How effective are CCTs in low income settings?

A review exploring factors impacting on programme outcomes in Honduras and Nicaragua

STEPHANIE DIEPEVEEN, CHRISTIAN VAN STOLK

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Through ESRC/DFID Award RES-167-25-0563, a two-year grant from the UK Economic Social Research Council (ESRC) and Department for International Development (DFID), RAND Europe and the Institute for Fiscal Studies (IFS) are assessing the effectiveness of CCTs in use in low- and middle-income countries. By analysing the impact of external conditions at the institutional and household level on CCT programme effectiveness, we aim to start addressing some of the gaps in understanding of the effectiveness of CCTs, and to contribute to more effective and efficient government programmes to alleviate short- and long-term poverty.

CCTs have been evaluated extensively and show by and large an increase of consumption amongst beneficiaries resulting in sometimes substantial reductions in poverty (Fiszbein and Schady, 2009: xii). But important questions remain outstanding. CCTs have very heterogeneous impacts in different contexts. We need to understand how this heterogeneity is explained by environmental factors, the availability of infrastructure for the provision of health and education services and by individual background variables. We need to gain a good understanding of the mechanisms through which CCT obtain the impacts they do. Without this knowledge, we cannot determine the likelihood that CCTs will effectively alleviate poverty in the current economic environment.

This paper contributes to this wider body of work being conducted with this grant by examining existing evidence on CCTs in low-income settings. Since the initial flagship CCTs in Brazil and Mexico, programmes are being implemented in a wide range of political and socioeconomic contexts, including areas with high levels of extreme poverty, poor education and health indicators, and limited public administration infrastructure and capacity. The aim of the paper is to comment on the evidence on what affects programme outcomes in settings where infrastructure and capacity for delivery might be low, and that have high levels of poverty at baseline. This will help to guide questioning and hypotheses for further research into effect variability in such settings.

The intended audience of this working paper are researchers and policy makers interested in the use of CCTs for poverty alleviation in low income settings. It is intended to provide an initial synthesis of evidence from the first conditional cash transfer programmes to be implemented and evaluated in such settings.

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For more information about RAND Europe or this document, please contact:

Stephanie Diepeveen
RAND Europe
Westbrook Centre
Milton Road
Cambridge CB4 1YG
United Kingdom
Tel. +44 (1223) 353 329
diepevee@rand.org
Executive summary (Abstract)

Conditional cash transfer (CCT) programmes have been evaluated extensively and show by and large an increase of consumption amongst beneficiaries resulting in sometimes substantial reductions in poverty. Nonetheless important questions remain outstanding. CCTs have very heterogeneous impacts in different contexts. This paper presents the findings of a systematic review of papers looking at evidence of effect of CCTs in Nicaragua and Honduras. In particular, this review wanted to look at wider contextual factors and their relationship with programme outcomes. These factors were: household characteristics and intra-household relations; programme design and delivery; supply side conditions; wider political, social and economic factors. The review included 13 papers and found that:

- Household, programme and wider contextual factors shape the size and nature of programme effects;
- Poorer households and communities tend to experience greater relative effects on school enrolment, and a reduction of child working hours;
- Programme effects are greater when economic conditions are favourable;
- However, CCTs also help to lessen the effect of economic shock on household consumption;
- How external factors (e.g. economic shock) affect nutrition and health outcomes remains unclear;
- Real and perceived dimensions of programme implementation affect participants’ incentives to comply with conditionalities.

The paper further suggests that a key gap in existing knowledge is better knowledge of the causal pathways through which different household and economic factors affect the outcomes experienced.
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Since the late 1990s, Conditional Cash Transfer (CCT) programmes have grown exponentially in size and number, beginning in Latin America and the Caribbean, followed by southeast and south Asia and Africa. The origins of CCTs as a means for efficient and effective poverty alleviation are rooted in past responses to national-scale economic recession and financial constraints, for example, PROGRESA one of the first flagship CCTs, began after the 1995 debt crisis when the Mexican government faced the task of reducing the fiscal deficit while trying to provide a safety net to the poorest sectors of society. The rationale underpinning CCTs suggests that targeting cash to the poorest in society, while also linking this to incentives to invest in human capital development, will direct state resources to those most in need while, at the same time, putting in place incentives to reduce or eliminate the intergenerational transmission of poverty.

Short-term impacts of CCTs on use of health and education services are well documented through these individual programme evaluations (Bourguignon, Ferreira and Leite, 2002; de Janvry and Sadoulet, 2006; Fiszbein and Schady, 2009). Also, there are some tentative suggestions that CCTS could help strengthen individual health, nutrition and education outcomes. (Lagarde et al, 2009). However, important questions remain to be addressed. In particular, there is limited understanding of potential heterogeneity of CCT effects between households and communities, the conditions and complementary programmes necessary for programmes to be effective, as well as how and why households might participate in programmes in different ways.

The scale of the current financial crisis and its likelihood to affect developing countries across geographic regions increases the importance of understanding the features of the wider context, programme design, and implementation that influence this heterogeneity and the effectiveness of CCTs in contributing to poverty alleviation (see for example, Fiszbein and Schady 2009:200-2). This review is part of a wider research project at RAND Europe and the Institute for Fiscal Studies to examine variation in CCT outcomes, and the reasons for this. This paper contributes to this wider body of work by

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1 CCTs can be an effective means of increasing the use of health services and improving health outcomes and nutritional status of children. However, evidence on impacts on final outcomes in health and nutrition – in contrast to changes in the use of services - is mixed. Studies have found that impacts are inconsistent between age groups. For a critical analysis of the impact of CCTs on such outcomes, see Manley, Gitter et al., 2011. http://www.aae.wisc.edu/mwiedc/papers/2011/Gitter_Serh.pdf
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synthesising existing evidence on this issue in low-income settings. Following from the initial flagship CCTs in Brazil and Mexico, programmes are being implemented in a wide range of political and socioeconomic contexts, including areas with high levels of extreme poverty, poor education and health indicators, and limited public administration infrastructure and capacity. The aim of the paper is to comment on the evidence on how outcomes might vary in settings where infrastructure and capacity for delivery might be low, and with high levels of poverty at baseline. This will help to guide questioning and hypotheses for further research into effect variability in such settings.

1.1 The scope of this review

To contribute to improved understanding of what effects CCTs’ delivery and outcomes in low-income settings, this review was guided by the following overarching question:

What is the state of the evidence on the significance of external variables in explaining the distribution and variation of outcomes of CCT programmes in low-income countries?

The review team interpreted this question in the following ways:

First, the team chose to limit the scope of the question temporally and geographically to ensure its feasibility. The decision was made to review evidence from the first large-scale CCTs implemented in low income countries: Honduras and Nicaragua. Programmes in both countries were implemented with extensive externally funded randomised evaluations. Their relatively early inception date and completed evaluations make them a potentially valuable source of evidence on CCTs in low income settings.²

Second, this review team was interested in potential factors that might help explain variation in participants’ experience across a wide breadth of programme outcomes. This included both intended and unintended outcomes on household behaviours and well-being indicators.

Third, the team determined that qualitative and quantitative empirical studies could be relevant to addressing the review question, as the different approaches could help inform understanding of both the reasons for variation in outcome, and the significance of explanatory variables.

1.2 Structure of this report

The report’s structure proceeds as follows: in the next chapter, we clarify the theoretical basis for the questions posed in this review. Chapter 3 then introduces the programmes which are the focus for the review, describing briefly the design and evaluation of CCTs in Nicaragua and Honduras. Chapter 4 summarises our methodological approach for

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² The first CCT was introduced in Honduras. Welfare benefits, conditioned on certain education, health and savings behaviour, were introduced as part of Honduras’ social protection programmes in 1990 and 1991 (e.g. Bono Escolar, Bono Materno Infantil, Di Mujer). In 1998, the conditional cash transfer programmes in Honduras were restructured with funding from the Inter-American Development Bank (IADB), under the new name of PRAF/IDB Phase II.
searching for and analysing evidence on these programmes, and reports on the search results. In Chapter 5, we present and analyse review findings. We highlight where the evidence base confirms, adds nuance, contradicts or fails to address our original theoretical assumptions (as outlined in Chapter 2). Chapter 6 concludes with main findings, key limitations of the evidence base, and suggested directions for policy and research.
2.1 **Conditional cash transfers could contribute to immediate and longer-term household wellbeing**

Conditional cash transfers (CCTs) tend to be directed at improving human capital outcomes in health, nutrition and education. CCTs provide households with cash incentives, conditioned on particular behaviours, usually with regards to use of health and education services. Through provision of immediate cash as well as incentives to invest in certain behaviours, they are intended to address immediate poverty while also building households’ capabilities to break the intergenerational transmission of poverty (Ferreira and Robalino, 2010).

Both the cash transfer itself and the conditionality could contribute to addressing immediate household welfare and longer term improved human capital outcomes. In the immediate term, CCTs provide cash resources for consumption, savings and investment. Through this, CCTs can smooth out consumption patterns. Also, the conditionality is intended to constrain health, nutrition and education behaviours in the immediate term, and also how households use the cash resource (e.g. for school supplies, health check-ups). At the same time, improved health, nutrition and education can also be seen as strengthening individuals’ capabilities, and hopefully their longer term opportunities. Finally, also potentially improving longer term outcomes, conditioned behaviours could become regularized within households.

Attaching a condition to the cash transfer is justified under two main assumptions: first, poor households are under-investing in human capital, relative either to what is optimal for the private household, or to what would be socially optimal (e.g. if there are positive externalities from private investments, such as returns to skilled labour, reduced crime, etc.), and second, wealthier households will be more willing to support income redistribution towards poorer households if it is accompanied by perceived positive behavioural changes (Fiszbein, Schady et al, 2009).

2.2 **However, potential and actual outcomes can be affected by the context within which the programmes are implemented**

The above rationale for CCTs does not comment on how the context within which programmes are implemented and within which individuals make decisions might affect potential outcomes. There is a wide range of factors at different levels that could affect the delivery, uptake and outcomes of CCTs: for example, household decision making patterns
could shape how the cash transfer is spent, access to and quality of health and education services could affect both willingness and outcomes of fulfilment of conditionalities, as well as a lack of transparency could limit trust in delivery and reduce individuals’ willingness to participate.

Factors ranging from those at the intra-household level (e.g. decision making power of men versus women), to the household (e.g. area of economic activity, location) to the social protection infrastructure, and the wider political, social and economic context could shape and constrain how the programme manifests. Taking into account hypotheses and suggestions regarding relevant external determinants of CCTs (Fiszbein and Schady 2009; Angelucci and Attanasio 2009; Attanasio and Lechene 2010), we suggest four broad areas with potentially important effects on CCT programmes and their intended outcomes: household characteristics, programme-level factors, supply side conditions, and the wider political, social and economic context.

