Preliminary: Comments Welcome

Education and Family Income

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Abstract

This paper considers links between education and family income using British data. Complex conceptual and modelling questions underpin the education-income relationship and we attempt to build up statistical models that are suited to dealing with these difficulties. We begin by presenting estimated education-income relationships from repeated cross-section data and then move on to consider more detailed models based on rich data from two British birth cohorts (one born in 1958, the other in 1970). All our models uncover a significant education-income correlation. This emerges in models where we use changes to the UK tax system as a quasi-experiment to provide exogenous variations in income that differentially benefited families at different points in the income distribution. It also emerges when we control for aspects of unobserved heterogeneity linked to childhood experiences and family background in the cohort data. Finally, when the same models are estimated across cohorts we uncover an increased sensitivity of education to family income in the later cohort. This reveals that the principal beneficiaries of the education expansion were children from richer families. Therefore a key feature of the expansion in education seen over the period we study was an increase in educational inequality linked to family income.

Keywords: Education; Family Income; Tax Changes; Education Sequences.
JEL Classification: I2.

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1. Introduction

Whether family income is a key factor determining educational attainment is a critical policy question. It matters for questions to do with equality of opportunity, for questions of child welfare and for broader questions of fairness in society. Yet, despite the existence of a large body of work on the role of income, we lack real insight into the extent to which income matters, and further if this has altered through time. Part of the reason for this is the emphasis of the academic work on detailed measurement questions. Another part is because the question is so closely linked to whether government should subsidise the education of children from lower income backgrounds.

All this has become even more relevant today, given sharp increases in income inequality, and rises in educational attainment and participation in post-compulsory education, seen in several countries in the last twenty years or so. In this paper we study what has happened in the UK where income inequality has risen since the late 1970s, and where the incomes of households with children progressively fell relative to households without children (Goodman, Johnson and Webb, 1997). Indeed, the poorest households with children saw virtually no rise in living standards for the twenty or so years since 1979 (Gregg, Harkness and Machin, 1999).

Existing research tends to show that children from poorer backgrounds do less well in a number of dimensions than the rest of society (see, for example, Gregg and Machin, 1999, 2000). However, the extent to which this follows from a causal relationship from low incomes to adverse outturns is less clear. The fundamental question is whether it is money itself that makes the difference to children's lives and opportunities, or whether it is actually other factors that are correlated with income that drive the observed relationships. If income matters then increasing inequality of family incomes will translate into inequalities in children's educational outcomes.

However, if the key determinants of educational outcomes are factors like innate ability, parental education and parenting styles then increased income inequality should not matter for children's educational attainment.

Recent US research from the US uses a variety of different ways of controlling for family background and heterogeneity and generally finds that family income does have a direct positive effect on educational attainment. However, there is substantial variation in both the strength of the estimated income effects and on when and how income impacts on children.[1] In the UK the simple correlation between worse education attainment and low income has been long established. More recently evidence has emerged 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. Unobserved child or family heterogeneity may well remain and, if correlated with income, can generate a bias in the education-income relationship. The task of separating the influence of income from family background is therefore not straightforward. In this paper we adopt two different empirical strategies to try and circumvent these problems. First we look at the relationship between education and family income in repeated cross-section data from the Family Expenditure Survey between 1979 and 2000, a period of rising income inequality, and also one where various government tax changes altered family income levels. We use these tax changes as instruments for family income in our child education regressions. Our second approach looks in more detail at changes in the education-income relationship through time using data from two British birth cohorts, the 1958 National Child Development Study (NCDS) and the 1970 British

Cohort Study (BCS). In these data sources, born twelve years apart, we are able to follow cohort members over time and can pay more attention to the sequence of education, thereby allowing for differential income effects at different levels of educational attainment. These surveys contain detailed childhood information that permit us to estimate education-income relationships that better purge the problems of unobserved heterogeneity as compared to other data sources that do not contain such rich information.[2] Moreover, we can use these to say something about the extent to which education-income correlations may have altered through time.[3]

The remainder of the paper is organised as follows. Section 2 presents some descriptive statistics on what has happened to educational attainment and to family income in the UK over time. Section 3 takes some time to discuss issues to do with identification of the impact of family income on children's education. In Section 4 we present our findings based on the analysis of FES repeated cross-section data. Section 5 then considers comparability of estimated education-income links from the cohort data vis-à-vis the FES and presents cross-cohort comparisons of basic education-income models. Section 6 then presents findings from the sequential education models. Section 7 concludes.

2. Descriptive Material

In this section of the paper we describe the key patterns of change in our two main variables of interest, educational attainment and family income, with particular reference to how the changes relate to our questions of interest.

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[1] See, amongst others, Mayer (1997), Duncan and Chase-Lansdale (2000), Levy and Duncan (2001), Cameron and Heckman (1998, 2001) and Acemoglu and Pischke (2001).

[2] A similar argument is made in Dearden, Ferri and Meghir (2002) who use the NCDS data to look at links between wages and school quality. Their modelling includes a detailed set of controls which they argue means their estimated models are rather like those used from applying matching techniques (as in Rosenbaum and Rubin, 1983).

Changes in Education

Figure 1 shows the rapid expansion of education participation seen in Britain in recent years. The Figure shows the DfES higher education age participation index since 1960 and shows the proportion staying on after the compulsory school leaving age from Family Expenditure Survey data since the late 1970s.

Education participation was at low levels at the start of the 1960s with around 6 percent of the 18 to 19 year old age cohort participating in higher education. This rose to around 14 percent by the mid 1970s, before dropping back a little in the late 1970s. Most of the 1980s saw small increases in higher education participation but the expansion from the late 1980s thereafter was very rapid indeed. By the year 2000 participation reached one in three.

The timing of the rapid increase seems in line with the reform of the age 16 examinations system that took place in 1988 with the introduction of the General Certificate of Secondary Education (GCSE). In that year the GCSE became the public examination taken by pupils at age 16+, and it represented something of a departure from the previous O level system (see Gipps and Stobart, 1997). Since introduction a higher proportion of the age group takes GCSE's than was the case with the previous 16+ exams and there is (an often substantial) coursework assessment. Moreover the aims of the exam moved away from separating children into high and low education streams, so as to move away from norm-referenced exams where relative performance most matters. In the GCSE system it was argued that the use of criterion-referenced assessment could get everyone (at least in theory) achieving the top grade.

That the examination system change stimulated a rise in education participation seems to be confirmed by the very sharp rise in staying on rates that

[3] To date not much research has used the birth cohorts to draw cross-time comparisons. An exception is the study of how intergenerational mobility has altered over time by Blanden, Gregg, Goodman and Machin (2002).

occurred from the late 1980s. Figure 1 confirms that staying on after the compulsory school leaving age had been a feature of the 1980s, with a rise from 36 percent of 17/18 year olds in 1979, up to 44 percent by 1988. But after this the pace of change accelerates as the 1990s sees a step change, with the staying on rate rising to 73 percent by 2001.

Changes in Income

Our interest in this paper is how education is linked to parental income and so it is interesting to consider how changes in income for families with children over the same period as the rapid rise in education participation. Figure 2 shows the gap in average log (equivalised) real income between families with and without children. In 1968 average log real income was around 23 percent lower in families with children, and this gap widens to around 30 percent lower by 1980. The gap clearly displays cyclical tendencies as it narrows again in the early 1980s before rising to just beneath 30 percent in the early 1990s. The late 1990s sees something of a climb back, especially in the in last couple of years.

This Figure does however not reveal the sharp rise in the dispersion of income amongst families with children. Figure 3 shows a very sharp increase in income inequality. The Figure shows the evolution over time of the 10th, 50th and 90th percentiles of the log real income distribution where each percentile is indexed to 1 in 1968. As such it shows income growth at each of the percentiles. After not much change in the 1970s the Figure shows the by now familiar pattern of flat real income growth at the 10th percentile for most of the post 1979 period. Only right at the end does the 10th percentile income start to grow in real terms. On the other hand there is significant growth at the median (of over 40 percent) and very substantial growth of around (of over 75 percent) at the 90th percentile.

