

What are the economic impacts of conditional cash transfer programmes?

A systematic review of the evidence



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Contents

List of abbreviations	iv
Executive summary	1
1. Introduction	4
2. Methodological issues	7
2.1 Search and inclusion criteria.....	9
3. Description of programmes.....	12
4. Household labour allocation.....	14
4.1 Child labour.....	14
4.2 Adult labour	18
4.3 Migration patterns	21
5. Household expenditure patterns	23
5.1 Consumption.....	23
5.2 Savings and investment	24
6. The insurance role of transfers	27
6.1 Coping with crisis.....	27
6.2 Risk sharing arrangements.....	29
7. Locality-wide effects.....	31
7.1 Summary of findings.....	32
8. Meta-analysis: methods and results.....	33
9. Conclusions	43
10. References	45
10.1 Papers included in the review.....	45
10.2 Other papers cited in the report.....	48
Appendices.....	50
Appendix 1.1: Authorship of this report	50
Appendix 2.1: Tables	51

List of abbreviations

3ie	International Initiative for Impact Evaluation
ATT	average treatment effect on the treated
BLDS	British Library of Development Studies
CCT	conditional cash transfers
CMO	context-mechanism-outcome
DFID	Department for International Development
FeA	Familias en Acción
GNP	Gross National Product
IC	Ingreso Ciudadano
NBER	National Bureau of Economic Research
NDLTD	Networked Digital Library of Theses and Dissertations
NGO	non-governmental organisation
PETI	Programa de Erradicação do Trabalho Infantil
PO	Progresas/Oportunidades
PRAFII	Programa de Asignación Familiar
RCT	randomised controlled trial
RPS	Red de Protección Social
SE	standard error
SMS	Scientific Methods Scale

Executive summary

Aim of the study

The growing attention to social protection within the international development community since the late 1990s reflects various factors: the failure of short-term emergency responses to deal with structural food deficits, particularly in Africa; periodic financial crises which have accompanied the increasing integration of the global economy; and growing informality of labour markets which means increasing numbers of the world's working population are outside the formal social security system. Social protection encompasses a variety of different instruments but it is the instruments grouped under the rubric of social transfers that have greatest relevance for poor people in poor countries. These are largely financed by taxes, whether the tax in question is paid by national citizens, or, via international aid, by citizens of other countries. Social transfer instruments most frequently take the form of cash transfers.

Conditional cash transfers (CCTs) are a recent innovation in the field of social protection. These provide cash transfers to low-income households, conditional on pre-specified investment in household human capital, usually children. It is argued that tying cash transfers to human capital investments in children will help to break the inter-generational transmission of poverty by increasing their productivity as adults. This in turn will help to counter the view held by many governments and donors that social transfers are unproductive and give rise to welfare dependence on the part of recipients. Whether such impacts on inter-generational poverty do indeed materialise will only be known when the current generation of children who have benefited from CCTs grow up and join the labour force.

What motivated this review is a need to know whether there is evidence of broader economic impacts associated with cash transfer programmes. If such evidence is found to be methodologically sound, it would help to offset some of the costs of financing social transfers, thereby easing their fiscal burden and increasing their appeal to policy-makers. This review investigates this possibility. It carries out a methodological mapping of studies relating to the economic impacts of CCTs, appraising these studies according to pre-defined standards of methodological rigour, selecting only those that meet these standards and then synthesising the findings of the selected studies so as to draw conclusions about the economic impacts to which a high level of confidence can be attached.

Why should we expect CCTs to have economic impacts?

Like most development interventions, CCTs operate with a theory of change which relates programme inputs to a set of impacts which represent the intended objectives of the programme. The causal pathways through which typical CCT programmes seek to bring about these intended changes include:

- Income effects, the lifting of liquidity constraints which prevent households from undertaking investments, including investments in children's human capital
- Substitution effects which reflect the increased opportunity cost of not investing in children's human capital and thus failing to meet the conditions for the transfer

What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence

- A gender-specific effect, the targeting of mothers for the cash transfers reflecting widespread evidence that their preferences are more closely aligned than the fathers' with children's welfare.

In this review, we are less interested in the direct intended impacts of CCTs on children's human capital than in their secondary, not necessarily intended, impacts on household behaviour with regard to a range of economic variables including household labour supply, consumption, investment, savings and migration. The rationale for expecting such economic impacts reflects 'our' theory of change. This allows for several possibilities: that the income effects of cash transfers were not exhausted by additional expenditure on children's health and schooling; that the substitution effect of the conditions attached to the transfers entailed more than simply moving children from work/play into school since such re-allocation would have implications for how other family members use their time; that gender is likely to mediate the generation of some of these secondary impacts; and finally that impacts are not confined to beneficiary households but may spill over into the wider community through such mechanisms as the 'demonstration effect', inflationary pressures or multiplier effects on local economic activity.

Our theory of change helped to specify the kinds of impacts that we would be seeking in our search of the relevant literature. To make our task manageable, we decided at the outset of the review to restrict our search only to those studies which met our pre-determined standards of rigour and to rely on these for any information or speculation with regard to the causal mechanisms specified by our theory of change.

The initial search was conducted using a number of online databases and search terms and resulted in 1,076 studies, all dealing with CCTs in some way. This was reduced to 624 studies once duplicates had been excluded. Subsequent stages led to a further narrowing down of the studies to only those which had used an experimental or quasi-experimental approach and dealt with economic impacts. The final list consisted of 46 papers. Additional handsearch of journals and the British Library of Development Studies (BLDS) did not turn up any new papers.

Findings

The studies included in the final synthesis cover a range of economic impacts, including

- Impacts on household labour allocation, by age and gender
- Migration patterns,
- Allocation of household budgets between consumption, savings and investments, including investments in productive assets - capacity to cope with shocks and risk sharing arrangements, and
- Locality-wide impacts, including impacts on consumption, loans, transfers and remittances and the incidence of poverty.

The review concludes the following:

- The evidence is strong that CCTs could lead to a rise in overall household consumption, increase investment in productive assets, reduce child labour and increase school attendance.
- The evidence is mixed as to the impacts on adult labour, with increases in market work by both men and women in some contexts and increases in leisure and domestic work in others.

- There is persuasive evidence that CCTs protect household consumption and educational patterns during times of crisis.
- There is limited evidence that CCTs have spill-over effects within communities in terms of poverty reduction, increased loans and transfers and household behaviour.
- There is no evidence that CCTs lead to inflationary pressure in the local economy.

Given the sample of studies covering the impact of CCTs on child labour, adult labour and consumption, we were able to run a meta-analysis to better synthesise the overall effect of CCT on these particular outcomes. This suggested that CCTs tend to reduce children's labour supply and that their impacts are higher among girls as long as domestic work is taken into account. We also found that CCT has no impact on adult labour supply but that there is an overall positive impact on consumption.

The review makes a number of broader points. First of all, evidence about the economic impacts of CCTs that meets the strict methodological criteria laid out in this paper is still extremely scarce. Because of the relatively recent development of CCTs, and their even more recent spread to countries other than Mexico and Brazil where they originated, studies using robust econometric methodologies and dealing with the economic impacts of CCTs turned out to be concentrated on a limited set of programmes in a limited range of countries, overwhelmingly in Latin America.

Secondly, the decision to confine our search for insights into the causal factors that might explain the presence or absence of impacts to information contained in the selected papers served to highlight the very limited attention paid to causal pathways in these papers. Authors speculated on the meaning of their findings or sought to infer it from the theoretical literature but concrete evidence was largely missing. This may reflect the fact that not only were the papers included in this review almost exclusively econometric in their approach but they are based on datasets that were not designed to explore causality. More purposively designed surveys which incorporate both quantitative and qualitative information on the theories of change that generate impacts of interest to researcher and policy-makers would add greatly to the analytical and practical value of these studies.

1. Introduction

The growing attention to the question of social protection within the international agenda since the late 1990s reflects a number of factors. These include the failure of short-term emergency responses to deal with structural food deficits, particularly in the African context, the periodic financial crises which have accompanied the increasing integration of the global economy as well as the growing informality of labour markets which means that increasing numbers of the working population are outside the formal social security system. The 2008 global economic crisis has given renewed emphasis to the importance of social protection as a 'smart investment' in an uncertain world (Lin and Phumaphi 2009: xi).

The consequences of this growing attention can be seen from the present scale of social protection initiatives: they are estimated to reach over 150 million poor households in poor countries and benefit around half a billion people (Barrientos and Hulme 2009). Social protection encompasses a variety of different instruments but it is the instruments grouped under the rubric of social transfers that have the greatest relevance for poor people in poor countries. These instruments are largely financed by taxes, whether the tax in question is paid by national citizens or, via international aid, by citizens of other countries. Given its concern with poverty reduction, social transfer instruments are likely to be of greatest interest to DFID (Department for International Development, UK). Social transfer instruments most frequently take the form of cash transfers. These are considered to be more flexible than in-kind transfers that restrict the value of the transfer to pre-determined commodities which may or may not be the most urgent priority for their recipients.

Cash transfers can vary considerably in their design features, for instance: between lump sum and periodic payments; between targeted and untargeted transfers; between conditional and unconditional transfers; and between transfers to providers of services and to users. While cash transfers conditional on work, as in public works programmes, have been around for a while, a recent innovation is cash transfers conditional on pre-specified investment in household human capital, particularly children. It is argued that tying cash transfers to human capital investments in children will help to break the inter-generational transmission of poverty. This is in contrast to the view that social transfers are unproductive and give rise to welfare dependency on the part of recipients. Whether such impacts on inter-generational poverty do indeed materialise will only be known when the current generation of children who have benefitted from CCTS (conditional cash transfers) grow up and join the labour force.

What motivates this review is more immediate evidence of the *economic impacts* that accrue to cash transfer programmes. By economic impacts, we are referring to impacts that relate to productive activity. Some of these are documented in a recent scoping study on the impacts of social transfer programmes carried out for DFID (Kabeer 2009). The study pointed out that cash transfers are generally too small, and the contributions of poor people to their country's per capita GNP (gross national product) too meagre, to generate any direct impact on macro-economic growth or national poverty levels. However, the absence of economic impact at national levels does not rule out the possibility of such impacts at the level of households and communities. Examples of household-level impacts highlighted by the scoping study include changes in household savings, investment in productive assets, labour force participation, increased returns to economic activity, and expansion and diversification of livelihood activities. Local economy impacts

include positive impacts on commercial activity and labour markets as well as negative impacts in the form of inflationary pressures.

If such perceived impacts are found to be based on methodologically sound evidence, they would help to offset some of the costs of financing social protection, easing its fiscal burden and increasing its appeal to policy-makers. This review explores this possibility. It carries out a methodological mapping of the evidence for the economic impacts of social transfer programmes, appraising studies on the economic impacts of social protection according to pre-defined standards of methodological rigour and synthesising the findings of the selected studies with regard to the likelihood of economic returns to investments in social protection.

As noted earlier, cash transfer programmes can take a variety of forms, with the dominant categories being conditional and unconditional cash transfers and public works programmes. While each of these entails the transfer of cash, they are aimed at different sections of the population and offered on different terms. Systematic reviews work best if they focus on a relatively homogenous set of interventions so as to minimise the effects of programme heterogeneity on the impacts reported. Consequently, each of these transfer programmes would require a separate systematic review. This review confines itself to the economic impacts of CCT. Reviews of the economic impacts of unconditional cash transfers and public works programmes would need to be undertaken separately.

CCTs are a relatively recent innovation within the field of social protection but as Fiszbein and Schady (2009) point out, they have spread at a rapid rate. In 1997, there were just three CCTs: in Brazil, Mexico and Bangladesh - although the Bangladesh CCT began as a conditional food transfer. By 2008, there were around 30 such programmes across different regions of the developing world and they continue to multiply. CCTs vary in scope and design. Some are nationwide, some serve just a segment of the population, while others are still small-scale pilot efforts. Differences in scale are evident from a comparison of their absolute coverage in Brazil (11 million families) and Kenya and Nicaragua (a few thousand). In terms of relative coverage, they vary from 40 percent of the population in Ecuador to 1 percent in Cambodia. Design features may vary in terms of which human capital investments are incorporated into the conditionality. Chile Solidario relies on social workers to work with targeted families to develop action plans to get them out of poverty. Implementing the action plan becomes the condition attached to the benefit. The generosity of the benefits varies from 20 percent of mean household consumption in Mexico to less than 1 percent in Bangladesh, Cambodia and Pakistan. Most CCTs transfer money to the mother within the household but some may target fathers or children themselves.

A search of the literature in preparation for this proposal suggests that there have been a number of attempts to synthesise the impacts of CCTs, including both conventional and systematic reviews but they largely focus on their human development impacts. Examples of systematic reviews of CCTs include Leroy et al. (2009); Gaarder et al. (2010) and Lagarde et al. (2009). This is not surprising since that is the primary objective of these programmes. To the best of our knowledge there has been no systematic review of the economic impacts of CCTs; hence the rationale for this paper.

The structure of the review is as follows: section 2 discusses a number of methodological issues which are relevant to the approach taken to the synthesis; section 3 provides a brief description of the programmes included in the review process; the next sections discuss different categories of economic impacts: section 4 discusses impacts relating to household labour allocation, and section 5 to

What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence

household expenditure patterns including consumption, savings and investment; section 6 relates to the insurance role of cash transfers, and section 7 to the local economy impacts. Finally, section 8 offers some concluding comments.

2. Methods used in the review

There are two different approaches to systematic reviews, one concerned with the internal validity of the methodology and the other concerned with the external validity of findings, and the extent to which they can be replicated across different contexts. The first approach uses clear, pre-determined methodological criteria to determine which impact assessments studies will be included. A widely used example is the Maryland Scientific Methods Scale (MSM), a five-point scale which allows researchers to rank impact assessment studies according to their methodological quality, with the highest ranking given to those using experimental or quasi-experimental methods, with randomised control trials (RCT) widely regarded as the 'gold standard' against which other methods are judged. While we did not use this scale to judge methodological quality, we have used it to determine appropriate studies for inclusion, as indicated below.

The use of ranking systems in systematic reviews of impact assessments allows a high level of confidence to be attached to their conclusions regarding the impacts of the interventions under study. This is clearly valuable from a policy perspective. However, the policy utility of a systematic review is likely to be enhanced if it is also able to provide insights into specific aspects of interventions, or their setting, which explain why they worked well, partially or not at all. The search for external validity draws attention to these aspects and hence offers important insights into the causal processes through which interventions are translated into outcomes.

Realist synthesis, which focuses on the analysis of the *context-mechanism-outcome* (CMO) configurations associated with different programmes, offers a widely used approach for establishing external validity (Pawson 2006). The idea of CMO configurations is to capture the theory of change embedded within particular programmes and to examine how the change mechanisms in question operate in particular contexts to produce particular outcomes. The selection of studies is made on the basis of their contribution to this explanatory challenge. Realist synthesis therefore draws on a much wider range of studies, both quantitative and qualitative, than does the systematic approach. The problem here is that the methodological quality of evaluations can be and often is a problem, particularly in terms of the impact of programmes and interventions. It can also open up the review to an unmanageable number of studies. As discussed below, we have taken a more restricted approach in deciding which studies to include.

