
REPORT

DOES MONEY AFFECT CHILDREN'S OUTCOMES? A SYSTEMATIC REVIEW

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This report examines whether money has a causal impact on children's outcomes. There is abundant evidence that children growing up in lower income households do less well than their peers on a range of wider outcomes, including measures of health and education. But is money important in itself, or do these associations simply reflect other differences between richer and poorer households, such as levels of parental education or attitudes towards parenting?

This report:

- reviews the evidence, focusing on research that investigates whether the relationship between money and children's wider outcomes is causal;
- uses systematic review techniques to reduce bias and maximise the number of relevant studies identified;
- considers intermediate outcomes such as parenting and maternal depression, as well as children's health, cognitive, social and behavioural outcomes; and
- given the current tight fiscal climate, provides important insight into the role government transfers to households with children can play in promoting children's life chances, and how these might compare to investments in public services such as education.

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EXECUTIVE SUMMARY

There is abundant evidence of a strong association between household financial resources and a range of wider outcomes for children, but the extent to which these associations are causal is not well understood. A number of confounding factors may explain the link, such as genetic endowment, levels of parental education and approaches to parenting. This uncertainty leaves room for considerable difference of opinion among policymakers about policy solutions. Will raising household income in itself make a difference to children's outcomes, or would it be better to focus on investing in schools or improving parenting skills?

This review examines the evidence on the causal impact of household financial resources on children's wider outcomes. Causation is difficult to establish in social science, but certain techniques allow us to be more confident that what we are observing is indeed the effect of money itself, not simply a reflection of other differences between richer and poorer households. We used a systematic review approach to try to identify all the studies that use randomised controlled trials, natural experiments, and sophisticated econometric techniques on longitudinal data to investigate the causal effect of money. We focused on children's health, social, behavioural and cognitive outcomes, and on intermediate outcomes such as expenditure on children's goods, maternal mental health, parenting and the home environment.

Our search strategy initially identified 46,668 studies. Most turned out not to be relevant and many others, while on the right topic, did not use methods which allowed conclusions to be reached regarding causation. Ultimately just 34 studies were judged to meet our full inclusion criteria. The majority of these studies are from the US, with some evidence from the UK, Canada, Norway and Mexico.

Our review indicates clearly that **money makes a difference to children's outcomes**. Poorer children have worse cognitive, social-behavioural and health outcomes in part *because they are poorer*, and not just because poverty is correlated with other household and parental characteristics. The evidence relating to cognitive development and school achievement is the clearest and there is the most of it, followed by that on social and behavioural development. Evidence about the impact of income on children's physical health is more mixed, and there were no studies that looked at children's subjective well-being or social inclusion.

Of the 34 studies, only five found no evidence of a money effect on any of the outcomes examined, and there appear to be methodological reasons for this in at least four cases. For example, two studies control for factors that other evidence suggests are likely to be mediators, such as parenting practices. If increased income leads to better parenting, and this in turn improves child outcomes, a model that controls for parenting will fail to pick up the true effect of income.

We went on to examine the size of estimated income effects, asking not just whether money matters but **how much** difference it makes. A key finding here is that the size of identified effects is highly sensitive to the methods used. Because of the paucity of experimental studies, many of the studies (14 of the 34) use fixed effect techniques on longitudinal data. Researchers focus on within-household changes in income and outcomes over time, to control for the possibility that other factors (e.g. raw intelligence or parental ambition) are responsible for better outcomes in richer households. However, income is always measured with error in household surveys, and accurately assessing income change over time using these surveys is a considerable problem. Perhaps as a result, these studies calculate much smaller effect sizes than experimental studies, which are not subject to measurement problems to the same degree. Indeed, the extent of downward bias in the fixed effect studies is so large that it raises questions about how useful these methods are to establish either the existence or the size of income effects; there is a danger that their results simply mislead.

Setting fixed effect studies aside and concentrating only on experimental studies or other studies that are able to exploit income changes beyond household control (e.g. changes to the benefit system that affect some groups more than others), effect sizes associated with a US\$1,000 increase in income (around £900 in 2013 prices) ranged from 5 per cent to 27 per cent of a standard deviation for cognitive outcomes, and from 9 per cent to 24 per cent for social and behavioural outcomes, with estimates of 14–15 per cent for maternal depression. Effect sizes for cognitive and schooling outcomes appear roughly equal in size to the estimated effects of spending similar amounts on school or early education interventions. They suggest that increases in household income would not eliminate differences in outcomes between low-income children and others but could be expected to contribute to substantial reductions in these differences. For example, we calculate that increasing household income for children in receipt of free school meals (FSM) by £7,000, which would bring them up to the average income for the rest of the population, might be expected to improve Key Stage 2 scores for FSM children by more than 1.5 points, eradicating half the gap in outcomes at Key Stage 2 between FSM and non-FSM children.

Looking to explain **why income matters**, we found evidence in support of two central theories, one relating to the stress and anxiety caused by low income (the Family Stress Model), and the other relating to parents' ability to invest in goods and services that further child development (the Investment Model). Several of our studies identified a causal impact of income on

Poorer children have worse cognitive, social-behavioural and health outcomes in part *because they are poorer*.

mediators linked to the Family Stress Model, including maternal mental health and parenting behaviour; and to a lesser extent on mediators linked to the Investment Model, such as the physical home environment. The findings in relation to mechanisms are important because they provide an indication that our evidence – which comes largely from the US – is applicable in the UK context. US public services – in particular health services – are very different from those in the UK, and this could result in a different relationship between income and some outcomes in the two countries. In fact, the mechanisms identified are likely to be equally relevant in the UK. However, it should be noted that the report focuses on quantitative evidence, which is not always good at answering ‘why?’ questions. Qualitative research would offer further insight.

In terms of differential effects of income, we found strong evidence that income effects are **non-linear**. Thirteen of the 34 studies asked whether income effects were larger at some points in the distribution than others, and all 13 pointed to significantly larger effects in lower-income households for at least some outcomes. Does this mean that beyond a certain point additional income ceases to have any effect at all? The evidence here is mixed. Some of the studies found no impact of additional income on families that are above the official US poverty line, but others found that income continues to affect some health and schooling outcomes much higher up the distribution. It should also be remembered that our studies focus on relatively small changes in income for the same households over time. They deliberately net out longer-term income differences between households because of the difficulty of untangling the effects of these differences from those of other hidden household characteristics. Thus income differences on a scale that would enable a family to afford, for example, to select their housing on the basis of school catchment area are unlikely to have been picked up in any of our studies. Nevertheless, it is very clear that the marginal pound or dollar has a bigger impact in lower-income than higher-income households.

Evidence on **whether money matters more at some stages of childhood than others** was mixed and to some extent seems to depend on the type of outcome. Just five of our included studies looked at this issue, mostly focusing on cognitive outcomes: the majority indicate that early childhood matters most. For behavioural outcomes, in contrast, the one relevant study found income in later childhood is more important. Because evidence in this area was so limited we also looked at a broader set of studies that use longitudinal data but not methods that qualify for inclusion in our main evidence base. These were more mixed in relation to the hypothesis that income in early childhood is most important for cognitive outcomes, but provided support for the idea that income in later childhood and adolescence matters more for behavioural outcomes.

The **duration of low income** also appears to be important: longer-term poverty seems to have a more severe effect on children’s outcomes than short-term experiences of poverty. This is a common finding in observational studies, but is difficult to interpret because families in longer-term poverty are likely to differ from those who experience poverty for shorter periods, and because income is likely to be more accurately measured over a longer time period. Nonetheless, some evidence from our experimental studies suggests that at least part of this association is causal.

There was least evidence on the final two questions we asked: Does the source of money matter? And does it matter who receives the additional money? None of our studies directly tested whether the **source of income** matters for children’s outcomes but a large group of studies examined

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increases in income as a result of benefit changes and found positive effects on a range of children's outcomes, similar to (the smaller number of) studies examining other exogenous sources of income change.

Just one of our studies examined the importance of **who within the household receives additional income**: in their analysis of the US casino experiment, Akee *et al.* (2010) found children's educational outcomes improved if mothers received the additional money but not if fathers did. This finding is consistent with other research from South Africa, Mexico and the UK, and with the 'purse versus wallet' theory that predicts that mothers are more likely to spend income on children than fathers are.

Summary

In conclusion, there is strong evidence that household financial resources are important for children's outcomes and that this relationship is causal. Protecting households from income poverty may not provide a complete solution to poorer children's worse outcomes, but it should be a central part of government efforts to promote children's opportunities and life chances. Our calculations have suggested that income increases have effect sizes comparable to those identified for spending on early childhood programmes or education, but income influences many different outcomes at the same time. Studies included in this review find income effects on intermediate outcomes including parenting and the physical home environment, maternal depression, and smoking during pregnancy; and on direct measures of children's well-being and development, including their cognitive ability, achievement and engagement in school, anxiety levels and behaviour. Even small income effects operating across this range of domains are likely to add up to a larger cumulative impact. Mayer (1997) refers to income support policies as the 'ultimate "multipurpose" policy instrument' (p. 145): few other policies are likely to affect so many outcomes at the same time.

The downside of this picture, particularly in the current economic climate, is that *reductions* in household income are likely to have wide-ranging negative effects. Part of the Coalition Government's deficit reduction strategy is to reduce welfare budgets in order to limit spending cuts to essential public services including education, with a view to protecting children's life chances (e.g. HM Treasury, 2010). However well-intentioned, the evidence in this review suggests that this strategy is likely to be self-defeating, especially in a context of high unemployment: reductions in household financial resources will damage the broader home environment in ways that will make it harder for public services to deliver for children.

More research would help to develop this evidence base. Our review has identified a number of gaps in the literature: we found no evidence using causal methods to look at the impact of income on children's subjective well-being and social inclusion, and less evidence on mediating mechanisms, including maternal mental health and parenting practice, than on cognitive development. It is also striking how much of the evidence is from the US, with only four studies for the UK included. At the same time, however, we encourage researchers to consider our findings regarding the lower significance levels and smaller effect sizes emerging from the longitudinal fixed effects models. This approach is tempting in the absence of experimental data or valid instruments, but both researchers and policymakers should be aware that the results of these studies are likely to seriously underestimate the size of true income effects.

1 INTRODUCTION

There is abundant evidence of a strong association between household financial resources and a range of wider outcomes for children, but the extent to which these associations reflect causal relationships is not well understood.

On average, children growing up in low-income households have poorer health than children from richer backgrounds and score worse on tests of cognitive, social and behavioural development (e.g. Duncan and Brooks-Gunn, 1997; Mayer, 1997; Bradshaw, 2001; Ermisch *et al.*, 2001). They go on to do less well in education, have lower self-esteem as adolescents, and are more likely to become involved in crime or delinquent behaviour (e.g. Haveman *et al.*, 1997; Hobcraft, 1998; Ermisch *et al.*, 2001).

However, the extent to which these associations reflect causal relationships is not well understood. A number of confounding factors may explain the apparent link between financial resources and outcomes. Genetic endowment including health and cognitive ability may plausibly be one part of the explanation. Beyond that, parents in low-income households tend to have lower levels of education and parental education itself is likely to influence child outcomes through a variety of pathways. Higher-educated parents will be better placed to help with school work, they may give more priority to educational achievement, and they are likely to be better able to negotiate public services.

This uncertainty leaves room for considerable difference of opinion among policy-makers about the drivers of poor outcomes for children, and hence about policy solutions. Will raising income in itself make a difference, or would it be better to focus policy on improving educational attainment for low income children by investing in schools, or on improving parenting skills?

This is a vital question for policymakers at any time, and is highly topical in the UK at present. The Coalition Government in power since May 2010 has raised explicit doubts about the previous Labour administration's focus on income poverty among children, and has consulted on redefining child poverty so as to give less weight to measures of income and more to worklessness, educational failure and family breakdown (DWP, 2012). An Independent Review on Poverty and Life Chances in 2010 called for

resources to be shifted away from the tax credit system towards investment in parenting programmes and services for young children (Field, 2010).

Reviewing the research evidence on how far money does make a difference to children's outcomes therefore seems a timely exercise. In order to reach a robust and unbiased answer to the question, we took two decisions in conducting this review. The first was to focus on studies that use reliable causal methods to examine the relationship between money and other outcomes. Causation is notoriously difficult to establish in social science, but a growing body of evidence makes use of longitudinal data, sophisticated econometric techniques and/or experiments to try to identify the extent to which money is itself a factor driving other outcomes. We excluded from our main findings the many studies that explore the cross-sectional association between financial resources and children's health or development, even where they control for other observed variables, because of the possibility that unobserved differences between households are driving part of the apparent link.

The second decision was to conduct the study using the principles guiding systematic reviews (see Oakley *et al.*, 2005; Gough and Elbourne, 2002; Wallace *et al.*, 2004). This meant setting out research questions at the outset; establishing clear criteria for inclusion and exclusion of studies; and specifying and publishing search terms. The approach is designed to reduce bias; given the political sensitivity of the question this seemed particularly important in this case. We wanted to be sure that we did not rely too heavily on studies already known to us and to colleagues, in case these pointed more in one direction than another. Of course, sources of bias remain, the main one being publication bias: in clinical literature in particular, studies that identify significant results have been found more likely to be published than those that do not (see, for example, Dubben and Beck-Bornholdt, 2005). It is plausible that the risk of publication bias in this review is fairly low because the finding that household income has no effect on wider outcomes for household members might be considered an interesting result in itself, but we do not know this to be the case. Publication bias can be reduced by including unpublished material, but this carries costs in terms of time investment that were too large for this study. Even focusing on published studies, a net large enough to bring in all useful studies, also brings in a lot of less relevant material, as will be clear from the high numbers of our search results.

In the next chapter we set out our research questions in greater detail. Chapter 3 explains our methodology including our search strategy and inclusion criteria. We then discuss our findings. We begin by asking what our studies say about *whether* money matters for different outcomes, before going on to ask *how much* it matters: how large an impact might a given change in resources be expected to have? We then examine what light our evidence base sheds on the potential mechanisms through which money affects child outcomes. We go on to explore a series of secondary or sub-questions: we ask whether a given change in resources makes more difference to children in more disadvantaged households; whether money matters more at particular stages of childhood; and what we can say about the relative importance of short-term fluctuations in financial resources in comparison to a household's long-term financial position. We also discuss whether the source of additional resources is relevant and whether it matters who within a household receives them, although little evidence was found on these last two questions.

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2 RESEARCH QUESTIONS

We set out to examine what existing research tells us about whether and how far money matters to children's outcomes. We looked for evidence on a wide range of different outcomes for children, under five broad headings: cognitive development and school achievement; social and behavioural development; physical health; mental health and subjective well-being; and social inclusion.

We also looked for evidence on how money affects a series of intermediate outcomes related to parenting and the home environment. These can be thought of as mediating mechanisms but can equally be seen as outcomes in their own right: family expenditure on children's items; financial stress and material hardship; the home learning environment; maternal physical and mental health; and parenting, both positive and negative, including child abuse and neglect.

By money, we understood financial resources at household level. Within this broad definition we included studies if they looked at total household income; specific components of income such as benefits or wages; household expenditure; household wealth and assets; or subjective perceptions of the household's financial situation. Resources could be measured using a continuous indicator (such as total household income) or a categorical variable (e.g. falling above or below a given poverty threshold).

We excluded studies that focused on resources at neighbourhood level or beyond, such as studies that looked at the impact of average income in the neighbourhood on children's school attainment, or the difference in infant mortality between US states with higher and lower levels of income inequality. These were considered beyond the remit of our review.

In addition to our central question – How much does money matter? – we hoped to use the evidence to answer a series of secondary or sub-questions. First, although this is a study of quantitative research findings, and not all quantitative studies are good at explaining *why* one variable affects another, we hoped to pull out evidence where possible on the mechanisms

through which money appears to operate. Second, we wanted to know whether, as common sense would suggest, a change in resources has more impact on children living in low-income than higher-income households, and third, whether the impact is greater at some stages of childhood than others. A fourth question was whether short-term fluctuations in resources have an effect on outcomes or whether only longer-term, permanent resources are relevant. Fifth, we were interested in whether the source of income is relevant (wages versus benefit income, for example), and sixth in whether it matters who within a household receives any additional resources.

Each of these questions has its own chapter, although in practice there was much more evidence on some questions than on others.

One question that our study does not explore in depth is the issue of potential time lags between changes in financial resources and changes in outcomes. For the most part, the studies examined here look at whether changes in resources over a given time period lead to changes in outcomes (such as measures of cognitive ability or behavioural problems) over that same period. A handful ask whether changes in income during a particular stage of childhood show up in differential outcomes later on, such as rates of high school graduation or delinquent behaviour in late adolescence. In practice, we simply report on the effects identified by the studies using whatever timeframe they have chosen. It may be, however, that a change in resources has an instant effect on some outcomes while for others the impact takes years to manifest itself. The question of whether money operates with a time lag for some but not other outcomes is an interesting and important one but we felt unable to address it adequately in this study.

3 METHODOLOGY

Our aim was to identify all the relevant papers that use reliable causal methods to investigate the impact of household financial resources on children's outcomes.

At the outset, we agreed on the following criteria for including studies.

- 1 One of the aims of the study, as stated in the study's abstract, must be to test the effect of household financial resources on one (or more) of our outcomes of interest. This restriction was intended to keep the search strategy manageable while also reducing bias: including studies that happened to identify an income effect while investigating a different relationship could bias results towards the positive.
- 2 The financial resources variable must be measured at the individual or household level: studies focusing on neighbourhood resources or national/state/regional poverty rates were excluded.
- 3 Financial resources must be measured during childhood, but outcomes could be measured with a lag – for example, the effect of childhood income on high school graduation or health behaviour in young adulthood.¹
- 4 Studies were included if they used one of the following methods: natural experiments, Randomised Controlled Trials, instrumental variables, fixed effects, or other techniques on longitudinal data which measure *within-household* changes in resources and outcomes. Relevant studies that used longitudinal data in other ways, or used standard regression methods on cross-sectional data, were not included in the main results.
- 5 However, *if* cross-sectional and excluded longitudinal studies focused on one of our secondary questions, *and* if they included controls for major confounders (in particular parental education), they were kept in a separate database for the analysis of our secondary questions, as discussed in more detail below. Studies that used Structural Equation Modelling were also kept in this database of studies for analysis of secondary questions.
- 6 Studies from countries not in the EU or the OECD were excluded. This was to keep studies focused on contexts relevant to the UK (although inevitably contexts differ in important ways across countries that were included, and we bear this in mind in discussion).²

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- 7 Studies without abstracts or without English-language abstracts were automatically excluded. Studies in a foreign language but with an English abstract were translated if they appeared to meet our other criteria. Five such studies (three in German and two in French) made it to the translation stage but none to the final main evidence base.

Having agreed on the inclusion criteria, we conducted the search strategy in three stages: developing search terms, conducting systematic searches and a round of initial exclusions based on article title and abstract; a second stage of exclusions and sorting after accessing the full papers; and a third stage in which we coded the final papers. We describe each of these stages in turn.

Stage 1: Developing search terms, systematic searches and initial exclusions

Developing the search terms and overall search template was an iterative process, based on trialling different search terms and combinations in databases, discussing probable terms in meetings and looking at other similar systematic reviews. As can be seen in the example in Box 1, there were four sections to each search template: a set of terms for financial resources; a set of terms for method and causal relationship; a set of terms for age, to limit the search to children's outcomes; and a set for the particular outcome. The first three sections were common to all searches, while the outcome terms differed (the search terms for each outcome are included in Appendix 1, see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp). A draft of each template was tested against a selection of relevant studies already known to the research team. Additional terms were then added to ensure the search was as inclusive as possible while still specific to the established criteria. Training on conducting systematic searches was completed at the LSE Library to ensure that different word endings and alternative spellings of search terms were included in the search template.

