DETERMINING CHILDREN’S LIFE CHANCES:
METHODOLOGICAL CHALLENGES AND STRATEGIES
ACKNOWLEDGEMENTS

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Summary

Government is committed to reducing the negative long-term effects of childhood deprivation but surprisingly little is known about processes that produce these effects (Section 1). Advance has been hindered by the complexity of the processes involved, the difficulty of establishing causation, limited interdisciplinary working, lack of high quality longitudinal data and, historically, by a lack of policy interest and adequate funding. This note, the product of a seminar sponsored by the Department for Work and Pensions that was held in London in late January 2003, focuses on ways of addressing the methodological and related obstacles to determining how childhood experiences affect outcomes in adulthood.

The principal methodological challenge is to identify, specify and map the complex web of influences on child outcomes: endowment or innate abilities, family resources and nurturing, and neighbourhood and community environment (Section 2). Insight is lodged (or locked) within many academic disciplines and most evidence is derived from correlation studies that cannot determine causation.

Technical obstacles are legion (Section 3). Comprehensive theory to guide analysis is largely lacking, longitudinal data necessary to establish causality are limited, the measurement of many key variables is crude and unreliable, experimentation is difficult and so far rudimentary, and technical methods for handling temporal processes are underdeveloped. Many of the potential causes are themselves related or associated with other factors that are unmeasured and even unmeasurable, which results in biased estimates of causation.

Three complementary (though not exhaustive) approaches to the conundrum of identifying causality are considered: analytic design; increased specificity in theory and analysis; and more strategic interpretation (Section 4).

Instrumental variable analysis seeks to overcome the problem of bias introduced when variables that are unwittingly or unavoidably omitted from analysis are correlated with ones that are included (Section 4.1.1). The method entails finding a variable (the instrument) that is related to the explanatory variable that is thought to be correlated with an omitted variable, but which is not directly related to the variable to be explained (child outcomes). The undisputed analytic power of the approach is undermined by the difficulty of finding adequate instrumental variables, which may have the effect of limiting the topics amenable to investigation.

In principle, studying siblings allows analysts, to discount genetic inheritance and family circumstances when seeking to establish the impact of other factors on children’s life chances (Section 4.1.2). In practice, while undoubtedly useful, stringent assumptions mean that sibling methods only eliminate family effects in very special circumstances, often limiting the generalisability of results. Where assumptions are of necessity violated, the validity of findings is of course undermined.

The aim of incorporating latent variables in structural equation models is to understand the social world in terms of the combined effect of batteries of variables, rather than to attempt to isolate the influence of individual variables that may be imprecisely measured, (Section 4.1.3). While reducing the susceptibility of explanation to measurement error, the approach limits the specificity of evidence produced to inform policy design. In sum, there is no best method of analysis, knowledge will be advanced through the creative application on a range of methods, singly or in combination.

Explanations of child outcomes are very partial (Section 4.2). There is a need to include a greater range of influences in the explanatory models and to specify the relationships in greater detail, with much more attention being paid to improving the quality of measurement.
Finally, given the sensitivity of research findings to choice of methodology, data source, definition of variables and model specification, sorely needed synthesis must be undertaken with care, prioritising relationships that are pervasive, persistent and underpinned by strong theory (Section 4.3).

A number of measures should be adopted to promote and improve research on child outcomes include (Section 5):

**In order to stimulate information acquisition and exchange:**

A) A Whitehall-based ‘champion’ should be appointed to ensure that knowledge on child outcomes is prioritised, assembled and disseminated, gaps in understanding are identified and appropriate new research is initiated.

B) A ‘knowledge centre’ should be established to identify, review and evaluate theoretical and empirical research on the factors influencing child outcomes (including policy interventions) on a continuing basis.

C) The ‘knowledge centre’ or others should develop conceptual framework(s) to identify gaps in knowledge, develop research priorities, stimulate the systematic accumulation and appraisal of theory and evidence and promote disciplinary and methodological triangulation.

D) Government should consider whether the new centre or any other organisation should have responsibility for coordinating research on child outcomes across government.

**In order to promote data generation and exploitation:**

E) An audit of new research and data collection activities should be establish on an on-going basis to assess their potential (with amendment if necessary) to inform research on child outcomes.

F) Greater emphasis should be placed on prioritising the collection of longitudinal data because of its value in determining causality.

G) Government and other funding organisations should plan for and promote the secondary analysis of survey and administrative data in order to ensure that costly data are fully exploited and to stimulate the development of technical and substantive expertise within the academic and research community.

H) Random assignment in policy evaluation should be prioritised (where appropriate), which, as a secondary consequence, would enhance the scope for causal modelling in secondary analysis.

**In order to improve measurement of child outcomes and causal influences:**

I) Funding should be made available for a review and comparative evaluation of measures of child outcomes and parenting in order to ensure that the best measures are used where appropriate, in all future studies, particularly those funded by Government.

J) The above measures should be routinely included in all one-off and regular surveys except when there are persuasive reasons for not doing so.

K) Researchers should prioritise the development and implementation of good measurement in their negotiations with funding organisations;
L) The time routinely allocated to the design of research and interview schedules should be increased.

M) Multiple measures of child outcomes should be including in surveys whenever possible.

To foster interdisciplinary working
N) Government and other funding organisations should continue to fund strategic research on child outcomes;

O) Government and other funding organisations should encourage and enable tenders from interdisciplinary teams for research on child outcomes;

P) Cross-discipline peer review of research proposals and research output should be increasingly employed;

Q) Funding should be made available for an interdisciplinary seminar series on child outcomes research to stimulate exchange;

R) A programmatic research initiative and/or national research network on child-outcomes, should be established, possibly organised through the ‘knowledge centre’;

S) An international forum or network should be funded to stimulate exchange between researchers working on child outcomes.

To stimulate methodological development
T) The possibility of routinely linking survey data and administrative records and making anonymised data sets available to bona fide researchers (within the boundaries of data protection) should be investigated.

U) Government and other funding organisations should encourage the use of innovative methods of design and analysis in its funded research (which will entail acceptance of the attendant risks)

V) All funding organisations including Government should encourage the dissemination of innovative methodological approaches.
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1 Introduction

Eradicating child poverty and tackling the scarring effects of childhood disadvantage are among the most important of the government’s objectives. Since 1997, numerous policy initiatives have been pursued, new cross-department institutions established to promote holistic and coordinated policy responses, and extensive monitoring undertaken\(^1\). Moreover, there have been significant policy successes with notable falls in the incidence of child poverty and in many of the other measures of social deprivation\(^2\).

However, the government’s aspirations in this area are high and the targets challenging. Moreover, it is inevitable that early successes will prove comparatively easy to achieve compared with the task of assisting children and parents who are multiply disadvantaged, and for whom policy developments have so far proved to be no more than partially successful. It is also true that very little is known about the processes through which deprivations in childhood serve to limit life-chances in later life. Without greater knowledge of the mechanisms involved, it is likely to prove very difficult to focus resources and assistance in ways that maximise the opportunities open to children, young people and their parents or increase the likelihood of them enjoying productive and fulfilling lives.

