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[Intervention Review]

The impact of conditional cash transfers on health outcomes and use of health services in low and middle income countries

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ABSTRACT

Background

Conditional cash transfers (CCT) provide monetary transfers to households on the condition that they comply with some pre-defined requirements. CCT programmes have been justified on the grounds that demand-side subsidies are necessary to address inequities in access to health and social services for poor people. In the past decade they have become increasingly popular, particularly in middle income countries in Latin America.

Objectives

To assess the effectiveness of CCT in improving access to care and health outcomes, in particular for poorer populations in low and middle income countries.

Search methods

We searched a wide range of international databases, including the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE and EMBASE, in addition to development studies and economic databases. We also searched the websites and online resources of numerous international agencies, organisations and universities to find relevant grey literature. The original searches were conducted between November 2005 and April 2006. An updated search in MEDLINE was carried out in May 2009.

Selection criteria

CCT were defined as monetary transfers made to households on the condition that they comply with some pre-determined requirements in relation to health care. Studies had to include an objective measure of at least one of the following outcomes: health care utilisation, health expenditure, health outcomes or equity outcomes. Eligible study designs were: randomised controlled trial, interrupted time series analysis, or controlled before-after study of the impact of health financing policies following criteria used by the Cochrane Effective Practice and Organisation of Care Group.

Data collection and analysis

We performed qualitative analysis of the evidence.

Main results

We included ten papers reporting results from six intervention studies. Overall, design quality and analysis limited the risks of bias. Several CCT programmes provided strong evidence of a positive impact on the use of health services, nutritional status and health outcomes, respectively assessed by anthropometric measurements and self-reported episodes of illness. It is hard to attribute these positive effects to the cash incentives specifically because other components may also contribute. Several studies provide evidence of positive impacts on the uptake of preventive services by children and pregnant women. We found no evidence about effects on health care expenditure.

Authors' conclusions

Conditional cash transfer programmes have been the subject of some well-designed evaluations, which strongly suggest that they could be an effective approach to improving access to preventive services. Their replicability under different conditions - particularly in more deprived settings - is still unclear because they depend on effective primary health care and mechanisms to disburse payments. Further rigorous evaluative research is needed, particularly where CCTs are being introduced in low income countries, for example in Sub-Saharan Africa or South Asia.

PLAIN LANGUAGE SUMMARY

The impact of conditional cash transfers on health outcomes and use of health services in low and middle income countries

We found 29 papers on the impact of conditional cash transfers (CCT) on access to care and health outcomes. Of these, ten papers, reporting results from six studies, satisfied the inclusion criteria; four of these studies were randomised experiments. Despite a number of methodological weaknesses in some studies, overall the research evidence suggests that CCT schemes may result in a number of benefits to health for poor populations. Many conditional cash transfer programmes include a number of components, including incentivising attendance for health education, measurements of height and weight, immunisations and nutritional supplementation. Conditional cash transfer programmes appear to be an effective way to increase the uptake of preventive services and encourage some preventive behaviours. In some cases programmes have noted improvement of health outcomes, though it is unclear to which components this positive effect should be attributed.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON *[Explanation]*

Outcomes	Relative effect	Quality of evidence	Comments
Health services utilisation	All studies reported an increase in the use of health services in the intervention groups (27% increase in individuals returning for voluntary HIV counselling, 2.1 more visits per day to health facilities, 11-20% more children taken to the health centre in the past month, 23-33% more children <4 yrs attending preventive healthcare visits)	Low	Findings taken from 5 studies (3 C-RCT, 1 RCT, 1 CBA). Two studies report results from facility-based routine data which are not always reliable (in one of these studies, there was a risk of contamination bias)
Immunisationcoverage	Mixed results were found (increased vaccination rates in children for measles and tuberculosis but only in specific groups or temporarily, and no change in one study)	Moderate	Findings from 3 C-RCT and 1 CBA. Differences in effects might be due to initial rates of immunisation (effects found in cases where pre-intervention rates were relatively low)
Health outcomes	Mixed effects on objectively measured health outcomes (anaemia) and positive effects on mothers reports of childrens health outcomes (22-25% decrease in the probability of children <3 years old being reported ill in the past month)	Moderate	Results from 3 studies (2 C-RCT and 1 CBA)
Childrens anthropometric outcomes	Positive effects found on childrens growth (increase in height by about 1cm amongst children < 4 years old and); however there were two contradictory findings on the impact on height-for-age Z scores (1 study found a significant increase while another one found a negative impact, equivalent in size) Decrease in the effects of malnourishment (decrease in the probability of being stunted, underweight or chronically malnourished)	Moderate	Positive effects found in three studies (2 C-RCT, 1 CBA) ; the only one negative outcome was found in a quasi-C-RCT (more risks of bias) which might have arisen from misunderstanding on programme conditions

BACKGROUND

Cash transfers are defined as the provision of assistance in the form of cash, with the objective of increasing the household's real income. They are generally made to the poor or to those who face a probable risk of falling into poverty in the absence of the transfer. Conditional cash transfers (CCT) have recently been introduced in several Latin American countries. Based on a similar principle, they provide monetary transfers to households on the condition that they comply with a set of requirements. Conditional cash transfers are increasingly being promoted over in-kind transfers and unconditional for several reasons. First, unlike in-kind transfers, which pre-determine the provision of a particular commodity, CCT are more flexible safety nets, that allow individuals to buy items according to their needs or preferences. Secondly, in-kind transfers have sometimes been criticised for the important logistical costs they usually entail (e.g. transportation costs to bring bulky products to remote areas, costs associated with loss of food, etc.). In addition, the conditionalities of CCT programmes have provided useful arguments against the critiques sometimes made to social transfers, which is that they are useless and a waste of resources. Promoters of CCT have emphasised that conditional transfers were a direct investment in human capital, from which there would be some long term benefits. Finally, CCT have been advocated for being more ambitious than unconditional transfers, since they are an incentive for households to adopt a behaviour that would positively impact on their well-being.

CCT programmes were developed in Latin America in the mid-1990s to counteract the devastating social and economic effects of the debt crisis of the 1980s and the financial crises of Mexico (1995) and Asia (1997). Some municipalities in Brazil introduced conditional cash transfers as early as 1995. In 1997, Mexico started a large-scale pilot programme, the Programa de Educación, Salud y Alimentación (Progresa, later called Oportunidades), which was extended at national level two years later. The widespread positive results from Progresa served as an encouragement to extend these programmes in many other countries.

CCT programmes are justified on the grounds that demand-side subsidies are necessary to address particular constraints and bottlenecks of social services provision. Market failures are usually cited as the main economic rationale: the consumption of some goods creates positive externalities that justify their subsidy, in order to maximise their uptake by the population. This is the reason why CCT programmes usually aim to increase demand for preventive health services and education, because such programmes generate positive spillover effects. CCT are also supposed to help overcome different barriers to access to social services. Monetary transfers provide households with money to compensate for indirect costs (e.g. costs of transport, or food during hospitalisation) or opportunity costs (for example the loss of income due to the time not spent on the usual income-generating activity) related to seeking health care or sending children to school. Finally, these programmes are often justified by social equity concerns. As poor people usually accumulate the detrimental effects of different barriers to access, CCT mechanisms are seen as a single transfer mechanism that can "level the playing field" and redistribute endowments in order to equalise opportunities in a society.

It is important to underline that the overall objective of recent CCT programmes is usually to provide support to families living in extreme poverty, in order to develop the long-term potential of the household members. Therefore, their aims are broader than those of scaling-up effective (preventive) health interventions, and include the larger issue of human capital building. They not only provide a financial incentive for households to comply with beneficial behaviours, but also usually entail free access to basic health services. Consequently, CCT can create a positive effect on the demand for health services by reducing or eliminating financial barriers to access, and potentially have positive effects on incomes for beneficiary households.

CCT have grown very popular in the recent past, and they have started to develop in many developing countries, notably outside of Latin America. Examples include conditional incentive programmes for pregnant women to deliver in health facilities in India and Nepal (Ministry of Health and Family Welfare 2005; Powell-Jackson 2009), but also programmes in Ecuador, Jamaica, Turkey and Kenya. Future impact assessments of their benefits should contribute to the current debate and knowledge on the issue, and will be included in an updated version of this review.

No systematic review has been done on this subject, although a couple of narrative reviews exist (Ensor 2003; Rawlings 2005). This review was published in a past issue of JAMA (Lagarde 2007).

OBJECTIVES

This review aims to assess the effectiveness of conditional monetary transfers in low and middle income countries to improve the health outcomes of populations and their access to health care services. Changes in access to health services will be evaluated through changes in the use of health services and changes in health care expenditures.

METHODS

Criteria for considering studies for this review

Types of studies

We examined all studies that met the Effective Practice and Organisation of Care Group (EPOC) inclusion criteria for study design and compared the effects (on a determined range of outcomes)

of offering conditional cash transfers to the populations with the absence of such incentive.
We included three types of studies:
1. Randomised controlled trials (RCT) or cluster-randomised controlled trials (C-RCT)
2. Controlled before and after studies
For these two types of studies, the comparison intervention was the provision of the same type of health services (by the same providers), but without offering incentives to the populations to come and use health services.
3. Interrupted time-series analyses provided that:

- the point in time when the intervention/change occurred was clearly defined;
- there were at least three or more data points before and after the intervention.

Types of participants

The review includes only studies that took place in low and middle income countries as defined by the World Bank (World Bank 2006).
Units of study were the populations who would potentially access health services. Issues of interest were the populations' access to health services, their utilisation patterns, and possibly their health outcomes. Hence, "participants" included users and non-users of health services, as well as institutions such as health facilities, where utilisation data could have been collected.
We permitted study designs that used facilities or districts as units of allocation and were thus cluster trials.
We included studies on all types of providers (private, governmental, NGOs). We did not limit the scope of our study to a particular level of health care delivery and all types of health services were eligible for inclusion.

Types of interventions

To be included, interventions had to meet the following criteria:

- consist of direct monetary transfers made to households.

We did not include in-kind transfers because the review focused on the effectiveness of financial incentives, which could be easily compared to each other;

- the transfer had to be conditioned on a particular behaviour or action (e.g. visit to a health facility for regular check ups) - unconditional transfers were not considered.

Types of outcome measures

Primary outcomes

Primary outcomes were changes in use of health services and changes in health outcomes.

- Only objective measures relating to the final consumption of health services were taken into consideration. Access to care can be measured by changes in utilisation patterns of health facilities or services (immunisation coverage, number of visits, rates of hospitalisation, numbers of people having bought an insecticide-treated net, etc.) and/or equivalent information collected directly from the study population through rigorous survey techniques. Information related to distance travelled or travel time was outside the scope of this review.

- Changes in health outcomes, measured by morbidity and mortality incidence (broken down by age group, sex, etc.) were also considered where available.

Secondary outcomes

Secondary outcomes included health care expenditures and outcomes reflecting changes in equity of access:

- Health care expenditure was considered when it reflected direct (and indirect) costs borne by the patient and/or her family.

- Changes in equity of access - increased access for disadvantaged groups or a reduction in gaps in coverage - could also be an important outcome measure. This required a preliminary analysis and categorisation of the population of interest along a socio-economic scale. We accepted any relevant methodology (e.g. wealth/asset index) provided it was rigorous and described in detail.

Objective measures of utilisation, performance or patient outcomes were required. We did not include studies based only on measurements of attitudes, beliefs or perceptions.

