Financial benefits for child health and well-being in low income or socially disadvantaged families in developed world countries (protocol)

Reviewers

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Dates

This protocol is co-registered within the Cochrane Collaboration

Date approved by Campbell: 15/11/2006
Date of last substantive update: 28/10/2006
Protocol first published in the Cochrane Library: Issue 1, 2007

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Contribution of reviewers

SD, CJ, GL and HR completed previous work that was the foundation of the current review. PL is coordinator and guarantor for the current review. HR, AS, MP, and PL are responsible for designing the review. PL designed and conducted the searches with support from the review group TSC. Screening of abstracts will be undertaken by SD, CJ, PL, JN and KM. Extracting data from papers will be conducted by SD, CJ, PL, KM. HR and AS will provide support where reviewers disagree. PL will be

responsible for entering data into RevMan and writing the review with contributions from all authors. MP and HR will provide methodological support, and HR, MP, AS and KM advice on interpreting the review and providing policy perspective.

Internal sources of support

University of Bristol, UK
City University, UK
University of Calgary, CANADA

External sources of support

Barnardo's, UK
Nordic Campbell Centre, DENMARK

Background

Within OECD (Organisation for Economic Co-operation and Development) countries relative poverty is a problem for a significant proportion of families with children. Data on relative poverty show that while the UK and the USA are among the wealthiest nations, they show high rates of relative poverty (UNICEF 2000, OECD 2006). The UK, USA, Australia and New Zealand constitute the small group of OECD countries where inequalities have increased since the 1960s (Weeks 2005). Whilst some data show that the UK, USA and New Zealand began reversing this pattern in the 1990s, overall the proportion of children living in relative poverty has increased (UNICEF 2005). According to US Census figures in 2004, around 17.8% of under 18s were living in households with income below what the US government defines as the poverty threshold (income below the federal poverty level (DeNavas-Walt 2005)). Using internationally recognised assessments of relative poverty this figure rises to 21.9% for the USA, second only within OECD countries to Mexico (27.7%), followed by Italy (16.6%), New Zealand (16.3%), Ireland (15.7%), Portugal (15.6%), and the UK (15.4%) (UNICEF 2005). These figures all consider income before housing costs are taken into account, but some consider income after housing costs to be a better estimate of household poverty. In the UK in 2002/3 28% of children lived in households with incomes below fifty percent of the mean after housing costs, giving the UK the fifth highest rate of relative child poverty in the EU (Bradshaw 2005). In contrast to a generation ago, poverty in the UK is now most prevalent in households with children (Darton 2003).

Inequalities in the distribution of resources are known to have marked impacts on child health and wellbeing. The impact of relative poverty, even in rich countries, is illustrated by the comparisons between infant mortality in urban areas of Kerala with that among African Americans living in Washington DC. Despite far higher national wealth, the infant mortality rate is higher in the USA group (UNDP 2005, Chapter 2). In high income countries, relative poverty reduces the life chances of children in many ways (Acheson 1998, Baker 2002, Dearing 2001, Petterson 2001,Shaw 1999,

Roberts 1997, Smith 1997, Duncan 1994, HM Treasury 2004). Spencer (Spencer 2000) contends that, "there is a consistent positive correlation between low socio-economic status and adverse […] child health outcomes," (p.170), while Roberts (Roberts 1997) points to the "long shadow forward" (p.1123) cast over physical and emotional health that can result from the experience of living in poverty during childhood. People from the lowest social classes are at increased risk from serious or long-term life-limiting illness. Children from these groups are less likely to meet their full potential in education and are more likely to be unemployed or working in unskilled, poorly paid manual jobs in adult life (Roberts 1997, Shaw 1999). Davey Smith (Davey-Smith 1999) argues that fluctuations in income also impact on health outcomes, with higher mortality rates amongst those who experience reductions in income levels, even if temporary.

The mechanism for the impact of income on child health is not clear, but it would appear that household income in itself is important over and above access to resources. One might suppose that, for example, lack of access to health care would be the key factor limiting the health chances of poor children in the USA. In fact, comparisons of data between USA, Canada and UK suggest that while the universal health care provided by the latter countries may lessen the impact of growing up poor, the association between health and wealth persists (Case 2002, Currie 2003, Currie 2004). US data shows increasing strength of relationship over the period of childhood suggesting cumulative impact on health, and while UK data showed a continuing relationship, the evidence for cumulative insults was less clear. These data imply that within-country factors may mediate the relationship between health and income. Research from Canada has also found that children from poorer backgrounds are more likely to be diagnosed with mental health problems in childhood (Currie 2005). Oral health shows similar income gradients, where international studies have shown that children from poorer families have higher rates of dental decay (caries) and poorer oral health than richer children living in the same country (Petersen 2003; Watt 1999).