The databases to be used for the searches were selected with the aim of including literature from a variety of disciplines, such as economics, sociology, psychology, demography and medicine. The final databases selected were based on those already known to be relevant, advice from colleagues who had completed systematic reviews, and consultation with LSE Library. After testing the search templates in all databases, a number were excluded from our choice if they were not practical for systematic searches, for example if the database did not allow exporting of search results. Databases were also excluded if they overlapped with others that included multiple databases. The final databases included were: EconLit, SocIndex, IBSS (International Bibliography of the Social Sciences), British Education Index, PsychInfo and Medline.

Systematic searches were then conducted, using the same overall search template, in each database. Searching took place between July and October 2012. In order to keep the searches manageable but inclusive, the research team decided to include only studies published in or after 1988 (this was deemed to cover most major research in the field, and indeed preliminary search results showed the majority of relevant studies retrieved were published after 1990). Because of the very high numbers of results returned we also took the decision to exclude working papers and other unpublished literature dated before 2009, using a filter on the databases where possible, and similarly to filter out dissertations and PhD theses. Working papers dated 2009 onwards were included as they might not yet have had time

Box 1: Example search template

Financial resources	AB(wealth* OR assets OR salary OR salaries OR earning* OR wage* OR pension* OR income* OR "socio-economic status" OR "socio-economic status" OR SES OR poverty OR poor OR depriv* OR disadvantag* OR hardship OR money OR cash* OR expenditure OR spending OR "standard* of living" OR "living standard*" OR "cost of living")
Method / causal relationship	AND AB(caus* OR effect* OR determin* OR impact* OR influenc* OR associat* OR correlat*)
Age group	AND AB(child* OR teenage* OR adolescen* OR infan*)
Outcome terms, e.g. Cognitive	AB(Cognitive OR Development* OR "school readiness" OR Reading OR Math* OR Writing OR vocabulary OR Test score* OR IQ OR Attainment OR Performance OR "School outcome" OR Qualification* OR "Exam* result*" OR "Exam* score*" OR Proficiency OR Achiev* OR Abilit* OR "Key stage" OR college OR "sixth form" OR NEET OR post-compulsory OR postcompulsory OR post-16)

to be published in journals, but studies that came out in working paper form before that time are not included in our review if they were not subsequently published.

The decision to exclude unpublished literature is an important one. In general, systematic reviews emphasise the importance of including unpublished studies because of the dangers of publication bias: in the clinical literature in particular, studies that identify significant results have been found to have a higher likelihood of publication than those that do not (e.g. Dubben and Beck-Bornholdt, 2005). However, we were simply dealing with too many search returns for the study to be manageable without taking this step.

A search log (available on request) was kept, recording the details of each search, including any filters used and the number of search results retrieved for each search in each database. All search results were then imported into EndNote where duplicates were automatically removed.

Studies were then manually excluded based on title and abstract if they did not fulfil the inclusion criteria set out above.

Following Greenhalgh and Peacock (2005) we also included studies referred to us by colleagues or cited in identified studies. Because of time constraints, the latter 'snowballing' method was only used to a limited extent, where it was clear that cited studies used credible causal methods.

Stage 2: Further exclusions and sorting

Once all search results had been screened based on the abstracts, the remaining studies that either met the first set of criteria or needed further investigation were imported into a spread sheet. The full papers were accessed and the methods section consulted for a second stage of screening. At this stage we were largely checking for two things. First, we needed to make sure that studies included a measure of income at the individual or household level, and that income was measured separately from other factors. For example, many studies referred to ‘socio-economic status’ in the abstract and turned out to use only index scores combining measures of income, education and occupational status.

Second, we needed to check that studies used methods that could reasonably be said to identify a *causal* effect of income. Up to a point this was straightforward. Studies that used randomised controlled trials or natural experiments to identify an income effect were included: in these studies income can be treated as an exogenous ‘shock’, affecting households independently of other hidden characteristics, and this allows us to attribute observed changes in outcomes to the impact of the income change itself. A good example is the opening of a casino in the US that distributed profits to local families if they included adults of Native American background (Costello *et al.*, 2003; Akee *et al.*, 2010).

Studies that used ‘instrumental variable’ approaches were also included. Rather than looking directly at income in individual households, these studies find a variable (the ‘instrument’) that is correlated with household income but not with any hidden household characteristics which would themselves drive child outcomes. For example, Milligan and Stabile (2011) make use of changes in child benefit payments across regions and over time in Canada. Not all instrumental variable studies were clear-cut cases, however. A study on France by Maurin (2002) uses grandfathers’ occupational status as an instrument for long-term income in the grandchild’s household, and identifies large positive effects of income on the probability of being held back at school. Maurin argues that a grandfather’s occupation is a good instrument because it predicts long-term income in the grandchild’s household (because the children of professional parents are more likely to have professional jobs themselves) but is not likely to have much correlation with the grandchild’s natural ability. Even accepting the assumption that grandfather’s occupation and grandchildren’s ability is uncorrelated, we were concerned that a grandparent’s occupation could also correlate with other drivers of schooling outcomes (such as parental interest in education), and the study was excluded.

Longitudinal studies that make use of fixed effects methods or other similar econometric techniques to focus on *within-household* change in income and outcomes were included. Studies that used longitudinal data as repeated cross-sections were excluded from the main findings (e.g. Lefebvre, 2006); as were studies that used longitudinal data to construct income trajectories that were then linked to outcomes (e.g. Kozyrskyj *et al.*, 2010); and studies that used a measure of income averaged over several waves (usually to reduce measurement error) alongside a single outcome measure (e.g. Aughinbaugh and Gittleman, 2003). Our concern in each of these cases was that other hidden characteristics could explain part of any association.

However, we decided to keep these longitudinal studies, and also relevant cross-sectional regression studies with rich control variables picked up by our searches, in a separate database to draw on in the part of the review that addresses secondary questions, largely because we felt that many of

We needed to check that studies used methods that could reasonably be said to identify a *causal* effect of income.

these studies contained valuable information and insight that we did not want to throw out. It remains true that they are limited in what they can tell us about causation, but whether the *association* between income and other outcomes is stronger for children at different parts of the distribution, for example, remains of interest. Furthermore, there is scant evidence in our causal studies about some of our secondary questions, so the additional information from these extra studies was especially important: for example, just five of our main studies address the question of whether income makes more difference to children at younger or older ages. When we discuss the secondary questions we take care to distinguish between studies included in our main results and other studies.

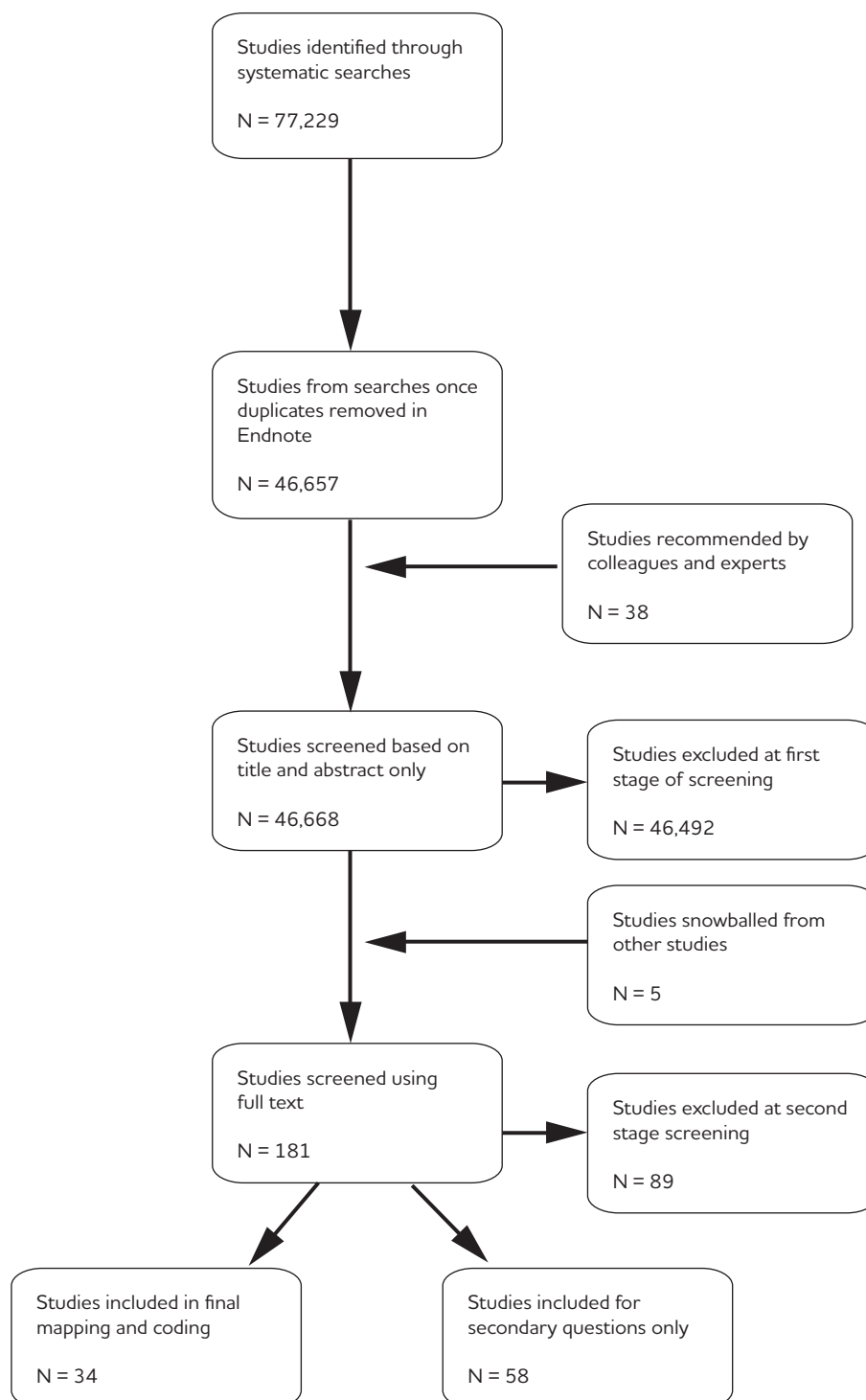
One final type of research was kept in the secondary questions database: studies using structural equation modelling to look at the relationship between income and outcomes. Structural equation modelling (SEM) studies make use of cross-sectional or cohort data in a way that does not satisfy our main criteria for identifying causal links, but the approach enables researchers to identify possible pathways between variables. We examine the SEM studies only when considering the mechanisms through which money may affect child outcomes.

Figure 1 tracks the number of references from the initial search results (77,229 in total, or 46,668 once duplicates were removed) to the final 34 studies included for our main findings. Of these 34, 18 came only from the searches, ten were included in the search results and had also been recommended by colleagues, four came from snowballing and two from colleagues only. We looked into why these last six studies had not been picked up by our searches and found there were two separate reasons. Two of the snowballed studies had in fact been returned in searches but subsequently excluded on the basis of the abstract: we had interpreted both studies as examining the impact of welfare-to-work programmes overall without adequately separating income effects from employment effects (Gennetian and Miller, 2002; Clark-Kauffman *et al.*, 2003). The other four studies were not among the 46,668 returned by the searches because the abstracts did not include one from each of the groups of search terms illustrated in Box 1. Two of these studies did not include any of the terms we had in the template to capture method/causal relationship (Gregg *et al.*, 2006; Riccio *et al.*, 2010), while two did not include any of our outcome terms: Løken *et al.* (2012) refer to ‘children’s outcomes’ in general, while Kaushal *et al.* (2007) refer to ‘learning’ and ‘enrichment’, neither of which are included. With the benefit of hindsight, of course, we would have included both of these terms as well as a general term for ‘children’s outcomes’, and their omission suggests that there are likely to be a number of other specific terms that we failed to include in the templates. A general methodological conclusion is that it is very difficult to make a search process entirely comprehensive, even if it is systematic. This underlines the importance of supplementing systematic search approaches with snowballing techniques and referrals from colleagues. On the other hand, because of the use of these additional strategies, and because most of the studies they threw up were also included in the search returns, we think it is unlikely that a large number of relevant studies have been missed.

Stage 3: Coding, mapping and effect sizes

Details for each study included after this second screening were entered into a spreadsheet. We included descriptive details of the study, such as dataset

Figure 1: Flow diagram of studies included/excluded by stage



Template adapted from PRISMA 2009 Flow Diagram from <http://www.prisma-statement.org/statement.htm>, accessed 20 September 2013.

used, sample size and method; the measure of financial resources; which child outcomes were included, how they were measured and what the results were for each outcome; a summary of overall findings; and any additional notes or concerns about the study's quality. This information enabled us to map the literature, summarising the evidence available about each childhood outcome. Similar spreadsheets were created for each of the secondary questions. For those studies where it was possible, we added the information necessary to calculate effect sizes in a consistent way across studies.

4 DOES MONEY MATTER?

What do our final 34 studies tell us about whether money matters to children's outcomes? The evidence points to a causal impact of money on a range of outcomes, including cognitive development, educational attainment, and social, behavioural and emotional development.

Table 1 summarises the overall story, with studies grouped by country and by our four main types of evidence – randomised controlled trials (RCTs), natural experiments, other exogenous income changes (instrumental variable approaches), and longitudinal (fixed effect style) studies. More detailed information on the studies can be found in Appendices 2–4 (see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp).

Overall, out of the 34 studies included, 23 show a significant and positive relationship between the measure of financial resources and each of the child outcomes they looked at (although not necessarily all measures of each outcome).³ Five studies find no significant causal relationships, while in a further six an effect is found for some but not all of the outcomes; for example, there is a significant effect on cognitive but not health outcomes. The clear majority of studies therefore indicate a causal relationship between financial resources and child outcomes. Below we consider each study in more detail, including those that find no significant relationship.

Three further points about our evidence base are worth highlighting at this stage. First, almost all of the studies investigate the impact of increases or decreases in *household income*, measured either continuously or by focusing on receipt of particular benefits or on movement above or below a particular income poverty line (see Tables A2 and A3 for details – see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp). Despite the broad set of terms for financial resources included in our template, none of the studies that met our other inclusion criteria examined wealth, assets or savings, and only one included a measure of self-reported financial hardship, reporting this alongside a household income measure (Gennetian and Miller, 2002). We identified several studies looking at the

The clear majority of studies therefore indicate a causal relationship between financial resources and child outcomes.

association between assets and child outcomes, for example, but none of these satisfied our causal criteria.

Table 1: Study results by country and evidence type

Studies by country and method	Positive results	Mixed results	No significant results	Total
Canada		1	1	2
– Exogenous variation		1		1
– Fixed effects			1	1
Mexico	1			1
– Randomised controlled trials	1			1
Norway	2	1	1	4
– Natural experiments	1		1	2
– Exogenous variation	1	1		2
US	17	4	1	22
– Randomised controlled trials	3	1		4
– Natural experiments	5		1	6
– Exogenous variation	1	1		2
– Fixed effects	8	2		10
US and Canada	1			1
– Randomised controlled trials	1			1
UK	2		2	4
– Natural experiments	1			1
– Fixed effects	1		2	3
All countries				
– Randomised controlled trials	5	1		6
– Natural experiments	7		2	9
– Exogenous variation	2	3		5
– Fixed effects	9	2	3	14
TOTAL	23	6	5	34

Second, as Table 1 testifies, the evidence is heavily concentrated in North America: 23 out of 34 studies cover the US, with just four studies from the UK and four from elsewhere in Europe (all from Norway). This is not a surprise as the US has high quality longitudinal datasets and a long tradition of research in this area, further helped by the advantage of state-level policy variation. But we may need to take care in generalising findings from the US to the UK. For instance, household income may be more important to child health in the US because of the absence of high quality public health care.

Indeed, of the four UK studies, two find no significant results, superficially suggesting a weaker relationship between income and outcomes than in the US. However, both of the non-significant studies use longitudinal fixed effect methods, and this brings us to the third point: some approaches seem to be more likely to identify positive results than others. There are methodological reasons for this that we discuss below. For now we simply note, first, that we need to take account of the methods used before reaching clear conclusions about the impact of income; and, second, that the mechanisms through which income appears to operate are important when thinking about whether results can be generalised to other contexts.

Table 2 presents evidence by individual outcomes. There are studies that cover three of our four broad outcome categories, but we found no studies meeting our inclusion criteria that look at children’s social inclusion and subjective well-being. There is most evidence on children’s educational attainment and cognitive development, followed by evidence on social-

behavioural and health outcomes. In the first two categories, a clear majority of studies find positive and significant income effects, but in the physical health category results are more mixed.

Table 2: Results by children's outcomes

Nature of outcomes	Studies including outcome	Positive	No effect
<i>Children's outcomes</i>			
Cognitive development and school achievement	21	16	5
Social, behavioural and emotional development	9	7	2
Physical health	8	5	3
Subjective well-being and social inclusion	0	n/a	n/a
Future earnings	1	1	n/a
<i>Mediating outcomes</i>			
Family expenditure on children's items	2	1	1
Financial stress and material hardship	3	2	1
The home learning environment	4	3	1
Maternal physical health	3	2	1
Maternal mental health	4	4	0
Parenting and parental behaviours	3	2	1
Total studies included*		34	

*Some studies measured more than one outcome.

We identified fewer studies that look at intermediate outcomes, such as parental health and the home environment. In most of these categories the majority of studies identify significant positive income effects, but results are mixed for mothers' physical health and for family expenditure on children's items. The clearest evidence is found for maternal mental health, where all the studies found positive results, although only four such studies were included.

We go on now to discuss the evidence in a little more detail. We organise discussion according to the type of evidence, and focus on whether or not studies identify a significant income relationship. In Chapter 5 we go on to look at the size of these effects.

Evidence from randomised controlled trials

RCTs are frequently described as the 'gold standard' of evidence on causal relationships (Sefton *et al.*, 2002). Their unique advantage lies in their ability to ensure that participants benefiting from a particular treatment or policy are distinguishable from a control group only by their receipt of the policy. This allows any differences in observed outcomes to be attributed to the policy rather than to other hidden factors. However, while RCTs are increasingly common in social policy, few shed direct light on our central question by allocating different amounts of money to otherwise identical groups. There are RCT evaluations of conditional cash transfer programmes (for example, the Oportunidades programme in Mexico) but as these programmes provide

cash transfers conditional on certain behaviours such as school enrolment or medical health check-ups they largely do not allow us to identify the effects of income itself.⁴ Similarly, RCTs have been conducted of welfare-to-work programmes in the US but on the whole these do not let us separate the income effects from the employment effects of these programmes.

However, we did identify six studies, five from North America and one from Mexico, that provide evidence on our central question, even if not quite at the 'gold standard' level of a pure RCT. Four of these studies make use of welfare-to-work RCTs in such a way that we get close to identifying income effects. Two examine the impact of the Minnesota Family Investment Programme in the US (Gennetian and Miller, 2002; Morris and Gennetian, 2003). This was a programme for lone mother families in the mid-1990s that randomly assigned participants into three research groups. The control group continued to receive Aid to Families with Dependent Children, which falls steeply as earnings rise; the second received financial incentives which allowed them to keep more of their welfare payments as earnings increased; and the third received the same financial incentives but it was also mandatory to participate in work and training. By comparing the incentives only with the incentives plus employment group, the authors are able to disentangle the effects of income from that of employment-related activity, because while the mandatory work programme increased employment relative to the incentives-only group, it did not lead to additional increases in income because of an off-setting reduction in welfare payments. Gennetian and Miller (2002) found that the incentives had significant effects on positive behaviour and compliance and on children's engagement in school, and reduced maternal depression and domestic abuse. Adding the mandatory employment part of the programme made no difference to most outcomes though it appeared to *decrease* children's social competence and autonomy. Using somewhat different methods on the same data, Morris and Gennetian (2003) find positive and significant effects of the incentives on positive social behaviour and school engagement, with effects for school achievement and problem behaviour not significant.