2.2.1 Household characteristics and intra-household relations
Looking at the household as a unitary entity, different characteristics of the household can affect what decisions might be most rational to that particular household. For example, the level of poverty, location or family size might affect the marginal benefit of changing behaviour or influence perspectives on the most appropriate way to spend cash resources. For example, higher income households might be less inclined to change behaviour despite the cash incentive, as they might depend less on the added resource for basic consumption (see e.g. Filmer and Schady, 2008 on the heterogeneity of the treatment effect).

Intra-household differences might also affect how each household responds to the CCT. A significant body of research shows that households do not tend to act as unitary or even Pareto-efficient entities (e.g. Chiappori, 1992; Thomas, 1990; Strauss and Thomas, 1995). This literature suggests that internal household dynamics could result in differential behaviours in terms of how households respond to incentives. For example, incentives to use health and education services might be unequally distributed within the household, for example, they might favour certain members or exclude others.

2.2.2 Programme design and delivery
The institutions, processes and politics around the implementation of the programme could also impact on how households perceive of, and respond to the CCT. Not all households, or household members, might have equal ease of access to the transfer, for example, depending on the distance and cost to travel to collection points, and health and education services. If the costs of participating are higher, households might be less likely to participate, or be required to use more of the cash transfer to fulfill conditions. Other programme variables that might affect realization of outcomes are how it is delivered, presented to the community, and monitored and evaluated (see Fiszbein and Schady 2009: 313-314). Experience and perceptions of fairness, clientelism or discrimination in selection of treatment households could also affect participation and the distribution of benefits through the programme.

2.2.3 Supply side conditions
CCT programmes are part of wider social protection infrastructure, which includes transfers to manage risks for households (social insurance) and also reduce poverty (social assistance) (Ferreira and Robalino, 2010). CCTs are only one aspect of a country’s social
protection landscape: they provide specific social assistance to poor households, but are usually not the only type of assistance being provided. Also, crucially, given transfers are conditioned on particular health, nutrition and education behaviours, their ability to contribute as planned depends on the quality and scope of these services. This conditionality means that variability in quality and access to services might influence, first, households’ willingness to abide with conditions, and second, the extent to which health, education and nutrition outcomes are realised through the use of the services. Fiszbein and Schady’s (2009) important overview work shows that questions remain on the long-term human capital effects of CCTs.

2.2.4 **Wider political, social and economic factors**

More widely, contextual factors beyond the recipient household, the programme and the social protection system can constrain and shape the effects of the programme. CCTs emerged within a particular country history and political context. The first CCTs were developed as flagship programmes by the national governments. Brazil’s Bolsa Escola and Bolsa Familia (Hall, 2004), and Mexico’s Progresa/Oportunidades were both wide-reaching and major social assistance initiatives by the national governments. Clear high level political commitment underpinned their creation and implementation. Additionally, international agencies and donors have become involved in supporting the development, implementation and evaluation of CCTs, introducing a wider group of interested and invested stakeholders (e.g. the World Bank, Inter-American Development Bank) (Hall, 2004). Political interests complicate the possibilities for programmes, and stakeholder interests must be considered for how they might influence the shape of the programmes. Finally, social and cultural, economic and environmental circumstances also constrain possible household outcomes, as well as incentives to behave in particular ways. For example, a drought or flood will affect households’ well being differently, depending on the household’s situation and economic activity.

The intersection of wider contextual factors with specific household characteristics creates a complex and varied landscape within which the programmes are implemented. Given this complexity, it is logical to hypothesise that households will participate and benefit from CCTs in different ways, depending on their particular situation, the way the programme is implemented, the wider social protection services, and the surrounding political, economic and social environment.
CHAPTER 3  Background to CCTs in the region

In this section, we present a brief narrative on the CCT programmes in Nicaragua and Honduras and their design features, including a summary of the evaluation evidence on average household effects.

CCTs were first introduced in Honduras in the early 1990s and in Nicaragua in 2000. The experimentally designed impact evaluations built into CCT programmes in Nicaragua and Honduras were unprecedented in scope and quality; the Red de Protección Social (RPS) pilot evaluation in Nicaragua was the first evaluation using a randomised experimental design conducted for a government programme in the country. Impact evaluations provide a wealth of information on the outcomes associated with the programmes\(^3\). These were commissioned and conducted by IFPRI for RPS in Nicaragua, PRAF II in Honduras and Atención a Crisis in Nicaragua.

3.1 Nicaragua’s Red de Protección Social

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<th>Key facts: Red de Protección Social</th>
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<tr>
<td><strong>Duration:</strong> 2000-2005</td>
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<tr>
<td><strong>Number of beneficiaries:</strong> 10,000 households during phase 1 which lasted from 2000-2002; 16,016 additional households in phase two</td>
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<tr>
<td><strong>Target population:</strong> Education benefits were targeted at poor households with children aged 7-13 enrolled in primary school grades 1-4. Health benefits were targeted to households with children aged 0-5 years.</td>
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<td><strong>Demand-side components</strong>(^4): Bono escolar (school attendance grant): C$240 per family every two months; Mochila escolar (School material support): C$275 per child per year; Bono alimentario (Health and nutrition): C$480 per family every two months; and Bono a la oferta (education supply incentive): C$80 per student per year to teacher/school</td>
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<td><strong>Supply-side components:</strong> Bono a la Oferta – Educación: transfers to families with children in grades 1-4 to be given to local school advisory committee or teacher (US$4.75 per student per year); Bono a la Oferta – Salud: a benefit of approximately US$54 per household annually to contracted private health service providers. These were conditional on service provision to beneficiary households.</td>
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<td><strong>Conditions:</strong> both health and education conditionalities applied. Health included: attendance at bimonthly health education workshops, at prescheduled health care visits monthly (aged 0-2) or bimonthly (aged 3-5); adequate weight gain and up-to-date vaccinations (aged 0-5) for households with children aged 0-5.</td>
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\(^4\) Amounts are recorded for phase one of RPS. Some programme components were adjusted in the second phase.
RPS was introduced as a social assistance programme for extremely poor households in Nicaragua, with the primary objective to contribute to human capital accumulation (Moore, 2009). Around the time RPS was introduced, in 2000, Nicaragua had the lowest per capita GDP in Central America and income per capita had stagnated. Nationally, 21% of households were extremely poor; in RPS communities, the proportion of extremely poor households was between 36 and 61%. Education and health indicators were also generally poor. For example, average net primary enrolment ratio in Nicaragua was 78%; adult and youth literacy rates were only a 54.2% in rural areas (Moore, 2009). Finally, devastation from Hurricane Mitch in 1998 had revealed poor households' vulnerability to shock.

RPS was targeted at poor households with children between the ages of 7 and 13 years enrolled in grades 1-4 (for the education benefit) and to children aged 0-5 (for the health benefit). Transfers were made to the child's caregiver in the form of cash at payment points; every two months the equivalent of $17 was allocated per family for school attendance and $34 per family for health and nutrition. Also, annually, $20 was allocated per child per year to support school materials to households, and $6 given per student per year to the school or teacher (Fiszbein and Schady 2009). As participation required use of health and education services, RPS was only implemented in communities meeting a threshold quality of supply side services. Also, the programme included provision of incentives to service providers (e.g. benefits from parents to teachers if lower levels of teacher absenteeism). Implementing and monitoring involved a complex network of stakeholders. For example, both service providers and beneficiary households had a role in monitoring co-responsibilities; local teachers and medical providers would indicate if households fulfilled co-responsibilities, and female beneficiaries had to validate data collection of service supplied by health service providers. There was some anecdotal evidence of variability in implementation and programme effects: for example, in how schools accommodated for increases in school attendance (e.g. some held additional daily sessions), and also in service providers’ compliance with their roles, as some medical teams were found not to visit the most remote rural homes. Also, in the programme’s second year, a severe drought hit participating households; this was at the same time as a persistent fall in international coffee prices. These placed particular stress on households dependent on the coffee sector.

Average household outcomes were reported after the first two years of implementation. Overall, the programme had positive effects on demand for health and education services (Fiszbein and Schady, 2009). Positive and significant effects were identified on the use of
health services. Improvements in treatment communities included an increased number of well-child check-ups for children aged three and younger, distribution of iron supplements to children, updates to child health cards and weighing of the child. Well-child check-ups increased by 16.3 percentage points from 70% coverage in the first year of RPS, and then dropped to an increase of only 8.4 percentage points in 2002. Large, positive and statistically significant effects were also noted on school enrolment and current attendance, with an average net increase in school enrolment of 13 percentage points, and of 20 percentage points on current attendance.

In contrast, the overall effect of RPS on expenditure varied slightly during the first two years of the programme. In 2001 the effect on household expenditure was slightly above the average transfer amount; in 2002 it was slightly below the transfer amount. Additional expenditure induced by RPS was mainly spent on food. However, caution must be taken in interpreting the effects on expenditure on food shares as the survey was conducted in different months at baseline (September) and the follow ups (October).

Final outcomes of RPS on health and learning are more difficult to estimate. RPS appears to have contributed to improved health outcomes for children. However, the evaluation did not analyse the pathways through which improvements in nutrition emerged making it difficult to trace the contribution of RPS to final outcomes in health. Still, overall, in treatment communities, households developed a more varied diet, and there was a net decline of 5.5 percentage points in the number of stunted children under the age of five. The number of underweight children (indicated by weight-for-age z scores) also fell in treatment communities by 3.9 percentage points; this indicator rose by two percentage points in control communities. However, there was no programme effect on haemoglobin levels or rates of anemia. Qualitative evidence indicated this could have been because parents chose not to administer distributed vitamins.

RPS was terminated in 2005. Moore (2009) suggests domestic support was insufficient to ensure programme sustainability; this raises questions about local perceptions and experiences of the programme, and the reasons why support might have been limited.

### 3.2 Nicaragua: Atención a Crisis

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<td><strong>Duration:</strong> 2005-2006</td>
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<td><strong>Number of beneficiaries:</strong> 3000 households; 90% of households in the region</td>
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<td><strong>Target population:</strong> Poor households in region affected by drought</td>
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<td><strong>Demand-side components:</strong> Food transfer: $145 per household per year. Education transfer: $90 per household per year. Training courses: $15 per household per month while participating in courses, up to 6 months. Occupational training benefit had an opportunity cost transfer up to $90 per household per year and course costs up to $140 per household per year. Business grant: $200 per household plus technical assistance to develop a business plan.</td>
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<tr>
<td><strong>Supply-side components:</strong> Supply-side transfer to schools: $13 per child; School supplies: $25 per child per year; Health transfer: $90 per household per year (the health transfer was never implemented)</td>
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<tr>
<td><strong>Conditions:</strong> Education: enrolment in grades 1-6 for children aged 7-15, regular attendance of 85%, and delivery of the teacher transfer to teacher. Occupational training: decide a member to take the course; conditional on attendance. Business grant: business plan approved by technical team in the Ministry of</td>
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Atención a Crisis is a small-scale CCT programme that was implemented in Nicaragua between November 2005 and December 2006, modelled after RPS, and introduced in the aftermath of a severe drought. The programme aimed to provide an immediate safety net to households through the cash transfer, and also strengthen households’ asset base and income diversification capacity (Del Carpio and Macours, 2009). Similar to RPS, a child’s caregiver was allocated transfers every two months as cash at a payment point; this was facilitated through existing public health infrastructure (in contrast to RPS which was allocated through private infrastructure). Per year, households received $145 for food and $90 for education. The school received $13 per child at the beginning of the school year (all schools receive this transfer per child of a household enrolled in the CCT); health providers were also to receive $90 per household per year but this was not implemented. Households also received $15 per month while participating in a training course for up to six months, as well as $25 per child per year for school supplies (Fiszbein and Schady 2009). Payments averaged 15% of recipient household’s per capita expenditures (Macours, Schady et al, 2008).

