How Effective are Cash Transfer Programmes at Improving Nutritional Status?

A Rapid Evidence Assessment of Programmes' Effects on Anthropometric Outcomes

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List of abbreviations

BMIZ	Body Mass Index for Age Z-score
CCT	Conditional Cash Transfer (Programme)
CT	Cash Transfer (Programme)
DPT	Diphtheria, Pertussis, and Tetanus (vaccine)
HAZ	Height for Age Z-score
PROGRESA	Mexican CCT later renamed *Oportunidades*
PSM	Propensity Score Matching
WAZ	Weight for Age Z-score
WHZ	Weight for Height Z-score

Abstract

Cash transfer programmes are a widely applied social protection scheme that has achieved successes in fighting poverty worldwide. A large literature has sprung up around these programmes, yet the relationship between cash transfer programmes and the nutritional status of recipient children is unclear. Can cash transfers be counted on to improve child health, or are mediating characteristics important? In particular, conditionalities are expensive: are conditional cash transfer programmes more successful than unconditional programmes?

To answer these questions, we reviewed the literature linking nutritional status to interventions involving cash transfers in developing countries. A systematic, but not exhaustive, search identified over 30,000 articles from which we identified 18 studies of 15 social assistance programmes that reported impacts on height for age and found that programmes diverged greatly in their effectiveness. To do a more detailed analysis of the pathways by which programmes succeeded, we gathered data on a variety of study, programme, recipient, and country characteristics. Statistical meta-analysis using this data allowed us to isolate some of the factors contributing to programme success.

Our key findings include higher marginal effects in the most disadvantaged areas and that girls benefit more than boys in terms of height for age measures. We find higher marginal effects in countries with poorer health care systems. We also find that in our sample, conditional programmes accomplish about the same as unconditional programmes: the difference is not statistically significant.

However, this hides an important dichotomy. Conditionalities with health components are statistically indistinguishable from unconditional programmes, while other types of requirements strongly inhibit child growth. Because of the ambiguity and potential for both help and harm, we conclude that programme evaluation must be integrated into such interventions, and encourage the further accumulation of evidence on this topic. We anticipated that more programs would have looked at impacts on height for age, but comparatively few programs examined this outcome. One important outcome of our search is a list of evaluated programmes which can now be used as a starting point for future research qualitatively investigating causal pathways from social protection interventions to child growth behind the broad correlations we have identified.

Executive Summary

Background

One of the most widely implemented development policies over the past years has been the Cash Transfer (CT) programme, implemented in as many as 35 countries as of 2008. Although achieving successes in areas like education (that is, getting children into school) and reductions in child labour, cash transfer programmes have had inconsistent effects on child nutritional status, a common measure of human capital accumulation. This paper summarises the state of the evidence regarding the relationship between cash transfer programmes and the nutritional status of children in recipient households. The paper also asks the question: Which intervention and population characteristics facilitate or limit the effects of transfers on nutritional status?

Nutritional status is a crucial, summary measure of overall child health and development potential. Specifically the primary outcome of interest in this paper is height for age z-scores (HAZ), a comparison of child height against international norms. Height for age indicates a child's underlying physical health and is associated with mental development as well. Cash transfer programmes seek to provide households with food and health care, and have succeeded in improving calories consumed and dietary diversity. However, changes in child nutritional status among recipients have been mixed.

Objectives

Our first purpose is to gather as much evidence as possible to assess the effectiveness of CT programmes, both conditional and unconditional, at improving child nutritional status. Previous studies have mostly centred on conditional programmes in Latin America, and we expand the evidence base to see the broad trends in the CT- nutritional status relationship in other contexts.

After collecting as much evidence as possible to describe the linkage, we identify variables that mediate the relationship. We review the literature for theoretical links between the programmes and HAZ, and based on our summary of the literature we create a theoretical framework from which we obtain a list of hypotheses to test. We look at the roles played by household and community characteristics, programme effects on use of health care, programme-inspired changes in food consumption, the effectiveness of nutritional supplements, payment size, the type of conditions imposed, and study characteristics.

Methods

We sought existing studies which examine the impact of a cash transfer programme on child anthropometric outcomes. In particular, included studies had to report authors' original estimates of the impact of an intervention, at least one component of which had to be a direct cash transfer to selected beneficiaries. In addition, the relevant studies had to focus on the programme effect on anthropometric outcomes, such as height for age and weight for age.

Starting with articles we knew through our own previous research, we first amassed references by "snowballing," i.e. recursively tracking down references of references. Next we developed a systematic but not exhaustive search strategy for citation search engines and ultimately reviewed over 30,000 search results from dozens of databases before paging through hundreds of additional articles through journal hand-searches. We also contacted many experts in the field asking for suggestions.