There are many reasons why social science has yet to provide the knowledge necessary to inform policy development in this area. Some are straightforward and, in principle, easy to remedy. Research was starved of funding over many years and childhood poverty was not a policy priority. Data sources were neglected and opportunities missed. While this reflected ideological preferences at the time, it was exacerbated by the failure of social science adequately to engage with the topic when it was last high on the policy agenda\(^3\). This, in turn, reflects the challenging intellectual nature of the topic itself.

The processes by which experiences in childhood affect later patterns of achievement are inherently very complex and long-term. Understanding them requires longitudinal data covering the breadth of relevant topics that are expensive to collect and take literally a generation to acquire most value. It also demands interdisciplinary working to capture insights from the disciplines that historically have focused on only part of the picture of childhood and its consequences. Most challenging of all, and the central topic of this short report, the questions to which policy makers require answers are inherently causal. This means going beyond observing that children brought up in low income households experience lower life-chances to establishing whether low income per se is a cause of social scarring, independently or in concert with other factors and, if so, how opportunities are curtailed by lack of income.

The report is based on transcripts of a seminar held in London on 27th January 2003 that brought together an interdisciplinary group of expert analysts. Discussion was stimulated by six articles that had recently been published or were nearing completion\(^4\).

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\(^1\) DWP, 2001  
\(^2\) DWP, 2002  
\(^3\) Deacon, 2002  
\(^4\) Blanden et al., 2003; Ermisch and Francesconi, 2001; Garces et al, 2000; Gershoff et al. 2003; Hobcraft 2003; Magnuson and McGroder, 2002
There was no expectation that the seminar would provide immediate answers to substantive questions or that methodological difficulties would be readily resolved. Rather the aim was to alert the policy community to some of the analytic challenges that lie ahead and to stimulate thought about how best to promote intellectual advance in this difficult area.
The core conundrum

A plethora of childhood experiences is likely to affect a child's progress through life and into adulthood, not to mention the child’s endowment or innate characteristics inherited in part directly from their biological parents. Some of these influences are likely to be direct: well educated parents, other things being equal, will probably provide a more intellectually stimulating home-life than parents who left school early. Other influences will be indirect: better-educated parents may have higher than average incomes and hence be able to finance educational excursions, or in other ways to provide life enhancing experiences for their offspring. Some of these additional opportunities will be cumulative, reinforcing other positive characteristics, while others may serve to compensate for some forms of disadvantage.

Certain characteristics may be necessary and sufficient to ensure children's economic or social success; others may be essential but insufficient by themselves; and yet others will be of less significance or, indeed, irrelevant. Important from the perspective of government, some aspects of childhood experience may be amenable to influence by public policy whereas others will not be. Government will therefore wish to identify and primarily target those factors that are both important in shaping a child's life chances and upon which it can exert a beneficial influence.

**Figure 1** A model of intergenerational continuities of disadvantage

![Diagram of intergenerational continuities of disadvantage](image)

Source: Such and Walker (2002)

The conundrum is how to identify and plot this web of potential influences and to separate the important from the consequential. Figure 1 draws attention to the way that a person’s life trajectory – the arrow – interacts with the range of environmental factors (family, community and society) in a process of recursive determination: one set of factors may influence another that may later affect the first\(^5\). Figure 2, repeats the three-fold hierarchy of influences, individual, family and community, but sketches in some of the 'known' linkages between poverty and measures of child outcomes based largely on correlation studies in the US. It also identifies points of possible policy intervention.

\(^5\) Gershuny, 1998
Figure 2 Schematic model of the processes by which income and financial hardship may affect child outcomes. Source: Gershoff, Aber and Raver (2003)

**Federal-, State-, and Community-Level Policy and Program Interventions**

**Parent- and Family-Level Predictors of Income And Hardship**
- Parent Work Status
  - Job Prestige
- Education Level
- Parent Marital Status
- Race-Ethnicity

**Family Income Poverty**
- Parent-Directed Human Capital Enhancement
- Parent-Directed Income Support
- Parent-Directed In-Kind Support
- Parent-Directed Parenting Interventions
- Two-Generation Interventions
- Child-Directed Interventions

**Financial Hardship**

**Parent Distress**

**Parent Investment**

**Parent Behaviour**

**Child Physical Development**

**Child Cognitive Development**

**Child Social-Emotional Development**

**Neighbourhood- and Community-Level Influences**
Children growing up in low-income families are at risk of a range of detrimental physical outcomes, deficiencies and delays in cognitive development and negative social and emotional adjustment. In the US, as in Britain, being born into a family experiencing financial hardship is associated with an increased risk of low birth weight and peri-natal mortality. Children who survive are disproportionately likely to be in bad health and to have low height-for-age and low weight-for-height ratios, and are four times more likely than their more affluent peers to suffer lead poisoning. Children in low income households are likely to have relatively low IQ scores at age 5 (even after controlling for IQ at age 3), and those who live in households with persistently low incomes fare particularly badly. In terms of social adjustment, US children from low income families are more likely to experience emotional problems, to suffer depression and to engage in anti-social behaviour.

As already noted, these studies are based on correlation studies. While the above relationships are thought to be causal, some or all of the associations with childhood poverty could be explicable in terms of other unobserved variables such as parents’ level of education, parenting ability, family structure or genetic inheritance. Equally, though, some of these other factors could themselves be a direct or indirect consequence of poverty. Nevertheless, there is evidence that much of the effect of low income on children is mediated by their parents, and perhaps, to a lesser extent through the neighbourhood.

Numerous US studies have shown, using the HOME inventory, that low-income parents provide less cognitive stimulation. Low income parents are also more likely to be stressed, partly because of financial pressures and sometimes associated marital conflict, and then to withdraw from, or become hostile towards, their children. This, in turn, can lead to negative parenting behaviour including more frequent use of corporal punishment which has been shown to be associated with negative child outcomes. On the other hand, positive parenting strategies can mitigate the worst consequences of low income, while child outcomes can also be shaped by neighbourhood characteristics independently of family factors.

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6 Brooks-Gunn and Duncan, 1997  
7 Ibid.  
9 Shonkoff and Phillips, 2000  
10 Miech et al., 1999  
11 Takeuchi, Williams and Adair, 1991  
12 Gershoff, Aber and Raver, 2003  
13 Mayer, 1997; Reiss, 2000  
14 Duncan et al., 1998  
15 Gershoff, Aber and Raver, 2003; Brynner et al., 2000; Kiernan, 1996  
16 Milar and Davis, 1997; Guo and Harris, 2000  
17 Conger et al., 1994  
18 Dix, Gershoff and Miller, 2001  
19 Pinderhughes et al., 2000  
20 Gershoff, 2002  
21 Rutter, 1990  
22 Ludwig, Duncan and Pinkston, 2000; Schoon et al., in press
Based primarily on US research, Figure 2 presents schematically the most likely pattern of relationships on child outcomes. The categories of influence and effect are very broad and their causal nature has yet to be satisfactorily established.
3 The technical challenge

The principal technical challenge is that of identifying the existence and true importance of the causal links between childhood disadvantage and negative outcomes. Some of the main obstacles to achieving this goal are now outlined.

First, relevant theory is scattered across disparate disciplines including developmental psychology, social psychology, sociology, health studies, epidemiology, child studies, economics and social work. Analyses are therefore partial, focusing on variables familiar within disciplines and conducted without reference to any general, comprehensive theory that might allow the prioritisation of influences. Consequently, there is both a need for strong theory to assist in winnowing the myriad of possible influences, and a need to avoid the disciplinary hegemony that excludes variables simply because they have not traditionally been of interest.

