# The Impact of Parental Income and Education on the Schooling of their Children

**Keyword:** Early school leaving, intergenerational transmission

**JEL Classification:** I20, J62

**Abstract:**

This paper addresses the intergeneration transmission of education and investigates the extent to which early school leaving (at age 16) may be due to variations in permanent income, parental education levels, and shocks to income at this age. Least squares estimation reveals conventional results - stronger effects of maternal education than paternal, and stronger effects on sons than daughters. We find that the education effects remain significant even when household income is included. Moreover, decomposing the income when the child is 16 between a permanent component and shocks to income at age 16 only the latter is significant. It would appear that education is an important input even when we control for permanent income but that credit constraints at age 16 are also influential. However, when we use instrumental variable methods to simultaneously account for the endogeneity of parental education and paternal income, we find that the strong effects of parental education become insignificant and permanent income matters much more, while the effects of shocks to household income at 16 remain important. A similar pattern of results are reflected in the main measure of scholastic achievement at age 16. These findings have important implications for the design of policies aimed at encouraging pupils to remain in school longer.

## 1. Introduction

A considerable literature has focused on the effects of parental background on such outcomes for their children as cognitive skills, education, health and subsequent income (see, for example, Jere R. Behrman, 1997). There is little doubt that economic status is positively correlated across generations. Parents, and the family environment in general, have important impacts on the behavior and decisions taken by adolescents.

The view that more educated parents provide a "better" environment for their children has been the basis of many interventions. Moreover, while the scientific literature is not so clear, it is widely believed that while raising the education for mothers and fathers has broadly similar effects on household income, the external effects associated with education is larger for maternal education than for paternal because mothers tends to be the main provider of care within the household. For example, a positive relationship between mother's education and child birth weight, which is a strong predictor of child health, is found not only in the developing world but also in the US (see, for example, Janet Currie and Enrico Moretti, 2003). The existence of such externalities provides an important argument for subsidizing the education of children, especially in households with low income and/or low educated parents. Indeed there may be multiplier effects since policy interventions that increase educational attainment for one generation may spillover onto later generations.

While the existence of intergenerational correlations is not disputed, the nature of the policy interventions that are suggested depends critically on the characteristics of the intergenerational transmission mechanism and the extent to which the correlation is causal. In particular, is has proved difficult to determine whether the transmission mechanism works through inherited genetic factors or environmental factors and, if it is the latter, what is the relative importance of education and income. For example, ability is positively associated with more schooling and ability may be partly transmitted from parents to children[1]. The link, therefore, between the schooling of parents and their children could be due to unobserved inherited characteristics rather than a causal effect of parental education *per se* in household production. A related issue is the extent to which any causal effect of education works through the additional household income associated with higher levels of education. That is, parental educations may be both direct inputs into the production function that generates child quality and may indirectly facilitate a higher quantity of other inputs through the effect of educational levels on household income.

This paper addresses two major issues in the existing literature: the causal effect of parental education on children, allowing for separate effects of mother's and father's education; and the causal effect of household income. To date no study has simultaneously tried to account for both the endogeneity of parental education and of income. This is a crucial distinction since important policy differences hang on their relative effects. A further important innovation of this paper is that, in addition to controlling for both income and education, we try to decompose the effects of income into permanent and transitory elements. The motivation for the former is that a policy might be concerned with providing financial transfers to households holding parental education levels constant – for example income support policies aimed at relieving child poverty. The importance of the latter innovation is that we need to distinguish credit constraints at the point of decision to enter post-compulsory schooling from the permanent effects of long term income differences so as to inform policy.

Using a British dataset, we begin by confirming the usual finding using OLS - that parental education levels are positively associated with good child outcomes by looking, in

[1] Taking IQ as a measure of ability, the correlation in IQ between parents and natural children is 0.42 for

particular, at early school leaving decisions. We go on to investigate the relative effects of parental education levels and household income on early school leaving and achievement at age 16 (measured as the number of GCSE qualifications obtained at the passing grades of A to C). These two measures are important because the present government has targeted a reduction in the proportion of pupils leaving at 16, and because the number of GCSE passes is influential in determining one's opportunities for post-compulsory study. We go on to use instrumental variable methods to take account of the endogeneity of both parental income and education. Finally we exploit the rotating panel aspect of our dataset to identify transitory and permanent income. The plan of the paper is as follows. Section 2 outlines the existing literature. Section 3 explains the nature of the data used. Section 4 provides the base estimates which are extended in Section 5 to separate permanent and transitory effect of paternal income, as well as focusing on GCSE score rather than schooling decision at 16. Section 5 concludes.

children living with their parents and 0.22 for those brought up apart (see Marcus W. Feldman *et al* , 2000).

## 2. Previous Literature

A number of studies have found a strong link between earnings of the parent (typically the father) and of the child with the intergenerational correlation in earnings between fathers and sons between 0.40 and 0.50 in the US and 0.60 in the UK. There is also a relationship between parental education and the education of their offspring. Estimates of the elasticity for intergenerational mobility in education lie between 0.14 to 0.45 in the US and 0.25 to 0.40 in the UK (see Lorraine Dearden *et al.* (1997) for the UK, and Casey Mulligan (1999) and Gary Solon (1999) for the US).

Children brought up in less favorable conditions obtain less education despite the large financial returns to schooling (see James J. Heckman and Dimitry Masterov (2004) for an extensive review). However the mechanism by which such intergenerational correlations are transmitted is not clear. Alan B. Krueger (2004) reviews various contributions supporting the view that financial constraints significantly impact on educational attainment. On the contrary, Pedro Carneiro and Heckman (2003) suggests that *current* parental income does not explain child educational choices but that family fixed effects such as parental education levels, that contributes to *permanent* income, have a much more positive role. This is the central conclusion of Stephen Cameron and Heckman (1998) using US data, and Arnaud Chevalier and Gauthier Lanot (2002) using the UK National Child Development Study data. Chevalier (2004), using the Family Resources Survey cross-section data, finds that including father's income in the schooling choice equation of the child, while itself a significant and positive effect, does not dramatically change the magnitude of the parental education coefficients. However, the potential endogeneity of income means that this correlation does not necessarily imply that parental income matters for children's human capital accumulation. Indeed if income is endogenous and is correlated with education, then the education coefficients are also biased.

So far, researchers have been able to identify the exogenous effect of parental education or income but not both effects simultaneously. The literature on estimating the causal effect of parental education on the child's educational attainment has relied on three identification strategies. Behrman and Mark Rosenzweig (2002) use the Minnesota Twins Register to examine educational choice of children of twin pairings (who are therefore cousins) to eliminate the nature effect of one of the parents. Based on simple OLS models, they find large effects: one year of maternal schooling increased children's years of education by 13% while the effect of paternal schooling was about twice as large. However between-cousins estimates of maternal education effects are negative albeit insignificant. This contradicts the general view that maternal schooling has a larger and positive effect than paternal schooling on the achievement of their children[2].

An alternative strategy to account for genetic effects is to compare adopted and natural children. Both Bruce Sacerdote (2002) and Erik Plug (2003) report that, controlling for ability and assortative mating, the positive effect of maternal education on children's education disappears. This literature assumes that the presence of adopted children is uncorrelated with unobservables across families. However adopted and natural children may have different characteristics, be treated differently in school or by society (especially when of different race from their parents), or may have incurred some stigma from adoption. Additionally, adoptive families may provide a different environment to children such as more (or less) attention to the child. As evidence of differences in the environment of adopted and natural children, Barbara Maughan *et al* (1998) find that adoptees performed more positively

[2] In a critical analysis of the Behrman and Rosenzweig (2002) data, Karen Antonovics and Arthur Goldberger (2004) show that results are quite sensitive to the selection of children who have completed education and who

than non-adopted children from similar families on childhood tests of reading, mathematics, and general ability. In contrast, Anders Bjorklund *et al.* (2004) uses a register of Swedish adoptees which confirms the findings of studies such as Plug (2003) and when correcting for the potential bias caused by non-randomness in this population they find that genetics account for about 50% of the correlation in education between generations but that after accounting for genetics, the causal effects of parental education remains highly significant.