Given the consistent observation of an association between economics status and health outcomes, this review seeks to answer the question of whether reducing relative poverty through additions to income may have beneficial effects. Income, rather than social support, is at the heart of the interventions explored in this systematic review, which aims to interrogate the evidence to assess the effectiveness of financial benefits in improving child health. 'Health' is interpreted here in its widest sense, incorporating physical and mental health, as well as social wellbeing indicated by factors such as educational attainment.

This review will consider evidence of effectiveness in randomised controlled trials and quasi-randomised trials of interventions that provide additional monies to socially and economically disadvantaged families. The history of the use of RCTs in the social sciences is mixed. While experimental methods have a significant history in the social sciences (Oakley 1998), they are not universally welcomed. Resistance to the use of trials in social interventions on practical, ethical or political grounds has been documented (see for example Petticrew 2005), and such views have had an impact on the types of studies conducted (for example see Seethaler 2005). In addition, some changes (such as universal policy interventions) can be documented only across a cohort as a whole, since an entire population is (or is intended to be) in receipt of such changes. In this context, while the findings of the review will be based on

experimental evidence from controlled trials only, studies of other types will be identified in an appendix to the main body of the review.

Objectives

To assess the effectiveness of direct provision of financial benefits to socially or economically disadvantaged families in improving children's health and educational attainment

Criteria for considering studies for this review

Types of studies

Randomised controlled trials and quasi-randomised (e.g. alternate allocation or allocation by date of birth) controlled trials.

Types of participants

Families with at least one child under 16, or in which a woman is pregnant, living in a 'high income country as reported in 2005 Human Development Report (UNDP 2005)

Participants must be identified by triallist as being from groups socially or economically disadvantaged within their country. This might be assessed by income or by geographical/neighbourhood data (i.e., having an address in area of high unemployment or low average income).

Types of interventions

Interventions to increase the amount of money available to a family. These include:

- Direct cash payments
- Positive taxation schemes, such as Negative Income Tax, which benefit low-income families

Excluded from the review are:

- Vouchers, loans, and conditional payments for commodities (cash that can only be spent in specified ways, for example to pay for personal care for disabled children).

Types of outcome measures

Primary Outcomes:

1. Any measure of physical child health, including anthropometry (body measurements) or measures of mortality, morbidity (illness diagnosed or treated by medical professionals), admissions to hospital, attendance at emergency medical services, attendance at routine health screening programmes, or uptake of

immunisation
2. Any measure of children's mental health or emotional state (e.g. quality of life measures, the CBCL (Child Behavior Checklist Achenbach 1991) or the Strengths and Difficulties Questionnaire Goodman 1997).
3. Oral health as assessed by the D(M)F (decayed (missing) filled) Index for permanent or deciduous teeth (dMF Index for milk or baby teeth) or restorative index (the ratio between health, filled and decayed teeth). The former provide well validated assessments of total dental health, and the latter the extent of untreated decay (Pitts 2006)

Secondary Outcomes:

1. Any standardised measure of children's psychomotor or cognitive development.
2. Any standardised measure of educational progress or attainment.
3. Numbers of pregnancies, births or sexually transmitted infections among under 16s in target families

Outcomes will be assessed at three time points: short-term, defined as up to one year following cessation of trial. medium-term (1-3 years following cessation of trial) and long-term (3+ years following cessation of trial). If data allow, we will also consider examining this variable continuously

Any adverse effects reported for any member of the family will be recorded.

Search strategy for identification of studies

Published or unpublished trials will be considered with no language restrictions.

The following electronic databases will be searched:

CENTRAL (Cochrane Library)
ASSIA
CINAHL
Econlit
Embase
ERIC
Index to theses
Medline
MDRC (Manpower Demonstration Research Corporation publications)
PsycINFO
SIGLE
SSRN elibrary
SRDC (Social Research and Demonstration Corporation publications)

Science and Social Science Citation Index will be used to carry out a forward citation search on papers meeting the inclusion criteria (i.e. papers indexed which refer to them).