Search methods for identification of studies

Electronic searches

The search to identify studies for this review was initially done as part of a much wider review on health financing mechanisms dealing with the effects of several financing strategies (Lagarde 2006). The broad review has been split into several sub-reviews, including the present one. Therefore the search methodology included terms that encompass a broader scope that the one defined for this review.
The following electronic databases were originally searched without language or date restrictions (the dates indicated refer to the original searches performed):
PubMED, 11/11/2005
EMBASE (Athens), 19/04/2006
Popline, 08/12/2005
African Healthline (bibliographic databases on African health issues), 28/04/2006

IBSS (International Bibliography in Social Sciences, Athens interface), 19/04/2006
The Cochrane Central Register of Controlled Trials (CENTRAL), 20/01/2006
The Database of Abstracts of Reviews of Effectiveness and the EPOC Register (and the database of studies awaiting assessment), 20/01/2006
BLDS, 03/11/2005
ID21, 24/11/2005
ELDIS, 25/11/2005
The Antwerp Institute of Tropical Medicine database, 26/01/2006
Jstor, 26/01/2005
Inter-Science (Wiley), 16/12/2005
ScienceDirect, 16/12/2005
IDEAS(Repec), 20/01/2005
LILACS, 19/04/2006
CAB-Direct (Global Health), 17/04/2006
Healthcare Management Information Consortium (HMIC), 17/04/2006
World Health Organization Library Information System (WHOLIS), 18/04/2006
MEDCARIB, 19/04/2006
ADOLEC, 19/04/2006
FRANCIS, 16/12/2005
BDSP, 16/12/2005
USAID database, 04/11/2005.
An updated search was done in May 2009. The detailed search strategy used for this updated search is indicated in Appendix 1. We have identified a few other studies as potentially relevant for this review and these will be assessed for inclusion in the next version of this review. These studies can be found under Studies awaiting classification.
The PubMED search strategy was mainly developed using reviews cited in the background section of the protocol and their references (Lagarde 2006).
The original search strategy was developed without the usual EPOC methodology filter. However, the updated search strategy included such a methodology filter to limit study designs to randomised trials, controlled trials, time series analyses and controlled before-after studies.
The detail of the search strategy used for PubMed for can be found in Appendix 1. We translated this search strategy into the other databases using the appropriate controlled vocabulary, as applicable. Search strategies for electronic databases used selected index terms and free text terms. In addition, we used a number of free text terms to browse more simple databases or lists of studies: "health financing", "contracting", "pay for performance", "outsourcing", "supply-side incentive", "performance payment", "output-based payment", "P4P".

Searching other resources

We also searched the following grey literature resources between December 2005 and February 2006.

- Websites and online resources of UNICEF, USAID and the World Bank, Partnerships for Health Reforms, Abt Associates, Management Sciences for Health (MSH), Oxford Policy Management, Save the Children, Oxfam, and a number of other networks or organisation websites such as The Private Sector Partnerships-*One*, the Indian Council for Research on International Economic Relations, Equinet - The Network for Equity in Health in Southern Africa, the Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

- Websites and online resources (working papers) of numerous university research centres: among others the Institute of Social Studies, The Hague, the University of Southampton, the International Centre for Diarrhoeal Disease Research and the Centre for Health and Population research, Dhaka, the Boston University Institute for Economic Development, Harvard Initiative for Global Health, Cornell Food and Nutrition Policy Programme, the Institute of Development Studies (University of Sussex), the London School of Hygiene and Tropical Medicine (HEFP website), the Institute of Policy Analysis and Research (IPAR) in Kenya, the Development Policy Research Unit of the University of Cape Town, the Netherlands Institute for Southern Africa.

We screened the reference lists of all of the relevant references retrieved. We contacted authors of relevant papers or known experts in the fields of interest to identify additional studies, including unpublished and ongoing studies.

Data collection and analysis

Selection of studies

Two authors (ML and AH) independently selected the studies to be included in the review. We resolved any disagreements by discussion.

Data extraction and management

We extracted the following information from the included studies using a standardised data extraction form:

- type of study (individual or cluster randomised trial, controlled before-after, interrupted time series);
- duration of the study;
- study setting (country, key features of the health care system, external support, other health financing options in place, other on-going economic/political/social reforms);
- characteristics of participants (catchment area size, characteristics of the population, existing health facilities, etc.);
- characteristics of the intervention (relative and absolute amount of the transfer, conditions to be fulfilled);

- main outcome measures and results.

Tables were prepared for each sub-category of intervention, including the following information: study ID, country and date of the intervention, characteristics of the intervention and the individual (facility/population level) and external/national level, health outcomes.

Assessment of risk of bias in included studies

We adapted slightly the standard criteria recommended by EPOC to match the particularities of the studies found in the field of interest (EPOC 2002). For example, criteria about following-up patients or doctors were not relevant as most of the studies used population survey data. Follow-up surveys, when carried out, would therefore not be done with the same population, but with a new random sample. In addition, we added some specific criteria to account for some of the limitations of studies found (e.g. no statistical analysis performed or failure to account for clustering effects). Appendix 2 presents the detailed list of all quality criteria used, and explains the amendments introduced to the original EPOC criteria for each type of design.

The criteria for RCTs and C-RCTs were:

1. Concealment of allocation
2. Protection against exclusion bias
3. Appropriate sampling strategy
4. Appropriate analysis
5. Reliable primary outcomes measures
6. Protection against detection bias
7. Baseline measurement of outcomes
8. Protection against contamination

The criteria for CBA studies were:

1. Baseline measurement of outcomes
2. Baseline characteristics of studies using second site as control
3. Protection against exclusion or selection bias
4. Protection against contamination
5. Reliable primary outcomes measures
6. Appropriate analysis of data

The criteria for ITS studies were:

1. protection against changes
2. appropriate analysis of the data (or re-analysis possible)
3. Protection against selection bias
4. Reliability of outcome data
5. Number of points specified
6. Intervention effect specified
7. Protection against detection bias

Our assessment of the risk of bias in the included studies is presented in Table 1.

Data synthesis

Due to the diversity in the nature of interventions and outcomes reported in the included studies, it was not appropriate to statistically combine the results of the studies.

For all studies, we tried to report the outcome measures before and after the interventions, but these were not systematically available. Ideally, we would have calculated the impact of the studies by comparing the outcome measures in both intervention and control areas. This was not made possible, due to insufficient data reported in the original papers.

All the reported estimates of effects therefore come directly from the original studies. We reported only the estimates of effects that accounted for differences in baseline outcomes. Some studies controlled for other baseline characteristics (e.g. socio-economic individual characteristics of survey participants). This was usually performed in a regression analysis, and therefore the estimated effect represents a change in the outcome of interest (e.g. percentage points if the outcome was a proportion, increase in the probability of the dependent latent variable of the regression is a probability).

RESULTS

Description of studies

See: Characteristics of included studies; Characteristics of excluded studies.

This review summarises the results from ten papers, reporting the results from six studies.

Study designs

We included ten papers reporting results from four randomised trials, and two controlled before and after studies in the review.

Characteristics of settings and patients

Thornton 2006 reported the results of a small-scale experiment in Malawi. All other included studies reported results from large-scale experiments or projects in Latin American middle-income countries: Mexico (the 5 papers that reported results from Progresa: Barham 2005a; Behrman 2005; Gertler 2000; Gertler 2004a; Rivera 2004), Brazil (Morris 2004b) Nicaragua (Maluccio 2004), Colombia (Attanasio 2005) and Honduras (Morris 2004a). We reported differences in beneficiaries in the following section, as targeting strategies were one of the core features of these interventions.

Characteristics of interventions

In all studies, the intervention was targeted at individuals, but was sometimes provided at the community level to all individuals.

In CCT programmes, even though all household members are likely to benefit from the monetary transfers, target populations are those who have to abide by some conditionalities. Thornton 2006 focuses on people who have been tested for HIV. All Latin American CCT programmes, which are very similar in their conception, target poor and disadvantaged groups, mostly infants and children, and pregnant and lactating women.

The benefit packages of CCT programmes vary not only across programmes, but also with the characteristics of the beneficiaries within a programme. We chose to report the main differences, but various operational dimensions, like targeting or frequency of transfers, are also noted.

All Latin American studies included are concerned with programmes that aim to strengthen the human capital of beneficiaries (in general children); therefore they provide cash, free access to health services (preventive health check-ups for infants and pregnant women) and sometimes also nutritional supplements. Further, there is an education component in all schemes (see Table 2 for a description of the benefits of each package).

In absolute terms, transfer sizes of the packages by households are quite variable, making it difficult to compare the effects of the packages due to the differences in economic environment across countries. Comparing the relative share of beneficiaries' income could have been useful, but unfortunately these data were not available in most studies.

Conditionality

All studies from Latin America described interventions combining nutrition, education and health conditionalities (see Table 2 and Table 3), as their objective is to improve the human capital of beneficiaries. These programmes therefore have several requirements. Monetary transfers are conditional on health check-ups and school attendance at primary level for young children and some programmes add a health education component for the parents, secondary education for older children and nutrition supplements. Unlike the Latin American programmes, Thornton 2006 tested the effectiveness of an incentive to be HIV tested and to collect the result.

Characteristics of outcomes

Health care utilisation is reported as visits to health facilities, which usually constitute one of the conditionalities of the programmes (Attanasio 2005; Gertler 2000; Maluccio 2004; Morris 2004a; Thornton 2006).

Other related outcomes include immunisation coverage, which is reported in four studies (Attanasio 2005; Barham 2005a; Maluccio 2004; Morris 2004a.

Two categories of health outcomes were found among the studies. A first group consisted of objective measures: height, weight, and their corollary measures of height-for-age Z-score, weight-for-age Z-score, haemoglobin value, prevalence of anaemia stunting or wasting. These were reported by Attanasio 2005; Behrman 2005; Gertler 2004a; Maluccio 2004; and Rivera 2004. The second set of health outcomes involved the probability of having reported illness symptoms or having fallen ill in a recall period (Attanasio 2005; Gertler 2000; Gertler 2004a).

None of the studies reported effects on patient health expenditures (although some reported details of other types of household expenditures). No equity outcome was included even though some studies included results broken down by groups which are sometimes used as proxies for socio-economic categorisation (e.g. rural/urban). However, as indicated in the inclusion criteria, these were not within the scope of equity outcomes we had defined.

Results of the search

The main literature search (for all financing strategies, not only conditional cash transfers) using electronic databases and websites resulted in more than 24,000 references to sift.

We identified 29 papers that were potentially relevant for the review. After further examination, 19 of these studies were excluded. The Characteristics of excluded studies table provides detailed reasons for exclusion. Most studies did not meet our study design criteria: they were primarily descriptive case studies, reviews, modelling or cross-sectional studies. Some studies were excluded on the grounds that their focus was not conditional cash transfers, but in-kind transfers, or non-conditional transfers.

Risk of bias in included studies

Methodological quality of included studies

Accounting for clustering effects in cluster randomised trials

Many of the studies were cluster-randomised trials, whose design and analysis needed to address clustering effects. Behrman 2005; Gertler 2004a; Morris 2004a; and Rivera 2004 all reported having taken clustering into account in their analyses (see Table 4 and Table 1). On the other hand, only Morris 2004a and Rivera 2004 mentioned that clustering was also addressed in the sample size calculation and design. Given the numbers of clusters and participants in most studies, however, it is unlikely that the statistical power of the analysis was seriously affected.

Quality of randomisation and implications for the analysis

Some randomised trials did not provide a baseline (Table 4 and Table 1), and the EPOC quality criteria penalise this absence. The usual argument supporting the absence of the baseline is that if the randomisation is done well enough, it eliminates any potential differences between the control and intervention sites at the

baseline. Therefore, the differences found after the intervention capture only the impact of the programme.

However, a methodological analysis of Progresa surveys (Behrman 1999) rejected the hypothesis of random assignment of Progresa cash transfers at household levels, despite random assignment at community level. Behrman 2001 further proved that similar problems hampered the nutritional sub-study (INSP surveys used by Behrman 2005; Gertler 2004a; and Rivera 2004). Both socio-economic characteristics and unobserved characteristics of households (e.g. level of concern of parents for their children, health status of children, etc.) may have influenced the eventual benefit received from the programme, and should therefore be accounted for in the analysis. Consequently all analyses of Progresa reporting results at individual level are susceptible to bias if they did not attempt to control for baseline differences.The nutritional sub-study that was done for Progresa to assess its impact on nutritional status took place after the beginning of the programme. We included these data whilst bearing in mind the potential bias stemming from the absence of a baseline. Other reports and surveys on the whole experiment provided enough details to inform potential flaws.

The biased distribution of financial incentives reported in Thornton 2006 also proves that it may be difficult to conform to the necessities of randomisation at all stages of the implementation of such a programme.