The third identification strategy is to use instrumental variables methods based on 'natural' experiments or policy reforms which change the educational distribution of the parents without directly affecting children. Sandra E. Black *et al*. (2003) exploit Norwegian educational reforms which raised the minimum number of years of compulsory schooling over a period of time and at differential rates between regions of the country. They find evidence of parental impact in the OLS estimates of education outcomes for the children but estimates based on IV do not show this effect, with the exception of (weak) evidence of mother/son influences. However, Philip Oreopoulos *et al.* (2003) using the same approach and pooling US Census data from 1960, 1970 and 1980 report that an increase in parental education by one year decreases the probability of repeating a schooling year (or grade) by between two and seven percentage points.

The UK provides similar policy changes which are exploited in Chevalier (2004) and Fernando Galindo-Rueda (2003). Changes in the minimum school leaving age which occurred just after World War II and in the early 1970s meant that the educational choice of parents was exogenously affected, at least for those wishing to leave school at the earliest

are aged 18 and over, rather than 16 and over. Behrman, Rosenzweig and Junsen Zhang (2004) repeat the original analysis on a large Chinese dataset and find strong support for their earlier analysis.

age. Some parents experienced an extra year of education compared to other parents who were similar to them in other respects except birth year. This discontinuity is exploited to identify the effect of parental education on their children's education. Chevalier (2004) finds that for both parents, OLS estimates of the effect of one year of parental education on the probability of post-compulsory education is about 4%, with the effects slightly larger for sons than daughters. Using the 1974 change in the school leaving age legislation as an instrument for parental education, the effects of a parent's education on the child of the same gender increased substantially (for a sample of natural parents). Galindo-Rueda (2003) exploited the earlier 1947 reform and, relying on regression discontinuity, find significant causal effect but only for fathers.

The literature on the causal effects of parental *earnings* or *incomes* on educational outcomes is not extensive. Random assignment experiments are potentially informative but uncommon. Jo Blanden and Paul Gregg (2004) review US and UK evidence on the effectiveness of policy experiments which largely focus on improving short term family finances. These include initiatives such as the Moving to Opportunity (MTO) experiments in the US which provide financial support associated with higher housing costs from moving to more affluent areas. MTO programs are associated with noticeable improvements in child behavior and test scores but whether these are caused by the financial gain or the environment, school and peer-group changes is unclear[3]. In the UK, the pilots of Educational Maintenance Allowances (EMA's) provided a sizeable means tested cash benefit conditional on participation in education and paid, depending on pilot scheme, either to the parents or directly to the child (UK Department for Education and Skill, 2002). Enrollments increased by up to 6% in families eligible for full subsidies. However, this transfer was conditional on staying in school and so does not tell us about the effects of unconditional variations in income.

In the absence of experimental evidence, instruments have been used to identify income effects. John Shea (2000) uses union status (and occupation) as an instrument for parental income and therefore assumes that unionized fathers are not more 'able' parents than nonunion fathers with similar observable skills, while Bruce Meyer (1997) uses variation in family income caused by state welfare rules, income sources and income before and after the education period of the child, as well as changes in income inequality. While strong identification assumptions are used in both these studies, they both find that unanticipated changes in parental long-run income have modest and sometimes negligible effects on the human capital of the children[4]. Using UK data, Blanden and Gregg (2004) find the correlation between family income and children's educational attainment has actually risen between the 1970 birth cohort data and the later British Household Panel Survey data containing children reaching 16 in the late 1990's. They estimate the causal effect of family income in ordered probit models of educational attainment (from no qualifications up to degree level) based on sibling differences in the panel data. They also provide estimates of the probability of staying-on at school past the minimum age of 16. However the paper cannot simultaneously provide estimates of the causal effect of parental education because this is treated as a fixed effect in the sibling difference estimates and so differences out.

[3] Note that new work on MTO by Lisa Sanbonmatsu *et al* (2004) suggests that MTO-driven neighbourhood effects on academic achievement were not significant.

[4] Daron Acemoglu and Jorn-Steffan Pishke (2001) use similar arguments to Meyer (1997) and exploit changes in the family income distribution between the 1970's and 1990's. They find a 10 percent increase in family income is associated with a 1.4% increase in the probability of attending a four year college.

Finally, Stephen Jenkins and Christian Schluter (2002) is notable for being one of the few studies to control both for income at various ages and education. Using a small German dataset to study the type of school attended (vocational or academic) they find that later income is more important than early income, but that income effects are small relative to education effects. The analysis in their paper, as in Blanden and Gregg (2004), assumes the exogeneity of income and parental education. Here, we follow Shea (2000) in using union status as an instrument for income.

## 3. Data and Sample Selection

To carry out this research, data on two generations are required in a single data source – education of the individual children and the education and incomes of their parents. Our analysis is based on the Labour Force Survey (LFS) which is a quarterly sample of households in the U.K. In each quarter there are roughly 138 thousand respondents from the approximately 59 thousand households surveyed. Households are surveyed for five consecutive quarters. We pool the data from households in the fifth quarter over the period 1993-2003[5]. Children aged 16 to 18 living at home are interviewed in the LFS so parental information can be matched to the child's record[6]. Our sub-sample consists of those children observed in LFS at ages 16 to 18 inclusive (and therefore have made their decision with respect to post compulsory education participation).

[5] Pre-1998, earnings data is available only for fifth wave respondents, post 1998, the earnings data is collected in the first and in the final wave.

[6] Chevalier (2004) uses the Family Resource Survey data which in many respects is similar to the LFS data in this paper but allow to distinguish between natural and step parents which is not possible with the LFS, therefore parents in our study could be natural, step or foster parents. The LFS on the other hand has information on union status which is crucial for the identification strategy adopted in this paper.

The key outcomes of interest in this paper are the decisions to participate in post-compulsory schooling, and the achievement of five or more GCSEs (at grades A to C). The latter is a requirement to continue in education for many schools, and a necessary condition for admission to university (together with having two or more A-Level passes in examinations taken at age 18). For the former outcome we define a dummy equal to one if the 16 to 18 year old child is either in post compulsory education at present or was in education between 16 and 18 but has left school at the time of interview (based on the age left full time education information in LFS). For the latter we also define a dummy variable based on the achievement of the required GCSE standards[7].

The age range is limited because we need to observe respondents while they are still living at home in order to observe their parent's education levels (respondents are not asked directly about the education of their parents). We only select teenagers where both parents are present to avoid some heterogeneity that would be hard to model[8]. This represents 94% of children aged 16-18. However, the selection becomes more severe with older teenagers - whilst 98% of 16 year old children are observed living with both parents, this proportion in down to 88% for those 18 years old.

We select teenagers where the father is working and reporting his income, where both parents were born after 1933 (and so were not affected by the earlier raising of the school leaving from 14 to 15), and where parents were born in the United Kingdom (or immigrated

[7] 27% of all those aged 16-19 do not report the number of GCSE's received. Of that group 95% report having no qualifications or qualifications below the level of five GCSE A-C grades and were therefore recoded as having failed to achieve this level.

[8] Whilst this may create some selection bias it would be difficult to overcome this in our data. Since separation is more likely for children with unobservably large propensities to leave school early, and it is negatively correlated with parental education and income we might expect to underestimate the effect of income and education on the dependent variables.

at a very young age). We also drop any observations where there is missing data. Full details on the original LFS data and the impact of the selection criteria can be seen in Appendix Table A1.

Table 1 shows some selected statistics for the sub-sample used in our analysis. The staying on rate is 72% for boys and 81% for girls. 69% (73%) of boy (girl) stayers have 5+ GCSE qualifications compared to 28% (34%) of leavers[9]. There are large differences in the parental education and household income levels between those that remain in school compared to those that leave. The differences in paternal union status between those that stayed past 16 and those that left is small. The union earnings gap in the raw data is 10%.