The Internet will be searched using the search engine Google Scholar (scholar.google.com) using exact phrases ["family income" change child health] and ["financial benefit" family child health]. The first 100 sites identified will be screened for relevance, following up potentially relevant sites to locate any studies (unpublished or published).

The general structure of the search strategy will be:
(terms for income and financial benefits including appropriate MeSH terms depending on the Thesaurus for each database)
'and'
Paediatric filter (see Mackway-Jones 2002)
'and'
Cochrane filters for the identification of RCT's will be used where available, e.g. Dickersin 1994; Robinson 2002, as detailed below.

The search strategy will be adapted to each database. Below is an example search strategy for Medline run on OVID platform:

1 exp CHILD/
2 child.mp.
3 exp PEDIATRICS/
4 pediatric$.mp.
5 paediatric$.mp.
6 or/1-5
7 perinat$.mp.
8 neonat$.mp.
9 newborn$.mp.
10 infan$.mp.
11 bab$.mp.
12 toddler$.mp.
13 boy$.mp.
14 girl$.mp.
15 kid$1.mp.
16 school-age$.mp.
17 school age$.mp.
18 juvenile$.mp.
19 (under-age$ or under age$).mp.
20 teen$.mp.
21 minor$.mp.
22 pubescen$.mp.
23 adolescen$.mp.
24 youth$.mp.
25 young person$.mp.
26 young people.mp.
27 or/7-26
28 infan$.jw.
29 child$.jw.
30 pediatric$.jw.
31 paediatric$.jw.
32 adolescen$.jw.

33 or/28-32
34 33 or 27 or 6
35 income$.tw.
36 financ$.tw.
37 payment$.tw.
38 social security.tw.
39 (cash or economic or (money or monetary) or charit$ or demogrant or temporary assistance for needy families or tanf or welfare or fiscal or budget or (tax$ adj4 credit$)).tw.
40 monies.tw.
41 Income/
42 Social Welfare/
43 Social Security/
44 Financial Support/
45 Public Assistance/
46 Financing, Government/
47 or/35-46
48 randomized controlled trial.pt.
49 controlled clinical trial.pt.
50 randomized controlled trials.sh.
51 random allocation.sh.
52 double blind method.sh.
53 single-blind method.sh.
54 or/48-53
55 (animals not human).sh.
56 54 not 55
57 clinical trial.pt.
58 exp Clinical Trials/
59 (clin$ adj25 trial$).ti,ab.
60 ((singl$ or doubl$ or trebl$ or tripl$) adj25 (blind$ or mask$)).ti,ab.
61 placebos.sh.
62 placebo$.ti,ab.
63 random$.ti,ab.
64 research design.sh.
65 or/57-64
66 65 not 55
67 66 not 56
68 comparative study.sh.
69 exp Evaluation Studies/
70 follow up studies.sh.
71 prospective studies.sh.
72 (control$ or prospectiv$ or volunteer$).ti,ab.
73 or/68-72
74 73 not 55
75 74 not (56 or 67)
76 56 or 67 or 75
77 34 and 47 and 76

Contact will be made with first authors of included studies and field experts to enquire of relevant further or unpublished research, and any additional research located in this way will be recorded and considered for inclusion.

References of retrieved articles and relevant reviews will be reviewed for eligible studies.

Methods of the review

Titles and abstracts of studies identified by searches will be read on screen and independently assessed for inclusion by two reviewers (SD, CJ, PL, JN, KM) against the inclusion criteria set out above.

Those studies that appear to meet the inclusion criteria at this stage will be retrieved in hard copy. These reports will be examined independently by two members of the research team (SD, CJ, PL, JN, KM). Records will be kept detailing reasons for rejection. Disagreements will be documented and resolved by consensus, with arbitration by a third member of the team (PL) if consensus cannot be achieved.

