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Changing economic circumstances in childhood and their effects on subsequent educational and other outcomes

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Abbreviations

ASHE	Annual Survey of Hours and Earnings
BAS	British Ability Scales
BCS70	British Cohort Study
BCS70(CC)	British Cohort Study, Children of Cohort
BHPS	British Household Panel Survey
DCSF	Department for Children, Schools and Families
DfES	Department for Education and Skills
DWP	Department for Work and Pensions
EPVT	English Picture Vocabulary Test
FES	Family Expenditure Survey
FSM	Free School Meals
KS	Key Stage
LEA	Local Education Authority
MCS	Millennium Cohort Study
ML	Maximum Likelihood
NCDS	National Child Development Study
NES	New Earnings Survey
NESPD	New Earnings Survey Panel Dataset
NPD	National Pupil Database

NVQ	National Vocational Qualification
OLS	Ordinary Least Squares
PI	Permanent Income
PLASC	Pupil Level Annual Schools Census
SD	Standard Deviation
SDQ	Strengths and Difficulties Questionnaire
SE	Standard Error
SECC	Survey of Early Child Care
SEN	Special Educational Needs
TI	Transitory Income

Glossary of terms

The definitions given here are designed to help readers to understand the more technical parts of this particular report but should not be seen as substitutes for the precise and generalisable definitions to be found in dictionaries of statistical terms. (Italics indicate cross-references to other entries.)

Autocorrelation	The association between a variable and an earlier or a subsequent measure of the same variable.
Binary variable/outcome	A variable with just two values, for example, 'yes' and 'no'.
Bivariate regression model	A *regression model* with two *outcome variables*.
Categorical variable	A variable taking more than two ordered or unordered values, for example social class or ethnic group.
Conditional model	A *regression model*.
Confounding variable	A variable not included in a *regression model* that might lead to biased causal conclusions.
Control variable	A variable included in a *regression model* to reduce the effects of self-selection.
Difference in differences model	A *regression model* in which the difference between two (or more) occasions in the *outcome variable* is related to the difference in the *explanatory variable* of causal interest.
Dummy variable	One or more binary variables generated from a *categorical variable* in order to represent that variable as an *explanatory variable* in a *regression model*.

Effect size	The effect of an *explanatory variable* on an *outcome variable*, expressed in standardised units.
Endogeneous variable	A variable that is part of the causal process generating an *outcome variable*.
Exogeneous variable	A variable that is unrelated to the causal process generating an *outcome variable*.
Explanatory variable	The variable on the right-hand side of a *regression model*, also known as a predictor variable or, misleadingly, as an independent variable which, in some circumstances, is a cause of the *outcome variable*.
Expected value	The mean.
Fixed effect	An effect associated with an explanatory variable that has a limited number of values. See also *random effect*.
Imputation model	A model that generates plausible values of a variable that has one or more missing values.
Instrumental variable/ Instrument	A variable correlated with an *explanatory variable* of interest but uncorrelated with the residual in a *regression model* and used in place of the explanatory variable in order to reduce bias.
Interaction	The product of two or more *explanatory variables*, used to represent the situation when the effect of one explanatory variable varies according to the value of another explanatory variable.
Interval scale	A scale, for example income, in which the same numerical difference between two scale values has the same meaning throughout the scale's range.
Linear effect	An effect of an *explanatory variable* that does not vary according to its own value or with the value of other explanatory variables.
Logistic regression model	A regression model in which the *outcome variable* is a *binary variable*.

Maximum likelihood	A method of estimation used with, for example, a *logistic regression model*.
Measurement error	When the true and observed values of a variable differ.
Monotonic	Increasing or decreasing but not both.
Multilevel model	A *regression model* in which the *outcome variable* can vary at two or more levels, for example between pupils within schools and between schools. These levels are represented as *random effects*.
Multivariate regression	A *regression model* with more than two *outcome variables*.
Non-linear effect	An effect of an *explanatory variable* that varies according to its own value or with the value of other explanatory variables.
Normal distribution	The symmetric bell-shaped *statistical distribution*.
Ordinary least squares	A method of estimation often used with, for example, a *regression model*.
Outcome variable	The variable on the left-hand side of a *regression model*, also known as a dependent variable or response which, in some circumstances, is an effect of an *explanatory variable*.
p-value	See *statistical significance*.
Point estimate	The actual estimate from the sample.
Power transformation	A transformation, for example square root or log, often used to bring an *outcome variable* closer to a *Normal distribution*.
Predicted value	The *expected value* of an *outcome variable* from a *regression model* for fixed values of the *explanatory variables*.
Prediction distribution	The distribution, often assumed to be Normal, of a *predicted value*.
Prediction model	A *regression model*.

Pseudo R squared	One measure of the extent to which the *explanatory variables* account for the variability of a *binary outcome* in a *logistic regression model*.
Quadratic term	A squared term, for example x^2.
Quartiles	The points that divide any *statistical distribution* into four equal sections.
R squared (R^2)	The proportion of the *variance* of an *outcome variable* accounted for by the *explanatory variables* in a *regression model* when the outcome is measured on an *interval scale*.
Random effect	An effect associated with *explanatory variables* (for example, schools) chosen at random from a population having a large or infinite number of possible values.
Regression model	A statistical model that relates one (or more) *outcome variables* to a set of one or more *explanatory variables.*
Residual	A variable in a *regression model* that represents the *variance* unexplained by the *explanatory variables*.
Singh-Maddala distribution	A *statistical distribution* used for income.
Standard deviation	The square root of the *variance*.
Standard error	A representation of the way in which an estimate, for example, from a regression model, varies from sample to sample.
Statistical distribution	A representation of the way in which a variable varies from case to case in a sample.
Statistical significance	A representation, using the *p-value*, of how likely it is that the effect of interest will be as far or further from a chosen value (often zero) in other samples.
Uniform distribution	A *statistical distribution* in which all values are equally likely.
Unobserved heterogeneity	The variance in an *outcome variable* attributed to *confounding variables*.

Variance	A measure of the spread in a *statistical distribution*.
Wald test	A test of *statistical significance* often applied to a set of estimates from a *regression model*.
z transformation	A transformation of any *statistical distribution* so that the mean is zero and the *variance* is one.

Summary

The context for this report is provided by the short-, medium- and long-term Government targets first to reduce, and ultimately to eliminate, child poverty by 2020.

The detrimental effects of poverty on children's development are well established: children growing up in poverty, especially persistent poverty, do less well at school, have poorer health and often end up in poorly paid jobs or unemployment as adults. Moreover, a lack of income can mean that children are unable to participate in social activities in the same way as their better-off peers are able to do. However, it is the nature and size of the link between changes in family income and income-related measures on the one hand and child outcomes on the other, particularly educational and behaviour outcomes, that form the essence of this report. In addition, more limited evidence is provided about the effects of economic changes experienced in childhood and these same children's incomes in adulthood. This focus on change in income (and in outcomes) over time rather than on levels of income at a particular point in time, is predicated on the view that this is the most appropriate method of separating genuine causal effects from processes of self-selection and choice. The approach does, however, carry with it a number of methodological challenges.

The findings have been generated from sophisticated statistical analyses of three longitudinal datasets: the British birth cohort study that started in 1970 (BCS70), the more recent Millennium Cohort Study (MCS) that started in 2001, and the National Pupil Database (NPD) – the administrative dataset generated from school records and pupils' test scores.

The report builds on previous research in this area, most of which emanates from North America. The balance of this evidence suggests that although income effects – the effects of changes in family income as experienced by children – on educational and behaviour outcomes do exist, they are not large. It seems that substantial improvements (or declines) in income are required in order to have a marked effect on these outcomes. This report adds to this literature from a contemporary British perspective.

The project consisted of two strands, one substantive and the other methodological. The findings from the substantive strand are presented in three sections determined by a division of childhood into three stages: early childhood (up to age six), middle childhood (from six to 11) and adolescence (the secondary school years up to age 16).

The methodological issues covered include those of establishing causal effects from longitudinal but non-experimental data; how to represent income in statistical models that link income change to outcomes; and how predicted or 'imputed' measures of income might replace measures of income that are known to be incomplete or inadequate in various ways.

Evidence from changes in early childhood

Evidence for the period of early childhood comes from analyses of up-to-date data from the MCS, more historical data from the 1970 birth cohort study and data provided in 2004 by the children of the 1970 cohort. The data vary in quality. Only change in parental employment status was available (as a proxy for income change) for the 1970 study; better income data was collected from the children of the cohort members but the sample is somewhat unusual; and relatively good income and child outcome data on a good sample is available for those children born in 2000. We would expect to find that income effects are greatest for this stage of a child's life.

Our evidence on income effects for this period is somewhat mixed: From the MCS – perhaps the best data source for this section of the analysis – we do find support for small income effects on cognitive outcomes and behaviour at age three. The conclusions from what we regard as the best model for these data suggest a small increase in cognitive scores, and slightly improved behaviour, for a substantial increase in income between nine months and three years. To put this more precisely, we find that, for two putative families having the same incomes when the cohort child is nine months, the child in the first family with twice the income at age three as the second family will score a little better – perhaps one-sixteenth of a standard deviation unit higher – on the cognitive tests. This effect represents a difference of about one month in terms of educational progress. An effect of a similar size (in terms of standard deviation units) is found for the behaviour measure.

This one positive effect needs to be considered alongside three findings of 'no effect'. Changes in family benefit status between nine months and three years are unrelated to cognitive and behaviour scores for the Millennium cohort at age three (although children in families on benefits on both occasions have lower cognitive scores and more behaviour problems); changes in parental employment status between birth and five years are unrelated to cognitive and behaviour scores for the 1970 cohort at age five, and to earnings at age 34; and changes in income between age 30 and 34 for a parent from the 1970 cohort do not affect their child's cognitive scores, although there is a hint of an effect on behaviour.

Evidence from changes in middle childhood

Again, we use data from the 1970 cohort and their children for this period. In addition, we draw on changes in claiming free school meals related to changes in Key Stage test scores from the NPD. There is slight evidence to suggest that a decline in economic circumstances between the ages of five and ten for the 1970 cohort – as represented by a reduction in parental employment – could have deleterious effects on some aspects of behaviour but there is no evidence for an effect on educational test scores nor on later earnings.

One of the strengths of the NPD is its size and coverage and this helps to compensate for the fact that it contains relatively little information about the pupils and their families. We assume an improvement in a family's economic circumstances if they move from a two-year period of claiming free school meals to two years of not claiming over the four years between Key Stages one and two and we assume a decline if they make a move the other way – two years not claiming followed by two years claiming. Only just under three per cent of the sample makes these two kinds of transitions. We find that pupils in families where economic circumstances appear to be improving, do just a little better on tests at age 11 than those living in an apparently worsening economic situation. The difference is, however, small – less than one month's progress over the four years in question.

Evidence from changes in adolescence

We rely on data from the 1970 cohort for our conclusions about the effects of changes during the adolescent period. The measures of income used then are not strong and the study suffered from a number of problems between 1980 and 1986 that reduced the quality of the data. We find evidence for only one effect: a change in family income between the ages of ten and sixteen does perhaps have an effect on earnings at age 34, although we are unable to provide convincing reasons for why this should be so when there is no effect on educational test scores or behaviour at age 16.

Overall evidence

The balance of the evidence presented in this report indicates that income effects on educational and behaviour outcomes – whilst probably not zero – are unlikely to be large. Our results are, therefore, in line with others in the literature. It is, however, important to remember that, for many analyses, we have had to rely on proxy measures of income taken at just two time points. Moreover, we have only looked at some childhood outcomes. There could be effects on, for example, physical and mental health and on other psychological constructs like self-esteem. It is also possible that parents protect consumption directly related to their children and thus, small changes in income have little effect on the children (but perhaps more marked effects on the parents themselves).

Methodological issues

The desire to generate causal conclusions from longitudinal observational data of varying quality meant that methodological issues were an important part of the study. Considerable attention was paid to questions about the measurement of income and how change in income, and in income-related measures, should be represented in statistical regression models. The most favoured model was one in which the effects on outcomes of income at a particular point in time were estimated for fixed values of income at an earlier occasion or occasions. This approach was compared with other representations of income, notably average or permanent income and income differences. Arguments were presented that cast doubt on the validity of equivalising income to account for different household compositions in this kind of work.

We were aware that the measures of income obtained for the 1970 cohort at ages ten and 16 were rather weak and that income had not been measured at all at younger ages. The project, therefore, explored different ways of imputing income by drawing on prediction equations for income generated from studies with good measures of income and also containing a range of other socio-economic variables of the kind included in the cohort study. The studies examined were the Family Expenditure Survey (FES) and the New Earnings Survey (NES). Our analyses showed that the NES was not very useful in this particular situation, partly because it focuses on individuals' earnings from the labour market rather than on the broader topic of family income and partly because the dataset contains rather few predictor variables.

Our predictive equations based on the FES were able to explain a little more than half the variation in household and family income. We did, however, find that the form of the prediction equation varied between 1980 and 1986. There was only a rather imprecise match between the predicted incomes based on the FES and the observed income bands used in the 1970 cohort study. Some of these differences might, however, be explained by imperfections in the 1980 and 1986 measures of income, based as they were on just a single question. We were also hampered by extensive missing data in the datasets for these ages. The comparison between the predicted and observed income bands was elaborated by taking five imputations for each case in the analysis in order to allow for the prediction error in the model.

This approach to dealing with the problem of absent, unreliable or incomplete measures of income shows promise, although future work would need to integrate these methods with methods for dealing with attrition and item non-response.

1 Introduction and literature review

1.1 Introduction

The substantive focus of the research reported here can be put rather plainly: do children start to do better at school and behave better at home – and does this improvement last into adulthood – if more money comes into the family, and do they fall back if family income drops? In other words, we are interested in the relation between family income and income-related measures such as benefit status and parental employment status on the one hand and, on the other, educational attainments and measures of behaviour during childhood, along with adult outcomes such as income and employment status. Income is the possible cause; educational, behavioural and later economic outcomes are the effects.

In addition to these substantive questions, the project had a substantial methodological focus in terms of (i) which combinations and functions of observed measures of income could be used in statistical models; (ii) whether it is possible to substitute externally generated estimates of income, either for weakly measured income or when income is not measured at all; and (iii) how to specify statistical models that might provide valid causal inferences.

The context for this study is provided by:

1 the short, medium and long-term Government targets first to reduce, and ultimately to eliminate, child poverty by 2020;

2 policies to increase lone parent employment and to reduce the number of children brought up in workless households.

The empirical bases of the project are data from the two most recent national British birth cohort studies – those started in 1970 (the British Cohort Study or BCS70) and in 2000 (the Millennium Cohort Study or MCS) – and from the Department for Education and Skills (DfES, now DCSF or Department for Children, Schools and Families) National Pupil Database (NPD). The aim has been to establish the causal status of any relations uncovered by relating changes in the explanatory variables of interest to changes in outcomes. So, for example, can changes in educational attainments (i.e. educational progress) be attributed to changes in income, what are the sizes of these effects and are they the same for all children? The important but arguably subsequent question of what the mechanisms or pathways might be that account for, or mediate, the causal effect – changed consumption patterns or changes in maternal mental health for example – did not form part of this particular project.

The report is structured as follows: This introductory chapter reviews recent work in this area by updating (but not repeating) the review provided in the feasibility study for this project by Plewis and Hawkes (2005). Chapter 2 is a methodological chapter that covers measurement and modelling issues. We then present our findings in five chapters: Chapters 3 to 5 deal with changes in economic circumstances at three different points in the life course: early childhood, middle childhood and adolescence. Chapter 6 presents our findings on predicting or imputing income from information on variables associated with income and using the predicted incomes in substantive models. We end with the conclusions from our investigations, some policy implications and suggestions for further research.

1.2 Recent research

Plewis and Hawkes (2005) reviewed research on the topic of income effects on childhood outcomes that was based on longitudinal data. They divided the literature into three sub-groups: evaluations of policy interventions, sibling comparisons, and observation studies that controlled for confounding variables. The last of these three groups is the most relevant here as all the evidence presented in this report is based on observational data. A substantial proportion of the recent literature emanates from North America – see Duncan (2005) for a recent review.

Jenkins and Schluter (2002) summarise the evidence about income effects on educational outcomes in the following way:

i The effects on outcomes of income averaged over childhood (sometimes referred to as 'permanent' income) are greater than the effects of income measured at the same time as the outcome ('current' income).

ii The estimates of the 'effects' of income decline as additional explanatory variables, or controls, are added into regression-like models for outcomes.

iii Income effects are small relative to the effects of other factors such as race.

iv Income effects for cognitive outcomes are generally larger than they are for behaviour outcomes.

v Effects of income on outcomes in early childhood tend to be greater than effects in late childhood.

vi Income effects are non-linear, tending to be larger for low income families.

We consider to what extent more recent evidence supports or rebuts these six statements and to what extent they support the view of Blow *et al.*, (2005) who, on the basis of their review of recent research, conclude:

> *'Thus, while it is clear that there are sizeable differences between the outcomes experienced by children across the range of parental incomes, the evidence mostly implies that income does not cause these differences… Income transfer programmes are not a quick fix for poor child outcomes.'*
>
> (Blow *et al.*, 2005, p.6)

Jenkins and Schluter (2002) studied the effects of income on parental choice of secondary school in Germany using a relatively small dataset (n ≈ 500) constructed from the German Socio-Economic Panel. Although this particular outcome is not directly relevant to the findings reported here, the measures and statistical approach used by them are similar to those used in other research. In particular, they measure income as annual household income post-taxes and post-benefits (or transfers) and they average this income (unequivalised or unadjusted for household size) across three stages of childhood (birth to five years; six to ten years; 11 to 14 years). They report but do not, however, exploit the movement across income quartiles in their analyses and they are unable to include any controls for child 'ability'. Their findings support the first two statements above but they find income effects only for native German children and not for children from 'guestworker' households, they find that school choice is influenced more by income after early childhood, and that these income effects appear to be linear.

The paper by Aughinbaugh and Gittleman (2003) is a comparative study of the effects of income on a range of child outcomes in the United States and Great Britain. Their British data comes from the National Child Development Study (NCDS – the 1958 cohort) and, more particularly, from the children of the female cohort members measured when the cohort mothers were age 33 in 1991. The income measure is annualised income of the mother and her partner post-taxes and post-benefits and the authors consider both current income and income averaged over two time points (ages 23 and 33). The results are in line with Jenkins and Schluter's first four points. They find that a change in income of $10K 1991 dollars will change educational test scores by between 0.060 and 0.082 Standard Deviation (SD) units (point estimates that vary from test to test). This is a small effect, equivalent to a change of about one point on a standardised test with a mean of 100 and an SD of 15 (or about one month's progress on Key Stage tests used in English schools). The authors do not find any evidence of non-linear income effects, perhaps because the sample of poor families is rather small. They do not fit separate models for children of different ages.

Blanden and Gregg (2004) use data from NCDS, although they focus more on the 1970 cohort (BCS70) as well as on the British Household Panel Survey (BHPS). Using BCS70 data, they show that a one-third reduction in income at age 16 having controlled for income at age ten leads to an increase of between 1.1 per cent and 7.1 per cent (depending on model specification) in the risk of having no higher grade (i.e. A to C) GCSE qualifications, a decline of between 0.9 per cent and 3.9 per cent in the chances of staying on at school after age 16, and a decline of between 1.0 per cent and 5.6 per cent in the chances of having a degree by age 30. They supplement the banded income data collected in BCS70 (and discussed in Chapter 5) with data from the FES.

Taylor *et al.*, (2004) study a range of outcomes at age 36 months using data from a US study of early child care (SECC). They use a measure of banded annualised family income (18 bands) from all household members but it is not clear whether it is net of taxes. They average this measure over the five occasions at which it was collected between one and 36 months. Their results are in line with five of Jenkins and Schluter's six points (they were unable to test the effects for children of different ages). They do, however, interpret them differently, arguing that the sizes of the effects for poor children (about 0.10 SD units for a change of $10K), although small in absolute magnitude, are comparable with the effects found for pre-school interventions such as Early Head Start.

Dahl and Lochner (2005) use a sophisticated modelling approach (and rather strong identifying restrictions) to study the effects of changes in the amount of tax credits in the US. They use predicted rather than observed income to show that the effects of a $10K change in income would change maths attainment by 0.21 SD units and reading attainment by 0.36 SD units, that the effects are even stronger for more disadvantaged families but they do not vary by the age of the child. These effects are a good deal higher than those found by other researchers but their generalisability is limited to families in receipt of low incomes from the labour market.

Chevalier et al., (2005) analyse data from the UK Labour Force Survey and use staying on at school and educational qualifications as outcomes. Like Shea (2000), they use union membership as an instrumental variable for the earnings of the household head: earnings in unionised occupations are higher but belonging to a union does not, they argue, directly affect educational outcomes. They find quite substantial effects of income but, in the absence of any controls for child ability and with doubts about the validity of union membership as an instrument (as discussed in Plewis and Hawkes, 2005), their findings should be treated with caution.

The report by Lethbridge and Phipps (2006) focuses on Canadian data. They restrict analyses to those children in stable families: those that have not experienced a change in family structure over the study period. They use average and other functions of income over three time periods, income being measured as total pre-tax annual household income and equivalised by the square root of family size. Their findings are generally consistent with those found by other researchers although they focus more on associations than on causal effects and they do not find differences by age of child.

Finally, Berger *et al.*, (2006) use data from a US birth cohort of 'fragile families' to study income effects on outcomes for children age 36 months with total family income (not explicitly defined) averaged over three occasions of measurement. Their findings are similar to those of Taylor *et al.*, (2004) but are given a different interpretation: the income effects are small and, according to the authors, probably smaller than those obtained from a 'child care' intervention with a similar cost.

1.3 Summary

All of points i, ii and iv as set out in Section 1.2 have been supported by the evidence reviewed here. There is, however, some dispute about just how important income effects are when compared with, for example, the effects of early interventions. Not all researchers find differences according to the age of the child, and not all of them find non-linear effects i.e. differential income effects according to the level of income.

We end with the most important conclusion from the feasibility study for this project:

> *'…we believe there are unexploited data that can be used in a statistically appropriate way to shine some lights on a problem that is far from easy to answer. We stress the need for several lights rather than a single beam. The studies that can be used each have their strengths and weaknesses but they do not all share the same limitations. The separate estimates of the effects of income changes can be compared and brought together by a process of triangulation to generate conclusions. Such a project would be different from – and would build on – what has been done up to now because most of the extant research concentrates on estimating models from just one dataset.'*
>
> (Plewis and Hawkes, 2005, p. 47)

This is just what the research reported here does.

2 Methodological issues

Plewis and Hawkes (2005) discuss what an ideal study might look like when trying to disentangle the effects of income changes on child and adult outcomes from other related changes. We elaborate these ideas here by addressing three methodological issues: how to measure income; how to get a reasonable prediction of income from other sources; and how to specify and estimate statistical models that generate plausible causal inferences.

2.1 Measuring income

Income is measured (usually in surveys) in different ways by different researchers, especially when income measurement is not the main purpose of the survey. It is almost inevitable, given financial constraints and the wish not to over-burden respondents, that the range and depth of income data collected will vary inversely with the range and number of outcomes measured (Plewis *et al.*, 2001). It is clear from the review of recent research in Chapter 1 that there is considerable variation in the way income is recorded. Thus, in some studies, income for the previous 12 months is recorded, in others an annual figure is obtained by weighting up income for a shorter reference period such as the last week or last month. The questions used in some studies ask about income from different sources (labour market, savings and investments etc.) but, more commonly, a global figure is requested, often within a band. If banded, the number of income bands used varies from study to study and, sometimes, from occasion to occasion within a study. Some measures of income include income from all household members; others are restricted to income from parents or carers (i.e. family income). Some measures of income are 'net' (post taxes and post transfers), others are 'gross' (pre taxes), some include one type of transfer but exclude another. Some researchers deflate income so that it is measured at constant prices, others do not deflate. Some equivalise income to allow for changing household structure according to one of several available formulae (e.g. McClements, 1977); some ignore this issue altogether; others allow for changing household structure in the way they specify their statistical and econometric models. Mullan *et al.*, (2007) describe how the measured incomes of families not paying a full market rent might be supplemented by the imputed rent accruing to owner occupation and social housing. This is related to the question of whether to measure income before or after housing costs.

In later chapters we provide detailed descriptions of how income was actually measured in the studies used in this report. Here we reflect on some more general issues.

2.1.1 Current and permanent income

The distinction is often made between current income and longer-run average (or permanent) income when modelling the effects of income on outcomes. This raises some interesting methodological issues not least of which is how to define 'long-run'.

We can decompose observed income (INC) for household (or family) i at measurement occasion t (t = 1..T) as follows:

$$INC_{it} = PI_i + TI_{it} + u_i + v_{it} \qquad (1)$$

where PI is permanent income, TI is transitory income, u is the measurement error for permanent income and v is occasion specific measurement error. It is usual to assume:

a $E_t(TI_{it}) = E_t(v_{it}) = 0$ for all i where E_t is the expected value over occasions t;

b $E_i(u_i) = 0$ where E_i is the expected value over households i;

c all the terms on the right hand side of (1) are uncorrelated with each other;

d neither transitory incomes nor their measurement errors are autocorrelated (correlated across occasions).