Figures 1a and 1b shows the participation rate in post-compulsory schooling in our final sample broken down by paternal and maternal education. The education of the children appears closely correlated with the education of their parents. The relationships are steep up to a leaving age of 18; whilst having parents with more education than this level does not substantially affect the staying-on probability of children. There are some sizable gaps between the participation of girls and boys from lower educated parents but this gap narrows with parental education. Figures 2a and 2b show similar patterns for the proportion achieving five or more GCSE passing grades.

## 4. Estimates

Our basic model of the impact of parental background on their children is:

[9] Official statistics from the Department for Education and Skills show 67% of boys and 75% of girls in the relevant cohort choosing to stay so our staying-on figures from LFS are a little higher. Some 48% of pupils achieved 5+ GCSE qualifications at grade A*-C in 1998, slightly lower than in our data.

$$(1) \qquad \begin{aligned} PC_c &= \beta_1 S_m + \beta_2 S_p + \beta_3 Y_p + \delta X_h + f(DB_c, DB_m, DB_p) + \varepsilon_c^{pc} \\ G_c &= \alpha_1 S_m + \alpha_2 S_p + \alpha_3 Y_p + \phi X_h + g(DB_c, DB_m, DB_p) + \varepsilon_c^{G} \end{aligned}$$

where the *c*, *m* and *p* subscripts refer to the child, maternal and paternal characteristics within a particular household *h*. The dependent variable $PC_c$ is a dummy variable defining participation in post compulsory education. The dependent variable $G_c$ is the dummy variable for whether or not the child had attained five or more GCSE passing grades. These are estimated as linear probability models, to facilitate the use of instrumental variables, and are functions of parental education measured in years of schooling of both the mother and father ($S_m$ , $S_p$) and parental income $Y_p$ measured by father's real log gross weekly earnings from employment[10]. *DB* refers to date of birth (year and month) so that *f*(.) and *g*(.) controls for trends in paternal, maternal and child education. Finally $X_h$ contains characteristics common to all three members of the family (i.e. year and month of survey dummies as well as region of residence at time of survey).

Table 2 summarizes our estimates of parental education and income on the probability of post-compulsory schooling of the child[11]. Specification (1) only controls for parental years of schooling and suggests positive, if modest, paternal and maternal education effects on the schooling choice of both sexes. The impact of a year of maternal education is an increase in

[10] Note that we use paternal income because the use of household income measures requires the inclusion of female earnings which is potentially much more heavily affected by endogenous labour supply decisions. However its exclusion may also cause a bias if female labour supply is correlated with educational outcomes for children as well as with the variable of interest in the model. We share our inability to resolve this problem with the rest of the literature. If maternal labour supply is uncorrelated with paternal income and if incomes are shared within the household then our estimate of the effect of paternal income is the same as the effect of household income. This is clearly an important problem for future research.

[11] Full results available on request. Similar estimates based on probit models are also available. While multiple observations of closely spaced children in each household are possible their incidence is small (10% of individuals have at least one other sibling in the dataset) and any improvement in standard errors from exploiting the clustering in the data would be marginal.

the probability of post-16 participation of about 4% for boys and 3% for girls – about 1% lower than reported in Chevalier (2004). The impact of paternal education is slightly lower and the effect on boys is larger than for girls. Specification (2) examines the impact of paternal income but excludes the parental education controls. These estimates suggest sizable and significant income elasticities with the effect slightly larger for boys (15%) than for girls (10%). Finally specification (3) includes both education and income controls. The direct effects of education estimated in Specification (1) are reduced slightly in (3) but the income effect is effectively halved in this specification. The estimated income effects here are closely comparable in magnitude to the results in Blanden and Gregg (2004)[12].

As already discussed the impact of parental schooling and income on education outcomes of children may suffer from endogeneity problems. In this paper we identify the effect of parental education on children's education using the exogenous variation in schooling caused by the raising of the minimum school leaving age (RoSLA). Individuals born before September 1957 (or, for Scotland, September 1960) could leave school at 15 while those born after this date had to stay for an extra year of schooling. This policy change creates a discontinuity in the years of education attained by the parents. Figure 3 illustrates this by showing mean years of schooling by birth year and month for the period around the reform date. There is a marked jump in the graph for parents born in September 1957 which coincides with the introduction of the new school leaving age. Individuals affected by the new school leaving age have on average completed half a year more schooling than those born just before the reform. Chevalier *et al.* (2004) show that the effect of this reform was almost entirely confined to the probability of leaving at 15.

[12] See their Tables 6 and 7 in particular.

Parental income is also potentially endogenous either because correlated with unobservable characteristics explaining educational performance, or because the parental education effect is through income. We identify parental income effects from the union status of the father. We assume that union status creates an exogenous change in income which is independent of parenting ability and the child's educational choice. Indeed the raw data, presented in Table 1, showed that children who stay on are just as likely to have unionized fathers as children who do not stay on in education. H.Gregg Lewis (1986), and much subsequent work, demonstrates that wages vary substantially with union status, controlling for observable skills. If union wage premia reflect rents rather than unobserved ability differences it seems plausible to make the identifying assumption, used in this paper; that union status is uncorrelated directly with the parental influence on educational outcomes of the children. Support for the view that unionization picks up differences in *labor market* productivity is mixed. Kevin Murphy and Robert Topel (1990) find that individuals who switch union status experience wage changes that are small relative to the corresponding cross-section wage differences, suggesting that union premia are primarily due to differences in unobserved ability. However Richard Freeman (1994) counters this view, arguing that union switches in panel data are largely spurious so that measurement error biases the union coefficient towards zero in the panel. In any event, we are assuming, as in Shea (2000), that unionized fathers (and their spouses) are not more 'able' parents than nonunion fathers with similar observable skills.

Dummy variables for *RoSLA* and union status are incorporated into the model. We therefore estimate the following system of first stage equations:

$$\begin{aligned} & S_m = \phi_1 RoSLA_m + \phi_2 X_h + r(DB_m) + \mu_m \\ (2) \qquad & S_p = \theta_1 RoSLA_p + \theta_2 X_h + s(DB_p) + \upsilon_p \\ & Y_p = \pi_1 UNION_p + \pi_2 S_p + \pi_3 X_h + t(DB_p) + \omega_p \end{aligned}$$

where the functions *r*(.), *s*(.), and *t*(.) control for smooth trends in school leaving so that the *RoSLA* dummy variables just pick up the effects of the reform. The top portion of Table 3 presents the first stage regressions for paternal and maternal education, and paternal earnings, with the schooling equations estimated separately from the earnings equation. The *RoSLA* variables are significant (although marginally so in the case of the fathers) and positive in both of the parental schooling equations. The paternal earnings equation shows a significant positive union membership wage premium and rates of returns to schooling consistent with existing UK evidence. The bottom half of Table 3 estimates these equations simultaneously using the seemingly unrelated regression method. A number of noticeable changes occur. While they are still significant the *RoSLA* dummy for the father is reduced in this simultaneous estimation. The earnings function shows a similar premium for union status. However the impact of education rises dramatically to almost 25%. This estimate is significantly larger than even those based on using school leaving age reforms as an instrument (Colm Harmon and Ian Walker, 1995). Our interpretation of this estimate is that it is a Local Average Treatment Effect associated with forcing those that left school at 15 to take an extra year of education and the subsequent estimates should be interpreted with that in mind[13].

Table 4 repeats the specification shown in Table 2 but parental educations and paternal income are treated as endogenous[14]. We estimated fitted values for the key variables from the first stage regressions in Table 3. The basic results from Table 2 now change

[13] Estimates of the paternal log income equation that use dummy variables for each year of education (not reported here) show the nonlinearity of returns with individuals leaving school at 16, 17, 18 and 20 having large returns to their educational investment. Returns to education past the age of 21 appear rather flat.