However, spelling out the theories of change embedded within programmes and using them to guide the search for causality is a useful way to proceed. In the context of the present review, we can distinguish between two theories of change with regard to CCTs: 'theirs' and 'ours'. Those who design programmes operate with a particular theory of change in which programme inputs are translated into impacts which represent the intended objectives of the programme. Fiszbein and Schady, (2009) have identified some of the likely causal pathways through which CCTs are likely to achieve their intended impacts. The first is an income effect and reflects lifting of the liquidity constraints that poor households are subject to in contexts characterised by imperfect or absent markets in credit and insurance. This in turn enables them to undertake investments that they could not otherwise undertake.

A second pathway relates to the conditionality associated with the transfer and operates through a substitution effect: increasing the opportunity costs of not taking children to health clinics and sending them to school. This is believed to offset the impact of misguided beliefs on the part of parents about returns to

education or a tendency to discount the future at a higher rate than children's interests require. There is a third gendered pathway: the frequent tendency to target mothers for receipt of the transfer reflects widespread evidence that their preferences are more closely aligned than the fathers' with children's interests. In addition, various other design components frequently found in CCT programmes, such as training of various kinds, can be seen as efforts to reinforce the basic causal mechanism: enabling parents to behave in a certain way while simultaneously imposing costs for not doing so.

In this review, however, we are less interested in the direct intended impacts of CCTs on the human capital of children than in their secondary, perhaps unintended, impacts on household behaviour with regard to a range of economic variables including household labour supply, consumption, investment, saving and migration. 'Our' theory of change allows for the possibility that the income effect of the cash transfer is not exhausted by the additional expenditure on schooling, that the substitution effect entails more than simply moving children from work/play into school, that gender is likely to mediate the generation of some of these impacts and, finally, that impacts are not confined to beneficiary households but may spill over into the wider community.

First of all, the income effect of the cash transfer may accommodate uses of the transfer other than, or as well as, children's health and education. Some of these uses are likely to be economic in nature, including the trade-off of work for leisure (the disincentive effect), meeting the costs of job search, including migration in search of work, or alternatively, reducing the need to migrate. Secondly, the re-allocation of children's time from work or play into school is likely to have ramifications for how other members of the family use their time. If children were previously in paid work, loss of their income will reduce the size of the income effect represented by the cash transfer and may have to be compensated for by other family members either taking up paid work or expanding their time in paid work. If children were previously helping out in domestic work, the reduction in their contribution as a result of increased time in school may have to be compensated for by other family members. If, of course, children were idle, the re-adjustments needed on the part of others are likely to be minimal.

Gender is likely to mediate these impacts in a number of ways. Firstly, men and women may have different priorities with regard to the use of cash so the gender of the direct recipient of the transfer may influence the pattern of use. Secondly, most societies are characterised by a gender division of paid and unpaid work. In general, men and boys are relatively more likely to be engaged in paid work while women and girls are more likely to be found in unpaid domestic labour. Consequently, changes in time allocation in response to the income effect of the transfer as well as to the substitution effect are also likely to be patterned by gender. For example, we would expect that an increase in girls' schooling is likely to entail a reduction in their involvement in domestic chores. This can have knock-on effects on their mothers' use of time, perhaps adding to their workloads, particularly when mothers are largely responsible for the time involved in meeting conditionalities. An increase in boys' schooling would be more likely to entail a reduction in their involvement in paid work, with implications for household income and a possible increase in fathers' time in paid work, although the reduction in income could equally be offset by drawing non-working mothers into paid work.

Finally, we would expect to find spill-over effects of cash transfers on the wider community. These may reflect an income effect due to the greater availability of money in the local economy which in turn may lead to increase in trade or to

inflationary pressure. It may reflect a ‘demonstration effect’ as behaviour change by participants is disseminated throughout the community. Or there may be a substitution effect as cash transfers crowd out previous risk sharing arrangements.

2.1 Search and inclusion criteria

The research team was made up of Naila Kabeer, Linnet Taylor and Caio Piza¹. The literature search and data extraction were carried out by Linnet Taylor and Caio Piza. The approach taken was a two-stage process. First we carried out a quantitative effectiveness review which used SMS criteria to rank studies dealing with the economic impacts of CCTs and to determine which studies meet the necessary standards of rigour. We then examined the selected papers for any information or speculation on the causal mechanisms explaining impact or absence of impact.

2.1.1 Search terms

The initial search was conducted using a number of online databases and search terms. The databases used are shown in Table A2.1 in Appendix 2.1, and included collections of scientific journals such as EBSCO, institutional sources such as the National Bureau of Economic Research (NBER) and the academic search engine Google Scholar. This first stage resulted in 1,076 studies, including much grey literature, and did not distinguish between methodologies or research questions. Since it was conducted by two researchers, it also produced duplicates. This phase of the review included a double check of the references, as well as a search of other papers found in the bibliographies of included studies.

The search terms used varied by database, but were a combination of ‘cash’, ‘transfer’ and ‘conditional’. The specific words used depended on the level of specialisation of the database: for some, such as NBER, it was possible to search for the term ‘cash transfer’ since the database specialised in economic papers (this term brought up 112 hits), but in contrast, a Google Scholar search for the specific term ‘cash transfer’ brought up 18,300 hits. In the latter case, the search was limited to ‘conditional cash transfer’, which brought up a more manageable, although still large, 2,890 hits.

2.1.2 Language and search period

The search terms (see Table A2.1) were entered in English, but this search also located studies published in Spanish and Portuguese whose abstracts had been translated. These papers were included, but all were ultimately found to have been published in English, so that the English language versions were used in the final analysis. The period searched in each database was as long as possible, and is noted in detail in Table A2.1. For the largest databases this included papers back to 1900, or to an unspecified date, while some more specialist ones allowed for searches dating back to 1980. For each search, the end date was the date of the search, i.e. July 2010. Stages in the search and exclusion process (see section 2.1.3 below) are summarised in Table A2.2 (Appendix 2:1).

2.1.3 Inclusion and exclusion criteria

The next step was to reduce the list by cutting out the duplicates. This resulted in a list of 624 papers, which all dealt with conditional cash transfers in some way. Next, the abstracts of all papers were read and the papers categorised according to

¹ We are grateful to Birte Snilstveit, Hugh Waddington and Martina Vojtkova from 3ie for technical advice in the course of this review and to the EPPI-Centre for comments on an earlier draft. We are also grateful to members of DFID Research Division for their helpful comments. However, we take full responsibility for any omissions and errors.

methodology. The papers retained were those that used either experimental approaches (in this case, all experimental evidence came from RCTs) or quasi-experimental approaches (regression discontinuity, propensity score matching, instrumental variable and difference-in-differences). The exclusion of papers which were exclusively qualitative resulted in a reduced list of 403 papers. The next stage was to exclude papers that did not deal with the economic impacts of CCTs. This categorisation was done in several iterations, according to the evolving discussion amongst the team of what constituted economic impact. Given the unexpectedly large number of papers using an experimental or quasi-experimental approach to assess the impact of CCTs, we opted to leave out qualitative studies as well as interviews with the authors of these studies. Instead, our efforts to draw on qualitative analysis were confined to what was contained in the selected papers.

Our discussion on the nature of economic impacts led to the exclusion of two sets of studies. The first set were studies that dealt only with the human capital investments required by the transfer, i.e. investments in health, nutrition and education, mainly of children. While a case could be made for the economic impacts of these investments in human capital, the studies in question dealt primarily with the fulfilment of the conditionalities of the transfers, whereas 'economic impacts' as we defined them would have referred to the impact of human capital investment on productive activities within the household or local economy. Since such impacts fell outside the predicted and specifically incentivised effects of the conditionality, they were not generally investigated. The second set of studies to be excluded were those that were based on RCTs but that were not dealing with the impact of CCT on an economic outcome, but instead testing the validity of a method to replicate experimental results (e.g. Buddelmeyer and Skoufias 2004).

Three further sets of papers were excluded in the next stage. General equilibrium model studies were cut from the list entirely since, although they constitute a systematic and robust methodology, they do not provide causal estimates. A significant number of studies also turned out to be earlier versions of others on our list, where a researcher or group of researchers would take advantage of a single set of results to publish a report for a particular funder or NGO (non-governmental organisation), followed by working papers, which then developed into papers for refereed journals. In these cases we gave each version a detailed reading to check that no results were dropped or conclusions changed, but in each case found we could use the final version only. Finally, we also excluded studies that were inaccessible, either because they were listed in databases but inaccurately cited and untraceable through other means, or because they existed only in hard copy in libraries in other countries (in this case, Brazil). The studies excluded under this last criterion were masters and doctoral theses.

The two researchers were able to control for mistakes in reading and aggregating the results of the studies by a process of overlapping assignments, where they would each be responsible for reading a certain number of papers, but would then re-read and check each other's results and constantly merge and re-merge the two lists of findings. Thus they were able to pick up similarities among papers as they proceeded, and to reduce the list to 46 papers. Finally, a handsearch of journals and of the British Library of Development Studies (BLDS) was conducted, but did not turn up any new papers. This was, we would suggest, due to the highly specific criteria of the search: because of the relatively recent development of CCTs, and their even more recent spread across different countries, papers using robust econometric methodologies and dealing with the economic impacts of CCTs turned out to be written over the last two decades by a fairly concentrated group of scholars and researchers, largely working on a central group of evaluation projects,

and published in established journals or as working papers by prominent institutions. Although there were a few more independently generated papers, the majority were written under the auspices of these institutions and thus the total was not increased as the search went outside the established search engines. For the same reason- that we found a fairly bounded group of datasets, investigated by high-profile institutions and scholars publishing in well-established journals - we did not make contact with these experts to seek further research.

Independent verification of the papers by the two researchers was thus carried out for both selection and data extraction. The impact variables in the studies included were qualitatively aggregated as shown in Table A2.3 (Appendix 2.1). Quantitative aggregation for the purposes of the meta-analysis had to take a restricted form since it was not possible where only one study existed for a particular impact or where the units of analysis were not comparable. The final synthesis and search for causality was carried out jointly with Naila Kabeer.

3. Description of programmes

The studies included in the final list deal with programmes in nine countries (see Table A2.4 in Appendix 2.1). They comprise 11 separate programmes. In two cases a single programme has two names: Mexico's Progresas/Oportunidades (henceforth PO), which is considered as a single programme in this review. The name changed when the programme was expanded by a new government to include urban areas. The transfers involved were thus extended to a new population, and several of the studies included in the review used a baseline from one programme with a dataset that extended into the other. Nicaragua's case is similar: the Atencion a Crisis (AC) programme was a pilot within a larger CCT programme, the Red de Proteccion Social (RPS), and took its target population from within the RPS. Thus the studies are referred to synonymously in the review.

The three Brazilian programmes (Bolsa Escola, Bolsa Familia and PETI [Programa de Erradicação do Trabalho Infantil]) all began as separate programmes with, importantly, different targeting criteria. However, Bolsa Escola and Bolsa Familia were merged in 2003 and since then have shared a distribution mechanism and various conditionalities. The papers dealing with these two programmes all date from after their merging, and although they acknowledged their separate origins, treated them as part of the same system. These two are therefore listed together. PETI was incorporated into Bolsa Familia later, in 2009, and no papers covered in this review deal with the period after its inclusion. The only paper included in this review that deals with PETI exclusively is Yap et al. (2002), and the analysis therefore distinguishes it as a separate programme.

As can be seen from Table A2.4, with the exception of one study from Pakistan, all the studies included in the final review deal with CCTs in Latin America. Table A2.5 (Appendix 2.1) lists the countries with CCT programmes that either have not been evaluated yet, or have not produced findings based on methodologies compatible with the protocol for this review. Had these been included, they would have extended the reach of the review to several Caribbean countries (Jamaica, the Dominican Republic and Trinidad and Tobago) but apart from this would not have greatly increased the geographic reach of the project. CCTs originated in Latin America, and those that have been studied with the most depth and methodological rigour are still those implemented there.

As a related issue, the synthesis included studies in Portuguese and English. Spanish-language papers frequently occurred in the initial list, but were dropped as the process advanced, since they were generally working papers that led to a final version or refereed article in English. Due to the lack of rigorous evaluations available for the programmes outside Latin America, language did not lead to any papers being dropped from the list.

The 11 programmes considered in the review focus primarily on advancing the nutritional and educational status of children and therefore, as can be seen from Table A2.4, their age range of focus is generally defined as school-age children and their health elements are designed to target younger children and pregnant mothers. However, they also benefit other household members, so that the studies frequently take the household as the unit of analysis (particularly those on consumption), or the adults in the household (those focusing on labour supply), and where they do, they consider these to be direct effects of the programme.

Summary information on all studies is available via an additional Appendix C, which can be downloaded from this review's homepage <http://eppi.ioe.ac.uk/cms/Default.aspx?tabid=3364>

4. Household labour allocation

The first set of economic impacts we look at relate to household labour allocation. As we noted earlier, we would expect such impacts for a number of reasons. First of all, the focus of programme conditionality on increasing children's educational attendance is likely to have direct implications for their participation in the labour market and domestic work. This in turn may have implications for the amount of time adults spend in market work, household work and leisure. Conditionalities may increase unpaid domestic workloads both because children are no longer available and because of the additional requirements the conditions impose, particularly on women's time. They may increase adult labour force participation to offset the loss of children's income, or they may have a disincentive effect on adult labour, the 'purchase' of increased leisure. These impacts may vary by gender. Studies of impacts on household labour allocation generally focus on the 'extensive margin' (whether or not working) but a number also looked at the 'intensive margin' (hours worked).

4.1 Child labour

Most studies show that, along with increased enrolment rates, CCTs have helped to reduce child labour. In general, they support the generalisation that impact on schooling/work varies by starting conditions, often a function of age and gender, so that these variables proved to be important factors in explaining heterogeneity of impact. It was generally older rather than younger children, and boys rather than girls, who were most likely to be working and hence showed the largest impacts. However, larger enrolment impacts were frequently reported for girls because they were less likely to be at school at the start of the programme.

The first set of studies reviewed here relate to the impacts of PO. Schultz (2004) found that the level of enrolment rates of poor children in PO communities were significantly higher than those in similarly poor households in comparison communities. The differences were generally larger for girls than boys. The programme thus had the effect of reducing economic and gender inequalities in education in PO areas compared with non-PO communities. He also found that children in PO communities, particularly those in secondary school age groups, were less likely to engage in either household or market work. Relative to comparison communities, secondary school females were 4.1 percentage points less likely to be engaged in household and market work while secondary school males were 2.6 and 2.0 percentage points less likely to engage in market work and household work, respectively, and worked 0.16 hours less per day in PO communities. Primary school children also worked less in both market and household work and put in fewer hours of work per day.

Sadoulet et al. (2004) found that PO's impact on schooling among children aged 8 to 17 was greater at secondary than primary school level, suggesting that the decision to enrol children at secondary school level is one of the biggest hurdles to children's schooling and the grade at which the programme had its largest effects. They also found that the impact was larger for girls than boys, a reflection of the higher grants to girls at secondary school level. In addition, the most dramatic impacts on child labour were among children aged 12-14. The impact here was considerably larger for boys, which was to be expected as girls were less likely to be doing paid work.

Behrman et al. (2005) examined the impact of PO participation on rural youth (those aged 9 to 15 in 1997 at the start of the programme) using data from a 2003

survey. They found significant positive effects of greater programme exposure on 2003 educational attainment levels for both boys and girls but little impact on achievement tests. This may reflect the fact that the tests were applied to only a subsample of the original sample or it may reflect low school quality. Boys with longer exposure to the programme were less likely to be working in 2003 - many had continued in school - and were also less likely to be working in agriculture. The impact on girls, whose labour market participation in rural areas is low to start with, was insignificant. It is likely that given the brevity of the programme, the search for impacts on work trajectories is premature.