Secondly, many of potential causative influences are themselves related. In the absence of theoretical priority, it is very difficult empirically to assess the true association between child outcomes and a chosen potential cause. To take an example, already noted and also discussed below, parental income and education are correlated which makes it difficult to determine the degree to which each contributes to positive child outcomes (and hence which should be given priority in policy terms). Multi-collinearity, the technical term for this phenomenon, results in imprecise estimation of the model coefficients, meaning that the relative importance of casual factors cannot be accurately established.

A related problem, thirdly, is that a potential causative element and a child outcome may both be related to variables not included in the analysis. Unobserved heterogeneity, as this phenomenon is termed, may make it appear that a causal relationship exists when in fact it does not, or in other ways distort an explanatory model by apparently inflating or reducing the observed effect compared with the true one. One approach to this problem is to include large numbers of variables in the analyses in the hope of capturing all that matters, but retaining insignificant terms marginally attenuates the more important associations and introduces ‘clutter’ into explanatory models, making them complex to interpret and to elucidate verbally. Three other approaches to addressing this problem - latent variable analysis, instrumental variable analysis and the analysis of sibling pairs - are discussed in Section 4 below but none provides a flawless solution.

Fourthly, longitudinal data are required in which information on potential causes is obtained for earlier time periods than those relating to outcomes since a necessary, though not definitive characteristic of a cause, is that it occurs before the effect. This cannot be established with traditional cross-sectional data. However, longitudinal data are often expensive to gather, present unique problems of attrition when, for example, respondents do not participate in every wave of a repeated (panel) survey,

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23 Adding extra variables reduces the variance in the residuals of a regression equation but bias will not always be reduced since it also dependent on the covariance.
and take time to become of value: a child has to attain adulthood before adult outcomes can be measured.

Expense means that panel and cohort surveys tend to be multi-purpose, to cover a large range of topics but in limited depth, and to have comparatively small sample sizes that limit analysis, especially for sub-groups. The period between waves of data collection is usually a minimum of a year and in the UK birth cohorts is much longer. (Selected data are sometimes collected retrospectively for shorter time-periods although this, in turn, has often proved problematic creating disjunctions in the time series – so-called ‘seam effects’ – resulting from respondents’ inability to recall distant events with the same precision as more recent ones.) The designers of longitudinal projects are required to be blessed with perfect foresight since many key explanatory variables - parental interest for example - cannot be gathered retrospectively. In reality, given the range of possible antecedents of poor child outcomes, it is inevitable that many influences simply go unmeasured rather than being intrinsically immeasurable. Also, the objectives of studies tend to evolve over time; for example, the original health focus of the UK birth cohorts was later complemented by an interest in social and economic determinants and outcomes.

Fifthly, some people hold to the view that, in addition to time series data, experimental evidence is required to establish causality. The logic of the classic experiment is that if, after effecting a change in variable A - and only in variable A - in time t, a change is observed in variable B in time t+1, then A is a cause of B. The practical challenge is to avoid contamination by ensuring that nothing changes that could affect B except A. In social experiments, this is almost impossible to achieve and instead the preferred method is randomly to assign people into each of two groups: members of the one group are made to experience a change in variable A, while A is held constant for members of the other. The logic of this approach is that changes in variables other than A will be experienced randomly by both groups. Provided the groups are large, the effect of these random changes will be the same in each group allowing any differences observed in the outcome variable B to be interpreted as the direct result of the change to variable A. In practice, it is quite difficult to ensure that the randomisation is properly achieved and that the experiences of the two groups are equivalent except for the change in A.

A further obstacle is the comparatively limited development of analytic devices for handling networks of influences over time. Seeking to explain an outcome at the age of 23 based on measures taken at age 11 or 16 is at best a gross simplification. The reality is that people's lives define trajectories across time that can be envisaged to relate to a large number of different dimensions of well being or experience - employment, income, health, social inclusion etc. There is a need for data at more frequent intervals in order to be able to plot these trajectories. However, this is not in itself enough since trajectories have proved difficult even descriptively to characterise and group. The difficulty arises from the fact that each point on the trajectory is likely to be related to each earlier point, possibly to a degree that is inversely

24 Walker, 1995
25 Greenberg and Schroder, 1997; Walker 2003
26 Stafford, 2002; Stafford, Greenberg and Davis, 2002
27 Sherer, 2001; Johnson, 2002
proportional to the time lapsed between them. Moreover, the direction of a person’s trajectory between two time points is likely to result from a ‘recursive interaction’ with trajectories in other domains and, indeed, between the hierarchies of influence (the individual, family and neighbourhood) identified above.

A related challenge concerns what might be called ‘external’ time. An individual’s life trajectory starts at birth (conception or possibly even earlier) with time being measured with respect to this origin. However, individual trajectories are embedded in external or historical time, indexed colloquially by terms such as ‘baby-boomers’ or the ‘sixties generation’. It remains largely unknown to what extent time specific cultural traits and behaviours uniquely shape child outcomes, although belief that they do so is common in popular discourse, with concerns about the decline of the traditional family and good parenting, and the supposed negative effects of the growth of individualism and consumerism.

As important as determining whether changes in the broader social and cultural environment affect children's life chances, is the realisation that current analysis of adult outcomes is based on experiences of childhood that were forged in a social environment that was arguably very different from that obtaining today, and that the patterns of causality may have changed. Similarly, it is important to explore whether the influence of the family and other environmental factors varies according to the age of the child. For example, there is some US evidence that, in the short term, a lone mother working is more likely to result in negative child outcomes for teenage than for pre-school children\(^{28}\) and that family break-up is also more disruptive for teenage offspring\(^{29}\). Therefore, it is at least possible that causal mechanisms are mediated by the interaction between internal and external time. It follows that, when possible, analyses should be repeated with different cohorts to explore the extent to which relationships are time specific.

The nature of these technical difficulties counsels against assuming that they are likely to be readily or rapidly resolved, and suggests that understanding of the factors that shape children's life-time experiences will remain fragmentary and probably contested for some while to come.

\(^{28}\) Gennetian et al., 2002
\(^{29}\) Hill, Hill and Walker, 1998
4 Tackling the obstacles

There are three broad strategies to address the challenges sketched out above: through design; by greater specificity; and via interpretation. Each is considered in turn but, in practice, all three will need to be followed in order to begin to make serious progress in mapping the influences on child outcomes.

4.1 Analytic strategies

Correlation studies, even those making use of longitudinal data, have severely limited potential and are best viewed as reconnaissance tools to be used in conjunction with theory to generate hypotheses. Explanation requires other approaches of which three are discussed in this section: instrumental variable analysis, sibling studies and structural modelling with latent variables. It should be stressed that these approaches are neither exhaustive nor necessarily mutually exclusive.

4.1.1 Instrumental variables

Instrumental variable analysis, a specific development of structural equation modelling, provides an approach specifically designed to overcome the bias that can be introduced when important variables are omitted from analyses and, as such, offers a way of approximating a causal analysis\(^\text{30}\). The core analytic problem occurs when the child outcome to be explained is caused by a variable missing from the analysis that is also a cause of an explanatory variable that is included. The result is to attribute spuriously some or all of the explanation to the included variable when the excluded variable is the true cause. Instrumental variable analysis entails defining a variable that is a cause of the included explanatory variable but that, by design (that is, in theory), is not a direct cause of the child outcome (nor is correlated with the error term included in the model equation). The instrumental variable then substitutes for the original explanatory variable in the model. Rather than offering a comprehensive critique of instrumental variable analysis\(^\text{31}\), its use is illustrated and certain generic limitations identified.