Leakage problems

In addition to the non-random assignment across households underlined by Behrman 1999, it was observed that the 'papilla' (the nutrition supplement provided by Progresa) was sometimes given to children who were not supposed to receive it (in control localities), and that supply-side bottlenecks led to discretionary choices from local administrators regarding the beneficiaries (Behrman 2005). Results from Rivera 2004 regarding intake of 'papilla' suggest that the allocation of the nutrition component of Progresa was far from being systematically followed: not only did they confirm leakage in control zones (with data from an INSP survey), but they also showed that less than 60% of children were actually consuming papilla regularly which may be due to the supply-side shortages mentioned earlier or some failure to comply with this condition from households. These factors would lead to an underestimation of the impact of the nutritional supplements by standard analyses.

This is another argument in favour of an individual-level analysis, as carried out by Behrman 2005, which actually confirms papilla intake (in addition to being a child from an eligible household residing in a treatment community).

Attrition bias

Attrition bias was found in the nutritional surveys done for the Progresa programmes. Attrition problems due to poor quality data and survey design problems are explained in detail in Behrman 2001. The magnitude of the attrition bias is confirmed by Rivera 2004. Although the authors tried to limit attrition bias by using datasets from 2000 and 1998, only 82% of the original cohort was assessed in 2000, and of this subgroup only 75% could be used for the analysis. Behrman 2001 showed that the attrition effect between 1998 and 1999 had resulted in an over-representation in the sample of children with a poor nutritional status. It is likely that a similar phenomenon occurred as a result of the attrition observed between 1998 and 2000. This is partially confirmed by the differences in mean height-for-age Z-score given in the two studies at 'baseline': while Behrman 2001 reported a mean of -0.24 for children aged 6-12 months old measured in 1998, Rivera 2004 reported a worse average nutritional status at baseline with a mean difference of -1.06 between the intervention group of children receiving Progresa and the comparison group.

This bias may have led to an over-estimation of the impact of Progresa by Rivera 2004, as individual characteristics were not accounted for and children who were worse off before the intervention benefited more from it. However, Behrman 2001 tried to compensate for these imbalances.

Synthesis of quality assessment

Two studies present some minor limitations and were deemed as presenting moderate risks of bias (Maluccio 2004; Morris 2004a). All other studies presented high risks of bias after applying the quality criteria. However, the authors of the recent publications were in general aware of the limitations of the studies and made efforts to compensate statistically for potential biases, which were generally due not to poor design but to the fact that the health workers implementing the CCT schemes tried to ensure that the poorest received the benefits of the treatment even if this meant overriding the random assignment at the individual level (Progresa).

Effects of interventions

See: **Summary of findings for the main comparison**

Impact on uptake of health services

Thornton 2006 reported a positive impact of a financial incentive conditional on getting people who had been tested for HIV to return for their results. Controlling for distance, she found that the proportion of people who went to collect their results increased by 27 percentage points in the intervention group compared to the treatment group. This study also showed that there is no differential impact of monetary incentives according to their amounts (from US$1 to US$3[1] per result collection), although the numbers receiving the higher payment were limited.

Gertler 2000 showed that in areas where cash transfers were offered to the population, there was an increase of 2.09 in the number of daily outpatient visits to health facilities.

Based on statistics from health centres in the control and the treatment areas, Morris 2004a found that use of services increased significantly for pre-school children but there was no significant increase in the uptake of antenatal care or 10-day postnatal check-ups (see Table 5). Based on mothers' reports, the same programme was found to have a significant impact on the uptake of antenatal care and routine well-child check-ups and growth monitoring visits for children (increased by 18, 19 and 15 percentage points respectively). However, there was no effect on the uptake of the 10-day check-up after delivery. The results on antenatal care uptake are at odds with registries.

Finally, another study from Nicaragua, displayed a positive impact on health care utilisation, with an increase by 19.5 percentage points after 1 year and 11 after 2 years in the proportion of infants (0-3 years old) taken to health centres in the past 6 months (Maluccio 2004) (see Table 5). The dip in estimated effect between the first and second year is due to an increase in the rates reported in the control group. The price year was not specified although it is probably 2005 (the year that the study was implemented).

Impact on health outcomes

Three studies reported health outcomes, measured as self-reported episode of illness in population surveys. See Table 6 for more details.

Attanasio 2005 reported mixed results regarding the impact on the probability of children suffering from diarrhoea. While Familias in Accion seems to have reduced the probability of reported diarrhoea symptoms for children aged under 48 months living in rural areas, older groups did not display any changes. The study also failed to detect any effect on the probability of respiratory symptoms being reported by children.

The analysis of a Nicaraguan programme by Maluccio 2004 found it did not have an impact on anaemia or mean haemoglobin among infants aged 6 to 59 months old.

The analyses performed by Gertler 2004a concluded that Progresa led to a 22% decrease in the probability of children younger than 3 years of age having been ill in the past month. It also showed that the longer the children have received the programme, the greater that beneficial effect.

In summary, available existing evidence shows that CCT programmes can have a positive impact on children's health outcomes, but this is neither systematic nor consistent across all age groups.

Impact on immunisation coverage

Four studies reported effects on immunisation coverage. See Table 7 for more details.

Barham 2005a reported mixed results of Progresa on immunisation coverage. The difference-in-difference estimators in OLS regressions showed a difference of 5 percentage points in TB immunisation coverage for Progresa children aged 12 to 23 months old (baseline of May 1998), 6 months after the beginning of the intervention. However, this was due to a decrease in coverage in control zones, and the difference was no longer significant once the 'control' children recovered from the drop 12 months after baseline. Measles vaccination increased by 3 percentage points for children aged 12 to 23 months old 6 months after the beginning of the programme, and by 6 percentage points after 12 months in low coverage villages.

Results from the study in Honduras (Morris 2004a) showed an increase of 6.9% in the coverage of the first dose DTP/pentavalent vaccine among children but not in tetanus immunisation among pregnant women, nor in measles vaccination among children. Familias en Accion in Colombia also increased the probability that 24-month-old children had complied with DPT vaccination schedule (Attanasio 2005).

The Red de Proteccion Social (RPS) programme in Nicaragua had no significant impact on vaccination coverage. However it seems that this was mainly due to a concurrent increase in vaccination coverage in both the intervention and the control areas due to, which both benefited from supply-side incentives strengthening the procurement of vaccines.

Impact on anthropometric or nutritional outcomes

Six papers reported outcomes on anthropometric measures and nutritional status from four different CCT programmes.

The results obtained by Attanasio 2005 on the short-term impact of Familias in Accion in Colombia showed mixed conclusions about the impact of monetary transfers on the nutritional status of children. They show a positive impact on nutritional status for children under 24 months (see Table 8), and an increase of 0.58 kg in newborn weight in the urban areas of treatment localities. However, no impact was detected on the nutritional status of children older than 24 months, or on newborn weight in rural areas.

The analysis of a Nicaraguan programme by Maluccio 2004 showed positive effects. It reduced the magnitude of stunting (net average improvement of the height-for-age score by 0.17*)* and the proportion of under-weight children aged 0 to 5 years old (a net impact of 6 percentage points after 2 years). However, it did not have an impact on the proportion of wasted children aged 0 to 5 years old.

The evaluation of the Brazilian programme (Morris 2004b) showed no effect on height-for-age measures and even a negative impact on weight-for-age for children under 7 years old (see discussion).

Finally, three papers reported findings for the Mexican programme, using different groups of reference for their analyses.

Rivera 2004 provided results mainly comparing a group having received Progresa for 2 years against a "crossover" one that received it for one year only. Their analysis showed a significant impact on growth for the youngest children in the poorest households (aged less than 6 months old at baseline in 1998): these infants gained 1.1 cm more after 2 years of the Progresa programme than those in the "crossover" group. However, they found no difference for older

children (aged 6 to 12 months at baseline) or for the youngest coming from less poor families. The authors also mentioned that, based on results from the 1999 survey, anaemia prevalence among children who have received Progresa for a year (44.3%) is significantly inferior to that among the "crossover" group (54.9%). In contrast, this difference had disappeared once the "crossover" group had entered the programme for a year.

Using linear regression models applied to post-intervention data only, Gertler 2004a reported a similar result on height gain. They found that beneficiary children aged 12 to 36 months in October to December 1999 were 0.96 cm taller than other children, and were 25% less likely to be anaemic. No difference in stunting was detected between treatment and control groups.

Behrman 2005 also found equivalent results on child growth with a different method, controlling for several sources of bias. Their findings showed a positive effect of Progresa on the height of children 12 to 36 months old: they indicated a growth gain of 1.02 cm more than children in the same age range who did not receive Progresa. They further showed that the programme appeared to have a significant effect for children aged 24 to 36 months (through a height increase of 1.22 cm) but not for other age groups.

DISCUSSION

Despite its widely acclaimed design, we found that the Progresa trial was undermined by a number of methodological issues that hampered the interpretation of its results. However, it was certainly a milestone that influenced the implementation and evaluation of many other programmes. Unlike other financing schemes, conditional cash transfer programmes have in general been evaluated by well-designed and executed evaluations, compared for example with user fees where the quality of studies was much poorer (Lagarde 2006). No less than 4 out of the 6 evaluated programmes we included had been designed to be evaluated by randomised trials. The Progresa data were analysed by different groups using different approaches and sometimes failing to reference each others' publications. Multiple statistical comparisons were undertaken without adjustment and this could have led to spurious 'statistically significant differences'. In addition, some conclusions were based on sub-group analyses (e.g. the effects of nutritional interventions at different ages). This again can lead to spurious 'significant' results. However, when weaknesses or bias arose, evaluators sometimes made strenuous efforts to correct them or to account for confounding factors (Attanasio 2005; Behrman 2005; Gertler 2004a; Maluccio 2004). We therefore concluded that, despite the remaining problems, the overall risk of bias is relatively moderate, particularly in light of the consistent effects in a number of different settings and especially compared to the evaluation of the effectiveness of other mechanisms. Overall this body of evidence finds that conditional cash transfers can be effective means to increase health service utilisation, health outcomes and nutritional status of children, although the significance and size of effects varies (see Summary of findings for the main comparison).

CCTs have been widely introduced as pro-poor policies, in particular because in many Latin American countries they only target the poorest groups of the population. This might be one of the reasons why equity of the programmes were not particularly studies (we did not find adequate detailed analysis of outcome measures by socio-economic groups, only some hints at results per sub-groups reported here). However, a couple of studies alluded to findings per sub-groups, and reported mixed results. In Nicaragua, the study reported that the increase in household expenditures was greatest for the poorest group as was the uptake of preventive services for infants (Maluccio 2004). However, nutritional benefits drawn from Progresa were greater for children whose mother had more than five years of schooling (Behrman 2001), which can be used as proxy to distinguish more disadvantaged groups.

There might be a danger that unanticipated perverse effects may occur, as illustrated by the study by Morris 2004b where some unexpected decrease in health outcomes amongst children have been explained by a seeming misunderstanding of the requirements by mothers, who would have kept their child malnourished in order to retain eligibility for the programme. Considering that this study was not a pure C-RCT design, it is also possible that this effect was subject to bias, and it should not necessarily be trusted. However, this underlines an issue that has been highlighted in the literature on incentives, where gaming strategies and unanticipated consequences have often been observed (Courty 2004; Propper 2003).

The good functioning of CCTs relies on different elements. First, in many countries, it has relied on the efficient targeting of poorer groups. However, this is not necessarily a requirement for a Conditional Cash Transfer Programme. For example, some south Asian countries have recently started to offer cash incentives to all pregnant women who would go and deliver in a health facility. Another element that is key to the good functioning of CCTs is the capacity of the programme implementer to monitor whether the requirements are met or not by the beneficiaries. To do this, it is essential to define as requirements some behaviours or actions that are easily controlled. The size and timing of the incentive is another dimension of the CCT programmes. Because they were initially conceived as social transfer programmes, incentives in many Latin American countries were calculated in relation to the poverty level. In some more recent programmes (not reviewed here), they are being designed in relation to the financial costs (indirect or direct) linked to accessing the health care services (Ministry of Health and Family Welfare 2005; Nepal 2005). Finally, CCT programmes assume that the provision of the health services are adequate, and ready to address the potential increase in the demand of services induced by the cash transfer programme. In fact, some programmes in Latin America (Maluccio 2004) also introduced some supply-side interventions to strengthen the delivery of health services in intervention areas.