Two other studies pull together data from a number of different random assignment programmes to try to compare the impact of programmes that increase employment with those that also raise income. Both focus on educational outcomes. Duncan *et al.* (2011) looked at data from studies evaluating ten different random assignment welfare programmes in the US and Canada, and find significant positive effects of family income on school achievement. Pooling data from 14 US programmes, Clark-Kauffman *et al.* (2003) found positive effects on cognitive development and school achievement only for programmes that increase income.

The last two studies in this category are evaluations of conditional cash transfer programmes. One examines the Mexican *Oportunidades* programme, managing to separate the income effect from the effect of other components of the programme by making use of variation in the total amount of cash received by different families (higher transfers were made to families with more children in school, and payments were also higher if children were at later stages, and higher still if they were girls) (Fernald *et al.*, 2008). The authors find that a doubling of cash transfers had significant favourable effects on a number of both health and cognitive outcomes. It improved height-for-age and BMI-for-age and reduced the prevalence of stunting and being overweight, but did not significantly affect the number of sick days taken, motor development or haemoglobin concentration. In terms of cognitive development, significant effects were identified for endurance, long-term/short-term memory, visual integration and language development.

Pooling data from 14 US [welfare] programmes, Clark-Kauffman *et al.* (2003) found positive effects on cognitive development and school achievement only for programmes that increase income.

Finally, we include one outcome from an evaluation of the New York City Family Rewards programme (Riccio *et al.*, 2010). Participants and control group were randomly selected through lottery, with participants (and their children) receiving cash transfers conditional on meeting certain education, health and employment-focused conditions. The fact that conditions themselves will have directly affected many of the outcomes measured rules out much of this study for our purposes, but we include it in a limited way, looking only at an intermediate nutrition outcome not linked to incentives: families of participants were significantly less likely than the control group to report sometimes or often not having enough food to eat.

Evidence from natural experiments

Our next group of studies makes use of so-called ‘natural experiments’: situations in which some people receive more income than others not because of a planned experiment, but simply because of natural policy variation due to circumstances of time, place or (observed) household characteristics. Natural experiments are often considered the second strongest source of evidence after RCTs because, as with RCTs, the change in income can be considered exogenous – driven by outside factors and not by hidden household characteristics that may be behind both higher income and better outcomes (Hakim, 2000). Some natural experiments are driven by natural events but more often researchers are exploiting a change in policy that affects some groups more than others.

The Norwegian oil shock in the 1970s provides a rare example of an experiment caused by nature: the discovery of off-shore oil led to a relatively short-lived economic boom which increased income sharply in some areas of Norway. Two studies examine whether the income gains from the boom affected later educational outcomes for children who were growing up in the 1970s. Løken (2010) and Løken *et al.* (2012) compare children born just before the boom in a county very close to the oil fields with children born at the same time in more distant counties, and with children born a decade later, when the effects of the boom were fading. Løken (2010) finds no effect of the boom on higher education attainment, but Løken *et al.* (2012) revisit the data using different techniques, in particular making greater allowance for the possibility of a non-linear relationship between income and education (meaning that additional income makes more difference at some parts of the income distribution than others). The second study finds a significant positive income effect, diminishing at higher levels of income.

A second promising natural experiment was created by the opening of a casino on an Eastern Cherokee reservation in rural Carolina, halfway through data collection for a longitudinal study of child mental health outcomes. A proportion of the profits from the new casino was distributed on a per capita basis to all adult tribal members, regardless of other characteristics, leading to an income jump of around US\$4,000 per year per adult. Households without tribal members received nothing. Costello *et al.* (2003) find that children in households which moved across an income poverty threshold (the US federal poverty line) as a result of the payments showed a significant decrease in the mean number of psychiatric symptoms, with most effect on behavioural symptoms and least on emotional symptoms. Children in households who were never below the poverty line were not affected. Akee *et al.* (2010) use the same data to look at a wider range of outcomes, and find that the payments led to increases in the average length of completed education, reductions in crime among teenagers, fewer arrests for parents,

Children in households which moved across an income poverty threshold (the US federal poverty line) as a result of the [casino] payments showed a significant decrease in the mean number of psychiatric symptoms, with most effect on behavioural symptoms and least on emotional symptoms.

increases in parental supervision and more positive interactions with mothers (as reported by children). Effects were biggest for the poorest households and there was a gendered effect: educational outcomes improved only if mothers received the money and not if fathers did. It should be said that Costello *et al.* (2003) note that the casino gave employment priority to tribal members, a fact which is not mentioned by Akee *et al.* (2010). However, the Akee paper compares trends in unemployment rates for households with and without Native American parents, and finds no evidence that they differ.

Our other natural experiments make use of changes in government benefit policy that mean that some family types (or families in some regions) receive more income than others.

Three studies exploit changes in the generosity of the Earned Income Tax Credit (EITC) in the US and the fact that these affected some household types more than others. EITC is a tax credit paid to low-income in-work families in some US states, which operates both to increase incomes directly and as an incentive to increase earnings, as payments are phased in as earnings rise. The three studies exploit reforms in the 1990s in different ways, all of them finding significant income effects on health and education outcomes. Evans and Garthwaite (2010) make use of the fact that in the early 1990s payments increased by more for households with two or more children than for those with just one. They compare changes over time in self-reported health, mental health outcomes and biomarkers indicating a health risk for mothers in these two groups (all of them mothers with a high school degree or less). Larger EITC payments significantly improved outcomes in all three domains. Strully *et al.* (2010) compare women in states with an EITC programme with similar women (unmarried mothers with a high school degree or less) in states without. They look at maternal smoking and at low birth weight: both outcomes show relative improvements in EITC states after payments increased. Finally, Dahl and Lochner (2012) compare higher-income families, who would have been unaffected by changes to EITC, with lower-income families who are likely to have been eligible, to see whether there are relative improvements in children's test scores among the families that stood to gain. They find significant positive effects for maths and reading scores, especially for boys, younger children and children with lower-educated mothers.

It is worth highlighting that all three of these studies make use of *likely eligibility* for higher payments, rather than actual receipt, which is not always known. The disadvantage of this approach is that it will push estimates of any income effect downwards, as income is being captured in an imprecise way: not all households will in fact have received the benefits ascribed to them. Estimates from these types of studies should therefore be considered as lower-bound estimates for a positive impact of income.

Finally, we include two studies here that examine how changing benefit generosity for some family types affected patterns of expenditure, and in particular whether higher benefit levels led to increased spending on children's items as income rose for particular family types. Gregg *et al.* (2006) exploit the fact that UK benefit reforms between 1998 and 2001 favoured low-income families over higher-income ones, and families with children under 11 over those with older children. They find significant gains in relative spending among low-income households on children's clothing and footwear, fruit and vegetables, and toys and books; higher spending on durables, including a car and a telephone; and reduced spending on alcohol and tobacco. Kaushal *et al.* (2007) take a similar approach to examining the impact of US welfare reforms on spending patterns, comparing changes in spending for families gaining most from higher welfare payments (low-educated single mothers) to other demographic groups. Like the UK study,

Kaushal *et al.* identify increased spending on durables, including a car, a telephone and a microwave, but they do not find significant changes in spending on children's clothing and footwear or on learning and enrichment. Instead, spending rose on transportation, food away from home, and to a lesser extent adult clothing and footwear. These are all items that may be linked to work outside the home, and the authors suggest that the mandatory employment aspect of US welfare payments may explain the different effects of higher benefits in the two countries. They also speculate that the labelling of UK benefits as Child Benefit or Child Tax Credit may increase the extent to which they are spent on children – a point that has relevance in the UK in the light of the introduction of Universal Credit, under which the Child Tax Credit label will be lost.

Evidence from other sources of exogenous income variation

Our third group of studies also look to identify the true effect of income by finding a source of income variation beyond household control, but they do so without the advantage of a particular change in income over time to exploit. For the most part, these studies use instrumental variable approaches: rather than looking directly at income in individual households, they find an 'instrument' which is correlated with household income (it could be a particular income source) but not with any hidden household characteristics which would themselves drive outcomes.⁵ This avoids the danger that results will be biased upwards, because income differences will not have been caused by hidden household characteristics, but it also introduces measurement error which will bias results downwards, as the instrument will not be perfectly correlated with actual income changes; this is a similar problem to that noted for the EITC studies that make use of changes in *eligibility* rather than *actual receipt* of benefits.

The first such study exploits variation in child benefit levels across time, family type and Canadian provinces. Milligan and Stabile (2011) simulate the benefits that a random sample of families would be eligible for in each province and year between 1994 and 2004, and examine whether the differences predict educational and health outcomes for children. The results are mixed. Significant favourable effects were found for maths scores and learning disabilities, but only for boys from lower-educated backgrounds, while there were significant improvements in physical aggression and indirect aggression, but only for girls from lower-educated backgrounds. Child health measures did not improve. There were strong positive effects on maternal depression but no impact on maternal physical health.

A second study in this group makes use of the fact that in Norway there is a sharp discontinuity in the eligibility for childcare subsidies. Black *et al.* (2012) establish that the subsidies do not seem to affect labour force participation or the use of childcare but operate in effect as a boost to disposable income for eligible families. Using administrative data on the entire Norwegian population, the authors compare families just below and just above the cut-off for subsidies. They find significant positive effects of the subsidies on medium-term educational outcomes (test scores in junior school), with larger effects in municipalities with larger subsidies and in those where the income cut-off is lower, implying larger effects for lower-income households.

Tominney (2010) exploited the richness of Norwegian administrative data in a different way, by using income data for 99 travel-to-work areas between 1970 and 1980 to identify annual income shocks that can be attributed to local labour market conditions rather than to household

Milligan and Stabile (2011) simulate the benefits that a random sample of families would be eligible for in each province and year between 1994 and 2004, and examine whether the differences predict educational and health outcomes for children. The results are mixed.

characteristics, and to distinguish between transitory shocks and permanent shocks with lasting effects on income. Results for health were insignificant for both permanent and transitory shocks, which Tominey thinks may be explained by crude measurement of health variables. But both transitory and permanent shocks had significant effects on educational outcomes (completed years of schooling, high school dropout and college attendance). The effects of permanent shocks were much larger, and (not surprisingly) had a bigger impact the earlier in the child's life they were experienced.

A study by Shea (2000) uses variations in fathers' earnings due to union status, industry and involuntary job loss to explore whether long-term childhood income affects children's schooling duration or future earnings. Shea's argument is that variations in income driven by these factors can be attributed to luck rather than to innate ability or other paternal characteristics. His study uses data from the US Panel Study of Income Dynamics, which contains both a representative and a low-income sample. For the representative sample no positive effects are found, but for the low-income sample variation in childhood income does appear to be predictive of children's future earnings and income, though not of their length of schooling.

Finally, we include here a small study by Meyers and Frank (1995) that examines the height and weight of children waiting at the paediatric department of an urban municipal hospital in the US. The study compares children in families in receipt of housing subsidies (which, like the Norwegian childcare subsidies, effectively boost household disposable income) with families who are on the waiting list for subsidies but have not yet received them. Examining demographic differences between the two groups, the authors conclude that those already in receipt of subsidies were more deprived, if anything, than the waiting list group. Despite this, children whose families received the housing subsidy were significantly less likely to have low growth indicators than children whose families were still on the waiting list, pointing to a positive impact of the additional income on child nutrition.

Evidence from studies that exploit income change within households over time

In our last group of studies, there is no source of exogenous income variation. Instead, authors make use of longitudinal data, tracking household income and children's outcomes over time. By using econometric methods such as fixed effect models, these studies are able to exclude differences between households, such as genetic endowments, social class and internal motivation, and focus on *within-household* changes over time. Some such studies track outcomes for the same children over time, while others use outcomes for siblings born at different time points, when family finances were better or worse. Of course it may still be possible that these models pick up spurious correlations between changes in income and outcomes that are driven by a third factor unobserved in the data (e.g. the death of a relative), but the major life changes which might drive both income and outcomes are usually observed and can be controlled for – a move into employment or the break-up of a marriage.

One disadvantage of the fixed effect approach is that discarding information on between-household variation leads to standard errors that are often considerably higher than in models which make use of both between-household and within-household variation, and this means results are much less likely to be significant (Allison, 2005). The most straightforward way to explain this is that, having discarded most of the available variation (the

differences across children in both their circumstances and outcomes), it is harder to accurately fit a model that explains the limited amount of variation remaining (the differences over time for each child) and a wider margin of error is needed around the model's estimates. Measurement error is a second important problem: income at any single point in time is always measured with error, and if the income measure in both year 1 and year 2 is a little imprecise, a calculation of income change from one year to the next is likely to be doubly inaccurate and may even get the direction of movement wrong. This means that the size of coefficients is likely to be biased downwards. Both of these problems will be bigger where fixed effect approaches make use of short panels, with data covering just a few years.

These drawbacks mean that we would expect fixed effects models to be less likely to pick up significant effects than the alternative approaches already reviewed, which have a particular source of exogenous income variation to exploit, and we would also expect that where positive effects are identified they would be smaller. A third potential issue is that fixed effect models focus in on short-term income changes; indeed, the models deliberately net out differences in permanent income. This is a disadvantage given that research that is able to compare short-term and longer-term income changes has suggested that permanent income has the greater impact, as discussed below. But this may also affect the other approaches, depending on whether households treat a change in benefit generosity (for example) as a short-term or permanent shift in income.

(All the studies reported here estimate conventional regression models as well as fixed effect models, as is standard in the literature. The conventional models all find positive and significant income effects, but we report here only on the longitudinal fixed effect results.)

Four studies use data on the children of the National Longitudinal Survey of Youth (NLSY) in the US to get at the relationship between income and a number of different outcomes. Blau (1999) uses data from 1986 to 1991 to examine the effects of income on cognitive and social-behavioural outcomes, comparing individual children's outcomes over time (many children had been tested two or three times) and also using sibling and cousin comparisons. Fixed effect models identified small significant effects for maths, reading, vocabulary, behaviour problems and social development. A range of alternative income measures were tested and not all of them were significant, but only one outcome, verbal memory, showed no significant result for any income measure. Nearly all income measures showed stronger significant effects on the home environment.

Also using the children of the NLSY, Votruba-Drzal (2003) focused on cognitive stimulation in the home environment, making use of scores at two time points, age 3–4 and age 7–8, for five cohorts of children. Income changes during this period of childhood are found to be significant predictors of changes in the level of cognitive stimulation provided by children's home environments, with larger effects for low income households. Votruba-Drzal (2006) looked at reading and maths scores, and at socio-emotional development for children in early childhood (birth to 5–6 years old) and middle childhood (5–6 to 11–12 years old). In place of fixed effects, the author uses residualised change models, examining the effect of income during middle childhood after controlling for early childhood income and outcomes. Income in middle childhood was found to have no explanatory power for academic skills but did affect the development of behaviour problems during middle childhood. The author suggests that behaviour may respond more quickly to income changes than academic skills, which may be more difficult to alter after the early years or which may respond more

slowly or with a lag. This study also found that children from low-income households were particularly sensitive to the effects of family income.

Burnett and Farkas (2009) also used the NLSY but over a longer time frame (1986 to 2002) to test whether poverty status has an impact on maths scores, which were measured at multiple time points between age 5 and 14, meaning data on up to five tests for each child. They found significant negative effects of poverty but only for younger children and only of modest size.

Three studies conduct sibling comparisons using another US dataset, the Panel Study of Income Dynamics (PSID). Duncan *et al.* (1998) found that family income is significantly associated with years of completed schooling. Conley and Bennett (2001) looked at whether income in pregnancy affects low birth weight. If other factors such as maternal education are controlled for, no significant effect is found overall, but the income-to-needs ratio is significant for children whose own mother was born at low birth weight, suggesting income has a protective effect on higher risk babies. Johnson and Schoeni (2011) look at whether income in pregnancy affects childhood health and educational outcomes. They find increases in income in pregnancy do have a significant effect on mother-reported health status but results for maths and reading outcomes are not significant. Furthermore, there is no effect on health for the highest-income families, and an unexpected negative effect for the very poorest.

Also for the US, three studies by Dearing and colleagues make use of the NICHD Study of Early Child Care and Youth Development (SECCYD), which collected data 1, 6, 15, 24 and 36 and 54 months after childbirth, to look at maternal depression, the home environment and children's behaviour. Dearing *et al.* (2004) found that income gains resulted in the alleviation of symptoms of maternal depression, especially when the gains were substantial enough to lift families out of poverty. Dearing and Taylor (2007) found that income changes were significantly positively associated with the quality of both physical and psychosocial home environments, with greatest impact at the lower end of the income distribution and for families with the least stimulating environments to start with. Dearing *et al.* (2006) found significant effects of income on externalising behaviour that were small across the whole sample but substantially larger for chronically poor children. For both externalising and internalising behaviour, low income was most strongly associated with problems when chronically poor children's mothers were partnered and employed.

Dooley and Stewart (2007) used Canadian data, the NLSCY, to look at emotional and behavioural development, including hyperactivity and conduct disorders, as reported by teachers, parents and the child themselves. Their fixed effect models found little evidence of significant effects of income. However, the authors acknowledge that this is a relatively short panel, with only three measurement points between 1994/95 and 1998/99. In addition, their fixed effect models controlled for parenting style, which is likely to be a mediator.

Finally, there are three fixed effect studies using UK data. Two use data from the first three waves of the Millennium Cohort Study (MCS), when children were 9 months, 3 years and 5 years old. Violato *et al.* (2009) looked at the effect of household income on childhood asthma. Fixed effects model found no significant effect for income. Violato *et al.* (2011) focused on cognitive and social-behavioural outcomes, including longitudinal measures of naming vocabulary and behavioural outcomes. In the fixed effect models income was only significant for the vocabulary test, and only for children from lone-parent families. In both the MCS studies, authors were able to

Dearing *et al.* (2006) found significant effects of income on externalising behaviour that were small across the whole sample but substantially larger for chronically poor children.

exploit only limited variation in income and outcomes, with the outcome measures captured just twice, at ages three and five. Furthermore, the fixed effect models control for a number of variables which might be expected to act as mediators, such as maternal depression, warm parenting practices, a calm home atmosphere and parental 'investments' such as reading and visits to the library. If income does have a causal impact, and it operates through one or more of these mechanisms, the income effect would be better captured in a model that did not include them as controls.

The third UK study, by Blanden and Gregg (2004), used sibling fixed effect models in the British Household Panel Study to look at the impact of income on educational outcomes at 16 and beyond. Income at 16 was found to be a significant predictor of staying on at school at 16, with a coefficient not much smaller than that in the cross-sectional model, though significant only at the 10 per cent level because of large standard errors. Large standard errors mean that income at 18 was not found to be significant in determining degree attainment.

Summary

Overall, then, the evidence supports a causal interpretation of the association between income and wider child outcomes. Out of 34 studies examined, 23 show a positive and significant relationship between income and all the outcomes examined (although not necessarily all measures of each outcome). A further six studies show an impact of income on some but not all outcomes, while only five studies found no evidence of an income effect. There is evidence of significant positive effects on all the child outcomes we looked at, except for subjective well-being and social inclusion, where no studies were found that met our criteria. The strongest evidence relates to cognitive development and school achievement, followed by social and behavioural development, both in the sense of the number of relevant studies focusing on these outcomes, and the clear majority identifying positive effects. Evidence about the impact of income on children's physical health is more mixed.

With regard to intermediate outcomes, the strongest evidence relates to maternal mental health, where all four relevant studies identify a positive income effect, followed by parenting and the home learning environment. Evidence is mixed in relation to maternal physical health and family expenditure on children's items (but there are only two studies in each of these categories).

