive effect on the education of their children. Here we consider a linear reduced form equation describing the schooling choice (S_j^c) of the child in family *j*:

$$S_j^c = \beta_S^m S_j^m + \beta_S^d S_j^d + X_j + \varepsilon_j \qquad (1)$$

where subscripts *c*, *m* or *d* define respectively the schooling of the child, mother or father in family *j*. The schooling of the child is assumed to depend linearly on the schooling achievement of her parents (*S*) and some other characteristics of the family (X_j). Since many of the characteristics of the child's environment are correlated with parental education, we adopt, as in the rest of the literature, a parsimonious model, and in the base model, include only the gender and age of the child, region of residence and, for each parent, a quadratic in birth year. Additionally, we control for calendar effects and trends in reported education, by controlling for the month and the year the survey was conducted. In subsequent models, we include measures of parental income and labour force participation, and number of dependent siblings and neighbourhood quality to assess whether the effect of parental education is direct or due to a change in the above mentioned characteristics.

Assuming that S_j^g (where the subscript *g* stands for *m* or *d*) is independent of ε_j leads to biased estimates of the effect of parental education on the child's educational choice[9]. The omitted variables are likely to be positively correlated with educational choices, thus the estimates of parental education would be biased upwards. To instrument parental education, we rely on their date of birth to determine whether they were affected by the reform of school leaving age (*RoSLA*). For each parent we estimate the following equation.

$$S_j^g = \gamma_R^g RoSLA_j^g + \gamma X_j + \upsilon_j^g \qquad (2)$$

To account for possible unobservable characteristics explaining the choice of both parents, the two equations are estimated simultaneously. Since parents are typically not from the same cohort, there is some variation in the instrument, thus this identifying strategy accounts for assortative mating. The predicted educational attainment of each parent, rather than the observed one, is then used to estimate (1). Standard errors are obtained by a 500-replication bootstrap of the two-step procedure.

This identifying strategy may lead to biased results due to time trends in educational attainment. In a second model, we include *RoSLA* and interactions between *RoSLA* and the quadratic function in the cohort of birth as instruments for each parent's education. Thus, we no longer assume that the effect of the change in SLA was homogenous for all post-reform cohorts. Additionally, the change in compulsory schooling mostly affected the educational decision of pupils who wanted to leave school at the first opportunity. In a signalling model of education, children not directly affected

[9] Behrman and Rosenzweig (2002) also assume that S_j^m is correlated with S_j^d due to assortative mating. More educated women tend to marry more educated men who potentially have a higher earning potential.

by the reform may also increase their schooling in order to maintain their signal but Chevalier et al. (2003) show that the change in the school leaving age did not lead to a large change in the distribution of post-16 schooling, rejecting the signalling model. Thus, the estimates can be interpreted as a Local Average Treatment Effect (Imbens and Angrist, 1994). The population identifying the LATE is also the one likely to be targeted by policy interventions, hence the estimates are of interest

The estimates on the effect of parental education may be biased upward since all children have to remain in school up to the age of 16. However, Oreopoulos et al. (2003) show that this bias may not be substantial, first by using cross-state variations in the SLA legislation between generation and secondly, since the estimates of parental education on lower grade repetition and high-school drop out are not statistically different from each others.

IV Data

As most other surveys, the British Family Resources Survey (FRS) surveys children aged 16 to 18 living at home, thus parental information can be matched to the child's record[10]. To achieve a reasonable sample size, nine cross-sections from the FRS (1994-2002) are pooled leading to a sample of 18,715 individuals aged between 16 and 18 at the time of the interview. Only teenagers living in the parental

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They 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).

[10] The FRS is a yearly cross section survey of household living in Great Britain sponsored by the Department of Work and Pensions. The aim is to monitor social security programmes and forecast benefit expenditures. The yearly sample size is in the region of 40,000 individuals.

nest can be matched[11], which represents 94% of the population of interest. The proportion of teenagers not studying full-time is 25% for those living with their parents but 70% for those living on their own. The proportion of teenagers living outside the parental nest is also disproportionately female (70%). The selection becomes more severe with older teenagers; whilst 98% of 16 years old are observed living with at least one of their parents, this proportion is down to 88% for the 18 years old. Thus, we test the sensitivity of our results by running regressions separately for each age cohort. Additionally, the analysis is conducted for two subgroups: children living with two parental figures and children living with both natural parents. The two groups are virtually undistinguishable, so focusing on children living with both natural parents does not lead to further selection bias (see Appendix 1).

The relation of interest in this paper is the intergenerational educational choice. As we concentrate on children living with their parents, completed schooling is not observed for the second generation. Instead, the focus is on staying in education beyond the compulsory school-leaving age[12]. This is a statistic of interest in the UK, where a high proportion of early school leavers is considered a problem. Reducing the proportion of pupils with a low level of schooling has been a priority of the recent governments. For example, since 1999, a means-tested allowance (Education Maintenance Allowance) is provided for children aged 16 to 18 staying in education (see DfES, 2002, for an evaluation of EMA).

[11] We define parents as natural, adoptive, step or foster parent. The dataset does not distinguish between natural and adoptive parents. See data Appendix 1.

[12] This dichotomous variable is defined as being currently in education or having left full time education after the age of 16.

In our sample, the proportions of individuals leaving school at 16 are 21% for girls and 31% for boys over the period (focusing on 17 year old pupils to limit measurement error). The national post-compulsory education in 1998 was 73% and 66% for 16 years old females and males respectively (DfES). As expected, focusing on children living with their parents leads to a sample of higher achievers, especially for girls. Figure 1 depicts the evolution of the staying-on rate over time. A general upward trend in achievement can be observed for girls but not for boys. An important gender gap in achievement is noticeable, with boys 10 percentage points less likely to remain in post-compulsory education.

[Figure 1: here]

The Education Act of 1972 extended the minimum school leaving age in England and Wales from 15 to 16. The first individuals affected were born on September 1957; their older peers could leave during the summer break, while they had to stay for an extra year of schooling. Compliance to the reform was high (see Harmon and Walker, 1995). As seen in Figure 2, this policy change creates a discontinuity in the years of education attained at the parental generation. As expected, there is a noticeable jump in completed schooling for children born after the reform was implemented. Scotland changed its minimum school leaving age from 15 to 16 three year later[13]. Thus, there is some (limited) variation in the timing of the reform, which allows us to disentangle trend and reform effects.