In theory, the impact of Conditional Cash transfer programmes can be altered by a number of factors.

The success of conditional cash transfers is probably dependent on the magnitude of the barriers to accessing services on the demand-side. If the main reasons for poor uptake of health services are linked to financial barriers, then CCTs are likely to be effective mechanisms. However if there are few obstacles impeding access to care, as demonstrated for example by high levels of uptake of health services, CCTs will be less, if at all, successful. This is what the experiences in Latin America suggest, where CCTs failed to have an impact on immunisation rates, when the rates were initially quite high.

Similarly, if the obstacles to health care utilisation by the population are on the supply side (lack of drugs, low density of facilities) CCTs will be less effective.

In fact, the quality and availability of health services is probably a pre-requisite to the success of CCT. There is ample evidence in the health services literature of households avoiding health services for their poor quality. It is likely that even financial incentives would not be sufficient (nor necessarily recommended) to encourage the use of poor quality services.

Secondly, the relative size of effects of CCT programmes is certainly linked to the nature and types of requirement required. For example, some programmes asked the beneficiaries to attend nutrition and health education workshops as part of the requirements, while others did not. It is likely that attending such workshops could have an impact on health behaviours amongst households, thereby influencing health outcomes.

Finally, the level of the financial incentive used might modify the size of the effect. It is likely that the larger the financial incentive the greater the likelihood of individuals to comply with the requirement, hence the more likely the entire targeted population will be reached.

The lack of empirical evidence for these issues, which are key to understanding under which conditions CCTs might work more or less efficiently, continues to shape the agenda for future research.

AUTHORS' CONCLUSIONS

Implications for practice

The CCT programmes implemented so far have not been targeted at reducing barriers to access to curative health services, as their health focus was on preventive services or health promotion, or both. Based on the evidence reviewed here, it seems that CCT programmes can be effective strategies to increase the uptake of preventive services which were already free. Their implementation in a context where services are not free remains to be tested.

Some results also suggest the limitations of CCT to achieve some results. For example, even with important financial incentives, some CCT programmes failed to improve vaccination coverage. It is essential for policy-makers, before embarking on a CCT programme that might be very costly, to analyse carefully the various barriers to health services faced by the population. If supply-side barriers (e.g. lack of vaccines or drugs) are responsible for a low uptake of services, CCTs are unlikely to provide a relevant solution, or at least not the only one.

Despite the highly publicised success of CCT programmes, at least partly confirmed by this review, several questions remain regarding their feasibility in poorer settings, and policy-makers in such environments should be aware of a number of issues. First, policy-makers willing to introduce CCTs should probably ensure a minimum quality on the supply side, so that the intervention can effectively address the demand-side obstacles. Most successful CCT programmes reported in this review have been implemented in middle-income countries, which have relatively well-functioning health systems. Second, it is likely that the success of existing programmes has relied on effective mechanisms to target and monitor beneficiaries, as well as to transfer the money in a timely fashion. There are several examples of strategies (e.g. exemption policies) in which such elements have failed to work correctly in low-income settings, and it is important to acknowledge their importance in the success of CCTs. Finally, policy makers should carefully study the cost implications of CCT programmes, in particular if no targeting mechanism is put in place. Indeed, some have shown that not targeting the groups who have the least access to health services will increase the marginal cost per person covered (Lagarde 2007), and therefore increase the opportunity cost of CCT programmes.

Implications for research

The first area for further research on CCT relates to the question of their cost-effectiveness. Given the financial constraints of most Sub-Saharan African countries, providing schools and health care facilities may be a more effective allocation of public spending than cash transfers. There has not been any study in the CCT literature that tried to address that issue. A careful analysis of the costs and benefits of these programmes versus other traditional delivery of health care services is urgently needed.

A second area of research on the effects of conditional cash transfers relates to a better understanding of the different pathways through which CCTs work. The relatively well-designed evaluations we presented do not necessarily explain why such monetary incentives are working, and in particular whether they effectively help overcome all financial barriers (would they have worked with non-free services?) and/or other types of obstacles such as cultural barriers, e.g. access to child and maternal health services in cultures where access is controlled by male members of the family.

Furthermore, so far, the positive impact of CCT programmes is

generally linked only to the presence of the financial incentives. But there are other potential reasons explaining why health outcomes have increased. As underlined by Gertler 2004a, the multiple components of the programmes may play a role, and their respective weight and role has so far not been isolated from that of the financial incentive. It would be interesting to know for example, if nutritional outcomes amongst children have been improved by the wealth effect of the financial bonus (allowing to buy more and better food as it was proved), or if these were improved by attendance of health and nutrition workshops in some programmes and by nutritional supplements provided to some children in other programmes.

A final area for future research is the issues of the relative effect of CCTs for different levels of incentives or different socio-economic groups. So far, only one of the evaluated programmes varied the size of cash transfers (Thornton 2006), but it did not assess the relative impact of those levels for different socio-economic groups. A better knowledge of the presence and the existence of threshold effects, and the potential marginal positive effects of the cash transfer for various income groups is needed.

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Attanasio 2005

Methods	CBA
Participants	Country: Colombia Program: FA (Familias en Acion) Eligible households (poorest in selected municipalities).
Interventions	Cash incentives conditional on health and nutrition interventions for under 7 years old and school attendance for 8-18 years old
Outcomes	*Health services uptake:* Attendance of preventive care visits by children *Immunisation coverage:* Coverage of DPT vaccination (children) *Health outcomes:* Reported incidence of diarrhoea or respiratory diseases (children) *Anthropometric or nutritional outcomes:* Height for height for age Z-score Chronic malnourishment (children)
Notes	

Barham 2005a

Methods	C-RCT
Participants	Country: Mexico Program: Progresa Same as Gertler 2000.
Interventions	Same as Gertler 2000. Up-to-date immunisation was part of the health requirements to get the monetary transfers
Outcomes	*Immunisation coverage:* Coverage of DPT and Measles vaccination (children)
Notes	

Behrman 2005

Methods	C-RCT
Participants	Country: Mexico Program: Progresa Same as Gertler 2000.
Interventions	Same as Gertler 2000.
Outcomes	*Anthropometric or nutritional outcomes:* Height increase
Notes	Progresa reanalysed: importance of baseline measurement and unobserved characteristics Subversion of randomisation.

Gertler 2000

Methods	C-RCT
Participants	Country: Mexico Program: Progresa Eligible households among selected communities (selected on poverty grounds)
Interventions	Families enrolled received two types of cash transfers: universal (dependent on attendance at health facilities for all family members) and specific (associated with school attendance of school-aged children)
Outcomes	*Health services uptake:* Daily visits in the nearby health facilities *Health outcomes:* Reported morbidity (children)
Notes	The trial was funded by the Inter-American Development Bank.

Gertler 2004a

Methods	C-RCT
Participants	Country: Mexico Program: Progresa Same as Gertler 2000.
Interventions	Same as Gertler 2000.
Outcomes	*Health outcomes:* Reported morbidity (children) *Anthropometric or nutritional outcomes:* Height increase Prevalence of stunting

Gertler 2004a (*Continued*)

Notes	Progresa. Re-analysed without taking into account baseline data because some variables were not measured at baseline

Maluccio 2004

Methods	C-RCT
Participants	Country: Nicaragua Program: RPS (Red de Protecion Social) 42 comarcas chosen to participate in the pilot phase (see Table 2): ½ randomly selected for intervention.
Interventions	Monetary transfer for children under 5 conditional on attendance at educational workshop and bringing child to preventive health programme ('bono alimentario') Monetary transfer conditional on school attendance for 7-13 year old children ('bono escolar')
Outcomes	*Health services uptake:* Attendance of preventive care visits by children *Immunisation coverage:* Reported up-to-date vaccination schedule (children) *Anthropometric or nutritional outcomes:* Prevalence of stunting, wasting and underweight (children under 5) Height for Age Z-score (children under 5) Prevalence of anaemia
Notes	Limited external validity at national scale due to the purposive selection of areas (Table 2).

Morris 2004a

Methods	C-RCT
Participants	Country: Honduras Program: PRAF (Programa de Asignacion Familiar) Children and women from poor households, living in the beneficiary municipalities
Interventions	Either or both: 1/ two types of monetary incentives (for health and education); 2/nutrition interventions + resources for local health teams
Outcomes	*Health services uptake:* Attendance of preventive and prenatal care by women Attendance of preventive care visits by children *Immunisation coverage:* Coverage for DPT, Measles (children under 3) and tetanus toxoid (mothers)
Notes	Programme created in 1990 to mitigate the effects of structural adjustment

Morris 2004b

Methods	CBA
Participants	Country: Brazil Program: Bolsa Alimentação Pregnant and lactating women and children under 7 from low-income households
Interventions	Mothers received capped monthly transfers based on the number of beneficiaries
Outcomes	*Anthropometric or nutritional outcomes:* Height for Age Z-score (children) Weight for Age Z-score (children)
Notes	

Rivera 2004

Methods	C-RCT
Participants	Country: Mexico Program: Progresa Same as Gertler 2000.
Interventions	Same as Gertler 2000.
Outcomes	*Anthropometric or nutritional outcomes:* Prevalence of anaemia Height increase
Notes	Sub-study on a cohort of infants to investigate the nutritional impact

Thornton 2006

Methods	C-RCT
Participants	Country: Malawi Individuals who tested for STD.
Interventions	Voucher randomly given to individuals and exchangeable for cash if they come back to get their test results
Outcomes	*Health services uptake:* Proportion of people who went back to get the results of their tests
Notes	

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Ahmed 2003	Very broad intervention: unconditional cash transfer, free provision of health and hygiene services (latrine, pregnancy care, etc.)
Attanasio 2005b	Summary of Attanasio 2005.
Barham 2005b	Based on Progresa C-RCT but the analysis uses a mix of other data and ends up with a modelling study, not a (quasi) experimental one
Behrman 2001	Working paper of Berhman 2005; we used the published version
Behrman 2004	Outcome variables related to education.
Borghi 2005	Voucher scheme for STI clinic attendance and treatment for sex workers; no cash transfer and not appropriate design (cost effectiveness)
Chase 2001	The programme described does not focus on CCT.
Coady 2001	Modelling study on Progresa, outcomes not of interest.
Dupas 2005	Subsidised nets not cash transfer.
Gertler 2001	Same as Gertler 2000 but with an additional wave that took place after Progresa began to be offered to the former control areas
Gertler 2004b	Outcome variables not relevant for our review (children's development)
Levy 2003	Methodology report on a forthcoming evaluation of a CCT programme in Jamaica
Mushi 2003	Targeted subsidy voucher scheme for bednets.
Pritchett 2002	Targeted subsidies for health, no conditional cash transfers
Saadah 2001	Case study.
Sandiford 2005	Case study on a voucher scheme.
Savedoff 2000	Case study on health reform.
Schubert 2005	Case study, study design not relevant.
Weeden 1986	Excluded on the grounds that the nature of the transfer did not meet our definition of "conditional cash transfer". Indeed, participants in this programme did not receive direct incentives to modify their uptake of contraceptive methods. The financial incentive was indirect as the programme provided easier access to small loans on the basis of village-level and individual-level contraception uptake

DATA AND ANALYSES

This review has no analyses.