[14] We use the SUR first stage results from Table 3 to correct for the endogeneity. Estimates that use estimates of the income and education equations separately show little difference and are available on request.

considerably. In specification (1) where earnings measures are not included the parental education variable are largely insignificant with only the impact of mother's education on daughters being close to significant. The impact of parental income in the second specification suggests sizable income elasticities with a doubling of paternal income increasing the probability of post-compulsory schooling of sons by 50% and of daughters by 34%. The income effect is more than halved when we reintroduce our parental education variables in the third specification but the impact of parental education is still largely insignificant. If we believe that this strategy, which relies on the exogenous component of union status on income, identifies the permanent effect of income, the results are not dissimilar to Carneiro and Heckman (2003).

It is possible that our results are contaminated by omitted ability. Extensive ability tests were conducted in National Child Development Study, a longitudinal dataset following children born in the UK during a given week of 1958, so we attempt to replicate Tables 2 and 4 using this data to investigate the extent of this problem. The results are reported in Table 5. Note that the NCDS results which exclude ability and the LFS results are also similar despite the fact that in the NCDS data we exploit the 1947 policy change, which raised the minimum school leaving age from 14 to 15, to provide and instrument. The important result to take away from Table 5 is that the NCDS estimates of income and education effects are hardly changed by the inclusion of the test scores. However, it is not possible to simultaneously account for the endogeneity of parental education and income in the NCDS as the union status of the father is not reported.

A major unresolved issue in the literature is the extent to which parental incomes affect outcomes for children through short run considerations, such as relieving credit constraints, as opposed to through long run permanent effects. In the UK, Dearden *et al* (2004) address the issue of credit constraints using the British Cohort Studies of 1970 (and the earlier NCDS) data and investigate the relationship between early school leaving and parental incomes. They find, using least squares, that there are current income effects even when controls for ability and parental background like education are controlled for which they presume capture credit constraints. However, Chevalier and Lanot (2002) report that income effects are dwarfed by permanent family characteristics.

We also are interested in the effect of transitory income shocks when the child is 16. The LFS data that we have used so far has included children aged 16 to 18. From 1998 onwards, LFS has included earnings information in both first wave and the last wave of the survey. Prior to this date earnings information was only recorded for the outgoing rotation. Having one earnings observation allows us to estimate an earnings function and compare the actual wage outcome with predicted to compute the shock when the child is 16, for those observations that contain a child aged 16 in wave 5. Having two observations on earnings allows us to use data on households that contain a child aged 15 in wave 1 (and hence 16 in wave 5 as before) as well as households with a child aged 16 in wave 1 (and hence 17 in wave 5). In the first case we can compute the residual directly while in the second case we can compute the residual when the child is aged 16 by ageing the parents back one year and predicting what they would have earned when the child was 16 in wave 1. Thus we can continue to use data from 1993 onwards, provided the child is 16 in wave 5, and we can supplement this by households from 1998 onwards who have a child aged 16 in wave 1. We can then capture the distinction between permanent and transitory elements of earnings by including both the predicted earnings and its residual.

The results in Table 6 take the specifications in Tables 2 and 4 and further include the residual from the earnings function. Specification (1) in Table 6 assumes that all variables are exogenous and correspond to Table 2; while specification (2) assumes that parental education and permanent income are endogenous and so corresponds to Table 4. The results here suggest that permanent income is insignificant and education effects are significant when they are assumed to be exogenous but there does seem to be some support for the notion that credit rationing may be a factor. However, in specification (2), allowing for the endogeneity of education and permanent income, we find that education is no longer significant but permanent income now appears to be so, while some credit rationing is still in evidence[15].

Table 7 shows the determinants of scholastic success measured by having 5 or more GCSE at grades higher than D. As this measure reflects long-term effort and does not have financial implication, in the way that the decision to remain post compulsory education has, the effect of permanent income is ambiguous whilst transitory income shocks are likely to have only a small impact. It contrasts the importance of education and income in the case where these are treated as exogenous with the unimportance of education when both as treated as endogenous. When education and income are assumed exogenous these results also suggest that parental education levels are important and that, despite their correlation, education remains important when income is also included. When both are regarded as endogenous we find, as with the early school leaving results, education is no longer significant but income is[16]. Table 8 replicates the model presented in Table 6 but now the outcome of interest is our measure of scholastic success: having more than 5 GCSE at grades A to C. In the exogenous model, maternal education matters and so do shocks in earnings, whilst surprisingly, the permanent income has a significant effect for boys only. However, when we used instrumental variable methods we found that the strong effects of parental

[15] The results in Tables 2 and 4 are essentially unchanged if we use this smaller sample. Results are available on request. We also split this sample into 16 year olds, for whom we are forecasting forward, and 17-year olds, who we are forecasting backwards. We lose precision in doing this and the results are not significantly different from those in Table 6.

education became insignificant and permanent income mattered much more, while the effects of shocks to household income at 16 remained important. A similar pattern of results were reflected in the decision to remain in post-compulsory schooling, surprisingly we find that shocks at age 16 are also significant for GCSE performance, maybe because the incentives to perform well at the test, and therefore continues beyond compulsory schooling, is reduced for individuals affected by a negative shock.

## 5. Conclusion

This paper has addressed the intergenerational transmission of education and investigated the extent to which early school leaving (at age 16) may be due to variations in permanent income, parental education levels, or shocks in income at this age. Least squares revealed conventional results - stronger effects of maternal education than paternal, and stronger effects on sons than daughters. We also found that the education effects remained significant even when household income was included. Moreover, when parental education was included we found that permanent income was insignificant while shocks to income at age 16 remained significant. Similar results were found when looking at scholastic achievement rather than the decision to stay-on.

The IV results contrast strongly with some earlier US work: it would appear that education does not have an independent effect when we control for exogenous variation in permanent income, and that permanent income remains important even when we allow for an impact of credit constraints at age 16. The implications for policy are that some policy to

[16] Independent regressions of post-compulsory schooling and obtaining 5 or more GCSE Grades A-C produce the same results as here and there is some gain in precision from exploiting the covariances between the two outcomes, shown in Appendix Table A2.

relieve credit constraints at the minimum school leaving age, say through an Educational Maintenance Allowance (see DfES (2002)), would be effective in promoting higher levels of education. And a policy of increasing permanent income, say through Child Benefit would also be effective. However, any claim that an EMA will benefit *future* generations through direct intergenerational transmission seem doubtful. Similar results apply for GCSE success.

Further work is needed in several areas. Most notably, it would be interesting to estimate the effects on the probability of obtaining 5+ GCSE passes and the probability of staying jointly – that is, allow for the direct effect of scholastic success on staying on. This would allow a more detailed investigation of the transmission mechanism that included the extent to which permanent income mattered for staying on *because* it mattered for GCSE success.

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*Figure 1a Post Compulsory Participation by Parental Education – Fathers*