Under the permanent income hypothesis (Friedman, 1957; Meghir, 2004), PI drives consumption behaviour. We might infer from this that it is permanent income that then drives the effect of income on outcomes like educational progress. This does, however, present some problems in empirical work: First, it is unclear whether permanent income is defined at the individual level or at the family or household level. If it is defined at the individual level then the concept is not especially useful when wishing to understand the ways in which families with children respond to changes in their combined incomes. If the concept applies to families or households then it is not clear how changes in the formation and reconstitution of households over time are accommodated. Second, it is not clear, even for stable households, what the appropriate time frame is: is PI fixed across the life course or does it change according to, for example, changes in macroeconomic circumstances (leading to more prosperity) or to changes in tax and benefits policy (leading to higher incomes for poorer families)?

Researchers in this field typically average observed income over the number of available measurement occasions in order to reduce measurement error and to get closer to the measure of permanent income that is implicitly assumed to affect outcomes. There are grounds for scepticism here. First, averaging over t in equation (1) might reduce occasion specific measurement error (v) but cannot reduce the measurement error that varies across households (u) – the tendency for some households always to over-report their income and others to under-report. Second, it is plausible to suppose that transitory income is itself autocorrelated, if only slightly, and so averaging over a small number of occasions (just two in the case of Aughinbaugh and Gittleman (2003), three for Lethbridge and Phipps (2006) and Berger *et al.*, (2006)) is not likely to eliminate the transitory component of observed income.

The alternatives to averaging observed measures of income are (i) to take differences in income (or, with more than two occasions, deviations from the individual's mean at each occasion) or (ii) to model the effects of income at time t+1 conditional on income at earlier times t-k, k ≥ 0. Both these alternatives imply that it is differences in transitory income that affect outcomes – because permanent income is differenced out in (i) and assumed to be part of the residual term in (ii).

Important as the distinctions between current, permanent and transitory income are theoretically, it is difficult to know just what the operational implications of the distinctions are for empirical work, especially when it is unusual for researchers to have available more than a few measures of income for a particular family or household. In this report, we use different functions of observed income (averages, differences, etc.) and look for consistency of findings across these different representations.

2.1.2 Equivalisation

There are a number of problems with methods of equivalising income when trying to establish causal relations of the kind that drives this report. Equivalising income is problematic for a number of reasons:

1 There a number of different equivalisations, each of which weights second and subsequent household members differently. Hence, estimates of the effects of income on outcomes could be sensitive to the scale chosen – see Banks and Johnson (1994) for a discussion of this issue.

2 Equivalisation should be applied differently according to whether family or household income is measured but the distinction between these two measures of income is not always clear.

3 The justification for equivalisation can be made more convincingly for cross-sectional and comparative analyses than it can in longitudinal work. In particular, **changes** in household size are arguably endogeneous to **changes** in income: for example, families choose to have another child as a result of an improvement or an expected improvement in their incomes. Suppose we equivalise by dividing by the square root of family size. A couple with one child would need a 15.5 per cent increase in income to maintain a constant equivalised income if they have a second child. However, their decision to have another child might be based on an expected improvement of less than 15.5 per cent and so their equivalised income falls. Under the hypothesis that a fall in (equivalised) income will have a detrimental effect on educational progress, their choice to have what they regard as an affordable second child will be expected to have a negative effect on the first child. This is somewhat implausible. Essentially the same argument will apply, even if a different method of equivalisation is chosen.

4 A related point is that equivalisation appears to be based around the idea of consumption: larger households can afford to buy fewer goods than smaller households with the same income, for instance. However, a change in household size can change the pattern of consumption so that, for example, a separating couple might need to make alternative childcare arrangements that will affect the income they have available to spend on their children. The main carer from a separating couple might have higher equivalised income but less money to spend on a child.

We have, therefore, taken the approach of using observed family or household income unadjusted for household size in this report. We do, however, sometimes use the number of children in the household as a control variable. Bradbury *et al.*, (2001, Chapter 2) present a more positive stance on equivalisation in their discussion of its pros and cons when studying the dynamics of child poverty. Their chapter also covers other methodological challenges that arise when studying income or poverty dynamics.

2.2 Alternative measures of income

Because measuring income accurately in surveys is not an easy task and also because item non-response to questions about income tends to be higher than for other questions, especially by the self-employed (see Hawkes and Plewis, 2006), we need to consider alternative approaches when using data from surveys without the necessary income data. One alternative is to use proxy measures for income, another is to replace banded income by a continuous measure and we outline two different ways of doing this.

2.2.1 Proxy measures for income

We consider two proxies for income and income changes in this report: benefit status and parental employment status. A third variable – material deprivation as measured by the ownership of consumer durables and by the ability to afford items such as new clothes for children and an evening out – is not considered here because it was not measured on a sufficiently consistent basis in the studies we have used. It could also be argued as Bradbury *et al.*, (2001, Chapter 2) do that it is consumption rather than income that is the important influence on outcomes: not so much whether income rises or falls but how these increases and declines are managed by families in terms of what they spend on their children. It is quite possible that parents ‘protect’ that part of the family budget that goes on their children, particularly during short-term periods of hardship. Plewis (2007a) suggests that consumption might be treated as a variable that mediates or explains the link between income and outcomes. We do not, however, know of any longitudinal studies that bring together detailed breakdowns of consumption and measures of outcomes.

By benefit status, we mean whether or not the family is receiving means tested cash benefits such as unemployment benefit (now Jobseeker’s Allowance (JSA)) and tax credits (but not universal benefits like Child Benefit). For change in benefit status from one occasion to another, we generate a four category variable:

1 Not on means tested benefit at either occasion.

2 On benefit at occasion one but not at occasion two (exits).

3 On benefit at occasion two but not at occasion one (entrants).

4 On benefit at both occasions.

The last of these four categories is likely to include many families living in persistent poverty. Categories two and three are the most useful in terms of understanding the effects of income changes because we might reasonably assume that most entrants into benefit status are on a downward economic trajectory, whereas the circumstances of most of those exiting from benefits are improving. This is, however, an assumption that is not easily verified by other data and it is likely that some members of these two ‘change’ categories make rather frequent transitions into and out of benefits as their circumstances marginally improve or deteriorate. The argument that some families are definitely improving or declining as they cross the benefits threshold can be made with more force when we have more than two measures of benefit status. We return to this issue in Chapter 4 when we use annual measures of free school meals status over four years to generate groups of entrants to, and exits from, benefit status. For now, we note that changes in benefit status are relatively unambiguous and less subject to problems of item non-response than income measures are but that:

1 We usually only have measures of benefit status at the measurement occasions; we know nothing about changes in the intervening periods or anything about the timing of any change. Thus, it is possible that families exiting from benefits have had greater exposure to the low income that is related to being on benefits over the period in question than the entrants.

2 Changes in benefit status mean only that a household has crossed an income threshold; we know nothing about the size of the change represented by crossing this threshold.

3 Any changes in the rules for eligibility for means-tested benefits could introduce problems of interpretation into the measure of change.

4 The interpretation, in terms of income change, of a change in benefit status will depend on just which benefits are included in the definition of being on benefits. For example, a move out of JSA into the receipt of tax credits (and thus, into employment) implies an improvement in economic circumstances that might not be picked up if a broad definition of benefit status is used.

Increases or reductions in the number of hours supplied by one or both parents to the labour market are likely to be associated with changes in income. We score parental employment status (for two parent families) at occasion t by assigning a score of 0 if neither parent is employed, 1 if one parent works full-time and the other does not work, 2 if both parents work full-time, 0.5 if the mother works part-time and the father does not work and 1.5 if the mother works part-time and the father works full-time. We expect increases (decreases) in this score to be associated with better (worse) outcomes because of their association with changes in income. We must, however, be aware that substitution effects between parents could lead to a rise in income without any corresponding increase in labour supply. Also, a decline in labour supply (and the consequent substitution of time with children for time in the labour market) might actually benefit children (and vice-versa). And, as with changes in benefit status, we know nothing about the existence and timing of changes between measurement occasions. In principle, a similar measure can be constructed for lone parent families but, because numbers of lone parents are small in the datasets used here, we do not use such a measure.

2.2.2 Replacing income bands: the Singh-Maddala approach

Singh and Maddala (1976) propose a flexible three parameter distribution for income. The Singh-Maddala distribution can be fitted to interval values of income in STATA using the 'smfit' procedure. A common procedure when income is measured in bands is to allocate the mid-point of each band to generate a quasi-continuous measure but this does assume a uniform distribution within bands. Modifying the 'smfit' procedure to deal with ordered categories is arguably more appropriate and we used a modified STATA procedure that allowed us to do this. We compare the mid-points of income bands with the Singh-Maddala estimates in Chapter 5.

2.2.3 Predicting income from other surveys

Plewis and Hawkes (2005) raised the possibility of predicting income from other variables known to be related to income and then using these predicted or imputed values in models relating changes in income to outcomes. The approach taken here, described in detail in Chapter 6, has been to use (i) the Family Expenditure Survey (FES) and (ii) the New Earnings Survey (NES), both for 1980 and 1986, to generate predictive equations that were then used to replace the income bands from British birth cohort 1970 (BCS70) at ages ten and 16 (see Chapter 5) by predicted income. In turn, these predicted or imputed incomes were used in some of the substantive models of interest.

2.3 Statistical modelling

Our modelling strategy is based on the need to find solutions to the self-selection problem: the fact that changes in income are usually endogeneous in the sense that the characteristics of families whose incomes increase or decline are themselves associated with our outcomes of interest. For example, one person's income goes up as a result of choosing to undertake training whereas another person's goes down because they were unable or unwilling to update their skills. These are endogeneous changes; the people select themselves into (i.e. choose) situations that affect their worth in the labour market and these people are likely to be more or less positive about the value of education for their children. We would much prefer to look at the effects of exogeneous changes but, unfortunately, our samples contain, for example, very few winners of lottery prizes. Longitudinal data are essential if we are to eliminate the effects of self-selection but they are not a panacea.

2.3.1 Regression models

All the substantive results in this report are generated from estimates of coefficients in regression models of different kinds. Plewis (1997, Ch. 1, 2 and 5) provides, in a relatively non-technical way, all the important underpinnings for the models used here. We explain when and how these estimates might reasonably be interpreted as causal effects in the next section. For outcomes measured on interval scales – educational attainment for example – we estimate multiple regression models that include the explanatory variable of interest (income or a variable related to income) and other control variables chosen to reduce the self-selection problem. Our primary interest is in the estimates of the coefficients for income in the model. We are less interested in the overall fit of the model to the data, often measured by R^2. The estimates of R^2 are often quite low for analyses of this kind, particularly when modelling change, but this need not detract from the most important piece of information from the model: the size of the income effect.

Some of the outcomes of interest – the measures of behaviour especially – are binary (no problem = 0/problem = 1). This does not alter our general approach except that we fit logistic regression models by maximum likelihood (ML) rather than multiple regression models by ordinary least squares (OLS). Estimating the fit of logistic regression models is not straightforward: there are a number of 'pseudo-R^2' measures in the literature but they can vary in size for the same dataset and do not have the same unambiguous interpretation that R^2 has in multiple regression. Consequently, we do not use any measures of pseudo-R^2 in this report.

We always present estimates of regression coefficients and their standard errors. We adopt a flexible approach to statistical inference rather than focus on a particular level of statistical significance (0.05 say) as this is often little more than a function of sample size. We look for consistent patterns of results across datasets, test estimates of regression coefficients in appropriate ways depending on the model from which they are derived and focus as much on effect sizes as on deviations from zero that are not due to chance.

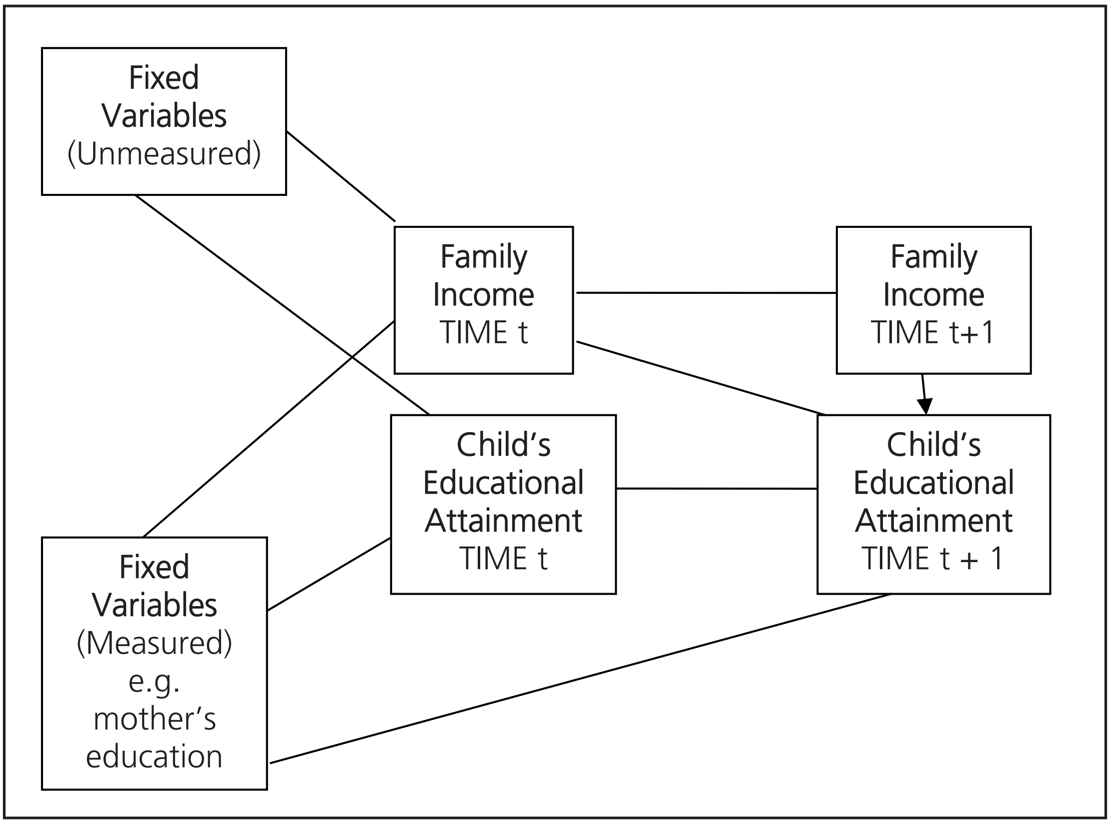
2.3.2 Causal models

The basic model we work with is shown in Figure 2.1 for two occasions. Our particular interest is in the effect of income (or benefit status or employment status) at time t+1 on educational attainment (or other outcomes) then (the arrowed line), having controlled for the effects of income and attainment measured at time t. We assume that the effects of fixed unmeasured or unobserved variables act through income just at time t and not also through income at time t+1. If this assumption does not hold, estimates of income effects will be biased. These unmeasured variables, for example 'motivation', are often bundled together and labelled 'unobserved heterogeneity'. We can, however, allow the measured variables like mother's education to affect progress (i.e. attainment at times t and t+1) by including such variables as control variables in our models.

Figure 2.1 represents the model when we look at the effects of income change in terms of income at time t+1 conditional on income at time t but a model that represents income change as a difference is essentially the same. If, however, instead of controlling for prior income, we represent income as an average over the measurement occasions, then to the extent that there are effects of unmeasured variables that are not controlled by educational attainment at time t then these estimates of income measured as an average will be biased.

Note that there is no need to deflate income to constant prices when using the conditional model of Figure 2.1. The interpretation of average income and income differences is perhaps easier if incomes are adjusted to take account of changing prices. Deflating income will not, however, have any important effects on parameter estimates from longitudinal models and we do not generally use adjustments for changing prices in the results presented in this report.

Figure 2.1 Model for relating changes in income to educational progress



An alternative approach, commonly used in econometric analysis ('differences in differences' models) but not used here, is to relate differences in income to differences in attainment. The difficulties with this approach are: (i) differences in outcomes like educational attainment are problematic when different measures of attainment are used at different occasions; (ii) although the approach can eliminate the effects of unmeasured fixed effects it does so at the cost of not being able to estimate interactions or moderator effects.

We write the regression model corresponding to Figure 2.1 as follows:

$$y_i = b_0 + \sum_k b_k x_{ki} + \sum_l c_l z_{li} + e_i \qquad (1)$$

where:

i y_i is the educational outcome (assumed continuous) for case *i* at time t+1

ii X_k are the explanatory variables of interest (K measures of income at time t+1 where K can be > 1) and b_k are their regression coefficients

iii Z_i denote the L control variables where L is usually > 1 and C_k are their regression coefficients. The set of control variables will include:

a prior measures (at time t) of the outcome and income;

b variables such as mother's education that are usually regarded as fixed;

c time t measures of time-varying variables such as the number of children in the household.

iv e_i is the residual term for case *i*

Note that we do not include measures of time-varying variables that might also be outcomes of any process that links income to child outcomes, for example, lone parent status. If, however, we include controls for, say, both mother's and father's educational levels, we do restrict the generalisability of our findings to two parent families at time t.

It is reasonable to suppose that if income is causally related to educational and other outcomes, this relationship should be non-linear. If a rise in annual family income from £10K to £20K has the same effect as a rise from £50K to £60K, this suggests that the model is mis-specified. It is easy to postulate processes that could explain why the move out of poverty for a low-income family could benefit children but more difficult to see why a rise of the same size would benefit children in a well-off family. We can represent non-linear effects of income by modelling log income or by including linear and quadratic terms in income, expecting the linear term to be positive and the quadratic term to be negative. With the conditional model (1), we could include an interaction between incomes at times t and t+1 to represent non-linear effects.

Rather than including income at times t and t+1 in the model, we sometimes use dummy variables to represent categories of changing binary variables such as benefit status (see Section 2.2.1). Also, if our outcomes are binary as they are, for example, for some measures of behaviour, then model (1) can be written as a logistic regression:

$$\text{logit } P_i = b_0 + \sum_k b_k x_k + \sum_l c_l z_{li} \qquad (2)$$

where P_i is the probability of, say, a behaviour problem for case *i* and the right hand side of (2) is essentially the same as (1).

We might want to allow for the possibility that income effects are moderated by background variables like housing tenure (see Plewis and Hawkes, 2005, p.32), and this we can do by including interactions between x and z in models like models (1) and (2).

In some cases, we have more than one related outcome – a set of educational test scores, different aspects of behaviour – and we then model these as a multivariate set where appropriate. This has the advantage of using the correlations between these related outcomes to improve the efficiency of estimation. Thus model (1) becomes:

$$y_{ij} = b_{0j} + \sum_k b_{kj} X_{kij} + \sum_l c_{lj} z_{lij} + e_i + v_{ij} \qquad (3)$$

where J (j = 1..J) is the number of outcomes under consideration. We use the multilevel package *MLwiN* (Rasbash *et al.*, 2004) for estimation. The logistic regression model (2) can be extended in a similar way.

3 The effects of changes in parental economic circumstances in early childhood on outcomes in early and middle childhood and adulthood

In this chapter, we concentrate on the effects of changing circumstances in early childhood (from birth up to the age of six) on outcomes both then and later in life. The literature reviewed in Chapter 1 suggests that this is the period when we might expect to find the most substantial effects of income and other changes. The chapter is organised into three separate sections: Section 3.1 uses data from the first two waves of the Millennium Cohort Study (MCS), Section 3.2 uses data from Birth Cohort Study 1970 (BCS70) and Section 3.3 uses data collected from the children of the BCS70 cohort when the cohort members were age 34.

3.1 Evidence from the Millenium Cohort Study

3.1.1 Introduction to MCS

The MCS is the fourth in the series of internationally renowned cohort studies in the UK. It includes 18,818 babies in 18,552 families born in the UK over a 12-month period during the years 2000 and 2001 and living in selected UK electoral wards at age nine months. Areas with high proportions of Black and Asian families, disadvantaged areas and the three smaller UK countries are all over-represented in the sample which is disproportionately stratified and clustered. The first and second waves took place when the cohort members were (approximately) nine months and three years old respectively. In nearly all cases the mother was the main respondent; partners were interviewed whenever possible. Questions about earnings and family income were included in both waves. Further details about the first wave of MCS can be found in Plewis (2007c) and Dex and Joshi (2005). Plewis and Ketende (2006) present response rates at wave two when data were collected from 79 per cent of the wave one sample.

3.1.2 Measures used

Three outcome measures are used for the children age three, two cognitive/educational assessments and one measure of the child's behaviour. The cognitive assessments are the naming vocabulary (i.e. naming pictures) subtest of the British Ability Scales (BAS) and the School Readiness Composite of the Revised Bracken Basic Concepts Scale (Bracken, 1998). Behavioural adjustment of the children is measured by the Strengths and Difficulties Questionnaire (SDQ), assessed by parental report and generating an overall score as well as scores for five sub-scales as described by Goodman (1997). Following George *et al.*, (2007), we use a score based on the four sub-scales that represent difficulties: emotional, conduct and peer problems and hyperactivity.

We do not adjust cognitive scores for age but include age at test in our models to allow for the fact that not all children were tested at exactly 36 months (the range was from 31 months to 54 months but 75 per cent were within one month of the target age). The Bracken score is skewed with a long tail to the right and so we use a square root transformation to bring it closer to Normality. The SDQ score is also somewhat skewed to the right and was transformed to Normality by using a power transformation of 2/3. We do not transform the BAS score as its distribution was close to being Normal. All scores are further z-transformed to mean zero and unit standard deviation so that they can easily be compared with each other. We analyse the BAS and Bracken scores separately because it is then much easier to represent the stratified and clustered aspects of the sample design.

A number of different income and income-related measures are used. The measure of net family income was based on a response to a prompt card with the main respondent asked to assess overall net family income (main respondent plus partner or just main respondent in lone parent families) in one of 18 bands. In wave one, respondents were given the option of choosing weekly, monthly or annual amounts but in wave two the choice was restricted to an annual figure. The bands were different for two and one parent families and the bands at wave two allowed for wage inflation. Because the bands are relatively narrow, we did not use the Singh-Maddala procedure described in Section 2.2.2 but instead translated all the banded income measures into quasi-continuous measures by taking the mid-point of each band (see Plewis, 2007c, Table A2.10).

In line with, for example, Taylor *et al.*, (2004) we average this measure of family income over the two waves. We use the log of this average as this gives a better fit to the data than the actual income does (see Section 2.3.2). We also model change in income in two ways: The first is by including net family income at wave one as a control and then examining the effects on outcomes of income at wave two. The second approach to income change is to calculate an income difference and to use this in the models.

We use claiming at least one of Working Families' Tax Credit (WFTC), Disabled Persons Tax Credit (DPTC), Income Support (IS) and Jobseeker's Allowance (JSA) (either contribution-based or income-based) to define benefit status, although other definitions are possible. For example, Bradshaw *et al.*, (2005) use a tighter definition by restricting 'on benefits' to those families claiming at least one of IS, JSA together with either Housing Benefit (HB) or Council Tax Benefit (CTB), and WFTC together with either HB or CTB. One advantage of the change in benefit status variable is that there is less item non-response for questions about benefits than there is for questions about income. Hawkes and Plewis (2006) describe the extent and correlates of missing income data in MCS.

We include, in our models, a number of control variables that are expected to be associated with the cognitive test scores and SDQ, to try to reduce any problems generated by the fact that we do not, indeed could not, have corresponding measures of cognitive ability and behaviour from wave one (at age nine months). The following control variables are used in at least one of our final models (others, such as ethnic group, were tried but were found to be unimportant):

1 A measure of developmental delay at age nine months as a proxy for cognitive ability, obtained by summing the number of items from the Denver Developmental Screening Test and the MacArthur Communications Developmental Inventory where the main respondent's response indicated a delay.

2 Three measures of temperament at age nine months: mood, adaptability and regularity from the Carey Infant Temperament Scale.

3 Whether or not the child had a diagnosed hearing problem at nine months.

4 Whether or not the child had been admitted to hospital up to age nine months.

5 Whether or not the child had any accidents that led to contact with the health services up to age nine months.

6 Birth weight (grams).

7 The educational level (National Vocational Qualification (NVQ) categories) of the main respondent.

8 Housing tenure at wave one: owner occupied or other.

9 Number of children in the household.

3.1.3 Results: average income

N.B. Throughout this report, sample estimates are presented to two significant figures.

Table 3.1 gives the estimates of the effect of the log of average income (measured in thousands of pounds) on the two cognitive tests, controlling for developmental delay, hearing problem, educational level of the main respondent, housing tenure and hospital admissions. Tables A.1 and A.2 give summary statistics for these variables for: (i) the observed sample and (ii) the analysis sample. The results for the control variables are in the expected directions although, having allowed for the sample design, the estimate for hospital admissions is not statistically significant at the five per cent level (its omission from the model does not affect the results). The introduction of the control variables reduces the estimate for log average family income (after controlling for age at test) from 0.45 to 0.27 for Bracken and 0.35 to 0.17 for BAS.

