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Family income and educational attainment : a review of approaches and evidence for Britain

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Family Income and Educational Attainment: A Review of Approaches and Evidence for Britain

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Abstract

It is widely recognised that, on average, children from poorer backgrounds have worse educational outcomes than their better off peers. There is less evidence on how this relationship has changed over time and, indeed, what exactly leads to these inequalities. In this paper we demonstrate that the correlation between family background (as measured by family income) and educational attainment has been rising between children born in the late 1950s and those born two decades later. The remainder of the paper is spent considering the extent to which these associations are due to the causal effects of income rather than the result of other dimensions of family background. We review the approaches taken to answering this question, drawing mainly in the US literature, and then present our own evidence from the UK, discussing the plausible range for the true impact of income on education. Our results indicate that income has a causal relationship with educational attainment.

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1. Introduction

As income inequality rose after 1980, incomes in households with children fell relative to those of other households whilst income inequality within this group grew sharply. The poorest households with children saw virtually no rise in living standards for twenty years (see Figure 1[1] and Gregg, Harkness and Machin, 1999, for more detail).

We know from existing research that children from poorer backgrounds do less well in a number of dimensions than their peers (see for example Gregg and Machin, 2000) and in the UK the simple correlation between low income and poor educational outcomes has been long established (Rowntree, 1901, Glennerster, 1995). In terms of completed education, children from low-income households go on to leave full-time education much earlier, and with fewer formal qualifications than their more affluent counterparts. For example, of children born in 1970, some 26% failed to achieve any O levels or equivalent by the age of 30, whilst 23% went on to get a degree. Among children from the poorest 20% of households at age 16, only 11% went on to get a degree and 41% failed to achieve any O levels. The extent to which the relationship between low income and poor attainment is causal is, however, less clear.

There is recent evidence that the relationship between family incomes and children's outcomes has increased over successive cohorts. Blanden et al. (2002) document that the intergenerational transmission of income has increased for children born in 1970 (British Cohort Survey) compared with those born in 1958 (National Child Development Survey). There is also evidence that the increased persistence is in part a consequence of an increased relationship between family income and educational attainment. Related papers by Blanden and Machin (2004) and Blanden, Gregg and Machin (2003) show increased educational inequalities by income group.

The fundamental question is whether it is money itself that makes the difference to children's lives and opportunities. If the real drivers of educational outcomes are

[1] Gini coefficients for families with children (after adjusting for family size using the McClements scales) are .224 in 1970, .228 in 1980, .320 in 1990, .337 in 1997/98 and .344 in 2002/03 (Institute for Fiscal Studies calculations from the Family Expenditure Survey and Family Resources Survey). It therefore appears that the rise in inequality for families with children was strongest in the 1980s, continued more slowly in the 1990s. Inequality appears to have been steady over the period of the current Labour Government with early increases in equality being offset by more recent reductions perhaps reflecting the impact of the new tax credits after 1999.

innate ability, parental education, parenting styles and other factors that are related to, but not caused by, income then increased income inequality will not matter to children's educational attainment. However there are clearly mechanisms by which income can directly influence attainment such as child care quality, the home environment, social activities, neighbourhoods and schools. If these are important then increasing inequality in family income will translate into inequalities in children's educational outcomes and their life chances. A clearer understanding of these issues is key to appreciating the extent to which goals of equality of opportunity (or meritocracy) can be reconciled with wide income inequalities, and they are essential to evaluating the educational benefits of reducing child poverty.

Evidence from the UK indicates that low income does have an independent effect on children's outcomes after controlling for key aspects of family background and child ability (see Gregg and Machin, 2000 and Hobcraft, 1998). However, to be confident that the effect of income has been accurately isolated requires more than controlling for family background. If unobserved child or family heterogeneity is positively correlated with income, this will generate an upward bias in the relationship between income and child attainment. The difficulty of controlling for this heterogeneity means that the task of separating the influence of income from other aspects of family background is not straightforward.

The latest research from the US uses a variety of different methods of controlling for family background and heterogeneity and finds that family income does have a direct positive effect on educational attainment. However, there is substantial variation in the strength of the identified effect (for example see Mayer, 1997, Houston et al., 2000, Levy and Duncan, 2000, Clark-Kaufman et al. 2003). Our aim in this paper is to review the evidence on the effect of family income on education and to explore British data using the same approaches.

We start by presenting a summary of the findings generated by the analysis undertaken in this paper. Table 1 summarises the results obtained from the different identification strategies we pursue. We group the results by the survey used. The data here is taken from two different sources, the BCS 1970 birth cohort and the British Household Panel Survey, meaning that we are comparing young people who reached 16 in 1986 with those who reached this age in the mid to late 1990s.

We present the marginal effects for a .4 reduction in log income (approximately an income reduction of one third, or £140 a week at the mean in the BHPS[2]) on staying in education on beyond 16 and final educational attainment. The first two columns for each dataset give the marginal effect of ln(income) at age 16 from ordered probit models of qualifications which control for individual and family characteristics. The other specifications use identification strategies based on transitory income variations within the family. Due to the different properties of the two datasets used, each of this type of specification can only be applied to one of the two datasets. Columns (4) and (5) in the first panel provide results from models using the BCS when ability scores at age 10 and income at age 10 are used to control for more of the differences between children. Column (7) in the lower panel reports results from a sibling fixed effect specification for the BHPS and column (8) gives the marginal effects from a specification where post-school income is controlled for as a proxy for permanent income, again using data from the BHPS.

Although this exercise is clearly based on some very different identification strategies (and in some cases, individual estimates are not significant), the results generally tell a consistent story. The strategies which focus on transitory income variations show results which are smaller than those from the models that control only for family characteristics. This is because these strategies rely on short run income variations and probably have greater measurement error. We can therefore think of the first two columns as upper bounds on the true education-income results and the second two columns as lower bounds; for this reason we show the range of estimates in the final column of each panel.

The results from the earlier BCS study indicate that a .4 reduction in log income (a shock of around one third of the level of income) increases the likelihood of a young person not obtaining GCSE A-C equivalents by between 7.1 percentage points and 1.1 percentage points, on average, depending on the methodology used, where all estimates are significant. Effects are of similar magnitude (but opposite sign) when we consider if young people stayed on at school, this is not surprising as age 16 attainment and staying on are obviously intimately related. A one third reduction in income reduces the same sample's probability of obtaining a degree by between 1 and

[2] In 2000 prices.

5.6 percentage points, again all our estimates indicate that the impact of income is statistically significant.