The programme was implemented among 3,000 households. Each household received one of three treatments: (i) a basic CCT conditioned on children’s primary school and health service attendance; (ii) a basic CCT plus a scholarship for occupational training; and (iii) a basic CCT plus a productive investments grant. Also, Atención a Crisis included a social marketing campaign (told that transfers were to improve children’s diversity and nutrient content of their diets, and to buy school materials.

Selection of treatment and control communities was random, conducted by a lottery. There were 56 intervention and 50 control communities in total. Program eligibility within selected communities was determined through proxy means testing.

Atención a Crisis was closely examined for how it affected child labour, education and health outcomes. A study by Del Carpio and Macours (2009) finds that the program contributed to decreased child labour, mainly for boys, due to larger decreases in agriculture and livestock activities. Older boys benefits most relative to their siblings; this appears to have lessened intra-household gender and age differences in child labour allocations. The change in intra-household allocation of labour also contributed to a reallocation of boys with lower skills or ability away from agricultural labour, potentially indicating compensation for lags in academic achievement. Another study finds the programme contributed to a significant increase in the proportion of expenditure towards schooling, but that this outcome did not extend to the wealthiest households (Macours, Schady et al, 2008).
Similar to RPS, Atención a Crisis contributed to improved use of preventive health care services: treated households were more likely to have their children weighted, and to receive vitamins, iron and deworming drugs. However, its contribution to health outcomes varied. Programme effects were not found for indicators of child health, specifically child height or weight. However, an effect on older children is evident when controlling for baseline characteristics. Finally, food expenditures increased overall for treated households; they also showed a disproportionate increase in expenditure on nutrient rich foods (animal proteins, fruit, vegetables). This was paralleled by similar increases in food intake for children below the age of seven.

At the end of nine months, Macours, Schady et al (2008) find some effects on learning outcomes (children engaged in the process of learning and educational outcomes). The CCT showed a large positive impact on personal-social and language development; this effect is largest for older pre-school aged children. The study also finds significant effects on child stimulation by caregivers, indicated by the availability of books, paper and pencils, and caregiver behaviour (e.g. read or tell the children stories).

3.3 Honduras - Programa de Asignacion Familiar II

**Key facts: Programa de Asignacion Familiar II**

**Duration:** 1998-2006

**Number of beneficiaries:** 240,000 households, from 17 departments, 133 municipalities and 1,115 towns (approximately 15% of the population)

**Target population:** Education transfers were targeted at poor households with children aged 6-12 who have not completed grade 4 of primary school. Health transfers were targeted at poor households with pregnant women and/or children less than 3 years. This population was identified through geographical targeting.

**Demand-side components:** Food security benefit: $113 per household per year; additional benefits in 4 departments with IDB support: (i) Education: $60 per household, (ii) Health: $40 per household and (iii) Delivery incentive: $60 per pregnant woman. There was also additional access to an integrated package of services: nutrition (AIN-C), health care and basic services.

**Supply-side components:** Health: Incentivo a la Calidad de los Servicios de Salud (Health Quality Incentive) (ICS) to Primary Health Care Units (UPS). The size of the benefit depends on the number of people the UPS served, ranging from US$ 3,000-15,000 with an average of US$ 5,000-6,000. Education: Incentivo al Desarrollo del Aprendizaje (Learning Development Incentive) to parent organisations (APFs). The size of the transfer depends on the size of the school, ranging between US$ 1,600-23,000, at an average of US$4,000 per year.

**Conditions:** Demand-side health: health centre visits, enforced only in IDB supported departments (either once a month or once every 3 months depending on age of child). Demand-side education: school enrolment and regular school attendance of 85%. Supply-side health: UPS managers must participate in quality-improvement processes, and units must provide health services according to standards outlined by the Secretary of Health and must meet annual goals regarding PRAF-II beneficiaries. Supply-side education: APFs must form and have a governing body, and schools must commit to use resources to fund education improvements.

**Frequency of transfer:** Every 6 months (but irregular), for as long as eligible

**Budget:** $20 million (2008)

**Stakeholders involved:** PRAF II was an autonomous program under the Secretaria de la Presidencia; management and provision of a loan by the IABD. BANHCARE and mobile units delivered the cash vouchers.
Conditional cash transfers were first introduced in Honduras in 1990, through a government programme which allocated grants to individuals from schools and health centres. These initial CCTs were developed to help prevent the poor from falling below a threshold level of poverty, and received funding from the World Bank and the IDB. Programme design for these original transfer programmes attached co-responsibilities to receipt of the transfers; however, monitoring was variable and it is generally concluded that conditionalities were not enforced.

Extreme poverty remained a challenge in Honduras into the late 1990s; 48.6% of households were in extreme poverty in 1999, and inequality had increased through the decade (Moore, 2008). In 1998, PRAF II was created as a separate CCT programme, with close involvement in management and funding by the IDB (Moore, 2008). PRAF II included demand and supply side components, and was implemented in municipalities with the highest prevalence of malnutrition (Morris et al., 2004). The IDB supported the programme in four departments in Honduras. In these areas, annually, each household was allocated $113 for the food security benefit, $60 for education and $40 for health; there was also $60 given per pregnant woman as a delivery incentive. Households with children aged 6-12 who had not completed grade 4 were targeted, as well as those with pregnant women and children less than 3 years old. Cash was given to the mother, through a voucher that could be cashed at local BANHCARE offices (a private national bank in Honduras also known as Banco Hondureño del Café) or mobile units every six months (in practice, timing of delivery was irregular) (Fiszbein and Schady 2009).

The evaluation design was a cluster-randomised trial, with a pre- and post-test control group repeated cross-section design. An intermediate evaluation of PRAF II was completed after two years; this revealed some challenges in delivery, monitoring and enforcement. Some reasons for these challenges were external; in the aftermath of Hurricane Mitch, NGOs could acquire cash incentives for working with international relief organisations, rather than for working with PRAF II. Some adjustments were made following this, for example, allocating the nutrition and health voucher to households rather than based on the number of children. A final evaluation assessment was not completed; IBD concluded at the end of 2006 that the evaluation design was too complex for the institutional infrastructure. As such, evaluation results are derived from the intermediate assessment.

Anecdotal evidence from the intermediate assessment indicates that despite intensive efforts to identify channels through which to transfer resources, there were still difficulties in transferring resources to the peripheral level. The timing of the evaluation also varied between years and locations. The evaluation was not conducted in the same months each year for all intervention and treatment groups. In 2000, some of the data collection for treatment groups was conducted between August and October, and some in November and December for one treatment and the control group. This is important because the latter time was coffee harvesting months (Glewwe and Olinto, 2004).

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5 See also Cohen, Franco and Villatoro, 2006
Still the intermediate evaluation provides some evidence of improved average household outcomes for PRAF II in the first years of its implementation. The transfer amount allocated through PRAF II equalled only 3.6% of a rural poor household’s total expenditures; this contrasts sharply with the total amount allocated through RPS, which was approximately 18% of a poor household’s expenditure (Moore, 2008). The transfer was also allocated biannually, versus every 2 months in RPS. Differences in the frequency and amount of the transfer arguably could affect the potential for improvements to human capital, for example, improved nutrition tied to changes to food expenditure.

Overall, positive effects were recorded for use of health and education services. In the first two years, the demand-side arm of PRAF II had a positive and statistically significant effect on grade progression at the 5% level (Glewwe and Olinto, 2004). Positive significant effects were also found on enrolment, and on reductions in days absent and dropout rates. However, the supply-side arm of PRAF II did not have significant effects on schooling outcomes. The exception is the effect on child working in the past seven days: while the demand-side intervention did not have a significant effect, the supply-side intervention had a small significant effect on whether the child had worked in the past seven days.

In the area of health, PRAF II appeared to have had a positive impact on the overall coverage of antenatal care and well-child check-ups. Associated with this, the coverage of growth monitoring also rose by 15–21 percentage points (p<0.01). However, there was not a significant effect on measles and tetanus toxoid immunisation (Morris et al, 2004). Analysis of programme implementation suggests that variability in delivery could have affected participants’ compliance with health care conditionalities. Households were not necessarily aware of the condition for a 10-day check up. Also, conditionalities were not enforced: no family was suspended from the programme for failing to maintain conditions for preventive health measures. Still, Morris et al (2004) postulate this effect could have been mitigated by the perception that families were monitored, derived from the requirement that families deposit coded slips on health centre visits.

No significant effects have been found on final health outcomes for children. Morris et al (2004) do not find any significant effects of PRAF II on child stunting, and height-for-age and weight-for-age z-scores. Blood haemoglobin levels and rates of anemia were also not significantly affected. One hypothesis put forward for this is ongoing deficiencies in the scope and quality of supply side services. Part of the reason for this could be failure to implement improvements to supply side services as had been planned (Morris et al, 2004).

Finally, the programme’s effect on learning outcomes, similar to RPS, has been more difficult to establish. Glewwe and Olinto (2004) suggest that PRAF II could have had a positive effect on educational attainment and learning by 2002. They found statistically

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6 Glewwe and Olinto (2004) used two methods to estimate the effects of PRAF II on school enrolment, school attendance and grade progression: cross-sectional difference estimators and double difference estimates. Data on drop outs, per capita expenditures, and enrolment was collected in 2001 and 2002. Attendance data was only collected in 2002. The results from Glewwe and Olinto’s (2004) double difference analysis differ slightly. DID estimates only find significant impacts of the demand intervention on enrolment in 2001; this estimate has a smaller standard error for enrolment than the CSDIF analysis. Variation in the nature of the variable for dropping out, and in interview dates at baseline, prevented effective use of DID to calculate effects on absenteeism and labour force participation.
significant higher attainment after two years, and suggest this could be attributed to improved learning because PRAF II did not have any statistically significant effect on dropout rates in 2002.

At a national level, Honduras continued to face challenges in educational attainment and poverty at the end of PRAF II; primary education levels had not improved and inequality continued to grow (Moore, 2008). A third phase in national CCT programming begun following the closure of PRAF II; the first transfers with PRAF III began in 2008.

3.4 Summary of findings

On average, the evaluations of Nicaragua’s and Honduras’ CCTs show that the programmes seem to have positive and significant effects on:

- the use of education and health services.
- household expenditure on schooling and food, which appears to consistently increase with the expansion of the CCT programme.

These two observations suggest direct consequences on household consumption. Immediate planned programme effects appear to be materialising.