The highlighted row in Table 3.1 shows that average log income is related to the cognitive scores after allowing for other variables. Because the relation with income is logarithmic, the same increase in average family income has a decreasing effect as average income rises: doubling income raises the Bracken score by 0.19 Standard Deviation (SD) units and the BAS score by 0.12 SD units. Another way of expressing this is to say that the effect of a rise of £10K in average annual family income from £10K (equivalent to a rise of 0.69 units on the log scale) raises the Bracken score by 0.19 SD units (i.e. 0.27 x 0.69) but the same increase from £30K (equivalent to a rise of 0.29 units on the log scale) only raises the score by 0.08 SD units. For BAS, the corresponding increases are smaller: 0.12 SD units (i.e. 0.17 x 0.69) and 0.05 SD units. Another way of expressing the sizes of these effects is to say that doubling average family income raises the Bracken score by roughly the equivalent of about two-and-a-half-months' progress for a child age 36 months and the BAS score by roughly the equivalent of two-months' progress. It is worth noting that the difference between the main respondent having no educational qualifications and having a higher degree (NVQ5), holding constant all other variables including average income, is 0.61 (= 0.48 - (-0.13) from rows 5 and 11 of Table 3.1) which is over twice the size of the effect of doubling income.

Table 3.1 Modelling average family income, MCS1 and 2: cognitive outcomes

	Bracken		BAS	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log average family income	0.27	0.022	0.17	0.019
Developmental delay	-0.13	0.029	-0.14	0.024
Hearing problem				
No (reference)	0	n.a.	0	n.a.
Yes	-0.086	0.035	-0.11	0.031
Birth weight	-	-	0.099	0.017
Main respondent's educational qualifications				
No qualifications	-0.13	0.052	-0.45	0.099
Overseas; other qualifications only	-0.024	0.094	-0.22	0.059
NVQ1 (reference)	0	n.a.	0	n.a.
NVQ2	0.15	0.044	0.11	0.044
NVQ3	0.22	0.046	0.20	0.048
NVQ4	0.34	0.043	0.27	0.048
NVQ5	0.48	0.066	0.23	0.066
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.13	0.030	0.14	0.029
Hospital admissions				
No (reference)	0	n.a.	0	n.a.
Yes	-0.046	0.030	-	-
Mean number of children, waves 1 and 2	-0.17	0.011	-0.13	0.011
Age at test	0.28	0.013	0.19	0.014
Sample size	9,267		9,668	
R^2	0.21		0.15	

Average log family income is also related to behaviour as measured by the SDQ. The introduction of the control variables changes the estimate for log average family income from -0.42 to the -0.21 highlighted in Table 3.2. Doubling average family income from £10K to £20K reduces the SDQ score by 0.14 SD units (i.e. improves behaviour) but the effect is smaller when initial incomes are higher. Again, we find a substantially greater difference between the top and bottom of the scale for the main respondent's educational qualifications (= -0.53) than we do for income. Table A.3 gives summary statistics for the SDQ and the temperament variables for the observed sample.

Table 3.2 Modelling average family income, MCS1 and 2: SDQ

	SDQ	
Variable	Estimate	S.E.
Log average family income	-0.21	0.018
Developmental delay	0.083	0.026
Temperament		
Mood	-0.15	0.016
Adaptability	0.10	0.015
Regularity	-0.13	0.014
Birth weight	-0.042	0.018
Main respondent's educational qualifications		
No qualifications	0.15	0.055
Overseas; other	-0.088	0.089
NVQ1 (reference)	0	n.a.
NVQ2	-0.098	0.044
NVQ3	-0.19	0.043
NVQ4	-0.32	0.043
NVQ5	-0.38	0.064
Tenure		
Rent (reference)		
Own	-0.17	0.028
Accidents		
No (reference)		
Yes	0.11	0.036
Sample size	10,892	
R^2	0.15	

3.1.4 Results: wave two income conditional on wave one

Table 3.3 shows the effects of log family income at wave two on the Bracken and BAS assessments, having controlled for log family income at wave one (and other variables as in Table 3.1). Doubling income at wave two for fixed income at wave one would raise the Bracken score by 0.08 SD units and the BAS score by 0.05 SD units. The alternative specification that includes income at waves one and two and an interaction between them gives a slightly less good fit than the specification with just the main effects of log income at both waves used here.

Note that the models underpinning Tables 3.1 and 3.3 are related in that the model for Table 3.1 constrains the coefficients for log income to be the same for the two waves. The model for Table 3.3 is, therefore, more general although the equivalence between the two models would not hold with more than two waves of data and family income averaged over all measurements.

We find that doubling family income at wave two for fixed income at wave one leads to a decline of 0.06 SD units in the SDQ score. See Table 3.4 for the model estimates.

Table 3.3 Modelling family income at MCS2 conditional on family income at MCS1: cognitive scores

	Bracken		BAS	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log family income, MCS2	0.11	0.017	0.062	0.016
Developmental delay	-0.13	0.029	-0.14	0.023
Hearing problem	-0.083	0.034	-0.11	0.031
Birth weight	-	-	0.099	0.017
Main respondent's educational qualifications				
No qualifications	-0.13	0.052	-0.45	0.099
Overseas; other	-0.030	0.094	-0.22	0.059
NVQ1 (reference)	0	n.a.	0	n.a.
NVQ2	0.16	0.044	0.11	0.045
NVQ3	0.22	0.046	0.20	0.049
NVQ4	0.35	0.043	0.28	0.048
NVQ5	0.49	0.065	0.24	0.066
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.14	0.030	0.14	0.029
Hospital admissions				
No (reference)	0	n.a.	0	n.a.
Yes	-0.044	0.030	-	-
Number of children, MCS1	-0.16	0.010	-0.13	0.010
Log family income, MCS1	0.13	0.020	0.097	0.017
Age at test	0.28	0.013	0.19	0.014
Sample size	9,267		9,668	
R^2	0.21		0.15	

Table 3.4 Modelling family income at MCS2 conditional on family income at MCS1: behaviour score

	SDQ	
Variable	**Estimate**	**S.E.**
Log family income, MCS2	-0.082	0.016
Developmental delay	0.084	0.026
Temperament		
Mood	-0.15	0.016
Adaptability	0.10	0.015
Regularity	-0.13	0.014
Birth weight	-0.042	0.018
Main respondent's educational qualifications		
No qualifications	0.15	0.055
Overseas; other	-0.088	0.089
NVQ1 (reference)	0	n.a.
NVQ2	-0.10	0.044
NVQ3	-0.20	0.043
NVQ4	-0.33	0.043
NVQ5	-0.40	0.064
Tenure		
Rent (reference)	0	n.a.
Own	-0.18	0.028
Hospital admissions		
No (reference)	0	n.a.
Yes	0.10	0.036
Log family income, MCS1	-0.11	0.017
Sample size	10,892	
R^2	0.15	

3.1.5 Results: income difference, wave one to wave two

Table 3.5 shows the effects of the difference in family income between waves one and two on the Bracken and BAS assessments, having controlled for other variables as in Table 3.1. The relationship is quadratic but the effect is small for the Bracken – a difference of £20K leads to an increase in the test score of just 0.02 SD units – and is not significantly different from zero for the BAS. The introduction of the control variables reduces the estimates for the functions of income difference (after controlling for age at test) from 0.0017 and 0.00013 (linear and quadratic) to 0.00038 and 0.000029 for Bracken and 0.0011 and 0.000073 to -0.00019 and -0.000013 for BAS.

The effect is also small for the SDQ: a difference of £20K leading to a fall in the behaviour score of just 0.03 SD units (see Table A.4).

Table 3.5 Modelling difference in family income from MCS1 to MCS2: cognitive outcomes

	Bracken		BAS	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Family income difference				
Linear	0.00038	0.00067	-0.00019	0.00065
Quadratic	0.000029	0.000014	-0.000013	0.000019
Developmental delay	-0.13	0.030	-0.14	0.024
Hearing problem				
No (reference)	0	n.a.	0	n.a.
Yes	-0.079	0.035	-0.11	0.032
Birth weight	-	-	0.10	0.017
Main respondent's educational qualifications				
No qualifications	-0.17	0.053	-0.45	0.098
Overseas; other	-0.036	0.091	-0.25	0.059
NVQ1 (reference)	0	n.a.	0	n.a.
NVQ2	0.20	0.045	0.14	0.045
NVQ3	0.28	0.047	0.24	0.048
NVQ4	0.47	0.043	0.36	0.048
NVQ5	0.66	0.067	0.36	0.066
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.31	0.026	0.26	0.024
Hospital admissions				
No (reference)	0	n.a.	0	n.a.
Yes	-0.057	0.030	-	-
Number of children, MCS1	-0.15	0.011	-0.12	0.010
Age at test	0.27	0.014	0.19	0.014
Sample size	9,267		9,668	
R^2	0.19		0.14	

3.1.6 Change in family benefit status, wave one to wave two

We find that 52 per cent of families are not on benefits/tax credits on either occasion, 24 per cent are on benefits on both occasions, 16 per cent move on to benefits and nine per cent move off (these percentages are weighted to take account of the sample design). We assume that families coming off benefits are on an upward economic path and those moving on to benefits are on a downward path as explained in Chapter 2 and again in Chapter 4. A change in benefit status is associated with a change in the number of parental figures in the household as shown by the emboldened figures in Table 3.6.

Table 3.6 Benefit status change and change in number of parental figures in the household: MCS1 and MCS2

	MCS1: NB, MCS2: NB (%)	MCS1: B, MCS2: NB (%)	MCS1: NB, MCS2: B (%)	MCS1: B, MCS2: B (%)	
MCS1:1P, MCS2:1P	3.1	7.6	6.6	83	100
MCS1:1P, MCS2:2P	6.0	**29**	**6.7**	58	100
MCS1:2P, MCS2:1P	12	**4.9**	**33**	50	100
MCS1:2P, MCS2:2P	54	10	16	20	100

Notes:
NB: not on benefits; B: on benefits.
1P: one parent; 2P: two parents.
Unweighted percentages; n=14725.

We do not, however, find the expected effects of benefit change on the educational test scores as shown in Table 3.7: entrants to, and exits from, benefits – the highlighted rows – do equally less well when compared with families not on benefits on either occasion in terms of test scores. Being on benefits on both occasions does, however, lead to lower scores on both Bracken and BAS. These children are about three months behind their peers who were not on benefits at either occasion. And the story is the same for SDQ (Table A.5): no effect for a change in benefit status but poorer child behaviour for families on benefits at both waves.

Table 3.7 Modelling change in family benefit status from MCS1 to MCS2: cognitive scores

	Bracken		BAS	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change in benefit status				
No (MCS1); No(MCS2) (ref.)	0	n.a.	0	n.a.
Yes (MCS1); No (MCS2)	-0.14	0.037	-0.071	0.035
No (MCS1); Yes (MCS2)	-0.12	0.027	-0.091	0.026
Yes (MCS1); Yes (MCS2)	-0.29	0.029	-0.20	0.030
Developmental delay	-0.14	0.026	-0.14	0.021
Hearing problem				
No (reference)	0	n.a.	0	n.a.
Yes	-0.076	0.033	-0.10	0.028
Birth weight	-	-	0.12	0.015
Main respondent's educational qualifications				
No qualifications	-0.19	0.049	-0.46	0.089
Overseas; other	-0.056	0.083	-0.29	0.059
NVQ1 (reference)	0	n.a.	0	n.a.
NVQ2	0.16	0.040	0.13	0.040
NVQ3	0.23	0.042	0.19	0.043
NVQ4	0.41	0.038	0.32	0.044
NVQ5	0.57	0.060	0.29	0.060
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.21	0.027	0.18	0.026
Hospital admissions				
No (reference)	0	n.a.	0	n.a.
Yes	-0.053	0.027	-	-
Number of children, MCS1	-0.15	0.0097	-0.12	0.0094
Age at test	0.27	0.012	0.20	0.014
Sample size	11,348		11,889	
R^2	0.20		0.16	

3.2 Evidence from British Cohort Study 1970 (BSC70)

3.2.1 Introduction to BCS70

BCS70 – the third in the series of the British birth cohort studies – has followed all children born in Great Britain in a single week in April 1970 from birth to age 34. Details of response, etc. up to age 30 can be found in Plewis *et al.*, (2004). We focus on the first three waves here when the longitudinal sample numbered 16,571 at birth, 12,981 at age five and 14,350 at age ten.

3.2.2 Measures used

There are two useable measures of cognitive skills at age five: the English Picture Vocabulary Test (EPVT), similar to the BAS test used in the second wave of MCS and a copying designs test. There are reading (the Shortened Edinburgh) and mathematics (the Friendly Maths) tests at age ten. Standardising (i.e. z) transformations are applied to all the cognitive tests. Behaviour was measured by the Rutter scales, separately for emotional, conduct and attention problems, at ages five and ten. These sub-scales were dichotomised in such a way that all scores above zero were coded '1'.

The only explanatory variable available is change in parental employment status as defined in Chapter 2. There are also rather few suitable control variables from wave one of BCS70: we use sex, birth weight, the social class of the father of the cohort member's mother and father (parental social class was not available). See Tables A.6 and A.7 for all the relevant descriptive statistics.

3.2.3 Results: outcomes at age five

Table 3.8 Modelling change in parental employment status from birth to five, BCS70: cognitive scores at age five

	EPVT		Copying designs	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status				
<= -1	-0.19	0.058	-0.22	0.054
-0.5	-0.015	0.070	-0.063	0.063
0 (reference)	0	n.a.	0	n.a.
0.5	0.040	0.032	0.0030	0.030
>=1	-0.23	0.055	-0.13	0.051
Sex				
Boy (reference)	0	n.a.	0	n.a.
Girl	-0.14	0.029	0.023	0.026
Birth weight	0.10	0.015	0.12	0.013
Father's father's social class				
I (reference)	0	n.a	0	n.a.
II	-0.24	0.11	-0.27	0.086
IIINM	-0.17	0.11	-0.27	0.091
IIIM	-0.28	0.10	-0.37	0.083
IV	-0.36	0.11	-0.42	0.086
V	-0.31	0.11	-0.48	0.093
Mother's father's social class				
I (reference)	0	n.a.	0	n.a.
II	-0.10	0.10	-0.25	0.085
IIINM	-0.089	0.10	-0.23	0.091
IIIM	-0.23	0.094	-0.43	0.082
IV	-0.37	0.097	-0.50	0.086
V	-0.31	0.10	-0.46	0.093
Sample size	4,357		5,571	

The estimates in Table 3.8 are based on a bivariate regression model (see Chapter 2). We see that there is an overall effect of change in parental employment status on cognitive scores at age five. The effects are, however, concentrated in the two extreme categories (± 1) and both are negative. As nearly all members of these categories will be fathers moving into or out of unemployment, these effects are probably measuring exposure to unemployment during this five-year period.

The estimates in Table 3.9 are based on a multivariate logistic regression model with three binary outcomes (see Chapter 2). Conduct and attention problems are more likely at the two extremes of the employment status change variable but there is a suggestion that emotional problems decline with a move into full-time employment.

Overall, there are no clear-cut findings for change in parental employment status on outcomes of the cohort members at age five.

Table 3.9 Modelling change in parental employment status from birth to five, BCS70: behaviour problems at age five

	Emotional		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status						
<= -1	0.082	0.12	0.32	0.12	0.18	0.12
-0.5	-0.20	0.14	0.19	0.14	-0.015	0.15
0 (reference)	0	n.a.	0	n.a.	0	n.a.
0.5	-0.17	0.064	0.047	0.064	-0.053	0.071
>=1	-0.28	0.11	0.30	0.11	0.19	0.12
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.16	0.056	-0.71	0.057	-0.16	0.062
Birth weight	-0.026	0.028	-0.043	0.028	-0.056	0.031
Father's father's social class						
I (reference)	0	n.a	0	n.a	0	n.a
II	0.054	0.19	0.095	0.19	0.76	0.26
IIINM	0.11	0.20	0.069	0.20	0.81	0.28
IIIM	0.23	0.18	0.16	0.18	0.89	0.26
IV	0.30	0.19	0.23	0.19	1.0	0.26
V	0.16	0.21	0.40	0.21	1.0	0.28
Mother's father's social class						
I (reference)	0	n.a.	0	n.a.	0	n.a.
II	-0.20	0.18	0.36	0.20	0.058	0.22
IIINM	-0.27	0.20	0.36	0.21	0.26	0.23
IIIM	-0.15	0.17	0.64	0.19	0.26	0.21
IV	-0.14	0.18	0.63	0.20	0.33	0.22
V	-0.11	0.20	0.66	0.21	0.32	0.23
Sample size	5,493		5,542		5,516	

3.2.4 Results: outcomes at age ten

Given the absence of any clear-cut findings from the previous section, it would be surprising to find effects of the same variable on outcomes at age ten. Indeed, the effects on the reading and mathematics tests then are small. Table 3.10 gives the results for the behaviour problems and shows that, if anything, the estimates are somewhat larger than they are in Table 3.9. This might be explained by events between ages five and ten that are associated with the change in employment status between birth and five or it could be evidence of a 'sleeper' effect: the changes between birth and five have a delayed effect on behaviour problems. We return to this issue in Chapter 4.

Table 3.10 Modelling change in parental employment status from birth to five, BCS70: behaviour problems at age ten

	Emotional		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status						
<= -1	0.00	0.15	0.36	0.15	0.32	0.15
-0.5	-0.32	0.19	0.14	0.18	0.30	0.17
0 (reference)	0	n.a.	0	n.a.	0	n.a.
0.5	-0.10	0.084	-0.066	0.091	0.16	0.088
>=1	-0.39	0.16	0.20	0.15	0.54	0.14
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.086	0.075	-0.59	0.081	-0.57	0.078
Birth weight	0.010	0.038	-0.069	0.040	-0.065	0.039
Father's father's social class						
I (reference)	0	n.a	0	n.a	0	n.a
II	0.35	0.28	0.68	0.37	0.60	0.34
IIINM	0.29	0.29	0.57	0.38	0.72	0.35
IIIM	0.40	0.27	0.87	0.36	0.79	0.33
IV	0.29	0.28	1.0	0.36	0.83	0.34
V	0.33	0.30	0.92	0.38	1.1	0.35
Mother's father's social class						
I (reference)	0	n.a.	0	n.a.	0	n.a.
II	-0.041	0.25	0.39	0.34	-0.14	0.28
IIINM	0.022	0.27	0.49	0.35	0.097	0.29
IIIM	0.057	0.24	0.72	0.33	0.16	0.26
IV	-0.072	0.25	0.78	0.33	0.33	0.27
V	-0.12	0.27	0.91	0.35	0.076	0.29
Sample size	4,706		4,701		4,726	

3.2.5 Results: earnings at age 34

In this section, we consider whether there is any long-term effect of changes early in life on later earnings. More specifically, we examine the effect of a change in parental employment status (and, therefore, perhaps a change in family income) between birth and age five on the (log) earnings of the cohort member at age 34.

Earnings at age 34 came from responses to two questions:

a Last time you were paid, what was your total take home pay – that is after all deductions for tax, National Insurance, union dues, pension and so on, but including overtime, bonuses, commission and tips?

and

b How long a period did that pay cover?

A weekly amount was derived from the combination of responses to these questions and to a question about earnings from self-employment. Note that cohort members out of employment are not included in this analysis. The median log earnings (n = 6228) is 5.71 corresponding to a weekly sum of £302. The mean (SD) are 5.62 (0.82).

The results in Table 3.11 show that, unsurprisingly given the small effects seen elsewhere, there is no effect on earnings at age 34 of change in parental employment status 30 or so years earlier. There are, however, associations with paternal (but not maternal) grandfather's social class and with birth weight. Cohort members age 34 (in 2004) whose paternal grandfather was in the professional social class earn, on average, 1.4 times more than their age peers whose paternal grandfather was an unskilled worker, holding other variables in the model constant.

Table 3.11 Modelling change in parental employment status from birth to five, BCS70: log earnings, age 34

	Log earnings	
Variable	**Estimate**	**S.E.**
Change score: parental employment status		
<= -1	-0.013	0.059
-0.5	0.043	0.067
0 (reference)	0	n.a.
0.5	0.030	0.031
>=1	-0.077	0.059
Sex		
Boy (reference)	0	n.a.
Girl	-0.61	0.028
Birth weight	0.039	0.014
Father's father's social class		
I (reference)	0	n.a
II	-0.19	0.090
IIINM	-0.16	0.097
IIIM	-0.26	0.087
IV	-0.31	0.091
V	-0.31	0.10
Mother's father's social class		
I (reference)	0	n.a.
II	0.074	0.089
IIINM	-0.032	0.095
IIIM	-0.048	0.086
IV	-0.079	0.090
V	-0.049	0.098
Sample size	2,881	

3.3 Evidence from BCS70 Children of Cohort

3.3.1 Introduction to BCS70 Children of Cohort

BCS70 Children of Cohort (CC) refers to the study of the children of the cohort members about whom data were collected for the first time when the cohort members themselves were age 34 (in 2004). A random half of all cohort members was selected and information obtained about all their children. In this section, we consider the children between the ages of three and five. It is important to note that these children form a self-selected and rather unusual sample because:

a They are bound to have one parent who was age 34-x when they were born, where x is the child's age in 2004. In other words, these three to five year olds were born having at least one parent between the ages of 29 and 31. We can make some allowance for this by including age of the child in our models. This link to the cohort member's age also means that the sample is skewed towards younger children.

b They are children of cohort members who had not dropped out of the study previously. We do not attempt to allow for this non-response.

c Some families have more than one child. We do not allow for this (modest) clustering within families here.

There are 3,536 children altogether, of whom 930 are under the age of three and so do not feature in any analyses (because they were not given cognitive tests and their behaviour was not measured), 968 are age three to five (and are covered in this chapter) and 1,638 are age six to 16 (see Chapter 4).

3.3.2 Measures used

There are two measures of cognitive skills for the three to five year olds taken from the BAS Early Years Battery: the naming vocabulary test as used in the second wave of MCS, and an early number concepts test. The scales for these tests were not transformed apart from z transformations. We analyse the two measures separately. Behaviour was measured by the SDQ just as it was in MCS.

We use total family weekly income as our main explanatory variable. This was obtained from a series of questions about income received by the cohort member and their partner from earnings and benefits but not from investments. As with the MCS data, we use the log of average weekly income and also we look at the effects of log income at age 34 controlling for income at age 30. There are some unlikely values of income that have been edited; for this reason, we do not use income differences as these might not be very reliable. See Table A.8 for all the relevant descriptive statistics.

Because we do not have longitudinal data on the children of the cohort members, our control variables are limited to those measured for the cohort members, some of whom will be mothers and the rest fathers. As well as age (at test) and age squared, we use:

a log weekly pay at age 26 (derived from a single question about 'usual take home pay');

b cohort member's NVQ level;

c housing tenure; the estimates were, however, small and are not shown.

3.3.3 Results: average income

Table 3.12 Modelling average family income, BCS70, ages 30 and 34: cognitive outcomes, BCS70(CC)

	BAS		Early number	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log average family income	0.018	0.051	0.024	0.044
Log weekly pay, age 26	0.081	0.042	0.085	0.036
Cohort member's educational qualifications				
No qualifications (reference)	0	n.a.	0	n.a.
NVQ1	-0.21	0.14	-0.13	0.12
NVQ2	-0.051	0.11	0.057	0.098
NVQ3	0.050	0.12	0.041	0.10
NVQ4	0.16	0.11	0.25	0.097
NVQ5	0.36	0.21	0.41	0.18
Age at test (z-transformed)				
Linear	0.46	0.67	1.2	0.58
Quadratic	-2.6	0.73	-2.3	0.63
Sample size	718		713	
R^2	0.42		0.56	

Table 3.12 gives the estimates of log average family income on the two cognitive tests, controlling for log weekly pay of the cohort member and educational level of the cohort member. Table A.8 gives summary statistics for these variables for the observed sample. The introduction of the control variables reduces the estimate for log average family income (after controlling for age at test) from 0.11 to 0.018 for BAS and 0.090 to 0.024 for early number. There is no effect of average family income on the two cognitive outcomes.

Table 3.13 gives the corresponding estimates for behaviour (SDQ). The introduction of the control variables reduces the estimate for log average family income (after controlling for age at test) from -0.32 to -0.19. This latter estimate is not statistically significant at the 0.05 level ($p < 0.06$) but there is just a suggestion here that the income effect is more substantial for behaviour than it is for cognitive skills in this sample.

Table 3.13 Modelling average family income, BCS70, ages 30 and 34: behaviour outcome BCS70(CC)

	SDQ	
Variable	**Estimate**	**S.E.**
Log average family income	-0.19	0.10
Log weekly pay, age 26	-0.14	0.081
Cohort member's educational qualifications		
No qualifications (reference)	0	n.a.
NVQ1	-0.54	0.27
NVQ2	-0.57	0.23
NVQ3	-0.52	0.24
NVQ4	-0.60	0.22
NVQ5	-0.87	0.42
Age at test (z-transformed)		
Linear	-2.1	1.3
Quadratic	-1.2	1.5
Sample size	739	
R^2	0.046	

Note that R^2 is much lower for SDQ than it is for the cognitive measures: this is explained by the fact that age at test is a much strong predictor for the latter measures.

3.3.4 Results: income at age 34 conditional on income at age 30

The effects of family income on each of the outcomes are very small after controlling for income at age 30 (and other variables). See Tables 3.14 and 3.15.