Results from the later BHPS data indicate a narrower range of magnitudes for the impact of income on outcomes at age 16 with a .4 fall in log income leading to an increase in the probability of leaving school without GCSE A-C grades of between 2 and 4 percentage points. The same shock in income leads to a reduction in the probability of obtaining a degree of between 6.7 and 3.3 percentage points. The advantages and disadvantages of all the approaches used here are explained in detail below as we discuss each method in turn.

Overall, the main results of our paper provide consistent evidence of a significant impact of family income on educational attainment in the UK. The results suggest that a one third reduction in income from the mean increases the probability of a child getting no A-C GCSEs by around 3 to 4 percentage points, on average, and reduces the chances of achieving a degree by a similar magnitude[3]. The results which rely less on transitory variations show a clear rise in the impact of family income on degree attainment, unfortunately it is not possible to judge if the causal effect of income on education has changed as our most stringent models cannot be applied consistently across both datasets.

The remainder of the paper discusses the concepts and methods behind the summary enclosed in Table 1. Section 2 describes the identification problem faced and discusses the strategies employed by researchers in the US to discover the true impact of family income on education. Section 3 discusses the data we use here. Section 4 considers the modelling strategies and the results for British data in detail. Conclusions are drawn in Section 5.

2. The Identification of the Impact of Family Income: Existing Literature and Modelling Strategies

[3] To give an alternative idea of the order of magnitude of these effects, the BHPS marginal effect on degree attainment -5.3 given in column (8) translates to an elasticity of degree attainment of .64 with respect to income. The BCS result of -2.9 given in column (5) indicates an elasticity of degree attainment of .40 with respect to income.

The relationship between family income and education

There are a large number of possible routes by which the children of low income families do less well at school; some of these are causal and others are non-causal. It is the impact of the causal factors that we seek to identify.

Non-causal relationships are circumstances that lead to low attainment that are linked to, but not caused by, low family income. Low income families contain adults with characteristics that may leave the children more prone to low educational achievement. Such characteristics would include low parental education or other less easily observed adult heterogeneity, which leads to lower home-based child development. Examples of this are: poorer innate ability; a lower emphasis on educational achievement in parenting; or a reduced ability to translate parenting time into educational development. Also in this category would be a shock leading to both low attainment and low income, such as a family break-up. In all these scenarios it is not low income itself that causes reduced attainment. A further mechanism emphasised in the child development literature is that financial problems increase family conflict and parental stress reducing the ability for parents to engage in effective parenting that improves educational outcomes.

The economic literature on the causal relationship between income and educational attainment has a strong emphasis on direct financial investments in children's human capital, (Becker and Tomes, 1986). The underlying theory is of utility maximisation over spending on investments in education, consumption and other investments, where the three alternatives are strictly substitutes. While there are clearly some direct investments that parents can make in their children's development (including money for fees and maintenance in higher education) this seems less relevant at early ages. During childhood a large portion of how income influences attainment is likely to come through as the co-production of education alongside consumption or other investments. Examples of this are the provision of a good home environment through books, toys and outings (Burgess et al, 2004 show these to be important for a cohort in Avon). Here the books and toys are purchased for current consumption as well as educational benefits. Equally the housing decision, while certainly influenced by school quality, has other benefits including the investment potential of the house itself.

The identification problem that we face can be stated more formally. Family income at a point in time, Y_{it} is positively correlated with a set of omitted variables for family characteristics that influence child attainment, A_{it} meaning that the $\operatorname{cov}(Y_{it}, A_{it}) > 0$. Consequently if we estimate an equation for the child's human capital attainment of the form:

$$H_{it} = \beta Y_{it} + v_{it} \qquad \textbf{(1)}$$

Then the estimated β will be biased upwards. The first step to overcoming this problem is to introduce a set of family characteristics in an attempt to parameterise A_{it} and relieve the omitted variable problem. It is, however, impossible to guarantee that a comprehensive representation of A_{it} has been achieved since the family's underlying propensity to produce low attainment among its children will contain a mixture of observable attributes X_{it} and unobservable attributes Z_i.

$$A_{it} = \delta X_{it} + Z_i \qquad \textbf{(2)}$$

Gregg and Machin (1999) attempt to parameterise A_{it} to discover if there is an independent effect of living in financial distress at ages 7 to 16 conditional on a wide set of family and child characteristics. In effect they estimate:

$$H_{it} = \beta Y_{it} + \gamma X_{it} + v_{it} \qquad \textbf{(3)}$$

But the omitted vector Z_i is still in the error term and will continue to lead to an upward bias in β under the assumption that $\operatorname{cov}(Z_i, A_{it}) > 0$.

A further difficulty with this approach is that it might be tempting to control for characteristics that are actually pathways between income and attainment; this can lead to over-parameterisation and the under-estimation of true income effects. For example, family break up will lead to lower incomes for lone parent families with children. How much of any adverse relationship between lone parenthood and attainment is mediated through income effects is far from clear. It is thus not easy to ascertain which variables should be included in X_{it} in equation (3).

Alternative Strategies and Previous Literature

Disentangling income effects from unobserved family or child heterogeneity requires some ingenuity and a careful statement of econometric models. To our knowledge, three approaches have been most widely used in the context of educational attainment (Blow et al. 2004, provide a comprehensive literature review for this area). The majority of these strategies exploit variations in incomes within families rather than longer term income differences which may have larger effects.

i) Experimental Trials of Policy Interventions

In the US there have been a number of welfare to work programmes undertaken under experimental conditions and evidence from these is perhaps the cleanest and clearest available. The relevant population in the trial is divided into a treated group who participate in the programme and an untreated control group. This random allocation ensures that treatment is not correlated with family or child characteristics. Such trials became more common from 1996 when the Clinton administration allowed states to administer their own welfare to work programmes. Under these programmes the treated receive an exogenously driven change in family income which is not received by the untreated programme families. In all cases the financial payment is attached to other conditions, but they can be nonetheless informative.

Some welfare reforms that focus on getting lone mothers into employment (and off welfare) have included child outcomes in their evaluations. The most recent and comprehensive assessment of the effects on children is contained in Clark-Kauffman et al. (2003); we report the key results from this paper in Table 2.

This analysis pools the data from a large number of random assignment welfare experiments and compares the treated and control groups. These programmes were aiming to raise employment and earnings of welfare dependent families in the US; some also offered additional cash assistance when mothers moved into work. Column 1 reports the evidence of programme effects on child educational attainment test scores for those programmes with cash assistance, so that the observed changes in child outcomes reflect the combined effect of work and income changes. The income gains among the treated participants in these earning-supplement programmes where modest at $1500-$2000 (£1000 to £1300) per year over the untreated participants for two to three years. Column 2 shows the impact on child test scores for programmes based on raising maternal employment without additional in-work financial support (job search counselling or education based approaches). These had positive employment and earnings effects but had very modest effects on family incomes, as benefit payments are withdrawn. The differences between columns 1 and 2 reflect the impact of the extra effect of income as both types of programmes led to similar employment and earnings changes. The size of the attainment gains for pre-schoolers is modest, but statistically significant, raising attainment by 8 percent of a standard deviation. At older ages there are no differences across the programmes except that at ages 12 to 15 there are large, but poorly identified, negative results associated with the programmes without earnings supplement.