Magnuson and McGroder’s 2003 study\(^\text{32}\) of the effect of increasing mother’s education on the academic performance of their children provides a particularly useful case-study of the application of instrument variable analysis because they also seek to exploit the power of experimentation (Exhibit 1).

Previous studies have been unable to separate the effect of maternal education on children’s development from the multitude of potentially confounding factors such as higher incomes and genetically endowed cognitive ability\(^\text{33}\). Magnuson and McGroder capitalise on a policy experiment in which mothers in receipt of welfare benefit were randomly assigned to a programme providing additional education and mandated to attend\(^\text{34}\). Because the assignment to education was, in this case,

\(^{30}\) Angrist, Imbens and Rubin, 1996
\(^{31}\) See Bartels (1997) for a critique of instrumental variable analysis.
\(^{32}\) Magnuson and McGroder, 2003
\(^{33}\) Resnick, Corley and Robinson, 1997
\(^{34}\) Heckman, 1996
unrelated to innate ability or to any other personal characteristic of the mother (since
the assignment was random), any association detected between child outcomes and
this measure of education provides an unbiased estimate of the impact of additional
maternal education on child outcomes.

However, Magnuson and McGroder wanted to establish how much education was
required to make a difference and focused on the duration of additional education
received. Given that participants remained in the programme until their skills
improved (or they found employment), the length of education was not randomly
determined but arguably related to participants’ ability and/or motivation. In this
case, one would have expected a traditional least squares regression analysis to
understate the importance of education on child outcomes, since the parents who
received additional education for the longest periods would probably have included
those who were most educationally disadvantaged or least well motivated. By
including an instrumental variable, defined as in Exhibit 1 to measure additional
maternal education independent of mothers’ baseline characteristics, they found that
the educational programme significantly increased offspring’s readiness for school,
measured two years after assignment into the programme (when the children’s age
averaged three and a half) and reduced the academic problems reported. Moreover, as
anticipated, inclusion of the instrumental variable increased the strength of the
association when compared to a least squares regression.

The Magnuson and McGroder study illustrates the difficulties that attach to
instrumental variable analysis. The instrumental variable must index the explanatory
concept of interest but be unrelated to other possible explanations or to other variables
not included in the analysis (that is, it must be uncorrelated with the error term
included in the equation model). Given the web of causation and influence that
shapes the social world, instrumental variables are likely to be rare and tax the
analyst’s ingenuity in finding and defining them. In the Magnuson and McGroder
Exhibit 1  Using instrumental variables for identifying the effect of maternal education on child outcomes

Our OLS model, the kind traditionally used in this type of research, is presented below:

1  \[ \text{Child’s school outcome}_i = \lambda_0 + \lambda_1 (\text{Mother’s number of months in educational activity}_i) + \lambda_2 (\text{Employment}_i) + \lambda_3 (\text{Baseline maternal characteristics}_i) + \lambda_4 (\text{Baseline child characteristics}_i) + \xi_{i1} \]

The coefficient of interest is \( \lambda_1 \), which represents the increase in a child’s academic school readiness score associated with an additional month of a mother’s participation in an educational activity. The OLS model provides unbiased estimates of coefficients under the assumption that the error term \( (\xi_{i1}) \) is not correlated with the independent variables. Unfortunately, omitting a variable that is correlated with both the dependent and an independent variable causes spurious correlation between the error term and the independent variable, and this may bias the OLS coefficient. …For example, most studies of the effect of education lack a good measure of the mother’s genetically-endowed cognitive ability. By omitting this variable, researcher’s may mistakenly attribute positive children’s outcomes to maternal schooling rather than to mother’s personal endowments, thereby overestimating the effect of maternal schooling on children’s school outcomes…

An instrumental variable (IV) approach provides a way to estimate our model without omitted variable bias\textsuperscript{35}. IV estimation amounts to estimating a two-equation system:

2  \[ \text{Mother’s number of months in educational activity}_i = \beta_0 + \beta_1 (\text{Experimental status}_i) + \beta_2 (\text{Baseline maternal characteristics}_i) + \beta_3 (\text{Baseline child characteristics}_i) + \xi_{i2} \]

3  \[ \text{Child’s school outcome}_i = \Phi_0 + \Phi_1 (\text{Predicted mother’s number of months in educational activity}_i) + \Phi_2 (\text{Employment}_i) + \Phi_3 (\text{Baseline maternal characteristics}_i) + \Phi_4 (\text{Baseline child characteristics}_i) + \xi_{i3} \]

In the first stage of estimation, the cumulative number of months a mother is in educational activities is the dependent variable and is predicted by our set of baseline covariates plus the experimental status of the mother. Because experimental treatment is, by design, unrelated to mothers’ characteristics at baseline, the measure of predicted months in education is purged of any correlation with unobserved maternal characteristics, and is also purged of any spurious correlation with the error term in equation (3). In the second stage, \( \Phi_1 \) is estimated by replacing the actual number of months in education gain with the predicted number of months in education obtained in the first stage (equation 2).


\textsuperscript{35} Foster and McLanahan, 1996
example, extra education could have affected child outcomes through its effect on mother’s employment and family income as well as through the intrinsic pathways of interest by mothers providing a more conducive learning environment. However, because the intervention the authors were utilizing also included random assignment into an employment without education route, they were able to construct a second instrumental variable for employment that was unrelated to education and mother’s characteristics. In addition, because extra income was deducted dollar for dollar from welfare benefit, education had no affect on family income (at least while mothers remained on benefit).

Even so, despite these unique circumstances, it is not clear that Magnuson and McGroder were entirely successful in creating the perfect instrumental variable. While allocation to the policy treatment groups was random, as already noted duration of education was not. One reason why duration of education was used was that continuous variables are preferable as instrumental variables to categorical ones. The effect of imperfect instruments is generally to substitute inefficient with inconsistent estimation. \(^{36}\)

Moreover, Magnuson and McGroder were unable to control for all alternative pathways of influence and mediating factors. They had no data, for instance, on the history of childcare used (only a point in time measure which proved to be insignificant). The formal childcare used when mothers were in education classes might in itself have provided a stimulated learning environment for their children while informal childcare could have had the opposite effect (although two years after assignment mothers in the control group were using as much formal childcare as participants). It is possible that the mothers assigned to education uniquely received other services by caseworkers. It may be that education resulted additionally in a change in household structure. Magnuson and McGroder’s results might also have been distorted by the effect of financial sanctions for non-compliance. These could have reintroduced an association between education and family income – with income being lower among mothers with short spells of education due to sanctioning.