Focusing on the studies that do not find significant income effects, there is good reason to believe that the methodology and data may explain the result. Three of these are fixed effect studies that make use of short panels, meaning that they have limited income variation to exploit and this is likely to affect the significance of results; two of these studies also control for factors we would consider to be mediators, such as parenting practices. The fourth is one of the studies of the Norwegian oil boom, but while this finds no evidence of a linear income effect, a follow-up study identified a significant *non-linear* effect, indicating that the additional income did make a difference to families at the bottom of the income distribution. The fifth study is the one that looks for changes in expenditure on children's items as income from welfare-to-work programmes rises in the US and finds very different results from a similar study conducted on UK data, most likely because additional income is being spent on work-related expenses in the US.

In the next chapter we go on to examine the size of the income effects identified, focusing not just on whether money matters, but to what extent.

With regard to intermediate outcomes, the strongest evidence relates to maternal mental health ... followed by parenting and the home learning environment. Evidence is mixed in relation to maternal physical health.

5 HOW MUCH DOES MONEY MATTER?

We have seen that the majority of our studies identify a positive and significant effect of income on a range of children's outcomes. We now want to explore how large this income effect is, and how it compares to the effect of other factors, such as government spending on schooling or early childhood interventions.

Many of the studies we looked at calculate 'effect sizes', or provide the information necessary for us to calculate them ourselves. Effect sizes give us the marginal effects of income change as a percentage of the dependent variable's standard deviation. In other words, if income was boosted by a given amount, how much of the average variation that exists between any given child and the mean score for a particular outcome would we expect to see eliminated?

The effect sizes for each study are presented in Appendices 3 and 4 (see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp), but it is difficult to get an overall sense of the scale of the income effects from reading through this information, for two main reasons. First, each study presents results for a different level of income change, and in a particular context. A second problem arises in interpreting what the effect sizes mean: is a reduction in behavioural problems of 12 per cent of a standard deviation to be considered large or small?

In this chapter we try to address both these difficulties, first by attempting to calculate standardised effect sizes which get closer to presenting effects for a common income input (though difficulties with this remain), and then by considering what the sizes mean by comparing them to effect sizes identified for other interventions.

Comparing standardised effect sizes across studies

The studies in the review that present their results as effect sizes use a variety of different options but the most common choice is a US\$1,000

change in annual income in 2000 prices. Given that the majority of studies are from the US, it seemed appropriate to leave results in dollar terms so we take US\$1,000 (2000 prices) as our standard level of income change (roughly equivalent to £900 in the UK in 2013). Effect sizes for studies using different dollar amounts were adjusted up or down accordingly, after dollar sums had been converted to 2000 prices using the US Consumer Price Index. For countries outside the US, currencies were first converted into US dollars using OECD Purchasing Power Parities (PPP), and then to 2000 prices using the US CPI.⁶

The standardisation is helpful but leaves a number of issues unresolved. For one thing PPP and price conversion adjust for the cost of living in terms of a basket of goods, but not for differences in average incomes. Depending on the mechanisms through which income operates, US\$1,000 might be expected to make a bigger difference to children's outcomes in a context where average income was US\$15,000 than where it was US\$30,000, but adjusting consistently for these differences is beyond the scope of this paper. A second, related, issue is that US\$1,000 is likely to mean more at the bottom of the distribution than the top, as discussed further in the next chapter. Our tables go some way towards addressing this by indicating whether the sample includes the full distribution or just low-income households, and in places by providing differentiated effect sizes for different groups. The third and largest problem arises from the different approach to equivalisation of income taken in different studies. Some but not all studies adjust household income in accordance with household size: thus Dearing *et al.* (2006) standardise income using the US income-to-needs ratio, while Blau (1999) looks at changes in total household income and Akee *et al.* (2010) at the impact of a transfer of US\$4,000, regardless of household size (see Tables A2 and A3 for more detail – see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp). This means that a standardised US\$1,000 is actually capturing something rather different across studies. As it would be near impossible to adjust effect sizes accurately to compensate for these different approaches, the standardised measures should be treated as giving us a broad idea of the range of effect sizes rather than a clean comparison across individual studies or outcomes.

Tables 3–6 present all the standardised effect sizes we were able to calculate for continuous variables, for each outcome in turn and with studies grouped within tables by research methodology. Only significant results were included and only results that can be interpreted as the size of the response to a given change in income: studies that estimate the impact of a move across a particular threshold – e.g. from 'poor' to 'not poor' – were not included.

The first thing that stands out from all four tables is that studies using longitudinal fixed effect (or similar) approaches identify smaller impacts of income than experimental designs. For instance, the fixed effect studies find that US\$1,000 consistently delivers just 1 per cent or 2 per cent of a standard deviation improvement in cognitive outcomes (Table 3), while studies using other approaches find effect sizes ranging between 5 per cent and 27 per cent, considering boys and girls together. For social and behavioural outcomes (Table 4) the range is 1 per cent to 3 per cent for fixed effect studies and between 9 per cent and 24 per cent for other methods. Evidence on health (Table 5) and maternal depression (Table 6) is more limited but tells a similar story. For the home environment, we only have effect sizes for the longitudinal studies: in one study, Dearing and Taylor (2007), these look larger than for other outcomes, but only where the effect size is calculated for the low-income population only.

Studies using longitudinal fixed effect (or similar) approaches identify smaller impacts of income than experimental designs.

Table 3: Effect sizes for cognitive development and educational achievement (standard deviation change linked to US\$1,000 in 2000 prices)

	RCTs				Natural experiments		Other exogenous variation (IV)		Fixed effects		
	Gennetian and Miller (2002)	Duncan et al. (2011)	Fernald et al. (2008)	Clark-Kauffman et al. (2003)	Akee et al. (2010)	Dahl and Lochner (2012)	Black et al. (2012)	Milligan and Stabile (2011)	Blau (1999)	Votruba-Drzal (2003)	Votruba-Drzal (2006)
Relevant population	Low income US	Low income US and Canada	Low income Mexico	Low income US	Low income Native American US	Low income US	Low/middle income Norway	Low education (max high school) Canada	All income groups US	All income groups US	All income groups US
Child achievement/ Performance in school	0.12 (0.23 boys)	0.06		0.05			0.18			0.02	
Maths						0.06		0.07 (0.23 boys)	0.01		0.02
Reading						0.05		0.37 (boys)	0.01		0.02
Peabody PPVT			0.21						0.01		
Long-term memory			0.14								
Short-term memory			0.15								
Visual integration			0.10								
Completed schooling (years)					0.10						0.01

Notes: Dahl and Lochner's reading figure is an average of effect size for reading recognition (0.04) and reading comprehension (0.06). Duncan et al. (1998) do not give standard deviation for completed schooling so result not included. Results from Loken (2012) are in a form difficult to adjust to this format.

Table 4: Effect sizes for social and behavioural outcomes (standard deviation change linked to US\$1,000 in 2000 prices)

	RCTs		Other exogenous variation	Fixed effects		
<i>Relevant population</i>	Gennetian and Miller (2002)	Morris and Gennetian (2003)	Milligan and Stabile (2011)	Blau (1999)	Dearing et al. (2006)	Votruba-Drzal (2006)
	Low income US	Low income US	All income groups Canada	All income groups US	Chronic poor and partnered only US	All income groups US
Behaviour Problem Index	0.12 (girls 0.22)			0.01		0.01
BPI internalising	0.12				0.02	
BPI externalising	0.11				0.03	
Positive behaviour	0.15	0.24				
Engagement in school	0.17	0.24				
Hyperactivity – inattention			0.07			
Emotional disorder – anxiety			0.10			
Indirect aggression			0.22 (low ed girls)			
Conduct disorder – physical aggression			0.10 (0.16 low ed girls)			

Notes: Dahl and Lochner's reading figure is an average of effect size for reading recognition (0.04) and reading comprehension (0.06).

Table 5: Effect sizes for child health (standard deviation change linked to US\$1,000 in 2000 prices)

	RCTs Fernald et al. (2008)	Other exogenous Milligan and Stabile (2011)	Fixed effects Conley and Bennett (2001)
<i>Relevant population</i>	<i>Low income Mexico</i>	<i>All income groups Canada</i>	<i>Children with a low birth weight parent</i>
Height for age	0.24	0.04 (boys 0.13)	
Low birth weight			0.03

Table 6: Effect sizes for maternal depression and home environment (standard deviation change linked to US\$1,000 in 2000 prices)

	RCTs	Other exogenous variation	Fixed effects			
	Gennetian and Miller (2002)	Milligan and Stabile (2011)	Dearing et al. (2004)	Blau (1999)	Votruba- Drzal (2003)	Dearing and Taylor (2007)
<i>Relevant population</i>	<i>Low income US</i>	<i>All income groups Canada</i>	<i>All income groups US</i>	<i>All income groups US</i>	<i>All income groups US</i>	<i>Low income US</i>
Maternal depression	0.15	0.10 (0.20 low ed only)	0.01 (0.06 for chronically poor)			
Home environment				0.02	0.01 (0.02 lowest income)	
Physical environment						0.05 (low income)
Psychosocial environment						0.06 (low income)
Learning materials						0.20 (low income, low HE)
Responsiveness						0.14 (low income, low HE)
Cognitive stimulation						0.06 (low income, low HE)

A second interesting point is that there is some evidence of differential results by gender, although this evidence comes from only two studies. Gennetian and Miller (2002) and Milligan and Stabile (2011) both find much bigger income effects for boys than girls with regard to cognitive outcomes, alongside bigger effects for girls than boys with regard to social and behavioural outcomes. (Few studies provide breakdowns in results by gender so this does not mean that differences were tested and found absent in other studies.)

There are three possible reasons why the fixed effect studies find consistently smaller effect sizes than studies using other approaches (see also discussion in Dahl and Lochner, 2012). The first is that, on

the whole, the experimental studies and those exploiting an exogenous change in income are focused on the low income part of the population, while the longitudinal studies make use of survey data which is nationally representative and so includes higher-income families. If income matters more in the lower part of the distribution, studies that focus on lower-income families will find larger effects. Our studies provide strong evidence that effects are indeed non-linear, and this is explored in detail in Chapter 7. Where the fixed effect studies isolate the income effect for the lower-income population (as, for example, Dearing *et al.*, 2006, do for the home environment), the effect sizes increase in size, though they remain lower than for the other types of study, making this at best a partial explanation.

The second reason is that fixed effect studies are perhaps more likely than the other approaches to be picking up short-term rather than more permanent changes in income. Fixed effect studies exploit any within-household changes in income from one wave of data to the next, some of which may reflect transitory income shocks. In contrast, the variations in benefit systems and subsidies exploited by many experimental and instrumental approaches *may* be changes that households consider to be longer term. In the case of the casino development, for example, profit disbursements have been made to households every six months since 1996, so the additional income is likely to feel like a permanent boost rather than a single windfall. If permanent income changes are more important in driving outcomes than short-term variation (and this is discussed further later on), then this could help explain why fixed effect studies find smaller effects.

The third reason is that longitudinal data is almost certainly capturing income variation and income change less accurately than the other types of study. As discussed earlier, longitudinal fixed effect studies focus on changes in income within households over time, and as income is measured with a great deal of error at each point, it is likely that there is considerable inaccuracy in identifying both the direction and scale of change from year to year. This degree of noise will make it harder to identify significant results and will bias coefficients downwards. Experimental approaches are able to identify income changes with much greater precision, because the experiment itself is driving the change. Instrumental approaches, which make use of other exogenous sources of income variation, plausibly fall in the middle: they can accurately estimate the scale of income change by examining policy, but they tend to use eligibility rather than actual take-up to identify recipient households, and this is another source of downward bias. Because of the sparsity of experiments and the difficulties of identifying good instruments, fixed effect studies are popular with researchers, but our findings indicate that we should be cautious about using their results for reliable information on the extent to which money matters. While the majority of the longitudinal studies we look at do identify significant income effects, with 11 out of 14 finding positive and significant effects in at least some outcome categories, they appear to considerably underestimate the true effects of income.

How big or small are these effects?

The results from the experimental and exogenous change studies fall in the region of 5 per cent to 27 per cent of a standard deviation linked to a US\$1,000 (£900 in 2013 prices) annual change in income, with larger effects for some sub-groups. But what do these numbers actually mean?

A first way of getting a handle on what this scale of change means is to put it into the context of the average gap between children from low-income and high-income backgrounds. Gregg (2008) notes that for Key Stage 2 results at age 11 in England, one-third of a standard deviation is 1.5 KS2 points, which is about half of the gap between children receiving free school meals (FSM) and the rest of the population. If we take the lower end of results from experimental studies (an impact of 5 per cent of a standard deviation from US\$1,000), and if we assume that results can be scaled up, with each increase in income yielding the same returns, it would take a boost in family income of around £6,000 a year in 2013 to close half the FSM gap, although there is evidence that effects may be larger for boys. Note that Hobbs and Vignoles (2010) found that average income in households where children benefit from free school meals was on average £110 lower per week in 2004/05 than in households where children do not, or around £135 per week (£7,000 per year) in 2012 prices. Our calculations suggest that closing the gap between FSM households and the average income for non-FSM households would not eliminate the achievement gap, but might be expected to reduce it by more than half. These calculations are set out in more detail in Box 2.

It would take a boost in family income of around £6,000 a year in 2013 to close half the FSM gap.

Box 2: Closing the FSM gap: a calculation

The results from the experimental and exogenous change studies indicate that a US\$1,000 change in income can be linked to a change in cognitive outcomes of between 5 per cent and 27 per cent of a standard deviation. The US\$1,000 is measured in 2000 prices, which corresponds to about £900 in 2013.

To get a handle on what 5 per cent or 27 per cent of a standard deviation means in practice, we take results from Gregg (2008), who finds that for Key Stage 2 results at age 11 in England, one-third of a standard deviation corresponds to 1.5 points. This in turn is roughly half the gap between the average score for children receiving free school meals and the rest of the population.

Let us take the bottom end of the estimated range from our experimental and exogenous change studies, with £900 yielding an improvement in cognitive scores of 5 per cent of a standard deviation. This keeps our calculation conservative in one sense: some of the estimates identified are five times this high. On the other hand, we make the (perhaps generous) assumption that results can be scaled up (that is, if £900 improves outcomes by 5 per cent of a standard deviation, £1800 will improve outcomes by 10 per cent). If £900 yields an improvement of 5 per cent of a standard deviation in KS2 scores, and if we can scale results up, it would take around £6,000 to raise achievement by one-third of a standard deviation ($5\% \times 6.5 = 0.325$, so we would need to spend £900 six and a half times over). Gregg's calculation tells us that an improvement in scores on this scale for children eligible for FSM would eliminate half the average achievement gap between these children and the rest of the population.

Hobbs and Vignoles (2010) find that the average income in households where children receive FSM is roughly £7,000 per year lower than in those where they do not: £110 per week in 2004/05 prices, which corresponds to £135 per week in 2012 (£7,020 per year). Thus we conclude that closing the income gap between FSM and non-FSM households (that is, increasing income in FSM households by £7,000 per year) might be expected to reduce the achievement gap by a little over half.

Of course, this is a large sum of money in the context of the UK benefit system, and as Gregg (2008) points out, broader reductions in inequality in the distribution of work and wages are likely to offer more substantial progress than straightforward redistribution through the tax-benefit system. Nevertheless, policy changes to the benefit system that affect family income by hundreds and even thousands of pounds a year are far from uncommon. To take one example, the decision from April 2011 to reimburse only 70 per cent rather than 80 per cent of childcare costs through the childcare element of Working Tax Credit meant an effective reduction in household income of £1,560 a year for low income working households eligible for the maximum level of support, assuming parents did not respond by reducing use of formal care.

A second useful exercise is to compare the income effect sizes to those calculated for other interventions. Meta-analysis of early intervention programmes points to average effect sizes of between 23 per cent and 52 per cent on measures of children's achievement or school readiness (Higgins *et al.*, 2012), but many studies do not record the level of spending on each programme. Duncan *et al.* (2011) point out that the Abecedarian early education project in the US found treatment effects on IQ of one full standard deviation at age 3 and 75 per cent at age 5, but the project cost more than US\$40,000 per child in 2003 prices (see also Karoly *et al.*, 2005). Similarly, the Perry Pre-school project delivered 60 per cent of a standard deviation improvement in IQ but cost US\$15,000 per child in 2003 prices (Duncan *et al.*, 2011; Karoly *et al.*, 2005).

For England, Higgins *et al.* (2012) report that the Effective Provision of Pre-School Education (EPPE) study suggests an effect size of 18 per cent for pre-school attendance in England in the mid-1990s on performance in reception class. More specifically, Sammons *et al.* (2004) conclude that 1–2 years in a pre-school tracked by EPPE leads to an improvement in pre-reading attainment of 15 per cent of a standard deviation and in early number concepts of 11 per cent, compared to attendance for less than one year. Effect sizes are larger if calculated for one year of experience versus none – 12 per cent for pre-reading, 33 per cent for early number concepts and 47 per cent for language – and larger still for 2–3 years versus none (up to 48 per cent for pre-reading, 55 per cent for numbers and 63 per cent for language). As current government spending on the free entitlement to part-time nursery education for three and four year olds is roughly £2,300 per child per year (authors' calculation from figures in NAO, 2012), this range of numbers indicates a broadly similar scale of return to what might be expected from boosting household incomes by the same amount. On the one hand, however, the EPPE figures are for all children, not just the disadvantaged, and EPPE effects have been found to be greater for children from lower-income backgrounds (Sylva *et al.*, 2004). But on the other hand, EPPE is not an experimental study and there are likely to be unobserved differences between children who attended pre-school and those who did not, as well as unobserved differences in their backgrounds, which would make the EPPE estimates biased upwards.

Studies of school education expenditure in England have largely found effect sizes at the lower end of the household income effects identified here, with a £1,000 increase in annual expenditure per child linked to between 2 per cent and 7 per cent of a standard deviation on test scores (Steele *et al.*, 2007; Holmlund *et al.*, 2010; Nicoletti and Rabe, 2012), although one study identifies a much larger effect of 25 per cent of a standard deviation (Gibbons *et al.*, 2011).

Finally, it is worth remembering that household income appears to affect a wide range of different outcomes at the same time. We have seen evidence of significant effects on parenting and the physical home environment, maternal depression, smoking during pregnancy, children's cognitive ability, achievement and engagement in school, and children's behaviour and anxiety. Even small income effects operating across this range of domains could add up to a larger cumulative impact, as Mayer (1997) points out, referring to income support policies as the 'ultimate "multipurpose" policy instrument' (p.145). Few other policies are likely to hit as many different outcomes at the same time.

Summary

In sum, effect sizes vary depending on the type of study, with longitudinal fixed effect approaches finding much smaller effects than studies that are able to make use of experiments or other situations where there are exogenous changes in income. This is likely to be related to the difficulty of accurately measuring income changes in the longitudinal studies. It is tempting for researchers to make use of longitudinal survey data to get closer to identifying causal relationships in the absence of experiments, but these findings underline the need to treat the results of these studies with care: they appear to substantially underestimate the true effects of income.

Effect sizes in the studies which make use of experiments or other exogenous income changes range from 5 per cent to 27 per cent for cognitive outcomes and from 9 per cent to 24 per cent for social and behavioural outcomes, with estimates of 14–15 per cent for maternal depression. These are comparable in size to effects calculated for other interventions, including studies of school expenditure and early childhood education programmes – although income appears to operate across a much wider range of outcomes.

These effect sizes suggest that increases in household income would not eliminate differences in outcomes between low-income children and others but could be expected to contribute to substantial reductions in these differences. For example, eliminating the income gap between households where children receive free school meals and those where they do not might be expected to eradicate half the gap in outcomes at Key Stage 2 between FSM and non-FSM children.

6 WHY DOES MONEY MATTER?

The evidence discussed in this report so far points clearly to a causal impact of household income on a range of wider outcomes for children. Studies identify a significant statistical relationship between changes in income and changes in outcomes, using research designs which allow us to be confident that this relationship cannot be explained away by other factors, such as differences in innate ability, parental education or parenting skills. However, a convincing story about a causal relationship also needs a plausible theory about the pathways or mechanisms through which income affects outcomes.