Another interesting set of experimental programmes are the evaluations of the Moving To Opportunity (MTO) programme (see Goering and Feins, 2003 for a full summary). In these programmes families from poor neighbourhoods are randomly selected into one of three populations. The first is given financial help with rents conditional upon moving to a more affluent neighbourhood.[4] A second group received rent support but could move to any neighbourhood. The third group received no help in moving from the deprived neighbourhood. So the treatment is that families receive financial support to meet higher housing costs associated with moving to more affluent (and high rent) areas, provided they make the move.

These studies provide crucial evidence of how higher incomes might influence children's educational attainment by enabling families move to live in affluent areas with better schools and peer groups. Importantly these moves were not associated with increases in employment or earnings among adults, so the effects observed are operating purely through neighbourhood change. Table 3 reports details of child outcomes across studies from two MTO sites in Boston and Baltimore, as reported in Goering and Feins (2003). The results suggest that moving neighbourhood (which is hand-in-hand with changing school and peer group for most children) is associated with marked improvements in behavioural problems and school test scores and, for older children, a reduction in the number of arrests for violent crime.

[4] The rent assistance was in the form of Section 8 housing vouchers. This is a rent assistance programme in the US which has some parallels with Housing Benefit in the UK but is more restricted in its availability.

These studies provide powerful evidence for income effects on child outcomes, however, the specific samples involved and the enforced link between income increases and other changes (employment or moving neighbourhood) may mean the results do not generalise to the population at large.

In the UK random-assignment experiments are very rare; however there is one policy evaluation that is relevant to our context. The Education Maintenance Allowance was piloted in 15 Local Authorities in the UK from 1999 onward. It offered youths from low income families a weekly financial payment (of up to £40 a week), for up to two years, provided they stayed on in full-time education after compulsory schooling ends at age 16. Non-attendance leads to payment withdrawal and there were bonuses for course completion. The EMA is therefore a means tested cash payment conditional on educational enrolment. Ashworth et al. (2003) report evidence of the impact of this programme where eligible and ineligible populations in the pilot areas are compared (through propensity matching techniques) to similar people in 11 areas not taking part in the programme. The evaluation suggests school/college enrolment increased by 6 percentage points for those eligible for full subsidy. Additionally there was no increase in drop out rates and staying on rates into a second year also improved. The EMA is being implemented nationally at the start of the 2004-2005 academic year.

ii) Sibling Studies

Our model of attainment and income is:

$$H_{it} = \beta Y_{it} + \gamma X_{it} + v_{it} \,. \qquad \textbf{(4)}$$

The principle behind sibling fixed effects models is to assume that the error v_{it} is composed of two elements $v_{it} = Z_f + e_{it}$ where Z_f is a family fixed effect which is equal across siblings and e_{it} is uncorrelated with Y_{it}. The sibling fixed effects model is estimated on deviations of H_{it} and Y_{it} from the family mean; this eliminates the impact of Z_f and generates unbiased estimates of β.

The variation in family incomes experienced by the siblings comes from the age gap between them. This means that siblings will be affected by income in different periods because other children have either not been born yet or have already left home. This approach uses income variations within a family rather than differences across families. Sibling studies require an income history for the family including some periods of differing income experience.

The central problems for sibling studies is that siblings will often be close in age and experience very similar income patterns for most of their childhood. Also, only families with two or more children can be considered. Further, measurement error in data reporting will lead to attenuation bias. An advantage of this approach is that income shocks in the family will be experienced by siblings at different ages; this can provide evidence on when in childhood income matters most. Levy and Duncan (2000) is a recent sibling study using the Panel Study of Income Dynamics. They find that parental income matters most for young children but that the magnitudes of the effects are small with a 2.7 fold increase in family income through childhood adding three quarters of a year to completed years of schooling by age 20. These are extremely small impacts compared with others found in the literature.

(iii) Post Educational Income

Mayer (1997) also considers whether transitory income fluctuations have an impact on child educational outcomes. Leaving aside the question of measurement error, income at a point in time can be thought of as composed of transitory and permanent components.

$$Y_{it} = Y_i^{perm} + Y_{it}^{trans} \quad \textbf{(5)}$$

Therefore in a regression of the relationship between income and education the income parameter will be a weighted-average of the coefficients that would be obtained if measures of permanent and transitory income could be entered into the model separately. The key assumption here is that the permanent component will be correlated with unobservable characteristics Z_i and that this leads to bias. The transient income component is assumed to be uncorrelated with fixed family characteristics $\mathrm{cov}(Y_i^{trans}, Z_i) = 0$, therefore the coefficient on Y_i^{trans} would be the true relationship between income and education. The strategy is to use a measure of family income after the child has completed the normal education process as a control for the permanent component of income. The estimation equation thus looks like

$$H_{it} = \beta_1 Y_{it} + \beta_2 Y_{it+1} + u_{it} \qquad \textbf{(6)}$$

Any correlation between the later income measure and attainment is not causal and its inclusion can be seen as an attempt to condition out the permanent income component. If Y_{it+1} was perfectly correlated with Y_i^{perm} β_1 would be the relationship of interest between education and transitory income at age 16. However this will not be the case as Y_{it+1} also contains a transitory component, meaning some residual bias will remain in this approach. This can be reduced by averaging over several years of later income.

Mayer uses a range of child outcomes and test scores as dependent variables. The addition of post-childhood family income reduces the estimated impact of a 10% increase in income on years of schooling from 1.86 to 1.68 (after conditioning on observed family fixed characteristics). The conditioning on later income makes only a minor difference but is more important for other outcomes such as teenage motherhood and dropping out of school. A concern with this approach is that income changes between the two periods considered reflect family shocks that influence child attainment independently. In addition, lifecycle models predict that anticipated income changes will affect behaviour in all periods if families can smooth consumption.

The US literature consistently shows that family income does influence a child's educational attainment. However, as studies consider a range of outcomes and sometimes refer to specific population groups or ages of children, it is difficult to form a clear picture of the results across identification strategies. The identified causal income effects appear small in the sibling study but much larger in the work by Mayer described above, and in the experimental studies. The majority of the identification strategies focus on income variations that are unrelated to fixed family characteristics. Naturally such variations tend to be small. They would not show the impact of changes in income sufficient to change residential neighbourhood, for example, which is so important in determining peer group and school quality. In this regard the MTO experiments are particularly revealing; showing that neighbourhood makes a substantial difference to child educational and behavioural outcomes.