ADDITIONAL TABLES

Table 1. Assessment of risk of bias in included studies

Controlled before and after (CBA) studies								
Study ID	**Baseline characteristics**	**Equivalent control site**	**Protection against exclusion or selection bias**	**Protection against contamination**	**Reliability of outcome measures**	**Appropriate analysis**	**Overall: Limitations**	***Notes***
Attanasio 2005	NOT CLEAR	NOT CLEAR	DONE	DONE	DONE	NOT DONE	high risk of bias	Does not take cluster correlation into account. Differences at baseline between control and treatment sites are mentioned in the text but no further precision is given
Morris 2004b	NOT DONE	NOT CLEAR	DONE	DONE	DONE	DONE	high risk of bias	Baseline measures were reconstructed afterwards. Authors mention potential differences at baseline

Randomised controlled trials										
Study ID	**Concealment of allocation**	**Protection against exclusion bias**	**Sampling**	**Appropriate Analysis (clustering)**	**Quality/ reliability of the data**	**Protection against detection bias**	**Baseline Measurement**	**Protection against contamination**	**Overall: Limitations**	***Notes***
Maluccio 2004	DONE	DONE	NOT CLEAR	DONE	DONE	NOT CLEAR	DONE	DONE	moderate risk of bias	No reliability data presented on anthropometric measures, and no details on sampling

Table 1. Assessment of risk of bias in included studies (*Continued*)

Morris 2004a	DONE	DONE	DONE	DONE	DONE	NOT CLEAR	DONE	DONE	moderate risk of bias	The only potential bias would be a declaration bias as some outcomes for children are not objective but measured on mothers' declaration and registries of facilities (the authors mention a problem of over-declaration)
Thornton 2006	NOT DONE	DONE	N/A	N/A	DONE	DONE	NOT DONE	NOT DONE	high risk of bias	Biased allocation (non random) : higher number of vouchers given out than would be expected by chance even when nurses where threatened with termina-

Table 1. Assessment of risk of bias in included studies (*Continued*)

										tion of employment; no baseline (assumption is that randomissation of subjects is perfect); some contamination was noted
Gertler 2000	NOT DONE	DONE	DONE	DONE	NOT DONE	NOT DONE	DONE	DONE	high risk of bias	Clustering effects mentioned on some occasions but not everywhere - health utilisation data from registers not necessarily reliable (+ includes both beneficiaries and non-beneficiaries); reported illness by mothers potentially

Table 1. Assessment of risk of bias in included studies (*Continued*)

										biased - Berhman and Hoddinot (1999) show that assignment was random at community level but not at individual level
Barham 2005a	NOT DONE	NOT CLEAR	DONE	DONE	NOT DONE	NOT DONE	NOT CLEAR	DONE	high risk of bias	The author had to adjust and modify the collected data that suffered many methodological problems - suffers same problem as other Progresa (randomisation by community but not at individual level where the author consid-

Table 1. Assessment of risk of bias in included studies (*Continued*)

										ers the results) - problems of data recording (cumulative immunisation collected instead of those in the last 6 months) which can possibly lead to overestimates of the positive results - differences in measles immunisation rates - problem of attrition of sample (not real cohort or panel data)
Gertler 2004a	NOT DONE	DONE	DONE	DONE	NOT DONE	DONE	DONE	NOT DONE	high risk of bias	
Rivera 2004	NOT DONE	DONE	NOT CLEAR	DONE	NOT DONE	DONE	NOT DONE	NOT DONE	high risk of bias	Doubts on the quality of the data confirmed by Berhman 2001/

Table 1. Assessment of risk of bias in included studies (*Continued*)

										2005 (same set of data used): leakage problems, non-random assignment of papilla, attrition of sample, etc
Behrman 2005	NOT DONE	NOT DONE	NOT CLEAR	DONE	NOT DONE	DONE	NOT DONE	NOT DONE	high risk of bias	Leakage problems, non-random assignment of nutrition supplements, attrition of sample between 1998 and 1999 causing bias towards over-representation of poor households in the usable cohort, important differences

Table 1. Assessment of risk of bias in included studies (*Continued*)

										at baseline (see Table 1 in Behrman 2001) - however strenuous attempts made to reduce bias and overall risk of biased results may be moderate

Table 2. Context and intervention description

Study ID	Nature of intervention and control sites	Individual contextual factors	Broader contextual factor
Attanasio 2005 Colombia (Familias en Accion')	2 types of monetary transfers to the mothers: 1/ conditional on their children under 7 attending preventive health care visits (where children are weighed), they receive US$15 per month. Mothers are also encouraged to attend courses on hygiene, nutrition and family planning 2/ conditional on their children aged 7-17 attending at least 80% of school classes, they receive a monthly grant per child (approx. US$8 for primary school and US$16 for secondary school)	Eligibility: being Colombian citizen, living in a community where the programme is offered, having children under 18 and belonging to the lowest level of the official socio-economic classification Eligible municipalities had to have less than 100,000 inhabitants and have enough health and education facilities to guarantee the absence of supply bottlenecks. Of the 1024 municipalities in the country, 691 qualified	"Familias en Accion" programmeis funded by a IADB and WB loan approved in 2000 An important nutritional programmewhere children receive nutrition supplements, Hogares Communotarios (HC) had been implemented for 16 years in Colombia. Mothers had to choose between enrolling in FA or in HC, so the effects of FA are in comparison with HC
Barham 2005a Mexico ('Progresa')	Same as Gertler 2000	Same as Gertler 2000	Same as Gertler 2000
Behrman 2005 Mexico ('Progresa')	Same as Gertler 2000	Same as Gertler 2000	Same as Gertler 2000

Table 2. Context and intervention description (*Continued*)

Gertler 2000 Mexico ('Progresa')	Intervention: 2 cash transfers every two months; one general and one depending on school attendance - nutrition component: food supplements for children aged 4-23 months, under-weight children aged 2-4 years, and pregnant and lactating women in beneficiary households - health component: regular health care appointments in health centres for the whole family - education component:	506 out of 50,000 eligible villages were randomly chosen. Intervention groups: households selected from 320 communities Control group: 186 communities. Value of the transfers: US$25, adding 20-30% to the household income	The controls should originally have acted as controls for 2 years, but for political reasons intervention in control communities occurred in late 1999 so only 1 year ½ of comparison was possible and the control communities were therefore considered as crossover intervention communities after 1 year of observation
Gertler 2004a Mexico ('Progresa')	Same as Gertler 2000	Same as Gertler 2000	Same as Gertler 2000
Maluccio 2004 Nicaragua (Red de proteccion social')	The programme has 2 components : - a monthly "food security" cash transfer (bono alimentario= US$224 per year= 13% of total amount of household expenditures in beneficiary households before the program) conditional on attendance at monthly health educational workshops, on bringing their children under age 5 for free scheduled preventive childcare appointments (which include the provision of antiparasites, vitamins and iron supplement), on having up-to-date vaccination, and on adequate weight gain - A "school attendance" cash transfer every two months (= US$112 per year=8% of total amount of household expenditures in beneficiary households) , contingent on enrolment and regular school attendance of children aged 7-13. Additionally the household receives an	The programme is ultimately targeted at poor households living in rural areas, but the pilot phase analysed in this study occurred in 2 departments (Madriz and Matagalpa) in the Northern part of the Central Region. This region is the only one in the country where poverty worsened during 1998 and 2001 These pilot sites are not representative of the country situation: - within the 2 chosen departments, 6 municipalities were chosen (out of 20) because they had benefited from a previous programme that developed the capacity of the governing bodies to implement and monitor social projects: "it is possible that the selected municipalities had atypical capacities to run RPS" - in the chosen municipalities, 78-90% of the population is extremely poor/poor, compared	The "Red de Proteccion Social" (RPS) project is financed by a loan from the IADB The impact analysis of the pilot phase was done by the International Food Policy Research Institute (IFPRI) Possible detection of the "Hawthorne effect" since performance of the programme was slightly lower the second year Over the 2 years the actual average monetary transfer to households represented 18% of total household expenditure (similar to PROGRESA but 5 times larger than PRAF). The nominal transfers remained constant during the 2 years of the programme, thus the real value of the transfer declined by 8% due to inflation

Table 2. Context and intervention description (*Continued*)

	annual cash transfer per eligible child for school supplies Beneficiaries did not receive the food or education cash transfers if they failed to comply with any of the conditions	to 21-45% at national level 42 eligible areas (the neediest) were chosen for the pilot programme based on wealth index Private providers were specifically trained to deliver the specific health-care services required by the programme Incentives were also given to teachers to compensate for the larger classes they had after the implementation of the programme 10% of beneficiaries were penalised at least once during the first two years of the programme; 5% were expelled or left the programme Some conditions (adequate weight gain) were dropped at the end of the pilot phase and others were not properly enforced (up-to-date vaccination while there were delays in the delivery of vaccines) Delays occurred in the implementation of the health component which finally started in June 2001. Therefore when the first follow-up survey was realised in Oct. 2001 the beneficiaries had been receiving the transfers for the education component for 13 months and those for the health and nutrition component for 5 months only	
Morris 2004a Honduras ('PRAF')	Either or both of : 1) 2 types of monetary incentives: an education one conditional on school attendance of children aged 6-12; a health transfer conditional on monthly visits to health centres for children and pre-natal check-ups for pregnant women 2) Resources to local health	Value of the transfer: - Monthly health bonus=£2.50 (conversion rate late 2001) per pregnant women or child under 3, up to a maximum of two - Monthly education bonus= £3.70 per child between 6 and 12 enrolled at school, up to a maximum of 3	First phase of PRAF funded by the government of Honduras since 1990. Objective of PRAF= increase demand for preventive health care in pregnant women, new mothers and children aged 0-3 The second phase was funded by a loan from the Inter-

Table 2. Context and intervention description (*Continued*)

	teams plus community-based nutrition intervention compared with standard services	Annual entitlement averaged £60 per household. It is reported that approx 75% of the population live on less than £1 a day Municipalities were those that had highest prevalence of malnutrition in country Transfer of resources to local health teams could not be properly implemented for legal reasons	American Development Bank (IADB) in 1998. The second phase increased the value of the vouchers, removed subjective elements in beneficiary selection
Morris 2004b (Bolsa Alimentacao)	Households received a monetary transfer whose size depended on the number of eligible members in the household The transfers were conditional on attendance to nutrition workshops by mothers, , regular attendance at antenatal care (if pregnant) and growth monitoring visits for children	Beneficiaries were selected in a two-stage process: in the first stage municipalities with high rates of malnutrition were chosen ; then selected municipalities identified beneficiaries Value of the transfers: from US$6.25 to US$18.7	Beneficiaries are compared with individuals who were deemed not eligible due to quasi-random administrative errors in the programme management (problems with data transfer from one body to another, problems with some characters in the names, problems of non-concordance of administrative records)
Rivera 2004 Mexico ('Progresa')	Same as Gertler 2000	This nutritional impact substudy was conducted in a randomly selected 205/320 intervention and 142/186 control communities Same as Gertler 2000	Same as Gertler 2000
Thornton 2006 Malawi	Vouchers given at time of taking test sample. Cash payment received on returning voucher when attending for either HIV or STD tests results	Test results became available 2-4 months after blood was taken Intervention group: monetary incentive ranging from $1-$3. Control group (20% of total participants): no payment.	HIV context in Malawi: availability of VCT.