This pattern of rising income inequality makes it clear that families at the top of the income distribution made much greater gains over time than those at the bottom of the distribution. We are interested in considering whether these income trends are important for rising education participation. In the next section of the paper we therefore consider how education and income may be linked. We place particular emphasis on the (sometimes difficult) modelling questions inherent in this.

3. Existing Literature and Modelling Strategy

Routes by Which Income Impacts on Education

There are many routes by which children from low income families can end up doing less well in the education system. These can be broadly grouped into two groups, causal and non-causal. The non-causal relationships occur when factors that are correlated with low income result in low levels of education. For example, we might think that low income families have characteristics that leave children more prone to low educational achievement. Such characteristics include innate (genetic) ability, low parental education and other less easily observed measures of adult heterogeneity which lead to lower home based child development. This may also produce a lower emphasis on educational achievement in parenting or a reduced ability to translate parenting time into educational development. A further example would be a shock that leads to both low attainment and low income, such as family break-up. In all these scenarios it is not low income in itself that causes reduced attainment.

Causal effects of income on educational attainment can be direct and indirect. Direct routes may occur through reduced investments in educational development outside the school (i.e. resources for high quality childcare, after school coaching, educational materials in the home, money for trips to zoos, days out, holidays etc.). Later on in the educational process the direct effects of low income are even more obvious, as poorer parents may lack the resources to fund their children through further and higher education. Indirect routes include purchase of housing in a good neighbourhood that leads to a better peer group or access to a better school. Gibbons and Machin (2001), for example, highlight that parents seem prepared to pay a lot more for housing located near to better achieving primary schools. Another mechanism is that financial problems increase family conflict and parental stress levels. This in turn reduces the ability to engage in parenting which is effective in helping children do well at school. Clearly we are interested in the extent to which one can uncover a causal relationship running from income to education.

Isolating the Causal Effect of Income

The key problem in identifying income effects on education is separating the effect of fixed characteristics and shocks which impact on attainment of children from the effect of income. Below we set out an illustrative model to set out the issues involved. Let H_{it} denote a child i's educational attainment at time t. This will be determined by previous financial based investments made by the child's parents FI_{it}, and previous non-financial investments made by the parents NFI_{it}. If we think of the effectiveness of these investments as ϕ and γ respectively and add a serially uncorrelated error term u_{it} we have

$$H_{it} = \phi\sum_{0}^{t} FI_{it} + \gamma\sum_{o}^{t} NFI_{it} + u_{it}$$

Financial investments at any point in time are a function of family income

$$FI_{it} = \varphi Y_{it} + v_{it}$$

and we can think of non-financial investments are driven by fixed background family characteristics A_i and family related shocks ΔA_{it} so that

$$NFI_{it} = \lambda A_i + \rho \Delta A_{it}$$

However, it seems almost certain that family background A_i influences income directly and this is currently not allowed for.[4] Hence if we estimated:

$$H_{it} = \beta \sum_{o}^{t} Y_{it} + v_{it}$$

To the extent that $Cov(Y_{it}, A_i) \neq 0$ then the estimated β will be biased by the omission of A_i and this bias will, in all probability, be upwards. One thing we can do (and we do this in our empirical work below) is to introduce a set of family characteristics in an attempt to parameterise A_i. However, the difficulty that emerges here is that A_i will contain a mixture of observable attributes X_i and unobservable attributes Z_i so that $A_i = X_i + Z_i$. Gregg and Machin (1999) have followed this route and they parameterise A_i to gain insight into whether a wide set of family and child characteristics create substantial upward bias on the coefficient on a term measuring financial distress in the family from age 7 to 16. In effect they estimate:

$$H_{it} = \beta \sum_{o}^{t} Y_{it} + \gamma X_i + v_{it}$$

But the omitted Z_i is now rolled up into the error term so that $v_{it} = Z_i + \varepsilon_{it}$ where ε_{it} is a white noise error. So the concern surrounding the possibility of an upwardly biased β remains (though now it is due to unobserved heterogeneity).

[4] Indeed it may also be that the effectiveness parameters ϕ and γ are related to the A_i as well.

Previous Literature

Disentangling income effects from unobserved family or child heterogeneity requires some ingenuity. To our knowledge, six broad approaches have been used in the context of educational attainment:

(i) Random Assignment Trials of Interventions – In the US there have been a number of welfare to work programmes undertaken under experimental conditions and (subject to their rather specific nature) 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 comparison group. The random allocation should ensure that treatment is not correlated with family or child characteristics. Such trials have been common and varied since 1996 when the Clinton administration allowed states to administer their own welfare to work programmes. So under these programmes the treated receive an exogenously driven change in family income.

The main focus of these programmes is on getting lone mothers into employment (and off welfare caseloads) but some evaluations have also considered child outcomes (although frequently only for children under 5). National Evaluation of Welfare-to-work Strategies (2000) provides a summary analysis of many US programmes and Duncan and Chase-Lansdale (2000) build on this report. Programmes based on raising maternal employment without offering additional in-work financial support (job search counselling or education based approaches) seemed to have modest effects on family incomes and rarely had significant effects on child outcomes. But programmes involving additional financial support saw significant advances in child development for the treated groups relative to the controls. However, the specific samples involved, with the focus always upon low income lone mothers, make it very likely that the results are not generalisable.

(ii) Cohort Grouping – Acemoglu and Pischke (2001) use a cohort based approach and assume first that unobserved heterogeneity is constant across cohorts and second that the relationship between this heterogeneity and position in the income distribution is also constant. But cohorts do differ in the relative income applying at each point in the income distribution because of the rise in inequality experienced in the US over the time period studied. Therefore if we observe cohorts with greater income inequality also experiencing greater educational inequality they argue that this is likely to be due to income effects rather than family heterogeneity. They find that a 10 percent increase in family income is associated with a 1.4 percent increase in the probability of attending a four-year college (undergraduate degree).

(iii) Sibling Studies – In this method unobserved family heterogeneity is assumed to be common to all children in the same family. The variation in family incomes experienced by the siblings comes from the age gap between them. So older children will exclusively experience a period of family income prior to their sibling's birth and the younger child a period after the older child has reached 16. This approach therefore gains identification from looking at income variations within a family rather than considering differences across families. Sibling studies require a full income history for the time period considered. This time period may be less than all childhood years if a measure of educational development is used.

The central problems for sibling studies are that siblings will often be close in age and experience common income patterns for most of their childhood and that only families with 2 or more children can be considered. However, the timing of income changes will be different and so sibling studies may give insight into whether income matters more at specific ages. Blau (1999) and Levy and Duncan (2001) are recent sibling studies using US data from the National Longitudinal Survey of Youth and Panel Study of Income Dynamics respectively. They find that parental income matters most for young children, but that the magnitudes are quite small. Blau (1999) also studies intra-family variations focussing on mothers' siblings and therefore using variation between cousins to identify the effect of income.

(iv) Income Changes – Mayer (1997) looks at whether transitory income fluctuations have an impact on child educational outcomes. She uses family income after the child has reached adulthood to predict permanent family income. The argument is that family income after the child has left home cannot in itself influence prior educational attainment. So any correlation between post-childhood family income and attainment reflects an underlying correlation with permanent income and background factors. Post-childhood family income is thus a benchmark for permanent income differences across families. Once the permanent income benchmark is controlled for, childhood income reflects only residual deviations from this benchmark and can be considered uncorrelated with fixed family factors and hence free from bias. Hence it is the impact of transitory family income that is identified.