British work on uncovering the causal impact of income on education is less well-developed than the research on US data. As has already been mentioned Gregg and Machin (1999) try to isolate the impact of financial disadvantage by carefully controlling for confounding factors. Ermisch et al. (2002) use the sibling methods described here on BHPS data to try and uncover the effect of parental employment (rather than income) on educational attainment. In the following sections we explore the extent to which the data available in the UK enables us to identify the impact of income on educational attainment.

3. Data

The primary data sources used in this paper are the British Cohort Study (BCS) and the British Household Panel Study (BHPS). These data have different strengths and weaknesses but both offer the possibility of examining the relationship between family income variations and the child's educational outcomes.

The BCS takes all children born in the same week in April 1970 and follows them at intervals until, to date, age 30. This data is particularly useful for our purposes as it contains substantial information on family background and child characteristics collected at ages 5, 10 and 16. Information on school leaving decisions and final educational attainments are available from the age 16 and 30 surveys.

The BCS contains two measures of family income, at ages 10 and 16. Having two measures allows us more scope to control for permanent income differences. However the income measures are not problem-free. In order to encourage response all questions ask parents to identify the income band they fall into rather than attempting to obtain a precise income measure. By considering similar families in the Family Expenditure Survey we find the median income within each band and set incomes to this value. Another important set of data we use is from tests administered to the children at age 10. We use the quintile attained in the Shortened Edinburgh Reading Test and Young's friendly maths test. Measures of test scores and income at age 10 enable us to consider the relationship between the change in attainments between ages

10 and 16 and the change in incomes, removing the permanent effect of characteristics correlated with income.

The first results we report below also include data from the National Child Development Study (NCDS). This data was the forerunner to the BCS, considering children born in a week in March 1958. The information available in the NCDS is very similar to the BCS, however, income information is only obtained once, at age 16; this limits the extent to which this data can be used to identify the causal impact of income.

The BHPS is a household panel study which started in 1991 with 10,000 households. Households have been sampled annually since and the most recent data available is from 2001. The advantage of this data is that we have annual measures of income for all households and information on educational qualifications and enrolment for all children and young people within the sample households. These aspects enable us to pursue two identification strategies which are not possible using the BCS data. First, parental household income continues to be observed after young people have left home, enabling us to pursue Mayer's idea that later incomes will not be directly correlated with outcomes aside from their correlation with permanent income. Second, the inclusion of all children enables us to use sibling variation to eliminate family fixed effects as in Levy and Duncan (2001) and Ermisch and Francesconi (2002).

The main disadvantage of the BHPS is its small effective sample size; there are few children of a particular age in each wave. This is particularly limiting for those estimations which require the observation of young people in several waves. We attempt to maximise samples by using information from other waves whenever possible. Nonetheless this limitation of the data sometimes affects the specifications we can estimate. In addition the BHPS contains no information on test scores; the attainment information available is age left full time education and educational qualifications obtained.

4. Results

We begin this section by estimating some basic models of how income and educational attainment are related in three time periods using data from the NCDS, BCS and BHPS as described previously. We add controls for a series of family characteristics in order to show the extent to which the patterns are modified by the most straight-forward attempts to reduce the bias on the income effect. In the final estimation we control for a group of variables for which it is less clear whether they independently drive attainment or just mediate the relationship between income and education. These provide the strictest test of this type of model.

The models estimated are ordered probits of the highest qualification obtained related to family income at age 16. Highest qualification has four categories: degree or equivalent; A levels or equivalent; GCSEs at A-C, CSEs at Grade 1, O levels or equivalent; and below this level. This measure is obtained from the age 33 data in the NCDS, the age 30 data in the BCS and at age 23 (or 22 if age 23 unavailable) in the BHPS. There is an obvious non-comparability here as highest qualification is taken at a much earlier age in the BHPS than in the other samples. This could potentially bias the income effects in the BHPS upwards if poorer young people take longer to reach their final qualification level, unfortunately this is unavoidable given the nature of the BHPS data. Reassuringly, results presented in Blanden and Machin (2004) show similar patterns for degree attainment when we consider graduation by age 23 in all the datasets.

The first panel in Table 4 presents results showing the association between family income and highest qualification with no controls added. In order to ease interpretation marginal effects are calculated to show the change in probability of obtaining the lowest and highest qualification category in response to an income shock, we show the impact on probabilities of a constant one third reduction in income (.4 log points). To give an idea of the magnitude of the shock; in these data a reduction of 33% from the mean is equivalent to moving from the median to around the 20th percentile.

The first key point is that the raw relationship between family income and education has strengthened considerably between the NCDS and BCS cohorts. The marginal effect of reducing income by .4 log points for the NCDS is to increase the chance of obtaining less than a GCSE A-C equivalent by 8.1 percentage points and reduces the probability of obtaining a degree by 4 percentage points. In the BCS a one third shock increases the chance of poor qualifications by 9.6 percentage points and reduces the probability of obtaining a degree by 7.4 points.

In the BHPS the implied marginal effect of obtaining no qualifications falls relative to the BCS, back to 5.7 points, whilst the change in the probability of going on to do a degree strengthens to 8.7 points. There is therefore prima facie evidence of an opening up in opportunities at lower levels of qualifications at the same time as there was a strengthening of the education and income relationship at the higher education level. These findings are consistent with those reported in Blanden, Gregg and Machin (2003) and Blanden and Machin (2004) who find a reversal in the inequality in staying on after the compulsory leaving age between students from richer and poorer families but no evidence of a similar fall in higher education inequalities.

This approach shows the impact of the changing influence of income on attainment but not the added effect of the increasing income inequality that was demonstrated in Figure 1. In our data the standard deviation of log income rises from .402 in the NCDS, to .481 in the BCS and .522 in the BHPS. If we estimate marginal effects that take into account the increase in inequality over the period (by estimating the impact of a standard deviation shock) marginal effects rise from -4 points in the NCDS, to -8.6 points in the BCS and -11.1 points in the BHPS for the probability of doing a degree. These compare to marginal effects of -4, -7.4 and -8.7 for the constant .4 shock. This shows how increased inequality magnifies the impact of the changes in the strength of the relationship between education and income; in this example growing inequality increases the marginal effects by 1.2 percentage points in the BCS and 2.4 points in the BHPS.