[13] Since the country where education was completed is not reported, we use current country of residence to determine whether individuals were affected by the RoSLA. This creates some measurement error.

[Figure 2: here]

At the pre-reform generation, about 40% of parents left education at the first opportunity (see Table 1). In our selected sample, the change in the school leaving age led to a reduction in the probability of attaining 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 disproportionately less educated. Since fathers tend to be older than mothers, only 13% of fathers experienced the minimum school leaving age of 16 compared to 25% of mothers. It is also important to note that prior to the reform the proportion of pupils leaving school at 15 was decreasing. For example, for the cohort born between 1943 and 1947, 55% of men left school at 15, but this proportion is down to 38% for the cohort born 10 years later. Due to this trend, it is important to control for parental birth cohort in a model of intergenerational educational mobility. We do so by including a quadratic function in parental birth year in both stages; in some models these functions are interacted with *RoSLA* to create additional instruments.

[Table 1: here]

Focusing on the intergenerational schooling attainment, Table 2 reports the proportion of children with some post-compulsory education, by years of parental

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However, mobility is limited. Most recent Labour Force Surveys reveal that 86% of individuals living in education. For both parents and school leaving age groups, a positive relationship between parental education and the child's investment 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 97% if their father went to university. The effects of both parents education on the decision of children to remain in post-compulsory education are similar in magnitude. A similar intergenerational link is found for the post-reform cohort. However, at all levels of parental education, children whose parents faced a school leaving age of 16 are less likely to be in post-compulsory education, than those with pre-reform parents. The difference is the largest for children whose parents left school at 16. This reflects that parents leaving school at 16 are, on average, of lower ability after the reform than before. Additionally, the reduction of the parental influence may be due to their younger age and be just an income effect.

[Table 2: here]

Table 3 confirms the assumption that the probability of the offspring remaining in post-compulsory education is correlated with parental age. Despite the lower educational achievement of older parents, their children are more likely to remain in education than those with younger parents. It is thus crucial to control for parental birth cohort.

[Table 3: here]

Scotland were born there, and 85% of Scottish born individuals live in Scotland.

V results

A] Base results

As completed education is not observed for the second generation, the focus is on attending post-compulsory education. This dichotomous variable is observed for all children and a probit model is estimated. The exogenous variables include dummies for the year and month the interview took place, region of residence and age and gender of the teenager, as well as quadratic functions in parental birth cohort. The estimates of interest are β_S (see equation (1)) respectively for the mother and the father. Contrary to Black et al. (2003) or Oreopoulos et al. (2003), we only focus on models including both parents' education to eliminate assortative mating bias. Thus, we initially restrict the population of interest to teenagers living with both parents.

Table 4 reports the estimated marginal effects of each year of parental education on the probability of attending post-compulsory education for three different models. The base model only includes parental years of education as additional determinants. The second model adds the logarithm of father's weekly pay and variables to control for labour force participation of the parents. The third model is completed with information on the neighbourhood (proxied by the local tax code) as well as the number of dependent children in the household. The additional variables introduced in model 2 and 3 can be thought of as being endogenous since they are correlated with parental education. Rather than having a causal interpretation, these variables are included to test the possible channels through which parental education affects the child's educational attainment. Initially, each model is estimated assuming that parental education is exogenous, then this assumption is relaxed.

The models assuming endogeneity of parental education are identified by the school leaving age reform. In the first panel of Table 4, a dummy for the post-reform cohort is used as an instrument, thus assuming that after controlling for trends in parental education, the reform shifted the educational attainment of all post-reform cohorts uniformly. Panel B relaxes this assumption of homogeneity of the treatment effect, as the reform dummy is interacted with the quadratic function in parental birth cohort.

[Tables 4A & 4B: here]

In the first model, assuming exogeneity of parental education, each year of parental education increases the probability of staying on by just under 4 percentage points. Father's and mother's influences on their children are not statistically different from each other. These results are quite similar to those of Black et al. (2003). Adding measures for parental participation to the labour force and paternal pay (model 2) has no effect on the estimated coefficients, which remain almost identical to those obtained for model 1. Doubling the paternal wage has almost the same effect on the probability of remaining in education as having a father with an extra 2 years of education (+6.3 percentage points). Having a working mother substantially increases the staying on probability, which is consistent with an income effect, a role model played by the mother, or some ethos of the family. Nevertheless, the intensity of parental participation has a small negative impact, statistically significant only for fathers; each additional 10 hours of paternal work reduces the child's probability of staying on by 0.9 percentage points. The effect of parental education is not solely due to an income effect.

Pupils living in higher tax bound houses are more likely to remain in higher education. This is consistent with Gibbons' (2002) findings on neighbourhood effects in education. The inclusion of the number of sibling and tax code has a marginal impact on the estimated effect of parental education, which is reduced to three percentage points.

In these three models, parental education was treated as an exogenous variable but this assumption is now relaxed. In panel A, each parent's education is instrumented by a variable indicating whether this parent faced a minimum school leaving age of 15 or 16. The estimated effects of the reform on parental years of education are reported at the bottom of Table 4A. Parents affected by the reform have 0.3 years more schooling than pre-reform parents. These coefficients are highly significant and pass the rule of thumb for a weak instrument (Bound et al, 1995). The correlation between the residuals of the two parental education equations is high at 0.55. This is not surprising as parents tend to belong to neighbouring cohorts and therefore experienced similar circumstances. Furthermore, similarities in the unobserved characteristics of parents may explain their choice of education and their decision to mate.

For mothers, the estimated effect of education remains almost identical to the one estimated in the exogenous model, but without the precision. Fathers' education on the other hand becomes insignificant and potentially negative, especially in the less parsimonious model. Due to the lack of precision of the paternal estimates and the similarities between the IV and exogenous estimates for mothers, the endogeneity of parental education is rejected either jointly or individually.

As stated above, this identification strategy assumes that the effect of the reform on the schooling achievement of parents is identical for all post-reform cohorts. To relax this assumption, interactions between the quadratic function in the year of birth and the dummy for post-reform cohort are also included as instruments in the first stage. These estimates are reported in Table 4B. The models assuming exogeneity of parental education are also reported to facilitate comparisons.