It is beyond the scope of this paper to do this question justice; for one thing, this is a review of quantitative evidence, and quantitative studies are not always the best placed to answer ‘why?’ type questions. (See Strelitz and Lister (2008) for insights from a range of qualitative sources on how low income impacts on families’ lives in the UK.) Nevertheless, in this chapter we draw out what our evidence base tells us about what the pathways might be, placing this evidence in the context of theories from the wider literature. This discussion is particularly important given that most of the studies included in the paper use data from outside the UK, with the majority referring to the US. An understanding of pathways can help to give us a sense of how far findings from other countries are likely to be generalisable. (For a more detailed discussion of the theory of causal pathways between income and health outcomes in particular, we refer the reader to Benzeval *et al.*, forthcoming 2013.)

We begin by discussing the broad theories in the literature, and go on to discuss three types of evidence. First, we summarise briefly what our 34 main studies tell us about the relationship between income and intermediate outcomes such as parenting or maternal depression, although this only

gives us half a picture: the other half is whether these factors in turn affect child outcomes, but as we only look at studies that include a measure of household financial resources this question is not directly covered here. We then examine those of our main studies that estimate the effects of income with and without potential mediators as controls, to see whether this explains part of the relationship. Finally, we consider a set of studies identified in our systematic searches which use a technique called structural equation modelling (SEM). This is a method that involves estimating multiple regressions between variables simultaneously and allows for exploration of the relative importance of different pathways of association. While this method cannot prove that these pathways are causal, it allows for the testing of theories of potential pathways from income to children's outcomes.

Theories of mechanisms

There are two main theories in the literature that seek to explain how financial resources impact on children's outcomes (see, for example, discussion in Gennetian *et al.*, 2009). The 'Investment Theory' refers to the direct effects of financial resources on the physical home environment, through parents' ability to afford certain goods that affect children's outcomes (Mayer, 1997). For example, families constrained by low financial resources have less money to spend on educational toys and learning materials, socially enriching and educational activities such as trips to museums (Magnuson and Duncan, 2002), and more basic needs such as good quality housing and a sufficiently nutritious diet. This theory predicts that as parents' financial resources increase they invest more in their children as they are able to buy more goods.

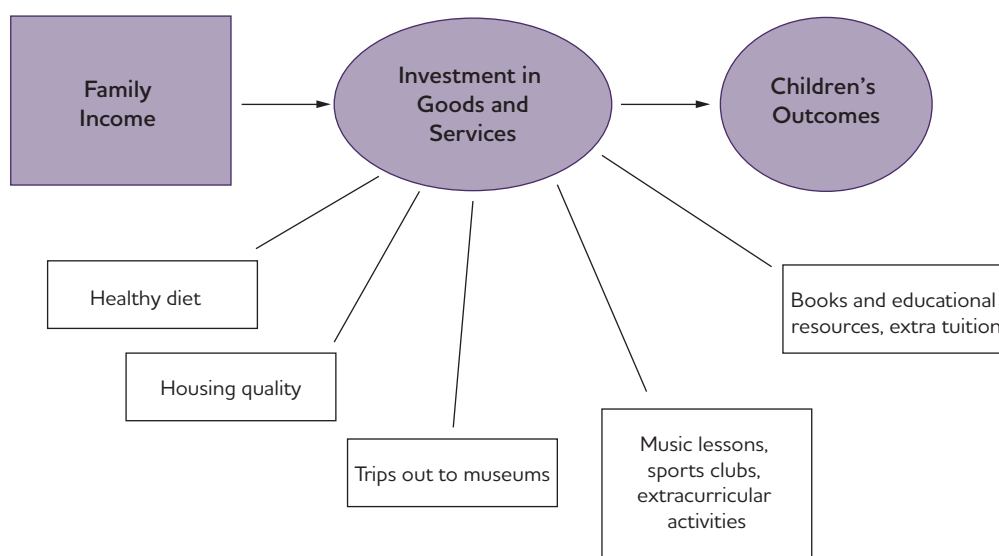
The second theory relates to the impact of economic hardship on the emotional home environment. The premise outlined by the 'Family Stress Model' holds that economic hardship causes economic stress for parents (for instance, from inability to pay bills), and in turn impacts on their parenting abilities (Conger *et al.*, 2000). This model identifies various pathways through which economic stress operates to impede parenting quality, for example through anxiety, depression and other threats to psychological well-being. Economic stress can make parents irritable, frustrated, less patient and lacking in the emotional resources needed for supportive and nurturing parenting behaviours, instead resulting in a more punitive parenting style, for example using more physical punishment rather than reasoning, as well as parenting that is withdrawn and unresponsive (McLoyd, 1990; Magnuson and Duncan, 2002). There is also recognition within this approach that not all families respond to economic hardship in the same way, as there are differences in resilience (Conger *et al.*, 2000) as well as factors that condition the various pathways.

These two models are not mutually exclusive, and some studies discussed below find evidence that the pathways from each interact with each other. It is also likely that each pathway is more important for some outcomes than for others. Some of the literature suggests that family investments may be more important for children's cognitive outcomes, with parental stress and parenting style relatively more significant for behavioural outcomes (Gershoff *et al.*, 2007, p. 73).

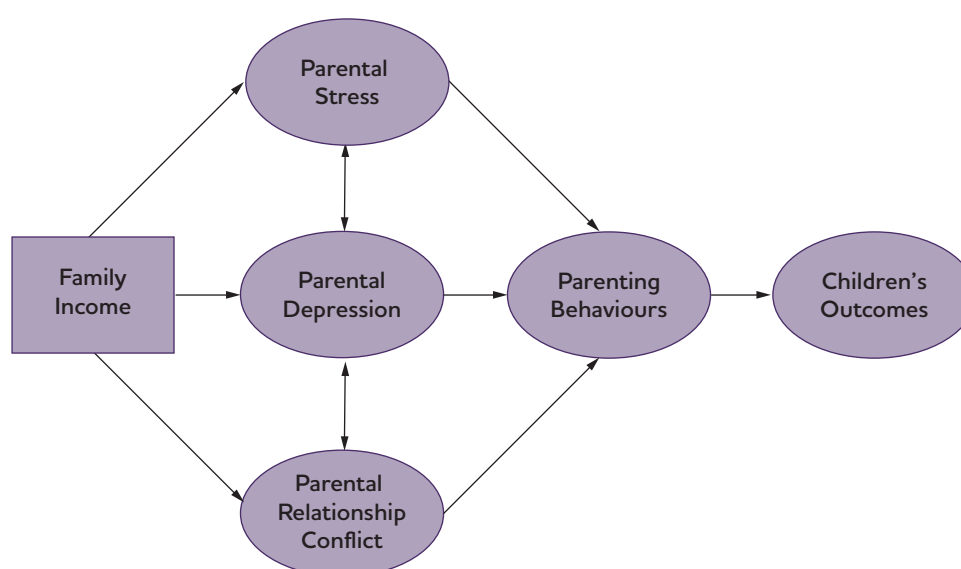
Family investments may be more important for children's cognitive outcomes, with parental stress and parenting style relatively more significant for behavioural outcomes (Gershoff *et al.*, 2007, p. 73).

Figure 2: The Investment Model and the Family Stress Model

The Investment Model



The Family Stress Model



Evidence from our main studies on intermediate outcomes

Several of our main 34 studies examine the relationship between household financial resources and intermediate outcomes, including nutrition and the physical home environment, which can be seen as fitting under the Investment Model; and maternal health and the warmth and responsiveness of parenting, which relate to the Family Stress Model. This evidence has already been discussed above, and the reader may wish to refer back to Table 2 for an overview (and to Appendix 4 for more detail – see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp). Here we summarise very briefly what it tells us, while noting again that this is just half of the story: to be part of a pathway we would also need to be sure that these factors are indeed predictive of children's outcomes.

In relation to evidence for the Family Stress Model, three studies research the impact of income on parenting behaviours, while four investigate the impact of income on maternal mental health. The findings from all these studies provide some support for the Family Stress Model, with evidence of an income effect on parental supervision and positive mother–child interactions (Akee *et al.*, 2010); the psychosocial home environment (Dearing and Taylor, 2007); and maternal depression (Dearing *et al.*, 2004; Gennetian and Miller, 2002; Evans and Garthwaite, 2010; Milligan and Stabile, 2011). On the other hand, Gennetian and Miller (2002) found no income effect for maternal warmth, harsh parenting or supervision.

The evidence in relation to the Investment Model is somewhat more mixed. Three studies consider the impact of income on children’s nutrition. One finds that families receiving cash transfers were significantly less likely not to have enough food to eat, either sometimes or often, than a control group (Riccio *et al.*, 2010). But Milligan and Stabile (2011) find that only boys from lower educational backgrounds were significantly less likely to experience hunger due to lack of money to buy food as incomes rose with child benefit changes, and Gennetian and Miller (2002) found no impact of welfare-to-work programmes on whether or not the family had enough to eat in the previous month.

In terms of the physical home environment and activities beyond the home, Gennetian and Miller (2002) found no significant increase in children’s participation in out-of-school activities as incomes rose, but Dearing and Taylor (2007) found significant improvements in the home environment. Votruba-Drzal (2003) found increased income significantly improved cognitive stimulation at home (a measure which overlaps with the Family Stress Model).

Finally, two studies look at whether expenditure on children’s items increases when families experience a rise in incomes and provide mixed evidence. In the UK, Gregg *et al.* (2006) find significant increases in relative spending on children’s clothing and footwear, toys and books as well as fruit and vegetables, and reduced spending on alcohol and tobacco, as incomes rise for low income families with children. But in a similar study for the US, Kaushal *et al.* (2007) do not find significant increases in spending on children’s items in families benefiting from increased welfare payments, most likely because additional income is being spent on work-related expenses in the US.

Overall, evidence from the studies that look at intermediate outcomes is more consistently supportive of the Family Stress Model, with most studies showing a significant impact of income on maternal depression and parenting behaviours. Findings from the studies that look at intermediate outcomes relating to the Investment Model were more mixed, in relation to children’s nutrition, the home learning environment, and spending on children’s items.

Evidence from studies that test the role of potential mediators as controls

The second way to approach this question is to consider studies that test for the mediating role of particular factors by running regression models with and without these included. Four of our main studies tested mediators in this way, providing some support for both of our two models. In their study of the US casino development, Costello *et al.* (2003) tested a range of mediators including neglect, harsh or inconsistent parenting, overprotective

or intrusive parenting, lax supervision and maternal depression, but find only parental supervision mediated the effect of changes in poverty level (defined as the official US poverty line) and accounted for approximately 77 per cent of the effect of change in poverty status on children's psychiatric symptoms. This provides mixed support for the Family Stress Model as maternal depression and parenting behaviours other than supervision were not found to be mediators. Also for the US, Votruba-Drzal (2006) found that the home environment (measured as an overall score for both cognitive stimulation and emotional support) partially mediated the relationship between income and reading and maths skills, as well as behaviour problems.

Using UK data, Violato *et al.* (2011) test variables relating to both the Family Stress Model and the Investment Model. They find that the impact of income on child behavioural outcomes and cognitive development operated partly through the impact on Investment Model variables (such as housing tenure, safety and quality of neighbourhood and time spent in cognitively stimulating activities) as well as through Family Stress Model variables (measured as parental depression, parental practices, discipline and child–parent relationship), although for both child outcomes the group of Family Investment Model variables had the strongest impact. However, when they examine individual variables separately that make up the constructs of the Family Stress Model and Investment Model, they conclude that investment variables are more important for cognitive outcomes and stress variables more important for behavioural outcomes.⁷

Just one of our main studies tests mediators for health outcomes. Strully *et al.* (2010) find that reduced maternal smoking during pregnancy partly accounts for the impact of increases in Earned Income Tax Credits (EITCs) on birth weight. This is a behavioural mechanism and highlights the fact that there are likely to be other pathways beyond the two models we focus on here. For a fuller discussion of these, see Benzeval *et al.*'s (forthcoming 2013) theoretical review of why money might matter for health.⁸

Evidence from studies using SEM

Finally, we consider the evidence from studies making use of SEM techniques. We identified 22 such studies: two are from the UK, one is from Finland and the rest are from the US. In order to be included the studies had to have a measure of financial resources at the household level, and aim to investigate one or more potential mechanisms between financial resources and children's outcomes (or intermediate outcomes such as parenting).

Although not all the studies reference either of the theories by name, they all test mechanisms related to either the Family Stress Model or the Investment Model. However, none of the studies investigate the Investment Model on its own, and more attention seems to have been given to the Family Stress Model using this technique (14 of the SEM studies test the Family Stress Model alone and the rest consider both models together). As well as receiving less attention, the measures used for the Investment Model are often limited to the physical home environment and cognitively stimulating resources, although some studies also included measures of extracurricular activities and trips outside the home (Gershoff *et al.*, 2007). Some of the measures used for the Investment Model also overlapped with the Family Stress Model in relation to parenting behaviours: for example, measures of cognitive stimulation often referred to parents reading to their child and interacting in a cognitively stimulating way, as well as the presence of cognitively stimulating resources.

Of the 14 studies that test the Family Stress Model, three investigate the relationship between income and parenting behaviours as an outcome, with parental mental health and marital interaction as mechanisms between the two (Leinonen *et al.*, 2002; Evans *et al.*, 2008, Lee *et al.*, 2009) and the rest include measures of parenting as a mediator between family income and children's cognitive and behavioural outcomes, along with other mediators such as maternal depression. All studies provide support for the Family Stress Model, with all variables included as mechanisms showing significant results for the indirect association between financial resources, parental stress or mental health, parenting behaviours and (for those that include it in the model) children's cognitive and social and behavioural outcomes. For a more detailed summary of results from these studies, including differences in pathways and mechanisms included, see Appendix 5 (http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp).

The rest of the studies discussed here test both models simultaneously. All studies find support for both models although evidence on the relative importance of different pathways for different outcomes was mixed. For example, Linver *et al.* (2002) found that as expected measures related to the Investment Model (physical home environment) explained indirect 'effects' of income on children's cognitive outcomes but not social and behavioural outcomes, while measures from the Family Stress Model (maternal mental health and parenting style) explained the relationship between income and social and behavioural problems. Similarly, Altschul (2012) found that only the Investment Model (measured as educational resources and extracurricular instruction) explained educational achievement. However, other studies have found evidence that the models are not restricted to distinct types of outcomes, with measures from the Family Stress Model explaining cognitive outcomes (Guo and Harris, 2000) and the Investment Model also explaining behavioural outcomes (Eamon, 2000; Eamon, 2002). The differences in results are likely to be due to different measures of mechanisms and outcomes used (Appendix 5 provides these details, see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp). Some studies find that variables representing the two models interact (Gershoff *et al.*, 2007; Yeung *et al.*, 2002; Eamon, 2002). For instance, Eamon (2002) found that behavioural problems influence cognitive outcomes, so any pathway that affects the former will also affect the latter. Yeung *et al.* (2002) found Investment Model mechanisms also affected maternal psychological well-being and parenting practices, among other cross-overs between the two models, and conclude that the Investment Model and Family Stress Model should be considered together.

Summary

In sum, it is beyond the scope of this study to provide a comprehensive exploration of the mechanisms through which income affects particular outcomes. But we have examined the evidence from our main studies and from additional studies using SEM in relation to two central theories about possible pathways: the Investment Model and the Family Stress Model.

Evidence from our main studies provides stronger support for the Family Stress Model, with income affecting children's outcomes through parental mental health and parenting behaviours. There is mixed evidence in support of the Investment Model, through which additional income allows families to buy goods and resources which promote their children's development, with some indication that investment mechanisms may be more important

for cognitive development and family stress mechanisms for behavioural outcomes.

Evidence from the SEM studies also supports both the theoretical models, although less attention appears to have been given to the Investment Model and this was not explored by any of the SEM studies on its own. Some of the SEM studies suggest that the material and psychosocial pathways outlined by the two models are not entirely distinct and may interact with each other.

Understanding the mechanisms through which income affects outcomes is particularly important because many of the studies identifying a causal relationship are from outside the UK, largely from the US, raising concerns about how far findings can be generalised to the UK context. In fact, the central mechanisms that emerge as important – parental stress and mental health, parental relationship quality, parenting behaviours, and to a lesser extent investment in educational resources and the physical environment – are likely to be equally relevant to the UK. However, the question of pathways from income to children's wider outcomes deserves much greater consideration than has been possible here and is worthy of exploration in future research, with further insight to be gained from qualitative as well as quantitative research.

7 DOES MONEY MATTER MORE FOR LOW-INCOME HOUSEHOLDS?

We turn now to consider whether income makes more difference to households at the bottom of the distribution than at the top. Intuitively we might expect this to be the case: an extra £1,000 for a family whose annual income is £10,000 for instance, is a much larger proportional increase in their original income than an extra £1,000 for a household with an income of £100,000.

Furthermore, both of the key mechanisms explored in the previous chapter predict a larger impact of the marginal pound in lower income households. If additional income is important because it relieves pressure on parents (the Family Stress Model), we would anticipate that income changes would have more effect in families close to the breadline than in those that are comfortably off.⁹ If the Investment Model is relevant, lower-income households will also be most affected if it is essential goods such as healthy food and sufficient heating which make the difference. The marginal impact of spending on wider goods such as books, toys, computers and educational outings may also fall as spending on them rises.

Both the Family Stress Model and the Investment Model could therefore be taken to imply that what matters most is income *adequacy*. This in turn suggests that there may even be a particular income cut-off point beyond which increases in income make little or no difference (an idea familiar in the poverty measurement; see e.g. Townsend, 1979). On the other hand, what counts as adequacy is likely to depend on average living standards, so if such a cut-off exists it is unlikely to be the same across time and place. In addition, there could be an impact through the Investment Model further up the distribution if income changes are large enough to enable families to

live in areas with better public services, or to support their children through additional years of education.

Many of the studies in our main evidence base that focus on a relatively low-income population identify larger income effects overall than those that look at the whole population, as shown in Tables 3 to 6, although they also happen to use different methodologies too. In this chapter we examine studies that explicitly explore whether income effects are non-linear, meaning that the marginal £1,000 has a greater impact in low-income households. The evidence discussed is from a combination of studies included in the final 34 that satisfied our main criteria (in that they were able to isolate the causal link between income and children's outcomes), and studies that did not satisfy this criteria fully but did include rich controls and provide valuable information about whether or not income has a linear effect. The studies were fairly evenly split with 13 main studies and eight studies retained from our searches specifically because they examined this question. We included these additional studies to widen the evidence base and because the dangers of upward bias caused by hidden confounding factors seem less important when exploring whether the income effect is different for different groups than when examining whether it exists at all. Nevertheless, we indicate which studies are from the main 34 and which are additional. We only include studies that actively explore whether the income effect is non-linear. Many studies include a natural log form for income, implying diminishing returns as income rises, but we only discuss these studies here if they test this assumption by trying out other models. (More detail on the 21 studies discussed in this chapter can be found in Appendix 6, see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp)

Table 7 shows the overall findings by study type. A clear majority of studies, 17 out of 21, found that the effect (or association) of income is greater for those at the lower end of the income distribution. These studies cover our full range of outcomes, including parenting and the home environment and measures of health, social and behavioural development and cognitive and schooling outcomes. Two studies find no evidence that effects were greater in lower-income households and two studies find mixed results (meaning they found non-linear effects for some of the outcomes they looked at, but not others). In order to examine this question more fully we begin by summarising the studies, grouping them by how they test for a non-linear effect of income, and signalling which of these studies were included as main studies. We then consider whether these studies indicate a cut-off point beyond which income ceases to matter at all.

A clear majority of studies, 17 out of 21, found that the effect (or association) of income is greater for those at the lower end of the income distribution.