Table 3.14 Modelling family income at age 34 conditional on family income at age 30: cognitive scores, BCS70(CC)

	BAS		Early number	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log family income, age 34	0.010	0.034	-0.012	0.029
Log family income, age 30	0.019	0.038	0.065	0.033
Log weekly pay, age 26	0.079	0.041	0.077	0.036
Cohort member's educational qualifications				
No qualifications (reference)	0	n.a.	0	n.a.
NVQ1	-0.21	0.14	-0.13	0.12
NVQ2	-0.050	0.11	0.059	0.098
NVQ3	0.053	0.12	0.046	0.10
NVQ4	0.16	0.11	0.24	0.10
NVQ5	0.36	0.21	0.42	0.18
Age at test (z-transformed)				
Linear	0.46	0.67	1.2	0.58
Quadratic	-2.6	0.73	-2.3	0.63
Sample size	718		713	
R^2	0.42		0.56	

Table 3.15 Modelling family income at age 34 conditional on family income at age 30: behaviour score, BCS70(CC)

	SDQ	
Variable	**Estimate**	**S.E.**
Log family income, age 34	0.012	0.067
Log family income, age 30	-0.20	0.76
Log weekly pay, age 26	-0.14	0.081
Cohort member's educational qualifications		
No qualifications (reference)	0	n.a.
NVQ1	-0.54	0.27
NVQ2	-0.57	0.22
NVQ3	-0.54	0.24
NVQ4	-0.60	0.22
NVQ5	-0.93	0.42
Age at test (z-transformed)		
Linear	-2.0	1.3
Quadratic	-1.1	1.5
Sample size	739	
R^2	0.050	

3.4 Summary

- Evidence from the MCS suggests that children age three years do slightly better on cognitive tests and have slightly better behaviour if they live in families that experience a substantial increase in family income.
- The above finding is not supported if, for MCS, we use change in family benefit status or, for children age five in BCS70, change in parental employment status, rather than change in family income. Nor is the first finding supported when we look at outcomes for children of the 1970 birth cohort.
- A mother's educational qualifications appear to be more strongly related to their child's outcomes than income changes are.

4 The effects of changes in parental economic circumstances in middle childhood on outcomes in middle childhood and adolescence, and in adulthood

In this chapter, we concentrate on the effects of changing circumstances in middle childhood (generally from the ages of six to ten) on outcomes at age ten and later in life. The chapter is organised into three separate sections: Section 4.1 uses data from waves two and three of British Cohort Study 1970 (BCS70), Section 4.2 uses data collected from the children of the BCS70 cohort when the cohort members were age 34 and Section 4.3 uses data from the National Pupil Database (NPD).

4.1 Evidence from BCS70

Section 3.2.1 provides an introduction to BCS70.

4.1.1 Measures used

For educational attainment, we use the reading (the Shortened Edinburgh) and mathematics (the Friendly Maths) tests at age ten as in Chapter 3. At age 16, we use tests (devised by the Assessment and Performance Unit of that era) of mathematics (arithmetic), vocabulary and two spelling tests. Standardising (i.e. z-) transformations are applied to all the educational tests. Behaviour was measured by the Rutter scales, separately for emotional, conduct and attention problems, at ages ten and 16 and dichotomised as in Chapter 3.

The only explanatory variable available is change in parental employment status as defined in Chapter 2 (because income was not measured at age five) and so the analysis is restricted to families with parents on both occasions (see Section 2.2.1). The range of this variable used in the models in the next section is wider than it was in Chapter 3 as there were more changes in employment status between the ages of five and ten. The control variables we use are: sex, housing tenure, the educational qualifications of the cohort member's mother and father, and the corresponding measures of attainment and behaviour at age five. See Tables B.1 and B.2 for all the relevant descriptive statistics.

4.1.2 Results: outcomes at age ten

The estimates in Table 4.1 are based on a bivariate regression model (see Chapter 2). We see that there is no consistent overall effect of change in parental employment status on attainment scores at age ten (Wald test: $X^2 = 16$, 14df, $p > 0.10$).

The estimates in Table 4.2 are based on a multivariate logistic regression with three binary outcomes (see Chapter 2). Although the overall effect of parental employment status on the three behaviour problems is not statistically significant, there is a suggestion that conduct and attention problems are worse for the '-1' category. As most of this category includes fathers who became unemployed between 1975 and 1980, this suggests that there could be an effect of this change in family circumstances.

Table 4.1 Modelling change in parental employment status from five to ten, BCS70: educational attainment scores, age ten

	Maths		Reading	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status				
-2 or -1.5	0.11	0.13	0.070	0.13
-1	-0.11	0.064	-0.10	0.063
-0.5	-0.083	0.046	-0.087	0.046
0 (reference)	0	n.a.	0	n.a.
0.5	0.051	0.027	0.031	0.027
1	0.017	0.043	-0.011	0.043
1.5	0.078	0.076	0.031	0.076
2	0.053	0.21	0.11	0.21
Sex				
Boy (reference)	0	n.a.	0	n.a.
Girl	-0.047	0.023	0.19	0.023
EPVT, age five	0.21	0.013	0.25	0.013
Copying designs, age five	0.29	0.023	0.26	0.013
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.15	0.025	0.13	0.025
Father's NVQ				
1 (reference)	0	n.a	0	n.a.
2	0.065	0.029	0.093	0.029
3	0.15	0.049	0.17	0.049
4	0.29	0.042	0.27	0.042
Mother's NVQ				
1 (reference)	0	n.a.	0	n.a.
2	0.18	0.028	0.21	0.028
3	0.35	0.070	0.36	0.069
4	0.38	0.055	0.41	0.055
Sample size	5,120		5,125	

Table 4.2 Modelling change in parental employment status from five to ten, BCS70: behaviour problems, age ten

	Emotional		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status						
-2 or -1.5	-0.064	0.40	0.16	0.37	0.11	0.37
-1	-0.38	0.19	0.60	0.15	0.35	0.16
-0.5	-0.014	0.12	-0.043	0.13	0.086	0.12
0 (reference)	0	n.a.	0	n.a.	0	n.a.
0.5	0.14	0.070	0.048	0.074	-0.031	0.073
1	0.048	0.11	0.039	0.12	0.22	0.11
1.5	-0.23	0.23	-0.18	0.22	0.055	0.21
2	-0.41	0.64	0.25	0.52	0.42	0.49
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.077	0.060	-0.34	0.065	-0.49	0.063
Emotional, age five	0.67	0.028	n.a.	n.a.	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.75	0.31	n.a.	n.a.
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.69	0.028
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	0.079	0.067	-0.21	0.068	-0.029	0.067
Father's NVQ						
1 (reference)	0	n.a	0	n.a.	0	n.a.
2	0.032	0.076	-0.15	0.078	-0.21	0.077
3	0.010	0.12	-0.40	0.14	-0.45	0.14
4	-0.038	0.11	-0.46	0.12	-0.48	0.12
Mother's NVQ						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	-0.010	0.073	-0.36	0.079	-0.19	0.076
3	-0.14	0.18	-0.69	0.23	-0.63	0.22
4	-0.16	0.13	-0.46	0.16	-0.23	0.15
Sample size	7,544		7,598		7,578	

4.1.3 Results: outcomes at age 16

Table 4.3 (divided into two parts) shows that there was no effect of change in parental employment status on attainments at age 16 (Wald test: $X^2 = 35$, 28df, $p > 0.10$). It is important to bear in mind, however, that there is a lot of missing attainment data at age 16 in that only about half the observed sample provided education data at 16 because of industrial action by teachers at that time (see Plewis *et al.*, 2004, Table 8.2). There is also no overall effect on behaviour (Wald test: $X^2 = 21$, 21df, $p > 0.10$) although there remains a suggestion of an effect on conduct and attention problems as measured by the Rutter scales.

Table 4.3A Modelling change in parental employment status from five to ten, BCS70: educational attainment scores: Maths and Vocabulary, age 16

	Maths		Vocabulary	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status				
2 or -1.5	0.15	0.30	0.12	0.24
-1	0.26	0.16	0.15	0.14
-0.5	0.018	0.11	-0.030	0.084
0 (reference)	0	n.a.	0	n.a.
0.5	0.049	0.058	-0.011	0.048
1	0.011	0.095	-0.10	0.079
1.5	-0.39	0.16	-0.20	0.14
2	0.21	0.51	0.19	0.42
Sex				
Boy (reference)	0	n.a.	0	n.a.
Girl	0.049	0.052	0.14	0.043
EPVT, age five	0.18	0.029	0.24	0.024
Copying designs, age five	0.26	0.028	0.18	0.023
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.12	0.058	0.064	0.047
Father's NVQ				
1 (reference)	0	n.a	0	n.a.
2	0.046	0.063	-0.019	0.052
3	0.12	0.10	0.19	0.082
4	0.31	0.087	0.18	0.071
Mother's NVQ				
1 (reference)	0	n.a.	0	n.a.
2	0.12	0.059	0.20	0.049
3	0.22	0.15	0.35	0.12
4	0.19	0.11	0.39	0.086
Sample size	1,168		1,971	

Table 4.3B Modelling change in parental employment status from five to ten, BCS70: educational attainment scores: Spelling A and Spelling B, age 16

	Spelling A		Spelling B	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status				
-2 or -1.5	-0.093	0.26	0.030	0.25
-1	-0.12	0.14	-0.092	0.13
-0.5	-0.13	0.090	-0.070	0.086
0 (reference)	0	n.a.	0	n.a.
0.5	0.016	0.051	0.060	0.048
1	0.032	0.085	0.010	0.081
1.5	-0.15	0.15	0.016	0.14
2	0.20	0.48	0.59	0.46
Sex				
Boy (reference)	0	n.a.	0	n.a.
Girl	0.32	0.046	0.32	0.044
EPVT, age five	0.085	0.025	0.088	0.024
Copying designs, age five	0.20	0.024	0.18	0.023
Tenure				
Rent (reference)	0	n.a.	0	n.a.
Own	0.039	0.051	0.085	0.048
Father's NVQ				
1 (reference)	0	n.a	0	n.a.
2	-0.001	0.056	-0.010	0.053
3	0.094	0.088	0.046	0.084
4	0.12	0.076	0.096	0.072
Mother's NVQ				
1 (reference)	0	n.a.	0	n.a.
2	0.086	0.053	0.11	0.050
3	0.21	0.13	0.23	0.12
4	0.25	0.091	0.24	0.087
Sample size	1,935		1,901	

Table 4.4 Modelling change in parental employment status from five to ten, BCS70: behaviour problems, age 16

	Emotional		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Change score: parental employment status						
-2 or -1.5	0.31	0.45	0.60	0.42	-0.41	0.60
-1	0.010	0.22	0.47	0.20	0.44	0.20
-0.5	0.11	0.14	0.26	0.13	0.050	0.14
0 (reference)	0	n.a.	0	n.a.	0	n.a.
0.5	-0.010	0.082	-0.015	0.078	0.010	0.079
1	0.11	0.13	0.16	0.13	0.16	0.13
1.5	0.23	0.25	0.10	0.23	0.20	0.23
2	-0.88	1.1	0.079	0.71	-0.16	0.74
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.47	0.072	-0.083	0.069	-0.44	0.069
Emotional, age five	0.44	0.033	n.a.	n.a.	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.57	0.036	n.a.	n.a.
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.48	0.032
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	-0.10	0.080	-0.29	0.074	-0.042	0.076
Father's NVQ						
1 (reference)	0	n.a	0	n.a.	0	n.a.
2	-0.10	0.091	-0.19	0.085	-0.27	0.087
3	-0.010	0.14	-0.14	0.14	-0.32	0.14
4	-0.37	0.13	-0.30	0.12	-0.51	0.12
Mother's NVQ						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	0.29	0.086	-0.19	0.082	-0.031	0.083
3	-0.095	0.22	-0.45	0.21	-0.63	0.24
4	0.084	0.15	-0.31	0.15	-0.037	0.15
Sample size	3,373		3,373		3,405	

4.1.4 Results: earnings at age 34

The results in Table 4.5 show that there is no effect on earnings at age 34 of change in parental employment status twenty five years earlier (Wald test: $X^2 = 6.8$, 8df, $p > 0.10$). There is, however, a small association with test scores at age five and a stronger association with father's than mother's educational qualifications.

Table 4.5 Modelling change in parental employment status from five to ten, BCS70: log earnings, age 34

	Log earnings	
Variable	**Estimate**	**S.E.**
Change score: parental employment status		
-2	-0.10	0.75
-1.5	0.32	0.24
-1	0.090	0.086
-0.5	0.081	0.053
0 (reference)	0	n.a.
0.5	0.043	0.031
1	0.011	0.049
1.5	0.010	0.090
2	0.22	0.19
Sex		
Boy (reference)	0	n.a.
Girl	-0.66	0.028
Emotional, age five	0.010	0.014
Conduct, age five	0.012	0.016
Attention, age five	-0.010	0.015
EPVT, age five	0.048	0.016
Copying Designs, age five	0.077	0.015
Tenure		
Rent (reference)	0	n.a.
Own	0.094	0.030
Father's NVQ		
1 (reference)	0	n.a
2	0.077	0.034
3	0.15	0.054
4	0.22	0.046
Mother's NVQ		
1 (reference)	0	n.a.
2	0.028	0.032
3	0.063	0.075
4	0.11	0.058
Sample size	3,580	

4.2 Evidence from British Cohort Study 1970, Children of Cohort

See Section 3.3.1 for an introduction to British Cohort Study 1970, Children of Cohort (BCS70(CC)). Here we focus on children age six to 16 rather than six to ten because there are relatively few children over ten.

4.2.1 Measures used

There are three measures of cognitive skills for the six to 16 year olds taken from the British Ability Scales (BAS) School Years Battery: word reading, spelling and number skills. The scales for these tests were not transformed apart from z transformations. We analyse the three measures separately. Behaviour was measured as before by the Strengths and Difficulties Questionnaire (SDQ). All the explanatory variables are the same as those used in Chapter 3 (Section 3.3.2).

4.2.2 Results: average income

Table 4.6 Modelling average family income, BCS70, ages 30 & 34: cognitive outcomes of children

	Word reading		Spelling		Number	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log average family income	-0.024	0.046	0.019	0.064	0.038	0.041
Log weekly pay, age 26	0.071	0.028	0.10	0.038	0.029	0.025
Cohort member's educational qualifications						
No qualifications (reference)	0	n.a.	0	n.a.	0	n.a.
NVQ1	0.20	0.096	0.45	0.13	0.20	0.085
NVQ2	0.25	0.076	0.41	0.10	0.20	0.067
NVQ3	0.20	0.083	0.37	0.11	0.13	0.074
NVQ4	0.34	0.085	0.43	0.12	0.26	0.075
NVQ5	0.36	0.25	0.69	0.35	0.24	0.22
Age at test						
Linear	2.1	0.11	0.91	0.15	2.4	0.099
Quadratic	-0.50	0.054	-0.22	0.073	-0.55	0.048
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	0.10	0.062	0.16	0.085	0.089	0.055
Sample size	885		885		882	
R^2	0.54		0.11		0.65	

Table 4.6 gives the estimates of log average family income on the three cognitive tests, controlling for log weekly pay of the cohort member, educational level of the cohort member and housing tenure. Tables B.3 and B.4 give summary statistics for these variables for the observed sample. The introduction of the control variables reduces the estimate for log average family income (after controlling for age at test) from 0.043 to -0.024 for word reading, from 0.11 to 0.019 for spelling but does not change the estimate for early number. There is no effect of average family income on the three cognitive outcomes.

Table 4.7 gives the corresponding estimates for behaviour (SDQ). The introduction of the control variables actually increases the estimate for log average family income (after controlling for age at test) from -0.33 to -0.40. This latter estimate is statistically significant at the 0.001 level, again suggesting that the income effect is more substantial for behaviour than it is for cognitive skills in this sample.

Table 4.7 Modelling average family income, BCS70, ages 30 and 34: behaviour outcome of children

	SDQ	
Variable	**Estimate**	**S.E.**
Log average family income	-0.40	0.12
Log weekly pay, age 26	0.12	0.070
Cohort member's educational qualifications		
No qualifications (reference)	0	n.a.
NVQ1	-0.0055	0.24
NVQ2	-0.27	0.20
NVQ3	-0.095	0.21
NVQ4	-0.44	0.22
NVQ5	-0.78	0.64
Age at test		
Linear	0.056	0.28
Quadratic	-0.038	0.13
Tenure		
Rent (reference)	0	n.a.
Own	-0.40	0.16
Sample size	902	
R^2	0.039	

4.2.3 Results: income at age 34 conditional on income at age 30

The effects of family income on each of the cognitive outcomes are very small after controlling for income at age 30 (and other variables) but somewhat larger for SDQ ($p < 0.07$). See Tables 4.8 and 4.9.

Table 4.8 Modelling family income at age 34 conditional on family income at age 30, BCS70: cognitive scores, children

	Word reading		Spelling		Number	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log family income, age 34	-0.027	0.034	-0.071	0.046	-0.035	0.030
Log family income, age 30	0.018	0.033	0.094	0.045	0.069	0.029
Log weekly pay, age 26	0.071	0.028	0.10	0.038	0.031	0.025
Cohort member's educational qualifications						
No qualifications (reference)	0	n.a.	0	n.a.	0	n.a.
NVQ1	0.20	0.096	0.44	0.13	0.19	0.085
NVQ2	0.25	0.076	0.40	0.10	0.19	0.067
NVQ3	0.20	0.084	0.37	0.11	0.13	0.074
NVQ4	0.34	0.085	0.43	0.12	0.26	0.075
NVQ5	0.36	0.25	0.70	0.35	0.25	0.22
Age at test						
Linear	2.1	0.11	0.90	0.15	2.4	0.099
Quadratic	-0.50	0.054	-0.22	0.073	-0.55	0.047
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	0.096	0.062	0.16	0.084	0.089	0.055
Sample size	885		885		882	
R^2	0.54		0.12		0.65	

Table 4.9 Modelling family income at age 34 conditional on family income at age 30, BCS70: behaviour score of children

	SDQ	
Variable	**Estimate**	**S.E.**
Log family income, age 34	-0.15	0.082
Log family income, age 30	-0.22	0.079
Log weekly pay, age 26	0.12	0.069
Cohort member's educational qualifications		
No qualifications (reference)	0	n.a.
NVQ1	-0.0026	0.24
NVQ2	-0.27	0.20
NVQ3	-0.12	0.21
NVQ4	-0.45	0.21
NVQ5	-0.81	0.64
Age at test		
Linear	0.054	0.28
Quadratic	-0.034	0.13
Tenure		
Rent (reference)		
Own	-0.41	0.15
Sample size	902	
R^2	0.040	

4.3 Evidence from the National Pupil Database

Another angle on the relation between changes in economic circumstances and pupils' progress at school is provided by data from the NPD in conjunction with Pupil Level Annual Schools Census (PLASC).

4.3.1 Introduction to the NPD

The NPD and PLASC in England are linked datasets, which have been constructed annually by Department for Education and Skills (DfES) (now Department for Children, Schools and Families (DCSF)) since 2002 and provide a census of pupils at state schools in England only.

The NPD has the following advantages:

- it contains both pupil-level and some school-level data;
- it contains rich information on pupils' Key Stage test scores;
- it is longitudinal, allowing us to control for prior characteristics of pupils.

There are, however, some drawbacks. In particular, the NPD contains no measures of parents' social class, educational level or income. Claims for free school meals (FSM) do, however, form an important variable in the dataset that is directly related to economic disadvantage.

We focus on the period between Key Stage 1 (KS1) (when pupils are age seven years) and Key Stage 2 (KS2) when they are 11, an age that usually marks the end of their primary school career, and on the cohort that reached KS1 in 2002 and KS2 in 2006. The cohort consists of 595,407 pupils.

The following groups were omitted:

a Special and independent schools, pupil referral units, etc. where the school experiences are very different (n = 22,448).

b Two very small Local Education Authorities (LEAs): City of London and the Scilly Isles (n = 71).

This left up to 572,888 pupils in 148 LEAs in about 14,750 schools (the exact numbers of pupils and schools depended on the relatively small amount of missing data at the pupil level in any particular model).

4.3.2 Measures used

Our outcome variables are the pupils' test scores at KS2 in English, maths and science and descriptive statistics for these three variables can be found in Table B.5. We control for attainments in reading, writing and maths at KS1 along with teacher assessments of their pupils' abilities in English, maths and science at that point. See Tables B.6 and B.7 for more details.

For each pupil for each of the four years, we use, as an indicator of a family's economic circumstances, information on whether or not a claim was made (at the time of the annual schools' census) for FSM. Although FSM is, conceptually, rather a simplistic indicator of what we really want to measure, it is, as we shall see, a powerful predictor of educational attainments. **Eligibility** for free school meals is based on receipt of Income Support (IS), Income Based Jobseeker's Allowance (JSA) or support under Part 6 of the 1999 Immigration and Asylum Act. Pupils are identified as receiving FSM only if they have actually **claimed** them **and** their eligibility has been confirmed.

Concern has been expressed about the validity of the FSM measure, especially whether claiming FSM is seen as stigmatising and, therefore, not every family that is eligible actually claims (either because they do not claim the benefits for which they are eligible or because claiming FSM is seen as especially stigmatising for children). Moreover, not all children living in disadvantaged families are eligible to receive FSM. If there is a stigma attached to claiming FSM then this could affect measures of change in FSM status. We do not go into this issue here but evidence presented by Plewis (2007b) suggests that there might be a small effect on FSM transitions.

We generate two measures of economic circumstances from the FSM variables. The first is a score – varying from zero to four – of the number of years that a pupil claimed FSM. The distribution of the FSM score is given in Table 4.10 and shows that over three-quarters of pupils never claim FSM, 24 per cent claim it at least once and 11 per cent claim it for each of the four years. Pupils in this latter group might be assumed to be living in persistent poverty. Using an FSM score in this way is akin to using average family income as we did in Chapter 3.

Table 4.10 Distribution of FSM score, NPD 2002/06

FSM score	%
0	76
1	4.2
2	3.9
3	4.5
4	11

Source: NPD, 2006; n = 572,888.

Our second measure is generated from the claims for FSM made each year and explicitly incorporates a representation of change. There are 24 (i.e.16) combinations of FSM claiming behaviour. We group these combinations into seven categories to create a variable we label 'FSM_dyn' (for dynamics):

i never claiming (76 per cent);

ii possible improvement (3.8 per cent);

iii definite improvement (1.7 per cent);

iv possible decline (2.7 per cent);

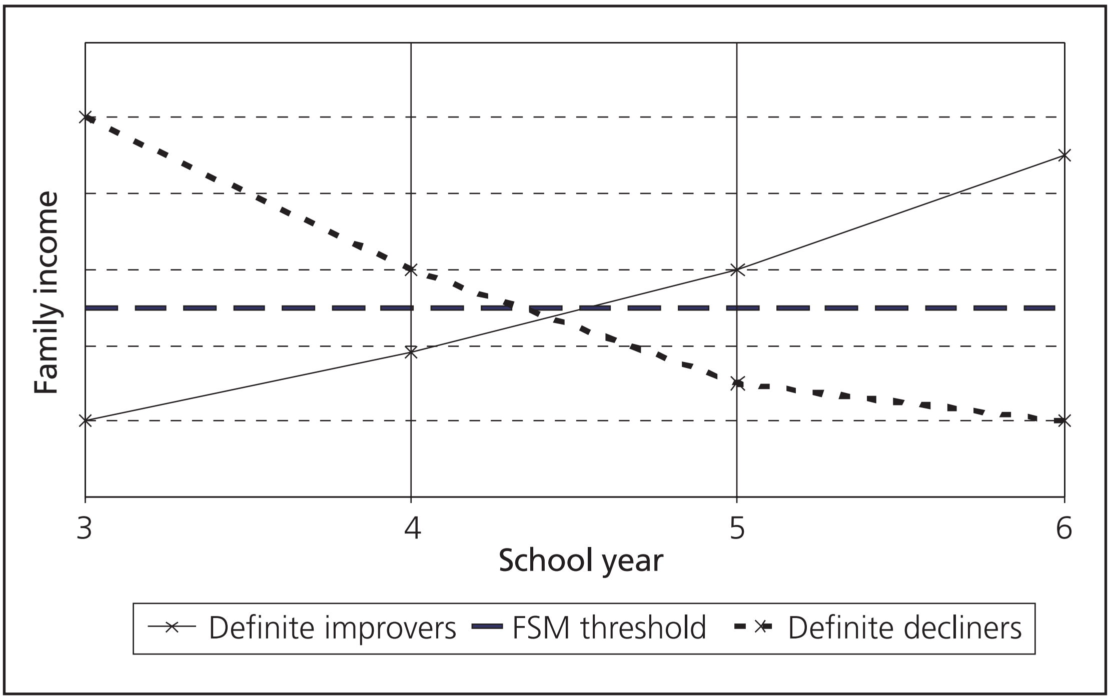
v definite decline (1.1 per cent);

vi erratic claiming (3.2 per cent);

vii always claiming (11 per cent).