However, the evidence is more mixed on human capital outcomes. Though some evaluations pointed to better pedagogy in school and some effect on learning outcomes, the effect on the health of children was not seen in several evaluations.

There were also implementation problems and contextual issues, which may contribute to the CCT outcomes. For example, at the extreme, it appears the supply side arm of PRAF II was not implemented at all. Also environmental conditions framing households’ experiences appear to differ – for example, between coffee growing and non coffee growing households in Nicaragua; these households would have been unequally affected by the drought in 2001, one year after RPS was introduced.
CHAPTER 4  Methodology

This review focused on studies that looked at factors that enabled or challenged the realisation of programme outcomes for the three CCTs implemented in Nicaragua and Honduras. Evidence on the average household outcomes for these programmes has been published; this review contributes to existing knowledge by synthesising and discussing studies which explore the ‘how and why’ for programme outcomes, and their variation between participating households and communities.

To locate relevant studies, a rapid evidence assessment was conducted to search for and assess relevant literature. The primary search was conducted via seven electronic databases and eight organizational websites (a list of databases and websites is provided in appendix A), as well as bibliographic back-searching. The search covered peer-reviewed published studies as well as grey literature and PhD dissertations. Search terms included cash transfer, conditional cash transfer, asset transfer, Central America, Honduras, Nicaragua. The search was restricted by language (only Spanish and English studies were considered due to language limitations of the review team) and timeframe (only studies conducted using data from RPS, PRAF II or Atención a Crisis). The search strategy was piloted, to ensure that the search terms were feasible and appropriate to retrieval of the type of studies sought. Studies were included or excluded based on topic relevance, and methodological rigour and relevance criteria. Due to resource constraints one researcher applied exclusion criteria to the studies. However, the list of included studies, and quality assessment was reviewed by senior researchers of the project team. Studies only identifying average outcomes were excluded.

A quality assessment was done of the included studies, to identify potential biases resulting from inclusion of studies that were comparatively less rigorous and of lower quality. For quantitative studies, criteria for methodological quality and rigour were developed by adapting and applying the Maryland Scale of Scientific Methods (MSSM) (Sherman and Gottfredson, 1998). To quality assess qualitative studies, we avoid using scores to assess qualitative studies, but instead use a narrative assessment.7 The framework is based on the

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7 Scores were assigned on seven variables: presence of control comparison group(s), use of control variables to account for initial group differences, variable measurement, controls for effects of attrition from study, use of statistical significance tests, and overall evaluation methodology. The detailed protocol is reproduced in Appendix A.

8 At the present time, to the best of our knowledge there is no agreement on the most effective form of quality assessment for qualitative studies. Indeed, some feel that “at present, opinion on the value of formal quality
Critical Appraisal Skills Programme (CASP)’s ten questions for appraising qualitative studies. These questions consider the rigour of the qualitative methods and their application to the study, the credibility of findings, and the relevance to the systematic review question.9

4.1 Search results

The figure below provides an overview of the results of the search for relevant studies. Our initial searches in the databases and websites listed in the previous section produced 1,121 potentially relevant hits before duplicates were removed. A total of 73 full-text papers were eventually selected for more in-depth review and application of the methodological quality score. Hand-searching of the reference lists of all selected papers yielded an additional 5 studies for review. Of these 78 papers, 47 were excluded on the basis of topic or methodological relevance. Of the remaining 31 papers, 18 were excluded as they only reported on average household outcomes, and did not report on variation in outcomes between households. This left us with a total of 13 included studies examining potential variation in outcomes for three CCT programmes in Nicaragua and Honduras.

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9 The detailed protocol is reproduced in Appendix A.
4.2 **Characteristics of included studies**

This section describes the main features of the studies included in this review. Appendix A provides further details on the methodology. Thirteen studies were found that considered impacts on variability of CCT programme outcomes in Nicaragua and Honduras. Table 1 presents the studies included in this review.

The search located ten quasi-experimental studies. Of these, six look at household characteristics, one considers the impact of programme variables on outcomes, and five consider how wider economic and socio-political factors impact on how communities and households benefit from the CCT.

Also relevant to the review question are qualitative studies: such methods can provide insight into the causal pathways through which households, programme structure and delivery, and wider socioeconomic considerations might influence programme outcomes (Adato and Roopnaraine et al, 2004). Qualitative analysis of why outcomes might vary was thin: our review only located two studies reporting qualitative evidence, one drawing on data from a qualitative evaluation conducted for RPS, Nicaragua (Adato and Roopnaraine et al, 2004), and a second study drawing a comparative analysis across six countries in Latin America, which included Nicaragua and Honduras.

<table>
<thead>
<tr>
<th>Author, year</th>
<th>Programme considered</th>
<th>Study design</th>
<th>Outcomes considered</th>
<th>Explanatory variables considered</th>
</tr>
</thead>
</table>
| 1. Adato and Roopnaraine et al, 2004 | RPS, Nicaragua | Qualitative, interviews and observations | Various | Stakeholder perceptions
Household behaviour
Programme delivery |
| 2. Coady, Olinto and Caldes, 2003 | PRAF II, Honduras | Quantitative, randomised experimental design | Household consumption | Economic shock (drop in coffee prices)
Area of economic activity (coffee growing/not coffee growing) |
| 3. Dammert, 2008 | RPS, Nicaragua | Quantitative, randomised experimental design | Schooling
Child work (hours)
Per capita expenditure (total, and of food)
Food share of total expenditures | Location of household
Level of household poverty
Household expenditure distribution |
| 4. Gee, 2010 | RPS, Nicaragua | Quantitative, randomised experimental design | Child work (total hours, probability of work) | Programme structures |
| 5. Gitter and Barham, 2008 | RPS, Nicaragua | Quantitative, randomised experimental design | School enrolment, spending Food and education expenditures | Women’s power in the household by years to schooling relative to husband |
| 6. Gitter and Barham, 2009 | RPS, Nicaragua | Quantitative, randomised experimental design | Child time in school and in work | Drought
Major decline in coffee prices |
| 7. Gitter and Caldes, 2010 | RPS, Nicaragua | Quantitative, randomised experimental design | Variety in food bundles
Food expenditure | Initial level of household poverty
Shock due to coffee prices |
| 8. Gitter, 2006 | RPS, Nicaragua | Quantitative, randomised experimental design | Household public goods
Housework
Child education | Women’s power in the household relative to men’s (e.g. by literacy levels) |
<table>
<thead>
<tr>
<th>Author, year</th>
<th>Programme considered</th>
<th>Study design</th>
<th>Outcomes considered</th>
<th>Explanatory variables considered</th>
</tr>
</thead>
<tbody>
<tr>
<td>9. Glewwe and Olinto, 2004</td>
<td>PRAF II, Honduras</td>
<td>Quantitative, randomised experimental design</td>
<td>School enrolment, dropout rates, and annual promotion to the next grade Child labour force participation</td>
<td>Household income</td>
</tr>
<tr>
<td>10. Maluccio, 2005</td>
<td>RPS, Nicaragua</td>
<td>Quantitative, randomised experimental design</td>
<td>Labour force participation Occupational choice Per capita expenditure School enrolment Child labour Child nutritional status</td>
<td>Shock due to coffee prices Area of economic activity (coffee growing/not coffee growing)</td>
</tr>
<tr>
<td>11. Maluccio and Flores, 2005</td>
<td>RPS, Nicaragua</td>
<td>Quantitative, randomised experimental design</td>
<td>Schooling Labour time and participation Health services</td>
<td>Household poverty/income level</td>
</tr>
<tr>
<td>12. Maluccio, Murphy and Regalia, 2009</td>
<td>RPS, Nicaragua</td>
<td>Quantitative, randomised experimental design</td>
<td>Child dropout rates, grade repetition rates</td>
<td>Grade availability, number of school sessions per day, and number of teachers</td>
</tr>
<tr>
<td>13. Vernon and Rivero-Fuentes, 2008</td>
<td>El Salvador PRAF II, Honduras RPS, Nicaragua</td>
<td>Qualitative, document and literature reviews, site visits and key informant interviews</td>
<td>Reproductive health Programme characteristics and delivery Household characteristics (e.g. adult literacy)</td>
<td></td>
</tr>
</tbody>
</table>
This chapter presents the results of the review on what the evidence says about what influences variability in programme outcomes in low income settings, and the reasons for this. We took the contextual factors (household, programme, external and wider economic) that we introduced in Chapter 2 and looked at the available evidence. The distribution of evidence across the independent and outcome variables examined is presented in Table 2. At first glance, the evidence base appears to be strongest on the extent to which economic factors (household and wider economic conditions) might affect variation in child schooling, household expenditure and the distribution of resources between household members. This focus is not necessarily surprising given each programme’s emphasis on educational benefits and conditionalities. What is perhaps more noticeable by its absence is the relatively few studies that consider variation in learning and cognitive development outcomes, and the few studies that consider how differences in supply side conditions related to the provision of education and health services and programme implementation (from targeting to delivery) might affect heterogeneity in outcomes. This is particularly notable given each programme had supply side components. As such, the weight of evidence differs per contextual factor.

In this chapter we examine and synthesise what the evidence says about the effects of household, programme, supply-side services and wider contextual factors on programme outcomes in turn.

Table 2: Explanatory and outcome variables considered by included quantitative studies

<table>
<thead>
<tr>
<th>Household-level: Household poverty/income</th>
<th>Expenditures and resource distribution</th>
<th>Schooling</th>
<th>Labour time/participation</th>
<th>Health services</th>
<th>Nutrition or health</th>
<th>Learning</th>
</tr>
</thead>
</table>

10 This table only includes associations examined through quantitative studies; qualitative studies tended to look widely across potential outcomes at the causal pathways involved and as such cannot be specifically mapped out on this table. How they inform understanding of variation in programme effects is discussed throughout this chapter.
5.1 Household characteristics

Programme effects on schooling and child labour outcomes are greater for poorer households

Programme evaluations of PRAF II and RPS indicate that household income levels can have a significant impact on effect size. Looking at evidence from PRAF II and RPS it appears that poorer households tend to experience larger effects on schooling outcomes. Glewwe and Olinto (2004)\(^\text{11}\) find that the effect size of PRAF II on school enrolment after two years of implementation was twice the average impact for those 1.7 standard deviations below the median level of expenditure, while those 1.7 standard deviations above the mean level of expenditure were not affected by the CCT. This difference in outcomes appears to only become apparent after the second year as households with lower income levels begin to show higher effects on school enrolment. In contrast, children from households with higher per capita expenditures were slightly less likely to drop out. The effect on hours worked by children was the reverse: the effect on hours worked by children appears to be slightly higher for poorer households (indicated by per capita expenditure).\(^\text{12}\)

Similar to PRAF II, improvements in use of education services appear to be largest for poorer households for RPS\(^\text{13}\). Gitter and Barham’s (2009) study indicates households with

\(^{11}\) Findings from this study are robust, and consider effects using both cross sectional difference and double difference estimators. They find that household income level has significant impacts on school enrolment of children in participating households, when using the log of households’ food and non-food expenditure (after dividing by household size) as a proxy for household income.