Finally, the stringent criteria for the definition of instrumental variables mean that researchers may not have good measures of the underlying concepts. Magnuson were able to measure the impact of marginal, remedial education not educational history, and still less educational achievement. Within the constraints of their study, they would have preferred to measure the increase in cognitive achievement or social performance attributable to the additional education, but the study only collected comprehensive information on skill levels for mothers in the educational programme rather than in the employment and control groups. Their measure of children’s educational problems was based on parental rather than teacher reports that are generally thought to be more accurate but which were only available for about half of the sample. \(^{37}\)

In sum, instrumental variable analysis provides a set of powerful techniques that theoretically allow the modelling of causation. In the Magnuson and McGroder

\(^{36}\) Bartels, 1997

\(^{37}\) The potential problem that parental reports are potentially related to mothers’ cognitive abilities is circumvented by using random assignment to education to construct an instrumental variable.
example, the effect of additional maternal education on child outcomes was isolated such that the result could not be biased by the effect of either the child’s cognitive ability nor that of the mother on measured outcomes. There is also a degree of symbiosis between social experimentation and instrumental variable analysis. As already noted, experimentation can be a powerful way of isolating the impact of single variable – for example a new policy. A bi-product is that the random assignment generates random variables that can in turn be used as instruments to explore other causal processes.

However, the value of instrumental variable analysis is likely to be constrained by practicalities. It helps only marginally where complex networks of causation are suspected, while the specificity involved in identifying and defining instrumental variables is likely to limit the generalisability of findings. In part, these concerns are discipline specific. Where, as in economics, general principles are thought to hold, instrumental variable analysis provides a potent technique by which empirical observation can be used to refute (or add support to) hypotheses derived from general theory. In other social sciences, where theory development is largely inductive and greater attention is paid to the particular, its role is likely to be limited.

4.1.2 Sibling studies

Another approach to reducing unobserved heterogeneity and the resultant bias in the measurement of presumed causal associations is to compare the experiences of siblings. The Magnuson and McGroder study, for example, was unable to identify and account for aspects of the mother-child relationship that might have mediated the effect of additional education\(^{38}\). Equally, they were unable to take account of the prior cognitive ability of the child, which would have been partially inherited from the mother (child outcomes were assessed only once, two years after assignment to the intervention). By comparing siblings who share the same inheritance, it is possible to take some account of differences in genetic and home background.

Garces, Thomas and Currie\(^{39}\) were interested in the long-term outcomes of the US Head Start, a public pre-school programme for disadvantaged children. No experimental data were available, and use had to be made of the longstanding US Panel Study of Income Dynamics. However, adults who had attended Head Start as children were necessarily very different from those who had not. Information collected when respondents were children was used to control for factors such as family income and parental education but, by limiting the study to families in which at least one sibling attended Head Start and one did not, the researchers were able to control for such unobserved differences as parental commitment to education. In Britain, Ermisch and Francesconi\(^{40}\) used a similar approach to investigate whether being brought up in a lone parent family affected outcomes in early adulthood.

The greater control offered by sibling studies is obvious and means that fewer assumptions are required legitimately to ascribe causality to observed relationships

\(^{38}\) Some preliminary unpublished work by the authors suggests that the home learning environment may be an important factor mediating the effects of additional education.

\(^{39}\) Garces, Thomas and Currie, 2000

\(^{40}\) Ermisch and Francesconi, 2001
than in ordinary regression models. Sibling studies take better account of the
dowment of characteristics and resources from parents to children, of the effects of
family characteristics and dynamics on child attributes, and of the response of parents
to the characteristics of their children.

Even so, significant difficulties in interpretation remain. It has to be assumed that
siblings have the same endowments and that their experiences of family life are
essentially the same insofar as they relate to the measure of child outcome. Clearly
the genetic make-up of siblings will vary, except in the case of identical twins, while
the birth order of siblings guarantees that they will experience different family
dynamics, if only because there are a different number of children for parents to
interact with. Parents may also learn from their earlier experiences of parenting or in
other ways respond differently to later children. Over time, family circumstances and
resources may change, altering the environment in which children are raised.
Children themselves have preferences, some linked to their differing endowments,
and these may vary between siblings such that the assumption of a shared family
environment is invalid. Similarly, siblings may have different relationships with each
of their parents and parents may respond to the different endowments of their
children, seeking to compensate for perceived deficiencies and to reinforce
acknowledged strengths.

These considerations all point to the possibility that the required assumptions will
hold only rarely. Whether this matters, depends on how closely the different family
dynamics are associated with the outcome being considered. For the most part, this
will not be known and opens up a research agenda in its own right. Indeed, the
assumption of homogenous family experience may be one reason why studies tend to
show that genetic endowment is much more important in determining child outcomes
than family characteristics. Nevertheless, it may be safe to assume that the variation
in the dynamics as experienced by children is less for siblings living in the same
family than for non-siblings living in different families.

However, sibling studies have other limitations. Most important is the selectivity that
they impose. Lone children, who make up an increasing proportion of all children,
are by definition excluded. Moreover, the selection may need to be more extreme,
as in the study by Garces, Thomas and Currie, when it was necessary to exclude
families where all children attended Head Start and those where none had done so.
Arguably, the circumstances of the families included in the study were less stable than
those excluded (which probably included disproportionate numbers of families who
were long-term poor) with possible implications for the generalisability of the
findings. The problem of selection is most severe when the effect is correlated with a
variable of interest and when relationships vary among sub-groups of the population.
On the other hand, it is sometimes incorrectly assumed that at least one child from
every family with an odd number of children has to be excluded from a study of
siblings (sometimes termed ‘sibling pairs studies’). In fact, this is generally not the
case because, analytically, the differences observed are not those between groups of
pairs but between individuals and their family’s mean. Even so, the focus on pairs

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41 Rutter and Silberg, 2002; Dunn and Plomin, 1990; Plomin, 1994
42 Although they can be included in analyses to add variance.
43 Garces, Thomas and Currie, 2000
halves the effective sample size with a corresponding effect on the robustness of the estimates, offset by the increased control over unobserved heterogeneity.

A further difficulty with sibling studies is that they shift the focus away from processes associated with sibling status. There is evidence that birth order does affect life chances and the gender of siblings may be important\textsuperscript{44}. This may be a small price to pay for the extra analytic control that can be achieved, but counsels against the assumption that sibling studies are a universal panacea.

To summarise, the great attraction of sibling analyses is that they enable account to be taken of both intergenerational inheritance and dynamics within the family without needing to be specific about either. This permits better causal modelling of the non-familial processes affecting child outcomes than do many other analytic methods. Nevertheless, the deficiencies of the method mirror its strengths. By not opening the black box of the family, it is impossible to know when the assumptions required for causal interpretation are likely to hold, nor the precise implications when they do not.

4.1.3 Structural modelling with latent variables

Using latent variables or constructs in structural equation models offers a rather different approach to mapping the pattern of influences on child outcomes. Whereas both instrumental variable analysis and sibling studies seek to isolate the effect of an individual variable on children’s life chances, holding other influences common, structural modelling with latent variables looks to understand the social world in terms of the combined effect of batteries of variables that index underlying theoretical constructs.

The difference in approach has much to do with the faith placed on the quality of social measurement. Advocates of the use of latent variables would argue that measurement in the social sciences is very difficult and is possibly made more so by the lack of resources devoted to measurement as compared to, say, experimental physics\textsuperscript{45}. Currently, it is unwise in the social sciences to rely on a single measure – often the only variable available - to represent an analytic construct such as parenting, social malaise or social inclusion. Indeed, some would argue that these constructs are inherently multi-dimensional and to index them by a single measure, or to treat them as the simple sum of a set of individual variables, is to do injustice to the underlying concept. Moreover, by defining constructs that are conceptually discrete, problems of multi-collinearity and unobserved heterogeneity are reduced because most of the correlations between individual variables are nested within constructs rather than existing between them. The strategy replaces the explanatory web of potential influences with thicker threads with effect, it is argued, that in empirical investigation the ratio of construct variance (that proportion of observed variation in child outcomes that can be explained) to measurement error is increased.