The estimates on the reform and interaction terms in the first stage are imprecise, nevertheless F-tests on their joint significance are between 26 and 53 in Model 1, so they are a valid group of instruments. A year of maternal education significantly increases her child's propensity of staying on by 8 to 10 percentage points. These estimates are precisely estimated and significantly different from zero. Father's effects are also larger than in the models presented in panel A, but are imprecisely estimated and bounce between -0.02 and 0.02. These results suggest that the reform did not have a uniform impact on the schooling achievement of all post-reform cohorts and accounting for the trend in education leads to more precise estimates.

Over-identification tests cannot reject the null hypothesis that all instruments lead to the same estimates which is not surprising since all instruments are functions of *RoSLA*. Finally, despite mother's estimate being twice as large in the 2SLS model, the endogeneity of parental education is rejected either jointly or individually.

Assuming a positive correlation between parental education and the error term (due for example to genetic effects), the probit estimates should be biased upwards; it is thus surprising that the IV estimates are larger. However, this is consistent with a LATE interpretation, as parents affected by the school leaving age reform are likely to have had a lower taste for education than the average parent. Assuming decreasing returns to parental education on their children, the reform compliers would have higher returns than average[14]. Since children whose parents have a low taste for education are also the one most at risk of leaving school at the first opportunity, these estimates are of interest.

In both types of identification, we find a larger impact of maternal education than paternal education, consistent with the common wisdom that mothers spend more time with their children than fathers do. These results are in contradiction with recent evidence that have suggested that paternal effects on the educational choice of the second generation were larger than maternal effects. The effect of maternal education appears to be causal and IV estimates are twice as large as estimates based on the assumption of the exogeneity of parental education. Furthermore, the effect of education is direct as models accounting for possible channels of transmission lead to similar results.

The above results may be affected by the family structure; more precisely we test the hypothesis that non-natural parents are less altruistic (Case et al., 2000). The data only records the following three statuses on the relationship between child and parents: (1) natural or adoptee, (2) step child, (3) foster child. Only 824 (134) fathers (mothers) were non-natural, leading to a rather small sample of children living with at least one non-natural parent, mostly a step-father. We drop the 43 children living with foster parents (see Table 5).

In the exogenous model, children with step-parents benefit more from the schooling of their natural parent; this is especially the case for natural mother's education. The natural parent may compensate for the lower attention of the step-parent. When assuming endogeneity of parental education, step-mothers' education has no effect at all on the educational attainment of her step-children and step-fathers' education has a large

[14] Oreopoulos et al. (2003) provide some evidence of decreasing parental returns by estimating the effect of change in SLA at various ages. The lower the new school leaving age, the larger the parental effect on children's educational attainment.

detrimental effect. These estimates are rather imprecise due to the small sample size. The instruments also appear to be weak in this set-up.

For natural parents, results are substantially different from those presented in Table 4B. Both parents' education matters to the same extent; each year of parental schooling increases the probability of staying on by about 8 percentage points. These estimates are significantly different from zero at the 10% confidence interval. The lack of significance of paternal education in the full sample was solely due to the inclusion of step-fathers. Focusing on natural parents only, there is no difference in the causal effect of parental education on their children[15]. As in Case et al. (2000), non-natural parents are found to be less altruistic, it is thus important to control for the relationship of children and the parental figures before assessing intergenerational effects. As policies will affect all children, we thereafter present results for both sub-samples.

[Table 5 here]

B] Testing the identification validity

The identification strategy assumes that parents increased their schooling when affected by the change in minimum school leaving age reform. In this section, we document the validity of the identifying strategy by focusing on the group of parents the most likely to have been affected by the reform. First, we restrict the sample to children with both parents born five (two) years before or after the reform. Second, assuming that

[15] None of the previous conclusions for Table 4 are affected by restricting the sample to natural parents only. The only difference is that paternal effects are larger and more precisely estimated in the restricted sample whilst maternal effects are somehow reduced (see Appendix 2).

signalling in education is limited, the reform should have only a marginal effect on the education investment of parents with high level of schooling.

[Table 6 here]

Educational attainment has been increasing at the parental generation; thus it would be possible that the school leaving age reform has no identifying power and that our previous results are driven by cohort effects, despite our attempts to control for them. To test this assumption, the sample is reduced to children with both parents born in the close vicinity of the reform (5 or 2 years) and living in England and Wales only. From the exogenous models, reported in column 2 and 4 of Table 6, it can be noted that the estimates of the effect of parental education on the propensity to invest in post-compulsory education are between 1 and 1.5 percentage points higher than for the full sample. 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 2% are trimmed by the upper bound.

Since we concentrate on a few cohorts of parents, the instrument chosen to identify the exogenous effect of education is simply whether the parent was affected by the reform (like model (1) in Table 4A). The first stage estimates of the effect of the reform on schooling are similar to those obtained for the full population. The estimated effects of maternal education on children's investment in post-compulsory education are almost identical those obtained with the full sample when interactions were also used as instruments. Thus the fully interacted model accounts for trends in educational attainment and is the favoured model.

For paternal education, the results are more ambiguous. Using the five year period, the estimated effect reaches -7 percentage points but 1.5% for the smaller window. The lack of precision and large variation of the estimates is largely due to the small number of fathers affected by the reform. As fathers are on average one year older than mothers (in the 5 years window sample), the sample is disproportionately composed of pre-reform fathers and the instruments are weaker for fathers than mothers. In the second panel of Table 6, we report results when restricting the sample to children living with both natural parents. They are not significantly different to those obtained for all children.

As a second check, the sample is split between parents most likely to have had their schooling decision influenced by the reform and other parents. If, as suggested by Chevalier et al. (2003), individuals with higher taste for education did not increase their schooling attainment after the reform in order to provide a signal of their ability, only individuals with low level of schooling would have increased their education when affected by the change in SLA. Table 7 reports the results on the effect of the reform on parental education for children whose parents left education at or before the age of 17, and for children whose parents left school after the age of 17. As expected, the reform has no effect on the educational attainment of parents with high level of schooling, and a large positive effect (adding 0.4 years of education) for the parents directly affected.