Data Extraction
Details of each study will be independently extracted by two researchers and entered into RevMan 4.2.8. Recorded data will include:

Participants:
Family composition
Family socio-economic position
Country and setting (e.g. rural, urban or region)
Age and gender of child(ren)

Intervention
Value of intervention in local currency
Duration of intervention

Comparator/ alternative interventions
Type of intervention
Detail of intervention (e.g. frequency of home visits, details of visitor)
Duration of intervention

Co-interventions
Type of intervention
Detail of intervention (e.g. frequency of home visits, details of visitor)
Duration of intervention

Assessment of quality of included studies
Two members of the research team will independently assess the following aspects of study quality for the included studies. Differences or disagreements will be resolved by consensus, with arbitration by a third member of the team if consensus cannot be achieved.

1. *Method of allocation*

Allocation (method by which participants are assigned to group) will be classified as follows:

(A) Allocation will be described as adequate if allocation was by a well described randomisation process (e.g. flipping a coin, central randomisation using number tables).
(B) Allocation will be described as unclear if the unit of allocation is not described or is not described in sufficient detail to be certain of quality of randomisation.
(C) Allocation will be described as inadequate if allocation was undertaken using a non-random method (e.g. by day of the week)

2. *Allocation concealment*

In the case of behavioural and service interventions, the intervention itself can't be provided blind to participants and providers (i.e. participants and providers will know what treatment is being received) which may have implications for detecting performance bias. However, the concealment of the allocation process should be blind and not susceptible to selection bias. Therefore, allocation concealment will be assessed as follows:

(A) Allocation will be described as adequately concealed if allocation was centralised (e.g. allocation by a central office unaware of participant characteristics), used pre-numbered sealed opaque envelopes, generated by computer or other methods not accessible to those in charge of allocation.
(B) Allocation concealment will be described as unclear if the method of concealment is not described or is not described in sufficient detail to be certain of concealment
(C) Allocation concealment will be described as inadequate if allocation was undertaken by personnel with access to participant characteristics.

3. *Loss to follow up*

We acknowledge that loss to follow up can be a significant source of bias in findings, and whilst random loss to follow up may be less problematic than systematic loss to follow up this difference can be difficult to tease out from studies. When considering loss to follow up a cut off is often used, for example, a loss of more than 25% of the sample may be judged unacceptable. The position of such a cut off at 25% rather that 30% or 20% is difficult to justify, and therefore loss to follow up as a percentage of those entering each study group will be reported where data are available. However, a summary of quality assessment is useful and thus in addition to this figure a description using the following categories will be given:

(A) Loss to follow up will be considered acceptable if attrition is both similar across intervention groups, and of an acceptable level. We take acceptable loss to follow up to be no greater than approximately 25% of sample entering intervention at follow up, but allow for reviewer judgement (for example greater levels of loss may be acceptable loss for follow up of 5+ years).
(B) Loss to follow up recorded as not reported (B)

(C) Loss to follow up recorded as unacceptable if loss is either high (greater than 25% overall allowing for reviewer judgment), or unevenly distributed across groups. Uneven attrition will be further considered in senstivity analysis.

4. *Blinding of outcome assessment*

In studies with multiple outcomes, adequacy of each outcome assessment will be consider separately. The study as a whole will be graded according to the most biased assessment, although in sub-group analysis by outcome the quality assessment for each outcome may be considered in its place. Outcome assessment will be judged as follows:
(A) Blinding of outcome assessment will be considered adequate if authors state assessor was blind to participant allocation, or outcome assessed by means outside of the study (e.g. school records).
(B) Blinding of outcome assessment will be considered unclear where insufficient information is provided to judge blinding.
(C) Blinding of outcome assessment will be considered inadequate where assessors are likely to know the group allocation of participants.

Disagreements will be resolved by consensus, and first authors contacted for clarification in the case of unclear methods. The impact of quality assessment criterion on study outcomes will be considered.

Data Management

Citations will be stored using Reference Manager, organised to generate a QUOROM-style flow-chart documenting the selection process for included and excluded studies.

A data extraction sheet will be piloted amongst reviewers with the aim of ensuring maximum utility and comprehensiveness. Data will be extracted and entered into the finished forms and stored electronically. Annotated copies of included studies will be stored in hard copy.

Contact authors of primary studies included in this review will be contacted to provide missing data concerning methods employed in the study and/or missing data from the results.) Missing data and dropouts will be assessed for each included study and the review will report the number of participants who are included in the final analysis as a portion of all participants in each study. The possible influence of missing data on the results will be discussed.

Data synthesis

1. *Meta-analysis*
Data will be analysed using both fixed effect and random effects models, although we expect a random effects model to be more appropriate due to expected heterogeneity across studies.