In the remainder of the panels in this Table we add controls for family background. The second panel adds controls for the child's sex, family size, parental age and race; this makes no substantive difference to the estimated income relationships. The third panel adds controls for parental education[5]. This is one of the main observable characteristics that we might think is correlated with both child's attainment and parental income. The implied income relationships are reduced by around a fifth to a

[5] To avoid complications in cases where the father may not be a member of the household we control for mother's education except in cases where the mother is missing when we use father's education instead.

quarter. So even with these controls included the income effects remain strong and the patterns over time are unchanged. For example, in the top panel a fixed .4 log point drop in income led to a 4 point fall in the probability of obtaining a degree in the NCDS compared with 7.4 points in the BCS and 8.7 points in the BHPS. Controlling for parental education these marginal effects are 2.8, 5.6 and 6.7 points respectively. Once again we can also allow the results to reflect rising income equality by applying a standard deviation shift in income, the increase is again magnified to 2.8 (NCDS), 6.5 (BCS) and 8.6 (BHPS).

The final panel in Table 4 conditions on a larger set of additional controls for which it is less clear whether or not they should be included. As noted in the earlier discussion we would wish to condition on factors that influence both income and child attainment but not factors that have an impact on attainment that is mediated though income. Including a control for living in a lone-parent household, for example, might wrongly attribute an effect to lone parenthood whereas it is actually the low income associated with lone parenthood that is the key issue. The factors introduced here are region, social class and lone parenthood. Once again adding these controls leads to a reduction in all the estimated relationships but here the interpretation is more problematic.

Table 4 gives a clear picture of how the relationship between incomes at age 16 and highest qualification has changed over time. However it would not be justifiable to say we have uncovered changes in the causal relationships. In the next section we use techniques borrowed from the US literature to explore the extent to which these impacts are causal. Unfortunately the different strengths and weaknesses of the datasets mean that no approach is applicable to the NCDS or to more than one of the other datasets, even so, we believe the exercise is informative.

Income Variation and Child Attainment

The results presented above use income at age 16 as the variable of interest, this includes permanent income through childhood, transient income at age 16 and any measurement error.

$$Y_{i16} = Y_i^{perm} + Y_{i16}^{trans} + \varepsilon_{i16} \qquad \textbf{(7)}$$

As discussed earlier the main concern with estimating income effects is that the permanent income component is correlated with fixed family characteristics that influence attainment. All the strategies pursued below attempt to control for permanent income effects and identify only the relationship between transient income and education. This has three very important implications. First, the implied time over which the income effect would have been applied to the family is much smaller. Transitory income, by definition, only applies for a small number of years whereas permanent income has an influence throughout childhood. Second, any measurement error will become an increasingly important proportion of the variance of income once the permanent income component is removed. This will bias our estimated effects downward. Third, by focusing on transitory income, income effects that require sustained differences to make an impact (e.g. moving to a better neighbourhood) cannot be captured. For these reasons the estimated effects of income on attainment will be lower than the effect of permanent income differences.

Child Development Trajectories – Controlling for Age 10 Ability and Income

Our first attempt to control for unobserved heterogeneity uses the BCS data on income and ability tests at age 10 to control for differences between children up to this age. This shows how income changes after age 10 influence educational attainment after this age.

Assuming that the measures are good, controlling for ability scores at age 10 accounts for the underlying differences in ability between children, one aspect of heterogeneity which we believe will be correlated with income (our Z_i). However, it may still be the case that unobserved heterogeneity impacts the achievement trajectory post-16 in a way that is correlated with permanent income. In order to account for this we also control for income at age 10. This will control for the part of income at age 16 which is correlated with income at age 16, in other words the permanent component that may be correlated with Z_i.

Controlling for income and ability at age 10 is similar to adopting a model in which the change in attainment after age 10 is regressed on the change in income between age 10 and 16, meaning that we eliminate Z_i in the style of a fixed effect model. As it is likely that family background will affect development after age 10 as well as before we also include a set of observable family characteristics; however to the extent that there are aspects of family background correlated with income that are still unobserved the coefficients will remain upward biased.

We control for income at age 10 in two different ways. First we include age 10 incomes as a RHS variable. So the estimating equation is:

$$H_{iadult} = \alpha H_{i10} + \beta_1 Y_{i16} + \beta_2 Y_{i10} + \gamma X_{i16} + v_{it} \qquad \textbf{(8)}$$

Here the estimated β on income at 16 is purged of any cross correlation with income at 10, family characteristics and test scores at age 10. To the extent that age 10 income does not provide a perfect measure of permanent income (because of its own transitory component) this approach will remain upward biased.

The second approach is to use the change in income between ages 10 and 16 as a direct measure of transitory income:

$$H_{iadult} = \alpha H_{i10} + \beta_3 (Y_{i16} - Y_{i10}) + \gamma X_{i16} + v_{it} \qquad \textbf{(9)}$$

where $Y_{i16} - Y_{i10} = (Y_{i16}^{trans} + \varepsilon_{i16}) - (Y_{i10}^{trans} + \varepsilon_{i10})$. **(10)**

Note that in addition to netting out permanent income in this approach we introduce more measurement error; also, the coefficient β_3 on $(Y_{i16} - Y_{i10})$ will be reduced if transitory income at age 10 has an impact on attainment after age 10. As a result we view the strict first difference model as producing a coefficient that is downward biased. In summary, the two approaches used will provide plausible upper and lower bounds on the true effect.

Results for this methodology are given in Table 5. We estimate these specifications with two dependent variables: highest qualification at age 30 and staying on beyond the school leaving age. Again we report the marginal effects of the standard .40 log point reduction in income. The first column reports results without conditioning on age 10 income for comparison purposes, this is equivalent adding controls for test scores to specification C in Table 4. The second and third columns provide upper and lower bounds for the effect of age 16 income conditioning on age 10 income. The upper panel gives the results for highest qualification achieved while the lower panel looks at staying on at 16. In column 2 a one third reduction in income increases the propensity to achieve no A-C GCSEs by just over 3 percentage points and has a similar magnitude reduction in the propensity to get a degree. The strict first difference in column 3, which represents a lower bound estimate, is around 1 percentage point for each of these attainment levels[6]. The lower panel finds similar but opposite signed effects for staying on as those for low qualification attainment.

Sibling Fixed Effect Estimation

The results of the sibling models for the BHPS are given in Table 6. Child-specific controls are included to account for any characteristics which change across children and may be correlated with income changes and attainment. In our models we control for the gender of the child, the number of children in the household and the work status of both parents when income is observed. Due to the shortness of the panel we do not observe family incomes and full education histories for all siblings so the results for different qualification levels use slightly different samples of individuals.