[Table 7]

These tests support the validity of the identification strategy; the reform of the school leaving age created an exogenous increase in parental education. However, the effect of the reform on post-reform cohorts is not homogenous, so it is important to fully control for trends in educational attainment. Additionally, as suggested previously, only a subgroup of the population complied with the reform and changed their education, thus the instrumental variable estimates can only be interpreted as a LATE.

C] Further results.

Due to the non-linearity in the effect of the reform, the additional results are based on the models where the identification variables are the SLA reform and the interactions between RoSLA and a quadratic function in parental birth cohort. Since additional variables are potentially endogenous and do not change our results, the following results are based on model (1). The regression is conducted separately for the three age groups in our data. The concerns are first misreporting of educational attainment for the 16 year old group, where some children may not yet have been able to make a decision regarding post-compulsory schooling[16]; second, selection issue, which gets more stringent as children age. Children with more caring parents will be more likely to invest in education and also to stay home, thus biasing upwards the estimated parental effect. Table 8 reports the estimates for the three age groups separately.

[Table 8 here]

Assuming the exogeneity of parental education, the estimates obtained at age 16 are significantly reduced compared to those for children aged 17 and 18. Conform to the increased selection of the sample, as children age, the effect of parental education increases, for example, the estimate of maternal schooling, for the 18 year old group is one percentage point higher than for children aged 17. However, when relaxing the exogeneity hypothesis, this pattern disappears, suggesting that selection does not bias the estimated parental effects upwards. On the contrary, parental effects are the largest for the youngest children with an extra year of maternal schooling increasing the probability of post-compulsory schooling by 12 percentage points. Paternal effects are much lower and not statistically different from zero. The instruments are weaker for older children, and the estimates become imprecise; children remaining longer in the parental nest are likely to have parents with higher taste for education and thus their education was less affected by the reform.

For the most restrictive sample of children living with natural parents, no clear pattern can be observed. Only maternal effects for 16 year old are statistically significant, but the point estimates for older children are in the same ball park. The estimated paternal effects range from 4 to 7 percentage points but are rather imprecise. To summarise, splitting the population by age group does not indicate that selection effects bias our estimates substantially, as the point estimates are similar to those obtained on the full sample and do not indicate significant pattern.

VI Further results

As parental effect may be gender specific, the base model is run separately for sons and daughters. The common wisdom states that same-sex intergenerational links are the strongest. Black et al. (2003) on the contrary estimate a positive effect of mother's

[16] In limited dependent variable models, mismeasurement in the dependent variables lead to biased estimates (Hausman, 2001).

education only on their sons. Note that differences by sex in the effect of parental education are not compatible with a pure genetic model.

[Table 9 here]

In the probit models, there is no evidence that mother's schooling has a stronger impact on daughters than on sons. The effect of either parent's education is slightly more important on sons than daughters, maybe because male participation in post-compulsory education is lower than female participation. However, when instrumenting parental education, a different pattern emerges. For girls, paternal education becomes close to nil, whilst maternal education increases. For boys, both parental education attainments increase the probability of post-compulsory education by nine percentage points (imprecisely estimated). However, when focusing on the sample of children living with both natural parents, paternal effect on son's education is greatly increased and reaches an insignificant 16 percentage points whilst the maternal effect is somehow reduced. These results are consistent with models where the same-sex parent plays a role model for the teenager or where parents exhibit preferences for same sex children.

Behrman and Rosenzweig (2002) highlight that education affects the choice of partners. This point has so far been neglected since we have assumed that the identifying strategy accounts for assortative mating. As a test, we estimate our favoured model for children living with a single parent (Table 10). 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 that of married parents. However, when assuming endogeneity of parental education, maternal effect remains at around 0.07 whilst paternal effects become negative, as in the full sample model. Instrumenting each parents' education and estimating the first stage equations jointly has allowed us to control effectively for assortative mating. Alternatively, these results may simply indicate that assortative mating is not an issue.

[Table 10: here]

VI Conclusions

As in other studies, we initially find that parental education has a significant effect on their children's educational attainment; increasing parental education by one year increases the probability of staying on by 4 percentage points. We identify the exogenous effect of parental education by relying on changes in compulsory school leaving age. This identification strategy estimates a local average treatment effect, since only parents who wished to leave school at 15, those with either a lower taste for education, a lower ability or a financial constraint, were affected by the reform. The IV estimates are therefore not directly comparable to the initial estimates.

Assuming the endogeneity of parental education leads to estimates of mother's effect on the decision to remain in post-compulsory education that are twice as large. In almost all the models presented, we reject the endogeneity of parental education. Maternal education thus has a causal effect on the education of her children. Paternal education on the other hand becomes close to zero or even negative in most models. However, when focusing on natural parents only, paternal education has as large an effect as maternal education and both are significantly different from 0. Furthermore, the effect of maternal education is larger for daughters while paternal education matters for son's educational decision only, suggesting a role model played by same sex parents. These estimates are consistent both for different age groups and children with different family structures, suggesting that they are not affected by selection effects. We also provide some evidence that the identification strategy is valid and not solely due to trends in educational attainment. A remaining research issue is to identify whether increased parental education improve parental skills or simply change the value that parents attach to the education of their children. Tentatively, controls for financial situation, labour market attachment, fertility and neighbourhood were introduced. Assuming that all these variables had an exogenous effect, the impact of parental education remains unchanged.

The estimates of the effect of parental education on their children's educational attainment are that each year of education increases the probability of staying on by 4 to 8 percentage points. How do these estimates compare with other policies implemented in the UK and with estimates of parental education available in the literature? The education maintenance allowance experiment, introduced in the UK at the end of the Nineties, has increased participation of 16 to 18 year old in the treated area by 6 percentage points[17] (DfES, 2002), which is equivalent to increasing one parent's education by one or two years. The population affected by EMA is likely to be similar to the one for which our IV strategy is valid since EMA is means tested and targeted at the poorest children, the parents of whom are likely to have had a low level of schooling.