Table 3. Details of requirements of included programmes

	Cash transfers conditional upon:						
	Primary Education	**Secondary Education**	**Health visits (pregnant**	**Health visits (children)**	**Nutrition supplements**	**Health education work-**	**Others**

Table 3. Details of requirements of included programmes (*Continued*)

			women)			**shops**	
Progresa Mexico	√	√	√	√	√	√	
PRAF Honduras	√		√	√			
RPS Nicaragua	√			√	√	√	
Bolsa Alimentação Brazil			√	√		√	
FA Colombia	√	√		√		√	
HIV testing in Malawi							HIV tested people go back to get their results

Table 4. Outcome measures and methods

Study ID/ Intervention	Types of outcomes	Methods used	Comments
Attanasio 2005 Colombia	*Health services uptake:* Attendance of preventive care visits by children *Immunisation coverage:* Coverage of DPT vaccination (children) *Health outcomes:* Reported incidence of diarrhoea or respiratory diseases (children) *Anthropometric or nutritional outcomes:* Height for height for age Z-score Chronic malnourishment (children)	Estimation of DD estimators with a regression model accounting for clustering effects, controlling for a vector of individual, household and municipal variables	The two health utilisation outcomes are directly linked to the conditionalities of the programme
Barham 2005a Progresa	*Immunisation coverage:* Coverage of DPT and Measles vaccination (children)	Estimation of treatment effects (DD) with a regression model accounting for clustering, controlling for a vector of individual, household variables	

Table 4. Outcome measures and methods (*Continued*)

Behrman 2005 Progresa	*Anthropometric or nutritional outcomes:* Height increase	Estimation of treatment effect with a child-level fixed effects regression, allowing for clustering, applied to 1998 and 1999 data, controlling for observable differences at baseline (incl. health and nutritional status)	
Gertler 2000 Progresa	*Health services uptake:* Daily visits in the nearby health facilities *Health outcomes:* Reported morbidity (children)	Estimation of treatment effects (DD) with regression models accounting for clustering, controlling for a vector of individual and household variables, using 4 waves of surveys (first one being the baseline). Health utilisation outcomes by provider type use the same methods but applied to only 2 survey waves (no baseline, only 'after' data). Finally public clinic visit outcomes use similar models applied to facility data	
Gertler 2004a Progresa	*Health outcomes:* Reported morbidity (children) *Anthropometric or nutritional outcomes:* Height increase Prevalence of stunting	Regression models (logistic/linear) controlling for SES variables, using 5 waves of household survey for child morbidity (one before, 4 after) ,and another panel survey from 1998 and 2000 for objective health outcomes; clustering accounted for . The analysis is restricted to 'eligible' households only	
Maluccio 2004	*Health services uptake:* Attendance of preventive care visits by children *Immunisation coverage:* Reported up-to-date vaccination schedule (children) *Anthropometric or nutritional outcomes:* Prevalence of stunting, wasting and underweight (children under 5) Height for Age Z-score (children under 5) Prevalence of anaemia	Estimation of treatment effects (DD) with a mixed effects regression model accounting for clustering effects and relating each outcome to intervention groups, time and interactions (+ control for individual and household characteristics)	The study also included outcomes related to schooling, child labour, total expenditures (not health care expenditures) and expenditures by type of food. These were not included here, although some might be alluded to in the discussion

Table 4. Outcome measures and methods (*Continued*)

Morris 2004a Honduras	*Health services uptake:* Attendance of preventive and prenatal care by women Attendance of preventive care visits by children *Immunisation coverage:* Coverage for DPT, Measles (children under 3) and tetanus toxoid (mothers)	Estimation of treatment effects (DD) with a mixed effects regression model accounting for clustering effects and relating each outcome to intervention groups, time and interactions (no individual or household characteristics) ;	Results from interviews sometimes corroborated by objective data (clinic cards) We reported only the results from the "household" intervention (i.e. pure CCT) as the other part of the intervention was only partly implemented, with difficulties.
Morris 2004b Brazil	*Anthropometric or nutritional outcomes:* Height for Age Z-score (children) Weight for Age Z-score (children)	Propensity Score matching technique are used to create controls as close as possible to beneficiaries	
Rivera 2004 Progresa	*Anthropometric or nutritional outcomes:* Prevalence of anaemia Height increase	Random intercept linear model for height (applied to 1998 and 2000 data) and Generalised Estimating Equation model for anaemia (applied to 1999 and 2000 data), both allowing for SES controls and accounting for clustering	Nutrition supplements were provided along with cash incentives No baseline for Hb.
Thornton 2006 Malawi	*Health services uptake:* Proportion of people who went back to get the results of their tests	Estimation of treatment effect with a regression model relating the outcome to intervention group, incentive amount, distance and other controls	None

Table 5. Impact on health service utilisation

Source	**Outcome description**	**Initial outcome (intervention areas)**	**Final outcome (intervention areas)**	**Relative treatment effect (difference in outcome measures between intervention and control sites, adjusting for baseline differences - e.g. net variations in percentage points or in the number of visits)**
Malawi				

Table 5. Impact on health service utilisation (*Continued*)

Thornton 2006[9]	% of individuals who attended a VCT centre to learn their results	-	72¶¶	**27.4***** (2.8)†
Colombia - *Familias en Acción*				
Attanasio 2005[12], Attanasio 2005b[20]	% of children under 24 months with up-to-date schedule of preventive healthcare visits	NP	40.0	**22.8**** (0.067)††
	% of children aged 24-48 months with up-to-date schedule of preventive healthcare visits	NP.	66.8	**33.2**** (0.115)††
	% of children over 48 months with up-to-date schedule of preventive healthcare visits	NP	40.4	1.5* (0.008)††
Honduras - *PRAF*				
Morris 2004a	% of women having completed more than 5 antenatal care visits	37.9	NP	**18.7***** [7.4 ; 30.0]
	% of women attending a 10-day post partum check-up	17.8	NP.	-5.6 [-015.6 ; 4.5]
	% of children taken to a health centre at least once in the past month	44.0	NP	**20.2**** [10.9 ; 29]
Nicaragua - *Red de Protección Social*				
Maluccio 2004	% of children age 0-3 taken to a health centre at least once in the past 6 months	69.8	92.7	**11.0*** (5.9)††
	% of children taken to health control *and* weighed in the past 6 months	55.4	89.1	**17.5**** (7.3)††
	% of children taken to health control *and* weighed in the past 6	NP	NP	**23.6**** (9.3)††

Table 5. Impact on health service utilisation (*Continued*)

	months - extremely poor group			
Mexico - *Progresa*				
Gertler 2000	Number of daily consultations per public clinic in Progresa localities	9.11	12.84	**2.09*** (0.067)††
	Number of visits to a public clinic in the 4 weeks preceding the survey - children aged 0-2¶¶¶	-	0.066	-0.011 (-0.314)†
	Number of visits to a public clinic in the 4 weeks preceding the survey - children aged 3-5¶¶¶	-	0.075	0.027 (1.487)†
	Number of visits to a public clinic in the 4 weeks preceding the survey - children aged 6-17¶¶¶	-	0.034	0.015 (1.858)†
	Number of visits to a public clinic in the 4 weeks preceding the survey - adults aged 18-50¶¶¶	-	0.050	0.015 (1.624)†
	Number of visits to all facilities in the 4 weeks preceding the survey - children aged 0-2¶¶¶	-	0.081	-0.032 (-0.871)†
	Number of visits to all facilities in the 4 weeks preceding the survey - children aged 3-5¶¶¶	-	0.097	0.027 (1.439)†
	Number of visits to all facilities in the 4 weeks preceding the survey - children aged 6-17¶¶¶	-	0.041	0.016 (1.893)†

Table 5. Impact on health service utilisation (*Continued*)

	Number of visits to all facilities in the 4 weeks preceding the survey - adults aged 18-50¶¶¶	-	0.071	0.011 (1.019)†

Note: NP denotes results that were not presented in the articles reviewed. Blank cells denote that outcomes was either not available (eg no baseline date) or that outcomes do not apply.
95% CI are shown in brackets
*** indicates significance at the 1%level; ** at the 5% level; and * at the 10% level.
¶ results refer to percentage points. The treatment effect represent the net effect, e.g. taking into account the comparison with control groups.
¶¶ mean attendance of people without incentives was 0.39 ; treatment effect is estimated with a model controlling for the impact of distance to the VCT centre.
¶¶¶computed with surveys carried out after the beginning of the intervention only.
†indicate absolute value of t statistics and ††standard errors.

Table 6. Impact on health outcomes

Source	Outcome description	Initial outcome (intervention areas)	Final outcome (intervention areas)	Relative treatment effect (difference in outcome measures between intervention and control sites, adjusting for baseline differences - e.g. net variations in percentage points or probability)
Colombia - *Familias en Acción*				
Attanasio 2005	Probability of diarrhoea being reported, for children in rural areas, under 24 months old	NP	NP	**-0.106*** (0.059)†††
	Probability of diarrhoea being reported, for children in rural areas, 24-48 months old	NP	NP	**-0.109**** (0.037)†††
	Probability of diarrhoea being reported, for children in rural areas, over 48 months old	NP	NP	-0.015 (0.026)†††

Table 6. Impact on health outcomes (*Continued*)

	Probability of diarrhoea being reported, for children in urban areas, under 24 months old	NP	NP	0.150 (0.103)†††
	Probability of diarrhoea being reported, for children in urban areas, 24-48 months old	NP	NP	-0.033 (0.041)†††
	Probability of diarrhoea being reported, for children in urban areas, over 48 months old	NP	NP	-0.042 (0.026)†††
	Probability of respiratory disease symptoms being reported, for children in rural areas, under 24 months old	NP	NP	-0.056 (0.083)†††
	Probability of respiratory disease symptoms being reported, for children in rural areas, 24-48 months old	NP	NP	-0.005 (0.054)†††
	Probability of respiratory disease symptoms being reported, for children in rural areas, over 48 months old	NP	NP	-0.012 (0.056)†††
	Probability of respiratory disease symptoms being reported, for children in urban areas, under 24 months old	NP	NP	-0.094 (0.103)†††
	Probability of respiratory disease symptoms being reported, for children in urban areas, 24-48 months old	NP	NP	0.034 (0.101)†††
	Probability of respiratory disease symptoms being reported, for children in urban areas, over 48 months old	NP	NP	-0.010 (0.080)†††

Table 6. Impact on health outcomes (*Continued*)

Mexico - *Progresa*				
Gertler 2000	% of children whose mother reported that they were ill in the past 4 weeks - under age 3 at baseline	0.402	NP	**-4.7***** (-2.368)†
	% of children whose mother reported that they were ill in the past 4 weeks - age 3-5 at baseline	0.280	NP	**-3.2***** (-2.591)†
Gertler 2004a	Likelihood of children (aged under 3 years old at baseline) to be reported ill in the past 4 weeks - global impact ¶	-	-	**0.777***** (0.000)††
	Likelihood of children (aged under 3 years old at baseline) to be reported ill in the past 4 weeks - impact after 2 months of programme¶	-	-	0.940 (0.240)††
	Likelihood of children (aged under 3 years old at baseline) to be reported ill in the past 4 weeks - impact after 8 months of programme¶	-	-	**0.749***** (0.000)††
	Likelihood of children (aged under 3 years old at baseline) to be reported ill in the past 4 weeks - impact after 14 months of programme¶	-	-	**0.836***** (0.005)††
	Likelihood of children (aged under 3 years old at baseline) to be reported ill in the past 4 weeks - impact after 20 months of programme¶¶	-	-	**0.605***** (0.000)††

Table 6. Impact on health outcomes (*Continued*)

	Likelihood of children (aged under 3 years old at baselin) to be reported ill in the past 4 weeks - global impact ¶	-	-	**0.747**** (0.013)††

Note: NP denotes results that were not presented in the articles reviewed. Blank cells denote that outcomes was either not available (eg no baseline date) or that outcomes do not apply.
95% CI are shown in brackets
¶ log-estimates of the impact on the probability of illness (e.g. an estimate of 0.75 means that children benefiting from the treatment were 25% less likely than the control ones to be reported as ill) ; the sample was limited to potentially eligible households in treatment and control areas.
*** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.
†indicates t statistics, ††p-value and †††Standard errors

Table 7. Impact on immunisation coverage

Source	Outcome description	Initial outcome (intervention areas)	Final outcome (intervention areas)	Relative treatment effect (difference in outcome measures between intervention and control sites, adjusting for baseline differences - e.g. net variations in percentage points or probability)
Colombia - *Familias en Acción*				
Attanasio 2005	Probability of compliance with DPT vaccination, for children under 24 months old	NP	NP	**8.9*** (0.047)†
	Probability of compliance with DPT vaccination, for children 24-48 months old	NP	NP	3.5 (0.026)†
	Probability of compliance with DPT vaccination, for children, over 48 months old	NP	NP	3.2 (0.039)†
Honduras - *PRAF*				

Table 7. Impact on immunisation coverage (*Continued*)

Morris 2004a	% of children under age 3 vaccinated with DPT1/pentavalent	72	NP	**6.9***** [1; 12.8]
	% of children under age 3 vaccinated for Measles	84	NP	-0.2 [-9.4 ; 9.0]
	%of mothers vaccinated for tetanus toxoid	56	NP	4.2 [-9.7 ; 18.2]
Nicaragua - *Red de Protección Social*				
Maluccio 2004	% of children aged 12-23 months old with up-to-date vaccinations	36.4	71.7	6.1 (10.2)†
Mexico - *Progresa*				
Barham 2005a Evolution after 6 months	% of children under 12 months old (at baseline) vaccinated for TB	88	89	**5.2***** (2.07)††
	% of children aged 12-23 months old (at baseline) vaccinated for Measles	92	96	**3.0**** (2.03)††
Impact after 12 months	% of children under 12 months old (at baseline) vaccinated for TB	88	92	1.6 (0.66)††
	% of children aged 12-23 months old (at baseline) vaccinated for Measles	92	91	2.8 (1.00)††

Note: NP denotes results that were not presented in the articles reviewed. Blank cells denote that outcomes was either not available (eg no baseline date) or that outcomes do not apply.