The top panel of Table 6 shows results for a linear probability model of staying on and income at age 16. We show the impact for this model of only focusing on a sample of siblings (column 2) rather than including single child families (column 1), which makes no difference here. In the third column we remove bias in the same way as we did in Table 4 by adding controls for parental education; this reduces the income effect somewhat to –3.5 points for a one third reduction in income. The final column estimates the sibling fixed effect models. Even in this stringent test of causality the coefficient and marginal effect do not fall very much further; with a marginal effect of –3.1 percentage points. If we believe the sibling fixed effects models has removed the upward bias, this indicates that the true effect is around one quarter smaller than that obtained in a model with only basic controls and more than a tenth smaller than a model when parental education is also added. The disadvantage of the fixed effect approach is that the fall in the signal to noise ratio leads to a rise in the standard errors leaving the income coefficient significant at only the 10% level.

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[6] The first difference model imposes the restriction that $\beta_3 = \beta_1 - \beta_2$ where β_3 is the coefficient on the change in income and β_1 and β_2 are the coefficients on income at age 16 and income at age 10 when these are entered separately, statistical tests show that the data does not reject this restriction in any of our models.

The lower panel uses degree attainment as the dependent variable. In this case the explanatory variable used is income at age 18. This seems appropriate as this is the age when university enrolment choices are made for most young people. In addition, choosing a measure of income obtained closer to the outcome maximises the available sample size. For this model the income effects change more between the full sample and sibling sample[7]. The marginal effect of a one third drop in income on the probability of degree attainment is –6.2 with basic controls, -5.1 with controls for parental education and –3.3 in the sibling fixed effects model. These reductions indicate a proportionately larger bias in these estimates compared with those for staying on. Although the magnitude of the sibling fixed effects coefficient remains quite large the standard errors are high for this model and the coefficient and marginal effect do not approach significance.

In summary, BHPS models that include sibling fixed effects give rather smaller estimates than those that include only controls for family characteristics and in neither case considered here are the estimates statistically significant at conventional levels. The recurring problem of sibling estimation is that by relying on differences in income within families for identification enhances the effect of measurement error and reduces the variance in income and outcomes that can be used to identify effects. This difficulty is clearly observed here as the standard errors rise sharply when sibling fixed effects are included. Note also, that by comparing siblings who are close in age the length of time over which income can be having an effect is sharply reduced. These estimates are, therefore, more like point in time impacts of income at 16 on post-16 education decisions rather than reflecting more cumulative effects of long-run income differences. Due to these problems, the results of the sibling models can be considered as a lower bound on the true income effect.

Post childhood income as a proxy for permanent family characteristics

Table 7 explores Mayer's approach using data from the BHPS. We add post-school income to our specification in an attempt to proxy permanent income. In choosing the age at which post-childhood income is observed we balance two factors. First,

[7] This may be because to be included here siblings need to be fairly close together in age as income and outcome variables are further apart in time. It seems plausible that income constraints on university attendance are more important for parents who are contemplating sending two or more children to university in quick succession.

income must be taken at an age sufficiently removed from the educational process to satisfy the assumption that it will not be correlated directly with educational outcomes. However, sample size considerations also play a role; the further away the income is from the outcome of interest the smaller the sample size will be. We show results conditioning on just income at age 20 and on an average of income between 18 and 21 (if age 21 income is not observed, we average up to age 20). Ideally we would wish to use income at later ages but sample sizes become prohibitively small if this is attempted.

The first panel reports the results from a probit model of highest qualification achieved. In both models the impact of controlling for age 20 incomes is limited while average income from age 18-21 reduces the impact of income somewhat more as we would expect. In the model with no additional controls for income the marginal effect of a .4 log point reduction in income is a 4.4 percentage point rise in the probability of obtaining GCSE A-C qualifications and a 6.9 fall in the probability of obtaining a degree, these effects reduce to 3.9 and 6.1 respectively when average income between 18 and 21 is controlled for.

The lower panel uses staying on at 16 as the dependent variable and finds similar results (with reverse sign) as for gaining poor or low qualifications. The evidence suggests that in the 1990s family income has a larger effect on the probability of obtaining a degree than it does on staying on or obtaining at least some good secondary qualifications. These relative magnitudes are very much consistent with the results in Table 4 and those found in related work by Blanden, Gregg and Machin (2003). In summary, this approach reduces the observed income effect rather less than the methods that control for earlier ability and income and the sibling fixed effect approach.

6. Conclusions

This paper presents evidence on two questions, first whether there is a causal impact of family income on educational attainment, and second whether the association between family income and attainment is increasing. Our evidence clearly indicates that there exist some important relationships between family income and educational attainment in the UK and that these relationships have been strengthening through time. In addition, as far as the data allows, we have also found evidence that income does have a causal impact on educational outcomes. Evidence on changes over time in the causal relationship between income and attainment is inconclusive as it is impossible to estimate any of our most stringent models in a consistent way across the datasets.

Although not all of our estimates are statistically significant, the consensus from our different approaches suggests that family income does affect educational outcomes. The models which attempt to net out permanent income (and therefore provide a lower bound estimate) suggest that a one third reduction in family income from the mean, which is about £140 a week or £7000 a year, reduces the chances of securing a degree by around 4 percentage points. The estimates based on only conditioning out family characteristics are somewhat larger. Effects of a similar magnitude are found for the other outcomes we consider, obtaining no GCSE A-C grades and staying on at school.

A natural question to ask is whether this is a large impact or not, especially as £7000 sounds like a large shock compared with the 4 percentage point change it leads to. In order to bring this into focus we can use our models to predict the difference between the probability of degree attainment for young people at the 90th percentile of the income distribution compared to the 10th percentile. The model which controls for age 10 ability and income using BCS data predicts that the probability of degree attainment is .18 at the 10th percentile compared with .27 at the 90th. Results from the BHPS which control for post-education income give a larger estimate of the income effect. This, combined with greater income inequality, means that the predicted gap is larger with the probability of degree attainment at the 10th percentile .21 and .42 at the 90th. These results demonstrate that when combined with substantial income inequality the impact of income has important implications for educational inequality.

From a policy point of view £7000 a year is a large amount of money, far beyond the income redistribution that is likely to be achieved by taxes and benefits. However, a broader attempt to reduce the inequalities in the distribution of work and wages offers hope of more substantial progress. In addition, direct interventions to raise attainment of those from poorer families, through early years' education and extra resources for schools can be cost effective if they are well targeted. Recent Government policy seems to be making a concerted effort to address these issues with financial redistribution to families and education investments ranging from pre-school programmes through to the EMA. It is for future research to discover if these attempts are successful in creating greater equality of opportunity.