Skoufias and Parker (2001) found the largest impacts on use of time for children aged 12 plus, i.e. those of secondary school age. They found a larger impact on school participation by girls than boys, at almost double the impact. These increases in schooling were accompanied by reductions in work. A reduction in both household and market work was shown for boys while a significant reduction in domestic work was reported for girls. However, one point worth noting is that while the reduction in boys' work time was approximately equivalent to the increased time in schooling, suggesting education and work were competing activities, in the case of girls, the reduction in working time was somewhat less than the increase in time at school, suggesting a certain degree of compatibility in the two activities.

Income poverty and other indicators of socio-economic disadvantage (ethnicity, location) also served to differentiate programme impact. Angelucci and de Giorgi (2009) posited that greater reductions in child labour seen in rural locations during PO are due to location. Since urban children receive higher wages than rural ones, the opportunity cost of going to school is much higher for them. PO's scholarships represent less than half an urban child's potential wage, but between half and two-thirds of a rural child's, which may explain the disparity in PO participation rates between urban and rural areas (some 50 percent of eligible urban poor do not take up the programme). Bando et al. (2005) used data from 1997 and 1999 to explore differences in the impact of PO by ethnic identity. They reported that indigenous children speaking only their own language had a greater probability of working compared with Spanish speaking and bilingual children, and a lower probability of working, at the start of the programme. Participation in the programme reduced the likelihood of working among indigenous children to a greater extent than the two latter groups (26 percent reduction for the former compared with 25 percent for all children). Programme participation also reduced the ethnicity gap in schooling, particularly in older (13-16) age groups. Household poverty, married parents and the number of children under 12 in the household were also associated, in participating households, with a reduction in the likelihood of working and an increase in the likelihood of education. This could be because indigenous families in Mexico, who are the most marginalised and poor, also see the greatest relative benefit from programme transfers.

Cardoso and Souza (2003) used 2000 census data to examine the impact of the Bolsa Escola programme in Brazil on children's education and work patterns. They found a significant difference in boys' school attendance among Bolsa Escola participants and non-participants but no discernible difference in child labour. Transfers reduced the percentages of children who worked to the exclusion of any schooling or who neither worked nor went to school. It increased the percentage of those who combined work and school. The results for girls were similar but there was a small positive impact on child labour, presumably because girls who did not work or go to school at the start of the programme began to work and go to school at the same time. The impact of transfers was larger among poorer children. Cardoso and Souza (2003) suggested that the amount of the transfer was too small

to incentivise children to give up working entirely, particularly as they had the option of combining work and school.

Oliveira (2005) used a 2005 survey to examine the impacts of Bolsa Familia on various aspects of household behaviour compared to households who participated in other programmes, including PETI and the School Grant (comparison group 1), and households who had not received any kind of cash transfer (comparison group 2). She found higher levels of school attendance for children aged 7-14 in comparison group 1 compared to Bolsa Familia participants, possibly because the other programmes had been in existence longer. This differential was higher for poorer families. In addition, higher rates of drop-out were found among males of Bolsa Familia families in the north/mid-west regions. However, school attendance was higher among Bolsa Familia participants relative to comparison group 2 and drop-out rates were lower.

The PETI programme was implemented in the poor rural states of north-east Brazil where the rate of child labour is extremely high. In addition to children aged 7-14 being required to attend school, the programme also requires them to attend an after-school programme which effectively doubles the length of the schoolday and makes child labour after school hours difficult. Using 1999 survey data, Yap et al. (2002) found that the programme increased hours at school for children from PETI households, with larger impact for children who had been in the programme longer. The programme did not reduce time in school for non-participating children. The probability of working fell in most states for PETI children along with the probability of working for non-participating children but by an unknown amount. It appeared to be the after-school element of the programme that was most important in combating child labour. Examining hours worked, it appeared that PETI had been more successful in reducing child labour among children in part-time work than those working full time. However, there was no evidence that the latter were entering riskier forms of labour.

In Nicaragua, Del Carpio and Marcours (2009) used survey data from 2005 and 2006 to examine the impacts of *Atencion a Crisis*, a pilot programme instituted after a severe drought in north-west Nicaragua in 2005. It should be noted that the study was carried out very soon after programme inception so that it is likely to capture only short-term effects. The programme had three components: (i) a basic CCT to all eligible households, (ii) a vocational training scholarship for a third of the selected households and (iii) a grant for productive investments, backed by training in basic commercial skills for a third of households. The programme was found to reduce child labour within participating households, with larger reductions for boys than girls: reductions were particularly marked for older boys (12-15) and in agriculture and livestock activities. The impact on girls did not vary by age although there was a small increase in domestic workloads of older girls.

The investment grant had complex effects. The researchers began with the hypothesis that programmes designed to increase all productive activity in the household might also increase that of children, and indeed they found that the grant increased non-physical work for both girls and boys, but did not increase their total working hours. There were reductions in physical labour hours for both genders, primarily among older boys, decreasing their work in agriculture and livestock, and among younger girls, who benefited more than older ones. There was, however, a larger shift of older girls into non-agricultural activities and domestic work compared to younger siblings. The grant thus appeared to reinforce the specialisation of older girls in domestic/non-agricultural activities and away from domestic chores.

Del Carpio's (2008) study also examined the impact of the CCT in Nicaragua on children's work patterns. This study found that, in general, child labour initially increased with income and then declined. The decline in girls' participation in work began at lower levels of income than for boys. The impact of the CCT was to 'tax' child labour, making parents less inclined to send their children to work. Disaggregation by types of work showed that the decline was largely in physical forms of labour while involvement in non-physical work increased or stayed the same.

The authors suggested that the findings of these two Nicaraguan studies relate to the timing of children's work activities. Farm work is generally a morning task and thus competes with school, whereas domestic and other work is less time-specific and may not. Boys' farm work can also be substituted by paid labour, while girls' tasks generally cannot. This explanation is supported by the finding that the programme increased the likelihood of households hiring agricultural labour by about 20 percent, and that the new productive activities funded by the business transfer, which were found to increase children's work, were generally small scale and did not involve hiring workers.

Dammert (2009) examined the impact of the RPS programme in Nicaragua, using data from 2001 and 2002 surveys. She found that the RPS programme increased school attendance by 18 percent for boys aged 7-13 in 2001 compared to 12 percent for girls. The reduction in probability of engaging in market activities, and hours worked, was also greater for boys than girls: 11 percent in 2001 and 14 percent in 2002 compared to 1 percent for girls in both years. Unlike PO, RPS did not provide larger stipends to girl enrolled in secondary school, which may explain the absence of a marked impact on girls. Dammert (2009) noted that the focus of the survey was on market work, where boys predominated, rather than unpaid domestic work, associated with girls. A broader definition of work might have reduced the gender differential in work-related impacts. Older children, who earned more, experienced smaller impacts in terms of both school and work. The size of the impact also varied according to variables that lowered the margins for improvement: more educated household heads, households headed by males, and children in larger households were all more likely to be in school already and less likely to work. Children living in more impoverished localities experienced greater impact in terms of their schooling in 2001 compared to other children but a smaller impact in 2002.

In Uruguay, where the school attendance rate for children aged 6-17 is nearly 100 percent, it is not surprising that Borraz and Gonzáles (2009) found no evidence of impact of the Ingreso Ciudadano (IC) programme on school attendance in this age group, using 2006 data. However, there was also no evidence of impact among children aged 12-14 where attendance rates are lower nor was there any evidence of impact on child labour with the exception of a negative effect for girls in the capital city of Montevideo. No explanation was provided for these findings.

Attanasio et al. (2010) examined the impacts of Familias en Acción (FeA) in Colombia on school and work participation by children at both extensive and intensive margins. They found that the programme increased school participation rates of 14- to 17-year-old children by between 5 and 7 percentage points, reaching enrolment rates of 64 percent and 82 percent in rural and urban areas, respectively. It also impacted on school enrolment rates for young children, despite their already high participation rates. In terms of impact on intensive margins, these were larger in urban areas where school attendance went up by 3.8 hours a day for older and 4.5 hours a day for younger children compared to 1.0 and 2.5 hours for older and younger rural children, respectively. There was also a sizeable

negative impact on domestic work participation in urban areas but time spent in such work was less than the increase in time spent in school, suggesting parents are substituting other uses of children's time, such as leisure, rather than significantly reducing time spent in income-generating activities. It is therefore unlikely that household income has been negatively affected by the programme.

4.1.1 Summary of findings

Summing up, the evidence for this set of impacts comes from a number of different countries. CCTs generally increased children's education but their impacts on schooling/work varied by age, gender, ethnicity and location. This partly reflected variations in initial conditions. Older rather than younger children, and boys rather than girls, were most likely to be working and hence report larger impacts. Larger enrolment impacts were often reported for girls because they were less likely to be in school at the start of the programme. Reductions in child labour were greater at secondary than primary school level. They were greater in rural locations since wages were lower there, and therefore the opportunity cost of going to school was lower than for urban children (Mexico). They were also larger among poorer children, ethnic minority children and children working part time rather than full time. In Brazil, transfers increased the number of children who combined work and school.

The issue of trade-offs proved to be important in explaining the gender pattern of some impacts. Very often, the larger reductions in work reported for boys reflected the fact that paid work was less compatible with schooling. Girls, on the other hand, were concentrated in housework, generally easier to reconcile with schooling and so lower reductions in work time were reported for them.

4.2 Adult labour

Given that decisions about the allocation of labour by household members are not likely to be made in isolation by individual members, we would expect CCTs to have impacts on the allocation of adult labour within the household, both directly as a result of the income transfer but also indirectly via possible impacts on child labour. Overall, the evidence base for CCTs' impact on adult labour participation is much weaker and less consistent than that for child labour. One study in our review suggested that CCTs had a disincentive effect on adult labour. Borraz and González (2009) investigated the impact of the IC programme in Uruguay on the labour market activities of household members aged 22-55. They found the programme had a significant negative effect on hours worked in urban areas outside Montevideo: women and men in households receiving the transfer worked on average 6.4 and 2.5 hours less a week, respectively. This amounted to a decrease of 17 percent in women's labour supply and 5 percent in men's. The programme had no effect on likelihood of working, or likelihood of working in the informal economy. The study did not offer any explanation for this apparent disincentive to adult work effort.

Other studies were less conclusive. A number of these studies related to the Bolsa Familia/Bolsa Escola programmes in Brazil. As noted earlier, Oliveira (2005) used a 2005 survey to examine the impacts of Bolsa Familia on various aspects of household behaviour compared to households who participated in other programmes, including PETI and the School Grant (comparison group 1) and to households who had did not receive any kind of cash transfer (comparison group 2). Along with impacts on children's schooling and work, she examined impacts on adult labour supply, in terms of both proportions of adults who worked in the previous month and proportions who looked for a job in the previous month. The results showed that a higher proportion of adults in Bolsa Familia households were

likely to have worked in the previous month relative to those who did not receive any transfer - with the exception of the poorest families in the north/mid-west region. However, the proportion of women from Bolsa Familia who had worked was lower relative to comparison group 1. This could reflect a disincentive effect of the programme or greater demands from domestic activities. The largest differentials were observed among the poorest households. At the same time, a higher proportion of adults in Bolsa Familia households had been looking for a job in the previous month, particular among lower-income families, relative to members of either comparison groups. The exception to this pattern was among poorer women in the south/south-east relative to comparison group 2 who were more likely to already be in work.

Tavares (2010) estimated the impact of Bolsa Familia on mothers' labour supply. Estimating the average treatment effect via a propensity score matching procedure, she found it to be between -4.15 percent and -8.96 percent. The Bolsa Familia programme appeared to have reduced mothers' time spent working by 4-9 percent per week. However, when she controlled for self-selection of women into the labour market (using factors such as education, location and marital status which might affect the likelihood of a woman receiving a job offer), the reduction in women's hours worked was somewhat higher, ranging between 9 percent and 12 percent per week.

Teixeira (2010) used the 2006 Brazilian National Household Survey to estimate the impact of Bolsa Familia on adult labour supply. Her overall conclusion was that the programme had no impact on the probability of work for men or women (the extensive margin) and a small but statistically significant impact on hours worked a week (the intensive margin). There was thus little evidence of a disincentive effect. She also found that the elasticity of labour supply varied according to gender, size of the transfer and occupation. The likelihood of women working was more responsive to the size of the transfer than was the likelihood of men working, with the effect concentrated among those receiving between US\$14 and US\$21: the transfers ranged from US\$7 to US\$45 per month.

The impact on hours worked per week was to reduce it by 0.56 hours for men and 1.18 hours for women (1.3 percent and 4.1 percent respectively). The reduction in time in market work translated into an increase in household work for women and leisure time for men. Men put in nine hours a week in housework compared to 24 hours by women. In terms of occupational patterns, those in formal work were least responsive to the transfer. Women in non-agricultural self-employment were most responsive, reducing their average labour supply by 2.1 hours (7.3 percent) per week. No impact was shown for men in this occupation.

Foguel and Barros (2008) examined the impact of a number of CCT programmes in Brazil on adult labour supply at both extensive and intensive margins. Their analysis was based on a national cross-section household survey which is carried out annually for the same set of municipalities. They used data for the period 2001 to 2005. They found little impact on female labour force participation rates as a result of programme participation and suggested that child care responsibilities might be inhibiting women's capacity to increase their labour force participation. There was a small but statistically significant increase in male participation rates: a 10 percent increase in CCT beneficiaries in the population of a municipality led to an increase of 0.05 percent in participation rates. CCT had a negative but negligible impact on female hours worked. Thus the impact of the programme was negligible in terms of female labour force participation but negative in terms of hours worked: 'since there are various channels through which these two effects may be connected it is difficult to offer an explanation for this result' (Foguel and

Barros 2008: 14). The effect on male hours was positive but not significant. Household income levels did not affect any of these results.

In Mexico, Skoufias and di Maro (2008) analysed the impact of PO on adult labour force participation using several rounds of data between November 1997 (the baseline survey) and November 1999. They found that, irrespective of age group, participation in Progres/Oportunidades had little or no impact on adult male labour force participation but some evidence that the transfers were initially used to shift men out of self-employment or unpaid work into waged work. Similar findings were reported for women. There is little evidence of any impact on the leisure time of either men or women.

The Chile Solidario programme differs from the other CCT programmes in its design features. It was introduced in 2002 and targeted at indigent households. It has a number of different components. The first component entails two years of intensive interaction between targeted households and social workers and the provision of direct cash transfers at a decreasing rate over this period. The interactions are intended to assist households in assessing their needs, devising strategies to exit extreme poverty and connecting them to various relevant social programmes. Households are then assured a direct cash transfer and preferential access to assistance and training programmes for an additional three years. The cash transfer is conditional on participation in the programme rather than on behavioural requirements relating to school enrolment or health visits.

Galasso (2006) evaluated the programme during its first two years of operation. She found no evidence of impact on total or labour income of participating households. While beneficiaries were more likely to take up labour market programmes, the results had not at that point translated into gains in their labour supply, either in terms of share of household members employed or share of household members in stable employment. This may reflect the short time-span covered by the evaluation. An area where the study did find considerable positive impact was the school enrolment rates, a result attributed to the role of the social workers.