\(^{12}\) Another study, using census data collected in 2001, also finds significant and positive effects on enrolment, and on child labour. Galiani and McEwan (2011) find that the CCT demand side intervention has a significant and positive effect on enrolment, and a small but significant effect in reducing child labour and home working. Again effects are found to vary with household-level characteristics. When separately out effects by household poverty (measured by height-for-age \(z\) scores) significant effects are found to be concentrated among the poorest two quintiles of households. This suggests that effects are relatively greater among the poorest households, versus households that are slightly richer although still absolutely poor.

\(^{13}\) Another study finds Atención a Crisis had a significant effect on the proportion of expenditure towards schooling, but that this outcome did not extend to the wealthiest households. Macours, Schady and Vakis, 2008.
the least land holdings in coffee growing communities experience the largest positive effect on school enrolment. Using data from the two year randomised evaluation, Dammert (2008) and Maluccio and Flores (2005) both confirm differential effects on schooling and child labour outcomes with household income level. Comparing across communities, Dammert (2008) finds the CCT to have a smaller impact on schooling outcomes, but larger impact on children’s working hours in more impoverished communities. At the household level, Maluccio and Flores (2005) report improvements in school enrolment, current attendance and grade progression are larger for poorer households. Improved school enrolment, attendance and grade progression also varied with the age of the child.\(^{14}\)

Gains in expenditures on education were concentrated among extremely poor and poor households, and households with children aged 7-13.

**The effect of household income on health and nutrition outcomes are ambiguous**

Gitter and Caldes (2010) suggest that household income levels might only affect certain programme outcomes; for example households with higher income levels might be more able to engage in conditioned behaviours prior to the CCT, and so the programme effects could be smaller than for lower income households. After raising this question, Gitter and Caldes (2010) indicate that baseline poverty levels did not have a significant effect on the extent to which the CCT impacted on food security indicators (i.e. household food expenditures and food variety). Maluccio and Flores (2005) also find that income did not contribute to any significant variation in programme effects on vaccination rates. However, income was found to matter for effects on the use of health services: a significant effect was found for the use of health services\(^{15}\); the CCT only showed significant effects on use of health services for extremely poor households and not for households categorised as poor or non-poor.\(^{16}\)

**Intra-household gender relations are found to contribute to variation in the distribution of outcomes within the household**

Another set of studies consider how intra-household gender dynamics might impact on the programme effects within the household level. Two studies find that the relative position of the female head in the household affects how programme outcomes are experienced within the household. Gitter and Barham (2008) measure women’s power in the household by her years of schooling relative to her husband’s. They find a greater proportion of household resources are allocated to children when women have relatively more years of schooling. However, the important of gender to programme effects seems to differ depending on which outcome is concerned. Gitter and Barham (2008) find women’s

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\(^{14}\) The evaluation could not determine statistically increased vaccination coverage due to RPS with the intervention group (vaccination rates rose 30 percentage points in the intervention and control areas while on average they were decreasing nationally in rural areas. RPS baseline in 2000 for up to date vaccinations was 40% (children ages 12-23 months)

\(^{15}\) Indicated by use of health services provided through the programme, VPCD (programa vigilancia y promoción del crecimiento y desarrollo).

\(^{16}\) The evaluation could not determine statistically increased vaccination coverage due to RPS with the intervention group (vaccination rates rose 30 percentage points in the intervention and control areas while on average they were decreasing nationally in rural areas. RPS baseline in 2000 for up to date vaccinations was 40% (children ages 12-23 months)
relative years of school does not significantly affect the distribution of school enrolment within the household. Gitter (2006) considers the effects of RPS on school enrolment and expenditure vary, depending if either the male or female household head is literate. The effects of RPS on education expenditure and on boys’ school enrolment are greater if only the female partner is literate; in contrast, the greatest increase on school enrolment occurred for girls when only the male partner was literate. By looking at different indicators, these two studies confirm the view that households are not necessarily Pareto efficient, and that relations between household members can affect the decisions made about household members’ welfare.

Overall, studies looking at differences in outcomes between households suggest household characteristics do correspond with variation in the extent to which households change their behaviour in the areas of health, nutrition and education. However, the direction and nature of this effect is complex. A large scale qualitative evaluation conducted with Nicaragua’s RPS provides some insight into the reasons why initial household poverty levels could impact on programme effect size (Adato and Roopnaraine, 2004). They identified households were locked into cycles of indebtedness to local stores before RPS began. Rather than help households break out of indebtedness, the CCT appeared to be used by families within cyclical patterns of indebtedness. Households would choose to first use the transfer to help pay off some of their debt, but then would accrue additional debt to purchase goods for consumption. Beyond this study, no further qualitative work into the causal pathways through which initial household characteristics and dynamics might impact on engagement with and benefits from the CCT programmes was identified.

5.2 Programme characteristics

While anecdotal evidence from RPS and PRAF II suggest that programme delivery was not always done to plan, few studies empirically examine how programme implementation might affect household outcomes. Gee (2010) considers this by first examining the causal impact of RPS on the incidence and duration of child labour. He then compares these results with studies looking at similar indicators in other CCT programmes in Latin America. Overall, Gee concludes findings are largely consistent between PETI, Bolsa

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17 Hernandez, Sam, Gonzalez-Vega and Chen (2009) planned to consider the differential effects of RPS in Nicaragua on access to loans by household characteristics (average age, homeownership, self-reported value of home plot, household shocks). However, they find using double difference impact estimates that on average RPS did not have a significant impact on household requests for credit. Looking at remittances, they find that the impact of the above household characteristics are also not significant. The only significant (and positive) parameter was the value of durable and non-durable goods owned by the household prior to collection of data. However, as RPS did not significantly affect selected outcome measures, variability of effects could not be assessed along the different household parameters.

18 Adato and Roopnaraine et al (2004) used interviewing and observations in 6 intervention communities, spending 7-8 weeks each, and 2 non-intervention communities (2 weeks in each).

19 Gee (2010) used difference-in-difference estimators to assess the effectiveness of RPS on changes in child labour within households. However, when considering how effectiveness might vary, this study is less rigorous and does not offer strong empirical evidence to ascertain causal relationships and/or pathways between programme characteristics, and programme outcomes.
Escola and Progresa. However, he notes two differences. First, RPS results in a statistically significant decline in a child’s probability to work, which contrasts with the effect of PRAF II. Second, Progresa results in a statistically significant decline in work hours per week for children who work; this was not found in Gee’s study of PRAF II. Gee suggests that these differences might be the result of variation in programme targeting, subsidy amounts and educational requirements. However, beyond offering hypothesis, Gee (2010) does not analyse the causal pathways through which such programme factors might impact on the effectiveness of the CCT of household outcomes.

Vernon and Rivero-Fuentes (2008) and Adato and Roopnaraine (2009) provide some evidence as to why households might respond differently, depending on how the programme is implemented. Vernon and Rivero-Fuentes (2008) posit that participants respond differently by who designs programme structures and delivery, and the flexibility of design. Participants seem to be more willing to support delivery and conditionalities when there is flexibility for them to engage in programme design and delivery. Adato and Roopnaraine (2009) suggest that household behaviour varies with individuals’ views of programme ‘fairness’. Some community members involved in RPS felt programme delivery was marked by errors of exclusion in targeting; negative feelings around targeting seemed to contribute to social tensions. Vernon and Rivero-Fuentes (2008) also find that misguided perceptions of what the programme entails can influence people’s behaviour, in unanticipated ways. For example, in Nicaragua, the evaluation of RPS found households would dissolve, separating a single mother from the extended family, as they perceived this would make them eligible for the programme. This could negatively impact on family relations and support networks (Vernon and Rivero-Fuentes, 2008).

5.3 External and wider economic factors

Communities with initially worse-off schooling conditions benefit most from the CCT

A crucial assumption behind CCT programmes’ potential contribution to improved use of health and education services, and final outcomes, is that supply of services is sufficient to meet demand. However, quality and access to services is not consistent, for example, the distance to services is often greater in rural versus urban communities.

One study examines if schooling outcomes vary with a community’s baseline quality of schools. To test this, Maluccio, Murphy and Regalia (2009) first examine the average effects of RPS across households; they find positive and significant average effects on enrolment and grade progression, and negative effects on grade repetition and dropout.

20 This study uses desk review, review of literature, and on-site visits including key informant interviews to review reproductive health outcomes in El Salvador, Honduras and Nicaragua, the Dominican Republic, Jamaica and Peru. Main conclusions are synthesised across all countries, making it difficult to tease out specific causal pathways between specific programmes and household outcomes. Also, through providing a list of documents reviewed, there is little information on the protocol used for the literature review, and the on-site visits, making verification of the findings difficult.

21 Maluccio, Murphy and Regalia (2009) use difference-in-difference techniques, and data from the 2000 RPS programme baseline survey and three follow-up surveys completed in 2001, 2002 and 2004, as well as administrative data. The authors test results with and without controls; this changes estimated impacts only marginally.
rates for 5-9 and 7-9 year old age groups. They then calculate whether or not there are potential differential effects could be attributed to initial supply conditions. They find larger effects on grade progression for children aged 5-9 in households in communities where initial supply conditions are poor (measured by grade availability and distance to school), and in communities with autonomous schools.

Maluccio, Murphy and Regalia (2009) also find the CCT has a spill over effect on the quality of services: treatment communities showed a marginal improvement in grade availability, number of sessions per day, and number of teachers, were greater.\textsuperscript{22} This was again more pronounced in communities with initially poorer grade availability and distance to school. Why this occurred was unclear: the authors hypothesise this was due to increased effort by programme personnel, who worked relatively harder to improve school conditions in worse-off communities.

Overall, evidence on the reasons why households might change their behaviour depending on different supply conditions have not been analysed through qualitative approaches. Vernon and Rivero-Fuentes (2008)’s review suggests real and perceived costs associated with participation shapes participants’ willingness to comply with conditionalities (Vernon and Rivero-Fuentes, 2008). However, more nuanced analysis of why engagement was higher in communities with autonomous schools or lower baseline quality of schools is not provided.

**School attendance and household expenditure outcomes seem to remain amidst economic shocks**

Our initial hypothesis proposed that wider social, political and economic contexts can constrain the scope of potential outcomes available to participants, as well as their incentives to do so. There is little existing evidence to test this hypothesis; evidence thus far is confined to considering the effects of macro-level economic circumstances on CCT outcomes.

Two studies analyse the effect of economic shocks and differences in productive economic activity on schooling outcomes in Nicaragua. They both find some larger effects on schooling in coffee growing communities versus non coffee growing communities. Gitter and Barham (2009) find households with the least land holdings in coffee growing communities experienced the largest positive effect on school enrolment. These effects were largest in higher price years. Maluccio (2005) looks specifically at schooling outcomes for boys and girls separately; this study concludes RPS has a larger effect on girls’ school enrolment and school attendance in coffee growing communities. As a result, it helped to equalise schooling rates between girls in coffee growing and non-coffee growing communities\textsuperscript{23}. Outcomes varied less for boys, but impacts on boys’ school attendance were slightly higher in coffee growing areas.