Oreopoulos et al. (2004) estimate a reduction in the probability of grade repeat between five and eight percentage points. Bjorklund et al. (2004), report estimates of maternal effect on probability of going to university reaching 6 percentage points. Other studies typically report years of education. Using EMA evidence, sixty-three percent of the pupils receiving support, registered in a short vocational course rather than followed 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, the 4 to 8 percentage points increase in post-compulsory education is equivalent to an average increase of 0.1 to 0.2 years of education for the whole population. This is in line with the estimates between 0.05 and 0.20 found in the literature.

Contrary to some recent estimates, we find that both parents' education has a causal impact on the schooling attainment of their children, when focusing on natural parents only, and that for the population of interest, the effects are twice as large as when assuming the exogeneity of parental education. Whilst these estimates are only valid for parents with a lower taste for education, and are likely to overestimate the effect of a reform affecting all parents, they are of interest, since they are relevant for the population targeted by recent policies in Britain. Increasing education has positive effects on the next generation. These long-term effects should be taken into account when estimating the social rate of returns to education.

[17] 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.

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Table 1: Parental schooling distribution by cohort

	Father					
	Pre-reform				Post-reform	
Age left school	1938-1942	1943-1947	1948-1952	1953-1957	1958-1962	1963-1967
15	55.2	43.7	39.4	38.1	1.3	0
16	17.4	20.9	25.1	31.3	79.0	83.3
17-18	11.4	13.9	14.4	15.8	12.3	11.4
19-20	3.4	3.8	3.7	4.3	2.2	1.6
21-22	5.5	9.3	10.7	7.3	3.7	2.4
23+	6.7	8.4	6.5	3.7	1.4	1.2
Obs	1250	2545	4298	3458	1487	245

	Mother					
	Pre-reform				Post-reform	
Age left school	1938-1942	1943-1947	1948-1952	1953-1957	1958-1962	1963-1967
15	58.5	40.3	34.9	37.2	0.8	0
16	17.4	22.0	26.3	29.4	75.8	83.5
17-18	13.0	19.0	17.5	20.6	18.2	13.7
19-20	1.5	3.5	4.0	4.2	2.2	1.4
21-22	7.2	12.1	14.4	7.1	2.1	1.3
23+	2.5	2.7	2.7	1.5	0.6	0.1
Obs	484	1840	4563	5298	3305	728

Table 2: Intergenerational schooling choices

Age parent left school	Proportion with post-compulsory schooling Father SLA 15	SLA 16	Proportion with post-compulsory schooling Mother SLA15	SLA16
15	68.24	-	67.32	-
16	80.33	69.21	79.27	68.12
17	87.10	86.11	86.52	85.64
18	91.40	87.76	92.30	89.61
19-21	96.44	94.87	97.19	96.30
22-25	97.33	92.21	97.37	98.00
Observations	11662	1632	12376	3866

Table 3: Parental cohort and proportion of children with post-compulsory education

	Parental cohort	% child in education			
		Dad	Observation	Mother	Observation
Pre-reform	Born 38-42	80.40	1250	78.72	484
	Born 42-47	81.57	2545	82.45	1840
	Born 48-52	80.90	4298	81.39	4563
	Born 53-57	76.95	3458	77.99	5298
Post-reform	Born 58-62	72.70	1487	72.34	3305
	Born 63-67	72.65	245	71.15	728

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

Panel: A	Instrument: Leaving age reform					
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.039	0.037	0.037	0.038	0.029	0.044
	(0.003)	(0.050)	(0.003)	(0.052)	(0.003)	(0.049)
Father's schooling	0.036	-0.009	0.033	-0.015	0.027	-0.055
	(0.002)	(0.059)	(0.002)	(0.056)	(0.002)	(0.072)
Father's income[A]			0.063	0.121	0.042	0.084
			(0.008)	(0.070)	(0.008)	(0.057)
Father's hours worked *100			-0.089	-0.169	-0.095	-0.184
			(0.032)	(0.072)	(0.032)	(0.104)
Mother works			0.094	0.122	0.067	0.091
			(0.058)	(0.085)	(0.055)	(0.074)
Mother's hours worked *100			-0.010	0.023	0.023	0.036
			(0.030)	(0.108)	(0.030)	(0.109)
Council tax dummies: $\chi^2(8)$					98.63	61.19
					Pr = 0	Pr = 0
Nbr dependent children					0.075	0.084
					(0.005)	(0.018)
Excluded Instrument (F test)[B]		Dad: 11.6		Dad: 12.8		Dad: 10.0
		Mom: 25.7		Mom: 25.1		Mom:26.7
Exog. Test (χ^2)[C]						
Joint		1.89 pr = .39		1.77 pr = .41		2.31 pr = .31
Father only		1.81 pr=.18		1.72 pr=.19		2.25 pr=.13
Mother only		0.26 pr=.61		0.21 pr=.65		0.05 pr=.82
1st stage equations						
Dad SLA 16		0.314		0.322		0.279
		(0.092)		(0.090)		(0.088)
Mom SLA 16		0.328		0.318		0.323
		(0.065)		(0.064)		(0.062)

Note: The model is estimated by for individuals living with both parents (12593 observations). The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

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

Panel: B	Instrument: school reform and interaction with parental age					
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.039	0.106	0.037	0.088	0.029	0.081
	(0.003)	(0.037)	(0.003)	(0.041)	(0.003)	(0.044)
Father's schooling	0.036	-0.009	0.033	0.020	0.027	-0.023
	(0.002)	(0.059)	(0.002)	(0.041)	(0.002)	(0.055)
Father's income[A]			0.063	0.025	0.042	0.011
			(0.008)	(0.040)	(0.008)	(0.037)
Father's hours			-0.089	-0.033	-0.095	-0.020
worked *100			(0.032)	(0.068)	(0.032)	(0.067)
Mother works			0.094	0.015	0.067	0.008
			(0.058)	(0.059)	(0.055)	(0.055)
Mother's hours			-0.010	-0.079	0.023	-0.065
worked *100			(0.030)	(0.087)	(0.030)	(0.087)
Council tax					98.63	61.36
dummies: $\chi^2(8)$					Pr = 0	Pr = 0
Nbr dependent					0.075	0.060
children					(0.005)	(0.011)
Excluded		Dad: 26.1		Dad: 24.1		Dad: 14.8
Instrument (F test)[B]		Mom: 53.2		Mom: 41.8		Mom:37.2
Exog. Test : (χ^2)[C]						
Joint		1.80 pr=0.40		0.89 pr = .64		1.41 pr = .49
Father only		0.04 pr=.84		0.00 pr=.99		0.38 pr=.54
Mother only		1.74 pr.19		0.81 pr=.37		0.63 pr=.43
Hansen J: (χ^2)[D]		2.81 pr = .59		3.17 pr = .53		3.51 pr=.48