In Pakistan, a CCT programme was introduced in Punjab in 2004 which offered a stipend to girls if they enrolled in grades 6 to 8 in a government school and maintained a minimum attendance of 80 percent. Hasan (2010) used data from successive rounds of a government survey on education and learning in the province to explore the impact of the programme on women's time allocation. The 2004, 2006 and 2007 surveys carried information on time use by mothers in the survey households. He found that mothers in stipend-eligible households increased their time in general household work by around 1 hour 40 minutes and reduced their participation in paid work. The increase in household work did not entail an increase in time spent on children's needs. The author suggested that this is because treatment households have more boys, fewer girls aged five to seventeen, and more infants under five. Thus the reduction in time spent on children's needs results from the girls going to school and being out of the house, while the increase in housework occurs where mothers can no longer delegate household tasks to their daughters on schooldays.

4.2.1 Summary of findings

The findings on impacts on adult labour were less consistent than those for children. A study for Uruguay found a disincentive effect in most urban areas, with men reporting a greater reduction of working hours than women. A study from Brazil, on the other hand, found that those in receipt of transfers were much more likely to have worked in the previous month than those without transfers, with the

effect stronger in households who had been in receipt of transfers for a longer period. Another study, also from Brazil, also found little evidence of a disincentive effect on hours worked but found that the likelihood of women working was more responsive than the likelihood of men working to the size of the transfers. There is evidence of an increase in job-seeking in Brazil and one study from Mexico found that transfers seemed to shift men out of self-employment or unpaid work into waged work. In Pakistan, a study found that the increased school attendance by girls had led mothers to shift out of paid work into household work but they were spending less time on children's needs. In general, therefore, we find that the impact of transfers on adult labour supply varied by gender, size and duration of transfer and type of employment.

4.3 Migration patterns

The impact of CCTs on household labour allocation may also include impacts on migration patterns, although the direction of the impact can go either way. For example, cash transfers may help to finance the costs of migration in search of employment or they may render migration less necessary. The available evidence on this issue is based on studies of PO. They offer apparently contradictory findings. Angelucci (2004) found that PO transfers were associated with a 60 percentage point increase in the average US migration rate, with no change in the domestic rate. She found that the proportion of households with at least one international migrant rose from 0.9 to 1.4, indicating a 60 percentage point change. However, average household migration was the same across the treatment and control households. She interpreted this to mean that programme participation leads to new households beginning to send their members abroad, rather than increasing all US migration. This could be either because households with pre-programme migration were wealthier to begin with, or because remittances made them better off. She noted that these effects occurred within months of the programme beginning, when very little money had been transferred, and before any schooling subsidies had begun. These effects, then, may be due to the anticipation of transfers among eligible households: 'the existence of the programme and the certainty of eligibility may have loosened financial constraints for poor households also through general equilibrium effects' (Angelucci 2004). She also demonstrated that this short-term rise in international migration is not likely to persist into the medium term.

Stecklov et al. (2005) found that while migration levels were increasing over the period under study, PO slowed this increase for households in treatment communities. It did little to stem the flow of rural migration to domestic, mainly urban, destinations but it had a substantial and significant effect on the flow of migrants to the USA. After only 20 months of operation, it reduced the probability of US migration by 0.2 percentage points. Given that levels of migration to the USA, particularly during such a short time period, were very low, this was the equivalent of reducing the odds of migration to the US by 58 percent. The authors did not find evidence to suggest that either the initial wealth of households or their migration networks at household or community level moderated the impact of PO.

The studies both offer 'average treatment effect on the treated' (ATT) estimates, but Angelucci (2004) used individual-level data, while Stecklov et al. (2005) used household-level data which take account of the theory that migration decisions are made on the household rather than the individual level. Another difference is that Angelucci (2004) used five years of migration data, some from before Progres/Oportunidades began, claiming that these pre-programme migration rates were not significantly different. Stecklov et al. (2005) claimed that they were

higher in treatment communities prior to the programme and this may explain her finding that PO increased migration.

In line with Stecklov et al. (2005), Behrman et al. (2005) found that the programme reduced migration (domestic and US) among male youth by 6 percent but did not impact on female migration. However the most recent study by Azuara (2009) found that participating villages lost 10 percent of their population to migration over the period 1995-2005. Sixty-four percent of the reduction corresponded to adults who had left their villages and 40 percent could be attributed to the programme. The rate of migration was higher for men than women, and was sensitive to the village's proximity to rail transport: the closer the village to railway lines, the larger the decrease in the number of males relative to females. Thus the authors found the programme was having a positive effect on male migration, particularly northwards.

The studies suggested several moderating factors including the size of the grant, with Angelucci (2004) finding a positive correlation between grant size and international migration. Angelucci (2004) and Stecklov et al. (2005) agreed that the conditionality of the programme reduced migration: Angelucci found that conditioning the grant to secondary school attendance reduced US migration, and Stecklov et al. posited that the condition requiring adults to make yearly clinic visits in order for their household to remain eligible caused them to return from migration or not migrate when they otherwise would have done.

Schultz (2004), in his study of the impact of Progresa/Oportunidades on child school enrolment and child labour, made a point about domestic migration that is useful to contextualise these results. He pointed out that if the programme results in more children enrolled in school, and in consequence gaining higher educational qualifications, this can be expected to result in increased domestic migration. This migration would in fact be economically desirable, since it would move the young out of regions of extreme poverty and into non-agricultural sectors of the economy, which in turn would result in increased long-term growth. Thus the programme may be considered to be indirectly encouraging a long-term rural-to-urban domestic migration flow, with corresponding potential growth effects.

4.3.1 Summary of findings

All the evidence on the impact of transfers on migration is from Mexico. The evidence appears to be mixed: transfers may help to finance migration, or may make it less necessary. One study found a short-term rise in international migration, possibly due to households' anticipation of transfers, but another found that transfers slowed the increase in migration, with stronger effects for men than women, and for those living close to railways. Moderating factors included the size of the transfer - larger grants meant more migration - and conditioning the transfer to secondary school attendance or clinic visits.

5. Household expenditure patterns

Our first set of impacts focused on possible changes in labour allocation within the household as a result of CCTs. A second set of impacts concern changes in household expenditure patterns. As Attanasio and Mesnard (2005) pointed out, it would be reasonable to assume that CCTs have a positive effect on total consumption expenditure, because they presumably increase the disposable income of very poor households. The actual effects may not be that straightforward. It is possible that disposable income does not increase by the full transfer amount because of costs associated with conditionality, including income foregone from child labour. Alternatively, some of the transfer may be used to save, to invest or to pay off debts. Households that use most of their transfers to finance programme conditionalities or their own basic consumption needs are unlikely to have a great deal left over for investments or savings. Consequently, understanding the impact of cash transfers on levels and patterns of current consumption is an important part of the overall story of longer-term economic impacts.

5.1 Consumption

Studies which examined the impact of transfers on household consumption found that both total and food consumption increased as a result of the transfer (e.g. Gitter and Caldes [2010] for Nicaragua; Angelucci and Attanasio [2006] for urban Mexico; Hoddinot and Skoufias [2004] for rural Mexico; Handa et al. (2009) for rural Mexico; Rubalcava et al. [2009] for rural Mexico; Attanasio et al. (2006) for Colombia).

However, there is some variation in how this impact was explained. Angelucci and Attanasio (2006) used data from successive rounds of surveys of urban households in Mexico to evaluate the impact of PO on household consumption, distinguishing between food and non-food consumption. They found that it was the poorest households that participated in the programme, that the overall propensity to consume food and non-food items bought with the transfers was around 80 percent, that this increased over time and that a large fraction of this increase was in food. This was not surprising given the poverty of the urban households who participated in the programme and who were least likely to be able to save or invest. These conclusions were supported by Resende and Oliveira's (2008) study of Bolsa Escola in Brazil, which showed that there, too, families consumed 85 percent of their transfer income.

Hoddinot and Skoufias (2004) used data on rural households to show that, over time, PO households consumed more calories, and higher-quality food, than similar households in control communities. While the increase in calories may have represented an income effect, the authors suggested that the greater diversity of diet may have reflected nutritional information imparted by programme lectures. There was some evidence of information 'spill-over' in that non-beneficiary households in PO communities also reported increased diversity of diet.

Handa et al. (2009), using data from rural households, similarly found an increase in consumption associated with the transfer. They interpreted this as a pure income effect. They found that the marginal propensity to consume out of general income in terms of total, food and adult clothing expenditure was significantly higher than for PO transfer income. This was also true when the focus was on the propensity to spend on schooling-related expenditure out of the school subsidy element of the transfer. This led them to suggest that increased levels of consumption observed in relation to PO transfers represented an income effect

rather than the effect of targeting of transfers to women rather than men, the key rationale for this design feature.

Findings from other studies on PO contest this finding, suggesting that the targeting of women for the transfers does play a role in explaining expenditure patterns. Adato et al. (2000) found that women in households in receipt of PO transfers were more likely to share in household decision-making regarding food expenditure and house repairs than women in control households. Rubalcava et al. (2009) reported that in households headed by a single males or females, PO transfers were not treated any differently from general household income but in couple-headed households, PO income was more likely to be spent on higher-quality nutritional intake, children's clothing and, as we discuss in greater detail later, female-associated forms of investment. This suggested that transfers to women in couple-headed households were treated differently from general household income.

Focusing on the impact of the Familias en Acción (FeA) programme in Colombia, Attanasio et al. (2009) found a divergence between the prediction of Engel's Law that the share of food in household expenditure should decline with receipt of the CCT and their empirical results, which showed an increase. They suggested that the theoretical assumption of a unified household welfare function which underpinned the Engel's curve failed to take into account the possibility that income transfers targeted to women changed the relative weight given to women's preferences in spending decisions.

5.2.1 Summary of findings

Both total and food consumption increased as a result of the transfer (Mexico, Colombia), with an increase in food consumption and dietary diversity (Mexico) and spill-over effects to other households. Transfers to women in couple-headed households resulted in higher-quality nutritional intake, purchases of children's clothing and female-associated forms of investment, but the same was not found in households headed by a single parent, whether male or female. This suggested that women were likely to use cash at their disposal very differently from men in their households.

5.2 Savings and investment

A number of studies have moved beyond the focus on consumption to consider impact on savings, assets and livelihoods. Davis et al. (2004) noted evidence of an increased propensity for productive investment spending by PO households over a period of one year of operations. Angelucci and de Giorgi (2009) found evidence of increased likelihood of agriculture-related expenses and purchase of livestock among PO households. A more detailed analysis was carried out by Todd et al. (2010). They found that PO increased the likelihood of household food consumption from own food production. The effect was largest in the case of landless households, smaller for smallholder households and negligible for large landowners. Together with increases in the value of consumption and seasonal variety of food items consumed, these results suggested that the programme increased entry into the production of foods of higher market and nutritional value, particularly among the asset-poor.

Direct support for this was provided by further evidence that PO significantly increased use of land for agricultural production, particularly among the landless where it increased both the probability of using land in this way and per capita hectares used. It also increased the probability of owning livestock as well as the quantity owned, particularly among smallholder farmers. In addition, the programme led to an increase in the probability of expenditure on variable

agricultural inputs and on spending per capita and per hectare: these effects were strongest for smallholder households. The authors concluded that prior access to land plays a critical role in the productive impact of PO. While those with no land were able to enter production and become involved in more activities, the impact was largest on smallholders who substantially expanded consumption from own production, increasing the amount of land under use and livestock owned. In general, the authors concluded that their findings were consistent with the view that cash transfer programmes can relax liquidity constraints and lead to increased investment in productive activities and assets.

Rubalcava et al. (2009) explored in some detail the extent to which the use of PO transfers for investment purposes reflected women's control over their allocation. They found that PO households were spending around 15 pesos per person per month more than control households. The average size of the transfer was 30 pesos per capita per month, suggesting that part of the transfer was being saved. The data confirmed that PO households saved over 13 pesos per capita per month more than control households. Few households had financial savings. Instead PO households invested their savings in livestock and poultry: they owned significantly more of both than control households. The study found that, after controlling for total household resources, increases in PO income are associated with increased probability of owning small livestock and poultry as well as the numbers owned. Since these are categories of investment that typically fall within women's economic domain, the authors suggested that patterns of investment reflect women's control over PO transfers. The effect for larger livestock, associated with male economic activity, is smaller and insignificant.

Gertler et al. (2006) also found that PO households reported higher levels of monthly consumption than households in control communities, around 88 percent of the average monthly transfer. The remaining 12 percent was invested or saved. The authors estimated the effects of PO on the probability of owning draft animals, production animals and land as well as the amount of assets owned. The probability of owning an asset conditional on having none at the baseline provided insights into whether the programme induced households to start a farm business (variation at the extensive margin) while the impact on the amount of an asset, conditional on having some of that asset at the outset, provided insights into impact on expansion of already existing business (variation at the intensive margin). The authors found that programme participation increased the likelihood of owning livestock and land, conditional on owning none at the outset. Conditional on owning these assets at the outset, PO households are likely to increase the number of livestock and poultry and increase land usage. Impacts here are larger for larger farms. The study also found programme participation increased likelihood of engagement in micro-enterprise, with somewhat larger effects for female activity. Another noteworthy pattern was that households receiving higher accumulated transfers over time had a higher likelihood of investing in agricultural assets. This pattern was only significant in the top quintile suggesting there is a possible threshold in accumulated transfers before households begin to invest.

In conclusion, Gertler et al. (2006) estimated that the return on each peso invested was 15.5 percent in terms of consumption and 17.5 percent in terms of income. The implied long-term effects of the transfer on living standards through the investment pathway were dramatic: after 5.5 years, transfers increased consumption by 34 percent through the investment pathway. Since this increase was through investment, it should continue even if the household no longer participated. These results suggested households were liquidity constrained. The estimated rate of return on investment was substantially higher than the average annual real interest rate in the economy for the entire period 1997-2005. In fact,

only 2.4 percent of households reported access to credit. 'Further understanding of the mechanisms through which cash transfers boost productive investments (softening of liquidity and/or credit constraints, reduction of risk aversion, insurance role) is crucially important in the determination and design of future policies to be undertaken...' (Gertler et al. 2006).

5.2.1 Summary of findings

CCTs seemed to lead to increased investment in productive activities and assets by relaxing liquidity constraints. They increased the likelihood that those with few assets would use land for agricultural production, and would start to produce foods of higher market and nutritional value. They also increased the amount of households' savings (Mexico). In Mexico the return on each peso invested was 15.5 percent in terms of consumption and 17.5 percent in terms of income, substantially higher than the average annual real interest rate in the economy. Women were able to use their receipt of cash transfers to purchase assets that they were better able to control.

6. The insurance role of transfers

As Maluccio (2005) pointed out, CCTs are not designed as traditional safety net programmes in the sense of reacting or adjusting to crises or shocks. At the same time, they have the potential to perform an insurance role in times of crisis, not only helping households to smooth consumption flows in the face of shocks of various kinds but also averting some of the more damaging crisis coping mechanisms (such as selling off assets, taking children out of school and/or putting them to work). At the same time, given the size of some of the transfers - Skoufias (2007) noted that PO transfers are close to 20 percent of pre-programme consumption - they also have the potential of not simply complementing but also displacing pre-existing risk sharing arrangements among households. Examination of the insurance role of CCTs has therefore taken a number of forms. A number of studies focused on whether they enable households withstand shocks and avoid negative coping strategies while others were concerned with impact on informal risk sharing arrangements.

6.1 Coping with crisis

The impact of both idiosyncratic and covariate shocks on children's schooling and work patterns, and the effects of PO transfers in moderating it, was examined by de Janvry et al. (2006). The shocks in question included unemployment and illness of the household head, droughts, natural disasters and loss of assets. As the authors pointed out, there was no reason to expect PO to have symmetrical effects on schooling and child labour in the event of a shock: dropping out of school induces a loss of the transfer but entering the labour market does not preclude receipt of the transfer.