Table 7: Summary of evidence for a non-linear effect of income

Study type	Evidence of non-linearities	Mixed evidence of non-linearities	No evidence of non-linearities	Total
Main study	11	2		13
Secondary study	6		2	8
Total	17	2	2	21

Studies that conduct analyses by separate income groups (10)

Half of the studies test for non-linear effects of income by dividing the sample into groups based on income level and analysing the effect of income

on children's outcomes for each group separately. The simplest version of this is where studies used the official poverty line as a cut-off to test whether income had a greater effect on those in poverty or not. Others allowed for a more nuanced interpretation of results by dividing the sample into income groups across the distribution. All but one of these studies found clear evidence that effects are larger for lower-income groups, while in one the picture was mixed.

Of the 11 studies that use this method, nine are from our main studies. This includes two from the US natural experiment with distributed casino profits. After dividing the sample based on previous poverty status (measured using the official US poverty line), Akee *et al.* (2010) found there was no effect on outcomes measuring education or crime for households that were not previously in poverty and that once separated by poverty status the coefficient for educational attainment by age 21 triples in size for the younger cohort (those with longest exposure to the increased income), while the effect size for school attendance almost doubles. Using the same natural experiment, Costello *et al.* (2003) divided the sample into never poor, ex-poor and persistently poor households (again using the official US poverty line) and compared differences in outcomes before and after the increased income as well as between groups, finding that extra income for the never poor families had no effect on their psychiatric symptoms, yet there was a significant improvement in psychiatric symptoms for families that were previously in poverty.

Another main study, by Dahl and Lochner (2012), exploited changes in Earned Income Tax Credits (EITCs) in the US to compare outcomes from those that benefited from the EITC changes to those that were unaffected. Those benefiting were from lower-income households, and Dahl and Lochner speculate that this may explain the much larger estimates compared to studies that look at the full distribution. To further explore the issue, they split their low-income sample into three income groups and estimated the effects of the increased income for each group separately. They found that the effect of the increased income on children's education (maths and reading scores) was two to three times larger for the lowest-income quartile (earning less than US\$18,031) than for those earning more than US\$41,790.

Shea (2000) uses the US PSID to explore whether childhood income driven by union differentials or involuntary job loss (and therefore arguably by luck) predicts children's schooling duration and future earnings and income. For the full national representative sample he finds no significant effects, but when he analyses only the PSID low-income sample he finds positive and significant effects for all the wage and income variables, though not the schooling outcome. On the basis of further investigation, Shea concludes that levels of parental education appear to act as a buffer to any income effect; that is, parental income has a greater impact on children's skills in households with lower levels of parental education.

The final four main studies that compared effects by different income groups analysed income change over time within households. Three of these are by Dearing and colleagues. Dearing and Taylor (2007) divided the sample by different income levels to test whether income has a non-linear effect on the quality of children's home environments. They found that while their linear estimates were small, the effect of increased income on home environments was much larger for families with the lowest incomes, particularly for the psychosocial elements of the home environment. For a family on US\$10,000, an increase of US\$10,000 meant 110 per cent of a standard deviation change in learning materials; 124 per cent in parental warmth, and 80 per cent in parental responsiveness. At US\$50,000 changes

were much smaller: 22 per cent for learning materials and 25 per cent for warmth and lack of hostility (see Appendix 4 for details of parenting measures used, http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp).

Dearing, McCartney and Taylor (2006) compared those who were chronically poor (their income-to-needs ratio was below the official poverty line at more than three assessments) to those who were transiently poor (in poverty at two assessments or less) and never poor. They found that although the size of the effect of income on children's behavioural problems was small when constrained to be equal across the children in the sample, the effect of income on externalising problems was significantly larger for chronically poor children: the average estimated decrease in externalising problems from an increase of US\$10,000 was nearly 15 times larger for chronically poor children than for children who were never poor. However, income was not associated with children's internalising problems. In another study, the same authors (Dearing, Taylor and McCartney, 2004) added 'change in poverty status' (again based on families' income-to-needs ratios using the US poverty line) as an extra variable to their analysis of the effect of income on maternal depression. They found that change in poverty status increased the probability of improved depressive symptoms. Women were 1.48 times more likely to have improved symptoms to a non-clinical level after moving out of poverty.

However, Blau (1999) found a less clear story when examining the effect of income on cognitive development, behavioural outcomes and the home environment. For the achievement and vocabulary tests the effects were found to be non-linear; but the largest effects were for the middle and lower middle income groups, not the very lowest, while other outcomes did not show a consistent pattern of diminishing returns for higher income groups.

The remaining three studies that compared different income groups were kept as secondary studies only. Dearing, McCartney and Taylor (2001) divided their sample based on poverty status, again using the official US poverty line, to calculate an income-to-needs ratio; they found that change in income was significantly associated with improved cognitive and behavioural outcomes for children, but only if they were from poor families. Taylor, Dearing and McCartney (2004) also divided their sample based on poverty status (using the US poverty line income-to-needs ratio) when assessing the association between income and cognitive and language development, as well as estimating income associations across different income quintiles. They found that changes in income did have stronger associations for the in-poverty group and the association decreases higher up the income distribution, but the point at which diminishing returns begins differs by outcome. Garrett *et al.* (1994) grouped their sample based on whether or not the child was born into poverty (again measured using an income-to-needs ratio based on the official US poverty line) and found that increased income has the strongest association with the home environment for children who were born into poverty.

Studies that allow the association with income to vary at different points in the distribution (6)

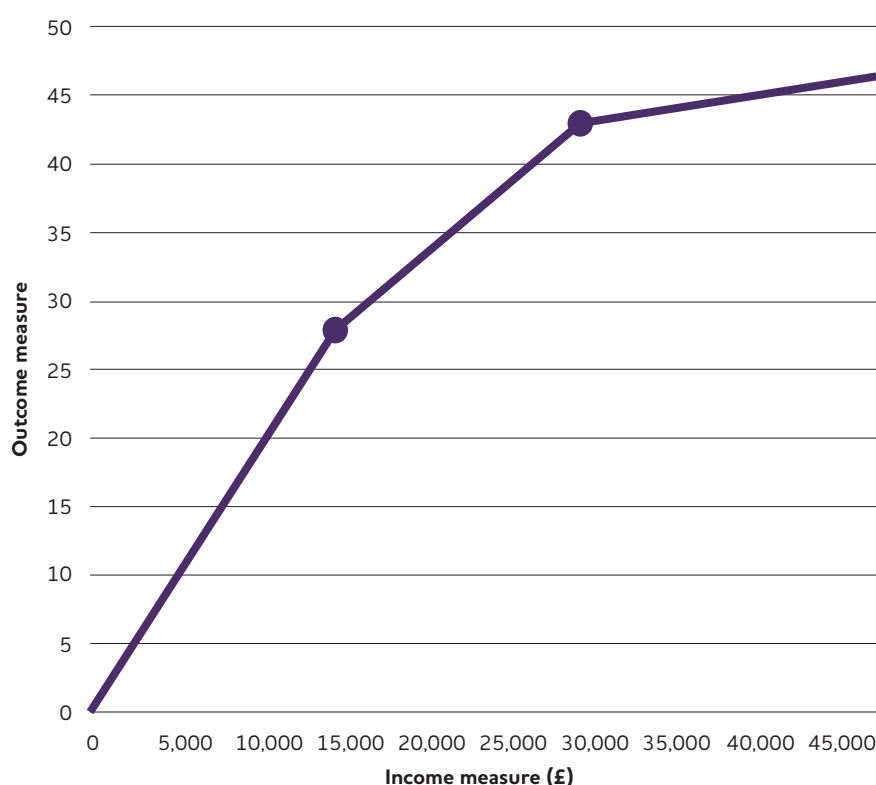
Six of the studies use what is known as a 'spline function': this allows the effect of income to vary either above and below a single point (known as a 'knot'), such as the official poverty line, or at two or more different points in the distribution. Figure 3 shows a hypothetical example, in which knots have been placed at £15,000 and £30,000. In some cases researchers test

different points to place the knots; in other studies a given point is chosen at the offset, often the official poverty line. Either way, the placing of the knots may be considered somewhat arbitrary, but if the relationship is indeed non-linear a spline function should identify this, even if the knot is not in the optimal place.

Only two of the six spline function studies were included as main studies. Johnson and Schoeni (2011) used a three-segment spline and found income has a greater impact on health status for poorer households and no effect on the highest-income households. However, they also found adverse effects on health for the poorest households and although there was a non-linear pattern for reading and maths achievement, this was not statistically significant at conventional levels, making this the second study (along with Blau, 1999) to find a mixed story about non-linearities. In contrast, Duncan *et al.* (1998) found strong evidence of a non-linear income effect for high school completion and additional years of schooling. Using a two-segment spline function with a knot at US\$20,000 increases their estimates almost ten-fold in comparison to a linear model. For children in low-income families a US\$10,000 increase in income is associated with 1.3 years of additional education, while the same amount of income for families with US\$20,000 or more is associated with only 0.13 additional years of education.

For children in low-income families a US\$10,000 increase in income is associated with 1.3 years of additional education, while the same amount of income for families with US\$20,000 or more is associated with only 0.13 additional years of education. (Duncan *et al.*, 1998)

Figure 3: Hypothetical example of a three-segment spline function



Note: In a spline function, the relationship between income and outcome is allowed to have a different slope at different points in the distribution. This figure shows a hypothetical example in which researchers have allowed the slope to vary at £15,000 and £30,000.

Two of the secondary studies also find evidence that the association between income and children's outcomes is non-linear. Isaacs and Magnuson (2011) allowed the income association to vary above or below US\$25,000. Results suggest extra money does have a stronger association with

maths, reading and behavioural outcomes for those with lower incomes. For example, a US\$10,000 increase for families whose income is below US\$25,000 was associated with a 7 per cent increase in the probability of being school ready, compared to less than a 1 per cent increase in school readiness for those with income greater than US\$25,000.

Plug and Vijverberg's (2005) study of the association between income and years of schooling and college graduation for adopted children (to control for genetic ability) uses a spline function to allow the income association to vary at the 20th, 40th, 60th and 80th percentile of the income distribution. They found income has a stronger association with educational outcomes for lower-income households and in fact the association was only statistically significant for the bottom quintile.

The final two secondary studies that use a spline function are also the two studies that find no evidence of a non-linear income association. Aughinbaugh and Gittleman (2003) placed their knot at half the median income in order to examine the association between income and cognitive and behavioural outcomes for children, comparing the US (using the NLSY) and the UK (using the 1958 cohort study). They found no evidence of diminishing returns for either country. When using a measure of permanent income, the difference above and below the knot was never statistically significant although it followed the expected pattern. When using a current income measure, the pattern for the UK data was more commonly in the opposite direction from that expected, with a greater effect at higher income levels. Differences for the US were as expected but statistically insignificant, except for motor and social development, where again the effect was greater at higher levels of income.

Using data from the Fragile Families study, Berger, Paxson and Waldfogel (2009) allowed the income association to differ below the US poverty line, between one and two times the poverty line and more than two times the poverty line. For reasons that are unclear, they found that the association between income and children's language ability was actually strongest for the higher income groups and when all controls are included, the association for low income groups is not statistically significant. For behavioural outcomes the results are sometimes in the expected direction but the differences between the groups are not statistically significant. The authors re-estimated the model using different techniques to test the linearity of the income effect, using a quadratic form of income and also estimating the impact of income separately for each income group, but found similar results.

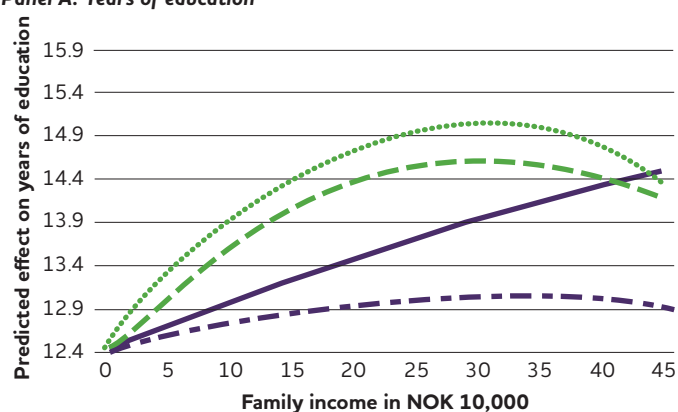
Studies that use a non-linear functional form (3)

The last three studies simply use a non-linear specification of income to allow for non-linear effects. Two of these were included as main studies: Løken *et al.* (2012) re-estimate original results from Løken's (2010) study of the Norwegian oil boom as a natural experiment, this time using a quadratic form of income. The quadratic estimates show a concave relationship between family income and children's educational outcomes, meaning that the impact of income rises more steeply at the lower end of the income distribution and then flattens higher up the distribution. Figure 4, reproduced from Løken *et al.* (2012), shows both the quadratic and (non-significant) linear estimates.¹⁰ Another main study, by Votruba-Drzal (2003), estimated results using both linear and semi-log functions (the semi-log is another form that allows the relationship to flatten as income rises); they found that the non-linear function best describes the impact of income on cognitive stimulation in the home environment, with income making a

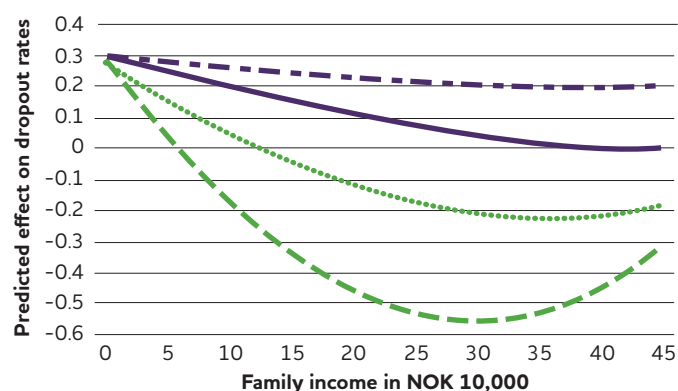
bigger difference to the home environments of poorer households. Figure 5 shows results from the semi-log function: a US\$10,000 increase in annual income for families at the bottom 1 per cent of the income distribution is associated with one-fifth of a standard deviation increase in the HOME cognitive subscale score, while the same amount of money for those with annual incomes at the median level results in a change of one-twentieth of

Figure 4: Linear and quadratic models of the relationship between income and educational outcomes

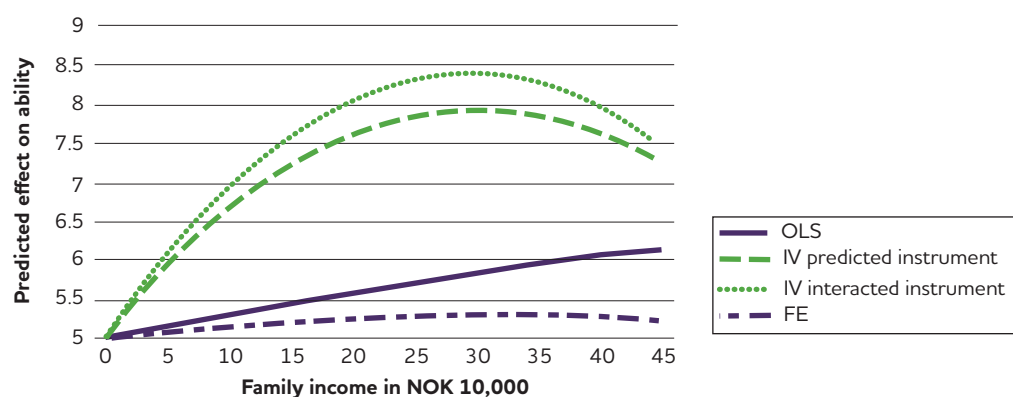
Panel A. Years of education



Panel B. Dropout rates



Panel C. IQ age 18 (males only)

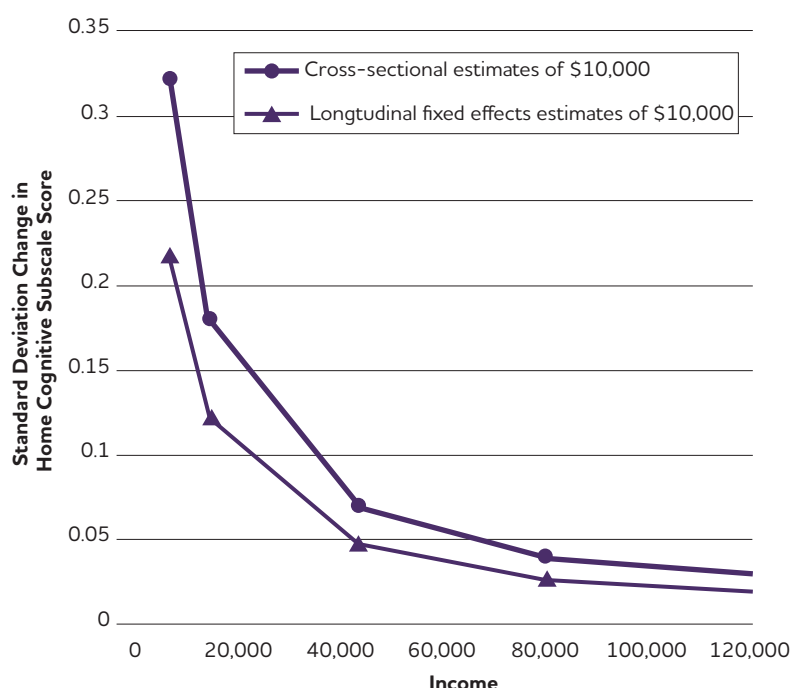


Notes: This figure shows the predicted (total) effects based on the estimates of Model 2. Each graph shows predicted effects from OLS estimates (panel A of Table 4), FE estimates (panel E of Table 4), IV estimates with interacted instruments (panel B of Table 4), and IV estimates with predicted family income instruments (panel C of Table 4).

Source: Reproduced with permission from Figure 1 of Løken et al. (2012, p. 21).

a standard deviation. (Figure 5 also shows cross-sectional results from this study, which are higher, as expected: some of the cross-section association is likely to be explained by unobserved characteristics which the fixed effect results nets out.)

Figure 5: The influence of a US\$10,000 income change on the level of cognitive stimulation provided by children's home environments under semi-log cross-sectional and longitudinal fixed effects models



Note: The slopes of the curves represent the differential sensitivity of children's home environments to income changes. These curves show that the home environments of families with incomes at the lower end of the income distribution are more sensitive to income changes than are those of children at the upper end of the income distribution.

Source: Reproduced with permission from Figure 1 of Votruba-Drzal's paper (2003, p. 351).

Finally, the secondary study by Finch (2003) uses non-linear functions of income when analysing the relationship between income and infant mortality. The steepest slope of the association between income and infant mortality is at the lower end of the income distribution and income above a certain threshold is no longer associated with infant mortality.

Other approaches (1)

A last study that investigates whether there is a non-linear effect is the main study by Black *et al.* (2012), which used childcare subsidies in Norway as an instrument for income. Having found a significant effect of childcare subsidies on children's educational outcomes, they compared effects for children from municipalities with cut-offs of eligibility at lower and higher levels of income and found larger effects for families from municipalities with lower cut-offs, implying that income makes more difference to lower-income families.

Can we identify a cut-off, beyond which income makes noticeably less difference?

Overwhelmingly, the evidence suggests that money does have a bigger impact for children whose family incomes are lower. Only two studies out of 20, both of them secondary studies looking at cognitive and behavioural outcomes, found no evidence of this. Is it then possible to identify a cut-off point at which income makes much less of an impact? And does extra income have any effect at all on those at the highest end of the income distribution? These questions are difficult to answer with the evidence at hand, partly because some of the studies only compare those in poverty with those not in poverty (usually understood as living below the official US poverty line) and so impose a cut-off point. They are unable to shed light on whether this is the relevant turning point, or on whether income effects diminish or disappear further up the distribution. But mainly these questions are difficult to answer because the studies here consider different outcomes, for different age groups, in different contexts, and capturing income in slightly different ways. Outcomes may be affected by different mechanisms and pathways and it is unlikely we would be able to identify a point in the income distribution at which we could expect the effect of income to disappear for a range of children's outcomes, in a range of different contexts. While for the purposes of this report we might be most interested in identifying a cut-off point for the effect of income in the UK, most of the evidence presented is from other countries. In light of these problems we can nevertheless attempt to say something about the point of diminishing returns with the few studies that offer some evidence on this.