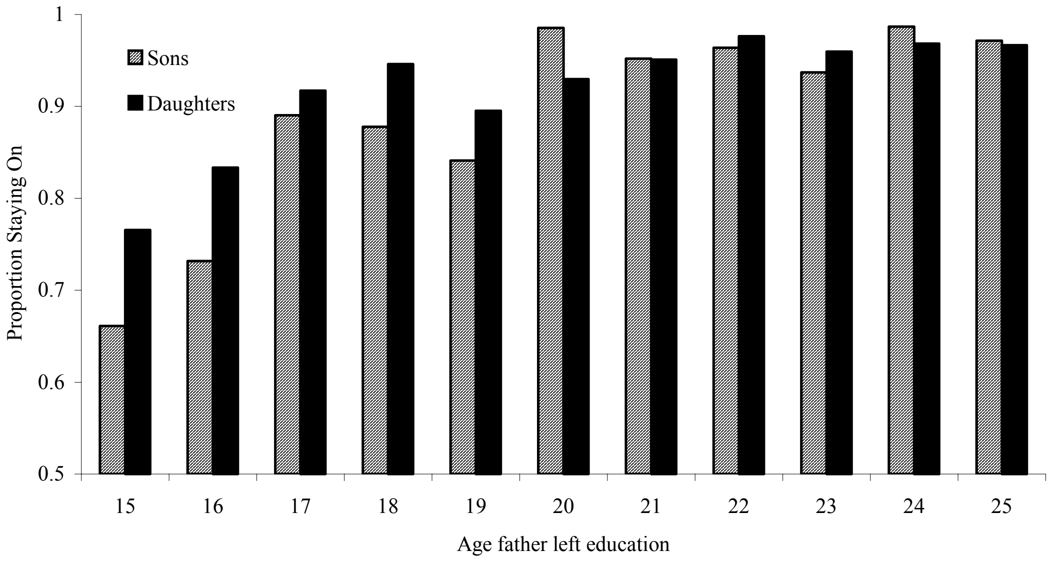


*Figure 1b Post Compulsory Participation by Parental Education – Mothers*

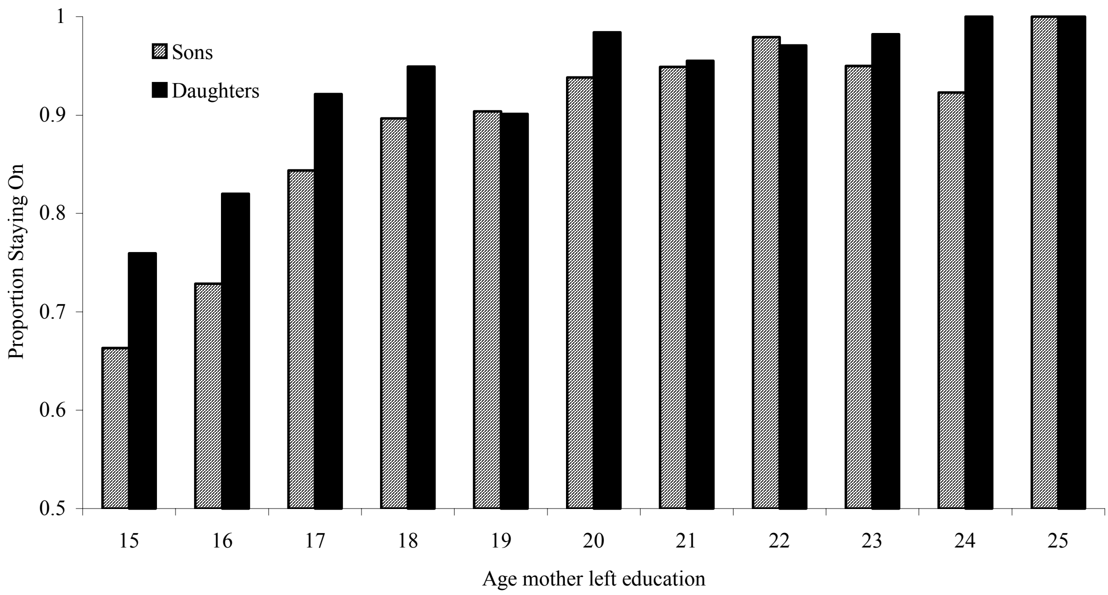

*Figure 2a:* *Exam Success Rate by Paternal Education*

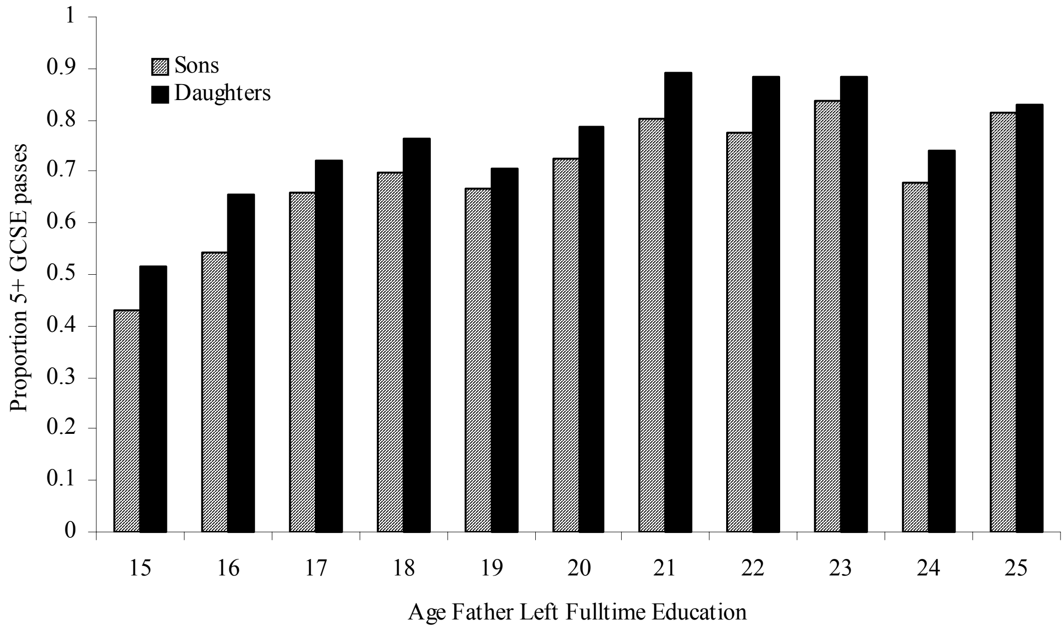


*Figure 2b:* *Exam Success Rate by Maternal Education*

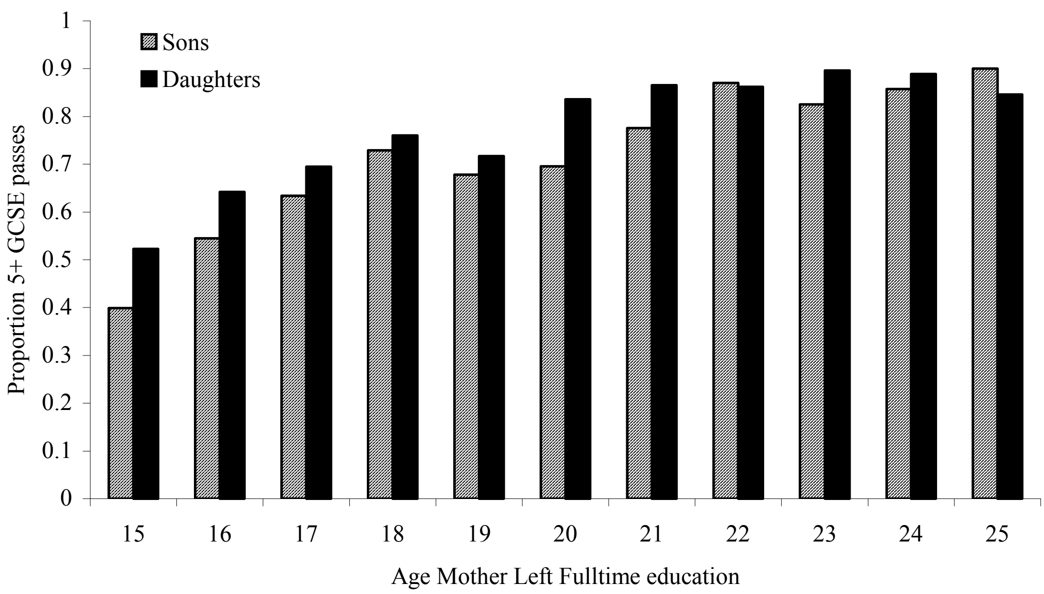

*Figure 3a* *Years of Schooling by Birth Month: Father Born Jan 1956- Dec 1958 (England & Wales)*