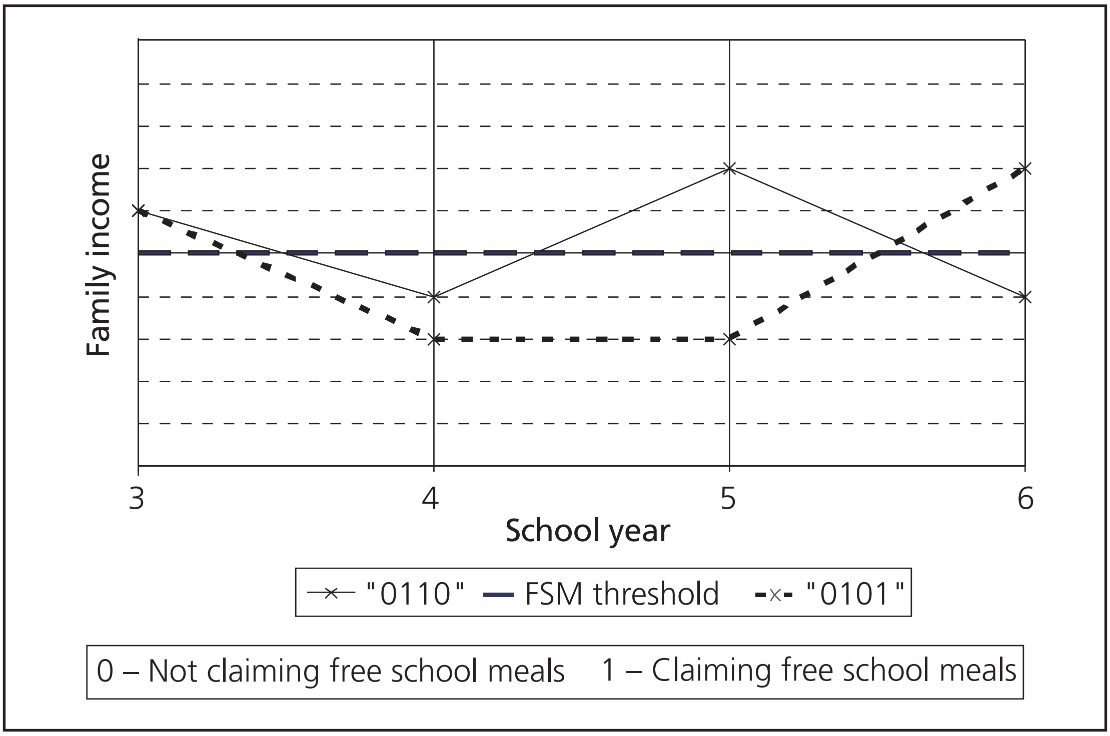
Our interest is in the contrast between the 'definite' improvers (those who claimed for the first two years but not for the last two) and the 'definite' decliners (those who did not claim for the first two years and claimed for the last two). Although pupils in both these groups have the same number of years of exposure to poverty (as represented by FSM), we would expect pupils in the first group to be living in families on an upward economic path and, therefore, to make more progress than those in the second, whose families are on a downward path. In other words, we assume that the majority of the 'definite improvers' group follow the path of monotonically increasing income in Figure 4.1, whereas the 'definite decliners' follow the downward path shown there. This approach is not dissimilar to one used by Rigg and Sefton (2006) who categorise income trajectories from the British Household Panel Survey (BHPS).

Figure 4.1 The dynamics of claiming free school meals and family income: monotonic changes



The erratic claimers are those with at least two changes in claiming behaviour over the four years (e.g. 0110 where 0 is not claiming); the possible improvers and decliners are those with just one change of claiming behaviour but spending just one year either claiming or not claiming (e.g. 0001). We cannot be sure that the possible improvers and decliners are not just erratic claimers in disguise (because we only have four observations) and so we do not have any hypotheses about their relative progress. We assume that the groups that exhibit changes in FSM status other than the definite improvers and decliners have incomes that fluctuate around the FSM threshold as illustrated in Figure 4.2.

Figure 4.2 The dynamics of claiming free school meals and family income: erratic changes



Although the NPD does not include many variables that can be used as additional controls, we can use sex and ethnic group (and the interaction between them). Information on ethnicity was collected by the school either from the child or a parent or, in some cases it was ascribed by the school. Ethnicity is coded in great detail (95 categories). We use a simplified, 12 category classification (see Table 4.11). FSM claiming behaviour varies by ethnic group (pupils from Indian and Chinese backgrounds claim less than white majority families, pupils from other minority ethnic groups claim more). Girls make more progress than boys in English but less in maths. Other possible controls that we might have used but chose not to include Special Educational Needs (SEN) status, and numbers of changes of address during the period. The latter is likely to be an endogeneous change and SEN is, arguably, an outcome rather than a control variable.

The NPD is hierarchically structured with pupils nested in schools and schools nested in LEAs and so a multilevel approach to analysis is called for. The multilevel package *MLwiN* (Rasbash *et al.*, 2004) was used for all the analyses reported in this section. We do not, however, treat the three outcomes as a multivariate set (as we did in Chapter 3) because of computing restrictions arising from the size of the dataset.

4.3.3 Results

Our first set of results refers to progress related to the FSM score. Tables 4.11 and 4.12 give the estimates from the complex models fitted separately to English, maths and science test scores at KS2. The main findings germane to the effects of changing economic circumstances are as follows:

1 There is an effect of FSM score on progress such that, in the average school and LEA (schools and LEAs where the random effect is zero), white pupils who never claim FSM make about one-fifth of a Standard Deviation (SD) unit more progress than those that claim each year (variable 6 of Table 4.11). This difference is equivalent to between three and four months of progress over this period.

2 The FSM effect is, however, less marked for pupils from minority ethnic groups, especially for English and science (variable 7 of Table 4.11).

3 The pupil FSM effect is somewhat less marked in schools with a high FSM score (i.e. a high proportion of pupils claiming FSM) (variable 9 of Table 4.11).

4 The FSM effect varies from school to school (variable 2 of Table 4.12). Thus, for science, the FSM effect varies between +0.019 and -0.13 for 95 per cent of schools, with similar ranges for English and maths. The effect also varies from LEA to LEA but this between LEA variation is much smaller than the between school variation.

Table 4.11 English, maths and science progress, KS1 to KS2: NPD 2002-2006

	English		Maths		Science	
Explanatory variables	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
1. KS1 tests						
Read	0.27	0.0012	0.082	0.0011	0.17	0.0012
Write	0.19	0.0013	0.080	0.0013	0.057	0.0014
Maths	0.11	0.0011	0.38	0.0014	0.24	0.0014
2. KS1 teacher assessments						
English	0.089	0.0024	0.019	0.0024	0.034	0.0027
Maths	0.034	0.0023	0.16	0.0025	0.041	0.0026
Science	0.090	0.0021	0.095	0.0022	0.17	0.0026
3. Sex: base boys						
Girls	0.18	0.0017	-0.16	0.0018	-0.067	0.0020
4. Ethnic Group: base white British						
Other European	0.13	0.0081	0.10	0.0084	0.075	0.0094
Mixed	0.056	0.0070	0.0030	0.0072	0.014	0.0081
Indian	0.097	0.0084	0.17	0.0086	-0.043	0.0097
Pakistani	0.085	0.0085	0.10	0.0088	-0.14	0.0098
Bangladeshi	0.21	0.013	0.21	0.014	-0.011	0.015
Other Asian	0.17	0.016	0.25	0.016	0.012	0.019
Black Caribbean	-0.069	0.011	-0.18	0.011	-0.18	0.012
Black African	0.11	0.011	-0.015	0.011	-0.073	0.013
Other black	0.0050	0.020	-0.11	0.021	-0.11	0.024
Chinese	0.24	0.020	0.35	0.021	0.11	0.024
Other	0.15	0.016	0.17	0.016	0.035	0.018

Continued

Table 4.11 Continued

	English		Maths		Science	
Explanatory variables	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
5. Ethnic group x sex: bases white British and boys						
Other European	-0.013	0.010	-0.015	0.011	0.037	0.012
Mixed	0.014	0.0087	-0.018	0.0091	0.029	0.010
Indian	0.013	0.010	0.0061	0.011	0.039	0.012
Pakistani	-0.0027	0.0094	-0.034	0.0098	0.0044	0.011
Bangladeshi	-0.0064	0.014	-0.037	0.015	0.011	0.016
Other Asian	0.0036	0.020	0.061	0.021	0.069	0.023
Black Caribbean	0.061	0.013	0.080	0.013	0.091	0.015
Black African	0.020	0.012	0.077	0.013	0.11	0.014
Other black	0.016	0.025	0.029	0.026	0.052	0.028
Chinese	-0.026	0.027	0.013	0.028	0.046	0.031
Other	0.020	0.019	0.023	0.019	0.093	0.022
6. FSM score	-0.047	0.0015	-0.044	0.0016	-0.053	0.0016
7. FSM score x ethnic group: base white						
Other European	0.0074	0.0035	0.010	0.0037	-0.0037	0.0040
Mixed	0.012	0.0029	-0.0025	0.0030	0.010	0.0033
Indian	0.025	0.0048	-0.0040	0.0051	0.025	0.0055
Pakistani	0.021	0.0031	0.0097	0.0033	0.022	0.0036
Bangladeshi	0.024	0.0045	0.0097	0.0048	0.019	0.0052
Other Asian	0.041	0.0069	0.022	0.0073	0.047	0.0080
Black Caribbean	0.011	0.0041	0.0044	0.0043	0.014	0.0047
Black African	0.026	0.0036	0.019	0.0038	0.024	0.0042
Other black	0.020	0.0074	0.027	0.0078	0.025	0.0085
Chinese	0.020	0.012	0.036	0.012	0.046	0.014
Other	0.022	0.0054	0.017	0.0057	0.022	0.0063
8. School FSM score	-0.17	0.011	-0.15	0.013	-0.19	0.013
9. FSM score x School FSM score	0.0068	0.0012	0.011	0.0012	0.0053	0.0013
10. LEA FSM score	0.056	0.017	0.043	0.014	0.022	0.016
11. School FSM score x LEA FSM score	0.055	0.011	0.048	0.013	0.071	0.014
12. Sample size	507,679		509,446		514,034	

Table 4.12 Random effects (variances) corresponding to Table 4.11

Explanatory variables	English		Maths		Science	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
1. Constant						
Between boys	0.29	0.00087	0.31	0.00091	0.40	0.0012
Between girls	0.26	0.00087	0.30	0.00091	0.0.36	0.0012
Between schools	0.20	0.0042	0.28	0.0054	0.41	0.0076
Between LEAs	0.0029	0.00061	0.0014	0.00037	0.0018	0.00045
2. FSM score						
Between schools	0.00064	0.00006	0.00092	0.00007	0.0014	0.00009
Between LEAs	0.00003	0.00001	0.00003	0.00001	0.00001	0.00001
3. KS1 measures (between schools)						
Read test	0.0037	0.00020	n.i.m		n.i.m	
Write test	0.0023	0.00024	n.i.m		n.i.m	
Maths test	n.i.m		0.0072	0.00025	0.0064	0.00021
English TA	0.0035	0.00075	n.i.m		n.i.m	
Maths TA	n.i.m		0.0097	0.00082	n.i.m	
Science TA	n.i.m		n.i.m		0.0077	0.00074
4. School FSM score						
Between LEAs	0.00051	0.00033	0.0021	0.00051	0.0023	0.00059

Note: n.i.m: not in model.

In addition, it is of interest to note that:

1 girls make more progress than boys in English but less in maths and science (variable 3 of Table 4.11);

2 pupils from minority ethnic groups generally make more progress than white groups (variable 4), the main exception being the black Caribbean boys (variable 5);

3 pupils make less progress in schools with high FSM scores (variable 8) but more progress in LEAs with high FSM scores (variable 10: holding all other variables constant).

Table 4.13 English, maths and science progress, KS1 to KS2: NPD 2002-2006

	English		Maths		Science	
Explanatory variables	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
KS1 tests						
Read	0.27	0.0012	0.082	0.0011	0.17	0.0012
Write	0.19	0.0013	0.081	0.0013	0.057	0.0014
Maths	0.11	0.0011	0.38	0.0014	0.24	0.0014
KS1 teacher assessments						
English	0.090	0.0024	0.019	0.0024	0.034	0.0027
Maths	0.034	0.0023	0.16	0.0025	0.042	0.0026
Science	0.091	0.0021	0.096	0.0022	0.17	0.0026
Sex: base boys						
Girls	0.18	0.0017	-0.16	0.0018	-0.067	0.0020
Ethnic Group: base white British						
Other European	0.13	0.0076	0.11	0.0077	0.069	0.0085
Mixed	0.064	0.0065	0.0030	0.0066	0.020	0.0073
Indian	0.10	0.0081	0.16	0.0083	-0.040	0.0092
Pakistani	0.097	0.0078	0.10	0.0079	-0.13	0.0088
Bangladeshi	0.23	0.012	0.21	0.012	-0.0051	0.013
Other Asian	0.20	0.015	0.26	0.015	0.048	0.017
Black Caribbean	-0.065	0.0098	-0.19	0.010	-0.17	0.011
Black African	0.14	0.0094	-0.0078	0.0096	-0.045	0.011
Other black	0.023	0.018	-0.079	0.018	-0.083	0.020
Chinese	0.25	0.020	0.37	0.020	0.13	0.022
Other	0.18	0.014	0.19	0.014	0.060	0.015
Ethnic group x sex: bases white British and boys						
Other European	-0.013	0.010	0.015	0.011	0.037	0.012
Mixed	0.014	0.0087	0.018	0.0091	0.029	0.010
Indian	0.014	0.010	0.0061	0.011	0.040	0.012
Pakistani	-0.0032	0.0093	-0.034	0.0098	0.0037	0.011
Bangladeshi	-0.0046	0.014	-0.036	0.015	0.014	0.016
Other Asian	0.0030	0.020	0.059	0.021	0.067	0.023
Black Caribbean	0.061	0.013	0.078	0.013	0.090	0.015
Black African	0.020	0.012	0.076	0.013	0.10	0.014
Other black	0.018	0.025	0.031	0.026	0.054	0.029
Chinese	-0.028	0.027	0.012	0.028	0.042	0.031
Other	0.022	0.019	0.024	0.020	0.093	0.022

Continued

Table 4.13 Continued

	English		Maths		Science	
Explanatory variables	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
FS_dyn: base never						
Possible improvers	-0.094	0.0042	-0.090	0.0044	-0.13	0.0048
Definite improvers	-0.080	0.0061	-0.082	0.0063	-0.12	0.0070
Possible decliners	-0.097	0.0049	-0.089	0.0052	-0.12	0.0057
Definite decliners	-0.11	0.0075	-0.10	0.0079	-0.12	0.0087
Random	-0.11	0.0045	-0.099	0.0047	-0.13	0.0052
Always	-0.14	0.0028	-0.12	0.0029	-0.17	0.0032
Sample size	507,679		509,446		514,034	

The second model replaces the FSM score by FSM_dyn – see Table 4.13. This is a somewhat simpler model as allowing the effects of this categorical FSM variable to vary by school would make the model too complicated. The other random effects are very similar to those presented in Table 4.12 and are not repeated here.

We find that, as expected, all pupils with some exposure to poverty in terms of claiming FSM make less progress than the majority who never claim. However, the definite improvers do, as hypothesized, make slightly more progress than the definite decliners in English (0.03 SD units; $p < 0.001$ (Wald test)) and in maths (0.02 SD units, $p < 0.05$ (Wald test)) but there is no difference for science. A difference of 0.03 SD units is equivalent to about two weeks progress.

4.4 Summary

- There is no evidence from BCS70 that educational progress or later earnings are affected by changes in income or income-related variables during middle childhood.
- There is evidence from the NPD of a very small effect on educational progress during middle childhood of a change in benefit status (as measured by claims for free school meals).
- There is slight evidence from BCS70 to suggest that a decline in economic circumstances during middle childhood has a negative effect on some childhood behaviours.

5 The effects of changes in parental economic circumstances in adolescence on outcomes in adolescence and adulthood

We turn finally to the effects of economic changes in adolescence on educational and behaviour outcomes at age 16 and in adulthood. In this chapter, we rely entirely on data from British Cohort Study 1970 (BCS70) as being the only source of our data that covers this period. As explained in Section 4.1.3, there is a lot of missing data at age 16, especially for the educational test scores. We present our findings in three sections: the effects of income change between ages ten and 16; the effects of changes in benefit status; and the effects of changes in parental employment status. Our outcomes are:

a Test scores in mathematics, vocabulary and spelling (two tests) at age 16 as described in Section 4.1.1.

b Binary indicators of emotional, conduct and attention behaviour problems as reported by mothers, generated from the Rutter A scales.

c Earnings at age 34 (as used in Chapters 3 and 4).

The following control variables are used:

a Educational test scores at ages five and ten (see Chapter 3).

b Measures of behaviour problems at ages five and ten (see Chapter 3).

c The educational level of the parents (or parent figures) of the cohort member (four ‘National Vocational Qualifications (NVQ)’ levels). If we include both these measures in our models we are, in effect, restricting our analyses to two parent families at age ten (some of whom will later split up) or to two parent families throughout (where, for some families, there could be a ‘hidden’ change of partner). In other words, we would exclude those families who are single parent families throughout the period and those where a new partner is acquired. Consequently, we only report the estimates from models where just mother’s educational level is used as a control variable but note any situations where the estimates are substantially affected by the addition of fathers’ educational levels.

d Housing tenure at age ten.

e Sex of cohort member.

5.1 Effects of change in income

5.1.1 Income measures

We have measures of parental income at ages ten and 16. At age ten, respondents were asked for an assessment of weekly income within one of seven bands. They were asked to include all earned and unearned income of both the mother and father before deductions but excluding child benefit. The question at age 16 was very similar but 11 income bands were used and respondents could choose to report either a weekly or an equivalent annual figure. We transform the income data to follow the grouped Singh-Maddala distribution as described in Section 2.2.2. Tables 5.1 and 5.2 give the distributions for the two years. We see that the values derived from the Singh-Maddala distribution are close to the mid-point of each band, especially when there are 11 bands in 1986.

All our models of income change are based on the effects of income at age 16, conditional on income at age ten where income takes the log of the Singh-Maddala value. We do not model either average income or income differences because different numbers of bands were used at the two ages and the effects for averages and differences would be difficult to interpret.

Table 5.1 Income distribution, age ten: BCS70

Band	Mid-point (£)	Singh-Maddala value (£)	Number	%
<£35 p.w.	n.a.	26.6	146	1.6
£35-£49	42.50	43.4	442	4.7
£50-£99	75	78.1	2809	30
£100-£149	125	123	3269	35
£150-£199	175	171	1575	17
£200-£249	225	221	611	6.5
>=£250	n.a.	334	546	5.8
Total			9,398	100

Table 5.2 Income distribution, age 16: BCS70

Band	Mid-point (£)	Singh-Maddala value (£)	Number	%
<£50 p.w.	n.a.	34.3	203	3.0
£50-£99	75	77.5	1138	17
£100-£149	125	125	1135	17
£150-£199	175	174	1200	18
£200-£249	225	224	942	14
£250-£299	275	273	748	11
£300-£349	325	323	489	7.2
£350-£399	375	373	285	4.2
£400-£449	425	423	259	3.8
£450-£499	475	473	121	1.8
>= £500	n.a.	670	285	4.2
Total			6,805	100

5.1.2 Results

The effects of income change on the four educational test scores are substantial before introducing any controls, as shown in Table 5.3. However, they become small (and not statistically significant) after introducing controls, especially once earlier measures of educational attainments are controlled for (Table 5.4). They do, however, remain positive as expected. The introduction of father's' educational level reduces the sample size by between 81 (maths) and 136 (vocabulary) cases: the income effects are then somewhat weaker. Descriptive statistics for the observed and analysis samples are given in Tables C.1 to C.3.

Table 5.3 Income change, ages ten to 16 and educational attainments at age 16: BCS70

Income	Maths 16		Vocab 16		Spell A 16		Spell B 16	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Log income, age 16	0.37	0.047	0.29	0.036	0.27	0.038	0.23	0.036
Log income, age ten	0.20	0.059	0.19	0.047	0.10	0.049	0.13	0.047
R^2	0.086		0.063		0.038		0.038	
Sample size	1,846		3,041		2,997		2,924	

Table 5.4 Income change, ages ten to 16 and educational attainments at age 16 (full model): BCS70

Variable	Maths 16		Vocab 16		Spell A 16		Spell B 16	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Log income, age 16	0.087	0.055	0.072	0.047	0.092	0.049	0.073	0.047
Log income, age 10	-0.012	0.067	0.011	0.058	0.0019	0.060	0.12	0.057
Sex								
Boy (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Girl	0.073	0.049	0.12	0.042	0.28	0.044	0.30	0.043
Tenure								
Rent (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Own	0.11	0.057	0.024	0.050	-0.10	0.052	-0.067	0.050
Maths, age ten	0.45	0.039	0.13	0.033	0.12	0.035	0.16	0.034
Reading, age ten	0.21	0.039	0.38	0.034	0.36	0.036	0.27	0.034
EPVT, age five	0.037	0.028	0.12	0.024	-0.0011	0.025	0.014	0.024
Copying designs, age five	0.087	0.027	0.012	0.023	0.037	0.024	0.054	0.023
Mother's NVQ								
1 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
2	-0.036	0.054	0.066	0.047	0.047	0.050	0.012	0.048
3	-0.16	0.12	0.24	0.098	0.12	0.10	0.016	0.098
4	0.013	0.090	0.13	0.078	-0.010	0.082	-0.10	0.078
R^2	0.44		0.30		0.23		0.23	
Sample size	966		1,611		1,585		1,556	

The results for the behaviour ratings (here derived from separate logistic regressions) are given in Tables 5.5 and 5.6. They show the same pattern of estimates as the test scores with the effects of income change essentially eliminated by the introduction of control variables, although there is a suggestion of an effect for conduct problems.

Table 5.5 Income change, ages ten to 16 and behaviour at age 16: BCS70

	Emotion		Conduct		Attention	
Income	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	-0.099	0.061	-0.34	0.057	-0.18	0.057
Log income, age 10	-0.0064	0.079	-0.30	0.074	-0.20	0.075
Sample size	5,414		5,409		5,456	

Table 5.6 Income change, ages ten to 16 and behaviour at age 16 (full model): BCS70

	Emotion		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	-0.052	0.079	-0.13	0.074	-0.030	0.076
Log income, age 1 ten 0	-0.030	0.10	-0.18	0.093	0.023	0.095
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.56	0.075	0.066	0.071	-0.30	0.071
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	-0.065	0.088	-0.12	0.081	-0.12	0.083
Emotion, age ten	0.59	0.037	n.a.	n.a.	n.a.	n.a.
Emotion, age five	0.17	0.023	n.a.	n.a.	n.a.	n.a.
Conduct, age ten	n.a.	n.a.	0.59	0.043	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.34	0.032	n.a.	n.a.
Attention, age ten	n.a.	n.a.	n.a.	n.a.	0.60	0.039
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.32	0.036
Mother's NVQ						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	0.22	0.086	-0.13	0.080	-0.13	0.082
3	0.11	0.18	-0.25	0.17	-0.092	0.17
4	0.19	0.14	-0.21	0.14	-0.41	0.14
Sample size	4,280		4,298		4,310	

We also examine the effects of income change on log earnings of those cohort members in employment at age 34 (see Section 3.2.5). The estimates from the full model are given in Table 5.7. They indicate that there is an effect in that doubling family income at age 16 for fixed income at age ten leads to a rise in weekly log earnings of 0.13 or £1.1 per week at age 34. This finding is not affected by the inclusion of fathers' educational level in the model but should, nevertheless, be treated with caution as we discuss in Chapter 7.

Table 5.7 Income change, ages ten to 16 and log weekly earnings at age 34 (full model): BCS70

	Log earnings, 34	
Variable	**Estimate**	**S.E.**
Log income, age 16	0.19	0.040
Log income, age 10	-0.012	0.049
Sex		
Boy (reference)	0	n.a.
Girl	-0.68	0.037
Tenure		
Rent (reference)	0	n.a.
Own	0.044	0.043
Maths, age ten	0.072	0.028
Reading, age ten	0.057	0.028
EPVT, age five	0.048	0.021
Copying designs, age five	0.012	0.020
Emotion, age ten	-0.017	0.020
Emotion, age five	0.017	0.012
Conduct, age ten	-0.0080	0.023
Conduct, age five	0.0045	0.018
Attention, age ten	-0.021	0.022
Attention, age 5	0.025	0.020
Mother's NVQ		
1 (reference)	0	n.a.
2	0.0018	0.041
3	0.050	0.086
4	0.016	0.072
R^2	0.23	
Sample size	1,758	

5.2 Effects of change in benefit status

5.2.1 Measures of benefit status

The family of the cohort member received income related (or means tested) benefits at ages ten and 16 when at least one of the three conditions was true:

1 Family received Family Income Supplement.

2 Family received Supplementary Benefit.

3 Family received Unemployment Benefit.

As claiming free school meals (FSM) indicates receipt of income-related benefits, we replaced 1895 missing benefit status entries at age ten and 118 at age 16 using FSM status at the corresponding ages. In addition, 299 observations at age ten and 143 observations at age 16 were changed from ‘not receiving benefits’ to ‘receiving benefits’ because they reported that they had been claiming FSM.