Mayer looks at a range of child outcomes and IQ type test scores. The addition of post-childhood family income reduces the estimated impact of a 10 percent increase in income on years of schooling from 1.86 to 1.68 (after conditioning on observed family fixed characteristics). Hence the conditioning here makes only a minor difference. However, the coefficients on income in a range of other social outcomes such as teenage motherhood and dropping out of school are reduced more substantially. The impact of this conditioning on the relationship between income and a range of IQ tests is somewhat inconclusive, with some coefficients falling and some rising. Hence she argues that most of the raw relationships between family income and child outcomes reflect background factors rather than income. But this general conclusion does not hold for educational attainment.

The main concern with the Mayer analysis is that income changes between the two periods she considers actually reflect family shocks that influence child attainment. Blau (1999) therefore goes a stage further and estimates child fixed effects models with permanent elements of attainment (age adjusted test scores) and family income. Hence he looks at estimates from regressions of changes in attainment on changes in income and finds small and statistically insignificant effects of current mother's wage and family income on child test scores. He finds larger effects for permanent income, but is clearly unable to estimate fixed effects models for these. He concludes by arguing that policies affecting family income are not likely to have a big impact on child development (unless they result in sizable permanent changes in income).

(v) Ordered Choice Models - Cameron and Heckman (1998, 2001) set out to investigate the importance of current (short term) family income on an individual's decision to pursue two or four year college because of credit constraints. They set up a sequential model of educational development through adolescence as a series of choices to pursue continued education. Each decision is influenced by prior decisions and attainment allowing family income and background factors to have distinct influences at each stage. Cameron and Heckman also introduce observed prior ability from IQ type test scores as a control for ability. They conclude that family income is important in terms of attainment by age 17 and the decision whether or not to complete high school, but not on the decision to attend college conditional on attainment and high school completion.

(vi) Instrumental Variables – Shea (2000) adopts an instrumental variables approach to consider the relationship between fathers' earnings and child outcomes, including education, with sources of earnings variations that he describes as luck (these are union status, industry wage premia and measures of the impact of job loss). His results are the only ones from the recent US work that suggest parental earnings do not influence child educational attainment. The instruments do, however, seem of dubious plausibility at best.

In summary the US literature, with the exception of Shea, consistently shows that family income does influence a child's educational attainment, but that the magnitudes of the impact vary somewhat. They are very small in the sibling studies, but much larger in the Mayer (1997) and Acemoglu and Pischke (2001) studies. On questions of timing, some of the literature also suggests that family income matters most when children are younger.

4. Education-Income Correlations From Repeated Cross-Section Data

Family Expenditure Survey Data

Our first approach to estimating the connection between child education and family income is based upon repeated cross-section data from the Family Expenditure Survey (FES). The FES is an annual survey of around 6000 households per year (covering around 10000 people). We look at net income measures in our empirical work on the grounds that it is net income that is the appropriate measure if one is thinking about resources available for investment in children. Education was first reported in 1978 and so our analysis forms a cohort of 17/18 year olds and links their education to family income and other characteristics of them and their parents. We therefore focus on the years 1979 to 2000. We have around 420 matched up 17/18 year olds and parents per year. This smallish sample size rather constrains our ability to look at changes over time and so we defer that to our later analysis based on the much larger samples in the birth cohort data.

The education data in the FES is fairly rudimentary and can be ascertained from two questions on age left full-time education and on whether the individual concerned is still in full-time education. So for this part of our analysis we model whether individuals stay on in the education system after the compulsory school leaving age. Later on when we look at cohort-specific panel data we consider different stages of education, and their sequential nature, in more detail.

Approach

For individual i we model the staying on decision S_i as a function of log(family income), $\ln Y_i$, and other individual and parental characteristics P_i as follows (ε_i is a random error)

$$S_i = \alpha + \beta \ln Y_i + \delta P_i + \varepsilon_i$$

As S is a discrete 0-1 (No-Yes) variable we can estimate this model by probit methods. But the conceptual problem discussed above remains, unobserved heterogeneity will bias the estimate if $\mathrm{Cov}(\ln Y_i, \varepsilon_i) \neq 0$.

One way to control for the unobserved heterogeneity is to introduce a detailed set of variables to proxy for what might think of as person fixed effects in an attempt to purge the bias of the estimated coefficient on log(family income). Writing the error term as a function of the fixed background characteristics A_i so that $\varepsilon_i = A_i + \omega_i$ and substituting in the above equation then gives

$$S_i = \alpha + \beta \ln Y_i + \delta P_i + A_i + \omega_i$$

To the extent that A_i controls for the unobserved heterogeneity this will ameliorate the bias in the β estimate. As noted above some authors have argued that cohort data, like the data we consider below, can purge bias by inclusion of a detailed set of A_i controls. In fact, in a different context in their study of school quality effects on wages, Dearden, Ferri and Meghir (2002) go as far as arguing that the rich cohort data they use (one of the two cohorts we use) enables one to control for bias due to school selection through inclusion of a detailed set of pre-school selection controls. They interpret this as a matching estimator in the sense of Rosenbaum and Rubin

(1983). We also adopt this procedure in our analysis of education and income from the two British birth cohorts.

But there is a serious worry that this approach either controls for too much or not enough so that the estimate of β remains biased. The other alternative route is to find an instrumental variable for $\ln Y_i$. A good instrument for $\ln Y_i$ is a variable, say T_i, that is strongly correlated with $\ln Y_i$ but that does not affect staying on, S_i, other than through its impact on income.

We believe that the changes made to the UK tax system in the period we study offer a convincing instrument for family income. The mechanism we are interested in is what happens to education if parental income is shifted. One can argue that the changes to the tax system introduced in the UK offer precisely this mechanism. Take the case of a cut in personal income taxes (several of which occurred over our period of study). Relative to people in an earlier cohort this means that the beneficiaries of the tax cut have more relatively more income. If this boosts investments in children (direct or indirect) then this provides scope for a positive link between education and income.

As noted above, an instrumental variable strategy in the context of education and family income is contained in Shea (2000) (though he only used a single cross-section). Our approach has greater similarity with the use of tax changes to instrument wages in a female labour supply equation for the UK in Blundell, Duncan and Meghir (1998). It amounts to using the changes in the tax system as a 'natural experiment' to generate tax induced changes in family income. We construct a measure of the income benefits of tax cuts (or income losses from tax gains) and use this as an instrumental variable. To do this we draw upon a fairly well established US literature that looks at the impact of tax changes on household income (see, amongst others, Lindsey, 1987, Feldstein, 1995, Auten and Carroll, 1999 and Saez, 1999).

The instrument we use is the gap between the tax take from an individual under the current applicable tax regime and the tax take under the initial period (1979) tax regime (uprated with prices) as a proportion of current gross income. This is basically the change in the average tax price over time. We formulate the change in the tax price such that it reflects the income gains from tax cuts (or income losses from tax increases). The measure is defined as the gap between the tax take in a given period and a counterfactual tax take assuming the 1979 tax structure remained in place. It is thus the regulatory changes in tax structure that drive the variations in the tax price. The changes made to the UK tax system are reported the Tables in the Appendix. It is clear that the tax changes were fairly complex and affected different components of family income at different times. We therefore use several tax price variables that vary over time in different ways, relating to individual earnings and national insurance, household unearned income (excluding benefits) and benefit income.

The main features of the tax changes over time were as follows:

i) Income Tax: basic tax rates were reduced at various times in the periods 1968-1988 and 1996-2000, whilst higher rates where reduced only in 1988. In contrast, the tax free allowance and the basic rate allowance failed to grow in line with earnings such that a greater proportion of income was subject to tax. This is known in the tax literature as "bracket creep" (see Saez, 1999). This was most marked in 1981 and 1993-94 when tax thresholds were frozen in nominal terms. Prior to 1990 Income Tax was based on as joint income for married couples but after that date it was assessed only on individual income. This switch benefited two earner couples even though the extra tax-free allowance available to married couples was reduced in value through the 1990s.

ii) National Insurance: by contrast employers' tax rates (National Insurance, NI) rose in the early 1980s. Unlike income taxes, NI is not levied on unearned income and has an upper limit for earned income above which earnings are not subject to NI. Hence switching taxation from Income Tax to NI is regressive.

iii) Benefits: The main Income Support (and its precursor Supplementary Benefit) rates for families with children were broadly constant in real terms over this period. But as incomes rose in real terms the value of these benefits lagged behind average incomes. In-work benefits (Family Income Supplement, 1979-1987, Family Credit 1988-1999 and Working Family Tax Credit 1999-2000) rose faster than Income Support rates especially around the transition points between regimes of 1988 and 2000.