Each search and instance of data extraction was performed independently by two researchers, who afterward compared their results.

Details of included studies

We finished our survey having found 24 papers on 18 programmes in 11 countries that met our criteria of including quantitative estimates of programme impacts on height for age. We were able to use height for age estimates from 18 articles on 15 interventions in 10 countries, while the rest used other anthropometric outcomes. Most papers provided multiple estimates of programme effects either through analysing different subgroups or using different statistical methods.

In addition to the assessed impacts we collected data on a wide variety of covariates. Based on availability we divide mediating factors into four groups: recipient characteristics, programme characteristics, study characteristics, and country characteristics. Recipient characteristics include the age and sex of the children analysed. Programme characteristics include use of nutritional supplements, type of conditionality, enforcement of conditionality, and the size of the transfer. Study characteristics include peer review, randomised programme design, sample size, the baseline HAZ, and an index of programme quality we developed ourselves based on Cochrane guidelines. Country characteristics reflect both the disease environment and the accessibility of health infrastructure. (Since community level data is unavailable for most of our sample, we use the World Bank's World Development Indicators for this portion of the analysis.)

Synthesis results

We analyse the data using two statistical methods, both involving the use of random effects weighting to combine data from multiple studies. Studies with less variance in their estimates are given more weight. To answer the question of whether cash transfers are always or almost always effective, we first combine the data to get a pooled estimate of CT programme effectiveness at improving HAZ. After weighting each of the data points, Stata's meta-analysis command estimates the average programme effect at about 0.04 standard deviations, a result that is not statistically distinguishable from zero. As we see below, this result hides considerable variation and calls attention to some factors strongly inhibiting program success.

To analyse covariates we use meta-regression analysis including inverse-variance weighted random effects to estimate the marginal impact of a given covariate. Low sample sizes limit the certainty with which we can announce findings, but indications are as follows. The overall average effect is positive but with a small magnitude that is not statistically distinguishable from zero. We see some evidence of larger programme effects on girls. In spite of the attention paid to making transfers conditional, we find little difference between unconditional and conditional transfers, and factors other than conditionality are likely to have greater significance than whether a transfer is conditional or unconditional. The point estimate for the effects of unconditional programmes is larger than that for conditional programmes, but separate consideration of conditionalities by type of condition reveals that on average health and education conditions have about the same impact on child height for age as unconditional programmes. Other types of conditions, mostly related to working or saving, show strongly negative impacts on nutritional status. One of our stronger results is that programmes in countries with poor infant mortality rates and low numbers of hospital beds per capita tend to see stronger results on nutritional status from social assistance programmes

incorporating cash transfers than countries that have a more developed medical infrastructure.

Conclusion and recommendations

While CT programmes have had successes on numerous fronts, improving education and reducing child labour, this study identifies factors have limited programme impacts on the nutritional status of children in recipient households. The grand average shows programme effects which are statistically indistinguishable from zero, but more detailed analysis shows circumstances under which programmes are effective. Meta-regression analysis identifies conditionalities linked to requiring work or savings behaviour as adversely affecting programme success. On the other hand, the marginal return to intervention is higher where hospital beds are scarce and infant mortality rates are high. (We do not compare the effectiveness of cash transfer programmes against interventions improving local health infrastructure, and we cannot tell which achieves greater returns per investment.) We do see overall trends toward increased effectiveness for girls' nutritional status. Unconditional programmes and conditional programmes promoting health are roughly comparable. Because outcomes are so variable, we are reminded of the importance of incorporating impact evaluation into social assistance interventions. Increased data will also improve the accuracy of models like ours that analyse the effectiveness of a given class of programmes.

1. Background

We initially sought to investigate the ambiguous link between conditional cash transfer programmes and height for age as described by Fiszbein and Schady (2009) and others. At the encouragement of our reviewers we expanded our purview to include also unconditional cash transfer programmes so as to analyse the importance of conditionalities in such interventions. We further expanded our scope to include other anthropometric measures such as weight for age in hopes of addressing the effectiveness of cash transfers in emergency situations, since in such cases we are unlikely to observe impacts on height for age, a long-term outcome.

A brief note about notation: we often refer to conditional cash transfer programmes, or CCTs, unconditional cash transfer programmes (UCTs) and/ or cash transfer programmes (CTs). The last group subsumes the first two. Since much of the previous literature focuses on CCTs, our literature review by necessity deals with these programmes at greater length, though we ultimately seek to analyse effects of all CTs. Most prior studies have focused on CCTs alone, but to the extent that their analysis ignores the conditional aspect of those programmes their work can be considered to apply to CTs in general. The importance of being conditional is a topic we address as part of our investigation.