Table 5.8 Benefit status change between ages ten and 16: BCS70

	Observed sample		Analysis sample	
Benefits at ages ten and 16	**Number**	**%**	**Number**	**%**
10: No, 16: No	6,208	73	1,715	77
10: Yes, 16: No	566	6.8	129	5.8
10: No, 16: Yes	1,076	13	244	11
10: Yes, 16: Yes	697	7.7	131	5.9
Total	**8,547**	**100**	**2,219**	**100**

Table 5.8 shows that the percentage receiving benefits increased between the ages ten and 16 from 15 per cent to 21 per cent, which is in line with the increase in the unemployment rate during the same period. The analysis sample (i.e. the sample used to generate the estimates in Table 5.12) was more advantaged than the observed sample and the numbers of cases either entering or exiting benefits are rather small.

Benefit status change between ages ten and 16 is associated with change in the number of parental figures in the household for the same time period. We can see from Table 5.9 that for the cohort members who live with one parental figure at age ten and two parental figures at age 16, the number of families who moved out of benefits is four times larger than the number of families that moved into benefits. And for the cohort members who live with two parental figures at age ten and only one at age 16, the number of families who moved into benefits is five times larger than the number of families who moved out of benefits. The same kinds of conclusions can be drawn from the association between paternal employment status change (full-time compared to other) and benefit status change (Table 5.10). In other words, movement into and out of benefits is, as expected, strongly associated with movement in and out of single parenthood and of full-time employment.

Table 5.9 Benefit status change, ages ten to 16, and change in number of parental figures in the household: BCS70

	10:NB, 16:NB (%)	10:B, 16:NB (%)	10:NB, 16:B (%)	10:B, 16:B (%)	Total
10:1P, 16:1P	29	11	9.0	51	251 (100%)
10:1P, 16:2P	30	31	8.0	31	291 (100%)
10:2P, 16:1P	45	6.0	33	16	330 (100%)
10:2P, 16:2P	78	5.0	12	5.0	7,369 (100%)
Total	6,023 (73%)	535 (6.5%)	1,036 (13%)	647 (7.9%)	8,241 (100%)

Note:
NB: not on benefits; B: on benefits.
1P: one parent; 2P: two parents.

Table 5.10 Benefit status change, ages ten to 16 and change in paternal full-time employment status: BCS70

	10:NB, 16:NB (%)	10:B, 16:NB (%)	10:NB, 16:B (%)	10:B, 16:B (%)	Total
10:FT, 16:FT	89	3.1	7.0	0.9	3,882 (100%)
10:FT, 16:O	44	3.0	41	12	489 (100%)
10:O, 16:FT	62	27	4.0	7.1	135 (100%)
10:O, 16:O	23	12	10	55	163 (100%)
Total	3,790 (81%)	208 (4.5%)	481 (10%)	190 (4.1%)	4,669 (100%)

Note:
NB: not on benefits; B: on benefits.
FT: full-time; O: not full-time.

5.2.2 Results

The estimates in Table 5.11 are based on a series of regressions and show that benefit status change and educational attainments at age 16 are associated. Adolescents with any experience of living in a family on means tested benefits have lower scores, especially if their family was on benefits at ages ten and 16. On the other hand, as set out in Chapter 2, our hypothesis that attainments will be higher if the family is moving out of benefits compared with those moving in is not supported. The differences between the two 'change' groups are small and are not consistently in favour of those exiting from benefits.

Table 5.11 Benefit status change, ages ten to 16 and educational attainments at age 16: BCS70

	Maths16		Vocab16		Spell A 16		Spell B 16	
Benefit status change	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
10:NB, 16:NB (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
10:B, 16:NB	-0.33	0.083	-0.19	0.066	-0.26	0.066	-0.16	0.066
10:NB, 16:B	-0.32	0.062	-0.30	0.048	-0.23	0.048	-0.18	0.047
10:B, 16:B	-0.56	0.085	-0.43	0.066	-0.40	0.066	-0.40	0.066
R^2	0.029		0.018		0.015		0.012	
Sample size	2,537		4,257		4,178		4,085	

The introduction of a range of control variables reduces the estimates of the benefit status change groups very close to zero as shown in Table 5.12. In other words, a change in benefit status is not associated with educational progress from age ten to age 16. Nor is there any evidence that it is associated with a change in the odds of having a behaviour problem as shown in Table C.4.

Table 5.12 Benefit status change, ages ten to 16 and educational progress: BCS70

	Maths16		Vocab16		Spell A 16		Spell B 16	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Benefit status change								
10:NB, 16:NB (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
10:B, 16:NB	-0.045	0.087	0.049	0.077	0.030	0.079	0.060	0.078
10:NB, 16:B	-0.021	0.065	0.022	0.059	-0.048	0.058	-0.016	0.058
10:B, 16:B	-0.0011	0.090	0.0085	0.080	-0.052	0.079	-0.062	0.079
Sex								
Boy (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Girl	0.019	0.042	0.079	0.037	0.26	0.037	0.27	0.037
Tenure								
Rent (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Owner	0.14	0.048	0.064	0.043	-0.061	0.042	-0.0018	0.043
Maths, age ten	0.46	0.033	0.16	0.029	0.12	0.029	0.13	0.029
Reading, age ten	0.19	0.033	0.34	0.029	0.34	0.029	0.28	0.029
EPVT, age five	0.030	0.023	0.094	0.021	-0.026	0.021	-0.0040	0.021
Copying designs, age five	0.10	0.023	0.016	0.021	0.045	0.021	0.052	0.021
Mother's NVQ								
1 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
2	0.015	0.046	0.11	0.041	0.070	0.041	0.061	0.041
3	-0.022	0.10	0.26	0.088	0.032	0.088	-0.029	0.087
4	0.12	0.075	0.28	0.066	0.068	0.065	0.019	0.065
R^2	0.44		0.27		0.22		0.20	
Sample size	1,316		2,219		2,183		2,143	

In the light of the results in Section 5.1.2, we might expect to find an effect of benefit status change on log earnings at age 34. However, as Table 5.13 shows, although the effect is in the expected direction, the difference between exiting and entering benefits (= 0.084) is not statistically significant at any conventional level ($p > 0.20$).

Table 5.13 Benefit status change, ages ten to 16 and log weekly earnings at age 34 (full model): BCS70

	Log earnings, 34	
Variable	**Estimate**	**S.E.**
Benefit status change		
10:NB, 16:NB (reference)	0	n.a.
10:B, 16:NB	-0.030	0.063
10:NB, 16:B	-0.11	0.050
10:B, 16:B	-0.12	0.074
Sex		
Boy (reference)	0	n.a.
Girl	-0.67	0.032
Tenure		
Rent (reference)	0	n.a.
Owner	0.097	0.036
Maths, age ten	0.073	0.024
Reading, age ten	0.060	0.024
EPVT, age five	0.039	0.018
Copying designs, age five	0.025	0.018
Emotion, age ten	-0.026	0.017
Emotion, age five	0.017	0.011
Conduct, age ten	-0.016	0.020
Conduct, age five	0.0012	0.015
Attention, age ten	-0.021	0.019
Attention, age five	0.023	0.017
Mother's NVQ		
1 (reference)	0	n.a.
2	0.029	0.035
3	0.069	0.076
4	0.077	0.059
R^2	0.21	
Sample size	2,394	

5.3 Effects of change in parental employment status

Parental employment status is defined as in Chapter 2 (Section 2.2.1) and as used in Chapters 3 and 4. Its distributions for the period in question are given in Table 5.14 which shows that the differences between the observed and analysis samples are small and also that there are few transitions in the analysis samples.

The effects of change in parental employment status are uniformly small and so the model estimates are confined to Appendix C (Tables C.5 to C.7). These models include both mothers' and fathers' educational levels because these results are generalisable only to families with two parents at each occasion (because of the way change in parental employment status is defined). Note that there is no effect of parental employment status change on log earnings at age 34.

Table 5.14 Parental employment status change between ages ten and 16: BCS70

	Observed sample		Analysis sample	
Parental employment status	**Number**	**%**	**Number**	**%**
-2	21	0.48	16	1.2
-1.5	65	1.5		
-1	277	6.4	95	7.0
-0.5	394	9.1	130	9.6
0	2291	53	722	53
0.5	1102	25	333	25
1	154	3.6	48	3.6
1.5	23	0.53	9	0.67
2	13	0.30		
Total	**4,340**	**100**	**1,353**	**100**

5.4 Summary

- There is no evidence that a change in family income benefit status or of parental employment status between the ages of ten and 16 has any effect on educational progress or behaviour during this period of adolescence. The evidence is, however, based on data that are less than ideal.
- There is a suggestion that a change in family income between ages ten and 16 affects earnings at age 34, although this finding is not supported by the analyses of changes in benefit status and parental employment status.

6 Predicting income and income change from other socio-economic variables

A drawback of the British Cohort Study 1970 (BCS70) data on income collected during the childhood years is that they are incomplete in a number of ways. We investigate the possibility of imputing income for the BCS70 cohort members by drawing on related data from other studies with good measures of (a) income such as the Family Expenditure Survey (FES) and (b) earnings – the New Earnings Survey (NES).

6.1 Family Expenditure Survey

The FES is a repeated cross-sectional survey of households in the UK which ran between 1957 and 2001. It was a survey of household expenditure and income and its original aim was to provide information on spending for the Retail Price Index. The FES contains additional information on household characteristics such as household composition and size and employment status and education of the household head.

6.1.1 Sample and variables

We use FES data for the years 1980 and 1986. The sample sizes are 6,994 households with 18,844 individuals (1980) and 7,178 households with 18,330 individuals (1986). The response rates were 67 per cent (1980) and 69 per cent (1986). We are interested in the relation between characteristics of households and heads of households to gross household and family income. We exclude all household heads with missing occupational social class, missing year that full-time education ceased, weekly gross household income less than a pound, and those who are self-employed or retired. The self-employed have been excluded as the relation between household head characteristics and household income is likely to be different from the corresponding relationship for employees. This might partly be due to the different ways that the self-employed report personal earnings as a proportion of profit. Hence, we include 4,238 household heads (61 per cent of the total) and 3,962 household heads (57 per cent of the total) in the 1980 and 1986 analysis datasets.

The outcome variables are (i) gross household income as defined in the FES and (ii) gross family income excluding Child Benefit as this had been excluded from the measure of income in BCS70. As expected, the income variables are not Normally distributed and so we use a log-transformation to obtain a Normally distributed response variable for each year. The quartiles of gross weekly household income in 1980 and 1986 are shown in Table D.1.

The explanatory or predictor variables used in our models for predicting household income have been chosen on the basis of the literature in this area (Dale *et al.*, 1995; Nicoletti and Peracchi, 2006). Because we are using the FES datasets to construct prediction or imputation models for the corresponding income variables in BCS70, we only include predictors that are contained in both FES and BCS70 in each year. In addition, we test the stability of the imputation models over time and for this reason we only include variables in the FES 1980 and 1986 datasets that are coded in the same way in both years.

The relevant determinants of gross household income can be placed into two groups: (i) household and (ii) head of the household. At the household level, we include the number of children (i.e. the number of persons less than 18 years old) and the total number of persons in the household (see Tables D.2 and D.3). From these tables, we can see that the proportion of childless households and the proportion of one person households increased between 1980 and 1986. It is important to bear in mind that we are trying to predict income for households containing a ten year old in 1980 and a 16 year old in 1986. We do not, however, have sufficient numbers of observations on households with these characteristics in FES so we assume that income predicted from the analysis sample (that contains households of all types) can be applied to these sub-samples of households.

Home ownership is another predictor in our analysis models (Table D.4). The majority of households in the analysis samples own the home the live in. The percentage increased from 60 per cent to 71 per cent between 1980 and 1986.

At the household head level, we include the age of the household head (Table D.5); gender (Table D.6) and marital status (Table D.7). The percentage of female household heads increased slightly from 11 per cent in 1980 to 13 per cent in 1986. The percentage of non-married household heads increased from 19 per cent in 1980 to 26 per cent in 1986.

The number of years of post-compulsory education is shown in Table D.8. This variable has been derived from the age of the person at the interview and the age full-time education ceased, to create a categorical variable with five categories as a proxy for the National Vocational Qualification (NVQ) levels. The percentage of household heads with no post-compulsory education decreased from 58 per cent in 1980 to 52 per cent in 1986.

Finally, the occupational social class of the household head was included as a predictor of 1980 and 1986 gross household income. The distribution of this variable moved in line with the changing occupational structure of that period so that there are slightly higher percentages of household heads in non-manual occupations in 1986 than in 1980 (Table D.9).

6.1.2 Statistical modelling

The statistical analysis links gross household and family income with the explanatory variables using FES for years 1980 and 1986. Our regression model is the following:

$$y = b_0 + \sum_{k=1}^{K} b_k X_k + e$$

where y is the logarithm of gross household or family income, x_k are the predictors set out in Section 6.1.1, b_k (k = 1..K) are the regression coefficients of interest and e is the residual term. We include linear and quadratic terms for age of household head and interactions between (i) occupational social class and the number of children and (ii) age (both linear and quadratic terms) and number of children. For 1980 only, we include an interaction between gender of the household head and the number of children in the household and for 1986 only we include the marital status of the household head and its interaction with the number of children in the household.

The results for the 1980 and 1986 model specifications are shown in Tables 6.1 and 6.2. In both models we have included variables that are statistically significant at the five per cent level.

Table 6.1 Predicting log (gross household income): 1980 FES

Variable	Estimate	S.E.
Age, household head		
Linear	0.036	0.0038
Quadratic	-0.00044	0.000043
Occupational social class		
I (reference)	0	n.a.
II	0.086	0.027
III non-manual	-0.12	0.031
III manual	-0.11	0.027
IV	-0.30	0.029
V	-0.38	0.039
Unemployed	-0.72	0.043
Number of children	-0.80	0.083
Number of children x Occupational social class		
Number of children x I (reference)	0	n.a.
Number of children x II	-0.024	0.019
Number of children x III non-manual	-0.022	0.023
Number of children x III manual	0.0027	0.017
Number of children x IV	0.060	0.019
Number of children x V	0.057	0.024
Number of children x Unemployed	0.079	0.034
Home owner		
No (reference)	0	n.a.
Yes	0.21	0.013
Household size	0.35	0.0094
Sex, household head		
Male (reference)	0	n.a.
Female	-0.19	0.023
Female household head x Number of children	0.038	0.019
Years of post-compulsory education		
0 (reference)	0	n.a.
1	0.039	0.016
2	0.060	0.021
3-5	0.11	0.023
6+	0.20	0.025
Age, household head x Number of children	0.018	0.0040
Age^2 x Number of children	-0.00017	0.000049
R^2	0.55	
Sample size	4,238	

Table 6.2 Predicting log (gross household income): 1986 FES

Variable	Estimate	S.E.
Age, household head		
Linear	0.036	0.0046
Quadratic	-0.00043	0.000052
Occupational social class		
I (reference)	0	n.a.
II	-0.048	0.032
III non-manual	-0.19	0.036
III manual	-0.26	0.031
IV	-0.41	0.035
V	-0.48	0.048
Unemployed	-0.74	0.044
Number of children	-0.73	0.12
Number of children x Occupational social class		
Number of children x I (reference)	0	n.a.
Number of children x II	-0.0067	0.023
Number of children x III non-manual	-0.030	0.029
Number of children x III manual	0.022	0.021
Number of children x IV	0.053	0.024
Number of children x V	0.021	0.034
Number of children x Unemployed	0.075	0.043
Marital status		
Unmarried (reference)	0	n.a.
Married	0.090	0.026
Marital status, married X number of children	-0.061	0.023
Home owner		
No (reference)	0	n.a.
Yes	0.40	0.017
Household size	0.34	0.012
Sex, household head		
Male (reference)	0	n.a.
Female	-0.23	0.029
Years of post-compulsory education		
0 (reference)	0	n.a.
1	0.073	0.019
2	0.10	0.028
3-5	0.23	0.027
6+	0.28	0.028
Age, household head x Number of children	0.018	0.0056
Age^2 x Number of children	-0.00019	0.000069
R^2	0.57	
Sample size	3,962	

To test whether the 1980 and 1986 model specifications are statistically different, we fitted a model with separate intercepts for each year and interactions between year and each of the predictors. The results indicate that the effect differences are statistically significant at the five per cent level for occupational social class of the household head, the number of children, the interaction between occupational social class and the number of children, home ownership and years of post-compulsory education of the household head. The overall joint F-tests for the 1980 model specification (using 1980 and 1986 data) and the 1986 model specification (again using 1980 and 1986 data) are respectively 76.7 (26 degrees of freedom; $p<0.001$) and 74.7 (27 d.f.; $p<0.001$). Table 6.3 shows how the point estimates for 1980 and 1986 differ when the 1986 model specification is used.

The main conclusions from modelling gross household income are:

i it is possible to account for over half the variation with a combination of variables that characterise the household and its head;

ii some estimates vary – although not markedly – according to whether there are children in the household but some do not. It is not unreasonable to predict income for households with children from the FES sample as a whole;

iii there are, as expected, marked effects of social class, years of education and home ownership and these effects are stronger in 1986 than in 1980. These differences point to the importance of basing predictions on data from the years in question.

As we have seen from Chapter 5, respondents to the question about family income in the 1980 and 1986 sweeps of BCS70 were expected to include mother's and father's earned and unearned income but not Child Benefit and not income from other household members. As gross household income is not necessarily equal to gross family income, we have investigated the size of the effects of variables in the 1980 and 1986 household income models for predicting family or parental household income in the corresponding years. We find that we predict 51 per cent of the variation in family income in 1980 and 56 per cent in 1986.

Tables 6.4 and 6.5 compare the estimates for the predictors of household income and family income in 1980 and 1986. We see that the effects of the number of children and number of persons in the household are larger for household income in both years. On the other hand, the negative effect of a female household head is stronger for family income than it is for household income in 1980 but not in 1986, perhaps because marital status is in the 1986 model.

Table 6.3 1986 model specification for log (gross household income) using 1980 and 1986 FES data

Variable	1980 Estimate	1986 Estimate
Age, household head		
Linear	0.037	0.036
Quadratic	-0.00044	-0.00043
Occupational social class		
I (reference)	0	0
II	0.086	-0.048
III non-manual	-0.12	-0.19
III manual	-0.11	-0.26
IV	-0.30	-0.41
V	-0.38	-0.48
Unemployed	-0.72	-0.74
Number of children	-0.76	-0.73
Number of children x Occupational social class		
Number of children x I (reference)	0	0
Number of children x II	-0.024	-0.0067
Number of children x III non-manual	-0.022	-0.030
Number of children x III manual	0.0035	0.022
Number of children x IV	0.061	0.053
Number of children x V	0.058	0.021
Number of children x Unemployed	0.080	0.075
Marital status		
Unmarried (reference)	0	0
Married	0.056	0.090
Marital status, married x number of children	-0.038	-0.061
Home owner		
No (reference)	0	0
Yes	0.20	0.40
Household size	0.34	0.34
Sex, household head		
Male (reference)	0	0
Female	-0.15	-0.23
Years of post-compulsory education		
0 (reference)	0	0
1	0.040	0.073
2	0.061	0.10
3-5	0.11	0.23
6+	0.21	0.28
Age, household head x Number of children	0.018	0.018
Age^2 x Number of children	-0.00017	-0.00019
Sample size	8,200	

Table 6.4 Comparing household and family income estimates: 1980 FES

Variable	Household	Family
Age, household head		
Linear	0.036	0.036
Quadratic	-0.00044	-0.00042
Occupational social class		
I (reference)	0	0
II	0.086	0.066
III non-manual	-0.12	-0.17
III manual	-0.11	-0.13
IV	-0.30	-0.35
V	-0.38	-0.47
Unemployed	-0.72	-0.89
Number of children	-0.80	-0.46
Number of children x Occupational social class		
Number of children x I (reference)	0	0
Number of children x II	-0.024	-0.015
Number of children x III non-manual	-0.022	-0.0043
Number of children x III manual	0.0027	-0.0031
Number of children x IV	0.060	0.053
Number of children x V	0.057	0.061
Number of children x Unemployment	0.079	0.0871
Home owner		
No (reference)	0	0
Yes	0.21	0.26
Household size	0.35	0.077
Sex, household head		
Male (reference)	0	0
Female	-0.19	-0.50
Female household head x Number of children	0.038	-0.062
Years of post-compulsory education		
0 (reference)	0	n.a.
1	0.039	0.051
2	0.060	0.065
3-5	0.11	0.11
6+	0.20	0.19
Age, household head x Number of children	0.018	0.018
Age^2 x Number of children	-0.00017	-0.00019
R^2	0.55	0.51
Sample size	4,238	

Table 6.5 Comparing household and family income estimates: 1986 FES

Variable	Household	Family
Age, household head		
Linear	0.036	0.044
Quadratic	-0.00043	-0.00051
Occupational social class		
I (reference)	0	0
II	-0.048	-0.071
III non-manual	-0.19	-0.21
III manual	-0.26	-0.27
IV	-0.41	-0.44
V	-0.48	-0.53
Unemployed	-0.74	-0.85
Number of children	-0.73	-0.39
Number of children x Occupational social class		
Number of children x I (reference)	0	0
Number of children x II	-0.0067	0.0030
Number of children x III non-manual	-0.030	-0.026
Number of children x III manual	0.022	0.017
Number of children x IV	0.053	0.054
Number of children x V	0.021	0.036
Number of children x Unemployment	0.075	0.094
Marital status		
Unmarried (reference)	0	0
Married	0.090	0.49
Marital status, married x number of children	-0.061	0.016
Home owner		
No (reference)	0	0
Yes	0.40	0.44
Household size	0.34	0.0094
Sex, household head		
Male (reference)	0	0
Female	-0.23	-0.22
Years of post-compulsory education		
0 (reference)	0	0
1	0.073	0.091
2	0.10	0.14
3-5	0.23	0.23
6+	0.28	0.31
Age, household head x Number of children	0.018	0.014
Age^2 x Number of children	-0.00019	-0.00016
R^2	0.57	0.56
Sample size	3,962	

6.2 New Earnings Survey

The main purpose of the NES (and since 2004 the Annual Survey of Hours and Earnings (ASHE)) is to record annual information about the earnings of employees. The information is collected by questionnaires completed by the employer using payroll records for the employee. The earnings, working hours and other related information correspond to a week in April of each year. The survey has been in almost the same format since 1970 and changed very little after 1975. This stability has made possible the creation of a linked data set. This longitudinal form of the NES that we have used for our analysis has become known as the New Earnings Survey Panel Dataset (NESPD).

6.2.1 Sample and variables

For the NES, we are interested in the relation between individuals' characteristics, as provided by the employer and the gross weekly earnings of the employee. We included individuals with information available in both 1980 and 1986 and exclude cases where the gender and age variables are not consistent. In addition, we removed from the analysis sample employees with loss of pay due to sickness, absence, short-time working or employment starting within the period of interest, and those who held more than one job in the same period. Finally, we excluded observations where the gross weekly earnings were less than a pound and those where the standard region in which the employee's place of work was based is missing. The analysis sample contains 76,725 individuals (**not** households).

The main outcome variable is the change in gross weekly log-earnings for each individual in the NES between years 1980 and 1986. This variable is derived by subtracting the logarithm of the total weekly earnings before deductions adjusted for the inflation between 1980 and 1986 from the logarithm of the total weekly earnings before deductions in 1986. The quartiles of the gross weekly log-earnings in 1980 and 1986 and the corresponding change variable mentioned above are shown in Tables D.10 and D.11.

In the model for predicting change in log-earnings, we use, in addition to age and gender, relevant time-varying predictors. One of them is change in employment status between 1980 and 1986. The baseline category is no change in the employment status and the other two categories are transition between full-time in 1980 and part-time in 1986 and between part-time in 1980 and full-time in 1986. The distribution of employment status change is shown in Table D.12. The percentage of employees moving from full-time to part-time employment is slightly higher from the percentage of those moving from part-time to full-time.

Change in occupational social class between 1980 and 1986 is the other variable used as a predictor of change in log-earnings for the same time period. For confidentiality reasons, it is not possible to present the complete distribution of this change as there are combinations with less than ten individuals. Instead we can see the distributions of occupational social class in 1980 and 1986 in Table D.13.

As one would expect, the characteristics of the FES and NES samples are different. Thus, 64 per cent of the NES sample are male compared with nearly 90 per cent for the head of household in the FES. The NES sample are also younger with a median age of 36 compared with 41 in the FES.

6.2.2 Statistical modelling

The statistical analysis links change in log-earnings between 1980 and 1986 with employee characteristics using the NESPD. The regression model used for the FES (Section 6.1.2) was used again with the explanatory variables as set out in Section 6.2.1. These predictors were chosen as they are also available for the same time period in the BCS70 analysis sample. The regression estimates are given in Table 6.6. They are generally in the expected direction with movements up the occupational scale usually predicting increases in earnings. The model explains 34 per cent of the variation in change in log earnings.