Correlations Between Education and Income

The first two columns of Table 1 show probit estimates of staying on equations for 17/18 year olds from Family Expenditure Survey data from 1979 to 2000. Column 1 reports a specification with just year dummies and controls for the gender of the child and the presence of one or two or more siblings. The reported marginal effect is the impact of a standard deviation reduction in income from the mean. It implies that a standard deviation reduction in income reduces staying on probabilities by 9.5 percentage points. Column 2 introduces controls for decade birth/education cohorts for each parent and whether there is just one parent (normally a lone mother). The decade birth/education cohort is defined as whether the mother/father was born before 1930, 1930-39, 1940-49 and after 1950 and then each birth decade cohort is divided according to whether the mother or father stayed on in education after age 16. These interactions capture both the increasing education levels of parents through time and the rising returns to education across cohorts of parents (see Blundell, Duncan and Meghir, 1998). The introduction of these controls reduces the estimated relationship between family income and staying on rates by approximately 40 percent with the marginal effect of a standard deviation reduction in income standing at 5.3 percentage points

Using Tax Changes As Instruments For Income

As we have already discussed in some detail, there are concerns that these estimates remain biased upward due to unobserved heterogeneity across families that is correlated with both family income and educational attainment of parents. In columns 3 and 4 of Table 1 we implement the Instrumental Variables estimator. The generation of the instrument set is described above and the detail of the changing tax parameters is laid out in the Appendix. Column 3 contains the first stage log(family income) regression. This contains very significant coefficients on the tax price variables. An F-test strongly rejects that they are jointly insignificant. They seem to be good instruments in that they are strongly correlated with family income, the endogenous regressor in our model. The income models mostly show that families with bigger gains from the tax changes were higher income families. This is true of changes in taxes on earned, unearned and benefit income where the tax price variables attract strongly significant positive coefficient estimates.[5] The opposite is true of income gains from reforms to National Insurance, since there is an upper limit to NI contributions that makes benefits from cuts in NI or losses from NI increases relatively smaller at the upper end of the income distribution.

Column 4 reports the IV estimates of family income on the propensity of the child to stay on in full-time education beyond the minimum school leaving age. The estimates given are Instrumental Variable Probit estimates (as outlined in Newey, 1987). The standard errors are corrected for being derived from the first stage

[5] The positive coefficients on the benefit tax prices reflect that the tax changes made here caused lower income groups to lose out since they had negative tax price changes relative to the tax price change of zero for families not receiving benefits.

equation according to procedure outlined by Newey.[6] The estimated coefficient falls somewhat, but remains strongly significant. The predicted marginal effect of a standard deviation reduction in income comes down to 3.7 percentage points. This is approach suggests a fall in the estimated effect of family income on staying on to around 70 percent of the Column 2 specification.

5. Staying On-Income Correlations From Cohort Data

The British Birth Cohorts

The second approach we take is to look at the relationship between education and income using data from two very rich British birth cohorts. These are the National Child Development Study (NCDS), a survey of all children born in the UK between 3 and 9 March 1958, and the British Cohort Survey (BCS), a survey of all children born between 5 and 11 April 1970. The NCDS is a very rich data source that has been used in previous work on the effects of family background on child outcomes in the UK (e.g. Gregg and Machin, 1999, 2000) and consists of the birth population with follow-up samples at ages 7, 11, 16, 23, 33 and 42.[7] The BCS has been used less by economists, but is very similar in style, with data collected at ages 5, 10, 16, 26 and 30. As well as being similarly structured the questions asked in the two cohorts are frequently identical, although there are some difficulties inherent in using them in a comparative study over time. Where relevant we discuss these below. The use of cohort data allows us to follow full sequence of the post-16 education development of cohort members in a way that is very difficult from even rich cross-sectional sources. In this respect it our approach is closest to that followed by Mare (1980) or Cameron

[6] We are grateful to Joe Harkness of John Hopkins University for developing this procedure in Stata.
[7] The NCDS data have also been used to look at the transmission mechanisms that may underpin intergenerational mobility: see Gregg and Machin (1999, 2000), Hobcraft (1998) or Kiernan (1995).

and Heckman (1998) who use look at sequential models of education using US cohort data.

These birth cohorts allow us to attempt to control for the unobserved heterogeneity by introducing a detailed set of variables to proxy for what one might think of as child fixed effects. These comprise a mixture of variables measuring experience during the childhood years and a set of parental characteristics. We consider ability scores based upon literacy and mathematics tests undertaken in both cohorts at age 11 (10 in the BCS). This sets the initial condition for attainment by age 10/11 and acts as to proxy a child fixed effect, so that the estimated equation can be thought of as measuring the impact of income at age 16 on child human capital development after age 10. The model is thus a human capital trajectory based on observed attainment by age 10. This has similarities to the value added type measures used to look at the impact of school quality and resourcing on educational attainment (for a review of these see Vignoles et al, 2000).

As noted above, this approach still leaves a concern of residual bias due to unobserved fixed family or child characteristics being correlated with income. To get a handle of the size (and direction) of possible residual bias we additionally introduce a lagged income measure in the BCS equation[8] in an attempt to purge the bias of the estimated coefficient on log(family income). This gives an estimating equation of

$$S_i = \alpha + \beta_1 \ln Y_{16i} + \delta P_i + A_i + \beta_2 \ln Y_{10i} + v_i$$

Summary Statistics

Table 2 outlines key summary information for these two cohorts. The percentage of each cohort achieving five or more O levels rises between the cohorts from 19 to 37 percent. Likewise staying on in full-time education post-16 rises from 29 to 46 percent. The percentage having at least a bachelor's degree nearly doubles.

Over the same period real incomes only rose marginally but income inequality had started its substantial rise through the 1980s. This is revealed by the standard deviation of family income rising by more than 20 percent.

Comparison With Family Expenditure Survey

We begin our cohort analysis by comparing the fixed effects proxy route with the Instrumental Variable approach from before. This is a useful exercise to see how well we appear to purge bias to the same extent using the two differing approaches. To facilitate this comparison we constructed a 'BCS cohort' from the Family Expenditure Survey by defining a window for the FES data that contains the BCS age cohort (plus two years either side to generate a sufficient sample size).

Table 3 contains the results from this comparative exercise. It reports IV estimates from the FES 'BCS cohort' and two BCS specifications that differ in whether or not they include age 10 income. The first two columns show the IV results. The first stage equation confirms that the instruments remain powerful, as in the full sample. The IV probit estimated coefficient for family income on staying on rates is slightly (though not much) larger than for the full sample, with a marginal effect showing a 5.3 percentage point reduction in staying on resulting from a standard deviation reduction in income (see Column 2).

Column 3 of Table 3 reports the equivalent BCS result including the same controls plus the child test scores at age 10. Here the estimated coefficient is above the IV estimate from the FES, though is of the same order of magnitude. Column 4 introduces age 10 income as well to net out any additional residual correlation between permanent income and unobserved family characteristics. The resulting coefficient and marginal are now only fractionally higher than the IV estimate. Given the very different nature of these approaches the similarity of these estimates is

[8] Unfortunately, income is only measured at cohort member age 16 in the NCDS.

encouraging and in line with the idea that a robust relationship has been identified. The results suggest that a standard deviation reduction in income (approximately 45 percent) reduces staying on rates by around 5 to 6 percentage points giving an elasticity of approximately 0.12.