They found that PO helped to mitigate the likelihood of reductions in school enrolment in response to illness or unemployment of the household head. While droughts had negligible impact on children's schooling, natural disasters generally had a dramatic effect. This effect was found to be completely mitigated by participation in PO. The smaller effect of asset loss was similarly mitigated.

Some heterogeneity was observed in the effects of shocks on different subgroups of children. The unemployment of the household head and natural disasters had the greatest impact on the enrolment rates of primary school children, indigenous children and children of agricultural workers compared to secondary school children, non-indigenous children and children of non-agricultural workers. Boys were more affected than girls by unemployment of the household head while girls were more affected by natural disasters. In all cases, PO largely or completely erased the negative effects on schooling.

An important point to note is that in the control villages, temporary disasters had both immediate effect in taking some children out of school and long-term impact through the state-dependence effect. The effect on children who quit school in the face of a temporary shock was to reduce the likelihood of attending school in the following semester. This state dependence was highly robust across shocks so that a one-time disaster reduced probability of enrolment by 3.4 percent immediately and 0.37 percent the following semester. State dependence was particularly high for secondary school children. Any event that takes a secondary school child out of school has a large, lasting effect. Conversely an event such as a PO transfer that induced a secondary school child to stay in school would also have a lasting effect.

As far as child labour was concerned, unemployment of the household head did not induce an increase in child labour but natural disasters, illness of the household

head and illness among siblings did: this effect was not offset by PO. Droughts were found to reduce child labour in the control villages, probably because of reduced opportunities for children to work and a greater supply of adult labour. In PO villages, children generally worked less, and this was not affected by drought. Older children (15-18) who were most likely to be in the labour market were strongly affected by climatic but not idiosyncratic household shocks. Younger children, by contrast, increased their work participation in the face of household-level shocks. Boys were more likely than girls both to participate in the labour market and to respond to shocks. Overall the conclusion was that the income effect of PO was not sufficient to prevent increase in child labour in response to shocks but the increase was not at the expense of schooling as a result of the price effect of the transfer.

Maluccio and Flores (2005) and Maluccio (2005) both examined the impact of RPS in Nicaragua using data collected before the start of the programme in 2000 and then after the programme began operating in 2001 and 2002. This was a period of economic down turn for the whole economy as a result of the collapse in coffee prices. They found that RPS households were using a large proportion of their transfers for current expenditure. This fraction had reduced by 2002 when the area under study underwent partial recovery compared to 2001. The transfer compensated for the large income loss experienced by non-beneficiaries during this period while producing a small overall increase in expenditure which continued into the recovery period. The difference was significant: the per capita real annual expenditure of RPS beneficiaries in coffee areas increased by 27 percent in 2001 and decreased by 22 percent in control households in coffee areas. Much of the increased expenditure was on food and led to an improvement in household diet.

There was also an increase in the propensity to spend on schooling. In addition, there was a smaller increase in working hours among beneficiaries as a result of the shock than non-beneficiaries in coffee areas, and a reduction in child work in these areas among girls 7-12 years old, although no effect was seen for boys.

In contrast to the findings reported for the PO programme in Mexico, Maluccio (2010) found no evidence that the programme led to increased investment in productive assets by participating households over the one, two and four years of its operation. Nor was there any impact in terms of off-farm micro-enterprise activity, a pathway out of poverty in rural Nicaragua. It may be that the emphasis on children's schooling discouraged investment in such activity or that a poor transportation infrastructure meant that micro-enterprise yielded little profit.

Coady et al. (2003) carried out an analysis of the impact of PRAFII (Programa de Asignación Familiar) in Honduras using data from a period (2000 and 2001) when the price of coffee, a major crop in terms of output, employment and export earnings, had fallen to its lowest in several decades. They found that the programme appeared to more than offset the drop in expenditure of coffee growing households caused by lower returns to coffee land. However it did not have any impact on returns to coffee land, suggesting that it had not been used to alleviate liquidity constraints on productive investments. There was also evidence that transfers could reduce the likelihood that children were taken out of school and put to work in response to household-level shocks. In other words, they reduced the likelihood of one of the negative forms of self-insurance noted earlier.

6.1.1 Summary of findings

Mexico and Nicaragua provided contrasting results. CCTs helped to mitigate the likelihood of reductions in school enrolment in response to illness or unemployment of a household head, largely or completely erasing negative effects on schooling

(Mexico). They mitigated market shocks for producers (Nicaragua), with the extra money spent on food and child schooling. However, in Nicaragua there was no increased investment in productive assets, or in off-farm micro-enterprise activity. In Honduras, CCTs reduced the likelihood of children being taken out of school and put to work in response to household-level shocks

6.2 Risk sharing arrangements

Skoufias (2007) examined the extent to which PO transfers helped households cope with income shocks and their effects on pre-existing risk sharing arrangements. He took low covariance between household income and consumption flows as indicative of effective self-insurance and/or risk sharing arrangements within the community. Households most vulnerable to shocks were found to be those with household heads who were poorly educated or who were self-employed, indigenous households and households with few assets. The study did not find any evidence that PO transfers either displaced or strengthened pre-existing risk sharing arrangements within the community or brought about any change in how households responded to shocks. However, PO households were able to insulate their consumption levels from fluctuations in income flows more effectively (by 1.4 percent, in the case of a 10 percent income shock to total consumption) than their counterparts in the control villages. This effect was stronger, at 9.5 percent, for self-employed heads of PO households.

Also in Mexico, Teruel and Davis (2000) explored whether the transfer of money to poor families has any impact on informal pre-existing private transfer arrangements among families, both those involving transfers to PO families and those involving transfers by PO families to others. Evidence of such impact would suggest disruption of long-term support systems within the community and could lessen the value of the net transfer represented by PO. They found that after 19 months of PO transfers, there had been a drop in the level of private transfers among treatment and control households within PO communities, but not within the community as a whole. They suggested that control households may have under-reported their transfers in order to gain eligibility for the programme, and treatment households may have done the same so as not to lose their benefits. There was evidence of a drop in non-monetary transfers in treatment households which could have been due to a PO crowding-out effect, but the authors could not attribute this effect to the programme with certainty.

Teruel and Davis (2000) also suggested that PO may have different impacts on transfers from different sources, with neighbours and friends reacting differently to the presence of the programme to those living further away, such as migrant children. In addition, it was possible that targeting certain households could cause conflicts in treatment areas that could affect transfers. However, the data did not conclusively support either suggestion.

In their study of the extent to which PRAFI in Honduras and RPS in Nicaragua crowded out other private transfers to recipient households, Nielsen and Olinto (2007) distinguished between different measures of private transfers: prevalence of private food transfers; household consumption of donated food; probability of receiving remittances; the value of remittances received; NGO transfers of food/money; and the probability of receiving any of these categories of transfers. They found no evidence that CCTs crowded out remittances but some evidence that they crowded out inter-household food transfers and NGOs' food and money transfers in Nicaragua. In Honduras, however, programme participation significantly increased the amount of food households received. The authors suggested that one reason why crowding out occurred in Nicaragua but not in

Honduras may be that the benefit payments were too low to crowd out other transfers in Honduras, while the larger amounts given out in Nicaragua had an effect on private networks and informal insurance schemes.

Hernandez et al. (2009) compared the impact of transfers from Nicaragua's RPS programme and remittances from domestic and international migrants on the decision of poor rural households to seek loans. The data related to 2000, the baseline year, and 2001 and 2002, the pilot phase of the programme. They found that receipt of RPS transfers did not influence household decisions to seek credit. Stronger determinants of such decisions appeared to be whether or not the household engaged in micro-enterprise and access to remittances. However, CCTs consistently favoured access to remittances in the later year of their study so it is possible that, over time, CCTs may influence access to credit through their impact on the likelihood of migration.

6.2.1 Summary of findings

In Mexico, CCTs did not affect pre-existing risk sharing arrangements, but let households insulate their consumption more effectively from shocks. They were also associated with a drop in the level of private transfers among treatment and control households. In Nicaragua, they did not crowd out remittances, and did not influence the decision to seek credit, but possibly did reduce inter-household food transfers and NGOs' food and money transfers. In Honduras, where benefit payments were very low, programme participation significantly increased the amount of food households received.

7. Locality-wide effects

Evaluations of CCTs, as with evaluations of most programmes, focused on impact on their recipients rather than on non-recipients or the wider community. A number of papers have argued for the need to broaden the scope of the evaluations to capture possible spill-over effects of programmes. Angelucci and de Giorgi (2009) noted that these effects may be large in communities where lack of formal markets and institutions have created strong interactions between small groups of households. Skoufias and di Maro (2008) pointed out that while cash transfers represent a direct income increment for PO households, there may be direct and indirect costs associated with participation which reduced the size of the increment. In addition, non-eligible households may have had an incentive to reduce their own income efforts in order to qualify for the programme. Estimates of poverty provided a useful summary of how the programme affected incomes of both eligible and non-eligible households in treatment and comparison localities. The fact that PO transfers were targeted to the poorest households may also mean that they should have led to a reduction in income inequality in PO communities relative to comparison communities. Finally, there is the possibility that the total accumulated injection of cash into poor rural communities may have created inflationary pressures.

Angelucci and de Giorgi (2009) took advantage of the fact that the PO evaluation data contained information on four groups of households - eligible and ineligible households in PO and comparison villages - to estimate these spill-over effects. Ineligible households in comparison villages provided a valid counterfactual for the ineligibles in PO villages (assuming assignment is truly random). If villagers shared risk, the PO transfers will have caused an increase in consumption, loans and transfers for ineligible households in the same villages. The authors found results consistent with this prediction: food consumption for ineligible households in PO villages increased by 10 percent per month per adult equivalent in 1999, around 50 percent of the average increase in food consumption for eligible households. There was no evidence that the increase in consumption was caused by changes in labour earnings or increased prices or volume of sales of goods caused by higher levels of demand. Instead they found that ineligible households in PO villages were able to consume more by borrowing more money (from family and friends), by receiving more transfers and, to a lesser extent, by reducing their stock of animals and grains at the start of the programme. This suggests that the programme relaxed borrowing constraints for non-poor households who could then receive extra resources from poor households if they faced a negative income shock. One other spill-over effect of the programme appeared to be health effects on ineligible households in PO villages who reported fewer days out of work due to health reasons.

Angelucci and de Giorgi (2009) examined the impact of PO on loans, transfers and remittances. They found that a higher proportion of the non-poor (ineligible households) received transfers or loans, compared to the poor, and that this was true for both treatment and control villages. This could be a scale effect: since the ineligibles were richer than the eligibles, there were more resources circulating in their households. Loans were larger than other categories of transfer, supporting the conclusion of Fafchamps and Lund (2003) that risk was shared through informal loans rather than straightforward transfers. Angelucci and de Giorgi (2009) suggested that ineligible households in treatment villages 'may receive more net resources from both the treated, whose income has increased, and other

ineligibles, who may shift resources away from the treated to the ineligible within their network, as the former group has become less needy’.

Handa et al. (2001) also explored spill-over effects from the PO programme over the period 1998-99 by comparing changes in poverty, the poverty gap and severity of poverty, income inequality and inflation pressure on basic foods in PO communities relative to comparison communities. They found that spill-over effects did appear to exist with poverty increasing less in PO villages than in control villages. They found this result for the relative poverty line (the twenty-fifth percentile of consumption) as well as for the poverty gap and squared poverty gap. Measuring inequality by the coefficient of variation and the standard deviation of the log of consumption, they found that the decrease in PO locations was even stronger when the richest 1 percent of households was excluded from the sample.

The study did not find any evidence that the programme had any inflationary effects: one reason for this may be that PO beneficiaries often spent their money outside their own communities (for instance in the municipal capital) especially when they had to travel outside their communities to receive their transfers. State-subsidised discount stores (known as Diconsa) may also have played an important role in maintaining a relatively constant supply of basic items at a fixed price.

The study did find an important spill-over effect in terms of the health care behaviour of non-beneficiary households in PO localities, and also found that children in the 10-12 age group, particularly girls, from non-beneficiary households in PO localities were far more likely to continue in school than similar age groups in control localities. While there was little evidence of any difference in the nutritional surveillance rates reported for non-eligible children in PO and control localities six months after the programme began, after a year a statistically significant difference appeared between the two groups. Spill-over effects clearly took some time to materialise.

These conclusions receive some support from the study by Skoufias and di Maro (2008) which reported the same results over a longer period. They found that PO led to a significant reduction in prevailing levels of poverty with stronger impacts on the poverty gap and the poverty gap squared over the period of the study, and with the size of the impact increasing over time.

7.1 Summary of findings

Evidence for these impacts all come from Mexico. It suggests that CCTs were associated with a lower rate of increase in poverty village-wide. It also suggests an increase in food consumption among ineligible households, partly because of greater availability of resources within the village but also because resources were shifted away from recipient households. There is also evidence of positive health care behaviour among non-recipient households, fewer sick days and greater likelihood of girls continuing in school.

8. Meta-analysis: methods and results

The approach we followed was by no means straightforward. In fact, many of the papers on child labour, for instance, measure the impact on quite a diverse group of beneficiaries, sometimes children without any distinction as to gender even though, in general, the analyses were performed for boys and girls separately. Some of the papers focused on households in rural areas whereas others were based on urban areas only. Just a couple of papers worked with a mixed sample of households, with some living in urban and others in rural areas. Meta-analysis is a powerful tool to tackle these issues.

According to Borenstein et al. (2009), 'meta-analysis provides a mathematically rigorous mechanism' to test whether a certain sort of intervention (or treatment) is effective or not. It is argued that even when most of the cases do not offer evidence to support the intervention, the meta-analysis is able to show a wider perspective. The lack of support reported by some studies might be, for instance, due to small sample sizes that compromise the precision of the estimates. The meta-analysis allows us to synthesise studies that took place in different contexts, such as urban and rural areas, different institutional settings, etc. This is exactly the case for most of the studies covered in this systematic review.

Whenever a study reported separated estimates for boys and girls but also estimates for a pooled sample of boys and girls in a broader category named 'child', we selected the estimates of the pooled sample to run the meta-analysis for child labour. When more than one point estimate was available, we took an average and worked with only one estimate per study. This approach avoided two types of problem. Firstly, if we used all point estimates of each study, we would assign more weight to studies with a greater number of point estimates. Secondly, this approach would lead to an incorrect estimation of the summary effect because 'it treats the separate outcomes as providing independent information...' (Borenstein et al. 2009: 226).

However, when a study did not report estimates for a pooled sample of boys and girls, but for boys and girls separately, we averaged all point estimates available to run the meta-analysis. We also averaged estimates for urban and rural samples when available, and for different types of work. For instance, when a paper provided estimates for 'all types of work', 'paid work', and 'domestic work', we opted for consistency to use only the point estimates of the first category. But when a study did not report estimates for 'all types of work', we took an average of the point estimates related to the categories addressed in the paper to run the meta-analysis.

Table 8.1 illustrates the (average of) treatment effects of each study and the respective standard errors (SEs). The treatment effect can be seen as a difference of (conditional) means since the aim of experimental and quasi-experimental designs is to estimate the difference in means (see, e.g., Cameron and Trivedi 2001). For an RCT, the treatment effect is estimated as a simple difference of two means: the mean of the outcome of interest for the treatment group (\bar{Y}_1), and the mean of the same outcome for the control (or comparison) group (\bar{Y}_0). When dealing with quasi-experiments, the treatment effect is given by a difference of two conditional means instead.