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Figure 1: Changes Over Time in the Distribution of Real Income For Families With Children, UK

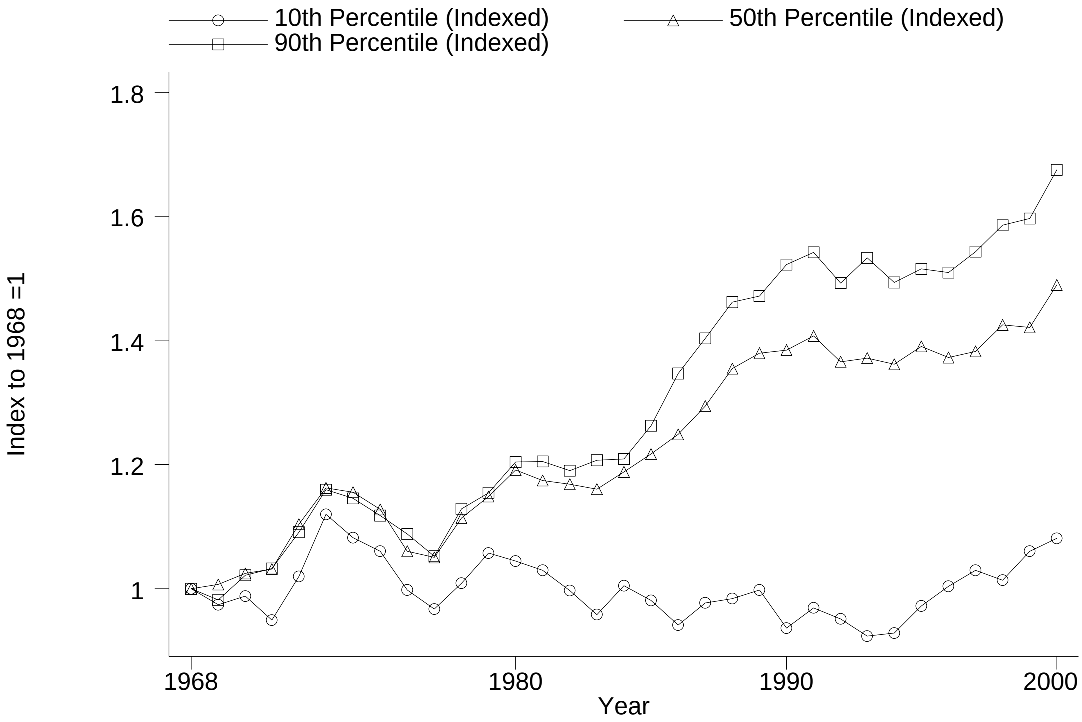

Table 1: Summary of Marginal Effects of Family Income at Age 16 on Educational Attainment

	(1)	(2)	(3)	(4)	(5)
BCS – 1970 Birth Cohort					
	From Table 4	From Table 4	From Table 5	From Table 5	
	Controlling for parental education and basic controls	(1) Plus Controls for Social Class, Region and Lone-Parenthood	(1) Plus Controlling for age 10 income and ability at age 10	Change in income between ages 10 and 16 as the explanatory variable, controls as (3)	Range of Estimates
No A-C GCSEs or equiv.	.071 (.005)	.062 (.005)	.034 (.007)	.011 (.005)	.011 to .071
Staying on			-.039 (.009)	-.009 (.008)	-.039 to -.009
Degree Attainment	-.056 (.003)	-.049 (.004)	-.029 (.005)	-.010 (.005)	-.010 to -.056
BHPS – Sample born 1974-1979					
	(5)	(6)	(7)	(8)	(9)
	From Table 4	From Table 4	From Table 6	From Table 7	
	Controlling for parental education and basic controls	(5) Plus Controls for Social Class, Region and Lone-Parenthood	Sibling Fixed Effects Model	Controlling for average post-school income, plus controls from (5)	Range of Estimates
No A-C GCSEs or equiv.	.043 (.008)	.022 (.008)		.039 (.008)	.022 to .043
Staying on			-.031 (.020)	-.039 (.016)	-.031 to -.039
Degree Attainment	-.067 (.010)	-.038 (.012)	-.033 (.063)	-.053 (.013)	-.033 to -.067

Notes

1. The O level was the GCSE A-C equivalent when the BCS cohort left school, however some may have obtained GCSEs in more recent years.
2. Marginal effects are calculated as the average impact of a .4 reduction in log income, which is approximately a third reduction in the level of income.
3. The results shown in this table can be found in later in the paper in the tables stated, where they are highlighted in bold. More details about the estimates can be found in the notes and text that accompany these tables.
4. All models control additionally for ‘basic controls’ which are the child’s sex, ethnicity and dummies for number of siblings in the household and for parent’s age group.

Table 2: The Impact on Test Scores of Welfare to Work, Results from Experimental Evaluations

	Treatment Effects on Test Scores	
	(1)	(2)
Treatment Age	Earnings-Supplement Programmes	All Other Programmes
Age 0-2	0.082**	-0.016
	(0.034)	(0.074)
Age 3-5	0.080**	0.035
	(0.026)	(0.026)
Age 6-8	-0.025	-0.015
	(0.033)	(0.070)
Age 9-11	-0.043	-0.046
	(0.040)	(0.082)
Age 12-15	-0.039	-0.167
	(0.060)	(0.102)
R-squared	.0346	.0409
Observations	18641	11982

Notes:

1. Source: Clark-Kauffman et al. (2003), Table 1.
2. The dependent variable is a within-study standardised measure of attainment, the precise nature of this varies by the study and in some cases more than one measure is provided.
3. Controls are included in all models for follow-up length, prior earnings, prior earnings squared, prior AFDC receipt, prior years of employment, high school degree, teen parent, marital status, number of children and age of youngest child.
4. Dummies are also added for the type of achievement measure and the study that the data is taken from.
5. * Statistically significant at the 10-percent level.
6. ** Statistically significant at the 5-percent level.

Table 3: Impacts of the Moving to Opportunity Programme

Mean outcomes by sample group				
Type of Impact	Population	(1) MTO Treatment group	(2) Section 8 comparison	(3) Control group
Differences in Child Behaviour	Children aged 6 to 15			
Behavioural Problems – boys		23.6**	21.3**	32.6
Behavioural problems – girls		17.0	14.3	19.3
Number of Arrests for violent crimes per 100 juveniles	Children aged 11 to 16	1.4**	1.6*	3.0
Differences in School Tests	Children aged 5 to 12			
Elementary School CTBS percentile reading scores		32.47**	31.52**	25.13
Elementary School CTBS percentile math scores		36.25**	30.25	28.77

Note:
1. Differences in child behaviour come from the Boston study of Katz, Kling and Leibman (2001), Table 6.
2. Violent crime results are from Baltimore study of Ludwig, Duncan and Hirschfield (2001) which are summarised in Table 6.3 of Goering and Liebman (2003)
3. The test scores results are from the Baltimore study of Ludwig, Ladd and Duncan (2001) and are summarised in Table 6.1 of Goering and Liebman (2003).
4. Behavioural problems are measured as the fraction of the seven types of behaviour that the child shows. For example, if he shows one type this score will be .142 (1/7)
5. CBTS is the Comprehensive Test of Basic Skills.
6. ** indicates that the treated group mean differs from the control group mean at a 5-percent level * shows that this difference is significant at the 10-percent level.