At a technical level, the approach blends confirmatory factor analysis with path analysis, although in reality both modelling approaches are performed simultaneously within the same procedure. The factor analysis specifies a ‘measurement model’ in

\textsuperscript{44} Modin, 2002
\textsuperscript{45} Marcoulides and Moustaki, 2002
which latent constructs are defined in terms of a set of measured variables in which each variable is usually constrained to ‘load’ on (or contribute to the definition of) a single construct. In the path analysis, correlations between the latent constructs are used as input to a second model to estimate the structural relationships between the latent constructs.

An example is provided by Aber. Reflecting the model presented in Figure 1, the aims were:

1. to tease apart the effects of low-income from the correlated but distinct effects of financial hardship on parenting processes and children’s development;
2. to explore the degree to which parenting processes may partially mediate the influence of low incomes and high hardship on children’s social-emotional and cognitive development; and
3. to develop a “family-level” model of relationships that could be used later in multilevel analyses of the influence of variation in state policies on individual child outcomes.

Early findings based on cross-sectional data are presented for ease of exposition.

Six latent variables were defined using cross-sectional data from the United States Early Childhood Longitudinal Study Kindergarten Cohort that follows a nationally representative sample of over 21,000 children (Table 1). These comprised two constructs indexing child cognitive and social-emotional development and four potential explanatory constructs: financial hardship; parental investment; parental behaviour; and parent distress.

Figure 3 displays the results of a structural equation model in which direct and parent-mediated relations between income and financial hardship and children’s outcomes were modelled simultaneously. The direct effects of income and hardship were found to be small, whereas several of the mediated pathways were substantial. In particular, family income was negatively associated with financial hardship and positively associated with investment, and hardship was positively associated with stress and negatively associated with investment. Increases in parent stress in turn were associated with decreases in levels of positive parent behaviour. Links between parent factors and child outcomes were specific and in predicted directions: more investment is associated with greater cognitive skill in children and more positive parenting behaviours are associated with more positive social-emotional development in children.

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46 Aber, 1998
Table 1 Measurement model for constructs related to child outcomes

<table>
<thead>
<tr>
<th>Manifest variables</th>
<th>Financial hardship</th>
<th>Parental investment</th>
<th>Parental behaviour</th>
<th>Parent stress</th>
<th>Cognitive development</th>
<th>Social-emotional development</th>
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<tr>
<td>Food insecurity</td>
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<td>Financial problems</td>
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<tr>
<td>Residential instability</td>
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<tr>
<td>Inadequate health care</td>
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<tr>
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<td>Buys cognitively stimulating materials</td>
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<td>Activities with child out of home</td>
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<td>Rules and routine</td>
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<td>-0.31</td>
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<td>Marital conflict</td>
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<td></td>
<td>-0.46</td>
</tr>
</tbody>
</table>

Comparative fit index (CFI): 0.987
Root-mean square error of approximation (RMSEA): 0.056


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48 Bentler 1992
49 Hong, 2000
Figure 3  Family income and financial hardship as predictors of parenting characteristics and child development.

Note. Standardized paths are shown; all structural paths are significant except for the dashed path. Demographic controls include whether child is Black, Hispanic, Asian, or of another race; the marital status of the parents; the mother’s and father’s work status; and the highest level of education in the family.


There were two unexpected, yet explainable, findings in this model. The first is that increases in income were associated with increases in parent stress. It appears that once the stress associated with hardship is accounted for, stress becomes a problem for parents on the high end of the income scale, perhaps due to stresses of managing two careers while caring for young children. The second unexpected finding is that increases in hardship were related to increases in positive parent behaviour. Again, once the negative effect of stress on parent behaviour is accounted for, it appears that parents experiencing hardship may actually try to compensate for their stress by increasing their positive parent behaviour. These interpretations are supported by the standardized indirect regression coefficients, which confirm that there is a strong indirect association between income and parent stress ($\beta = -.42$) and between financial hardship and parent behaviour ($\beta = -.60$).

In addition to this model, the authors tested several alternatives (featuring: direct paths only; indirect paths only; one parenting factor; and one income-hardship factor)
but the model in Figure 3 fitted best (determined by comparing change in the Akaike Information Criterion (AIC) and Chi-square difference tests) (Gershoff et al., 2003).

Because the relationships mapped in Figures 3 are based on cross-sectional data they do not provide evidence of causation. However, the approach can be extended to longitudinal data using latent growth curve modelling and growth mixture modelling. In essence, these approaches focus on changes in latent structures and the relationships between them, modelling the intercept (origin), rate of change (slope) and acceleration or deceleration (quadratic change), growth mixture modelling allows for differences in the trajectories of subgroups. Analysis of this kind is currently being undertaken exploiting panel data from the Early Childhood Longitudinal Study, which follows a nationally representative sample of US children from kindergarten through to fifth grade.

While structural equation models with latent variables allow cogent stories to be told about the social world, the lack of specificity can present problems for policy makers. It is not generally possible to identify what precisely it is about, say, parenting that makes a difference to child outcomes. Equally, though, it may be inappropriate to focus on responses to individual survey questions given all that is known about the sensitivity of responses to wording. Similarly, it is not self-evident that much is achieved by focusing on casual pathways in multivariate analyses where only a very small proportion of the variance in the outcome of interest is explained. The implication may be that more attention and resources should be directed towards measurement and that a more diverse range of measures should be used. Given the time taken to administer questionnaires and problems of respondent fatigue and attrition, this points to the need for more, and more tightly focussed, studies and the development and application of data-fusion techniques that enable data from different datasets to be amalgamated for subsequent combined analysis.

To conclude, none of the three approaches to moving beyond descriptive correlation to causal analysis is without its problems. Sibling analyses permits a focus on process outside the family but in so doing may direct attention away from a particularly important influence on child outcomes: family dynamics. Instrumental variable analysis concentrates on measuring the precise impact of very specific influences but may only be practical on rare occasions, while structural models with latent variables forsake (perhaps spurious) detail to chart the bigger picture. None is the preferred approach; rather they answer different questions, achieving unique insights in varying circumstances. The trick is to combine these and other approaches together, both in single studies and globally, to explore the various complex influences on child outcomes.

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50 Duncan et al., 1999; Scott and Handcock, 2000
51 Jones, Nagin and Roeder, 2001
52 Aber et al., 2001
53 Such and Walker, 2002
54 Ginn, 2000; Putten, 2000
4.2 Greater specificity

The second broad approach to the explanatory conundrum, greater specificity, would initially appear to conflict with the philosophy of latent structures. Rather than seeking simplification, the assertion is that existing explanations are simplistic, and perhaps dangerously so given the public and policy interest in affecting the life-chances of children. The conflict is more apparent than real given the joint concern with comprehensiveness and robust measurement.