Note: The model is estimated by for individuals living with both parents (12593 observations). The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform and the interactions of this dummy with a quadratic function in parental birth cohort. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

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

	2 natural parents		Step mother		Step-father	
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.037	0.076	0.020	-0.002	0.061	0.061
	(0.003)	(0.037)	(0.027)	(0.100)	(0.014)	(0.172)
Father's schooling	0.036	0.078	0.038	0.055	0.044	-0.173
	(0.002)	(0.044)	(0.020)	(0.226)	(0.012)	(0.162)
Excluded Instrument		Dad: 24.9		Dad: 1.33		Dad: 6.11
(F test)[B]		Mum: 47.7		Mum: 9.10		Mum: 7.46
Endogeneity Test[C]						
Joint		3.19 pr=.20		0.14 pr=.93		4.69 pr=.09
Father only		3.14 pr=0.08		0.00 pr=.99		4.69 pr=.03
Mother only		0.76 pr=.38		0.13 pr=.72		0.01 pr=.90
Hansen J: (χ^2)[D]		2.98 pr=.56		2.42 pr=.66		4.84 pr=.30
Observations	11460	11460	147	147	824	824

Note: The model is estimated by for individuals living with both parents. The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform and the interactions of this dummy with a quadratic function in parental birth cohort. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

[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 – Size of the window around reform

All children	Both parents born 5 years around the reform		Both parents born 2 years around the reform	
	Probit	IV	Probit	IV
Mother's schooling	0.049	0.081	0.053	0.101
	(0.007)	(0.074)	(0.010)	(0.091)
Father's schooling	0.047	-0.071	0.052	0.014
	(0.006)	(0.154)	(0.009)	(0.103)
Excluded Instrument		Dad: 4.50		Dad: 10.1
(F test)[B]		Mum: 21.2		Mum: 18.9
Endogeneity Test[C]				
Joint		1.10 pr=.57		0.33 pr=.85
Father only		1.08 pr=.30		0.24 pr=.62
Mother only		0.00 pr=.94		0.08 pr=.78
Observations	3590	3590	2104	2104
1st stage equations				
Dad SLA 16		0.253		0.445
		(0.119)		(0.140)
Mom SLA 16		0.402		0.498
		(0.087)		(0.114)
Natural children only				
Mother's schooling	0.048	0.080	0.054	0.099
	(0.007)	(0.117)	(0.011)	(0.155)
Father's schooling	0.048	-0.126	0.053	0.007
	(0.006)	(0.377)	(0.009)	(0.113)
Excluded Instrument		Dad: 2.4		Dad: 8.7
(F test)[B]		Mum: 17.6		Mum: 14.6
Endogeneity Test[C]				
Joint		1.66 pr=.44		0.42 pr=.82
Father only		1.66 pr=.20		0.40 pr=.53
Mother only		0.00 pr=.99		0.02 pr=.90
Observations	3238	3238	1914	1914
1st stage equations				
Dad SLA 16		0.199		0.433
		(0.127)		(0.147)
Mom SLA 16		0.380		0.454
		(0.091)		(0.119)

Note: The model is estimated by for individuals living with both parents. The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications). The regression is based on England and Wales only.
For additional information, see note for Table 4.

Table 7: Effect of school leaving age reform on parental education, by education group

	Parents leaving education at 17 or before	Parents who left school after the age of 17
Mother's SLA	0.439	-0.142
	(0.026)	(0.199)
Father's SLA	0.411	-0.057
	(0.030)	(0.288)
Observations	8339	2558

Note: The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. The model was estimated for children with both parents leaving school before or at the age of 17, and for those with both parents leaving school after the age of 17.

Table 8: Parent's education and child's probability of post-compulsory schooling Children living with both parents, by age of child

All children	Age 16		Age17		Age 18	
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.026	0.117	0.044	0.082	0.054	0.065
	(0.004)	(0.052)	(0.004)	(0.042)	(0.006)	(0.177)
Father's schooling	0.025	0.028	0.043	-0.030	0.040	0.015
	(0.003)	(0.049)	(0.005)	(0.058)	(0.005)	(0.111)
Excluded Instrument (F test)[B]		Dad: 12.40 Mum: 22.44		Dad: 11.85 Mum:31.81		Dad:7.05 Mum: 7.20
Endogeneity Test[C]						
Joint		3.00 pr= .22		1.02 pr=.60		0.08 pr=.96
Father only		0.22 pr=.64		0.88 pr=.35		0.04 pr=.85
Mother only		3.00 pr=.08		0.00 pr=.97		0.07 pr=.79
Hansen J: (χ^2)[D]		0.91 pr=.92		1.58 pr=.81		4.95 pr=.29
Observations	4635	4635	4369	4369	3589	3589
Natural children only						
Mother's schooling	0.025	0.072	0.041	0.053	0.049	0.081
	(0.003)	(0.037)	(0.005)	(0.051)	(0.006)	(0.103)
Father's schooling	0.025	0.068	0.043	0.037	0.041	0.052
	(0.003)	(0.057)	(0.004)	(0.073)	(0.005)	(0.076)
Excluded Instrument (F test)[B]		Dad: 9.71 Mum: 23.87		Dad: 11.07 Mum:24.42		Dad:8.90 Mum: 8.64
Endogeneity Test[C]						
Joint		2.07 pr= .33		0.32 pr=.85		0.24 pr=.89
Father only		1.90 pr=.17		0.10 pr=.75		0.24 pr=.63
Mother only		0.85 pr=.35		0.12 pr=.73		0.07 pr=.79
Hansen J: (χ^2)[D]		4.36 pr=.36		1.00 pr=.80		1.93 pr=.75
Observations	4187	4187	3966	3966	3307	3307

Note: The model is estimated by for individuals living with both parents. The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform and the interactions of this dummy with a quadratic function in parental birth cohort. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

[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 Children living with both parents, by gender