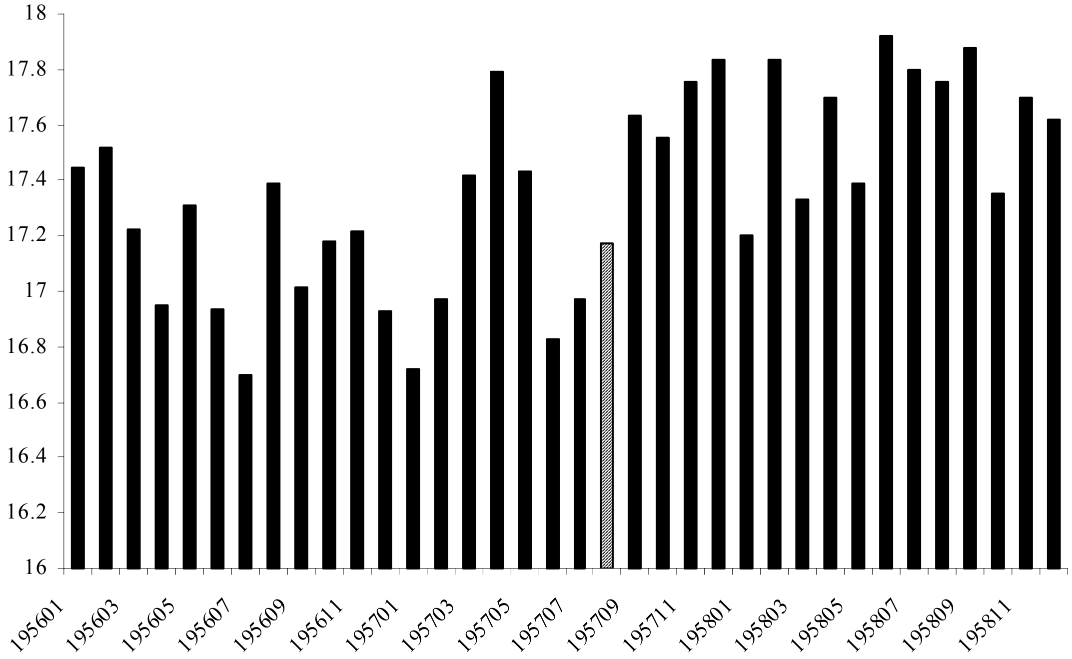


*Figure 3b* *Years of Schooling by Birth Month: Mother Born Jan 1956- Dec 1958 (England & Wales)*

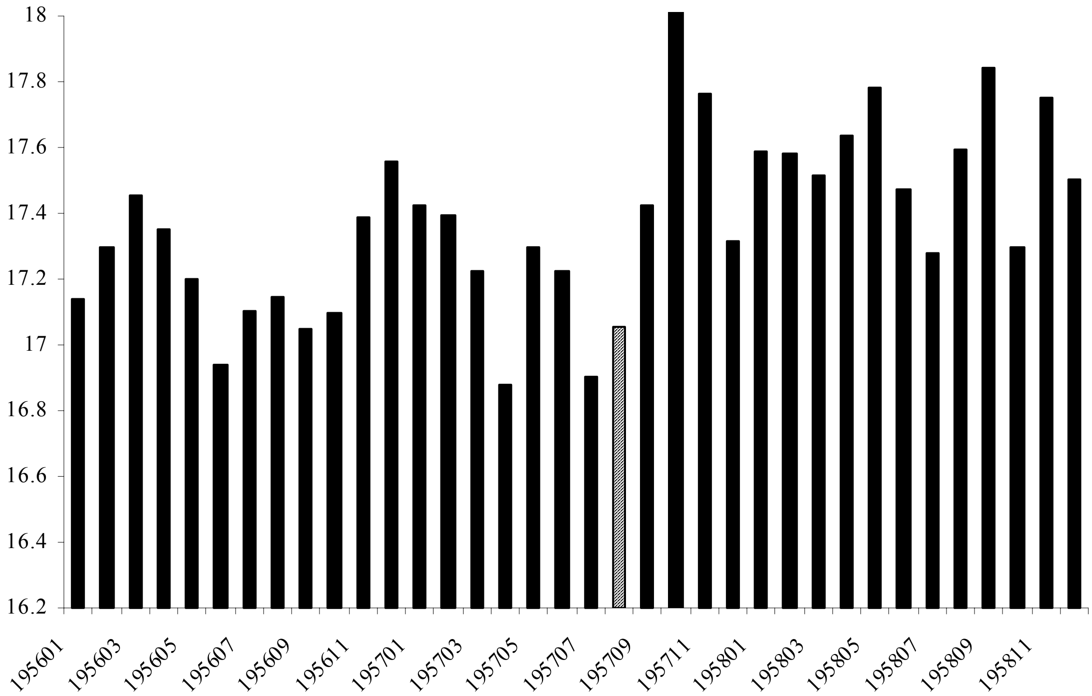

*Table 1 Descriptive Statistics: LFS 1993-2003 – Estimation Sample*

| | Boys | | | Girls | | |
|---|---|---|---|---|---|---|
| | All | Did Not Stay | Stayed | All | Did Not Stay | Stayed: |
| | Mean<br>*Std dev* | Mean<br>*Std Dev* | Mean<br>*Std Dev* | Mean<br>*Std Dev* | Mean<br>*Std Dev* | Mean<br>*Std Dev* |
| *Father's log earnings* | 6.09 | 5.88 | 6.17 | 6.09 | 5.87 | 6.14 |
| | *0.59* | *0.53* | *0.59* | *0.59* | *0.49* | *0.60* |
| *Household log earnings* | 6.39 | 6.17 | 6.48 | 6.40 | 6.17 | 6.46 |
| | *0.56* | *0.52* | *0.55* | *0.57* | *0.50* | *0.57* |
| *Father's education* | 17.07 | 15.88 | 17.53 | 17.00 | 15.85 | 17.26 |
| | *2.61* | *1.44* | *2.81* | *2.54* | *1.48* | *2.66* |
| *Mother's education* | 16.93 | 15.92 | 17.32 | 16.89 | 15.85 | 17.13 |
| | *2.20* | *1.27* | *2.36* | *2.18* | *1.23* | *2.28* |
| *Father's age:* | 46.44 | 45.28 | 46.89 | 46.63 | 45.33 | 46.93 |
| | *4.84* | *4.88* | *4.75* | *5.09* | *5.43* | *4.96* |
| *Mother's age:* | 44.45 | 43.10 | 44.97 | 44.64 | 43.39 | 44.92 |
| | *4.50* | *4.49* | *4.39* | *4.53* | *4.75* | *4.43* |
| *% 5+ O Levels* | 55 | 28 | 69 | 66 | 34 | 73 |
| *Age at time of survey %* | | | | | | |
| *16* | 10 | 9 | 10 | 10 | 11 | 11 |
| *17* | 46 | 47 | 47 | 47 | 48 | 47 |
| *18* | 44 | 43 | 43 | 43 | 42 | 42 |
| *Father's Union Status %* | 39 | 37 | 39 | 40 | 37 | 41 |
| *Father affected by RoSLA %* | 10 | 14 | 9 | 11 | 15 | 10 |
| *Mother affected by RoSLA %* | 18 | 24 | 15 | 16 | 21 | 15 |
| *N* | 4932 | 1366 | 3557 | 4524 | 839 | 3685 |

*Table 2* *Effects of Parental Education and Income on the Probability of Post-Compulsory Schooling of Children*

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls |
| Maternal School | 0.031 | 0.024 | | | 0.030 | 0.022 |
| Leaving age | *0.003* | *0.003* | | | *0.003* | *0.003* |
| Paternal School | 0.027 | 0.017 | | | 0.021 | 0.013 |
| Leaving age | *0.003* | *0.003* | | | *0.003* | *0.003* |
| Paternal Log | | | 0.141 | 0.103 | 0.081 | 0.065 |
| Income | | | *0.011* | *0.01* | *0.012* | *0.010* |
| Observations: | 4923 | 4524 | 4923 | 4524 | 4923 | 4524 |

Data source: LFS 1993-2003. Standard errors in italics. All specifications include year of survey dummies, regional dummies, child's date of birth, and cubics in the date of birth of both parents. Date of birth is a continuous variable measured in months and divided by 100; January 1934 is used as the base and equals zero.