\textsuperscript{22} Distance ceased to be a significant variable when looking only at 7-9 year olds

\textsuperscript{23} There was a negligible impact on enrolment for girls aged 7-12 years in non-coffee growing areas, but more than a 20% point increase for their counterparts in coffee growing areas.
This review also found that economic shocks and the type of household economic activity contribute to differences in household expenditure and consumption. Maluccio (2005)\(^{24}\), focusing on differences at the community level with RPS, finds the effect on per capita expenditure to be substantially larger in coffee growing areas, compared to non-coffee growing areas.\(^{25}\) Gitter and Caldes (2010) also identify that the effects of RPS on consumption appear to hold during a drought and a fall in international coffee prices. Between 2000 and 2002, treatment and non-treatment household in coffee growing communities experienced a fall in per capita food consumption; however, on average, households in treatment communities were less affected. In non-coffee producing communities, treatment households’ consumption increased by 10%, contrasting with a decline of 10% in non-participating households in these communities.

This evidence from RPS differs from a similar study conducted using data from PRAF II. Here, Coady, Olinto and Caldes (2003) also compare between coffee and non-coffee producing households, but do not identify significant variation in per capita consumption, despite differences in initial consumption levels.\(^{26}\) However, they do find that PRAF II continued to show a significant effect on per capita consumption amidst a decline in international coffee prices.

Finally, the effect of economic factors on nutrition and health outcomes is ambiguous. While Maluccio (2005) finds that there was a difference in girls’ schooling outcomes between coffee and non-coffee growing communities, he does not identify any significant difference in effects on child height-for-age $z$ scores between these communities. He suggests one reason for this could be the smaller sample size. However, Gitter and Caldes (2010) also do not identify a significant difference in nutrition outcomes between coffee and non-coffee growing communities, despite variation in food expenditures between treatment and control groups in the two communities. The only significant difference found by Gitter and Caldes (2010) is that treatment households in coffee communities on average began to substitute fat for meat during the economic crisis. Gitter and Caldes (2010) conclude that the marginal impact of RPS on food expenditure is relatively constant across household income levels, and with economic shocks affecting production.

\(^{24}\) Maluccio (2005) divided poor and non-poor households using a proxy means model, based on a set of indicators highly correlated with logarithmic per capita expenditures, and based on the size of landholdings as less or greater than 1 hectare.

\(^{25}\) RPS was found to reduce the total number of hours dedicated to agriculture in coffee growing areas, but the effect on the fraction of labour hours spent in agriculture was negative among non-coffee growing areas and positive in coffee growing areas by 2002.

\(^{26}\) This finding held when comparing eligible and non-eligible households in program municipalities, when restricting the sample to households fitting with programme eligibility criteria (e.g. with children aged less than 13 years or with a pregnant woman); and when analysing only those households actually receiving transfers.
CHAPTER 6  Conclusions and recommendations for future research

This review focused on analysing the evidence on what factors impact on variability in CCT programme implementation and outcomes in low income settings, using early data from Nicaragua and Honduras. The aim of this was to summarise early evidence on CCTs in low income settings, and suggest hypotheses for further research. In this final chapter we summarise and reflect on the key emerging findings from the evidence, limitations of the evidence base, and finally suggest key future research directions.

6.1 Summary of key findings

Household, programme and wider contextual factors shape the size and nature of programme effects

From the evaluations of RPS and PRAF II, it is clear that the context in which CCTs are implemented matters. While the particular variables of significance, and the ways that they might contribute to outcomes varies, variation in household effects was found to vary.

Poorer households and communities tend to experience greater relative effects on school enrolment, and a reduction of child working hours

CCTs were implemented in Nicaragua and Honduras with the intention of helping to alleviate immediate and longer term poverty for extremely poor households within already small and low income countries. Evidence from this review suggests that the CCTs can contribute to this objective with regards to education and child work indicators. Reports of main findings from the PRAF II and RPS evaluations find evidence of larger effects on school enrolment among poorer households. Similar effects were found at the community level: with larger effects on school enrolment and grade progression in communities with initially lower levels of grade availability and greater distances to schools.

However, why this is occurs, and the extent to which this relationship necessarily holds, is ambiguous. Contradictory evidence is found regarding the effects on different education indicators and RPS. Maluccio and Flores (2005) report larger improvements for poorer households for enrolment and per capita expenditures. In contrast, Dammert (2008), using quantile regression, concludes that poorer households experience less of an impact on total and per capita food expenditures, and on schooling outcomes. However, Dammert (2008) does find poorer households benefit more with regards to a reduction in child working hours.
Programme effects are greater when economic conditions are favourable

Studies comparing effects during variable macro-economic conditions find that while programme effects on schooling are greater when macro-economic conditions are more favourable. This is illustrated through a comparison between years with lower and higher coffee prices, in coffee growing communities.

However, CCTs also help to lessen the effect of economic shock on household consumption

While absolute effect size on school outcomes was found to be greater under more favourable economic conditions, CCTs were still found to help mitigate the potential detrimental effect on household consumption during economic shocks. Treatment households in both coffee and non-coffee growing communities demonstrated relatively higher levels of consumption during periods of economic shock than their counterparts. In coffee growing communities this meant a smaller fall in per capita food consumption; in non-coffee growing communities this equated an increase in per capita food consumption (Gitter and Caldes, 2010).

How external factors affect nutrition and health outcomes remains unclear

While baseline household poverty levels, productive activity and economic shock were found to affect the relative effect size in education for treatment households, evidence of a similar effect on nutrition and health was unclear. In terms of use of health services, household poverty did not affect vaccination rates. Effects on nutrition appear also not to differ significantly: food security outcomes were not significantly affected by baseline poverty levels (Gitter and Caldes, 2010), as well, nutrition and child height-for-age z scores were also not significantly different between households in coffee and non-coffee growing communities. This suggests that such outcomes might be less sensitive to difference in the economic situation of participating households.

Real and perceived dimensions of programme implementation affect participants’ incentives to comply with conditionalities

Finally, while evidence on the specific pathways through which implementation affects programme effects, this review indicates that both real and perceived dimensions of programme delivery shape people’s incentives for engagement in the CCT programme. Various hypotheses are put forward to explain why this might occur, including: differences in programme design (e.g. targeting, subsidy amount, conditionalities), how this is implemented (e.g. level of enforcement, margin of error), and how this is perceived (e.g. do participants perceive implementation to be fair?).

6.2 Limitations

This review faces particular limitations, first linked to the review methodology itself, and second, to the nature of the evidence. Most of the evidence came from studies which examined data from the randomised evaluations implemented in the initial years of the programmes. All three programmes use a similar evaluation design. As a result, their evidence base faces some shared limitations:

- **Lack of analysis of long-term outcomes**: Evaluations were only completed for the first phases of the CCTs: Atención a Crisis lasted only one year, PRAF II’s evaluations were only completed for the pilot phase, and Nicaragua’s RPS was
terminated after five years. As a result, evaluations have no collected data on long term impacts.

- **Randomisation at the community, not household, level**: Randomisation was done at the community or group level, while outcomes were most often measured at the household level. This contributes to a potential grouped error problem. All studies used Difference in Difference (DID) methods to account for unobserved differences at baseline point between treatment and control groups. However, the extent to which studies considered these limitations sought mitigation strategies and brought varied, affecting their relative methodological reliability.

- **Attrition biases**: There was variation in the degree to which studies reported on attrition. A few studies identified low attrition rates (Del Carpio, 2010) and/or nearly identical rates between treatment and control groups (Gitter, 2006). However, still some suggest that though the overall attrition rates might be similar or small, they are not necessarily random. Regarding RPS, Maluccio (2005) suggests attrition from treatment and controls groups is unlikely to be random, and could impact on the validity of results.

These differences do not prevent the use of the survey data from the three programmes for analysis of outcomes; however, they do warrant consideration in the analyses. However, still, overall the randomised evaluations conducted are relatively robust and rigorous in both countries. Finally, qualitative evidence provides further context to quantitative evaluations, indicating variability or errors in implementation and enforcement, and further reasons for particular behaviours and outcomes. In this review, we found a limited number of qualitative papers of good quality, possibly reflecting on the strong reliance among policy-makers on impact evaluations to understand the outcomes of CCT programmes.

Qualitative evidence raises particular challenges in assessing and verifying its reliability and robustness. Qualitative studies are limited by a reporting bias; studies can only be evaluated on the basis of what the researchers report on the completed work, not necessarily the work itself. However, such research often draws on unstructured methods that are sensitive to the social context, rich in detail, complex, and involve an inductive analytic process. However, qualitative research may be judged as methodologically relevant to developing an understanding of the conceptual framework. For this reason, we have chosen to include qualitative findings in the discussion.

Finally, the review was also limited as we could not include papers for which the quality of evidence was not verified. Methodological quality was primarily assessed based on information reported in papers, articles or books. If an author did not comprehensively report their methodology and findings, their study would not necessarily pass the quality appraisal criteria, even if the original work was of high quality. Also, papers that were completed by students as part of a Masters or Bachelors-level degree were excluded. Still, we sought to be rigorous in searching for evidence, and also cross-validate findings from quantitative and qualitative studies and from each country. Additionally, we report both

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27 Reporting bias affects both qualitative and quantitative studies, due to potential selectively or incomplete reporting. However, this is a particular concern for qualitative studies, when the data is bound up with researchers' experience.
trends and outliers in the evidence. As such, we have sought to be comprehensive and objective in reporting on the evidence from published studies on Nicaragua’s and Honduras’ CCTs.

6.3 Future research directions

Despite the potential rich evidence from the evaluations conducted for Nicaragua’s and Honduras’ CCT programmes to date, there remain some key areas for further research. Building on the key findings summarised in Section 6.1, we suggest a key gap in existing knowledge is about the causal pathways through which different household and economic factors affect the outcomes experienced at household levels. The evidence suggests that differences at household and macroeconomic level matter, but exactly why this is the case is unclear. Only two qualitative studies were conducted to consider the reasons for potential variation in incentives and final outcomes.

Second, there has been a focus to date on outcomes in education, with little evidence on the extent to which outcomes in learning, nutrition and health might vary between communities and households. CCTs have multiple objectives, often including improvements to both health and education indicators for children, in particular. Further study on these other planned outcomes of CCTs is important to building a holistic picture of how different factors shape the extent to which programmes achieve their aims.

Another key area where there is little evidence is on the political and governance dimensions of programme implementation, and their effects on household experiences and outcomes. A qualitative review of the history of RPS (Moore, 2009) suggests the programme had political dimensions, which contributed to its eventual termination in 2005. However, what is less known is how political and governance variables might impact on how the programme is implemented in different communities, and how different households are engaged. In-depth examination into the nuances around actual programme delivery, and its impacts on participants’ experiences remains another area for future research.