Cross-Cohort Comparisons

As we have two broadly comparable birth cohorts we are also interested in whether the education-income relationship has changed as income inequality started to rise in the UK. As noted earlier in a previous paper intergenerational income transmission appears to have risen between these two birth cohorts (Blanden et al, 2001). A strengthening relationship between family income and educational attainment is a plausible candidate to explain this observed pattern.[9]

Table 4 thus starts the cross-cohort comparison on the same basis as we have explored so far, namely looking at connections between staying on in full-time education beyond age 16 and income. In the next section we explore the full sequence of educational development from age 16 onwards. As we do not have a previous income measure in the NCDS we report estimates equivalent to Column 3 in Table 3. The results suggest a sharp increase in the relationship between income and staying on across the cohorts. The estimated change in the relationship is strongly significant suggesting an increasing impact of a standard deviation of income from 2.5 to 8 percentage points (in absolute terms). Note also that the estimated BCS relationship with the addition of age 10 income (presented earlier in column 4 of Table 3) remains substantially above the NCDS estimate with no lagged income variable. The results in the Table strongly support the notion that there was a rise in the sensitivity of education to parental income. Put another way, the children who benefited most from

[9] In the Blanden et al (2001) paper we present a simple model arguing that the extent of intergenerational mobility can shift over time due to changes in labour market returns to education and in the sensitivity of education to parental income.

the education expansion that occurred over the period we study were from richer families.

6. Sequential Models of Education-Income Correlations From Cohort Data

Motivation

As cohort members are observed several times it is possible to observe their educational achievement at a number of key stages. It is clear that the movement towards the highest level of schooling is a progression that is dependent on attainment during previous stages. For example, those who did well in O levels are more likely to stay on at school and those who stay on at 16 have a higher probability of going on to complete higher education. Therefore models that focus on the effect of family income on final achievements may confound the influences of income on school attainments, the decision to stay on and the decision to go to university. In this section we attempt to disentangle these effects by exploring a three stage model where individuals move through O level achievement, the staying on decision and obtaining a university degree. In addition we recognise that all these stages are affected by what has gone before, so all the effects we estimate are allowed to vary by the previous stage.

The models estimated here have been substantially influenced by those of Mare (1980) and Cameron and Heckman (1998) for the US, who look at grade completion as a sequence where it is assumed that you must complete one grade to move to another. The UK education system is rather less linear than the US and therefore our models allow for any combination of outcomes. As we observe O level achievement and the staying on decision at age 16 and the degree outcome in the early 30s it is not impossible for an individual to fail both the first stages and still obtain a degree. Obviously the probability of this is rather less likely than the traditional route, as we shall see in the next sub-section.

Descriptive Statistics

Figure 4 presents “trees” showing the probability of each educational route for the two cohorts. Moving from left to right we first observe that roughly one fifth of the NCDS cohort attained five or more O levels at age 16. As we would expect the chances of staying on are substantially influenced by whether this was achieved or not. Of those who got 5 O levels over four fifths stay on at school after the compulsory leaving age while this is only seventeen percent for those with less than 5 O levels. A similar story can be shown for the probability of obtaining a degree with about 60 percent of those who obtained 5 plus O levels who also stayed on at school (this can be thought of as the “sealed train” route through education) going on to get a degree. There is, however, some evidence of individuals getting back into education as for those who achieved either O levels without staying on or who stayed on without O levels the probability of getting a degree is 15 percent.

The BCS cohort shows a sharp rise in examination achievement with 37 percent of this cohort doing well at O level. Slightly less of this larger number stay on at school than they do in the NCDS, however. In contrast, a rather larger proportion that did not do well at O level stay on as compared to the NCDS. This may demonstrate increased opportunities for non A-level further education in the second cohort. Patterns at degree level are fairly similar across the cohorts. One change is a widening gap in the probability of getting a degree for young people who do well at O level but do not stay on compared to those who did not achieve O levels but do stay (again this might be because those who stayed on without O levels are likely to be taking non-academic qualifications). Taken together the diagrams indicate that the increase in educational attainment between the cohorts seems to be coming from an increase in O level attainment, which then filters through to the second and third stages.

Estimated Sequential Models

Our statistical models are designed to allow for different income effects at different education stages. We thus estimate a sequential model that corresponds to the trees given in Figure 4. This amounts to estimating conditional probit models at each node of the tree. We estimate these by Maximum Likelihood where we also allow errors to be correlated for the same individuals at different stages. At a particular stage s of the education sequence E the estimating equation is:

$$\Pr[E_i^s = 1 \mid E_i^{s-1} = 1,0 \mid E_i^{s-2} = 1,0] = \alpha_s + \beta_s \ln Y_i + \delta_s P_i + A_i + \omega_i^s$$

Here, like in Cameron and Heckman (1998), an event $E_i^{s-j} = 0$ says (for j = 0, 1, 2) an individual stops their education at stage s-j and the event $E_i^{s-j} = 1$ says they continue on to the next stage. Our sequential model estimates these models following the sequence given in the tree Figures (so the parameter estimates all have s subscripts attached to them).

Table 5 presents the income effects for the sequential model controlling for children's test scores and family background as in the earlier staying on models. The top panel presents the NCDS, the middle panel the results for the BCS and the lower panel the changes between the two. In both the NCDS and BCS the largest income effects are noticed for staying on when individuals have O levels and the second largest are for degree attainment when the cohort members have good O levels and stayed on. This is noteworthy as we know that this traditional route is the most common way to move onto the next stage. If income has an important effect here it will be important in determining the overall likelihood of staying on and obtaining a degree.

When we look at the cross-cohort changes the first thing to note is that, in every case but one, the income effects for the BCS are larger than those for the NCDS, reconfirming the earlier picture of an increase in the influence of parental income on education. However in not all cases are these changes statistically significant. The largest change occurs at the O level stage where the marginal effect of a standard deviation fall in log income rises (in absolute terms) by 3.5 percentage points (i.e. from lowering the probability of obtaining good O levels by 0.9 in the NCDS to 4.4 points in the BCS). The magnitude of this change is highlighted when we recall that less than a third of this group stay on in the BCS so the marginal effect amounts to around 15 percent of the group mean as compared to about 8 percent for the NCDS.

In addition a large rise occurred in the effect of income on the staying on decision for those who did not obtain O levels. The effect of income on this decision is very small in the NCDS, but in the BCS a standard deviation drop in parental income reduces the probability of staying on for that group by 4.5 percentage points.

The third significant increase in the income effects is for the probability of getting a degree for those who passed five or more O levels but who did not stay on. In the NCDS there is a significant negative income effect here. This means that those with lower income found it easier to come back and get a degree. We might think that poorer individuals may have failed to stay on for financial reasons rather than because of a lack of ability or motivation and therefore more likely to reenter later than their better-off peers. By the BCS however any such advantage has been lost with this group showing a positive relationship between income and degree attainment. However it must be borne in mind that in both cohorts this group is a very small proportion of the population.

If we return to consider the descriptive statistics (in the tree Figures) it is noticeable that in all the cases where a significant rise in the income effect has occurred there was a substantial rise in the mean of the outcome between the cohorts as well. The proportion of people achieving 5 or more O levels between the cohorts doubled, the proportion of those staying on with no O levels rose by 70 percent and the rise in the proportion obtaining degrees in the O levels but did not stay on was rather lower, but still evident at about 30 percent.

In summary, the sequential models reconfirm our previous results that income and educational attainment have become more closely linked over time. The most marked increase in the relationship occurred for school examination achievement and for staying on for those who did not attain O levels. These are the two points with the largest growth in numbers between the cohorts however and it appears that the additional opportunities were not being evenly distributed through the population and, in fact, that the expansion of education occurring over the time period we study actually increased educational inequality.