*Table 3* *First Stage Equations*

<table>
<tr><th></th><th colspan="2">(1)</th><th>(2)</th></tr>
<tr><th>Estimated as separate equations</th><th>Paternal Schooling</th><th>Maternal Schooling</th><th>Paternal Earnings</th></tr>
<tr><td>Paternal RoSLA</td><td>0.216</td><td></td><td></td></tr>
<tr><td></td><td>*0.122*</td><td></td><td></td></tr>
<tr><td>Paternal Education</td><td></td><td></td><td>0.081</td></tr>
<tr><td></td><td></td><td></td><td>*0.002*</td></tr>
<tr><td>Paternal Union Status</td><td></td><td></td><td>0.064</td></tr>
<tr><td></td><td></td><td></td><td>*0.012*</td></tr>
<tr><td>Maternal RoSLA</td><td></td><td>0.314</td><td></td></tr>
<tr><td></td><td></td><td>*0.086*</td><td></td></tr>
<tr><td>F test of instruments</td><td>3.15</td><td>13.47</td><td>31.07</td></tr>
<tr><td>*p-value*</td><td>*0.0761*</td><td>*0.0002*</td><td>*0.0000*</td></tr>
<tr><td>F test of instruments</td><td colspan="2">15.15</td><td></td></tr>
<tr><td>*p-value*</td><td colspan="2">*0.0005*</td><td></td></tr>
<tr><th>Estimated simultaneously</th><th>Paternal Schooling</th><th>Maternal Schooling</th><th>Paternal Earnings</th></tr>
<tr><td>Paternal RoSLA</td><td>0.172</td><td></td><td></td></tr>
<tr><td></td><td>*0.114*</td><td></td><td></td></tr>
<tr><td>Paternal Education</td><td></td><td></td><td>0.241</td></tr>
<tr><td></td><td></td><td></td><td>*0.013*</td></tr>
<tr><td>Paternal Union Status</td><td></td><td></td><td>0.048</td></tr>
<tr><td></td><td></td><td></td><td>*0.012*</td></tr>
<tr><td>Maternal RoSLA</td><td></td><td>0.304</td><td></td></tr>
<tr><td></td><td></td><td>*0.085*</td><td></td></tr>
<tr><td>F test of instruments</td><td>2.30</td><td>12.73</td><td>16.40</td></tr>
<tr><td>*p-value*</td><td>*0.1295*</td><td>*0.0004*</td><td>*0.0001*</td></tr>
<tr><td>F test of instruments</td><td colspan="2">13.85</td><td></td></tr>
<tr><td>*p-value*</td><td colspan="2">*0.0010*</td><td></td></tr>
<tr><td>F test of instruments</td><td colspan="3">30.25</td></tr>
<tr><td>*p-value*</td><td colspan="3">*0.0000*</td></tr>
<tr><td>N:</td><td colspan="3">9447</td></tr>
</table>

Data source: LFS 1993-2003. Year of survey (in earnings equation) and regional dummies omitted. All equations estimated simultaneously using SUR. Dates of birth are a continuous variable with months divided by 100 being the unit of measurement with September 1934 being equal to zero.

*Table 4* *Impact of Parental Education and Income on Probability of Post-Compulsory Schooling of Children – IV Estimates*

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls |
| Maternal SLA | -0.052 | 0.135 | | | -0.067 | 0.144 |
| | *0.093* | *0.086* | | | *0.090* | *0.084* |
| Paternal SLA | -0.065 | -0.190 | | | -0.136 | -0.188 |
| | *0.165* | *0.148* | | | *0.160* | *0.146* |
| Paternal Log Earnings: | | | 0.491 | 0.338 | 0.171 | 0.116 |
| | | | *0.028* | *0.026* | *0.010* | *0.009* |
| Observations: | 4923 | 4524 | 4923 | 4524 | 4923 | 4524 |
| Exogeneity test | 3.6 | 1.92 | 209.62 | 113.48 | 4.43 | 4.03 |
| Significance of residuals: | pr=0.16 | pr=0.38 | pr=0.00 | pr=0.00 | pr=0.22 | pr=0.26 |

Data source: LFS 1993-2003. Bootstrapped standard errors in italics. All specifications include year of survey dummies, regional dummies. child's date of birth and cubics in the date of birth of both parents. Dates of birth are a continuous variable with months divided by 100 being the unit of measurement with September 1934 being equal to zero. Exogeneity test is from Smith and Blundell (1986). The residuals from each first stage regression are included in the probit model along with the variables that the first stage equations would have instrumented. Estimation of the model gives rise to a test for the hypothesis that each of the coefficients on the residual series are zero.

*Table 5 Comparison of NCDS and LFS (linear probability model)*

| | Parent Education: Exogenous | | | | | | Parent Education: Endogenous | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | LFS Boys | LFS Girls | NCDS Boys | NCDS Girls | NCDS Boys | NCDS Girls | LFS Boys | LFS Girls | NCDS Boys | NCDS Girls | NCDS Boys | NCDS Girls |
| Paternal SLA | 0.027 | 0.017 | 0.048 | 0.041 | 0.041 | 0.035 | -0.065 | -0.190 | 0.149 | -0.086 | 0.113 | -0.038 |
| | *0.003* | *0.003* | 0.005 | 0.005 | 0.005 | 0.005 | *0.165* | *0.148* | 0.125 | 0.128 | 0.121 | 0.125 |
| Maternal SLA | 0.031 | 0.024 | 0.046 | 0.053 | 0.041 | 0.046 | -0.052 | 0.135 | 0.001 | -0.024 | 0.033 | -0.009 |
| | *0.003* | *0.003* | 0.006 | 0.006 | 0.006 | 0.006 | *0.093* | *0.086* | 0.096 | 0.1 | 0.093 | 0.097 |
| Ability | -- | -- | -- | -- | 0.054 | 0.054 | -- | -- | -- | -- | 0.069 | 0.07 |
| | -- | -- | -- | -- | *0.004* | *0.005* | -- | -- | -- | -- | *0.004* | *0.005* |
| *Observations:* | *4923* | *4524* | *4130* | *3903* | *4130* | *3903* | *4923* | *4524* | *4130* | *3903* | *4130* | *3903* |

t-stats in italics. Data source 1: LFS 1993-2003. Bootstapped standard errors in italics. All specifications include year of survey dummies, regional dummies, interactions of year and region, child's date of birth and cubics in the date of birth of both parents. Dates of births are a continuous variable with months being the unit of measurement and September 1934 being equal to zero. The endogenous model is estimated using fitted values from first-stage equations of parental education. This includes dummy for RoSLA16, cubic of parental date of birth, interaction of RoSLA and parental date of birth and regional dummies. Data source 2: NCDS. All specifications include year of regional dummies and cubics in the date of birth of both parents. Dates of births are a continuous variable with years divided by 100 being the unit of measurement. The endogenous model is estimated using fitted values from first-stage equations of parental education. This includes dummy for RoSLA15, cubic of parental date of birth and regional dummies.

*Table 6* *Impact of Parental Education and Income on Probability of Post-Compulsory Schooling of Children – Estimates from LFS Rotating Panel*

| | (1) Exogenous | | | | (2) Endogenous | | | |
|---|---|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls | Boys | Girls |
| Maternal SLA | 0.019 | 0.011 | 0.019 | 0.011 | 0.111 | -0.016 | 0.140 | -0.023 |
| | *0.007* | *0.007* | *0.007* | *0.007* | *0.159* | *0.156* | *0.163* | *0.159* |
| Paternal SLA | 0.022 | 0.012 | 0.024 | 0.018 | -0.200 | -0.378 | -0.325 | -0.399 |
| | *0.024* | *0.023* | *0.006* | *0.005* | *0.324* | *0.296* | *0.331* | *0.301* |
| Permanent Earnings | 0.015 | 0.072 | | | 0.185 | 0.133 | | |
| | *0.291* | *0.272* | | | *0.025* | *0.024* | | |
| Shock in earnings | 0.073 | 0.061 | 0.073 | 0.060 | 0.076 | 0.063 | -0.037 | -0.021 |
| | *0.024* | *0.023* | *0.024* | *0.023* | *0.024* | *0.023* | *0.019* | *0.017* |
| Observations: | 1127 | 996 | 1127 | 996 | 1127 | 996 | 1127 | 996 |

Data source: LFS 1993-2003 for above results. For those aged 16, father's wages are predicted and a residual calculated using wave 5 income. For those aged 17, only observations from 1998 onwards are used. The residual is calculated using the fitted value and the wave 1 income. Bootstrapped standard errors in italics. All specifications include year of survey dummies, regional dummies, interactions of year and region, child's date of birth and cubics in the date of birth of both parents. Dates of birth are a continuous variable with months divided by 100 being the unit of measurement with September 1934 being equal to zero.