All Children	Daughter		Son	
	Probit	IV	Probit	IV
Mother's schooling	0.036	0.127	0.043	0.087
	(0.004)	(0.053)	(0.004)	(0.059)
Father's schooling	0.028	0.000	0.043	0.090
	(0.003)	(0.051)	(0.004)	(0.098)
Excluded Instrument		Dad: 16.93		Dad: 11.95
(F test)[B]		Mum: 26.05		Mum: 29.26
Endogeneity Test[C]				
Joint		2.65 pr=.27		1.53 pr=.46
Father only		0.07 pr=.80		1.40 pr=.24
Mopther only		2.15 pr=.14		0.60 pr=.44
Hansen J: (χ^2)[D]		5.66 pr=.22		8.09 pr=.09
Observations	6080	6080	6513	6513
Natural Children only				
Mother's schooling	0.029	0.125	0.042	0.046
	(0.004)	(0.051)	(0.004)	(0.057)
Father's schooling	0.029	0.023	0.041	0.162
	(0.003)	(0.038)	(0.004)	(0.109)
Excluded Instrument		Dad: 18.48		Dad: 8.51
(F test)[B]		Mum: 22.56		Mum: 27.79
Endogeneity Test[C]				
Joint		2.54 pr=.28		4.53 pr=.10
Father only		0.45 pr=.50		3.94 pr=.05
Mopther only		2.51 pr=.11		0.02 pr=.88
Hansen J: (χ^2)[D]		4.16 pr=.38		6.97 pr=.14
Observations	5518	5518	5942	5942

Note: The model is estimated by for individuals living with both parents. The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform and the interactions of this dummy with a quadratic function in parental birth cohort. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

[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 10: Parent's education and child's probability of post-compulsory schooling: Single parents

	Single mother		Single father	
	Probit	IV	Probit	IV
Mother's schooling	0.088 (0.006)	0.071 (0.051)		
Father's schooling			0.060 (0.012)	-0.049 (0.359)
Excluded Instrument (F test)[B]		9.55		0.31
Endogeneity Test[C]		0.067 pr=.79		0.31 pr=0.57
Hansen J: (χ^2)[D]		3.75 pr=.15		0.99 pr=.61
Observations	3649	3649	701	701

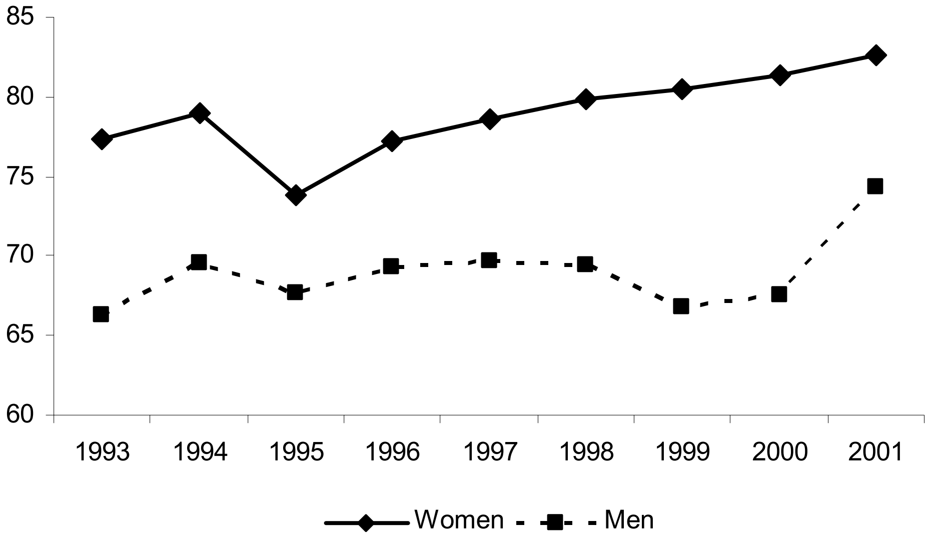
Note: The model is estimated for individuals living with one parent only. The instruments include a dummy for minimum school leaving age, year of birth and interaction year of birth, SLA

[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.

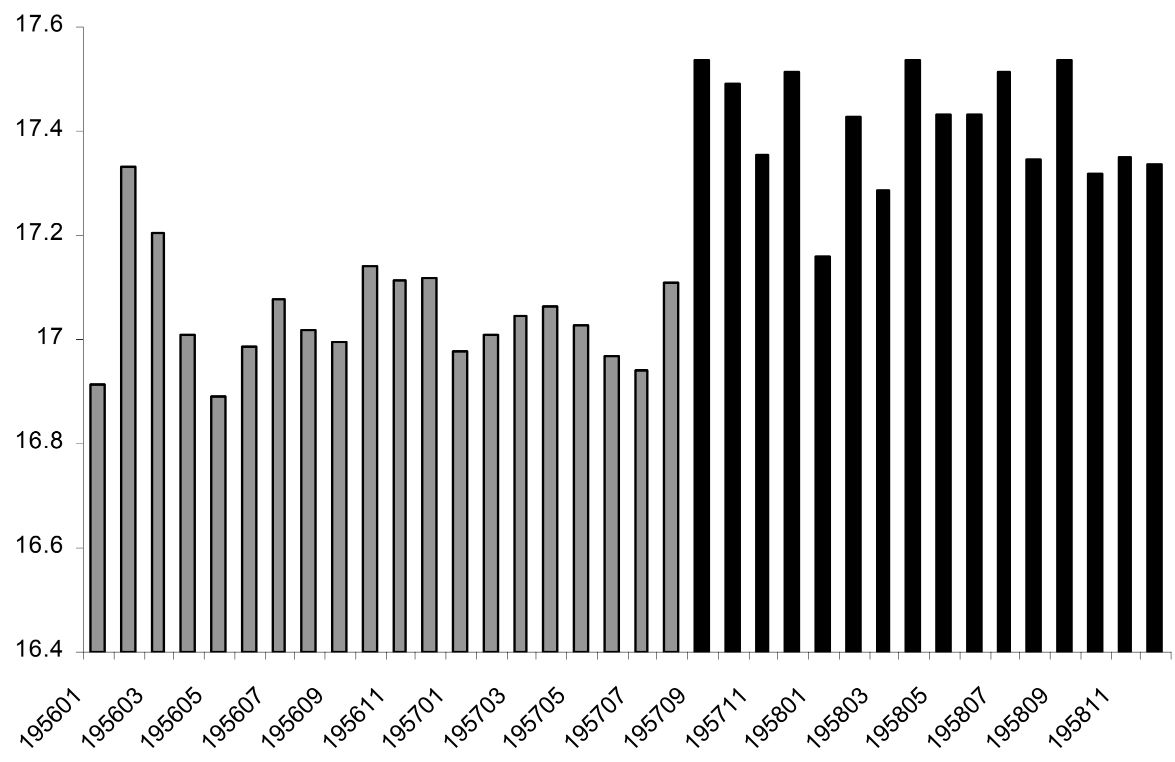
[D]: Hansen J statistics is distributed as a (χ^2) and was obtained by estimating a linear model by GMM. Results from this estimation were almost identical to those presented.