Simulations

The nature of the sequential model means that it is not possible for the effect of a change in income to be entirely captured by coefficients and marginal effects alone. A change in income which increases the proportion of people with O levels will means that the extra people getting O levels now have a higher probability of staying on at school and going on to university. In this section we present some fairly rough (back of the envelope) calculations that take account of these feed-through effects. Our calculations are based on the effect of a one standard deviation rise in log income on the lowest quintile of the family income distribution.

For the simplest cases where individuals remain on the same part of the "tree" we estimate the predicted probability of passing into the next stage and add to this the relevant marginal effect as calculated in Table 5. However things become rather more complicated in situations where we follow individuals down a branch of the tree that they did not take in reality. For example, in the BCS the marginal effect of income on O levels is 4.4, meaning that an extra 4.4 percent of the sample will obtain O levels when log income is increased by a standard deviation. In our sample these people did not obtain O levels so in order to find out how they would do in the next stage we predict the probability out of sample that those who currently do not have O levels stay on if they did obtain O levels. In order to do this we use the coefficients from the staying on equation for those who do have O levels. We can follow this procedure throughout the model.

However in order to do this a number of assumptions must be made. First, we must assume that the marginal effects that operate at the mean are the same for the bottom quintile.[10] Second it must be the case that marginal effects are of the same magnitude for those who change across branches of the tree as for those who remain in the same place (we do not make the assumption that the probability of moving to the next stage is the same for these two groups). However, the results obtained here ought to be indicative, at least.

Table 6 presents the consequences for each stage if incomes in the bottom quintile are increased by one standard deviation. For the NCDS the effects are fairly small, although they do rise as we move through the stages, with the income rise leading to a 12.7 percent rise in the probability of obtaining a degree for this group, from .071 to .080. As we would expect, the income effects for BCS are much larger. For all three stages they raise the probability of success for the group by between a fifth and a quarter. Overall the results demonstrate the importance of considering the

[10] We have also performed the simulation computing a new set of marginal effects for the bottom quintile of the model. It is clear, however, that the non-linearity inherent in the probit model means that

total effects on the population rather than just considering the marginal effects. For example, the fact that the largest effect is found on the O level probability in the BCS is a consequence of the whole population being affected by this probability. In the sequential econometric model the size of the marginal effect at this point is not that large.

7. Conclusions

In this paper we focus upon links between education and family income using British data. We pay quite a lot of attention to the mechanisms that can underpin such a relationship and build up empirical models that are suited to dealing with possible biases. We begin by presenting estimated education-income relationships from looking at repeated cross-section data from the Family Expenditure Survey from the late 1970s onwards. We uncover a significant positive link between education and income, both in raw correlations and when we instrument family income using the changes to the tax system. We treat these tax changes as a quasi-experiment that provided exogenous variations in income that differentially benefited families at different points in the income distribution. The instrumentation does reduce the income coefficient in a staying on equation, but we still isolate an important income effect: children from families with income a standard deviation lower than the average had a staying on rate of around 4 percentage points lower.

The second route we follow is to consider the education-income relationship using data from two British birth cohorts (covering people born in 1958 and 1970). There is a big increase in educational attainment across these cohorts, at the same time as income inequality rose. We present models, when structured to control for aspects of unobserved heterogeneity linked to childhood experiences and family

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this is not obviously the right thing to do. In actual fact, there is some variation in the two types of marginal effects, but it is typically not the case that one type is always larger than the other.

background, showing income effects of reasonably similar magnitude to those from the repeated cross-section analysis.

Moreover, when the same models are estimated across cohorts we find an increased sensitivity of education to family income in the later cohort. This is true in staying on equations and in models where we consider the sequential nature of education, and its possible links with income, in more detail. It seems that the principal beneficiaries of the education expansion were children from richer families. This is also in line with the findings using tax cuts as an instrument as we reported that richer families benefited more from tax cuts and therefore that this went hand-in-hand with increased educational inequality.

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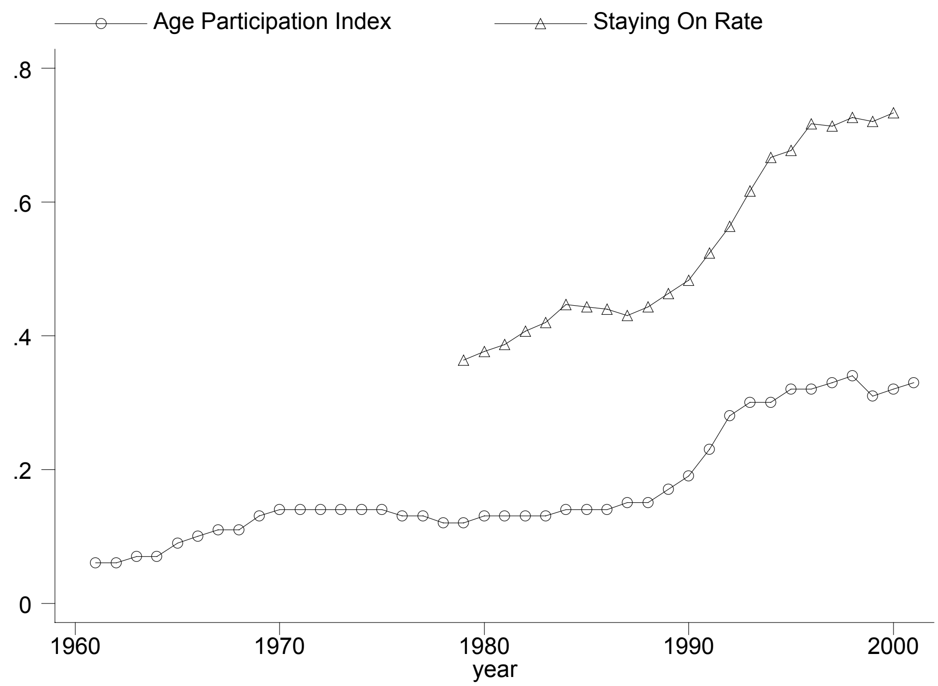
Figure 1: Staying on Rates and Higher Education
Age Participation Index For Young People

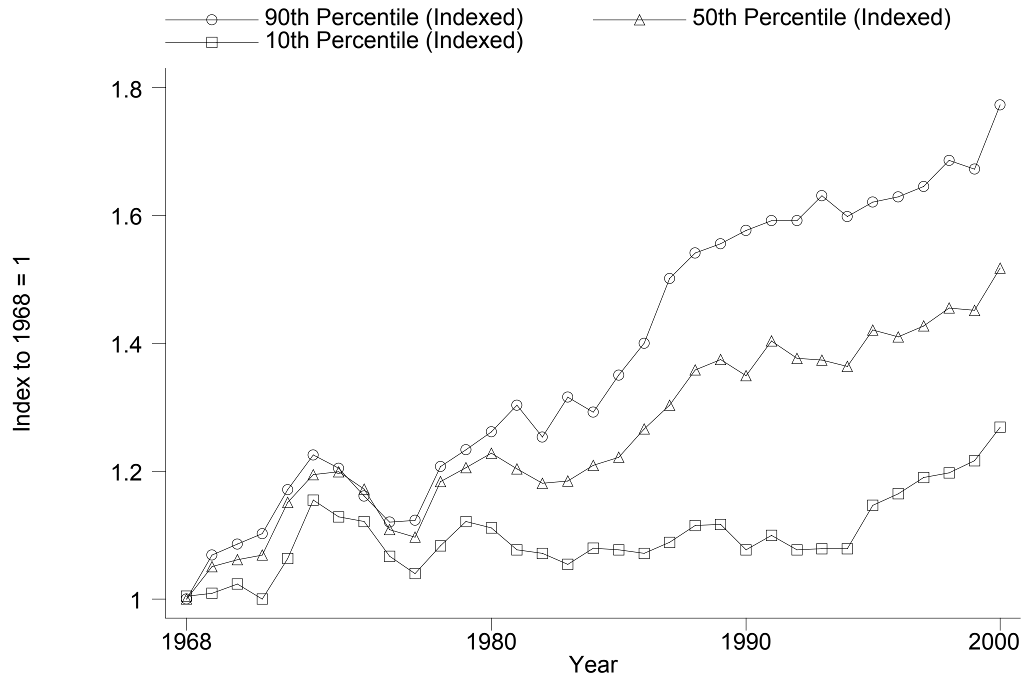


Notes:
Staying on rates calculated as proportion of Family Expenditure Survey cohort of 17/18 year olds still in full-time education. Source: own calculations.
Higher education age participation index is the number of young (under 21) home initial entrants expressed as a proportion of the averaged 18 to 19 year old population. Source: DfES.