Finally, a challenge in low-income and small countries is to effectively and efficiently administer targeting, delivery of the cash transfer, supply of services, and monitoring of compliance with potentially limited resources and capacity. An interesting finding from this review is that communities with lower quality schools can actually show larger improvements in school quality, versus communities with higher quality schools at baseline. This suggests value in further research into how CCTs might be able to benefit poorer communities, and help mitigate inequalities.

While rigorous pilot evaluations have been conducted on these early experiences with CCTs in low income settings, much scope remains for more in-depth analysis of programme implementation and delivery in particular, and potential variation in the realisation of a diversity of planned outcomes between participating households.
6.4 **Wider implications for policy-makers**

Policy-makers have a good understanding that CCTs are likely to have a positive effect on household expenditure and the take-up of public services in low-income settings, resulting in most cases in reductions in poverty. However, questions remain about:

- the impact of CCTs on longer-term human capital outcomes such as child health and social mobility;
- the differential impact on different types of households;
- the relationship between institutional factors related to delivery and monitoring (i.e. the “black box” of implementation) and outcomes;
- the relationship between the incentive and level of incentive and the effect of a CCT;
- the relationship between external and macro-economic factors and the effect of the CCTs; and
- the long-term sustainability of CCT programmes and outcomes.

These questions raise some important implications for policy-makers. Firstly, policy-makers need to look beyond the benefit level and targeting of a CCT and consider macro-economic factors, monitoring and support of the supply-side when developing the programme. Secondly, evaluators may have relied too much on standardised impact evaluations. To gain a picture of a broader set of factors and their relationship with CCT outcomes, a further set of studies and process evaluations need to accompany standard impact evaluations. Specifically qualitative approaches and mixed methods approaches have been underappreciated in how evidence has been collected resulting in important gaps of evidence in many countries implementing CCT programmes. As such, to gain a broader understanding of CCTs, different evaluation approaches may be required.
Reference list


Appendix A: Detailed review methodology

For this systematic review, the primary search was conducted via seven electronic databases and eight organizational websites, Google scholar, as well as bibliographic back-searching. The search covered peer-reviewed published studies as well as grey literature and Ph.D dissertations. Two tiers of search terms were used in combination. First tier terms included cash transfer, conditional cash transfer and asset transfer. Second tier terms were Central America, Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua and Panama. Each of the keywords in tier 1 was used in combination with each of the keywords in tier 2. We used combinations of the search terms in each tier in formats applicable to each database and website. Truncation was used as appropriate. Individual search terms were used in combination with each term from the other two levels, to ensure retrieval of the breadth of potentially relevant studies. Relevant subject headings were included as appropriate, along with “keywords” from titles and abstracts. There were no limits by language in the searches.

Exclusion decisions were made by first reviewing all the titles retrieved through the searches and selecting relevant titles as well as those which did not provide enough information to ascertain relevance for review of abstracts. Abstracts of these studies where then read to screen for identification of relevant studies. The full text if those studies that met this criteria, as well as those without sufficient information to ascertain relevance, were retrieved for further assessment.

Due to resource constraints the application of exclusion criteria and review of the studies was completed by one researcher. However, the list of included studies, and quality assessment was quality reviewed by senior experts in the project team.

6.5 Initial pilot testing

The search strategy was piloted before the full search was conducted, to ensure that the search terms used were yielding sufficient and relevant hits. The search terms provided a manageable number of hits and preliminary inclusion/exclusion criteria were applied. In addition, the pilot testing indicated that studies relevant to our review were published from the 1990s, when CCT programmes were first implemented in the region, so the cut-off date for the search was set to 1990.
6.6 Sources

Based on the pilot testing, we conducted searches for materials dated back to the year 1990. With the assistance of a reference librarian, reports were identified through searches in two phases. The first phase consisted of searches in the following sources:

<table>
<thead>
<tr>
<th>Subscription and non-subscription databases</th>
<th>Key websites (institutions and organizations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Web of Science</td>
<td>Inter-American Development Bank</td>
</tr>
<tr>
<td>EconLit</td>
<td>DFID</td>
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<tr>
<td>Academic Search Complete</td>
<td>ELDIS</td>
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<tr>
<td>Worldcat</td>
<td>Centre for Global Development</td>
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<tr>
<td>Jstor</td>
<td>World Bank</td>
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<tr>
<td>BLDS at IDS</td>
<td>USAID Development Experience Clearinghouse</td>
</tr>
<tr>
<td>IDEAS</td>
<td>Office of Evaluation and Oversight of the Inter-American Development Bank</td>
</tr>
<tr>
<td></td>
<td>Latin American and Caribbean Economic Association</td>
</tr>
</tbody>
</table>

In the second phase we added to these results by ‘snowballing’ (hand-searching bibliographies of relevant papers that met the relevance inclusion criteria as described below) to identify additional articles. Additional studies (published and unpublished) were searched by making direct contact with authors and experts in the field.

6.7 Screening

Titles and abstracts were excluded strictly on the basis of their topic relevance to the review question (i.e. does the focus of the study under review contribute to answering the review’s research question), and if they were an empirical study. At this stage, any study considering outcomes of a CCT programme in Central America, and those where there was uncertainty about relevance, was included. Only at the stage of review of full text was the methodological quality of the empirical studies considered. Inclusion criteria for selection of full text were as follows:

- Location: does it look at either Nicaragua or Honduras
- Type of programme: does it consider a programme coupling a cash transfer with specific conditions for receipt of that transfer?

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Variables: does the study consider outcomes in the context of external conditions (e.g., not just direct causational effects of programme implementation on particular outcomes, but consideration of factors affecting variability within or between CCT programmes)

Methodology: only empirical studies (this could be experimental, quasi-experimental or qualitative) were included.

Studies considered were also restricted by language (only Spanish and English studies were considered due to language limitations of the review team) and timeframe (only studies since 1990).

Finally, though to be included in the final synthesis and analysis studies were required to consider potential variability in programme outcomes, when reviewing titles and abstracts, we included any empirical study considering outcomes of CCT programmes in Central America, as such this included evaluations that provided an overview of average outcomes, or that provided evidence on some variability but did not attempt to explain why this occurred. To situate the evidence on variability of outcomes, evidence from these impact evaluations on outcomes generally is summarised in this review. However, the synthesis and conclusions from the review focus strictly on studies that consider explanatory variables.

6.8 Methodological quality for quantitative studies

For quantitative studies, criteria for methodological quality and rigour were developed by adapting and applying the Maryland Scale of Scientific Methods (MSSM) (Sherman and Gottfredson, 1998). The MSSM provides a rating system for comparing and assessing methodological rigor and effect size for primary evidence analysis. It considers, for instance, sample size, use of control variables, variable measurement and control for effects of attrition, and can be applied across a variety of types of quantitative studies. A rating was assigned to each study through an assessment of their description of the methodological approach. Scores were assigned on seven variables: presence of control comparison group(s), use of control variables to account for initial group differences, variable measurement, controls for effects of attrition from study, use of statistical significance tests, and overall evaluation methodology. In addition, information was recorded on sample size and duration of study. If, for a particular study, in any category the methodological approach was unclear or unstated (e.g., no mention of attrition was made), up to a maximum of two efforts were made to contact the authors for clarification. If the methodological approach could not be clarified further, the lowest rating for that category was assigned.

6.9 Methodological quality for qualitative studies

At the present time, to the best of our knowledge there is no agreement on the most effective form of quality assessment for qualitative studies. Indeed, some feel that “at present, opinion on the value of formal quality assessment is divided and there is insufficient evidence to inform a judgement on the rigour or added value of various approaches” (Noyes et al., 2008, 20.3.2.2). For this review, we thus avoid using scores to
assess qualitative studies, but instead use a narrative assessment, considering the relevance and rigour of the findings, study design, data collection, analysis, reporting and research conduct. The framework is based on the 18 questions for appraising qualitative studies in Spencer (2003), adapted to include elements from the Critical Appraisal Skills Programme (CASP)’s 10 questions for appraising qualitative studies. These questions consider the rigour of the qualitative methods and their application to the study, the credibility of findings, and the relevance to the systematic review question. It considers these aspects through an assessment of the research design, sampling, data collection, reflexivity and recognition of researcher biases, ethical issues, data analysis, findings, and the value of research. Although no explicit numerical scoring is applied, the framework is used to provide evaluate the quality of reporting of study methodology, and whether the study then meets reasonable overall standards.

6.10 Applying inclusion and exclusion criteria

All titles selected, abstracts reviewed and full papers retrieved were saved in an electronic database. Titles which appeared to fulfil inclusion criteria for topic and methodological relevance and those which did not provide enough information to ascertain suitability were earmarked for extraction of abstracts. The inclusion criteria for relevance were again applied to abstracts. These, as well as abstracts which did not provide enough information to ascertain suitability for inclusion, were selected for retrieval of full texts. Finally, selected full texts were also screened for topic and methodological relevance. Those that fit the relevance inclusion criteria were eligible for methodological quality assessment; those which did not were excluded. Studies were then assessed for methodological quality. Studies using qualitative and quantitative methodologies were assess separately according to the criteria outlined in Chapter 4. No study was excluded based on methodological quality; rather this information was used to inform discussion of the results, and to identify key gaps and limitations of the evidence base.

6.11 Characterisation of included studies

In reviewing the full-text of studies meeting the topic and methodological relevance criteria, the reviewer captured key data from each of the papers using a data extraction form designed by the research team and entered in an excel spreadsheet. Descriptive information recorded included.

- Full bibliographical reference
- Publication type (peer review journal article, institution working paper)
- Country or region of intervention studied
- Type of asset transfer examined (micro-credit, conditional cash transfer, etc)
- Other intervention characteristics (funded and/or run by NGO, government, private entity; pilot, small scale intervention, large scale intervention; etc).
- Client characteristics
- Study design, time period, and sample size
- Outcome/s under investigation
- Findings (quantitative and qualitative).
6.12 **Synthesis of evidence**

We conducted a narrative synthesis of findings guided by the conceptual framework, which presents the results reported in each included paper, and discusses findings by type of intervention examined. The synthesis of evidence considers both how qualitative and quantitative studies inform the research question and the conceptual framework.

The narrative synthesis addresses the evidence from quantitative studies on whether, and under what circumstances, specific external factors impact on the outcomes of CCT programmes in Central America (and for what types of outcomes). Insights from qualitative studies are considered for how they inform understanding of the mechanisms and processes through which specific external factors might impact on specific outcomes of CCT programmes in the region.
Appendix B: Assessment frameworks for quantitative and qualitative studies

Quantitative quality appraisal tool

Scoring Protocol for Methodological Rigour (completed for each analytical module or programme in the paper (some papers may evaluate more than one programme or group, or analyse more than one unit (e.g. households and villages)).