Figure 1: Proportion of 17 years old with post-compulsory schooling



Note: The year on the axis refers to the year the child was 16

Figure 2: Years of schooling by birth cohort: Jan 1956- Dec 1958



Note: Source: LFS 1993-2001

Appendix 1: Sample selection in FRS (1994-2002)

A- Sample selection

18,715	age 16-18
17,634	Living with at least one parent
16,943	Parents older than 15 when kid born or less than 55

B- Selection issues due to family situation

	Live with at least one parent	Live with both parents	Live with natural parents	Live alone
Left school at 15 or before	7.21	6.55	6.49	26.27
Currently in FT education	70.45	79.31	80.13	24.61
Female	48.50	48.28	48.15	69.75
Age 16	37.28	36.81	36.54	9.62
Age 17	34.68	34.69	34.61	27.94
Age 18	28.05	28.50	28.86	62.44
Observations	16,943	12,593	11,460	1,081

Appendix 2: Results for subsample of natural children only

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

Panel: A	Instrument: Leaving age reform					
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.037	0.024	0.035	0.027	0.028	0.032
	(0.003)	(0.051)	(0.003)	(0.053)	(0.003)	(0.050)
Father's schooling	0.036	0.044	0.033	0.025	0.027	-0.018
	(0.002)	(0.081)	(0.002)	(0.075)	(0.002)	(0.098)
Father's income[A]			0.057	0.058	0.039	0.071
			(0.009)	(0.106)	(0.009)	(0.074)
Father's hours worked *100			-0.072	-0.108	-0.080	-0.158
			(0.032)	(0.102)	(0.032)	(0.134)
Mother works			0.102	0.098	0.078	0.115
			(0.063)	(0.146)	(0.059)	(0.119)
Mother's hours worked *100			0.000	-0.009	0.036	8.921
			(0.030)	(0.153)	(0.030)	(9.757)
Council tax dummies: $\chi^2(8)$					77.13	49.62
					Pr = 0	Pr = 0
Nbr dependent children					0.075	0.089
					(0.005)	(0.026)
Excluded Instrument (F test)[B]		Dad: 7.4		Dad: 8.2		Dad: 6.3
		Mom: 20.3		Mom: 18.6		Mom:19.8
Exog. Test (χ^2)[C]						
Joint		0.62 pr = .73		0.56 pr = .76		0.64 pr = .73
Father only		0.25 pr=.62		0.27 pr=.60		0.57 pr=.45
Mother only		0.49 pr=.48		0.40 pr=.53		0.17 pr=.68
1st stage equations						
Dad SLA 16		0.269		0.278		0.239
		(0.099)		(0.097)		(0.095)
Mom SLA 16		0.314		0.295		0.300
		(0.070)		(0.068)		(0.067)

Note: The model is estimated by for individuals living with both parents (11460 observations). The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

Table A4 B: Parent's education and child's probability of post-compulsory schooling. Natural Children living with both parents

Panel: B	Instrument: school reform and interaction with parental age					
	Probit	IV	Probit	IV	Probit	IV
Mother's schooling	0.037	0.076	0.035	0.060	0.028	0.053
	(0.003)	(0.035)	(0.003)	(0.040)	(0.003)	(0.041)
Father's schooling	0.036	0.078	0.033	0.077	0.027	0.056
	(0.002)	(0.044)	(0.002)	(0.051)	(0.002)	(0.065)
Father's income[A]			0.057	-0.055	0.039	-0.012
			(0.009)	(0.062)	(0.009)	(0.050)
Father's hours			-0.072	-0.005	-0.080	-0.010
worked *100			(0.032)	(0.067)	(0.032)	(0.094)
Mother works			0.102	-0.033	0.078	0.010
			(0.063)	(0.081)	(0.059)	(0.079)
Mother's hours			0.000	-0.166	0.036	-2.700
worked *100			(0.030)	(0.102)	(0.030)	(9.594)
Council tax					77.13	46.00
dummies: $\chi^2(8)$					Pr = 0	Pr = 0
Nbr dependent					0.075	0.059
children					(0.005)	(0.018)
Excluded		Dad: 24.9		Dad: 21.2		Dad: 12.8
Instrument (F test)[B]		Mom: 47.7		Mom: 36.2		Mom:31.0
Exog. Test : (χ^2)[C]						
Joint		3.19 pr=0.20		2.54 pr = .28		1.07 pr = .59
Father only		3.14 pr=0.08		2.54 pr=.11		1.03 pr=.31
Mother only		0.76 pr=0.38		0.34 pr=.56		0.27 pr=.60
Hansen J: (χ^2)[D]		2.97 pr = .56		3.38 pr = .50		4.21 pr=.38

Note: The model is estimated by for individuals living with both parents (11460 observations). The IV first stage regressions are estimated simultaneously. For each parent, the instrument is a dummy taking the unit value if in compulsory education after the SLA reform and the interactions of this dummy with a quadratic function in parental birth cohort. The model contains the following additional control: quadratic function in father and mother's age, gender and age of the child, dummies for years and month of interview, and dummies for administrative region of residence. Standard errors in the IV models are obtained by bootstrap (500 replications).

[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 each of the first stage regression.

[C]: Smith and Blundell (1986) test of exogeneity. The residuals from each first-stage 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. The residuals are also introduced separately and their significance tested.

[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.