Figure 2: Changes Over Time in the Log(Real Income) Gap Between Families With and Without Children

Notes: Own calculation from Family Expenditure Surveys of 1968 through 2000. Sample is all non-pensioner families.

Figure 3: Changes Over Time in the Distribution of Log(Real Income) For Families With Children



Notes: Own calculation from Family Expenditure Surveys of 1968 through 2000. Sample is all non-pensioner families with children.

Figure 4: Education Sequences by Cohort

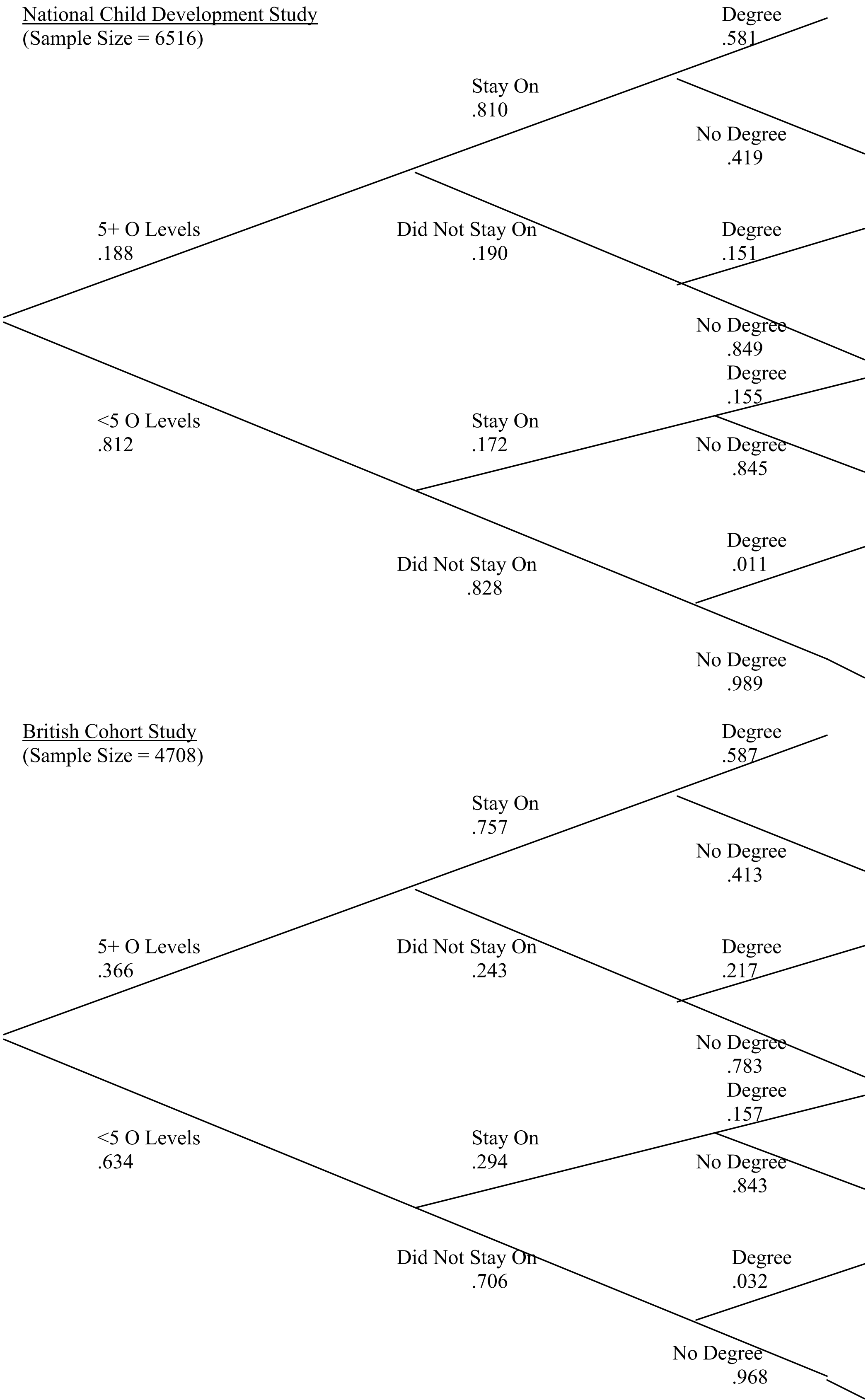


Table 1: Staying on Rates and Family Income
(Family Expenditure Survey, 1979-2000)

	Staying On		**Log(Income)**	**Staying On**
	Probit	Probit	First Stage	Instrumental Variables Probit
Log(Income)	.502 (.029)	.285 (.034)		.199 (.062)
Marginal Effect	-9.5	-5.3		-3.7
Tax Price: Earned Income			2.816 (.238)	
Tax Price: Unearned Income			13.688 (.764)	
Tax Price: National Insurance			-24.166 (.533)	
Tax Price: Income Support			.338 (.023)	
Tax Price: Family Credit			4.536 (.193)	
Controls for Sex, Siblings, Age Cohort	Yes	Yes	Yes	Yes
Controls for Mother Age, Father Age, Mother Education, Father Education, Plus Age-Education Interactions and Lone Parent Dummy	No	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	-	-	.544	-
P-Value of F-test of Instruments	-	-	.000	-
Sample Size	9266	9266	9266	9266

Notes: Coefficient estimates (standard errors in parentheses). The marginal effects of log(income) are the percentage point impact of a one standard deviation reduction in log(income).

Table 2: Education and Mean Family Income From the Birth Cohorts

	1958 Cohort	1970 Cohort
Education of Cohort Member		
Five or More O Levels at Age 16	19	37
Stayed on at Age 16	29	46
Degree	12	23
Family Income (Weekly)		
Income of Cohort Member at Age 16	305 (129)	311 (164)
Income of Cohort Member at Age 10	-	285 (133)

Notes: Based upon 6516 people in the 1958 cohort and 4708 people in the 1970 cohort. Incomes are in January 2001 prices. Standard deviations in parentheses. The sample size for the BCS is smaller at 3773 when we require families to have income information at age 10 as well. However the means for the education variables are very similar (37 for O levels, 47 for staying on and 23 for degree), the mean of age 16 income rises to £319.

Table 3: Staying On Rates and Family Income:
Family Expenditure Survey 'BCS Cohort' Compared to BCS

	Family Expenditure Survey 'BCS Cohort' (Centred on 1986)		**British Cohort Study**	
	Log(Income)	**Staying On**	**Staying On**	**Staying On**
	First Stage	Instrumental Variables Probit	Probit	Probit
Log(Income)		.272 (.110)	.367 (.051)	.246 (.058)
Marginal Effect		-5.3	-7.1	-4.7
Tax Price: Earned Income	5.194 (.921)			
Tax Price: Unearned Income	6.430 (1.424)			
Tax Price: National Insurance	-41.347 (1.493)			
Tax Price: Income Support	.310 (.061)			
Tax Price: Family Credit	1.120 (.263)			
Controls for Sex, Siblings, Age Cohort	Yes	Yes	Yes	Yes
Controls for Mother Age, Father Age, Mother Education, Father Education, Plus Age-Education Interactions and Lone Parent Dummy	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	.477	-	-	-
Test Scores	-	-	Yes	Yes
Log(Income) at Age 10	-	-	No	Yes
P-Value of F-test of Instruments	.000	-	-	-
Sample Size	2739	2739	3773	3773

Notes: Coefficient estimates (standard errors in parentheses). The marginal effects of log(income) are the impact of a one standard deviation reduction in log(income).