95% CI are shown in brackets

*** indicates significance at the 1%level; ** at the 5% level; and * at the 10% level.

¶ results refer to percentage points for proportion and point estimates for scores. The treatment effect represent the net effect, e.g. taking into account the comparison with control groups.

†indicates standard errors in parentheses and ††absolute value of t statistics

Table 8. Impact on anthropometric and nutritional outcomes

Source	Outcome description	Initial outcome (intervention areas)	Final outcome (intervention areas)	Relative treatment effect (difference in outcome measures between intervention and control sites, adjusting for baseline differences: eg. net variations in percentage points or scores)
Colombia - *Familias en Acción*				
Attanasio 2005	Height-for-Age Z-score of children under 24 months old	NP	NP	**0.161*** (0.085)†††
	Height-for-Age Z-score of children aged 24-48 months	NP	NP	0.011 (0.055)†††
	Height-for-Age Z-score of children over 48 months old	NP	NP	0.012 (0.033)†††
	Probability of chronic malnourishment for children under 24 months old	NP	NP	**-0.069**** (0.034)†††
	Probability of chronic malnourishment for children aged 24-48 months	NP	NP	0.004 (0.022)†††
	Probability of chronic malnourishment for children over 48 months old	NP	NP	-0.021 (0.014)†††
Nicaragua - *Red de Protección Social* (evolution 2000-2002)				
Maluccio 2004	Height-for-Age Z score for children under 5	-1.79	-1.65	**0.17**** (0.08)

Table 8. Impact on anthropometric and nutritional outcomes (*Continued*)

	% of children under age 5 who are stunted	41.9	37.1	**-5.3*** (3.1)
	% of children under age 5 who are underweight	15.3	10.4	**-6.0**** (2.6)
	% of children under age 5 who are wasted	1.0	0.4	-0.4 (0.5)
	Hemoglobin for children 6-to-59 months of age	11.2	11.4	-0.1 (0.2)
	% of children 6-to-59 months of age with anemia	33.7	32.8	-0.2 (6.8)
Brazil - *Bolsa Alimentação*				
Morris 2004b	Height-for-Age Z score for children under 24 months old	-	-0.68	-0.25 (± 0.13)†††
	Height-for-Age Z score for children under 24-48 months old	-	-0.75	-0.11 (± 0.10)†††
	Height-for-Age Z score for children aged 4-7 years old	-	-0.77	-0.08 (± 0.08)†††
	Mean Height-for-Age Z score for children under 7 years old	-	-0.75	**-0.13**** (± 0.06)†††
	Weight-for-Age Z score for children under 24 months old	-	-0.90	-0.11 (± 0.13)†††
	Weight-for-Age Z score for children under 24-48 months old	-	-0.85	-0.19 (± 0.11)†††
	Weight-for-Age Z score for children aged 4-7	-	-0.95	-0.04 (± 0.09)†††

Table 8. Impact on anthropometric and nutritional outcomes (*Continued*)

	years old			
	Mean Weight-for-Age Z score for children under 7 years old	-	-0.90	-0.11 (± 0.06)†††
Mexico - *Progresa*				
Rivera 2004	Growth (cm) of children aged under 6months old (at baseline), from poorest households¶¶	-	26.4	**1.1**** (0.046)††
	Growth (cm) of children aged 6-12 months old (at baseline), from poorest households¶¶	-	19.7	-0.6 NS
	Mean hemoglobin (g/dL) value among children (after a year of Progresa vs. no exposure in the control group)		11.12	**0.37**** (0.01)††
	Prevalence (%) of anemia (after a year of Progresa vs. no exposure in the control group)	-	44.3	**10.6**** (0.03)††
	Prevalence (%) of anemia (after 2 years of Progresa vs. 1 year in the control group)	-	25.8	-2.8 (0.40)††
Behrman 2005	Height of children (cm) aged between 4-12 months old (at baseline in Aug. 1998)	-	-	0.503 (0.96)†
	Height of children (cm) aged between 12-36 months old (at baseline in Aug. 1998)	-	-	**1.016**** (2.55)†
	Height of children (cm) aged between 24-36 months old (at baseline in Aug. 1998)	-	-	**1.224**** (2.05)†

Table 8. Impact on anthropometric and nutritional outcomes (*Continued*)

	Height of children (cm) aged between 36-48 months old (at baseline in Aug. 1998)	-	-	-0.349 (0.66)†
Gertler 2000	Height (in cm) of children aged 12-36 months old (in Sept 1999)	-	80.7	**0.959***** (0.004)††
	Likelihood of children aged 12-36 months old (in Sept 1999) to be stunted ¶¶¶¶	-	NP	0.914 (0.495)††

Note: NP denotes results that were not presented in the articles reviewed. Blank cells denote that outcomes were either not available (eg no baseline date) or that outcomes do not apply.

95% CI are shown in brackets

*** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

¶ results refer to percentage points for proportion and point estimates for scores. The treatment effect represent the net effect, e.g. taking into account the comparison with control groups.

¶¶ The intervention group include children aged under 6 months old at baseline (Aug 1998) and exposed to 2 years of Progresa, while the control group is a crossover group (e.g. it includes children without treatment for a year and then exposed to 1 year of Progresa when they are 12-18 months old and after)

¶¶¶ the difference was computed by the reviewers, using data from control and intervention groups from the article; statistical significance of the difference was computed by the authors of the article.

¶¶¶¶ log-estimates of the impact on the probability of illness (e.g. an estimate of 0.75 means that children benefiting from the treatment were 25% less likely than the control ones to be affected)

†indicates absolute value of t statistics, ††p-value and †††standard errors

APPENDICES

Appendix 1. Search strategy used for Pubmed

The search in PubMed was also restricted to all the developing countries listed on the World Bank website, by selecting all relevant geographical categories as exploded terms.

Some pilot searches led us to use quite general (exploded) MeSH terms, as it was noticed that several relevant articles were indexed with generic MeSH terms, or not particularly appropriate ones. For example, a study on Ghana would not be referenced under "Ghana" but under "Africa". Besides, since including "Africa[MeSH]" would also include all MeSH terms of lower levels, it was decide to include mainly higher level MeSh terms for delimiting the geographic scope of the study (see #1 below). A few countries were excluded (see # 6).

A similar approach was taken for specifying the topic filters of the search. Generic MeSh terms were used (see #2), and more selective terms that are currently used in the literature were added as free text references (see #3). However, because this was potentially return a large number of irrelevant studies, it was decided to limit this by excluding some irrelevant studies (see #4).

These different filters were then rearranged together (see #7, #8 and #9).

1	Search "Developing countries"[MeSH] OR "Africa"[MeSH] OR "Central America"[MeSH] OR "South America"[MeSH] OR "Latin America"[MeSH] OR "Mexico"[MeSH] OR "Asia"[MeSH] OR "Commonwealth of Independent States"[MeSH] OR "Pacific Islands"[MeSH] OR "Indian Ocean Islands"[MeSH] OR "Europe, Eastern"[MeSH]
2	Search ("Economics"[MeSH] OR "Economics"[SH] OR "socioeconomic factors"[MeSH]) AND ("Delivery of health care"[MeSH] OR "health services research"[MeSH] OR "health planning"[MeSH] OR "health services "[MeSH] OR "utilization"[SH])
3	Search "Fees and charges"[MeSH] OR user fee[TIAB] OR user fees[TIAB] OR social insurance[TIAB] OR health insurance[TIAB] OR community-based insurance[TIAB] OR prepayment plan[TIAB] OR prepayment plans[TIAB] OR prepayment scheme[TIAB] OR prepayment schemes[TIAB] OR conditional cash transfers[TIAB] OR cost recovery[TIAB] OR prepayment[TIAB] OR contracting out [TIAB] OR output-based contract[TIAB] OR pay for performance [TIAB]
4	Search "Personnel Downsizing"[MeSH] OR "workplace"[MeSH] OR "health planning guidelines"[MeSH] OR "patient freedom of choice laws "[MeSH] OR "preferred provider organizations"[MeSH] OR "provider-sponsored organizations"[MeSH] OR "emergency Medical Service Communication Systems"[MeSH] OR "Genetic Services"[MeSH] OR "Medical Errors"[MeSH] OR Chemicals and Drugs Category[MAJR] OR "Drug industry"[MAJR] OR "epidemiology"[MAJR] OR "Patents"[MAJR] OR "War"[MAJR] OR Anatomy Category[MAJR] OR "Child Abuse"[MeSH] OR ("Technology and Food and Beverages Category"[MAJR] NOT "food supply"[MeSH])
5	Search Practice Guideline[ptyp] OR Letter[ptyp] OR Editorial[ptyp] "Clinical Trial"[ptyp] OR "Clinical Trial, Phase I"[ptyp] OR "Clinical Trial, Phase II"[ptyp] OR "Clinical Trial, Phase III"[ptyp] OR "Clinical Trial, Phase IV"[ptyp]
6	Search "Japan"[MeSH] OR "Korea"[MeSH] OR "Taiwan"[MeSH] OR "New Zealand"[MeSH] OR "Singapore"[MeSH] OR "Israel"[MeSH]
7	Search #1 AND #2 NOT #4 NOT #5 NOT #6
8	Search #1 AND #3 NOT #4 NOT #5 NOT #6
9	Search #8 OR #7

Ovid MEDLINE(R) 1950 to April Week 4 2009
Searched 05.05.2009

1. "Fees and Charges"/
2. Fees, Dental/
3. Fees, Medical/
4. Fees, Pharmaceutical/
5. Prescription Fees/
6. Hospital Charges/
7. Capitation Fee/
8. Fee-for-Service Plans/
9. "Cost Sharing"/
10. Contract Services/
11. Outsourced Services/
12. Prepaid Health Plans/
13. Prospective Payment System/
14. Insurance, Health/