Table 4: Relationship between Highest Qualification and Income at Age 16

Marginal Effects of Log Income at Age 16 from Ordered Probit Models			
A. No Controls	(1) NCDS 1958	(2) BCS 1970	(3) BHPS 1975-80
No A-C GCSEs	.081	.096	.057
	(.005)	(.004)	(.008)
Degree Attainment	-.040	-.074	-.087
	(.002)	(.003)	(.009)
B. Specification A Plus Basic Controls	(4) NCDS 1958	(5) BCS 1970	(6) BHPS 1975-80
No A-C GCSEs	.082	.095	.054
	(.006)	(.004)	(.008)
Degree Attainment	-.040	-.073	-.082
	(.002)	(.003)	(.009)
C. Specification B Plus Parent's Education	(7) NCDS 1958	(8) BCS 1970	(9) BHPS 1975-80
No A-C GCSEs	.057	**.071**	**.043**
	(.005)	**(.005)**	**(.008)**
Degree Attainment	-.028	**-.056**	**-.067**
	(.002)	**(.003)**	**(.010)**
D. Specification C Plus Region, Social Class and Lone Parent Status	(10) NCDS 1958	(11) BCS 1970	(12) BHPS 1975-80
No A-C GCSEs	.039	**.062**	**.022**
	(.005)	**(.005)**	**(.008)**
Degree Attainment	-.020	**-.049**	**-.038**
	(.003)	**(.004)**	**(.012)**
Sample size	7138	4708	580

Notes:
1. The dependent variable is highest qualification which is coded as 1 "No qualifications, or qualifications below GCSE A-C or equivalent" 2 "GCSE A-C or equivalent" 3 "A level of equivalent" 4 "Degree or equivalent". For the NCDS this variable is measured at age 33, for the BCS at age 30 and for the BCS age 23 (or 22 if this is not available).
2. All family income data is in 2001 prices.
3. In all the BHPS specifications controls are added for year of birth and the wave in which the child is 16.
4. Basic controls are the child's sex, ethnicity, dummies for number of siblings in the household and controls for parents' age group.
5. Marginal effects are calculated as the average impact of a .4 reduction in log income, which is approximately a third reduction in the level of income. This is £98 at the mean for the NCDS, £96 at the mean for the BCS and £140 at the mean for the BHPS.
6. Standard errors are in parenthesis, these are obtained from a bootstrap procedure for marginal effects.

Table 5: Relationship between Educational Attainment and Income at 16: Controlling for Earlier Income and Test Scores in the BCS

Ordered Probit Models of Highest Qualification			
	(1) Log Income at age 16	(2) Log Income at age 16 with Log Income at 10 controlled	(3) Change in Log Income Between 16 and 10
Coefficient	.420 (.044)	.312 (.050)	.098 (.046)
Coefficient on Age 10 Income		.240 (.057)	
Marginal Effect on No A-C GCSEs	.046 (.006)	**.034** **(.007)**	**.011** **(.006)**
Marginal Effect on Degree Attainment	-.039 (.004)	**-.029** **(.005)**	**-.010** **(.005)**
Test score controls	YES	YES	YES
Probit Models of Staying On			
	(4) Log Income at age 16	(5) Log Income at age 16 with income at 10 controlled	(6) Change in Log Income Between 16 and 10
Coefficient	.438 (.053)	.299 (.062)	.065 (.056)
Coefficient on Age 10 Income		.307 (.070)	
Marginal Effect on Staying On	-.056 (.007)	**-.039** **(.008)**	**-.009** **(.008)**
Test score controls	YES	YES	YES

Notes:
1. All models include basic controls, maths and reading score quintile and parent's education.
2. Marginal effects are calculated as the average impact of a .4 reduction in log income, which is approximately a one third reduction in the level of income.

Table 6: Relationship between Educational Attainment and Income at 16: Controlling for Sibling Fixed Effects, BHPS

Linear Probability Model of Staying On in Post-Compulsory Education				
	(1) No Fixed Effects – Full Sample	(2) No Fixed Effects - Sibling Sample	(3) (2) Plus Controls for Parent's Education	(4) Sibling Fixed Effects
Marginal Effect on Staying On	-.040 (.010)	-.042 (.012)	-.035 (.012)	**-.031** **(.020)**
Sample Size	1613	984	984	984
Linear Probability Model of Obtaining a Degree by Age 23/22 on Family Income at age 18				
	(5) No Fixed Effects – Full Sample	(6) No Fixed Effects - Sibling Sample	(7) (6) Plus Controls for Parent's Education	(8) Sibling Fixed Effects
Marginal Effect on Degree Attainment	-.034 (.012)	-.062 (.021)	-.051 (.020)	**-.033** **(.063)**
Sample Size	709	309	309	309

Notes
1. Basic controls are once again added to these models as these are child specific at age 16.
2. Addition controls are added for parents work status in the year that income is observed as this may be correlated with differential income and performance between siblings.
3. The definition of a sibling is an individual in the sample who shares the same parental identifier. This is defined as the mother and father's combined identifiers when both these are listed or the lone parent's identifier where only one is listed. A wider sibling sample can be generated by matching just one parent; however this raises complications about how long children have been co-resident.
4. Marginal effects are calculated as the average impact of a .4 reduction in log income, which is approximately a third reduction in the level of income.

Table 7: Relationship between Educational Attainment and Income at 16: Controlling for Permanent Income Proxied by Later Income, BHPS

Ordered Probit Model of Highest Qualification by Age 23/22			
	(1) Log Income at age 16	(2) Log Income at age 16 with Log Income at 20 controlled	(3) Log Income at 16 with average of 18-21 income controlled
Marginal Effect on No A-C GCSEs	.044 (.007)	.041 (.007)	**.039 (.008)**
Marginal Effect on Degree Attainment	-.069 (.009)	-.065 (.011)	**-.061 (.012)**
Sample Size	540	540	540
Probit Model of Staying On			
	(4) Log Income at age 16	(5) Log Income at age 16 with Log Income at 20 controlled	(6) Log Income at 16 with average of 18-21 income controlled
Marginal Effect on Staying On	-.050 (.013)	-.047 (.014)	**-.039 (.016)**
Sample Size	856	856	856

Notes:
1. All models include basic controls and parent's education.
2. Marginal effects are calculated as the average impact of a .4 reduction in log income, which is approximately a third reduction in the level of income.