Table 6.6 Predicting change in log-earnings between 1980 and 1986: NES

Predictor	Estimate	S.E.
Age		
Linear	-0.032	0.00073
Quadratic	0.00030	0.00000099
Female	0.020	0.0029
Employment status change		
No change (reference)	0	n.a.
Full-time to part-time	-0.87	0.0069
Part-time to full-time	0.68	0.075
Occupational social class change		
No change (reference)	0	n.a.
I to II	0.12	0.010
I to III non-manual	0.017	0.018
I to III manual	0.047	0.029
I to IV	-0.010	0.033
I to V	-0.29	0.050
II to I	0.034	0.093
II to III non-manual	-0.19	0.010
II to III manual	-0.080	0.021
II to IV	-0.18	0.020
II to V	-0.19	0.031
III non-manual to I	0.15	0.011
III non-manual to II	0.11	0.007
III non-manual to III manual	-0.029	0.015

Continued

Table 6.6 Continued

Predictor	Estimate	S.E.
III non-manual to IV	-0.10	0.011
III non-manual to V	-0.18	0.015
III manual to I	0.017	0.020
III manual to II	0.075	0.014
III manual to III non-manual	-0.075	0.013
III manual to IV	-0.10	0.094
III manual to V	-0.19	0.013
IV to I	0.094	0.028
IV to II	0.016	0.017
IV to III non-manual	-0.048	0.012
IV to III manual	-0.039	0.011
IV manual to V	-0.17	0.011
V to I	0.030	0.031
V to II	0.13	0.027
V to III non-manual	0.074	0.015
V to III manual	-0.030	0.017
V manual to IV	-0.065	0.012
R^2	0.34	
Sample size	76,725	

6.3 Comparing the FES and BCS70

To assess the predictive performance of the regression model linking FES gross family income to the characteristics of households and household heads, we examine whether the predicted income values are in agreement with the observed banded income in both the FES and BCS70. We convert the continuous predicted measure (y^P – derived from regression models predicting log-family income, see Section 6.1.2 for details) into the banded version and then we compare the predicted band to the corresponding observed band in FES (internal validation) and BCS70 (external validation).

The samples sizes for BCS70 in 1980 and 1986 are 14,875 and 11,615 respectively. We exclude all cohort members who lived in households where the household head has missing values for any one of the following variables: gender, age, occupational social class, years of post-compulsory education and employment status of the household head. We also exclude households with self-employed household heads. In addition, we do not include cohort members who lived in households where the total number of children or number of persons in the household is missing. Using these exclusion criteria, the number of BCS70 cohort members for whom family income imputation has been applied is much reduced: 4,224 in 1980 and 2,262 in 1986.

For external validation, we can only use BCS70 observations where income is not missing. In this case, the number of BCS70 cases used for imputation validation is 3,986 in 1980 and 1,779 in 1986.

From Table 6.7 we can see that for FES data in 1980, 39 per cent of the predicted income bands are the same as the observed bands whereas for BCS70 the corresponding success in predicting income band falls to 27 per cent. There is a tendency for the FES observed banded income distribution to be shifted up by one band when compared to the observed BCS70 distribution, probably because the mean observed income in BCS70 (using the Singh-Maddala estimates for the bands) is lower than the mean predicted value from FES (£131 compared with £165 for 1980 and £262 compared with £281 for 1986). In turn, this discrepancy might have arisen because the single question used in BCS70 was not picking up all sources of income.

It is, therefore, sensible to expect that the predicted band using an FES regression model will predict a number of observations in a band higher than that observed in BCS70. Thus, we also assess the predictive performance of the model in 1980 by comparing the predicted band with the actual band or the actual band plus one. In this case, the agreement between predicted and target band (actual band or actual band plus one) is 62 per cent for FES observations and 68 per cent for BCS70 observations.

Table 6.7 Agreement between actual and predicted income band (1980 data)

Agreement with predicted band	Actual FES		Actual BCS70		Actual FES or (Actual FES +1)		Actual BCS70 or (Actual BCS70 +1)	
	Number	%	Number	%	Number	%	Number	%
No	2,586	61	2,913	73	1,607	38	1,258	32
Yes	1,652	39	1,073	27	2,631	62	2,728	68
Total	**4,238**	**100**	**3,986**	**100**	**4,238**	**100**	**3,986**	**100**

In a similar way to Table 6.7, but now using 1986 FES and BCS70 data, we find that the agreement of the predicted and actual class is lower than in 1980: 25 per cent for FES data and 17 per cent for BCS data. This is also true for the comparison between the agreement of predicted band with actual or actual plus one band in 1986 when compared with 1980. The fact that the agreement is lower in 1986 compared to 1980 might be due to the fact that, in BCS70, there are 11 bands for the 1986 income variable compared to seven for the corresponding 1980 variable. It is also important to realise that we are comparing predicted income from FES with the observed band in BCS70. The observed band is not necessarily the true band; indeed, the discrepancy between the observed and true bands is likely to increase as the number of bands increases.

Table 6.8 Agreement between actual and predicted income band (1986 data)

Agreement with predicted band	Actual FES		Actual BCS70		Actual FES or (Actual FES +1)		Actual BCS70 or (Actual BCS70 +1)	
	Number	%	Number	%	Number	%	Number	%
No	2,989	75	1,483	83	2,222	56	1,068	60
Yes	973	25	296	17	1,740	44	711	40
Total	3,962	100	1,779	100	3,962	100	1,779	100

As there is prediction error attached to the maximum likelihood (ML) prediction from the regression model, it is important to take into account this uncertainty around the prediction by drawing samples from the prediction distribution for each observation. We assume that the prediction distribution of each observation follows a Normal distribution with mean and standard deviation corresponding to the ML estimate and its associated prediction standard error and we draw five imputations for each observation. Then we count the number of predicted bands agreeing with the actual income band.

From Table 6.9, we can see that 55 per cent of FES observations have no predicted classes agreeing with the observed class (61 per cent in Table 6.7) and the corresponding percentage for BCS70 is 68 per cent (73 per cent in Table 6.7). On the other hand, 32 per cent of FES and 23 per cent of BCS70 observations have all five of the imputations agreeing with the actual band.

Table 6.9 Number of imputations agreeing with actual income band (1980 data)

	FES		BCS70	
Number of agreements	Number	%	Number	%
0	2,318	55	2,725	68
1	158	4	114	3
2	122	3	70	2
3	120	3	67	2
4	149	3	97	2
5	1,371	32	913	23
Total	**4,238**	**100**	**3,986**	**100**

From Table 6.10, we see that the percentage of observations with no agreements between the multiple imputations and the actual band in 1986 is higher. For FES predictions in 1986, there are no agreements for 68 per cent of all cases and 77 per cent for BCS70 in the same year (83 per cent in Table 6.8). As expected, the percentage of complete agreements between multiple imputations and observed bands is lower when compared with 1980. Specifically, only 17 per cent of FES observations have all multiple imputations in agreement with the observed band in 1986 and the corresponding percentage for BCS70 in 1986 is 11 per cent.

Table 6.10 Number of imputations agreeing with actual income band (1986 data)

	FES		BCS70	
Number of agreements	**Number**	**%**	**Number**	**%**
0	2,693	68	1,369	77
1	159	4	68	4
2	131	3	51	3
3	146	4	45	2
4	171	4	56	3
5	662	17	190	11
Total	**3,962**	**100**	**1,779**	**100**

6.4 Comparing NES and BCS70

There are 1,419 observations in BCS70 where the variables in Table 6.6 are available and can be used to obtain a relationship between earnings and family income in BCS70 and NES. We obtain predicted changes in log-earnings from NES and we compare these with changes in family income bands in BCS70 after adjusting for inflation between 1980 and 1986. The predicted changes in log-earnings for the BCS70 family income change categories are shown in Table 6.11.

If there were a relationship between log-earnings changes and changes in family income, we would not expect to find an overlap between the distributions of change in log-earnings corresponding to each of the possible family income change types (negative, no change, positive). If this were so, the change in log-earnings could be used as a proxy for family income change.

Table 6.11 Predicted change in log-earnings for BCS70 family income change categories

Negative change (n=401)		No change (n=593)		Positive change (n=425)	
Quartile (%)	**Value**	**Quartile (%)**	**Value**	**Quartile (%)**	**Value**
25	0.08	25	0.08	25	0.09
50	0.12	50	0.12	50	0.13
75	0.17	75	0.16	75	0.16

From Table 6.11 we can see that the distributions for the different BCS70 change categories are in fact similar. Therefore, we conclude that predicted change in log-earnings cannot be used to separate those who experienced no change in self-reported family income from those who experienced negative change and those who experienced positive change.

6.5 Using predicted income in substantive models

In this section, we look at the effects of substituting our predicted values of income (generated from the FES) into some of the substantive models of interest that appear in Chapter 5. In particular, we replace the Singh-Maddala income values used in Tables 5.3 to 5.7 with the FES predicted values.

Table 6.12 Income change, ages ten to 16 and educational attainments at age 16: BCS70

	Maths 16		Vocab 16		Spell A 16		Spell B 16	
Income	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	0.81	0.18	0.37	0.14	0.11	0.13	0.16	0.13
Log income, age ten	0.63	0.27	0.71	0.21	0.61	0.20	0.49	0.20
R^2	0.12		0.056		0.027		0.023	
Sample size	591		1,121		1,095		1,073	

Table 6.13 Income change, ages ten to 16 and educational attainments at age 16 (full model): BCS70

	Maths 16		Vocab 16		Spell A 16		Spell B 16	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	0.61	0.20	-0.23	0.18	-0.13	0.17	-0.0027	0.17
Log income, age ten	-0.26	0.34	0.082	0.30	0.30	0.28	-0.099	0.28
Sex								
Boy (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Girl	-0.058	0.078	0.10	0.071	0.24	0.06	0.14	0.068
Tenure								
Rent (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Own	0.18	0.13	0.14	0.12	-0.088	0.11	0.11	0.11
Maths, age ten	0.36	0.062	0.10	0.057	0.12	0.054	0.094	0.054
Reading, age ten	0.24	0.060	0.35	0.056	0.32	0.053	0.30	0.053
EPVT, age five	0.088	0.045	0.13	0.041	0.029	0.039	0.037	0.039
Copying designs, age five	0.11	0.044	0.022	0.039	0.035	0.037	0.053	0.038
Mother's NVQ								
1 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
2	-0.17	0.085	0.015	0.078	-0.015	0.073	-0.11	0.074
3	-0.11	0.21	0.050	0.19	0.083	0.18	0.14	0.18
4	-0.16	0.15	0.27	0.13	0.077	0.13	-0.076	0.13
R^2	0.46		0.21		0.21		0.17	
Sample size	368		700		683		671	

Comparing Tables 6.12 and 6.13 with Tables 5.3 and 5.4, we see that:

i the sample sizes are much smaller in Tables 6.12 and 6.13 and the standard errors are higher;

ii the estimates for log income, age ten in Tables 6.12 and 6.13 are generally higher;

iii the estimates for log income, age 16 are much less consistent in Tables 6.12 and 6.13 across the four tests than they are in Tables 5.3 and 5.4.

Table 6.14 Income change, ages ten to 16 and behaviour at age 16: BCS70

	Emotion		Conduct		Attention	
Income	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	-0.56	0.27	-0.75	0.26	-1.01	0.27
Log income, age ten	0.23	0.41	-0.76	0.39	-0.059	0.40
Sample size	1,615		1,622		1,627	

Table 6.15 Income change, ages ten to 16 and behaviour at age 16 (full model): BCS70

	Emotion		Conduct		Attention	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Log income, age 16	-0.77	0.33	-0.57	0.31	-0.87	0.32
Log income, age ten	0.83	0.57	-0.033	0.54	0.25	0.57
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.46	0.13	0.030	0.12	-0.27	0.12
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	-0.0081	0.22	0.076	0.21	-0.073	0.22
Emotion, age ten	0.66	0.066	n.a.	n.a.	n.a.	n.a.
Emotion, age five	0.20	0.039	n.a.	n.a.	n.a.	n.a.
Conduct, age ten	n.a.	n.a.	0.60	0.08	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.34	0.06	n.a.	n.a.
Attention, age ten	n.a.	n.a.	n.a.	n.a.	0.64	0.073
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.44	0.066
Mother's NVQ						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	0.16	0.14	-0.16	0.13	-0.15	0.14
3	0.44	0.31	-0.17	0.32	0.099	0.32
4	-0.17	0.24	-0.10	0.23	-0.015	0.24
Sample size	1,504		1,516		1,522	

Comparing Tables 6.14 and 6.15 with Tables 5.5 and 5.6, we see that:

i the sample sizes are again much smaller in Tables 6.14 and 6.15 and the standard errors are higher;

ii the estimates for log income, age ten in Tables 6.14 and 6.15 are generally higher;

iii the estimates for log income, age 16 are much higher in Tables 6.14 and 6.15 across the three behaviours than they are in Tables 5.5 and 5.6.

Table 6.16 Income change, ages ten to 16 and log weekly earnings at age 34 (full model): BCS70

Variable	Log earnings, 34	
	Estimate	S.E.
Log income, age 16	0.34	0.15
Log income, age ten	-0.21	0.26
Sex		
Boy (reference)	0	n.a.
Girl	-0.70	0.060
Tenure		
Rent (reference)	0	n.a.
Own	0.20	0.10
Maths, age ten	0.054	0.048
Reading, age ten	0.053	0.048
EPVT, age five	0.060	0.038
Copying designs, age five	0.015	0.033
Emotion, age ten	-0.024	0.032
Emotion, age five	0.011	0.018
Conduct, age ten	-0.036	0.044
Conduct, age five	0.062	0.031
Attention, age ten	0.010	0.039
Attention, age five	-0.047	0.034
Mother's NVQ		
1 (reference)	0	n.a.
2	-0.0041	0.063
3	0.12	0.17
4	0.16	0.11
R^2	0.24	
Sample size	681	

Comparing Table 6.16 with Table 5.7, we see that the estimates for income are higher although the standard errors are also higher. We find an effect of income change on later earnings when we use imputed income just as we do when we use observed income.

6.6 Summary

- It is possible to get reasonably good predictions of household and family income from other socio-economic variables in the FES.
- Earnings as measured in the NES are less predictable, partly because there are rather few potential explanatory variables in the NES dataset.
- Using predictions from the FES to replace observed banded income in BCS70 (and other studies) shows some potential. There is a degree of consistency between the two sets of estimates although the model estimates when predicted income is used are rather unstable.
- There does not appear to be any value in using predicted earnings from NES to impute family income in the cohort studies.

7 Conclusions

In this final chapter, we reflect on the progress we have made in terms of establishing whether or not changes in families' economic circumstances lead to changes in their children's educational attainments and behaviours and also in their economic circumstances later in life. We do this by setting out our conclusions from each of the substantive chapters as they relate to changes in early childhood, middle childhood and adolescence and then by bringing those separate conclusions together to determine to what extent the existing state of knowledge, as set out in Chapter 1, has been reinforced or rejected.

The report has also had a strong methodological component and we assemble our various thoughts on these methodological issues in the final section of this report, along with suggestions for how research in this area might be taken further.

7.1 Substantive conclusions

It is important to bear in mind while reading these conclusions that none of them are based on what Plewis and Hawkes (2005) describe as an ideal observational study. Although some of the findings have been generated by analyses of data collected and brought together very recently – notably in the Millennium Cohort Study (MCS) and the National Pupil Database (NPD) – others are based on data collected as part of British Cohort Study 1970 (BCS70) in the 1980s. We cannot be sure that the relations between income and educational and behavioural outcomes that apply to the cohort born in Great Britain in 1970, would necessarily apply to more recent or future cohorts or indeed, to cohorts born elsewhere in the world. For example, the changing distribution of the ethnicity of families living in the UK might alter or modify the effects found here, as might changes in the distribution of family incomes. On the other hand, we have no evidence to support the idea that these relations are strongly linked to time period. Equally, we cannot be sure that the problems of missing data and attrition that are common to all longitudinal studies but which hit BCS70 particularly hard in the 1980s, have not had an effect on our results although, again, we have no evidence to suggest that missing data and non-response have affected our results. In other words, we have been analysing data of varying quality and we should not, therefore, regard the absence of evidence of a particular effect as evidence of absence of that effect. It is also important to remember that we have only analysed a limited range of outcomes; we do not know what the income effects on mental health might be, for example. Finally, we should not ignore the comments of Bradbury *et al.*, (2001, Chapter 2) who point to the importance of income for quality of life in the here and now, regardless of any causal link between poverty and later life chances.

7.1.1 Effects from early childhood

Changes in early childhood were considered in Chapter 3. For the analyses where we measured economic circumstances by income, we find some evidence of an effect on outcomes at age three from MCS. Our preferred analysis is the one that considers the effect of income at wave two (age three) on outcomes then, having controlled for income at wave one (age nine months) and including in the model as many other relevant control variables from wave one as possible. Essentially we find that, for two putative families having the same incomes at wave one, the child in the first family with twice the income at wave two as the second family will score a little better – perhaps one-sixteenth of a standard deviation unit higher – on the cognitive tests (Bracken and British Ability Scales (BAS)). This effect represents a difference of about one month in terms of educational progress. An effect of a similar size (in terms of Standard Deviation (SD) units) is found for the behaviour measure (the Strengths and Difficulties Questionnaire (SDQ)) although it makes no sense to translate this into a difference measured in months. These conclusions are supported if we average income over the two waves but not if we use the difference in income over the two waves.

Although there is evidence that the income effect for the MCS sample is probably causal and not due either to chance or to self-selection mechanisms, it is nevertheless small. It would be unusual for a family to experience such a marked rise in income over such a relatively short time span and, even if they did, their child can be expected to perform only a little better on the selected tests and measures. On the other hand, it is possible that this rather small difference at age three might be subject to a 'multiplier' effect of some kind such that a small difference at age three generates much larger differences at older ages and in adulthood. Evidence about such a multiplier effect depends on the passage of time as the MCS cohort ages.

We do not find an effect of income for children age three up to age six from the BCS70 sample of children of the cohort. The effects of income on cognitive tests and behaviour are very small and are not greater than we would expect by chance. The sample is, however, much smaller than the MCS sample. It is also an unusual sample in that it only includes children with mothers or fathers who are age 34.

We find no effects on outcomes when we use two other representations of economic circumstances: change in benefit status (MCS) and change in parental employment status (BCS70).

7.1.2 Effects from middle childhood

The only study that measured income for middle childhood is the study of the children of the BCS70 cohort. There was no support for any effect of change in economic circumstances on educational test scores from these data although there was a hint of an effect on behaviour. The same conclusions were reached when changes in parental employment status were modelled using data from BCS70.

The analysis of data from the NPD focused on change in benefit status in terms of claiming free school meals (FSM). There was a small effect on test scores at the end of Key Stage 2 (KS2) for reading and maths but not for science, after controlling for measures at Key Stage 1 (KS1). Pupils in families moving off FSM did make a little more progress – but no more than one-thirtieth of an SD unit or less than one month's progression – than children moving on to benefit. It is, of course, difficult to know just how great the increase in income was for the families exiting from benefit which means that comparisons with the effects found for early childhood are problematic.

7.1.3 Effects from adolescence

The literature would lead us to expect that changes in adolescence are less likely than changes earlier in childhood to produce effects. This is indeed what we find. The one exception to this pattern of results is a possible effect on weekly earnings at age 34: adolescents whose families experience an improvement in economic circumstances appear to earn a little more at age 34.

We should be wary of accepting this finding at face value because:

i it could just be one chance effect among many negative findings.

ii the analysis is restricted to those in employment; the picture might look different if the unemployed or people out of the labour market are included in the analysis.

iii we have no insight into the process underlying the possible effect (given that it does not appear to operate through improving test scores or reducing behaviour problems) nor do we know at present whether the effect is found at ages 26 and 30.

iv it is based on a sample that suffers from a lot of missing data.

Nevertheless, it is a finding that warrants further investigation and it is worth noting that Blanden and Gregg (2004) did find effects of income change during this period on educational qualifications.

7.1.4 Associations with poverty

The focus of this report has been on the causal effects of income as represented by the effects of changes in income that have, as far as it is possible to do so, been purged of self-selecting influences. It is, nevertheless, noteworthy that the associations of outcomes with being in poverty (as represented by receiving means tested benefits on at least two occasions) are higher than the income effects after controlling for at least some self-selecting features. Thus, from MCS, we find that being on benefits when the cohort member is age nine months and also three years, represents a difference of a little over three months in educational test scores at age three compared with those not claiming benefits at either age. And pupils claiming FSM for each of the four years leading up to KS2 tests at age 11 make nearly four months less progress between the ages of seven and 11 than pupils not claiming FSM at all. There are, however, no discernible effects from BCS70 of being on benefits at both ages ten and 16 on educational progress during that adolescent period.

7.1.5 Summary

We now return to the six statements about income effects set out in Section 1.2.

i *The effects on outcomes of income averaged over childhood (sometimes referred to as 'permanent' income) are greater than the effects of income measured at the same time as the outcome ('current' income).*

We did not test this hypothesis explicitly, preferring to model the effects of income at time t conditional on earlier measures of income rather than comparing the effects of average and current income. We do, nevertheless, find an effect for average income for early childhood. But we question (in Chapter 2) both the relevance of the concept of permanent income and its operationalisation for research questions of the kind considered here.

ii *The estimates of the 'effects' of income decline as additional explanatory variables, or controls, are added into regression-like models for outcomes.*

This is certainly borne out by our analyses such that effects of income changes are often substantially reduced by the introduction of other controls. In other words, the associations that are found between income change and outcomes before controls are introduced, probably reflect self-selection or choices made by family members rather than the causal effects of income.

iii *Income effects are small relative to the effects of other factors such as race.*

Again, we did not explicitly test this hypothesis. Indeed, we would question whether it is possible to talk about the causal effects of ascribed characteristics such as race. We do, however, find that even the income effects that we establish to be statistically significant are generally very small.

iv *Income effects for cognitive outcomes are generally larger than they are for behaviour outcomes.*

To the extent to which we are able to make these comparisons, we do not find this to be so. The effects for cognitive tests and a measure of behaviour are of a similar size in early childhood and equally small later on.

v *Income effects in early childhood tend to be greater than effects in late childhood.*

This is supported by our analyses: small effects from early childhood, smaller effects from middle childhood and no effects from adolescence (apart from the one possibly chance finding about effects on earnings at age 34).

vi *Income effects are non-linear, tending to be larger for low-income families.*

This appears to be true in that using a log transformation of income appears to fit the data better than using untransformed income.

To summarise even further, we find few effects on children's attainments and behaviour as they are growing up of changes in their families' economic circumstances and those that we find are small. It is interesting to note that the analyses based on what are probably the best two datasets we have used – MCS and NPD – do generate evidence of effects, albeit small ones. The analyses based on BCS70 have tended to rely either on proxies for income or on rather poor measures of family income and have, perforce, been based on a rather small proportion of the whole cohort.

We present little evidence in this report to gainsay the conclusion of Blow *et al.*, (2005) that income transfer programmes are not 'a quick fix' for improving child outcomes. On the other hand, the effects of such programmes or interventions are not directly equivalent to the naturally occurring changes analysed here. It is, nevertheless, plausible to suppose that in societies like the UK in the 21st century, where nobody experiences the extremes of poverty found in other parts of the world, that most parents are able to protect the spending required to enable their children to develop properly, at least in the short run, and this, combined with the universal services provided by the state, explains why the income effects reported here are small. In other words, parents borrow and go without luxuries for themselves when incomes are low and therefore, they smooth out expenditure directly related to children. This report is certainly not the last contribution to the debate: some outstanding research issues are outlined in the next section.

7.2 Methodological conclusions and outstanding issues

7.2.1 Imputing income

We have shown how it would be possible to introduce externally generated measures of family income into a dataset either (i) to obtain a measure of income where none previously existed; (ii) to replace a poor measure of income; or (iii) to impute income where it is missing for a particular family (item non-response). As discussed above, we believe the quality of the income data in BCS70 is not high for the childhood waves of the survey (although it is much improved for adulthood). We were able to generate predicted incomes from the Family Expenditure Survey (FES) and the New Earnings Survey (NES). The NES predictions of income change were not satisfactory, mainly because NES is limited to earnings of employees and has rather few potential predictors. We do not recommend the use of the NES (or Annual Survey of Hours and Earnings (ASHE)) for this kind of work.

We were more successful with the FES. We show that it is possible to explain over half the variation in parental income at a particular time point using predictors that are common to both the FES and BCS70. One way of thinking about this is to say that if we use the predicted income in place of the observed band, we are using a variable with a reliability of between 0.5 and 0.6 and this could be higher than the (unknown) reliability of the income bands when they are turned into continuous measures through the Singh-Maddala or other transformations. On the other hand, we found that our model parameters appeared to vary from year to year so that a prediction model is required for the year in question or, in a more sophisticated way, the parameters are themselves allowed to vary in a smooth manner with time by analysing all the annual FES surveys and its successor, the Family Resources Survey.

Although we think that using the FES as an imputation tool is promising, we recognise that there are drawbacks in the approach. The overlap between the predicted and observed bands was not high, although some of those discrepancies might be explained by biases in the BCS70 measures. And we were somewhat hamstrung by other kinds of missing data in BCS70 so that we in fact ended up with fewer numbers of imputed incomes than there were observed income bands in the analyses reported in Chapter 6.