15. ((medical or dental or pharmac$ or dispensing or drug or drugs or medicament? or medicine? or prescript$ or consultation? or treatment? or registration? or hospital? or care) adj3 (fee? or charge?)).tw.
16. ((user? or patient? or outpatient? or inpatient?) adj3 (fee? or charge? or pay$)).tw.
17. fee for service?.tw.
18. capitation.tw.
19. ((pay$ or cash or money or monetary or economic or financial) adj3 incentive?).tw.
20. (pay$ adj3 performance).tw.
21. p4p.tw.
22. ((result? or performance) adj based).tw.
23. ((result? or performance or output or out put) adj2 (financ$ or pay$ or incentive? or initiative? or bonus$)).tw.
24. ((cash or pay$) adj3 (condition$ or contingent or requirement?)).tw.
25. ((cash or pay$ or monetary ot money) adj3 transfer$).tw.
26. cost sharing.tw.
27. cost recover$.tw.
28. price change?.tw.
29. (contract or contracts or contracting).tw.
30. (outsourc$ or out sourc$).tw.
31. (risk sharing or shared risk?).tw.
32. (prospective adj (pay$ or reimbursement?)).tw.
33. (prepay$ or pre pay$ or prepaid or pre paid).tw.
34. ((health or medical) adj insurance?).tw.
35. ((social or community) adj3 (insurance? or financ$)).tw.
36. demand side.tw.
37. supply side.tw.
38. (financ$ adj (strategy or strategies)).tw.
39. or/1-38
40. Developing Countries/
41. Medically Underserved Area/
42. exp Africa/ or exp "Africa South of the Sahara"/ or exp Asia/ or exp South America/ or exp Latin America/ or exp Central America/
43. (Africa or Asia or South America or Latin America or Central America).tw.
44. (American Samoa or Argentina or Belize or Botswana or Brazil or Bulgaria or Chile or Comoros or Costa Rica or Croatia or Dominica or Equatorial Guinea or Gabon or Grenada or Hungary or Kazakhstan or Latvia or Lebanon or Libya or Lithuania or Malaysia or Mauritius or Mexico or Micronesia or Montenegro or Oman or Palau or Panama or Poland or Romania or Russia or Seychelles or Slovakia or South Africa or "Saint Kitts and Nevis" or Saint Lucia or "Saint Vincent and the Grenadines" or Turkey or Uruguay or Venezuela or Yugoslavia).sh,tw. or Guinea.tw. or Libia.tw. or libyan.tw. or Mayotte.tw. or Northern Mariana Islands.tw. or Russian Federation.tw. or Samoa.tw. or Serbia.tw. or Slovak Republic.tw. or "St Kitts and Nevis".tw. or St Lucia.tw. or "St Vincent and the Grenadines".tw.
45. (Albania or Algeria or Angola or Armenia or Azerbaijan or Belarus or Bhutan or Bolivia or "Bosnia and Herzegovina" or Cameroon or China or Colombia or Congo or Cuba or Djibouti or Dominican Republic or Ecuador or Egypt or El Salvador or Fiji or "Georgia (Republic)" or Guam or Guatemala or Guyana or Honduras or Indian Ocean Islands or Indonesia or Iran or Iraq or Jamaica or Jordan or Lesotho or "Macedonia (Republic)" or Marshall Islands or Micronesia or Middle East or Moldova or Morocco or Namibia or Nicaragua or Paraguay or Peru or Philippines or Samoa or Sri Lanka or Suriname or Swaziland or Syria or Thailand or Tonga or Tunisia or Turkmenistan or Ukraine or Vanuatu).sh,tw. or Bosnia.tw. or Cape Verde.tw. or Gaza.tw. or Georgia.tw. or Kiribati.tw. or Macedonia.tw. or Maldives.tw. or Marshall Islands.tw. or Palestine.tw. or Syrian Arab Republic.tw. or West Bank.tw.
46. (Afghanistan or Bangladesh or Benin or Burkina Faso or Burundi or Cambodia or Central African Republic or Chad or Comoros or "Democratic Republic of the Congo" or Cote d'Ivoire or Eritrea or Ethiopia or Gambia or Ghana or Guinea or Guinea-Bissau or Haiti or India or Kenya or Korea or Kyrgyzstan or Laos or Liberia or Madagascar or Malawi or Mali or Mauritania or Melanesia or Mongolia or Mozambique or Myanmar or Nepal or Niger or Nigeria or Pakistan or Papua New Guinea or Rwanda or Senegal or Sierra Leone or Somalia or Sudan or Tajikistan or Tanzania or East Timor or Togo or Uganda or Uzbekistan or Vietnam or Yemen or Zambia or Zimbabwe).sh,tw. or Burma.tw. or Congo.tw. or Kyrgyz.tw. or Lao.tw. or North Korea.tw. or Salomon Islands.tw. or Sao Tome.tw. or Timor.tw. or Viet Nam.tw.
47. ((developing or less$ developed or third world or under developed or middle income or low income or underserved or under served or deprived or poor$) adj (count$ or nation? or state? or population?)).tw.

48. (lmic or lmics).tw.
49. or/40-48
50. randomized controlled trial.pt.
51. random$.tw.
52. intervention$.tw.
53. control$.tw.
54. evaluat$.tw.
55. effect?.tw.
56. or/50-55
57. Animals/
58. Humans/
59. 57 not (57 and 58)
60. 56 not 59
61. 39 and 49 and 60

Appendix 2. Quality criteria used for appraising quality of included studies

This appendix presents the detail of all of the criteria used in the appraisal of included studies.

CBA studies:

In the following list, criteria one, two and four are directly taken from the list of standard criteria of the EPOC Group.
Criteria three and five are adapted from the original criteria to make them more relevant to the specificities of the studies included in this review. Standards to judge the risk of exclusion or selection bias were rephrased to be more adapted to the types of population-based studies that might be included in the review. The criterion on quality and reliability of data was also adapted to reflect better the risks of bias relating to the type of outcomes that were the primary focus of the review.
Criteria six was added following preliminary findings which showed that statistical significance of studies was not systematically computed or available in the studies found.
Finally, we omitted a standard criterion of the Cochrane Collaboration textbook on the blinded assessment of primary outcomes. We judged that this was not relevant for the types of outcomes this review focused on.

1. **Baseline outcome characteristics**: DONE if outcomes were measured prior to the intervention, and no significant differences were present across study groups (e.g. where multiple pre intervention measures describe similar trends in intervention and control groups); NOT CLEAR if baseline measures are not reported, or if it is unclear whether baseline measures are significantly different across study groups; NOT DONE if there are differences at baseline in main outcome measures likely to undermine the post intervention differences (e.g. are differences between the groups before the intervention similar to those found post intervention?)
2. **Equivalent control sites**: DONE if characteristics of study and control sites are reported and similar (in terms of 1/population 2/facilities and 3/external influence characteristics); NOT CLEAR if it is not clear in the paper e.g. characteristics are mentioned in the text but no data are presented; NOT DONE if there is no report of characteristics either in the text or a table OR if baseline characteristics are reported and there are differences between study and control providers.
3. **Protection against exclusion or selection bias**: DONE if outcome measures obtained from the whole population or a representative sample of the population (and the control group) was studied; NOT CLEAR if not specified in the paper; NOT DONE if outcome measures were not obtained from a representative sample.
4. **Protection against contamination**: DONE if allocation was by community, institution, or practice and is unlikely that the control group received the intervention; NOT CLEAR if communication (i.e individuals present in one control group cannot move and benefit from the interventions in experimental areas) between treatment and control group was likely to occur; NOT DONE if it is likely that the control group received the intervention (e.g. cross-over studies or if patients rather than providers were randomised).
5. **Quality/reliability of outcome measures**: scored DONE if the outcome is obtained from some automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (will be treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data is reportedly of a poor quality.
6. **Appropriate analysis**: DONE if statistical significance of differences in outcomes was tested and/or statistical analysis was appropriate. NOT CLEAR if statistical significance of results is not specified in the paper or if the analysis chosen was not appropriate; NOT DONE if statistical significance of results was not tested.

Randomised Controlled Trials

All the following criteria are taken from the standard EPOC criteria (EPOC 2002), except for criteria three and four. Indeed, we judged important to add specific criteria for cluster-randomised for two reasons. Firstly because interventions of interest would be more likely to be implemented at community level, they would require such study designs. Secondly, issues regarding sampling and analysis have identified as particular concerns that might lead to biases when analysing cluster-randomised trials (Ukoumunne 1999). We also omitted one criteria on exclusion bias concerning the follow-up of professionals. It was judged not relevant for the focus of our review (where studies are all focusing on populations).

1. **Concealment of allocation**: DONE if the unit of allocation was by institution, team or professional and any random process is described explicitly, e.g. the use of random number tables or coin flips; OR the unit of allocation was by patient or episode of care and there was some form of centralised randomisation scheme, an on-site computer system or sealed opaque envelopes were used. NOT CLEAR if the unit of allocation is not described explicitly OR the unit of allocation was by patient or episode of care and the authors report using a 'list' or 'table', 'envelopes' or 'sealed envelopes' for allocation. NOT DONE if the authors report using alternation such as reference to case record numbers, dates of birth, day of the week or any other such approach (as in CCTs) OR the unit of allocation was by patient or episode of care and the authors report using any allocation process that is entirely transparent before assignment such as an open list of random numbers or assignments OR allocation was altered (by investigators, professionals or patients).
2. **Protection against exclusion bias**: DONE if outcome measures obtained for 80-100% of subjects randomised (or a biased sample) or for patients who entered the trial (do not assume 100% follow up unless stated explicitly); NOT CLEAR if not specified in the paper; NOT DONE if outcome measures obtained for less than 80% of subjects randomised (or a biased, non-representative sample).
3. **Sampling (for cluster-randomised trials)**: DONE if sampling took cluster effects/bias into account or if the sample is large enough to provide robust results; NOT CLEAR if not specified in the paper; NOT DONE if the sampling is too small to provide robust results.
4. **Appropriate Analysis (for cluster-randomised trials)**: DONE if the analysis accounted for cluster effects/bias; NOT CLEAR if not specified in the paper; NOT DONE if the analysis did not account for cluster effects/bias.
5. **Quality/reliability of the data**: scored DONE if the outcome is obtained from some automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (will be treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data is reportedly of a poor quality.
6. **Protection against detection bias**: DONE if the authors state explicitly that the primary outcome variables were assessed blindly OR the outcome variables are objective, e.g. length of hospital stay, drug levels as assessed by a standardised test; NOT CLEAR if not specified in the paper; NOT DONE if the outcome(s) were not assessed blindly.
7. **Baseline Measurement**: DONE if performance or patient outcomes were measured prior to the intervention, and no substantial differences were present across study groups (e.g. where multiple pre intervention measures describe similar trends in intervention and control groups); NOT CLEAR if baseline measures are not reported, or if it is unclear whether baseline measures are substantially different across study groups; NOT DONE if there are differences at baseline in main outcome measures likely to undermine the post intervention differences (e.g. are differences between the groups before the intervention similar to those found post intervention?).
8. **Protection against contamination**: DONE if allocation was by community, institution or practice and it is unlikely that the control received the intervention; NOT CLEAR if professionals were allocated within a clinic or practice and it is possible that communication between experimental and group professionals could have occurred; NOT DONE if it is likely that the control group received the intervention (e.g. cross-over trials or if patients rather than professionals were randomised).

ITS analyses

1. **Protection against changes**: scored as DONE if the intervention occurred independently of other changes over time; NOT CLEAR if not specified (NOT DONE if information cannot be obtained from the authors); NOT DONE if reported that the intervention was not independent of other changes in time.
2. **Appropriate analysis**: DONE if ARIMA (Auto-Regressive Integrated Moving Average) models were used, OR time series regression models were used to analyse the data and serial correlation was adjusted/tested for, OR if reanalysis performed; NOT CLEAR if not specified; NOT DONE if it is clear that neither of the conditions above are met.
3. **No selection bias in the sample framing**: DONE if outcome measures are obtained from the whole population or a representative sample of the population studied; NOT CLEAR if not specified (treated as NOT DONE if information cannot be obtained from the authors); NOT DONE if data set is not drawn from a representative sample.

4. **Quality/reliability of outcome data**: DONE if the outcome is obtained from an automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data are reportedly of a poor quality.
5. **Number of points specified**: DONE if 3 or more data points before and 3 or more data points recorded after the intervention. Score NOT CLEAR if not specified in paper e.g. number of discrete data points not mentioned in text or tables (will be treated as NOT DONE if information cannot be obtained from the authors). Score NOT DONE if less than 3 data points recorded before and 3 data points recorded after intervention.
6. **Intervention effect specified**: DONE if point of analysis was the point of intervention OR a rational explanation for the timing of intervention effect was given by the author(s).
7. **Detection bias**: DONE if it is reported that the intervention itself was unlikely to affect data collection (for example, sources and methods of data collection were the same before and after the intervention).

HISTORY

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CONTRIBUTIONS OF AUTHORS

AH and NP secured the funding. ML and NP prepared the protocol, ML conducted the searches, ML and AH applied the inclusion criteria, assessed the quality and extracted the data for the included studies. ML prepared the report and NP and AH edited the draft.

DECLARATIONS OF INTEREST

None known.

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INDEX TERMS

Medical Subject Headings (MeSH)

Developing Countries [*economics]; Financing, Government [*economics; statistics & numerical data]; Health Behavior; Health Services Accessibility [*economics; statistics & numerical data]; Medical Assistance [economics; statistics & numerical data]; Outcome Assessment (Health Care) [*economics]; Poverty Areas; Preventive Health Services [*economics; utilization]

MeSH check words

Humans