Table 8.1: Summary of the impact of CCTs on child labour

Studies	Year	Average treatment effect (pp)	Mean baseline	Average treatment effect mean (%)	Average SE	Average SE/mean (%)
Bando G et al.	2005	-0.093	0.164	-0.740	0.066	0.402
Behrman et al.	2005	-0.020	0.460	-0.077	0.014	0.030
Borraz and González	2009	-0.018	0.050	-0.350	0.019	0.372
Dammert	2009	-0.120	0.140	-0.857	0.064	0.401
Duryea and Morrison	2004	0.020	0.073	0.274	0.030	0.411
Schultz	2004	-0.018	0.290	-0.062	0.009	0.032
Skoufias and Parker	2001	-0.033	0.250	-0.130	0.016	0.066
Gignoux	2009	-0.064	0.120	-0.533	0.064	0.533
Attanasio et al.	2010	-0.130	0.389	-0.334	0.046	0.119
Cardoso and Souza	2003	-0.007	0.016	-0.092	0.003	0.164

Note: pp = percentage points

Table 8.1 summarises the impact of CCTs on child labour. Most of the studies suggested that CCTs tend to reduce children’s participation in labour activities. Since we are investigating the impact of CCTs on the incidence of child labour, the treatment effect is expressed in ‘percentage points’. In order make the numbers more meaningful from a public policy perspective, we also express them as the standard errors in percentage change by dividing both measures by the mean value of the outcome in the baseline (the fifth and seventh columns in Table 8.1). Thus the meta-analysis considers the normalised treatment effects.

The standard error of a study that provided more than one treatment effect, say two, was computed as follow:

$$SE = \left[\frac{1}{4} (Var(\beta_1) + Var(\beta_2) + 2Cov(\beta_1, \beta_2)) \right]^{0.5} = \left[\frac{1}{4} (Var(\beta_1) + Var(\beta_2)) \right]^{0.5}$$

We assumed throughout that the covariance between the point estimates (or treatment effects) was zero (see Borenstein et al. 2009). This was particularly problematic when the point estimates were related to the same individuals, say, when the estimates being averaged were related to different kinds of work (e.g. paid work, domestic work, etc.) performed by the same individuals. This was less problematic, though, when the point estimates being averaged came from subsamples of boys and girls, or of individuals from rural and urban areas. These subsamples were more likely to be independent of one another - unless, of course, the boys and girls were from the same household). To some extent, this was as if we were imposing no-spill-over (or peer) effects running from boys to girls, and vice-versa. In this case, the problem can arise when a study reported different point estimates for the same subsample of boys (and girls).

Borenstein et al. (2009) presented the pros and cons of this assumption. They showed that imposing zero correlation when it is in fact different from zero leads

to an underestimation of the standard error, and therefore to an overestimate of the precision (we would end up rejecting the null even when it should not be rejected). When the correlation is unknown, the authors suggested the use of a sensitivity analysis to check how the precision changes for a range of different values of the correlation. In the present case, we focused on one extreme case only because most of the studies worked with samples characterised by independent and identically distributed observations, and because the composition of the samples from which the point estimates were drawn (rural and urban areas, boys and girls) was unlikely to imply a high correlation between the point estimates.

The overall effect was computed assuming random effects because it is implausible that such a diverse sample of studies should share a common (true) effect. As Borenstein et al (2009) argued, ‘the effect size might be higher (or lower) in studies where the participants are older, or more educated, or healthier than in others, or when a more intensive variant of an intervention is used, and so on. Because studies will differ in the mixes of participants and in the implementation of interventions, among other reasons, there may be different effect sizes underlying different studies’ (Borenstein et al. 2009: 61).

Another advantage of using random effects rather than fixed effects was that the latter does not take into account the variance between studies whereas the former separates the total variance of the overall effect into two components: the within-study variance, and the between-study variance. The within-study variance is proportional to the study’s sample size - i.e. the number of observations used in the study to compute the treatment effect - and therefore to the estimate’s standard error. The between-study variance is related to the number of studies selected, and does not depend on any study’s sample size. Thus given the number of studies analysed in this systematic review, we could expect to find the ‘between variance’ explaining most of the total variance, but this was not necessarily the case. Given this characteristic of random effects, the null being tested under random effects was that the mean effect of a distribution of effects was zero. In fact, the random effects estimate of the overall effect size used the inverse of the sum of these two sources of variance to assign weights to each study (see Borenstein et al. 2009).

The heterogeneity of effect sizes (or treatment effects) was computed with the statistic I^2 , a measure proposed by Higgins et al. (2003)² that captures the proportion of total variance across the observed effects that is explained by the heterogeneity between the effect sizes. As Borenstein et al. (2009: 117-118) argued, the statistic can be represented as follows:

$$I^2 = \left(\frac{\text{Variance}_{\text{between}}}{\text{Variance}_{\text{between}} + \text{Variance}_{\text{within}}} \right) \times 100\% ,$$

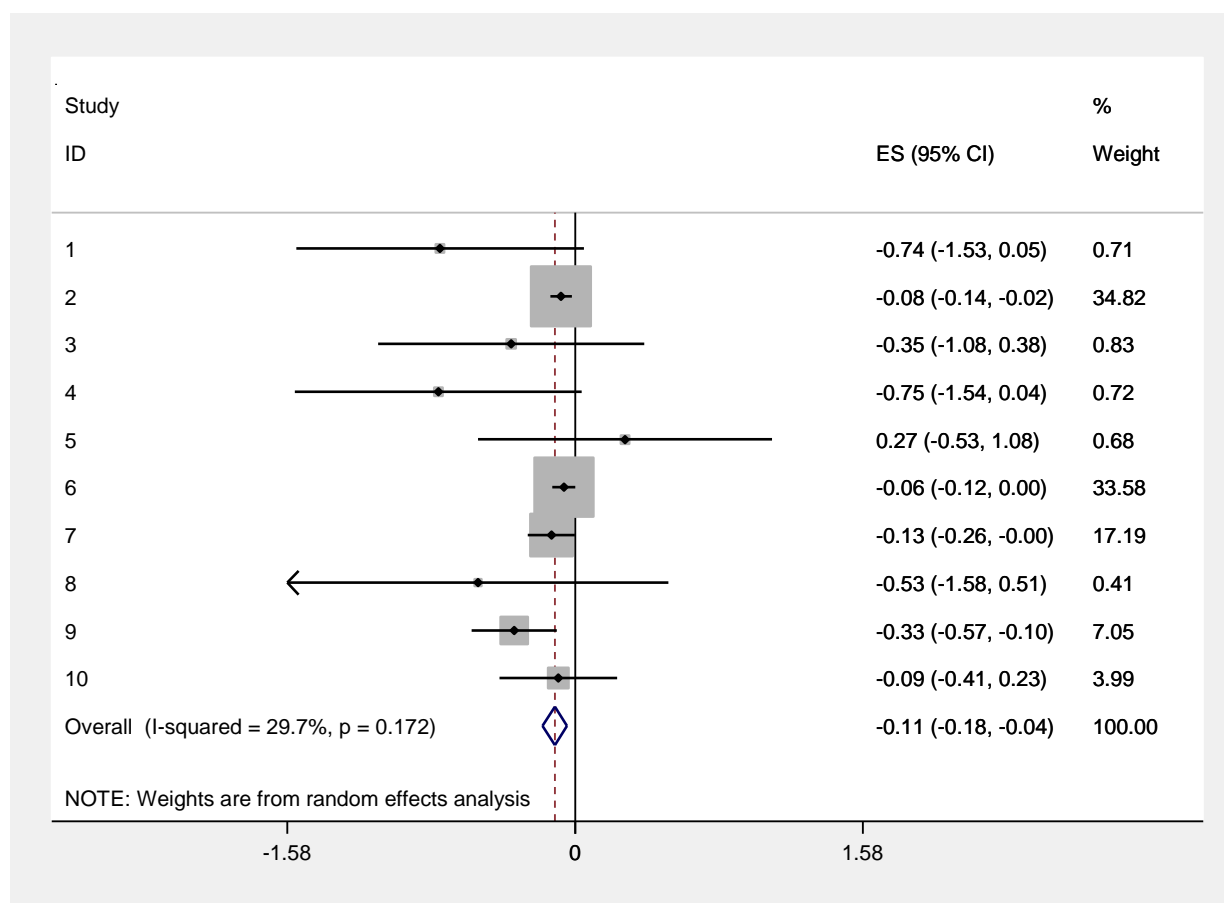
although ‘this is not the true definition of I^2 because in reality there is not a single within variance, since the within-study variances vary from study to study. The I^2 is a descriptive statistic and not an estimate of any underlying quantity.’ Therefore, the authors argued that this measure is in fact informing the ‘inconsistency across the findings of the studies.’ Thus, we alternatively report an estimate for the variance of the ‘true effect size’, τ^2 , a measure that can be seen

² Measuring inconsistency in meta-analysis. *British Medical Journal* 327: 557-560.

as an estimate for the between variance. Thus, the smaller τ^2 the narrower the interval confidence around the summary (overall) effect.

The forest plot is used to illustrate the synthetic effect of the sample of studies³. It shows the treatment effect of each study, its standard error, and the correspondent confidence intervals, and more importantly the overall effect. The solid line that crosses the horizontal axes at zero represents the null effect (no difference in means), whereas the dashed line corresponds to the overall effect, represented by a diamond.⁴

Figure 8.1: Forest plot - effect of CCT on child labour



The overall effect estimate is 11 percent, and it is statistically significant at the 0.1 percent level (p -value = 0.001). The confidence interval shows that it can range between 4 percent and 18 percent. This result suggests that, on average, CCT causes a reduction in child labour incidence. Based on this sample of studies, the average effect is of 11 percent.

Most of the total variance comes from the low precision of each study rather than the between-study variance. In fact while the I-squared, i.e. the variation in effect size attributable to heterogeneity, is 29.7 percent, the estimate of between-study

³ Given the relatively small number of studies that looked at the impact of CCTs on intensive margin of child labour (hours worked) we decided to focus the meta-analysis on studies that reported the treatment effect on the extensive margin (participation in work activities).

⁴ All figures and estimates were computed using the command *metan* in Stata.

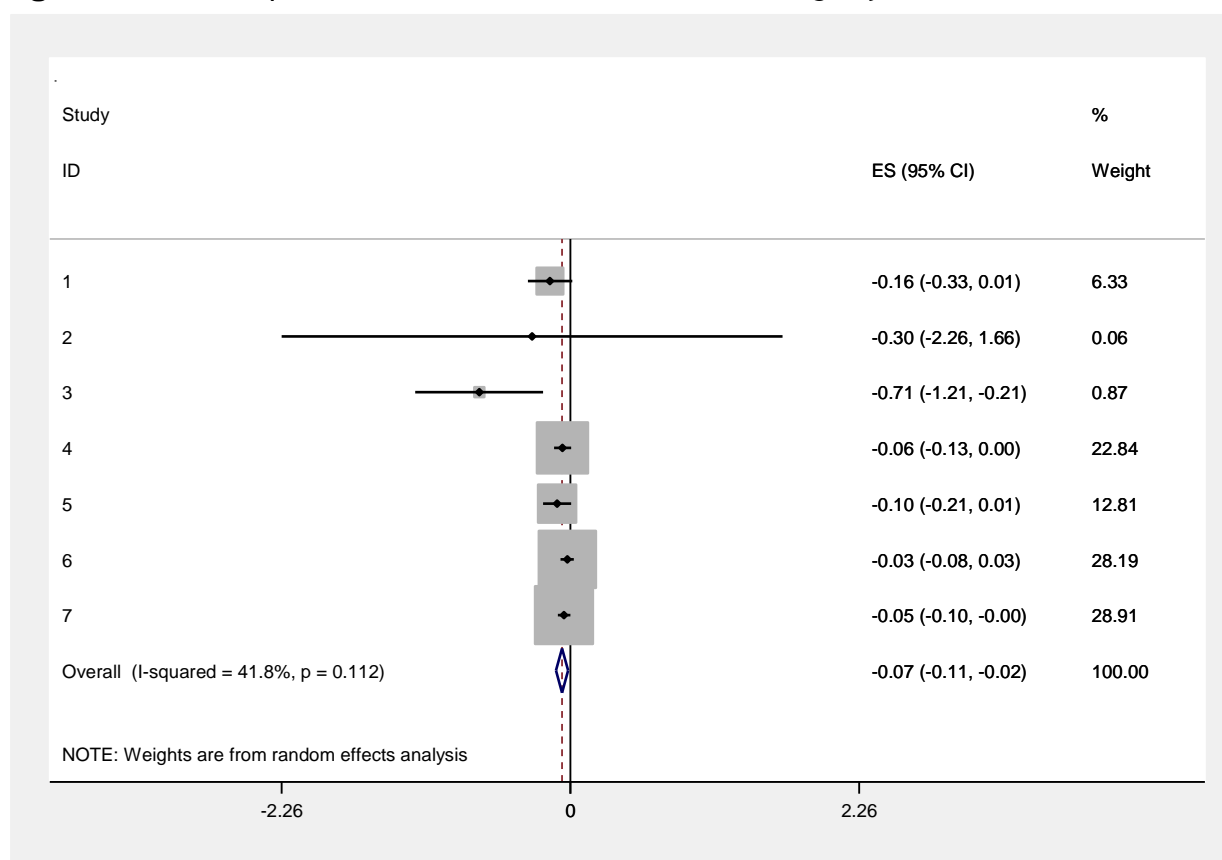
variance is only 0.0025. Table 8.2 summarises the estimates for the impact of CCTs on boys' participation in labour activities.

Table 8.2: Summary of the impact of CCTs on child labour among boys

Studies	Year	Average treatment effect (pp)	Mean baseline	Average treatment effect/mean (%)	Average SE	Average SE/mean (%)
Behrman et al.	2005	-0.0270	0.1700	-0.1070	0.015	0.088
Borraz and González	2009	-0.0150	0.0500	-0.3000	0.050	1.000
Dammert	2009	-0.1000	0.1400	-0.7143	0.036	0.255
Schultz	2004	-0.0157	0.2540	-0.0618	0.009	0.034
Skoufias and Parker	2001	-0.0395	0.3798	-0.1040	0.021	0.056
Gignoux	2009	-0.0030	0.1200	-0.0250	0.003	0.026
Cardoso and Souza	2003	-0.0080	0.1600	-0.0500	0.004	0.025

Note: pp = percentage points; The treatment effects of CCTs on child labour among boys are negative, suggesting that CCT reduces child labour among boys, and the impact ranges from 2.5 percent to 71.4 percent. The forest plot (Figure 8.2) offers a summary of these effect sizes.