Some of the studies do find that there is no effect of additional income for certain income groups, although as mentioned this is mostly crudely defined as those who are above the poverty line. Akee *et al.* (2010) and Costello *et al.* (2003) both found that the additional income from the casino profits natural experiment had no significant effect on education, crime or psychiatric symptoms for children who were not previously living below the poverty line. Similarly Dearing *et al.* (2001) found that change in income is not significantly associated with cognitive and behavioural outcomes for children from non-poor families. Plug and Vijverberg (2005) found that increased income was only significantly associated with educational outcomes for the bottom quintile.

Looking in more detail across the distribution, and focusing on the association between income and infant mortality, Finch (2003) found that the cut-off point and whether or not there is an association with income at the top depend on the exact outcome measure. For endogenous infant mortality (deaths from genetic causes), income ceases to have a significant association once a family reaches just past the median point of the income distribution, but for exogenous infant mortality (deaths from external causes such as infections) the gradient is steeper across the full distribution, suggesting income continues to be associated with infant mortality, although the effect size reduces. Looking at cognitive and behavioural outcomes, Taylor *et al.* (2004) also found that the point at which diminishing returns begins depends upon the type of outcome. For example, for language comprehension it is not until US\$61,000 (the 80th percentile) that an increase in income begins to have a differential association, while for behavioural outcomes these differences start much sooner at US\$17,000 (the 20th percentile).

Summary

Overall then, there is very strong evidence that increases in income have a bigger impact on outcomes for those at the lower end of the income distribution. All 13 of the studies in our main evidence base that asked this question found evidence of a non-linear income effect for at least some outcomes, as did six of our eight secondary studies. Non-linear effects were found across a wide range of outcomes, including health, cognitive and schooling outcomes and measures of social and behavioural development. It is clear that the marginal £1,000 makes more difference to children in lower-income than in higher-income households. This is consistent with what both the main theories in the literature would predict. If additional income is important because it relieves pressure on parents (the Family Stress Model), we would anticipate that income changes would have more effect in families close to the breadline than in those that are comfortably off. If the Investment Model is relevant, lower-income households are also likely to be most affected, as extra income may enable them to increase spending on essential goods such as healthy food and sufficient heating, as well as books, computers and educational outings.

For the question of whether or not there is a cut-off point after which increased income ceases to have any impact at all, the evidence is much more limited and any conclusion must be very tentative. Several of the studies that shed light on this question suggest that additional income has no effect on children's outcomes for households that are not poor, but two secondary studies identify an association high up the distribution for very particular measures of health and learning. It is very possible that large changes in income could affect outcomes higher up the distribution if they enable families to afford, for example, housing in areas where public services are better, but none of our causal studies examine income changes on this scale.

8 DOES MONEY MATTER MOST FOR THE YOUNGEST CHILDREN?

In this chapter we examine what our studies say about whether income matters more at some stages of childhood than others.

In their theoretical review of causal pathways between money and health outcomes across the lifecourse, Benzeval *et al.* (forthcoming 2013) refer to the 'critical period model', which suggests that some stages of the lifecourse, such as gestation and very early childhood, are particularly crucial for child development and therefore low income or related risks at these times will have deeper consequences for future health. The emphasis on early childhood is consistent with recent developments in human capital theory which suggest that, for cognitive development, educational investments may be most effective early in the lifetime of a child (e.g. Cunha and Heckman, 2007; 2008).

The idea that there are particular stages of childhood that are especially important for longer-term development can be separated from what Benzeval *et al.* (forthcoming 2013) call the 'accumulation model', which suggests continued exposure to low income and related risks may have a cumulative negative effect on health (and perhaps on other outcomes), so that disparities between children from households with different income levels widen as they get older. We try to focus here on studies which investigate the first issue rather than the second, asking whether we can identify particular stages in childhood when income levels have a greater impact on children's outcomes than other stages.¹¹ In the next chapter we look at whether long-term exposure to low income has a more severe effect than short-term experience of poverty.

We draw on 16 relevant studies from the systematic searches on children's outcomes. Of these only five are studies that met our full causal criteria. The remaining 11 were kept specifically to help answer this question. They all use longitudinal data and include rich controls. (More detail on all 16

can be found in Appendix 7, see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp)

As Table 8 shows, the majority of the studies – ten out of the 16 – find that the timing of money is important, five find no evidence that money matters more at a particular age than others, and two have mixed results. However, as is clear from Table 9, there is no clear consensus about which stage of childhood is most important. The findings are discussed below in relation to particular outcomes.

There is no clear consensus about which stage of childhood is most important.

Table 8: Evidence for the importance of income at different stages of childhood

Type of study	Evidence timing is important	Mixed evidence that timing is important	No evidence timing is important	Total*
Main studies	4		1	5
Secondary studies	7	1	1	9
Secondary within early childhood (0–4 years)		1	1	2
Total	11	2	3	16

*This refers to the absolute number of studies, not the total findings per outcomes.

Table 9: Evidence for the importance of income at different stages of childhood by children's outcomes

	Timing not important	Mixed results*	Early childhood important	Middle childhood important	Adolescence important	Total**
Health outcomes	1	1				2
Cognitive/ educational outcomes	3 (1)	1	4 (3)	1(1)	2	11
Behavioural outcomes	1			2(1)	1	4
Home environment/ parenting	1			1		2

Number of main studies in brackets.

*Mixed results regarding which age is most important.

**Total number of studies that include each outcome. Some studies include more than one outcome.

Health

Evidence in relation to health is very limited. None of our main studies are informative here. A group of four secondary studies appear on the surface to get at our question: using pooled cross-sectional data for the US or Canada (plus further longitudinal investigations), all four find that the association between contemporary income and health status (measured on a 5-point scale from poor to excellent) is stronger for older children

(Currie and Stabile, 2003; Condcliffe and Link, 2008; Murasko 2008; Allin and Stabile, 2012). But what is being picked up appears at least in part to be the cumulative effect of low income over the child's lifecourse, rather than the greater importance of income during adolescence. These studies were therefore excluded.

A related study by Case *et al.* (2002) does shed light on our question of interest. The authors used longitudinal data from the US Panel Study on Income Dynamics on income from before the children's birth until the most recent measure. They examined whether income at particular periods in the child's earlier life (before birth, 0–3, 4–8 or 9–12 years) has a stronger association with *current* health status (as reported on a scale from excellent to poor by mothers). They found no significant differences and conclude that the timing of low income is not significant for children's health status.

Nikiéma *et al.* (2010) use longitudinal data from the UK (MCS) and Canada (QLSCD) to examine three indicators of health: experience of asthma attacks, occurrence of any longstanding illness and the presence of limiting longstanding illness. Using data from two waves of each survey (the first and fourth years of life), they find that, in the UK, poverty in the first year of life significantly increased the risk of asthma attacks while poverty in the fourth year significantly and more strongly increased the risk for all three indicators of poor health. For the Canadian data, results were not significant in either year (which the authors suggest is due to a much smaller sample size). The study therefore provides mixed or weak support that *during the first five years* later poverty has a bigger impact.

Cognitive outcomes

There is a little more evidence for children's cognitive development than there is for health, but results do not tell a consistent story. Of the 11 studies that consider the timing of income for cognitive and educational outcomes, three of the main studies and one secondary study find early childhood to be most important, one main study finds middle childhood is more significant for cognitive development and two secondary studies find that it is the adolescent period that matters most for children's educational outcomes. We might expect the variation in results to depend on which measures are used; perhaps income or poverty at different stages of childhood matter more for different aspects of cognitive development, and indeed one study finds adolescence or early childhood to be most important depending on the specific outcome. Three studies, including the last main study, find no evidence that the timing of income is more significant at any particular age.

The three main studies that find early childhood to be most important are by Duncan *et al.* (1998), Clark-Kauffman *et al.* (2003) and Votruba-Drzal (2006). Using US panel data (PSID), Duncan *et al.* (1998) re-estimated their original regressions for years of schooling and high school completion on family income, this time by childhood stage, averaging income over 0–5 years, 6–10 years and 11–15 years. Their results suggest that increases in income during the first five years of childhood have a greater effect on the number of years of schooling and the probability of high school graduation. For example, controlling for income at other stages, a US\$10,000 increase in income averaged over the first five years of childhood for children whose income is below US\$20,000 is associated with an increase in 0.81 years of schooling (compared to the insignificant coefficients for middle childhood and adolescence, of 0.45 and 0.32 respectively).

Clark-Kauffman *et al.* (2003) used pooled data for families randomly allocated to 14 different welfare programmes. To test the significance of different ages they used interaction terms of child age group (0–2; 3–5; 6–8; 9–11; 12–15 years) with the experimental dummy variable as well as including dummy variables for age group and type of cognitive test taken. As well as finding that there were only significant effects on cognitive outcomes for programmes that increased income, they found that this was only significant for the two youngest age groups (0–2 and 3–5).

Votruba-Drzal also uses US data (NLSY children) to assess the impact of income on children's maths and reading scores (as well as socio-emotional development discussed later). They estimate change models for income across early (1–6 years) and middle childhood (6–12 years) separately and find that only income during early childhood was significantly associated with change in middle childhood maths and reading skills. A secondary study by Wagmiller *et al.* (2006) supports the results of these main studies in finding that children who were poor early but then moved out of poverty were less likely to graduate from high school than children who were not poor during early childhood but moved into poverty.

The main study by Burnett and Farkas (2009) is the only study that finds middle childhood (age 5–9) to be most important, although this is only compared to adolescence (10–14 years) as test results for children below five years are not included. Again using US data (NLSY children), and adding an interaction between older age group and poverty status to their model, they found that poverty significantly affects maths score but this effect disappears among older children.

Two of the secondary studies find that income is most important during the adolescent period. Haveman *et al.* (1991) used more US data (PSID) to test the importance of income at different stages of childhood for high school completion. The three age categories used were 4–7, 8–11 and 12–15 years. They found that poverty has a significant association with high school completion for the older age group only (12–15 years). This result sits at odds with the later studies by Duncan *et al.* (1998) and Wagmiller *et al.* (2006) who found poverty early in childhood to be most important, using different cohorts of the same dataset, but Haveman *et al.* do not include children under four, and this may account for the different findings.

Najman *et al.* (2009) used Australian data, estimating the impact of income on scores for two cognitive development tests taken at age 14, using income measures taken at specific points in time (pregnancy, 6 months after birth, 5 years and 14 years). Results showed being in poverty at 5 years and 14 years was significant for one of the cognitive measures, and poverty during pregnancy, at 5 years and 14 years was significant for the second measure (poverty at 6 months was not significant for either measure). However, the strongest association for both scores was for poverty at age 14 years.

A study by Guo (1998) highlights the importance of different types of outcomes within the broad category 'cognitive outcomes': they use longitudinal US data (NLSY children) and a dummy variable for early adolescence to distinguish observations for five cognitive tests by the same child from childhood (5–8 years) and early adolescence (11–14 years). Their findings support their original hypotheses that poverty experienced during adolescence (11–14 years) was more important for the cognitive tests that the authors suggest represent 'achievement', but for the tests measuring 'ability', the opposite pattern was observed: poverty during childhood (5–8 years) was more important.

Just three of these ten studies find no evidence that money has a stronger impact at any particular childhood stage. One of these is a main study by Tominey (2010), which used Norwegian administrative data to estimate the effects of income shocks across the life cycle, distinguishing between shocks that are permanent and transitory. Unsurprisingly, Tominey found that the impact of permanent income on educational outcomes (years of schooling and probability of completing high school and college) is bigger at earlier ages (as a permanent income shock in early years affects all future income). However, the results for transitory income shocks were fairly constant across all ages (0–16 years). Humlum (2011) uses longitudinal data from Denmark (PISA), alongside administrative data, to estimate the association of stage specific family income (divided into 0–3, 4–7, 8–11 and 12–15 years) with reading score at age 15. They found no significant association between reading score and poverty at any age. Finally, a study by Allhusen *et al.* (2005) used US longitudinal data to test the impact of being in poverty in early childhood (6 months–3 years), in middle childhood (4.5–9 years), never or both early and late, on cognitive and language skills measured longitudinally from ages 2 to 9 years. They included interactions between poverty group and age and age-squared to test the importance of timing. While they found significant results for behavioural outcomes (discussed later), once full controls were added, the poverty and child age interaction was no longer significant, providing no evidence that timing of poverty is important for cognitive and language skills. (It is worth adding, however, that the controls included some factors that may actually be mediators of poverty rather than confounding variables, such as maternal depression, home enrichment and maternal sensitivity.)

Overall, then, if we look only at the small group of studies that satisfy our causal criteria, the evidence indicates that the early years are most important for cognitive outcomes, as predicted by Cunha and Heckman (2007; 2008). However, if we widen our outlook to include good quality longitudinal studies that may not fully control for confounding factors, the evidence is much more mixed, and there are studies pointing to each stage of childhood as the more important.

If we look only at the small group of studies that satisfy our causal criteria, the evidence indicates that the early years are most important for cognitive outcomes.

Behavioural outcomes

Just four of the studies assessed the importance of timing of income for behavioural outcomes, and of these only one was a main study, previously discussed in relation to cognitive outcomes: while Votruba-Drzal (2006) found that early childhood income (0–6 years) was more important than income in middle childhood for academic achievement, the results for socio-emotional development measured with the Behaviour Problems Index showed that middle childhood income was more strongly associated with a reduction in behaviour problems.

The study by Allhusen *et al.* (2005), also previously mentioned in relation to cognitive outcomes, provides weak evidence that income later (4–9 years) matters more than earlier income (before 3 years). Again the importance of timing of poverty was tested using interaction terms for age and age-squared with poverty group (poor early, late, never or always), this time for externalising and internalising behavioural problems (measured longitudinally between ages 2 and 9 years using the Child Behaviour Checklist from maternal and teacher/caregiver reports). They found that children from families that experienced poverty later had higher internalising and

externalising behaviour problems as reported by mothers than children who were poor early.

Another secondary study by Najman *et al.* (2010) used Australian data to assess the association between income during pregnancy, at 6 months, 5 years and 14 years and aggressive and delinquent behaviour at ages 14 and 21 and smoking and drinking at age 21 years, controlling for income at other stages. They find that only poverty at 14 years independently predicts aggression and delinquency at ages 14 and 21 years. Poverty at 5 and 14 years predicts smoking at age 21, and the authors conclude that poverty during adolescence has the most consistent association, although the association is weak. (Again some of the controls include possible mediators, such as mother's mental health.)

Finally, Duncan *et al.* (1994) used two samples from US data (PSID and data from the Infant Health and Development Program) to test the effect of income at different points *within the first five years* on internalising and externalising behaviour problems and IQ at age 5. The authors used dummy variables to separately analyse the effects for those poor at 1 or 2 years of age only, those poor at 3 or 4 years of age only and those poor at both ages. Results are not reported in the article, but the authors conclude that timing of poverty within the first five years was shown to be unimportant: for all outcomes there were highly significant effects of being poor at both ages 1–2 and 3–4 years, and smaller, roughly equal effects of being poor only early or late.

Overall then, the one study looking at income within the first five years finds no evidence that timing is important for behavioural outcomes, while the three studies that examine a longer timeframe provide weak evidence that income at later ages is more important. Two of the three studies do not include the adolescent age group, but all three studies provide some evidence that income in early childhood matters less where behavioural outcomes are concerned.

Home environment/parenting

Only two secondary studies consider the importance of timing of poverty for parenting and the quality of children's home environment. This includes the study by Allhusen *et al.* (2005), which measured home environment along two main dimensions using age-appropriate versions of the Home Observation for the Measurement of the Environment (HOME) to capture a measure of home enrichment, including the availability of stimulating toys and at later ages learning materials, as well as parental efforts to stimulate the child. They also measure maternal sensitivity through videotaped mother–child interaction. The interaction between poverty group and age was only significant for home enrichment before the full controls were added (again these controls included maternal depression, which might be considered a mediator), and the age interaction was not significant at all for maternal sensitivity, providing no evidence for the importance of a particular stage of childhood.

Miller and Davis (1997) used US data (NLSY children) and average income-to-needs ratio across early and middle childhood, grouping children into: poor early and late; poor early only; poor late only; and never poor. In the final analyses the authors combined the poor early and never poor groups as they found no difference between them. They measured parenting and the home environment similarly using questions from the HOME assessment, distinguishing the cognitive stimulation results (relating to the

amount of stimulating toys and learning materials, educational trips, etc.) and emotional support results (including how the child is disciplined and whether the mother encouraged the child to talk to the interviewer). They found that late poverty was more important for emotional support (and compared to early poverty more important for cognitive stimulation as well, although the worst scores for this aspect of the home environment were for children who were poor both early and late).

Summary

To conclude, the majority of studies found that the timing of income is significant (only three out of 16 studies found no evidence to support this), but the evidence is mixed about which stage of childhood is most important. To some extent, income at particular stages seems to be more important for some types of outcome than others.

In relation to cognitive outcomes, if we look only at the small group of studies that satisfy our causal criteria, the evidence indicates that income in the early years is most important, as predicted by Cunha and Heckman (2007; 2008). However, if we widen our outlook to include good quality longitudinal studies the evidence is much more mixed, and there are studies pointing to each stage of childhood as the more important. One study finds that it depends on the type of outcome, with evidence that earlier childhood matters more for measures of 'ability' and later childhood more for 'achievement' (Guo, 1998).

Where behavioural outcomes are concerned, the story is rather different: only four studies (including one main study) looked at behavioural outcomes using income across childhood but of these, three provide some evidence that income in middle childhood or adolescence is more important than income early on.

Little evidence was found that income at a particular stage is most important for either child health or the home environment, but only two studies were identified in each case and none of them were studies that met our main causal criteria.

9 SHORT-TERM OR PERMANENT INCOME?

The intention in this chapter is to explore what the evidence tells us about whether longer-term, permanent income has a larger impact than short-term or transitory changes. Just as it is important for policy formulation to clarify whether periods of low income have a greater impact at some life stages than at others, so is it key to understand the cumulative effect on children of long-term exposure to limited financial resources. In practice, however, very few of our main studies are able to shed clear light on this question.

We did identify a large number of observational studies which examine the difference between short-term and longer-term experience of poverty. Almost all of these find that longer-term poverty is associated with worse intermediate outcomes, including parenting and the home environment, and with poorer outcomes for children in health, cognitive development and educational attainment, and social and behavioural outcomes. We summarise this evidence briefly below, and provide more details on the relevant studies in Appendix 8 (see http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp), but we caution that it cannot be used to draw conclusions about causality, for two separate reasons. First, it is clear that households that spend longer in poverty may differ from those that only experience short-term poverty in ways that cannot be controlled for in the data. Second, income measured at a single point in time is more vulnerable to measurement error, so longer-term measures may show stronger associations with child outcomes simply because they give us a more accurate picture of income. We think these problems present stronger objections to the use of the secondary, associational studies in relation to this question than to those on non-linearities and timing discussed above. However, five of our main studies address the issue of permanent versus

short-term income and these support the idea that longer-term income has the greater effect.

Outside of our main evidence base, we identified 23 secondary studies exploring the different association between short-term and longer-term income and children's outcomes. Almost all focus on the bottom of the distribution, asking whether a longer duration of poverty (more years in which the household fell below the poverty line – usually understood as the official poverty definition for that country) has more negative associations than a more transitory experience of poverty (usually understood as being poor at the time at which the outcome in question was measured).