The strategy of greater specificity has three components: inclusion; detail; and precision. The first is a response to the comparatively narrow focus of much of the research conducted to date. This, in turn, reflects disciplinary preoccupations with issues such as income, employment and parenting and perhaps the fact that the study of child outcomes is in its infancy in Britain, certainly as a multidisciplinary enterprise stimulated by government priority. Many transmission processes are not well understood or even studied at all. In the British analysis undertaken by Hobcraft\(^55\), no childhood factor shows men to be more disadvantaged than women and yet for each measure of adult outcomes, the relationship with childhood disadvantage is stronger for men than for women. Why is unknown; the associations could be real or reflect gender-based differences in the meaning and measurement of concepts. Virtually no research has been conducted on ethnic differentials, partly because of the small numbers of respondents from ethnic minorities captured in longitudinal studies. The consequences of this lack of research are pervasive. They mean even known relationships cannot be precisely specified because the processes may be mediated through other routes of causation\(^56\).

The challenge is to integrate perspectives and constructs drawn from across the disciplines, perhaps by developing a conceptual framework derived from a systematic review of theoretical and empirical research that might inform the focus of subsequent work. Given the fragmented, discipline specific nature of current research, it is possible that no framework would enjoy universal support and dissent might well increase the more detailed it is. Nevertheless, such a framework would reveal where new research is most needed, or likely to be particularly fruitful, and could foster interdisciplinary discourse.

With regard greater specificity, the above discussion of the ‘black-box’ of the family and parenting is instructive. Few, if any, of the relationships identified have been researched in detail. Moreover, disciplinary insights are scattered across the disciplines of developmental and social psychology, sociology, education and social work and are seldom discussed systematically in the context of public policy, although this has occurred more frequently in the USA than in Britain\(^57\). Consistent with the logic of structural models and latent variables leading to more tightly focussed studies, there is need to move away from general all-purpose models to focus on a limited set of outcomes, theoretically to derive the likely causal paths and to implement longitudinal studies and surveys that specifically record interim as well

\(^{55}\) Hobcraft, 2002

\(^{56}\) Instrumental variables can only be constructed if theory or other findings suggest that they are necessary.

\(^{57}\) Gershoff et al., 2003
as final outcomes. This approach would encourage the development of theory to explain how experiences of childhood are mediated through existing structures embracing gender and ethnicity, and how they might be further mediated through policy intervention. It could also stimulate further development of analytic techniques to handle trajectories (such as sequence and optimal matching analyses and latent growth curve and growth mixture modelling). To be of most value, specific studies would need to investigate causal pathways identified as important in the conceptual framework.

Finally, again echoing the discussion of latent variables, there is a need to increase precision in the measurement and classification of dependent and independent variables, the child outcomes to be explained and the factors that might explain them. A great deal hangs on retrospective questioning about quite distant events. Answers to questions on income are notoriously unreliable unless supported by documentary evidence. Responses to questions about behaviour are likely to be unreliable if there is marked temporal variation in the behaviour in question or if is socially unacceptable; the tendency, for example, of respondents to include abortions with miscarriages can turn a random variable into a non-random one. Single, essentially arbitrary, questions are generally inadequate as an index of complex social processes and outcomes. Informed independent assessment may have greater reliability and validity than self-assessment.

The response to these problems is more time, care and testing in the specification of survey questions which means longer timetables for the collection of data and greater expense but better value for money. It may require less proprietary rights over questionnaires and more dissemination of questions and methods that failed as well as those that proved to be successful. It probably means greater use of administrative data, especially where these are checked and validated and more triangulation with multiple sources of information. Overall, it means greater priority being attached to the development and practice of social measurement.

4.3 Strategies for interpretation

The third response to the conceptual and technical conundrums of mapping the processes by which childhood experiences determine life-chances and adult outcomes focuses attention on the interpretation of findings. While all analytic findings are sensitive to assumptions made in data collection and analysis, the complexity of the processes influencing child outcomes, the difficulty of analysis, the strong assumptions that are sometimes required and the inevitable selection resulting from the need to exert control make synthesis of findings particularly difficult. The challenge is to sift and weight findings, prioritising those that are theoretically consistent and based on good data and analysis. Taking account of the pervasiveness and persistence of findings is also important.

It is important to be clear about the nature of the interpretative synthesis required. It is not about producing an average of all estimates, irrespective of quality. The requirement is to check whether similar estimates are generated by different methods,

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58 Halpin and Chan, 1998
59 Scott and Handcock, 2000; Jones, Nagin and Roeder, 2001
a process that should take place throughout the hierarchy of interpretation. Whenever possible, such triangulation should be attempted within individual studies. Replication needs to be encouraged so that findings are independently validated. In certain circumstances, meta-analysis - the statistical assembly of findings from multiple studies - can enhance the robustness and precision of estimates and reveal weak but important relationships (because of cumulative sample size), as well as identifying findings that are context specific and not readily generalised. Finally, the cumulative assembly of knowledge – the focus of the following paragraphs – needs to be more systematic, driven by the requirement for collective understanding and, as appropriate, a policy response rather than being circumscribed by the puzzles of individual disciplines.

All findings are sensitive to methodologies and data sources, to the definition of variables and the specification of models. No study can therefore be more than suggestive. Priority in synthesis needs to be given to findings that are based rigorous analysis of high quality data and which are pervasive, recurring in different studies, perhaps in different societies, using a variety of techniques and formulations. While the exceptional finding should not be ignored if it stands up to analytic scrutiny, it should nevertheless be treated as the rationale for further inquiry rather than being sewed into the fabric of assumed knowledge.

Priority should also be given to relationships (processes as opposed to outcomes) that are omnipresent, being observed for different groups in different situations. Likewise, priority should attach to persistent relationships: ones that are repeated in different cohorts for different generations. The argument is that processes that really matter probably do not change greatly over time. Moreover, if such a view could be substantiated, it would lay to rest concerns that longitudinal data is by definition out of date. Nevertheless, caution is required in relation to between-cohort comparisons since certain phenomenon do change with time. For example, because of the increase in children staying on in education after school leaving age, staying on at school is likely to explain less of the variation in adult outcomes than would have been the case in earlier generations.

From a policy perspective, priority also has to be given to processes that are, in principle at least, amenable to government intervention. Equally, though, it is important to assess the importance of such processes within the entire gamut of influences on children’s life-chances. If the overall influence of factors that are controllable is minimal, it might be judicious not to intervene at all. Alternatively, if the overall effect is small, but large for a subset of particularly disadvantaged children, then logic would suggest introducing targeted interventions.

Finally, and in some ways most importantly, priority should be judged in terms of theoretical relevance, since it is theory that imposes order on inconsequential observation. The challenge, as noted above, is to negotiate disciplinary boundaries in the formulation of theory, since no one discipline has a monopoly of insight in seeking to explain the pathways through which child experiences impact on later outcomes. The conceptual framework, discussed with respect to the need for greater

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60 Lipsey and Wilson, 2001; Ashworth et al., 2002
specificity, would also assist when establishing priorities in the interpretation of research as well as when commissioning new studies.
5 Conclusion and recommendations

Determining how childhood experiences shape life-chances and how policy might be used to influence these processes is, for social science, a task that is at the edge, perhaps beyond the edge, of what is possible. Much is still to be learned about the substantive issue and many methodological questions remain very difficult to resolve; indeed, some may prove to be intractable. What is clear, however, is that there no single best approach to determining child outcomes and different methods are likely to result in somewhat different answers due to necessarily different assumptions. Research needs to read very carefully, appraised critically and assembled systematically.