Figure 8.2: Forest plot - effect of CCT on child labour among boys

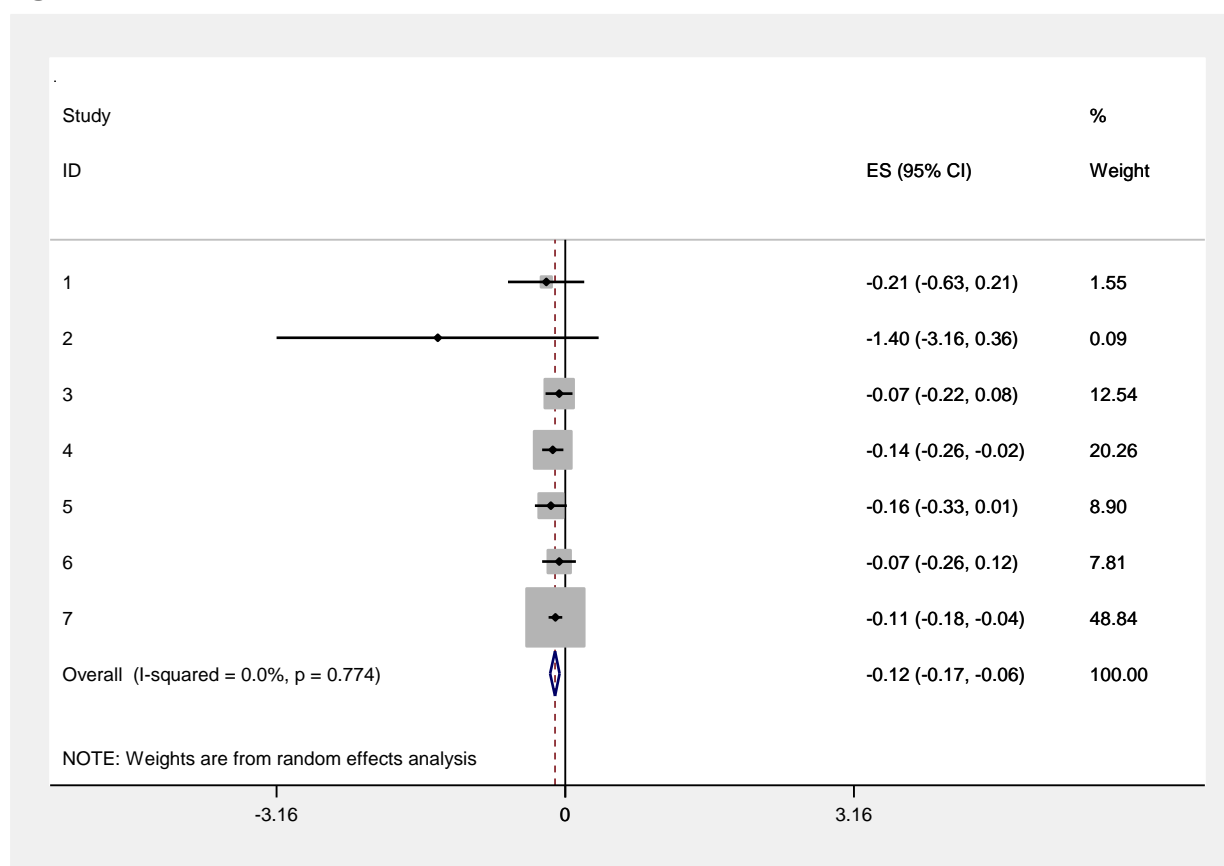


The overall effect of 7 percent is slightly lower for boys than for the pooled sample, although the confidence interval includes the mean effect of 11 percent found for the pooled sample of studies. The estimate is statistically significant at the 0.6 percent level (p-value = 0.006). As with the pooled sample, most of the total variance is explained by the within-study variance rather than the between-study variance. While the I-squared was 41.8 percent, the estimate of between-study variance is only 0.0014. Table 8.3 offers an ‘average’ of the treatment effects of CCTs on girls’ participation in labour activities. The table suggests that the impact of CCTs on girls’ labour is more modest than that reported for boys in Table 8.2. However, the effects are more homogeneous, ranging from 4.7 percent to 15.6 percent. The forest plot (Figure 8.3) summarises the information.

Table 8.3: Summary of the impact of CCTs on child labour among girls

Studies	Year	Average treatment effect (pp)	Mean baseline	ATE/mean (%)	Average SE	Average SE/mean (%)
Behrman et al.	2005	-0.013	0.061	-0.047	0.013	0.213
Borraz and González	2009	-0.07	0.05		0.045	0.900
Dammert	2009	-0.01	0.14	-0.071	0.011	0.075
Schultz	2004	-0.02	0.148	-0.135	0.009	0.059
Skoufias and Parker	2001	-0.02	0.128	-0.156	0.011	0.089
Gignoux	2009	-0.008	0.12	-0.067	0.011	0.095
Cardoso and Souza	2003	-0.01	0.088	-0.114	0.003	0.038

Note: pp = percentage points

Figure 8.3: Forest Plot - effect of CCT on child labour among girls

The overall effect of 12 percent suggests that CCTs have a higher impact on girls than on boys. The effect size is precisely estimated, with a p-value of 0.000. The confidence interval suggests that the effect of CCTs on girls ranges from 6 percent to 17 percent. Given that girls' work participation is likely to be higher once domestic activities are taken into consideration, it is not really unexpected. The estimate of tau-squared shows that there is no between-study variance, which explain why I-squared is zero. This finding illustrates the value-added effect of meta-analysis in summarising the overall effect of a sample of studies. A quick look at Tables 8.2 and 8.3 points to the higher effect of CCTs among boys compared to girls, but the meta-analysis reports an overall higher effect on girls.

Table 8.4 provides a summary of CCTs' effects on adult labour supply. The adult labour supply refers to the beneficiaries' parents and this can be seen as a complementary analysis for the impact of CCT on child labour as long as an income shock is expected to change the time allocation of many household members and not exclusively the beneficiaries'.

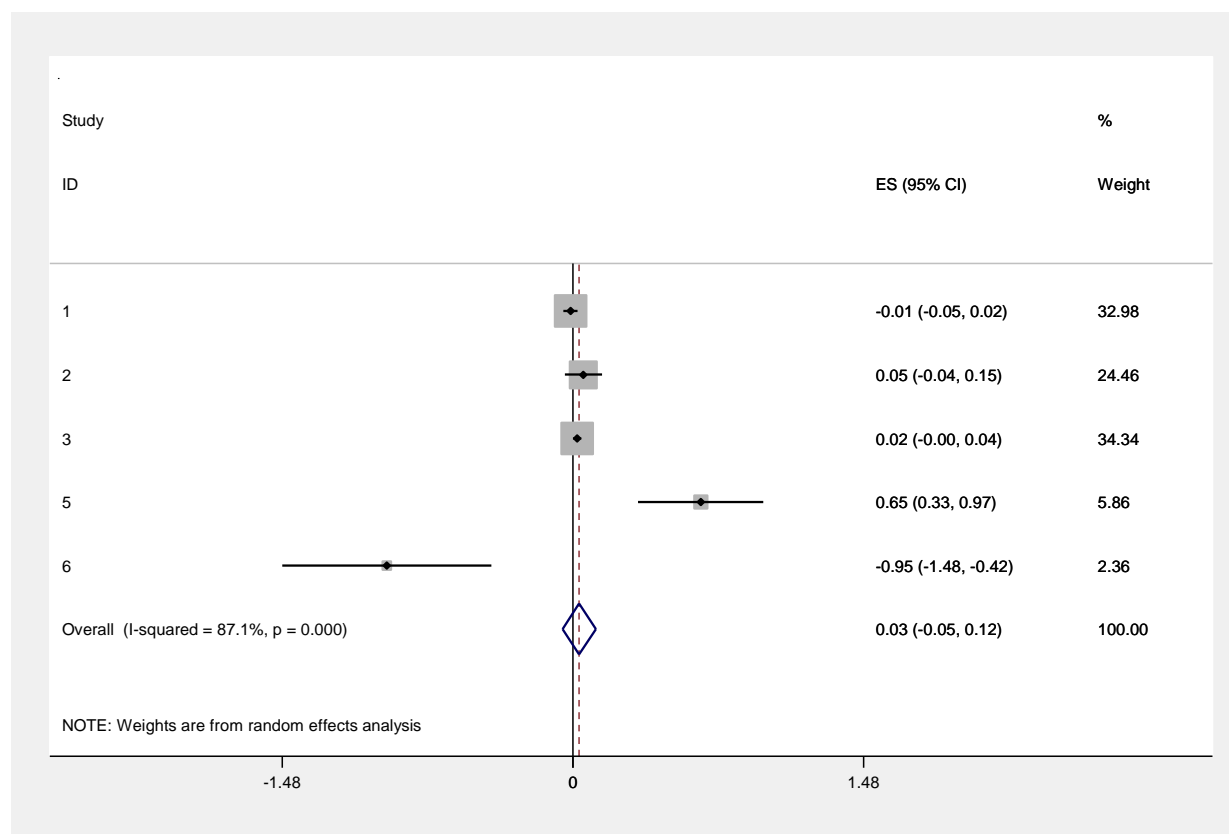
Table 8.4: Summary of the impact of CCTs on adults' labour supply

Studies	Year	Average treatment effect (pp)	Mean baseline	Average treatment effect/mean (%)	Average SE	Average SE/mean (%)
Skoufias and di Maro	2008	-0.0065	0.53	-0.0123	0.0098	0.0185
Skoufias et al.	2008	0.02	0.375	0.0533	0.018	0.0480
Teixeira	2010	0.019	0.915	0.0208	0.01	0.0109
Oliveira	2005	0.0076	Na	Na	0.017	Na
Tavares	2010	0.208	0.32	0.6500	0.052	0.1625
Hasan	2010	-0.096	0.1014	-0.9467	0.0275	0.2712

Note: pp = percentage points

Note: unfortunately, Oliveira (2005) did not report the mean value of the outcome variable in her study.

Figure 8.4: Forest plot - effect of CCT on adult labour supply



Most of the studies pointed to some perverse (moral hazard) effect of CCTs on adults' behaviour, but overall the evidence is mixed. The forest plot (Figure 8.4) illustrates this information as well as providing an estimate for the overall effect size.

The overall effect is 3 percent but it is not statistically significant at the 5 percent level (p-value = 0.46). The confidence interval suggests that CCTs could reduce adult labour supply by 5 percent or increase it by 12 percent. In fact, the absence of an effect of CCTs on the labour supply of adults is predicted by the CCT

rationale since, for most of the beneficiaries, the transfer had an income effect that was not big enough to allow poor households to trade off hours worked (or work participation) for extra leisure. Leisure was a luxury good that most of the beneficiaries' households could not afford.

Although most of the studies displayed treatment effects around the solid line, we strongly reject the null that there is no heterogeneity across the studies (chi-squared = 30.93 with four degrees of freedom). The very high I-squared measure (approaching 90 percent) means that most of the variance is explained by the between-study variance component. When this is the case, some authors recommend the application of other techniques such as subgroup analysis to understand what underlies the high variance. While we did not apply any another technique due to limited sample of studies, we speculate that this is caused by the two outliers that appear in the forest plot.

Table 8.5 shows the 'average' treatment effect of CCTs on household consumption. This variable corresponds to total consumption, when reported by the study, or to food consumption when a study, such as Hoddinott and Skoufias (2004), looked specifically at the impact of CCTs on food consumption. The great majority of the studies, though, reported estimates for total consumption. The normalisation of treatment effect estimates is crucial in this case because some studies reported the impact on consumption expressed in different currencies or even in kilocalories, thus transforming the point estimates into percentage change.

Table 8.5: Summary of the impact of CCTs on consumption

Studies	Year	Average treatment effect (pp)	Mean baseline	Average treatment effect/mean (%)	Average SE	Average SE/mean (%)
Gignoux	2009	4.810	1050	0.005	6.329	0.006
Skoufias et al.*	2008	0.160	293	0.16	0.037	0.037
Maluccio	2010	3584.33	20188	0.178	1398	0.069
Resende and Oliveira	2008	307.56	Na	Na	101.171	Na
Todd et al.*	2010	0.041	0.688	0.041	0.0186	0.0186
Attanasio and Mesnard	2005	52.576	413.559	0.127	13.551	0.03
Hoddinott and Skoufias*	2004	0.03725	1963	0.03725	0.008	0.008

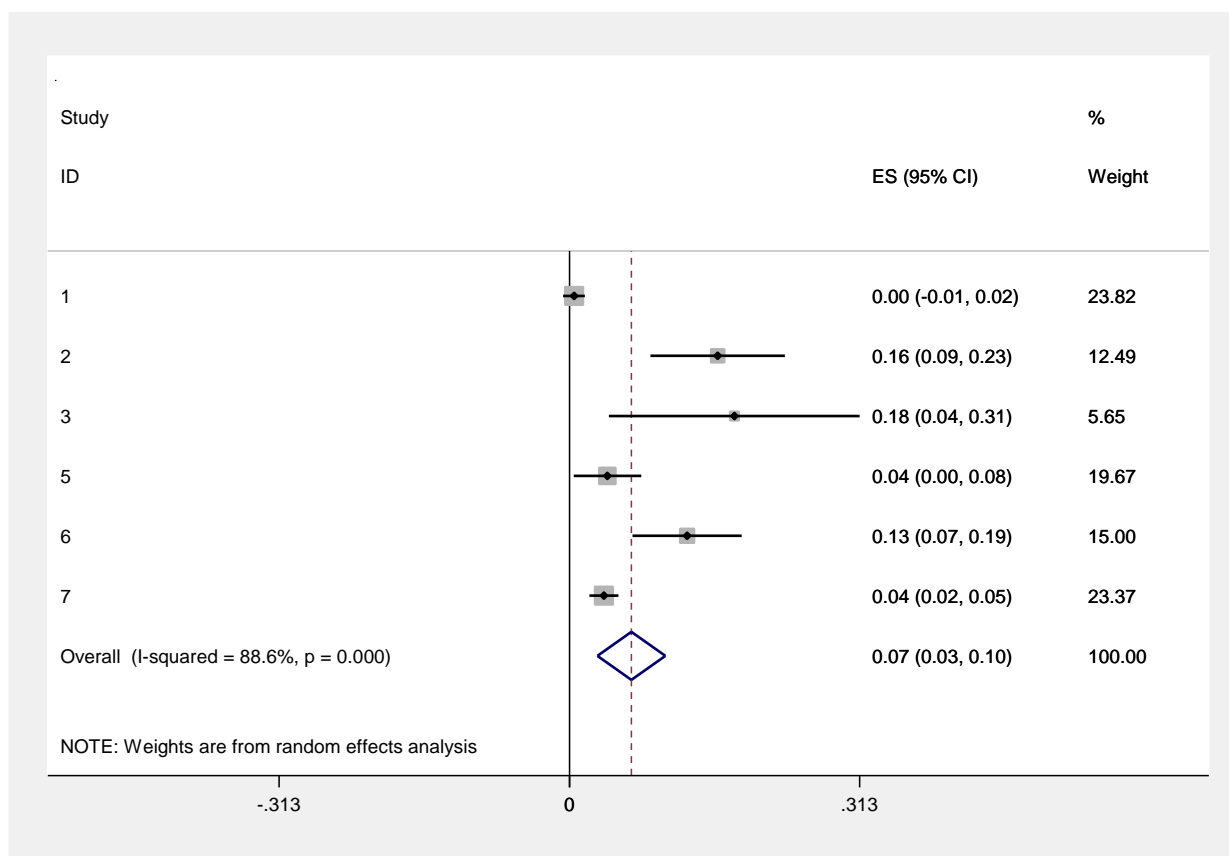
Note: pp = percentage points.

Note: unfortunately, Oliveira (2005) did not report the mean value of the outcome variable in her study.

*These studies specify the outcome (total consumption) in natural logs so that the point estimates already represent percentage changes.

The impact of CCTs is heterogeneous but unambiguously positive. According to Table 8.5, CCTs seem to increase consumption by between less than 1 percent and 17.8 percent. However, we can expect some high I-squared, i.e. a high real variance. Figure 8.5 shows the overall estimate.