*Table 7* *Effects on the Probability of Obtaining 5+ GCSE*

| All Exogenous | Boys | Girls | Boys | Girls | Boys | Girls |
|---|---|---|---|---|---|---|
| Maternal SLA | 0.034 | 0.025 | | | 0.032 | 0.023 |
| | *0.004* | *0.004* | | | *0.004* | *0.004* |
| Paternal SLA | 0.021 | 0.023 | | | 0.014 | 0.019 |
| | *0.003* | *0.003* | | | *0.003* | *0.003* |
| Paternal Log Earnings: | | | 0.147 | 0.120 | 0.095 | 0.072 |
| | | | *0.012* | *0.012* | *0.012* | *0.012* |
| **All Endogenous** | **Boys** | **Girls** | **Boys** | **Girls** | **Boys** | **Girls** |
| Maternal SLA | -0.119 | -0.042 | | | -0.132 | -0.032 |
| | *0.101* | *0.102* | | | *0.099* | *0.100* |
| Paternal SLA | 0.096 | -0.212 | | | 0.034 | -0.210 |
| | *0.179* | *0.177* | | | *0.176* | *0.174* |
| Paternal Log Earnings | | | 0.433 | 0.391 | 0.150 | 0.139 |
| | | | *0.031* | *0.032* | *0.011* | *0.011* |
| Observations: | 5062 | 4597 | 5062 | 4597 | 5062 | 4597 |
| Exogeneity test: | 3.47<br>pr= 0.18 | 2.94<br>pr=0.23 | 107.93<br>pr=0.00 | 97.91<br>pr=0.00 | 6.51<br>pr=0.09 | 12.21<br>pr=0.01 |

Data source: LFS 1993-2002. Bootstrapped standard errors in italics. Dates of birth are a continuous variable with months divided by 100 being the unit of measurement with September 1934 being equal to zero. Exogeneity test is from Smith and Blundell (1986). The residuals from each first stage are included in the probit model along with the variables that the first stage equations would have instrumented.

*Table 8* *Probability of Child Attaining 5+ GCSE A-C grades and Income Shocks*

| | (1) Exogenous | | | | (2) Endogenous | | | |
|---|---|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls | Boys | Girls |
| Maternal SLA | 0.035 | 0.025 | 0.035 | 0.025 | 0.015 | 0.004 | 0.045 | -0.003 |
| | *0.071* | *0.008* | *0.007* | *0.008* | *0.174* | *0.192* | *0.178* | *0.195* |
| Paternal SLA | -0.041 | 0.039 | 0.016 | 0.024 | 0.314 | -0.187 | 0.177 | -0.217 |
| | *0.027* | *0.028* | *0.006* | *0.006* | *0.354* | *0.364* | *0.363* | *0.371* |
| Permanent Earnings | 0.691 | -0.190 | | | 0.207 | 0.181 | | |
| | *0.315* | *0.333* | | | *0.028* | *0.030* | | |
| Shock in earnings | 0.098 | 0.053 | 0.098 | 0.055 | 0.115 | 0.063 | -0.013 | -0.052 |
| | *0.026* | *0.028* | *0.026* | *0.028* | *0.027* | *0.028* | *0.021* | *0.021* |
| Observations: | 1127 | 996 | 1127 | 996 | 1127 | 996 | 1127 | 996 |

Data source: LFS 1993-2003 for above results. For those aged 16, father's wages are predicted and a residual calculated using wave 5 income. For those aged 17, only observations from 1998 onwards are used. The residual is calculated using the fitted value and the wave 1 income. Bootstapped standard errors in italics. Dates of birth are a continuous variable with months divided by 100 being the unit of measurement with September 1934 being equal to zero.

# Appendix

Table A1 outlines the composition of the final sample used in the main estimates presented in this paper together with the full LFS sample. The key factors to note in moving from a full sample of 26,683 16 to 18 year olds, are the exclusion of single parents (sample reduced to 22,097), non-working fathers (reduced to 18,523), excluding parents born before 1933 (reduced to 18,067), excluding parents born outside of the UK and hence not subject to UK education laws (16,332) and finally the elimination of parents without wage information which generates this final sample.

*Table A1    Descriptive Statistics:  LFS 1993-2003*

| | BOYS<br>All aged<br>16, 17, 18 | Final<br>Sample | GIRLS<br>All aged<br>16, 17, 18 | Final<br>Sample |
|---|---|---|---|---|
| | Mean<br>*Std. Dev.* | Mean:<br>*Std. Dev.* | Mean<br>*Std. Dev* | Mean:<br>*Std. Dev.* |
| *Father's log earnings* | 6.05 | 6.09 | 6.06 | 6.09 |
| | 0.61 | 0.59 | 0.61 | 0.59 |
| *Household log earning:* | 5.99 | 6.39 | 6.00 | 6.40 |
| | 0.89 | 0.56 | 0.89 | 0.57 |
| *Father's education* | 16.98 | 17.07 | 17.01 | 17.00 |
| | 2.60 | 2.61 | 2.62 | 2.54 |
| *Mother's education* | 16.87 | 16.93 | 16.89 | 16.89 |
| | 2.25 | 2.20 | 2.23 | 2.18 |
| *Father's age:* | 46.83 | 46.44 | 46.96 | 46.63 |
| | 6.74 | 4.84 | 6.86 | 5.09 |
| *Mother's age:* | 44.15 | 44.45 | 44.27 | 44.64 |
| | 5.93 | 4.50 | 5.77 | 4.53 |
| | | | | |
| *% Post compulsory schooling* | 67 | 72 | 77 | 81 |
| *% Five or more O Levels* | 48 | 57 | 57 | 66 |
| | | | | |
| *Age at time of survey:* | | | | |
| *16* | 10 | 10 | 10 | 10 |
| *17* | 46 | 47 | 46 | 47 |
| *18* | 44 | 43 | 44 | 43 |
| | | | | |
| *Father's union status* | 20 | 39 | 20 | 40 |
| | | | | |
| *Father min school leaving age* | | | | |
| *14* | 2 | | 2 | |
| *15* | 84 | 90 | 84 | 89 |
| *16* | 13 | 10 | 13 | 11 |
| | | | | |
| *Mother min school leaving age* | | | | |
| *14* | 1 | | 1 | |
| *15* | 76 | 82 | 76 | 84 |
| *16* | 23 | 18 | 23 | 16 |
| *% Already left home:* | 3.85 | | 9 | |
| *% In single parent household:* | 24 | | 28 | |
| *N* | 15,324 | 4923 | 14,359 | 4524 |

*Table A2    Estimated Covariances from Bivariate Probit of Staying in Post Compulsory Education and Obtaining 5+ GCSE Grades at A-C level*

| | Model with parental education only: | | Model with paternal income only: | | Model with paternal income and parental education: | |
|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls |
| Exogenous model: | 0.310 * | 0.278* | 0.333 * | 0.295* | 0.302* | 0.272* |
| Endogenous model: | 0.355 * | 0.3123* | 0.321* | 0.2865* | 0.321* | 0.287* |

* Breusch-Pagan test that the correlation is zero. The null hypothesis is rejected at the 1% level