1.1 Aims and rationale for current review

One of the most widely implemented development policies over the past years has been the Conditional Cash Transfer (CCT) programme, implemented in as many as 48 countries as of 2008.[1] Targeted toward the poor, these programmes distribute cash payments to participants if they meet conditions typically including sending children to school and/or getting regular health care.

Although achieving some successes, these programmes have not consistently improved recipient children's nutritional status, a common measure of human capital accumulation. This paper summarises the state of the evidence regarding the relationship between cash transfer programmes and the nutritional status of children in recipient households. It addresses the question of which intervention and population characteristics facilitate or limit the effects of transfers on nutritional status.

Maximising programme effectiveness requires identifying the characteristics of successful and unsuccessful programs, but this analysis has not previously been undertaken. While previous works have mentioned the issue as part of broader surveys of the relationship between CCTs and health, none have focused on the issue per se, none have included unconditional cash transfer programmes as a comparison, and none have looked at anthropometrics beyond five programmes in Latin America. This paper accomplishes all of these aims.

[1] Countries with programmes or pilots underway include Argentina, Bangladesh, Bolivia, Botswana, Brazil, Brunei, Burkina Faso, Cambodia, Chile, China, Colombia, Costa Rica, the Dominican Republic, Ecuador, El Salvador, Honduras, India, Indonesia, Jamaica, Kenya, Lesotho, Malawi, Maldives, Mauritius, Mexico, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Nigeria, Pakistan, Panama, Paraguay, Peru, Philippines, Sierra Leone, South Africa, Swaziland, Tanzania, Thailand, Trinidad and Tobago, Turkey, Uganda, the United States, Uruguay, Yemen, and Zambia, among others (Fiszbein and Schady 2009; Fernald, Gertler, and Neufeld 2009; Gaarder, Glassman, and Todd 2010, Bassett 2008, Barrientos, Niño-Zarazúa and Maitrot 2010).

1.1.1 Cash Transfer programmes

CTs are targeted interventions that provide cash to selected beneficiaries. CCTs provide cash contingent upon the recipients' engaging in certain specified actions such as sending children to school, attending educational talks, or getting health care. Because they require complementary supply-side inputs such as schools and clinics, middle income countries were the first to provide such services, but the programmes are now found in countries all over the world. Mexico and Brazil started the first CCTs in the late 1990s, and almost 15 years later several national programmes service millions of people. The programmes in Mexico and Ecuador now provide income to 25% and 40% of the countries' populations respectively while Brazil's Bolsa Família covers about 46 million people (Fiszbein and Schady 2009). Even many states within the United States have begun implementing CCTs in an attempt to improve educational outcomes (Bassett 2008; Fryer 2010).

CTs have expanded so widely in part because they have made important gains in improving the well-being of recipients. Fiszbein and Schady (2009) show that in each of four Latin American countries (Colombia, Mexico, Honduras, and Nicaragua) CTs have made a statistically significant impact on poverty according to the three consumption-based indices that comprise the Foster-Greer-Thorbecke measure. Studies also show clear impacts on educational enrolments (Skoufias and McClafferty 2001; Schultz 2004) and a few show positive effects on cognitive development in early childhood (Fernald, Gertler et al. 2008; Macours, Schady et al. 2008; Fernald, Gertler et al. 2009; Paxson and Schady 2010).

1.1.2 Cash Transfer programmes' effects on nutritional status

Cash transfer programmes' effects on child nutritional status are less clear. Fiszbein and Schady's (2009) report calls attention to unresolved questions: "Although there is clear evidence that CCTs have increased the use of education and health services, evidence on the impact of CCTs on 'final' outcomes in education and health is more mixed" (p. 20). Likewise, in their review of the CCT literature, Glassman, Todd, and Gaarder (2007) document a "mixed result" of CCTs on nutritional status (p. 27). As Leroy, Ruel, and Verhofstadt (2009) also note:

> Notwithstanding the enormous potential of CCT programmes to contribute to reducing childhood undernutrition, this potential has yet to be unleashed: the programmes are far from eliminating linear growth retardation, and their impact on micronutrient nutrition is disappointingly small. (p.124)

Some programmes have indeed found success in improving nutritional status. Studies show that the Mexican programme *Oportunidades* (formerly known as PROGRESA) improved child growth in the short run (i.e. follow-up after 2-5 years of enrolment) in both rural (Gertler 2004; Rivera, Sotres-Alvarez et al. 2004) and urban areas (Leroy, Garcia-Guerra et al. 2008). Height gains were apparent in the medium run of 6-10 years when impacts were evaluated in terms of transfers received rather than time on the programme (Fernald, Gertler, and Neufeld 2008; Fernald, Gertler, and Neufeld 2009), though those findings have been questioned (Attanasio, Meghir et al. 2010). Behrman and Hoddinott (2005) find mixed results, while other authors cast doubt on the early findings of enhanced growth (Fiszbein and Schady 2009). Improvements in the height of preschool children have also been shown in analyses of CCTs in Ecuador (Paxson and Schady 2010) and Colombia (Attanasio, Battistin et al. 2005). On the other hand, no impacts were shown in Nicaragua (Maluccio and Flores 2005) or Honduras (Moore 2008), and negative impacts on height were shown in Brazil (Morris, Olinto et al. 2004).