Figure 8.5: Forest plot - effect of CCT on consumption



The overall effect of CCTs on consumption is 7 percent and is highly significant (p-value = 0.000). The confidence interval shows that the impact seems to range from 3 percent to 10 percent. However, just as for adult labour supply analysis, we strongly reject the null hypothesis of no heterogeneity across studies (chi-squared = 43.87 with five degrees of freedom). Despite the low value of tau-squared (0.0014), the I-squared approaches 90 percent, suggesting that there is a high between-study variance. This high variance may be explained by the second and third studies which reported very high effect sizes.

The bottom line is that while conditionalities (responsible for the substitution effect) and the income effect of CCTs explain the reduction of child labour, the income effect per se seems to be strong enough to increase consumption but not enough to lead to a reduction in adult labour force participation.

9. Conclusions

A number of general points can be made on the basis of the analysis in this paper. First of all, it is evident that despite some of the recent claims made for the economic impacts of CCTs, evidence that meets the strict methodological criteria laid out in this paper is still extremely scarce, reducing the credibility of these claims as well as the extent to which they can be generalised. Not only do almost all the studies that could be selected for this review come from Latin America, but a disproportionate number focused on the PO programme in Mexico. The findings from Mexico were generally positive and consistent but either the same questions have not been asked elsewhere, or where they have (as in relation to RPS in Nicaragua), they have not supported the same conclusions.

Secondly, we took the decision to confine our search for qualitative insights into the causal and contextual factors that might explain the presence or absence of impacts to information contained in the selected papers. This served to highlight the very limited attention paid to pathways of causality in these papers. Authors generally speculated on the meaning of their findings or sought to infer it from theoretical literature but concrete evidence was largely missing.

One reason for this might be that not only are the papers included in this review almost exclusively econometric in approach, but they are based on a limited group of datasets that constitute the best available for the quantitative exploration of programme results. These datasets were either household surveys (as in the case of most Latin American countries) or dedicated surveys conducted specifically to evaluate the programme in question, as was the case with PO, the focus of the majority of the studies (IFPRI 2000).

In the case of a dedicated dataset, econometric principles have to be used far in advance of programme implementation to determine what data should be collected. The designers of these surveys therefore cannot predict where interesting effects will be found, and thus the data collected seldom go deep enough to fully explain them. Equally, these surveys do not offer researchers the opportunity to include the kind of qualitative questions that might have further illuminated the results found. This applies even more strongly in the case of the household surveys that researchers used to assess the impacts of other programmes. A further problem is that where qualitative data are available on the programmes covered here, they are collected separately and therefore cannot be guaranteed to cover the same groups as the quantitative information. This could be remedied if monitoring and evaluation experts built qualitative elements into evaluation plans from the start, to give greater depth to the data and to increase the chance that interesting effects could be explained. More generally, purposively designed surveys which incorporate quantitative and qualitative information on the theory of change informing particular interventions would add greatly to the analytical and practical value of these studies.

Finally, keeping in mind the small number of countries represented in the selected papers, our review suggests that the evidence of the economic impact of CCTs is strong and consistent for certain kinds of impacts, weaker and less consistent for others. The findings were strong and consistent for increased overall household consumption, particularly in relation to the amount and diversity of food consumption. There is also evidence to suggest that the fact that the transfers were generally targeted to women had an effect on household expenditure patterns, with a bias towards food and educational expenditure and specific kinds

of productive assets. However, the strongest evidence that transfers were invested in productive assets and activities comes from Mexico.

There is strong and consistent evidence that CCTs reduced child labour as well as increasing children's school attendance but it appears that the educational effect was generally stronger than the labour market effect. The impact on adult labour was more mixed, with increases in market work by adult men and women in some contexts and increases in leisure or unpaid domestic work in others.

There is persuasive evidence that CCTs protected household consumption and educational patterns during times of crisis and that they did not in general erode pre-existing risk sharing arrangements within programme communities. And, once again with evidence coming from Mexico, studies testified to spill-over effects within PO communities in terms of poverty reduction, increased loans and transfers and behavioural impacts but provided little evidence of inflationary pressure.

The studies pointed to the importance of household income, education levels, ethnicity and location in modifying the impact of CCTs. Children's characteristics, particularly age and gender, also mattered in explaining the size and significance of impacts on child labour and schooling. CCTs often appeared to have more impact on the type of work that adults do, rather than whether they work at all, particularly in the case of women's time allocation. The size of the transfer also contributed to variations in impacts.

The studies on migration presented mixed conclusions, indicating that longer-term evaluation is necessary. Although CCTs may act to delay migration, particularly that of young men (if only directly through their conditionality), the weight of the evidence does not indicate either reduction or increase in migration overall. Reasons to approach these results with caution include that they all come from one country (Mexico), and are from a period which saw a large overall increase in the Mexico-USA migration rate. Given that migration is also dependent on many external factors whose influence a CCT programme is unlikely to limit, evidence from more countries is needed in order to gauge CCTs' potential impact in this area. Furthermore, none of the studies in question distinguished clearly between labour and non-labour migration, which could explain some of the contradiction.

In conclusion, CCTs appear to be an effective measure for achieving what they were designed to achieve: promoting children's education and reducing child labour among poor and marginalised groups. They do have a variety of other economic impacts but the significance of these varied considerably because they appear to depend on variations in programme design and experience (the size of transfers, how long they had been provided), on individual characteristics (gender, ethnicity, age and so on), occupational status (self employment or wage employment) and wider context (rural/urban, macroeconomic environment, infrastructure development). More systematic research into which of these moderating factors matter most and under what circumstances would help to flesh out the theory of change adopted in this paper.

10. References

10.1 Papers included in the review*

*All studies included in the review, along with summary information, are contained in Appendix C, which can be downloaded from this report's homepage at <http://eppi.ioe.ac.uk/cms/Default.aspx?tabid=3364>

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Appendices

Appendix 1.1: Authorship of this report

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Conflicts of interest

There were no conflicts of interest in this report.

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Appendix 2.1: Tables

Table A2.1: Initial search: criteria and engines

Database/ search engine	Keyword	Search field	No. of records	Time period	Language	No. of records downloaded
EBSCO	cash AND transfer*	all content	242	any	any	100
EBSCO	cash AND transfer* AND conditional	all	66	any	any	62
Google Scholar	"conditional cash transfer**"	Google Scholar	2,890	any	any	75
IDEAS	cash transfer	all	696	any	any	
NBER	"cash transfer**"	'full-text search of publications', including working papers and books	112	any	any	15
NBER	"conditional cash transfer**"	'full-text search of publications', including working papers and books	21	any	any	2
NDLTD (Networked Digital Library of Theses and Dissertations)	cash transfer*	Scirus ETD - full text of document, all fields	18,260	1900-2012	any	too general: went to more specific search
NDLTD	cash transfer* AND conditional	Scirus ETD - full text of document fields: social sciences, psychology, environmental sciences, economics, sociology	623	any	any	10 (after 350 records, only downloaded 10; thus changed to more specific search)
NDLTD	"cash transfer**"	Scirus ETD - full text of document, all fields	276	any	any	33
ProQuest	conditional cash transfer*	database: multiple databases	79	any	any	49
ProQuest	cash transfer*	database: scholarly journals	190	any	any	39

What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence

Database/ search engine	Keyword	Search field	No. of records	Time period	Language	No. of records downloaded
ProQuest	cash transfer*	database: working papers	11	any	any	3
Royal Society Journals	("cash" AND "transfer*")	all journals	37	1970-2010	any	1
Royal Society Journals	cash transfer*	all journals	50	1970-2010	any	1
Royal Society Journals	conditional cash transfer*	all journals	2	1970-2010	any	0
Sage Journals	"cash transfer*" (N.B. tried searching cash AND transfer* and retrieved 7,600 records)	all journals	159	any	any	39
ScienceDirect	conditional cash AND transfer*	social sciences	559	any	any	26
ScienceDirect	"conditional cash transfer*"	social sciences	57	any	any	
Social Sciences citation index (via Web of Knowledge)	cash transfer*	topic	596	any	any	100
Social Sciences citation index (via Web of Knowledge)	cash transfer* AND conditional	topic	103	any	any	131
SpringerLink	"cash transfer*"	all content	123	any	any	6
SpringerLink	"conditional cash transfer*"	all content	21	any	any	3
World Bank publications database	"conditional cash transfer*"	documents	995	any	any	50
EconLit	'conditional cash transfer'	social sciences	280	Earliest - July 2010	any	116

Database/ search engine	Keyword	Search field	No. of records	Time period	Language	No. of records downloaded
EconPapers	'conditional cash transfer'	all content	73	any	any	3
IngentaConnect	'conditional cash transfer'	all content	59	1980 - July 2010	any	45
JSTOR	'conditional cash transfer'	all content	3,586	1980 - July 2010	any	73
Taylor & Francis	"conditional cash transfer"	all content	50	any	any	28
Wiley	"conditional cash transfer"	all content	3,125	1980 - July 2010	any	47

Table A2.2: Stages of search and exclusion of papers

Stage	Goal	RCT	Quasi-experimental	Micro-simulation (general equilibrium models)	Regression models and surveys	Other (including inaccessible)	Number of papers
1	Search multiple databases for papers on CCTs						1,074
2	Delete duplicate papers						624
3	By abstract: categorise by methodology	94	56	18	153	79	403
4	By abstract: categorise by economic impact	54	39	17	128	33	271
5	Full reading: include according to methodology, economic impact and accessibility	28	17	0	1	0	46
6	Further searches: journals and library shelves	No additional papers	No additional papers	No additional papers	No additional papers	No additional papers	46

Table A2.3: Categories of findings and numbers of papers

Consumption (changes in proportions, type of consumption)	Brazil - 2 Colombia - 2 Honduras - 1 Mexico - 6 Nicaragua - 3
Child labour	Brazil - 2 Chile - 1 Colombia - 1 Mexico - 6 Nicaragua - 3 Pakistan - 1 Uruguay - 1
Adult labour supply	Brazil - 4 Chile - 1 Mexico - 2 Nicaragua - 1 Pakistan - 1 Uruguay - 1
Migration (domestic and US)	Mexico - 4
Risk/crisis/insurance	Honduras - 1

	Mexico - 1 Nicaragua - 4
Savings and investment	Mexico - 5 Nicaragua - 2
Spill-overs	Mexico - 3 Nicaragua - 1

Table A2.4: Programme studies included in systematic review

Country	Programme Title	Location of programme	Active since	# of studies ⁵	Target age group
Brazil	Bolsa Familia/Bolsa Escola	Urban and rural	1995	6	6-15
Brazil	Programa de Erradicação do Trabalho Infantil	Urban and rural	1996	1	7-14
Chile	Chile Solidario	Urban and rural	2002	1	All
Colombia	Familias en Accion	Urban	2000	3	School-age children
Honduras	Programa de Asignación Familiar	Rural	2000	2	Pregnant women and children <12
Mexico	Progresas/Oportunidades	Progresas - rural Oportunidades - rural and urban	Progresas - 1997 Oportunidades - 2002	23	5-17
Nicaragua	Atencion a crisis; Red de Proteccion Social	Rural	2000	8	7-13
Pakistan	Learning and Education Achievement		2000	1	School-age children
Uruguay	Plan de Equidad	Rural and urban - study of urban impacts only	2007	1	6-14

Table A2.5: Programmes not included in the systematic review

Country	Programme title
Argentina	Plan Familias (Jefes y Jefas)
Bangladesh	Female Secondary School Assistance Programme
Bolivia	Juancito Pinto
Bolivia	Bono Juana Azurduy
Cambodia	Japan Fund for Poverty Reduction
Cambodia	Education Sector Support Project
Colombia	Subsidio Condicionado a la Asistencia Escolar-Bogotá
Costa Rica	Avancemos
Dominican Republic	Programa Solidaridad
Ecuador	Bono de Desarrollo Humano
El Salvador	Programa de Red Solidaria
Guatemala	Mi Familia Progresas
Indonesia	Jaring Pengamanan Sosial
Jamaica	Programme of Advancement through Health and Education

⁵ Some studies deal with two programmes.

What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence

	(PATH)
Kenya	Cash Transfer for Orphans and Vulnerable Children
Pakistan	Punjab Education Sector Reform Programme
Panama	Red de Oportunidades
Paraguay	Programa Tekoporã
Peru	Juntos
Trinidad and Tobago	Conditional Cash Transfer Programme
Turkey	Social Risk Mitigation Project
Yemen	Basic Education Development Project

Table A2.6: Impacts by programme

Country	Programme title	Consumption	Labour allocation	Migration	Insurance	Spill-over effects
Brazil	Bolsa Familia/Bolsa Escola		<ul style="list-style-type: none"> • Increase in children's schooling ; • No decrease in child labour for boys, small decrease for girls; • Reduction in mothers' working hours 			
Brazil	Programa de Erradicação do Trabalho Infantil		<ul style="list-style-type: none"> • Increases time in school for both sexes; • Decreases probability of child work 			
Chile	Chile Solidario		<ul style="list-style-type: none"> • No significant impacts on adult work 			
Colombia	Familias en Accion		<ul style="list-style-type: none"> • Increases time in school for both sexes • Reduces domestic 			

Country	Programme title	Consumption	Labour allocation	Migration	Insurance	Spill-over effects
			work hours of children			
Honduras	Programa de Asignación Familiar				<ul style="list-style-type: none"> • More than offsets drop in expenditure during shock • Mitigates likelihood children will be taken out of school for work • No impact on productive investments • Increase in food transfers 	
Mexico	Progres a/Oportunidades	<ul style="list-style-type: none"> • Food and non-food consumption increase • Food diversity increases • Savings and investment increase, including land cultivation and livestock 	<ul style="list-style-type: none"> • Decrease in child labour for both sexes • Increase in schooling (greater for boys) • Small shift among adult men into waged work 	<ul style="list-style-type: none"> • Inconclusive results on international migration to USA • Inconclusive results on rural-urban migration 	<ul style="list-style-type: none"> • Mitigates/erases negative effects of shocks on children's schooling • Complex effects on child labour during shocks • Complex effects on intra-household transfers 	<ul style="list-style-type: none"> • Food consumption increase among non-eligible households • Positive effects on health and girls' schooling for non-eligible households • Loans and transfers increase for non-eligibles in treatment and control areas • Less increase in poverty in treatment areas • No spill-overs on inflation
Nicaragua	Atención a Crisis		<ul style="list-style-type: none"> • Decrease in child 			

What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence

Country	Programme title	Consumption	Labour allocation	Migration	Insurance	Spill-over effects
			labour (greater for boys) <ul style="list-style-type: none"> Increases girls' work specialisation 			
Nicaragua	Red de Proteccion Social		<ul style="list-style-type: none"> Decrease in child labour and increase in school hours (both greater for boys) 		<ul style="list-style-type: none"> Sustains expenditure during shocks; small increase persists during recovery Small increase in schooling expenditure during shocks No increase in productive assets CCTs crowd out inter-household and NGO food/money transfers No influence on decision to seek credit 	
Pakistan	Learning and Education Achievement		<ul style="list-style-type: none"> Increases mothers' domestic work hours, decreases paid work 			
Uruguay	Plan de Equidad		<ul style="list-style-type: none"> No significant effect 			

Country	Programme title	Consumption	Labour allocation	Migration	Insurance	Spill-over effects
			on child labour • Reduction in male and female adult working hours			

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