Table 4: Staying On Rates and Family Income: NCDS Compared to BCS

	National Child Development Study	**British Cohort Study**	**Cross-Cohort Change**
	Staying On	**Staying On**	**Staying On**
	Probit	Probit	Probit
Log(Income)	.105 (.040)	.377 (.044)	.272 (.059)
Marginal Effect	-2.0	-7.4	-5.4
Controls for Sex, Siblings, Age Cohort	Yes	Yes	Yes
Controls for Mother Age, Father Age, Mother Education, Father Education, Plus Age-Education Interactions and Lone Parent Dummy	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Test Scores	Yes	Yes	Yes
Sample Size	6516	4708	

Notes: Coefficient estimates (standard errors in parentheses). The marginal effects of log(income) are the percentage point impact of a one standard deviation reduction in log(income).

Table 5: Income Coefficients at Each Stage in the Education Sequence Conditional on Outcomes in Previous Stage(s)

<table>
<tr><th colspan="3">National Child Development Study</th></tr>
<tr><td>5+ O Levels</td><td>Stay On|5+ O Levels</td><td>Degree|Stay On|5+ O Levels</td></tr>
<tr><td rowspan="7">.120 (.051)
-0.9</td><td rowspan="3">.298 (.091)
-3.9</td><td>.195 (.091)
-3.6</td></tr>
<tr><td>Degree|Did Not Stay On|5+ O Levels</td></tr>
<tr><td>-.439 (.201)
5.1</td></tr>
<tr><td>Stay On|<5 O Levels</td><td>Degree|Stay On|<5 O Levels</td></tr>
<tr><td rowspan="3">.027 (.049)
- 0.3</td><td>.059 (.112)
-0.6</td></tr>
<tr><td>Degree|Did Not Stay On|<5 O levels</td></tr>
<tr><td>.265 (.162)
-0.3</td></tr>
<tr><th colspan="3">British Cohort Study</th></tr>
<tr><td>5+ O Levels</td><td>Stay On|5+ O Levels</td><td>Degree|Stay On|5+ O Levels</td></tr>
<tr><td rowspan="7">.323 (.048)
-4.4</td><td rowspan="3">.442 (.081)
-7.1</td><td>.333 (.084)
-6.4</td></tr>
<tr><td>Degree|Did Not Stay On|5+ O Levels</td></tr>
<tr><td>.245 (.188)
-3.3</td></tr>
<tr><td>Stay On|<5 O Levels</td><td>Degree|Stay On|<5 O Levels</td></tr>
<tr><td rowspan="3">.273 (.056)
-4.5</td><td>.204 (.120)
-2.2</td></tr>
<tr><td>Degree|Did Not Stay On|<5 O Levels</td></tr>
<tr><td>.205 (.123)
-0.7</td></tr>
<tr><th colspan="3">Cross-Cohort Changes</th></tr>
<tr><td>5+ O Levels</td><td>Stay On| 5+ O Levels</td><td>Degree|Stay On|5+ O Levels</td></tr>
<tr><td rowspan="7">.203 (.070)
-3.5</td><td rowspan="3">.144 (.122)
-3.2</td><td>.138 (.124)
-2.8</td></tr>
<tr><td>Degree|Did Not Stay On|5+ O Levels</td></tr>
<tr><td>.684 (.275)
-8.4</td></tr>
<tr><td>Stay On|<5 O levels</td><td>Degree|Stay On|<5 O Levels</td></tr>
<tr><td rowspan="3">.246 (.074)
-4.2</td><td>.145 (.164)
-1.6</td></tr>
<tr><td>Degree|Did Not Stay On|<5 O levels</td></tr>
<tr><td>-.060 (.203)
0.4</td></tr>
</table>

Notes: coefficient estimates (standard errors) from full tree sequential probit model. Sample sizes are 6516 for NCDS and 4708 for BCS. The models include the same full set of independent variables as in the staying on equations in Table 4 with coefficients allowed to differ at all stages, plus a set of stage dummy variables. Errors are allowed to be correlated across stages. Marginal effects of log(income) calculated as the percentage point impact of a one standard deviation reduction in log(income) reported beneath coefficient and standard errors.

Table 6: Using the Sequential Model to Recover the Impact of Raising Incomes of the Bottom Quintile by One Standard Deviation

	National Child Development Study		British Cohort Study	
	Prediction	Prediction with extra income	Prediction	Prediction with extra income
O levels				
Predicted proportion	.120	.130	.179	.223
Percentage change		8.3		24.5
Staying On				
Predicted proportion	.208	.218	.284	.346
Percentage change		4.8		21.8
Degree				
Predicted proportion	.071	.080	.087	.108
Percentage change		12.7		24.1

Note: It is not necessary that the predictions for a sub-group will be the same as the mean for the subgroup. The means of each education stage for the bottom income quintile for the NCDS are .123, .217 and .078, for the BCS they are .183, .311 and .105, which seems to indicate quite a good fit for the models.

Appendix

Table A1: Tax Rates, Tax Allowances and Major Benefit Rates 1979-2000

Year	Basic Tax Rate	Annual Tax Free Allowance	Annual Basic Rate Allowance	NI Rate	Weekly Lower Earning Limit	Weekly Upper Earning Limit	Weekly FIS / FC / WFTC	Weekly Supp. Ben and IS
1979	30	1165	10000	6.5	19.5	135	14.5	46.3
1980	30	1375	11250	6.75	23	165	18.5	56.4
1981	30	1375	11250	7.75	27	200	20	63.3
1982	30	1565	12800	8.75	29.5	220	23	69.9
1983	30	1785	14600	9	32.5	235	24	72.9
1984	30	2005	15400	9	34	250	25	76.3
1985	30	2205	16200	9	35.5	265	29	80.2
1986	29	2335	17200	9	38	285	29.4	81.1
1987	27	2425	17900	9	39	295	30.6	82.3
1988	25	2605	19300	9	41	305	58.3	87.0
1989	25	2785	20700	9	43	325	62.9	93.0
1990	25	3005	20700	9	46	350	65.6	97.8
1991	25	3295	23700	9	52	390	74.5	105.7
1992	25	3445	23700	9	54	405	79.7	113.6
1993	25	3445	23700	9	56	420	82.6	117.6
1994	25	3345	23700	10	57	430	85.9	122.2
1995	25	3525	24300	10	58	440	87.5	124.4
1996	24	3765	25500	10	61	455	90.1	128.2
1997	23	4045	26100	10	62	465	92.4	131.5
1998	23	4195	27100	10	64	485	94.7	134.7
1999	23	4335	28000	10	66	500	96.7	137.5
2000	22	4385	28400	10	67	535	110.3	140.3

Table A2: Change in Value of Allowances and Benefit Rates Relative to Average Earnings

Period	Annual Tax Free Allowance	Annual Basic Rate Allowance	Weekly Lower Earning Limit	Weekly Upper Earning Limit	Weekly FIS / FC / WFTC	Weekly Supp. Ben and IS
1979-2000	-13.2%	-34.5%	-20.8%	-8.6%	62.2%	-30.1%
1979-1990	-7.8%	-11.8%	-6.4%	-2.7%	0	-7.8%
1990-2000	-6.2%	-26.0%	-16.4%	-6.3%	61.8%	-24.5%

Notes: In work benefit rates are calculates from the maximum for a single adult, two child household where children are aged 11 and 16. Income support rates were calculated for a two adult, two child household with children aged 11 and 16. Figures for benefits and taxes were obtained from the Institute for Fiscal Studies "Fiscal Facts" on the Institute's web-site (http://www.ifs.org.uk/taxsystem/contents.shtml).

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