<table>
<thead>
<tr>
<th>1. Sample size</th>
<th>Location [not scored]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>individuals_________</td>
</tr>
<tr>
<td></td>
<td>households___________</td>
</tr>
<tr>
<td></td>
<td>blocks, cities, states, or other geographical units________</td>
</tr>
<tr>
<td></td>
<td>communities/villages________</td>
</tr>
<tr>
<td></td>
<td>other relevant unit (specify)__________</td>
</tr>
<tr>
<td></td>
<td>Country__________</td>
</tr>
<tr>
<td></td>
<td>Rural/Urban_________</td>
</tr>
<tr>
<td>Time period</td>
<td>Inception date_____</td>
</tr>
<tr>
<td></td>
<td>Total study length____</td>
</tr>
<tr>
<td></td>
<td>Length of time from end of treatment to last follow-up (in months)____</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>2. Presence of control comparison group(s)</th>
<th>1=No comparison group present</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2=Separate comparison group present, but non-randomly constituted and limited (e.g., only demographic variables) or no information on pre-treatment equivalence of groups</td>
</tr>
<tr>
<td></td>
<td>3=Separate comparison group present but non-randomly constituted; extensive information provided on pre-treatment equivalence of groups; obvious group differences on important variables</td>
</tr>
<tr>
<td></td>
<td>4=Separate comparison group present; extensive information provided on pre-treatment equivalence of groups; only minor group differences evident</td>
</tr>
<tr>
<td>Rating</td>
<td>Description</td>
</tr>
<tr>
<td>--------</td>
<td>-------------</td>
</tr>
<tr>
<td>5</td>
<td>Random assignment to comparison and treatment groups; differences between groups are not greater than expected by chance; units for random assignment match units for analysis. Note: Sometimes random assignment takes place at a different level from the analysis. For example, microfinance groups are randomly assigned to conditions, but members are the unit of analysis. These cases should not be treated as random assignments.</td>
</tr>
<tr>
<td>3</td>
<td>Use of control variables to account for initial group differences</td>
</tr>
<tr>
<td>1</td>
<td>No use of control variables to adjust for initial group differences.</td>
</tr>
<tr>
<td>3</td>
<td>Control variables used, but many possible relevant differences uncontrolled.</td>
</tr>
<tr>
<td>5</td>
<td>Most relevant initial differences (e.g., differences on a pre-treatment measure of the dependent variable or variables highly associated with the dependent variable) between groups controlled statistically or random assignment to groups resulted in no initial differences.</td>
</tr>
<tr>
<td>4</td>
<td>Variable measurement</td>
</tr>
<tr>
<td>1</td>
<td>No systematic reproducible approach to variable measurement is employed.</td>
</tr>
<tr>
<td>2</td>
<td>No indication of how study variables were constructed or obtained.</td>
</tr>
<tr>
<td>3</td>
<td>Some attention to constructing or obtaining high quality measures, but reliability not demonstrated.</td>
</tr>
<tr>
<td>4</td>
<td>Variables developed or selected with some consideration of use in prior studies and reliability of measurement; reliability reported; not all measures demonstrated to be reliable.</td>
</tr>
<tr>
<td>5</td>
<td>Careful selection of relevant variables considering their prior use and reliability demonstrated for all or most of the measures.</td>
</tr>
<tr>
<td>5</td>
<td>Control for effects of attrition from study</td>
</tr>
<tr>
<td>1</td>
<td>Attrition from treatment or control group is greater than 50% and no attempt is made to determine the effects of attrition on the outcome measures.</td>
</tr>
<tr>
<td>2</td>
<td>No accounting given of cases that dropped out of study or attrition from treatment or control group is moderate and no attempt is made to determine the effects of attrition on the outcome measures.</td>
</tr>
<tr>
<td>3</td>
<td>Differences between study participants (both treatment and comparison) who were present at the pre-test and absent at the post-test are identified and discussed.</td>
</tr>
<tr>
<td>4</td>
<td>Differences between study participants (both treatment and comparison) who were present at the pre-test and absent at the post-test are identified and discussed; possible differential attrition between treatment and comparison groups is discussed.</td>
</tr>
<tr>
<td>---</td>
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</tr>
<tr>
<td>5</td>
<td>Careful statistical controls for the effects of attrition are employed, or attrition is shown to be minimal; threat of differential attrition for treatment and comparison groups is addressed adequately.</td>
</tr>
<tr>
<td>Note: Attrition is loss from the initial sample or population identified as the treatment group or the comparison group. Sometimes attrition occurs even before a pre-test is administered.</td>
<td></td>
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</tbody>
</table>

| 6. Use of statistical significance tests | 0 | No statistical tests or effect sizes |
|  | 1 | Statistical tests used or effect sizes computed |

| 7. Overall evaluation methodology | 1 | No reliance or confidence should be placed on the results of this evaluation because of the number and type of serious shortcomings(s) in the methodology employed |
|  | 3 | Methodology rigorous in some respects, weak in others |
|  | 5 | Methodology rigorous in almost all respects |
| Note: Key elements in your rating of overall methodology should be: |

- **Control of extraneous variables**: Have the influences of independent variables extraneous to the purpose of the study been minimised (usually through random assignment to conditions, matching treatment and comparison groups carefully, or statistically controlling for extraneous variables)?

- **Minimisation of error variance**: Are the measures relatively free of error?

- **Sufficiency of power to detect meaningful differences [if not explicit, consider whether sample size seems reasonable]**
Qualitative quality appraisal framework

For this review, we avoid using scores to assess qualitative studies, but instead use a narrative assessment, considering the relevance and rigour of the findings, study design, data collection, analysis, reporting and research conduct. The following framework is based on the 18 questions for appraising qualitative studies in Spencer (2003), adapted to include elements from the 10 questions for appraising qualitative studies in the Critical Appraisal Skills Programme (2006).

<table>
<thead>
<tr>
<th>Criteria</th>
<th>Quality indicators (possible features for consideration)</th>
</tr>
</thead>
</table>
| Topic relevance | • Considers a CCT in Nicaragua or Honduras  
• Directly examines if there are differences between households or communities in experiences of programme outcomes, and the reasons for this |
| Credibility of findings | • Findings/conclusions are supported by data and study evidence, and resonate with other knowledge and experience  
• Findings/conclusions have a coherent logic  
• There is evidence of validation/triangulation of findings to support or refine findings  
• Quotations are numbered or otherwise identified to demonstrate that they are not from one/two people |
| Breadth and depth of study findings (scope for wider inferences) | • Study aims and design are set in the context of existing knowledge/understanding (e.g. literature review to summarise knowledge to date)  
• Findings are presented/conceptualised in a way that offers new insights/alternative ways of thinking  
• Presents the potential for wider inferences from the study:  
  o Describes how findings are relevant to the wider population from which the sample is drawn  
  o Describes the contexts in which the study was conducted to allow applicability to other settings to be assessed  
  o Discusses the limitations on drawing wider inferences |
| Extent to which the evaluation addresses original aims and purposes | • Clear statement of study aims and objectives, and of reasons for any changes in objectives  
• Findings are clearly linked to the purposes of the study  
• Discusses limitations of study in meeting aims (e.g. gaps in coverage, restricted access to setting or participants, unresolved areas of questioning, time constraints, etc.) |
| Defensibility of design | • Discusses how overall research strategy was designed to meet the aims of the study  
• Discusses the rationale for study design; including convincing arguments for different features of research design (e.g. multiple methods, time frames, reasons for components of research, etc.)  
• Use of different features of design/data sources is evident in the findings presented  
• Discusses limitations of research design and their implications for the study evidence |
### Criteria

<table>
<thead>
<tr>
<th>Quality indicators (possible features for consideration)</th>
</tr>
</thead>
<tbody>
<tr>
<td>- Describes any changes made to the design, their justification, and implications for the study</td>
</tr>
</tbody>
</table>

### Defensibility of sample design and coverage

**Specifically consider the gender of participants included/excluded from the study, and factors affected the participation and coverage of men versus women.**

<table>
<thead>
<tr>
<th>Quality indicators (possible features for consideration)</th>
</tr>
</thead>
<tbody>
<tr>
<td>- Target sample is appropriate to accessing the type of knowledge sought</td>
</tr>
<tr>
<td>- Discusses how sample/selections allowed the required comparisons to be made</td>
</tr>
<tr>
<td>- Sample profile is detailed</td>
</tr>
<tr>
<td>- Describes location/areas and how/why chosen</td>
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<tr>
<td>- Describes population of interest and sample’s relationship to it</td>
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<tr>
<td>- Discusses missing coverage and implications for evidence</td>
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<tr>
<td>- Documents reasons for non-participation/exclusion among the sample</td>
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<tr>
<td>- Evidence of maximising inclusion (e.g. language matching, translation, specialised recruitment)</td>
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<tr>
<td>- Discusses access and methods of approach and how these might have affected participation and coverage</td>
</tr>
<tr>
<td>- Discusses why some people may have chosen not to participate</td>
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### Quality of data collection

<table>
<thead>
<tr>
<th>Quality indicators (possible features for consideration)</th>
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<tbody>
<tr>
<td>- Data collection tools were piloted</td>
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<tr>
<td>- Data collection was comprehensive, flexible and sensitive enough to provide a complete and/or rich description</td>
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<tr>
<td>- E.g. time with participants, more than one method of data collection, follow-up?</td>
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<tr>
<td>- Discusses how fieldwork methods or settings may have influenced data collected</td>
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<tr>
<td>- Discusses the saturation of data</td>
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<tr>
<td>- Clear discussion of data collection tools/approach</td>
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<tr>
<td>- Who conducted data collection</td>
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<tr>
<td>- Procedures and documents used for collection and recording; conventions for field notes</td>
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<tr>
<td>- Checks on origins, status and authorship</td>
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<tr>
<td>- Audio or video recording of interviews, discussions and/or conversations (or justification for why not)</td>
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<tr>
<td>- Steps taken to ensure participants were able and willing to contribute</td>
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<tr>
<td>- Discussion of strengths and weaknesses of data sources and methods</td>
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</table>

### Quality of the approach and formulation of the analysis

<table>
<thead>
<tr>
<th>Quality indicators (possible features for consideration)</th>
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<tbody>
<tr>
<td>- Data analysis methods were systematic:</td>
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<tr>
<td>- Describes the form of the original data</td>
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<tr>
<td>- Clear rationale for choice of management methods, tools and package</td>
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<tr>
<td>- Evidence of how analytic categories, classes, labels, etc. have been generated and used</td>
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<tr>
<td>- Discussion, with examples, of how any constructed analytic concepts/typologies have been devised and applied</td>
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<tr>
<td>- Analysis is balanced in the extent to which it is guided by preconceptions or by the data</td>
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<td>- Diversity of perspectives explored</td>
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<td>- Discussion of bias in forming the research question (e.g.</td>
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<tr>
<td>Criteria</td>
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<td>---------------------------------------------------------------</td>
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<td><strong>gender of researcher?)</strong></td>
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<tr>
<td><strong>Clarity and coherence of reporting</strong></td>
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<tr>
<td><strong>Clarity of assumptions that have shaped the form and output of the evaluation</strong></td>
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<tr>
<td><strong>Attention to ethical issues</strong></td>
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