The central problem is that a myriad influences (individual endowment, family circumstances and social and community resources) are likely to influence a child’s trajectory into adulthood varying in relative importance over time. Disentangling chains of causality from the multitude of descriptive correlations is exceedingly difficult and requires strong theory, excellent technique and social-scientific imagination. Unfortunately, theory and knowledge are only partial and both are distributed thinly across competing intellectual disciplines. Even analytic techniques tend to be discipline specific. Unless these different insights can be brought together, learning how best to enhance children’s life chances is likely to continue to be a very slow process.

There is a need to stimulate the systematic acquisition and exchange of knowledge about the factors that determine child welfare, drawn from across the disciplines. This will, in turn, require the generation of better data and improved analytic techniques alongside the development of stronger, and perhaps more encompassing, theory. The following measures, organised under objectives, could significantly assist this process.

To stimulate information acquisition and exchange

What little knowledge and research evidence there is on child outcomes, is widely scattered across the disciplines, difficult to access, not well audited in terms of quality, nor rigorously appraised with respect to its relevance to public policy. Enacting major change requires joint action by government, funding organisations and the research community. To initiate this process, a well-resourced ‘champion’ is required charged with the task of ensuring that existing knowledge is systematically assembled and that further research is promoted as required. Their remit should be strategic and cross-disciplinary. To exert maximum leverage, such a champion might need to be located in government.

To stimulate the acquisition and exchange of information on child outcomes:

A) A Whitehall-based ‘champion’ should be appointed to ensure that:
   1) Knowledge on child outcomes is prioritised, assembled and disseminated;
   2) Gaps in understanding are identified; and;
   3) Appropriate new research initiated.
B) A ‘knowledge centre’ (similar to the EPPI-Centre based at the Institute of Education and the Centre for Evidence-Based Medicine at the University of Oxford) should be to identify, review and evaluate theoretical and empirical research on the factors influencing child outcomes (including policy interventions) on a continuing basis; this might entail undertaking a coordinated programme of systematic reviews.

C) The ‘knowledge centre’ or others should develop conceptual framework(s) to:
   1) Identify gaps in knowledge
   2) Develop research priorities
   3) Stimulate the systematic accumulation and appraisal of theory and evidence
   4) Promote disciplinary and methodological triangulation

D) Government should consider whether the new centre or any other organisation should have responsibility for coordinating research on child outcomes across government.

To promote data generation and exploitation

Given how recently policy interest has focussed on child outcomes and the time taken for longitudinal data to mature to cover the trajectory of children into adulthood, it is unsurprising that data requirements outstrip availability. Nevertheless, it is important to ensure that all opportunities for collecting necessary data are fully exploited, especially adding appropriate questions to one-off and routine studies. An ongoing audit of research and data collection relating to child outcomes is required. There is a case for this being undertaken by central government as the major funder of research although scope of the audit should extend beyond research commissioned by government. It might therefore be a task for the knowledge centre, which would then provide independent advice to government.

To promote the generation of new data and the better exploitation of new and existing information:

F) An audit of new research and data collection activities should be establish on an on-going basis to assess their potential (with amendment if necessary) to inform research on child outcomes.

G) Greater emphasis should be placed on prioritising the collection of longitudinal data because of its value in determining causality. This might entail:
   1) Including supplementary questions relevant to research on child outcomes whenever longitudinal studies planned;
   2) Supporting and strengthening the Family and Children Study as a vehicle for investigating child outcomes;
   3) Ensuring that the Millennium Cohort Study continues to be resourced adequately to ensure maximum quality and relevance, appropriate coverage and frequency;
   4) Investigating the possibility of an early childhood longitudinal study comparable with that in the USA to establish the importance of pre and early school experiences on child outcomes;
5) Further enriching and extending the Youth Cohort Study as the major source of information plotting young people’ entry into the labour market, post-school age education and adulthood;

6) Whenever possible over-sampling ethnic minorities in order to address the dearth of evidence on outcomes for children from ethnic minorities. (The important British Household Panel Study contains very small numbers of respondents from ethnic communities).

H) Government and other funding organisations should plan for and promote the secondary analysis of survey and administrative data in order:
   1) To ensure that costly data are fully exploited; and
   2) to stimulate the development of technical and substantive expertise within the academic and research community.

I) Random assignment in policy evaluation should be prioritised (where appropriate), which, as a secondary consequence, would enhance the scope for causal modelling in secondary analysis.

To improve measurement

Research on child outcomes is frustrated by weak measurement; concepts are often poorly specified and research instruments hastily constructed and subject only to limited prior testing. Since much data collection relevant to the study of child outcomes is undertaken by, or on behalf of, central government, government departments are uniquely able to influence the quality of measurement.

To improve measurement of child outcomes and causal influences:

J) Funding should be made available for a review and comparative evaluation of measures of child outcomes and parenting in order to ensure that the best measures are used, where appropriate, in all future studies, particularly those funded by Government;

K) The above measures should be routinely included in all one-off and regular surveys except when there are persuasive reasons for not doing so;

L) Researchers should prioritise the development and implementation of good measurement in their negotiations with funding organisations;

M) The time routinely allocated to the design of research and interview schedules should be increased.

N) Multiple measures of child outcomes should be including in surveys whenever possible.

To foster interdisciplinary working

While the topic of child outcomes is inherently interdisciplinary, research and theory rarely is with obvious negative consequences. This reflects fundamental institutional
impediments to interdisciplinary working that have proved resistant to change. Increased funding of interdisciplinary initiatives might succeed in promoting more interdisciplinary engagement where mission statements and cajoling have failed.

To foster interdisciplinary working:

O) Government and other funding organisations should continue to fund strategic research on child outcomes;

P) Government and other funding organisations should encourage and enable tenders from interdisciplinary teams for research on child outcomes;

Q) Cross-discipline peer review of research proposals and research output should be increasingly employed;

R) New forums for debate should be created to encourage multi-disciplinary exchange. These might include a new peer reviewed journal and an on-going series of seminars;

S) A programmatic research initiative and/or national research network on child-outcomes should be established, possibly organised through the ‘knowledge centre’;

T) An international forum or network should be founded to stimulate exchange between researchers working on child outcomes. This might be organised by the ‘knowledge centre’ and be focussed initially on Britain and the USA.

To stimulate methodological development

Techniques for handling longitudinal data and exploring causality are in their infancy. Further methodological development is required, as is the diffusion of existing expertise throughout the research community.

To stimulate methodological development:

U) The possibility of routinely linking survey data and administrative records and making anonymised data sets available to bona fide researchers (within the boundaries of data protection) should be investigated.

V) Government and other funding organisations should encourage the use of innovative methods of design and analysis in its funded research (which will entail acceptance of the attendant risks)

W) All funding organisations including Government should encourage the dissemination of innovative methodological approaches

61 These might include methods for modelling trajectories (sequence analysis, optimal matching latent growth curve modelling, growth mixture modelling, etc), data fusion techniques to combine data from different sources and the various approaches to modelling causation some of which are discussed above.
References


Modin, B (2002) *Setting the Scene for Life: Longitudinal studies of early social disadvantage and later life chances*, Stockholm, Department of Sociology, University of Stockholm, PhD Thesis