7.2.2 Causal models

All our analyses have been based on longitudinal observational data and so we cannot rule out the biasing effects of self-selection into income change as an explanation for our results. We have used as many control variables as possible within a framework that excludes possible mediating variables such as changes in family structure as controls. We have, however, been restricted by the small number of measurement occasions available to use – mostly just two apart from some aspects of the NPD data. As Plewis *et al.*, (2001) and Plewis and Hawkes (2005) point out, we would have a much better chance of understanding income effects with more frequent measures of income and outcomes. One way of achieving this without extensive and expensive new studies – although the new UK Longitudinal Household Survey might help as will MCS when more waves become available – would be to link administrative data sources both with each other (NPD and Department for Work and Pensions (DWP) benefit records for example) and with other longitudinal studies. The analyses presented here do suffer from the problem that we know nothing about changes in the intervening years between measurements nor anything about the timing of any changes.

7.2.3 Outstanding issues

To the extent that there are income effects on child and adult outcomes – and this report does suggest that there might be some, albeit small – then an important next step is to establish the mediating processes behind the effects. Again, at the moment, we are somewhat restricted by the available data. In particular, we do not know enough about how changes (both increases and decreases) in income are managed by families, especially in relation to their children and possibly in association with changes in family structure. A greater emphasis on consumption, and on intra-household transfers, could be helpful.

We have not uncovered any moderating effects during the course of this research. In other words, income changes appear to apply in the same way regardless of the characteristics of the sample member. These moderating effects, or statistical interactions, tend, however, to be difficult to pick up when the main effects are small and so further investigation with a large sample could be warranted.

Finally, the combination of attrition from longitudinal studies, the tendency for income questions to remain unanswered by respondents, especially by the self-employed, and measurement error in some or all of the explanatory variables, all pose problems for statistical modelling. We have not been able to make any adjustments for non-response or measurement error in this report but they are important methodological issues that need to be considered as part of any continuing programme of research into income effects.

Appendix A
Chapter 3 tables

The descriptive statistics in Tables A.1 to A.3 are corrected for the survey design through the use of the 'svy' procedures in STATA. Thus, the means and percentages are weighted to take account of the disproportionate stratification and the standard errors take account of the clustering.

Table A.1 Descriptive statistics, MCS: cognitive tests, income and control variables

	Observed sample			Analysis sample		
Variable	Mean	Standard error	Sample size[1]	Mean	Standard error	Sample size[2]
Bracken (transformed)[3]	0.10	0.023	13,294	0.16	0.021	9,267
BAS (transformed)[3]	0.11	0.018	13,968	0.17	0.016	9,668
Test age[4]	0.10	0.013	14,432	0.077	0.014	9,267
Dev. Delay[5]	0.20	0.0061	18,552	0.16	0.0063	9,267
Log average family income (£K)	3.1	0.021	11,791	3.1	0.020	9,267
Log family income, wave 1 (£K)	2.9	0.020	16,941	3.0	0.021	9,267
Log family income, wave 2 (£K)	3.1	0.022	12,605	3.1	0.021	9,267
Income difference (£K)	2.8	0.18	11,791	3.1	0.20	9,267
Hearing problem	0.12	0.0051	15,643	0.12	0.0059	9,267
Owner occupier	0.64	0.0096	18,492	0.72	0.0095	9,267
Hospital admission	0.14	0.0038	18,530	0.14	0.0051	9,267
Accident	0.080	0.0027	18,530	0.084	0.0038	9,267
Mean number children	2.0	0.012	14,777	2.0	0.012	9,267
Birth weight (Kg.)	3.4	0.0057	18,484	3.4	0.0066	9,668

Notes:
1 Varies from the maximum (18,552) because of item and domain non-response.
2 Varies according to the model fitted.
3 The unweighted means = 0; the weighted means are higher because advantaged families are under-represented in the selected sample. The means for the analysis sample are a little higher than for the observed sample.
4 Measured as nine three month intervals.
5 Range: 0 to 6.

Table A.2 Descriptive statistics, MCS: educational qualifications

NVQ	% (Observed)	% (Analysis)
None	12	7.3
Overseas or other	2.4	1.4
Level 1	8.2	7.3
Level 2	30	30
Level 3	14	15
Level 4	30	35
Level 5	3.8	4.3
Sample size	18,499	9,267

Table A.3 Descriptive statistics, MCS: SDQ and Temperament

Variable	Mean	S.E	Minimum	Maximum	n
SDQ, wave two					
Total[1]	9.2	0.078	0	33	13,834
z score	-0.066	0.015	n.a.	n.a.	13,834
Temperament, wave one					
Mood	2.8	0.0078	0	4	17,911
Adaptability[2]	0.96	0.0078	0	4	17,662
Regularity	3.3	0.0089	0	4	17,837

Notes:
1 This is based on the four sub-scales as described in Section 3.1.2
2 This is scored in the opposite direction, i.e. lower scores are better.

Table A.4 Modelling difference in family income from MCS1 to MCS2: behaviour outcome

	SDQ	
Variable	Estimate	S.E.
Family income difference		
Linear	-0.00050	0.00068
Quadratic	-0.000044	0.000014
Developmental delay	0.084	0.026
Temperament		
Mood	-0.15	0.016
Adaptability	0.11	0.016
Regularity	-0.14	0.015
Birth weight	-0.044	0.018
Main respondent’s educational qualifications		
No qualifications	0.19	0.055
Overseas; other	-0.090	0.093
NVQ1 (reference)	0	n.a.
NVQ2	-0.13	0.044
NVQ3	-0.26	0.043
NVQ4	-0.43	0.043
NVQ5	-0.54	0.065
Tenure		
Rent (reference)	0	n.a.
Own	-0.32	0.025
Accidents		
No (reference)	0	n.a.
Yes	0.11	0.036
Number of children, MCS1	-0.053	0.011
Sample size	10,892	
R^2	0.14	

Table A.5 Modelling change in family benefit status from MCS1 to MCS2: behaviour score

	SDQ	
Variable	**Estimate**	**S.E.**
Change in benefit status		
No (MCS1); No (MCS2) (Ref.)	0	n.a.
Yes (MCS1); No (MCS2)	0.094	0.026
No (MCS1); Yes (MCS2)	0.12	0.035
Yes (MCS1); Yes (MCS2)	0.27	0.028
Developmental delay	0.094	0.023
Temperament		
Mood	-0.15	0.014
Adaptability	0.11	0.014
Regularity	-0.14	0.012
Birth weight	-0.043	0.016
Main respondent's educational qualifications		
No qualifications	-0.074	0.076
Overseas; other	0.13	0.048
NVQ1 (reference)	0	n.a.
NVQ2	-0.12	0.039
NVQ3	-0.22	0.038
NVQ4	-0.38	0.039
NVQ5	-0.49	0.058
Tenure		
Rent (reference)	0	n.a.
Own	-0.21	0.026
Accidents		
No (reference)	0	n.a.
Yes	0.10	0.033
Number of children, MCS1	-0.060	0.010
Sample size	13,268	
R^2	0.15	

Table A.6 Descriptive statistics, BCS70: Cognitive tests, behavioural outcomes and control variables, age five

	Observed sample			Analysis sample		
Variable	Mean	Standard error	Sample size[1]	Mean	Standard error	Sample size[2]
EPVT (transformed)[3]	-0.0017	0.010	10,032	0.076	0.015	4,357
Copying designs (transformed)[3]	-0.0055	0.0089	12,641	0.057	0.013	5,571
Emotional	0.37	0.0021	12,433	0.38	0.0032	5,493
Conduct	0.43	0.0022	12,526	0.40	0.0032	5,542
Attention	0.27	0.0018	12,468	0.27	0.0027	5,516
Sex (boys)	0.52	0.0022	12,742	0.52	0.0034	5,516
Birth weight (z-transformed)	0	0.0089	12,732	0.011	0.013	5,516

Notes:
1 Varies from the maximum because of item and domain non-response.
2 Varies according to the model fitted.
3 The means for the analysis sample are a little higher than for the observed sample.

Table A.7 Descriptive statistics, BCS70: Social class measures and parental employment change score

Variable	% (Observed)	% (Analysis)
Mother's father's social class		
1 (Professional)	2.5	2.8
2	16	16
3	8.4	8.2
4	46	47
5	19	18
6 (Unskilled)	8.3	8.0
Sample size	10,828	5,516
Father's father's social class		
1 (Professional)	2.5	2.7
2	17	17
3	7.8	8.4
4	47	47
5	18	18
6 (Unskilled)	8.1	7.8
Sample size	9,14	5,516
Change score: parental employment status		
<= -1	6.6	6.7
-0.5	4.3	4.5
0	51	51
0.5	30	31
>=1	7.7	7.8
Sample size	7,960	4,357

Table A.8 Descriptive statistics, BCS70(CC): cognitive tests, income and control variables (age three to five)

	Observed sample		
Variable	Mean	Standard error	Sample size
BAS (transformed)	0.026	0.033	887
Early number (transformed)	0.020	0.034	882
SDQ (Raw score)	3.8	0.052	863
Log average family income (£/week)	6.1	0.020	875
Log family income, age 30 (£/week)	5.9	0.028	883
Log family income, age 34 (£/week)	6.1	0.030	879
Log weekly pay, age 26 (£)	5.3	0.025	729
Owner occupier	0.86	0.012	880

Table A.9 Descriptive statistics, BCS70(CC): educational qualifications of cohort member

NVQ	% (Observed)
None	9.1
Level 1	9.7
Level 2	29
Level 3	19
Level 4	31
Level 5	2.1
Observed sample	886

Appendix B
Chapter 4 tables

Table B.1 Descriptive statistics, BCS70: Cognitive tests and behavioural outcomes, ages ten and 16

	Observed sample		
Variable	**Mean**	**Standard error**	**Sample size**
Maths, age ten (transformed)	0	0.010	9,641
Reading, age ten (transformed)	0	0.010	9,653
Emotional, age ten	0.20	0.0039	10,662
Conduct, age ten	0.20	0.0039	10,694
Attention, age ten	0.20	0.0039	10,716
Maths, age 16 (transformed)	0	0.017	3,414
Vocabulary, age 16 (transformed)	0	0.014	5,389
Spelling A, age 16 (transformed)	0	0.014	5,268
Spelling B, age 16 (transformed)	0	0.014	5,157
Emotional, age 16	0.27	0.0050	8,008
Conduct, age 16	0.36	0.0054	8,009
Attention, age 16	0.32	0.0052	8,082

Table B.2 Descriptive statistics, BCS70: Education measures and parental employment change score

Variable	% (Observed)
Mother's educational qualifications	
1 (Lowest)	56
2	33
3	3.6
4	7.6
Sample size	11,048
Father's educational qualifications	
1 (Lowest)	49
2	28
3	7.5
4	15
Sample size	10,409
Change score: parental employment status	
<=-1.5	0.69
-1	3.6
-0.5	7.5
0	48
0.5	30
1	8.1
1.5	2.1
2	0.31
Sample size	8,946

Table B.3 Descriptive statistics, BCS70 (CC): cognitive tests, income and control variables (age six to 16)

	Observed sample		
Variable	Mean	Standard error	Sample size
Word reading (transformed)	0.0076	0.026	1,488
Spelling (transformed)	0.049	0.026	1,488
Early number (transformed)	-0.0070	0.026	1,488
SDQ (raw score)	3.7	0.048	1,439
Log average family income (£/week)	6.0	0.014	1,471
Log family income, age 30 (£/week)	5.7	0.020	1,479
Log family income, age 34 (£/week)	6.0	0.014	1,471
Log weekly pay, age 26 (£/week)	5.0	0.029	903
Owner occupier	0.69	0.012	1,467

Table B.4 Descriptive statistics, BCS70 (CC): educational qualifications of cohort member

NVQ	% (Observed)
None	17
Level 1	12
Level 2	35
Level 3	18
Level 4	16
Level 5	0.67
Observed sample	1,488

Table B.5 Descriptive statistics: KS2 test scores

Test	Mean	SD	Range	Sample size
English	59	17	0-100	549,584
Maths	64	22	1-100	553,173
Science	57	13	2-80	560,120

Note:
These scores were transformed to z-scores in the statistical models reported in Tables 4.5.2 to 4.5.4.

Table B.6 Descriptive statistics: KS1 test scores

Score	Reading (%)	Writing (%)	Maths (%)
0	2.2	3.9	1.3
1	13	9.1	7.3
2	11	27	15
3	19	31	20
4	26	20	26
5	30	9.2	31
6	0.16	< 0.1%	< 0.1%
Sample size	547,554	547,308	547,164

Note:
The reading score was derived from the original reading and comprehension values, taking the higher of the two values.

Table B.7 Descriptive statistics: KS1 teacher assessments

Score	English (%)	Maths (%)	Science (%)
1	12	9.4	8.8
2	66	63	65
3	22	27	26
4	< 0.1%	< 0.1%	< 0.1%
Sample size	536,806	542,123	543,432

Appendix C
Chapter 5 tables

Table C.1 Descriptive statistics, BCS70: cognitive tests, income and control variables

	Observed sample			Analysis sample		
Variable	Mean	S.D.	Sample size	Mean	Standard error	Sample size
Maths test, age 16	0	1	3,414	0.01	0.97	1,316
Vocab. test, age 16	0	1	5,389	-0.014	0.98	2,219
Spell A test, age 16	0	1	5,268	0.016	0.93	2,183
Spell B test, age 16	0	1	5,157	0.024	0.91	2,143
Maths test, age ten	0	1	8,664	0.093	0.92	1,316
Read test, age ten	0	1	8,674	0.11	0.91	1,316
EPVT, age five	0	1	7,154	0.14	0.94	1,316
Copy designs, age five	0	1	9,145	0.074	0.97	1,316
Log weekly family income (£), age ten	4.8	0.49	9,398	4.8	0.47	1,556
Log weekly family income (£), age 16	5.2	0.64	6,805	5.3	0.60	1,556
Log weekly earnings (£), age 34	5.6	0.80	5,922	5.6	0.83	2,394
Owner occupier	0.64	0.48	10,125	0.70	0.46	1,316

Table C.2 Descriptive statistics, BCS70: behaviour measures

	Observed sample			Analysis sample		
Variable	Mean	S.D.	Sample size	Mean	Standard error	Sample size
Emotion, age 16[1]	0.27	0.44	8,008	0.27	0.44	5,897
Conduct, age 16[1]	0.36	0.48	8,009	0.35	0.48	5,939
Attention, age 16[1]	0.32	0.47	8,082	0.31	0.46	5,954
Emotion, age ten[2]	0	1	9,978	0.018	1.0	4,280
Conduct, age ten[2]	0	1	10,002	-0.042	0.94	4,298
Attention, age ten[2]	0	1	10,024	-0.028	0.97	4,310
Emotion, age five[3]	1.6	1.5	9,054	1.6	1.6	4,280
Conduct, age five[3]	1.4	1.2	9,073	1.4	1.2	4,298
Attention, age five[3]	0.94	1.1	9,033	0.91	1.0	4,310

Notes:
1 Binary scores.
2 z-transformed scores.
3 Raw scores.

Table C.3 Descriptive statistics, BCS70: mother's educational qualifications

NVQ	% (Observed)	% (Analysis)
Level 1	47	42
Level 2	38	43
Level 3	4.8	4.3
Level 4	10	10
Observed sample	9,574	1,316

Table C.4 Benefit status change, ages ten to 16 and behaviour: BCS70

	Emotion		Conduct		Attention	
Variable	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Benefit status change						
10:NB, 16:NB (reference)	0	n.a.	0	n.a.	0	n.a.
10:B, 16:NB	0.25	0.13	0.43	0.12	0.25	0.12
10:NB, 16:B	0.061	0.10	0.25	0.093	0.046	0.096
10:B, 16:B	0.27	0.13	0.52	0.12	0.20	0.12
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.51	0.063	0.071	0.061	-0.30	0.061
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	-0.064	0.074	-0.11	0.068	-0.094	0.070
Emotion, age ten	0.61	0.032	n.a.	n.a.	n.a.	n.a.
Emotion, age five	0.17	0.020	n.a.	n.a.	n.a.	n.a.
Conduct, age ten	n.a.	n.a.	0.61	0.037	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.34	0.028	n.a.	n.a.
Attention, age ten	n.a.	n.a.	n.a.	n.a.	0.61	0.033
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.29	0.030
Mother's education level						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	0.16	0.072	-0.19	0.067	-0.082	0.068
3	0.23	0.15	-0.28	0.15	-0.15	0.15
4	0.12	0.11	-0.25	0.11	-0.29	0.11
Sample size	5,897		5,939		5,954	

Table C.5 Parental employment status change, ages ten to 16 and educational progress: BCS70

	Maths 16		Vocab 16		Spell A 16		Spell B 16	
Variable	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**	**Estimate**	**S.E.**
Parental employment status change								
≤-1.5	0.087	0.25	-0.29	0.22	0.13	0.20	0.18	0.20
-1.0	-0.015	0.12	0.028	0.096	0.045	0.093	-0.048	0.091
-0.5	-0.012	0.097	-0.049	0.082	-0.0080	0.080	-0.0072	0.077
0 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
0.5	-0.051	0.070	-0.043	0.057	-0.062	0.055	-0.0091	0.054
1.0	-0.32	0.15	-0.21	0.13	0.098	0.13	0.031	0.12
≥ 1.5	0.30	0.44	0.21	0.29	0.20	0.28	0.045	0.27
Sex								
Boy (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Girl	0.058	0.060	0.091	0.049	0.23	0.047	0.23	0.047
Tenure								
Rent (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
Own	0.16	0.069	0.017	0.058	0.025	0.046	0.037	0.055
Maths, age ten	0.42	0.045	0.12	0.038	0.11	0.037	0.13	0.036
Reading, age ten	0.13	0.047	0.31	0.039	0.35	0.038	0.23	0.037
EPVT, age five	0.0074	0.034	0.096	0.028	0.0026	0.027	0.017	0.026
Copying designs, age five	0.084	0.033	0.034	0.028	0.019	0.027	0.024	0.026
Mother's education level								
1 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
2	-0.11	0.066	0.060	0.055	0.041	0.054	0.030	0.053
3	0.19	0.15	0.18	0.13	0.031	0.12	0.0021	0.12
4	-0.047	0.11	0.15	0.093	0.065	0.090	0.047	0.087
Father's education level								
1 (reference)	0	n.a.	0	n.a.	0	n.a.	0	n.a.
2	0.17	0.073	-0.0034	0.061	0.030	0.059	0.043	0.057
3	0.25	0.10	0.22	0.084	-0.10	0.083	-0.038	0.080
4	0.31	0.095	0.16	0.079	0.010	0.077	0.046	0.074
R^2	0.39		0.23		0.21		0.16	
Sample size	732		1,353		1,333		1,302	

Table C.6 Parental employment status change, ages 10 to 16 and behaviour: BCS70

	Emotion		Conduct		Attention	
Variable	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Parental employment status change						
≤ -1.5	0.45	0.31	0.38	0.29	0.18	0.30
-1	-0.12	0.20	0.13	0.17	0.085	0.18
-0.5	0.20	0.15	-0.17	0.15	-0.18	0.16
0 (reference)	0	n.a.	0	n.a.	0	n.a.
0.5	0.18	0.10	0.095	0.099	0.026	0.10
1	0.62	0.23	0.58	0.22	0.17	0.23
≥1.5	0.59	0.44	0.20	0.43	-0.54	0.51
Sex						
Boy (reference)	0	n.a.	0	n.a.	0	n.a.
Girl	0.62	0.088	0.12	0.084	-0.39	0.085
Tenure						
Rent (reference)	0	n.a.	0	n.a.	0	n.a.
Own	0.098	0.11	-0.27	0.098	-0.14	0.12
Emotion, age ten	0.49	0.047	n.a.	n.a.	n.a.	n.a.
Emotion, age five	0.30	0.044	n.a.	n.a.	n.a.	n.a.
Conduct, age ten	n.a.	n.a.	0.57	0.052	n.a.	n.a.
Conduct, age five	n.a.	n.a.	0.45	0.050	n.a.	n.a.
Attention, age ten	n.a.	n.a.	n.a.	n.a.	0.63	0.045
Attention, age five	n.a.	n.a.	n.a.	n.a.	0.33	0.047
Mother's education level						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	0.25	0.10	-0.19	0.099	0.022	0.10
3	0.46	0.20	-0.12	0.21	-0.18	0.22
4	0.0010	0.17	-0.20	0.16	-0.030	0.17
Father's education level						
1 (reference)	0	n.a.	0	n.a.	0	n.a.
2	-0.051	0.11	-0.22	0.11	-0.12	0.11
3	-0.16	0.15	-0.22	0.15	-0.23	0.16
4	-0.33	0.15	-0.42	0.14	-0.34	0.15
Sample size	2,928		3,230		3,240	

Table C.7 Parental employment status change, ages ten to 16 and log weekly earnings at age 34: BCS70

	Log earnings, 34	
Variable	Estimate	S.E.
Parental employment status change		
≤-1.5	0.13	0.16
-1	-0.061	0.091
-0.5	0.12	0.073
0 (reference)	0	n.a.
0.5	0.021	0.050
1	-0.033	0.12
≥1.5	0.055	0.26
Sex		
Boy (reference)	0	n.a.
Girl	-0.64	0.045
Tenure		
Rent (reference)	0	n.a.
Own	0.082	0.052
Maths, age ten	0.071	0.035
Reading, age ten	0.053	0.035
EPVT, age five	0.061	0.026
Copying designs, age five	0.018	0.025
Emotion, age ten	-0.021	0.024
Emotion, age five	0.027	0.014
Conduct, age ten	0.0014	0.031
Conduct, age five	-0.0065	0.022
Attention, age ten	-0.012	0.027
Attention, age five	0.015	0.025
Mother's education level		
1 (reference)	0	n.a.
2	-0.049	0.049
3	0.060	0.11
4	0.0044	0.085
Father's education level		
1 (reference)	0	n.a.
2	0.028	0.055
3	0.088	0.077
4	0.28	0.072
R^2	0.21	
Sample size	1,343	

Appendix D
Chapter 6 Tables

Table D.1 Gross household income (£ per week) (FES 1980 and 1986)

Quartile (%)	Value (1980)	Value (1986)
25	116.35	167.52
50	162.40	261.66
75	219.06	366.16

Table D.2 Number of children (FES 1980 and 1986)

Number of Children	1980		1986	
	Number	%	Number	%
0	2,036	48	2,100	53
1	813	19	742	18
2	951	22	775	19
3	319	7	267	6
4	89	2	59	2
5	24	1	13	1
6	3	1	6	1
7	3	1	-	-
Total	**4,238**	**100**	**3,962**	**100**

Table D.3 Household size (FES 1980 and 1986)

	1980		1986	
Household size	**Number**	**%**	**Number**	**%**
1	474	11	582	14
2	1,300	31	1,211	30
3	875	20	809	20
4	1,021	24	914	23
5	374	8	333	8
6	127	3	82	2
7	50	2	20	1
8	11	1	7	1
9	6	1	2	1
10	-	-	1	1
11	-	-	1	1
Total	4,238	100	3,962	100

Table D.4 Home ownership (FES 1980 and 1986)

	1980		1986	
Home ownership	**Number**	**%**	**Number**	**%**
No	1,675	40	1,151	29
Yes	2,563	60	2,811	71
Total	4,328	100	3,962	100

Table D.5 Age of the household head (FES 1980 and 1986)

Quartile (%)	Value (1980)	Value (1986)
25	32	32
50	42	40
75	54	51

Table D.6 Gender of the household head (FES 1980 and 1986)

	1980		1986	
Gender	**Number**	**%**	**Number**	**%**
Male	3,758	89	3,442	87
Female	480	11	520	13
Total	4,238	100	3,962	100

Table D.7 Marital status of the household head (FES 1980 and 1986)

	1980		1986	
Marital status	**Number**	**%**	**Number**	**%**
Not married	821	19	1,033	26
Married	3,417	81	2,929	74
Total	4,238	100	3,962	100

Table D.8 Years of post-compulsory education of the household head (FES 1980 and 1986)

	1980		1986	
Years of post-compulsory education	**Number**	**%**	**Number**	**%**
0	2,460	58	2,080	52
1	736	17	777	19
2	393	9	316	8
3-5	330	8	380	9
6+	319	8	409	10
Total	4,238	100	3,962	100

Table D.9 Occupational social class of the household head (FES 1980 and 1986)

	1980		1986	
Occupational social class	**Number**	**%**	**Number**	**%**
I	515	12	571	14
II	772	18	749	19
III non-manual	455	11	445	11
III manual	1,398	33	1,228	30
IV	749	18	575	14
V	220	5	186	5
Unoccupied	129	3	208	5
Total	4,238	100	3,962	100

Table D.10 Log gross weekly earnings (NES 1980 and 1986)

Quartile (%)	Value (1980)	Value (1986)
25	4.16	4.74
50	4.54	5.10
75	4.84	5.41

Table D.11 Difference between log earnings (NES 1980 and 1986)

Quartile (%)	Value
25	-0.003
50	0.17
75	0.38

Table D.12 Employment status change (NES 1980 and 1986)

Status change	Number	%
No change	71,094	93
Full-time to part-time	3,071	4
Part-time to full-time	2,560	3
Total	76,725	100

Table D.13 Occupational social class (NES 1980 and 1986)

	1980		1986	
Occupational social class	**Number**	**%**	**Number**	**%**
I	7,321	10	8,435	11
II	16,930	22	18,363	24
III non-manual	20,993	27	19,344	25
III manual	13,989	18	12,445	16
IV	10,161	13	10,456	14
V	7,331	10	7,682	10
Total	76,725	100	76,725	100

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