2. *Binary data*
For binary outcomes, e.g. 'pregnant' or 'not pregnant', a standard estimation of the Odds Ratio with the 95% confidence interval will be calculated.

3. *Continuous data*
Effect sizes will be calculated from continuous data if means and standard deviations are provided, obtainable or can be derived from available data (such as test statistics). Post-intervention means and, where baseline data are available, pre-intervention means scores will be reported. Where possible, absolute change from baseline in the intervention group will be calculated (intervention group change - control group change), along with standard deviations and 95% confidence intervals. Continuous variables that are measured on different scales in different studies will be analysed as standardized mean differences. Confidence intervals (95%) will be reported.

5. *Heterogeneity*
We will conduct a meta-analysis if the following assessments of heterogeneity suggest that it is appropriate:
1)Common sense. Are the participants, interventions or outcomes sufficiently similar to justify consideration of meta-analysis? (Kristjansson 2003). For example we will not combine findings from interventions of short and long term duration.
2)Quantification of inconsistency across studies. The consistency of results will be assessed using the I^2 statistic (Higgins 2003). If there is evidence of heterogeneity (Q-statistic=0.1 coupled with an I-squared value of 25% or greater), the authors will consider sources according to pre-specified subgroup analyses and sensitivity analyses (below) but will not calculate an overall estimate of effect size. If the primary studies are judged to be substantially heterogeneous (e.g. across participant characteristics) even within these subgroupings, only a narrative synthesis account will be conducted.

If there is significant heterogeneity among primary outcome studies, the following (common sense) factors are considered as possible explanations: type of intervention (e.g. direct payment or tax transfer); value or duration of the intervention, comparator interventions, co-interventions, and differences in participant characteristics such as socioeconomic position, etc.

If the primary studies are too heterogeneous e.g. each study uses a different intervention or different outcome measures, or the data are insufficient for meta-analysis within RevMan, then only a narrative (descriptive) analysis will be undertaken.

6. *Sub-group analyses*

Subgroup analyses will be carried out if the data are available for any of the following subgroups, in line with existing guidance:

intensity of intervention (amount of financial assistance given);
underlying health/social welfare provision (e.g. countries with universal healthcare systems in place vs. those without)
Method of delivery of intervention (e.g. direct cash payment versus indirect tax benefits);
Effects of co-interventions
Socioeconomic position (where sample includes more than one socioeconomic group)

7. *Sensitivity analyses*
Primary analyses will be based on available data from all included studies relevant to the comparison and outcome of interest. In order to assess the robustness of conclusions to quality of data and approaches to analysis (e.g. ITT), sensitivity analyses will be performed. These will include:
a) Study design.
A sensitivity analysis may be undertaken to assess the effects randomisation may have on results.
b) Intention to treat. For dichotomous outcomes, such as 'pregnant' or 'not pregnant', the authors will assume that those who were lost to follow up (i) had proportionately the same outcomes as those who completed in the control group (ii) experienced the successful outcome (iii) all experienced the unsuccessful outcome.
c) Differential drop-out. Studies with severe imbalance in terms of numbers of attrition will be excluded from the analysis to assess their influence on the overall result
d) Outcome measures. The effect of inclusion of measures assessing mental health or emotional states compared to including only observed behaviour or physical health.

8) *Assessment of bias*
Funnel plots will be drawn to investigate any relationships between effect size and study precision in terms of sample size. Such a relationship could be due to publication or related biases or due to systematic differences between small and large studies. If a relationship is identified, clinical diversity of the studies will be further examined as a possible explanation (Egger 1997).

Description of studies

Methodological quality of included studies

Results

Discussion

Reviewers' conclusions

Implications for practice

Implications for research

Acknowledgements

Our thanks to Barnardo's for funding the original review on which this is based and to the Nordic Campbell Centre for additional funds made available to complete this review. We would also like to thank Aubrey Sheiham for helpful advice on oral health outcomes, the Cochrane Developmental, Psychosocial and Learning Problems Group for their support and assistance in the preparation of this protocol and anonymous reviewers for their careful comments on behalf of the Cochrane Group.

Potential conflict of interest

As a group of researchers we acknowledge that we have a tendency towards favouring equality over inequality, and a predisposition in favour of the health promoting effects of an adequate income.

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