Most of these studies (14) are from the US, and using a variety of datasets these tend to find that spending more years in poverty is worse than a short-term experience for a range of outcomes. In early childhood, studies point to a stronger association with longer poverty duration for cognitive and language development; social and behavioural development; emotional well-being; and physical health as rated by mothers. Studies looking at adolescence also find stronger associations for social, behavioural and emotional development; for high school completion; and for health measured by asthma prevalence and by cortisol and cardiovascular response. (See Appendix 8 for references and details – http://sticerd.lse.ac.uk/case/_new/research/money_matters/children.asp)

Four studies look at Australia, three using a single dataset, a prospective longitudinal study which followed babies born in Brisbane between 1981 and 1984 until they were 21. These find that recurrent experiences of family poverty had stronger associations with behavioural problems at age 5, cognitive development at age 14, aggressive or delinquent behaviour at 14 and 21 and alcohol consumption at 21 (Bor *et al.*, 1997; Najman *et al.*, 2009; Najman *et al.*, 2010). The fourth Australian study examines a younger cohort followed in Perth between 1989 and 1991 and followed up to age 14. This finds that children in chronically low income households had a considerably higher risk of asthma than children in households with increasing income, while there was no association between asthma and single point measures of low income. One study from Quebec uses an annual survey following newborns to age 5, and finds that income averaged over all previous years has a stronger association with health in each wave than contemporary income (Lefebvre, 2006).

The three studies on the UK all make use of the first two or three waves of the MCS (to age 5). One of these is only able to look at two waves of data, and finds mixed evidence that poverty at both 9 months and 3 years has worse associations than poverty at either one (Kiernan and Mensah, 2009). But the other two studies make use of three waves, and find that being poor in all three waves has stronger associations with both cognitive and behavioural development than being poor in just one or two, although any experience of poverty predicts lower scores (Kiernan and Mensah, 2011; Holmes and Kiernan, 2013).

However, in addition to Kiernan and Mensah (2009), four other studies indicate exceptions to the general rule that chronic experience of poverty has stronger associations with outcomes than a short-term poverty experience. Three of these used different cohorts of the same dataset, the US NLSY. McLeod and Shanahan (1993) found that while persistent poverty affects internalising behavioural symptoms beyond the effect of current poverty, only current poverty predicted externalising symptoms for 4–8 year olds in 1986. For children aged 6–9 years old in 1992, Miller and Davis (1997) found recent experience of poverty to be associated with deficits in the home environment nearly as largely as those associated with

long-term poverty. In this study, though, it was not current poverty that was contrasted with persistent poverty, but poverty in the most recent 3–4 years with poverty across the child's whole life. Guo (1998) examines cognitive ability and achievement for children aged 3–8 in 1986 and 9–14 in 1990 or 1992 and finds that longer exposure to poverty is not necessarily related to a larger influence, and that what seems to matter most is timing, with poverty in childhood seeming to be more important in forming ability than poverty in adolescence, but poverty in adolescence more important for achievement.

The last exception is a longitudinal study of boys in poor neighbourhoods in Montreal. Pagani *et al.* (1999) finds that persistent financial hardship predicts grade repetition, but that unstable financial hardship (making at least two transitions in and out of poverty between 10 and 16) is most strongly associated with delinquency at 15–16.

These latter studies underline that short-term experience of poverty cannot be dismissed as unimportant. Associations with longer-term duration are clearly stronger overall, which is likely at least in part to reflect selection issues and measurement error in single-year income measures, but could plausibly also reflect a stronger causal effect of a longer-term experience of living in a household on a low income. But there is some evidence that short-term experience also matters. The evidence on very short-term or transitory experience appears to relate in particular to behavioural outcomes, but there is also some suggestion that poverty experienced for a few years can be as important to the home environment and to child cognitive ability and achievement as poverty across a lifetime.

We turn now to consider the evidence of our main studies, although few of these shed light on this question. Among the studies which exploit experiments or exogenous income changes, just three are relevant. In their study of the casino experiment in the US, Akee *et al.* (2010) made the most of different age cohorts and found that those who have experienced additional income for four or six years are significantly more likely to finish high school and significantly less likely to be involved in crime at 16–17 than those experiencing it for two years; other differences between the cohorts are not significant. Examining the Minnesota Family Investment Program, conducted as an RCT, Morris and Gennetian (2003) used two income measures – first year post-random assignment, and average income in first three years post-assignment – and find larger and more significant effects for behaviour problems and school achievement using the three year measure. Finally, Tominey (2010) exploited income shocks to local labour markets in Norway, and found that permanent income shocks had the biggest effect on children's cognitive outcomes, and that shocks experienced early in life had the largest impact.

In general, the fixed effect style studies are unable to average income across years or to look at the impact of duration, because they need to make use of change in income between one year and the next. For example, in their analysis of the UK MCS, Violato *et al.* (2011) used both a measure of transitory income (annual income at each wave) and a measure of permanent income (averaged over all waves), but only in cross-sectional regressions; in these they find larger effects for cognitive and behavioural development of the permanent measure. In their fixed effect models they discard permanent income because it leaves no variation to exploit. However, Blau (1999) makes use of 'grandparent' fixed effects (cases where a mother has a sister who is also a sample member with assessed children), so can examine whether income averaged across a number of years has greater explanatory power over cognitive, social and behavioural development than income in

There is also some suggestion that poverty experienced for a few years can be as important to the home environment and to child cognitive ability and achievement as poverty across a lifetime.

the most recent year. Blau finds the effects of permanent income to be substantially larger than the effects of current income.

Summary

In sum, observational studies indicate that longer-term duration of poverty has stronger associations with child outcomes than a short-term experience of poverty, and there is some limited evidence from our experimental studies that suggests that at least part of this association is causal. However, the observational studies also suggest that even short-term experience of low income may have negative effects, with evidence that unstable income is associated with worse behavioural outcomes. These studies also echo the point made in the previous chapter that timing seems to matter, so experience of poverty in early childhood may be particularly important for some developmental outcomes, and experience of poverty in adolescence for others.

10 DOES THE SOURCE OF INCOME MATTER?

From our systematic searches, we did not identify any studies examining whether or not the source of income is important, in terms of whether it is received from employment or cash benefits. No main studies and no secondary studies focused on this directly.

In practice, many of our main studies look at the impact of changes in benefit generosity, because this provides a clear source of variation which is not driven by hidden household characteristics. Of the 19 main studies that were able to exploit an exogenous source of income variation, 14 focused on benefit changes, with just five identifying a source of variation in income which is not driven by benefits: two casino studies, two studies of the Norwegian oil shock and Tominey's (2010) analysis which makes use of information on income over time in the local travel-to-work area.

Nearly all these studies – 17 out of 19 – find positive effects of income on children's outcomes, with no obvious systematic differences in the size or significance of findings between the studies that look at benefits and the studies that look at other income changes. Nevertheless, a couple of points are worth noting. First, the Kaushal *et al.* (2007) study of spending patterns in the US found that benefit increases for single parents had no effect on spending on children's items, in contrast to the findings of a similar study in the UK (Gregg *et al.*, 2006), and the authors hypothesise that this may be because benefit recipients were required to work in the US but not the UK: the US study found spending rose on items linked to adult employment such as transport and adult clothing. Second, two studies examining a Randomised Controlled Trial of the Minnesota Family Investment Program in the US found that the increased income received by lone parents in the intervention groups had positive effects on children's behaviour and engagement with school engagement, as well as reducing maternal depression and domestic abuse, but that there were no additional gains for the families who gained the income increases but were also required to increase work participation (Gennetian and Miller, 2002; Morris and Gennetian, 2003). Indeed, there

[There are] no obvious systematic differences in the size or significance of findings between the studies that look at benefits and the studies that look at other income changes.

were negative effects on children's social competence and autonomy for the mandatory employment group compared to the income only group (Gennetian and Miller, 2002). There is therefore some evidence that income changes can have less positive effects if they are linked to mandated increases in employment, although any conclusion drawn from just three studies must be very tentative.

The 14 studies that use longitudinal data without a specific cause of exogenous income variation to exploit are likely to be picking up changes as a result of a combination of changes in wages or employment and changes in benefits. These studies found considerably smaller effects than the studies using experimental approaches (most of which are focused on benefit changes), but no conclusions can be drawn from this: as discussed above, it is likely that there are methodological explanations for the difference in effect sizes.

In sum, we found no studies focusing directly on whether the source of income matters for children's outcomes. Studies examining increases in income as a result of benefit changes find positive effects on a range of children's outcomes, as do studies examining other exogenous sources of income change, although there are fewer of these studies. There is some evidence that income changes can have less positive effects on certain outcomes if they are linked to mandated increases in employment, but the evidence base for this is small.

11 DOES IT MATTER WHO RECEIVES THE MONEY?

There was also little evidence among our causal studies regarding the importance of who in the household receives any additional income. Just one of our main studies sheds light on this question.

Akee *et al.* (2010) looked at the US natural experiment in which a new casino distributed profits to adult tribal members in rural Carolina, and finds that the extra income has a more positive impact if mothers rather than fathers are the recipients. When mothers receive the income there is a significant positive effect on children's total years of education and high school graduation rates, but there is no noticeable impact on children's educational outcomes when fathers receive it. The authors argue that mothers may anticipate reaping more benefit from their children in later life, citing Duflo's (2003) study of the impact of pension reforms in South Africa, which found that pensions received by women had a positive impact on the height and weight of girls in the household but not boys, while pensions received by men had no effect. (Duflo's study was excluded from our review because South Africa is not in the OECD.)

If Akee *et al.* are right about the reason that mothers' income has a greater impact in the casino study, it is a finding that will not necessarily translate more widely to societies with different cultural norms. But there may be other reasons that mothers are more likely to spend additional resources on children, notably their role as the main carer. In the UK, Lundberg *et al.* (1997) found that a policy change to Family Allowances in the late 1970s that redistributed resources from husbands to wives was followed by a substantial increase in spending on women's and children's clothing. This study was not picked up by our systematic searches because the abstract did not include terms relating to cause/effect, and indeed the authors do not claim to be identifying a causal relationship. The reform also entailed a renaming of Family Allowances as Child Benefit, and it is possible that labelling of benefits has an impact on how they are spent. This is a point also raised by Kaushal *et al.* (2007) in considering why benefit increases

under Earned Income Tax Credits (EITCs) in the US appear less likely to be spent on children's items than benefit increases under Child Tax Credit in the UK.

Yoong *et al.* (2012) conducted a systematic review on the impact of transferring resources to women versus men in the developing country context, finding cash transfers towards women appear to improve child nutrition and health. Most of the studies they cite do not meet our criteria because they refer to countries beyond the OECD, though they refer to Lundberg *et al.* (1997) as a seminal study, and they include two relevant studies of Progres in Mexico.¹² Both Rubalcava *et al.* (2009) and Davis *et al.* (2002) found that transfers in the hands of women favour spending on children's clothing, while Rubalcava *et al.* (2009) found less clear-cut evidence of an impact on educational expenditures and school enrolment.

In sum, just one of our studies examined whether it matters who within the household receives any additional income. In the US casino experiment, Akee *et al.* (2010) found children's educational outcomes improved if mothers received a profit disbursement, but not if fathers did. This is consistent with other findings from South Africa, Mexico and the UK which were excluded because they were from outside the OECD or were not picked up in our searches.

12 CONCLUSIONS

In conclusion, this review has identified significant effects of household financial resources on wider outcomes for children, including cognitive, social-behavioural and health outcomes, as well as mediating factors such as maternal depression, the home environment and expenditure on children's items. Because we focus only on studies that use credible causal methods we can be confident that unobserved household differences are not responsible for these effects. Money itself makes a difference to children's outcomes.

The evidence relating to cognitive development and school achievement is the clearest and there is the most of it, followed by that on social and behavioural development. Evidence about the impact of income on children's physical health is more mixed, and there were no studies that looked at children's subjective well-being or social inclusion. In relation to intermediate outcomes, the strongest findings were those for maternal depression, where four out of four studies find a causal effect of income.

We also examined the *size* of estimated income effects, asking not just whether money matters but **how much** difference it makes. A key finding here is that the size of identified effects is highly sensitive to the methods used. Using fixed effect techniques on longitudinal data is the most common approach in the literature because of the scarcity of experimental data or good instruments, but these studies appear to seriously underestimate the scale of income effects. Indeed, the extent of downward bias is so large that it raises questions about how useful these methods are to establish either the existence or the size of income effects; there is a danger that their results simply mislead.

Concentrating only on experimental studies or those that exploit other sources of exogenous income variation, effect sizes associated with a US\$1,000 increase in income (around £900 in 2013 prices) ranged from 5 per cent to 27 per cent of a standard deviation for cognitive outcomes, from 9 per cent to 24 per cent for social and behavioural outcomes,

with estimates of 14–15 per cent for maternal depression. At a rough comparison, effect sizes for cognitive and schooling outcomes appear roughly equal in size to the estimated effects of spending similar amounts on school or early education interventions. They suggest that increases in household income would not eliminate differences in outcomes between low-income children and others but could be expected to contribute to substantial reductions in these differences. For example, if income effects can be scaled up, eliminating the gap in average incomes between households where children receive free school meals and those where they do not might be expected to eradicate half the gap in outcomes at Key Stage 2 between FSM and non-FSM children.

Looking to explain **why income matters**, we found evidence in support of two central theories. Several of our studies identified a causal impact of income on mediators related to family stress, including maternal mental health and parenting behaviour; and to a lesser extent on mediators linked to parental investment in goods and services that affect children, such as the physical home environment. While much of the research relates to the US, these mechanisms are likely to be equally applicable in the UK context. However, it should be noted that the report focuses on quantitative evidence, which is not always good at answering ‘why?’ questions. Qualitative research would offer further insight.

We found very clear evidence that income effects are **non-linear**: all of the included studies that addressed this found evidence that income gains have a larger impact in households lower down the income distribution, across a range of outcomes including health, cognitive and schooling outcomes and social and behavioural development. Some of the studies find no impact of additional income on families that are above the official US poverty line, but in others income continues to affect some health and schooling outcomes much higher up the distribution.

Evidence on **whether money matters more at some stages of childhood than others** was mixed. Just five of our included studies look at this issue. All five looked at cognitive outcomes, with the majority indicating that income in early childhood matters most. In the one study also examining behavioural outcomes, in contrast, income in later childhood emerged as more important.

The **duration of low income** appears to matter: many observational studies have found that longer-term experience of poverty has more severe associations with child outcomes than short-term experiences, and evidence from our experimental studies suggests that at least part of this association is causal.

There was much less evidence on our final two questions. None of our studies directly tested whether the **source of income** matters for children’s outcomes. Many of the included studies examined increases in income as a result of benefit changes and found positive effects on a range of children’s outcomes, similar to (the smaller number of) studies examining other exogenous sources of income change. Just one study examined the importance of **who within the household receives additional income**: from their US casino experiment Akee *et al.* (2010) find children’s educational outcomes improved if mothers received the additional money but not if fathers did.

In sum, there is strong evidence that household financial resources are important for children’s outcomes and that this relationship is causal. Poorer children have worse cognitive, social-behavioural and health outcomes in part *because they are poor*, and not just because poverty is correlated with other household and parental characteristics, such as levels of education

Eliminating the gap in average incomes between households where children receive free school meals and those where they do not might be expected to eradicate half the gap in outcomes at Key Stage 2 between FSM and non-FSM children.

or attitudes to parenting. Our calculations have suggested that income increases have effect sizes comparable to those identified for spending on early childhood programmes or education, but it should also be remembered that income influences many different outcomes at the same time. Studies included in this review found effects on a range of different intermediate outcomes including parenting and the physical home environment, maternal depression, and smoking during pregnancy; as well as a range of direct measures of children's well-being and development, including their cognitive ability, achievement and engagement in school, anxiety levels and behaviour. Even small income effects operating across this range of domains are likely to add up to a larger cumulative impact.

The downside of this picture, particularly in the current economic climate, is that *reductions* in household income, and increases in income poverty, are likely to have wide-ranging negative effects. Part of the Coalition Government's deficit reduction strategy is to reduce welfare budgets in order to limit spending cuts to essential public services including education, with a view to protecting children's life chances (e.g. HM Treasury, 2010, p. 5). However well-intentioned, the evidence in this review suggests that this strategy is likely to be self-defeating: rising income poverty will damage the broader home environment in ways that will make it harder for public services to deliver for children.

More research would help to develop this evidence base. Our review has identified a number of gaps in the literature: we found no evidence using causal methods to look at the impact of income on children's subjective well-being and social inclusion, and less evidence on mediating mechanisms, including maternal mental health and parenting practices than on cognitive development. It is also striking how much of the evidence is from the US, with only four studies for the UK included. At the same time, however, we encourage researchers to consider our findings regarding the lower significance levels and smaller effect sizes emerging from the fixed effects models. This approach is tempting in the absence of experimental data or valid instruments, but both researchers and policymakers should be aware that the results of these studies may be misleading.

Even small income effects operating across this range of domains are likely to add up to a larger cumulative impact.

NOTES

- 1 Because of time constraints, we did not search directly for adult outcomes such as future earnings and employment. One study (Shea, 2000) examined schooling outcomes as well as later earnings and we report on both findings.
- 2 This criterion meant we excluded Esther Duflo's examination of the impact of old-age pensions on child health in South Africa (Duflo, 2003), which arguably has greater relevance to the UK than the Oportunidades programme in Mexico. In retrospect this is a shame, but the OECD/EU criterion seemed clear-cut and sensible at the time of searching.
- 3 Where more than one measure was used to test the effect of income on a particular outcome, the study was coded as showing a positive effect if at least one of the measures showed significant positive results. For example, if a study measured cognitive development using both maths and reading tests and results were significant for reading but not maths, the study was coded as finding a positive effect of income on cognitive development. Studies were coded in this way for practical reasons and to make the result summaries more comparable (as some studies include a number of different measures for each outcome and others use only one).
- 4 A growing number of RCT studies of unconditional cash transfer programmes are being conducted in developing countries, but these were excluded because of our decision to restrict our review to evidence from the OECD. See, for example, Oxford Policy Management and Institute of Development Studies (2012) on a safety net programme in Kenya.
- 5 Several of the studies in the natural experiment group also used instrumental variable techniques in analysis. We split the studies by the nature of the data they exploit rather than strictly by the techniques used.
- 6 See www.usinflationcalculator.com and www.oecd.org/std/prices-ppp/
- 7 The models that test mediators are from their cross-sectional analyses.
- 8 Benzeval *et al.* (forthcoming 2013) distinguish between three main types of pathway potentially linking income to health outcomes: material, psychosocial and behavioural.
- 9 In relation to health outcomes, Benzeval *et al.* (forthcoming 2013) discuss the theory that stress can arise from relative social position, meaning that relative income could have a psychosocial effect right up the distribution, as hypothesised, for example, by Siegrist and Marmot (2004) in seeking to explain the fact that there is a gradient in health across the social spectrum, not just a health divide between those who are poor and those who are not. But Benzeval *et al.* note that the strength of evidence supporting this theoretical pathway has been questioned, and in any case it appears more relevant for adult than child outcomes.
- 10 Figure 4 also shows the fixed effects estimates in a 2012 paper by Løken *et al.*, which are much smaller than the results that make use of the oil boom instrument. This is consistent with our findings on effect sizes for different types of study, as discussed in Chapter 5.
- 11 A separate issue is distinguishing between whether income has more effect at a particular stage because that is a crucial developmental period, or simply because it is a period at which money is more relevant for development. Drawing out this distinction would require deeper theoretical work beyond the scope of this review.
- 12 Davis *et al.* (2002) is unpublished so was not picked up in our searches. Rubalcava *et al.* (2009) was picked up but excluded as its abstract did not indicate that it examined whether the transfers had a causal effect on outcomes.

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