Are the conditionalities having unforeseen effects? Are other factors impeding programme success? Do some programmes simply not improve human capital? Little is known.

1.1.3 Role of Conditionalities

The conditions placed on receiving payments from CCTs are designed to incentivise household investment in human capital accumulation. High discount rates or the undervaluing of services such as education or health care are assumed to be keeping the poor from making optimal decisions. Positive externalities from education and health care also imply that the socially optimal level of investment may not be chosen by those cognizant only of the private benefits of these services, so subsidies may be socially optimal in any case (Bassett 2008).

On the other hand, an insistence on conditionalities may be misplaced. A book published this year (Hanlon, Barrientos et al. 2010) questions the importance of conditionalities, citing numerous cases in which unconditional programmes improved welfare. For example, in countries lacking sufficient health infrastructure, unconditional transfers may be the only realistic alternative. Fortunately, such programmes too can be effective: Duflo (2003) has shown that such transfers have improved child height for age in South Africa.

More ominously, designers of conditional transfer programmes may be imposing burdens on households that limit the efficacy of the transfers. For example, de Janvry et al.(2006) show that sending children to work is a strategy used by some poor households to cope with negative shocks, and if households are not permitted to cushion shocks in this way, there could be negative repercussions for at least some household members. Gitter, Manley, and Barham (2010) find that limiting the household's use of child labour to cushion shocks may have inhibited child development of younger siblings as measured by height for age in households participating in a Nicaraguan CCT. Finally, misunderstanding conditionalities can have adverse implications that may undercut a program's effectiveness. Gaarder, Glassman and Todd (2010) describe cases where this happened in Honduras, Turkey and Brazil.

To maximise the efficacy and efficiency of investments in child development, aid organizations need to know the factors conditioning cash transfers' success or failure in improving the nutritional status of children. The importance of conditionality is a main "knowledge gap" as in most cases the effects of conditionalities cannot be separated from the effects of the rest of the intervention.

This paper seeks to fill that gap, at least with respect to child nutritional status. Below, we justify the choice of height for age as the main outcome of interest and consider the various pathways through which cash transfers may affect anthropometric outcomes. We have compiled a list of programmes whose impacts on children's height for age have been studied and we construct a list of those programmes' characteristics. Our analysis tests links between programme effectiveness and those characteristics using a mix of statistical procedures described below.

Grouping a number of studies in this way and looking for trends is an important first step in identifying the factors that affect a programme's success. We abstract from some programme details and on occasion are forced to group programmes that are somewhat dissimilar in hopes of teasing out which factors are in fact of central importance. Because of this we are unable to make specific diagnoses of the problems affecting particular programmes. Qualitative investigation of a single programme would be much more effective for that purpose.

However, by identifying the characteristics of programmes which have had a significant positive impact on child nutritional status, we are able to offer suggestions to policymakers crafting interventions designed to improve nutritional status as cost effectively as possible.

1.2 Definitional and conceptual issues

1.2.1 Nutritional Status

Though social assistance programmes seek to achieve a number of ends, nutritional status is an indicator of paramount significance. It is a crucial, summary measure of overall child health and development potential: "Stunting or chronic malnutrition is … a strong indicator of a broad number of factors leading to child mortality" (Yablonski and O'Donnell 2009). Black et al.(2008) ascribe to undernutrition as many as 3.5 million deaths and 35% of the disease burden in children under 5 years. Effects also extend to other sectors of society:

> Under-nutrition in turn has negative effects on income and on economic growth. Under-nutrition leads to increased mortality and morbidity, which lead to loss of economic output and increased spending on health. Poor nutrition means that individuals are less productive (due to both physical and mental impairment), and that children benefit less from education…. Achieving goals in primary education, reducing child mortality, improving maternal health, and combating HIV/AIDS, malaria, and other diseases all depend crucially on nutrition.(Horton, Alderman et al. 2009).

"Malnutrition's economic costs are substantial: productivity losses to individuals are estimated at more than 10 per cent of lifetime earnings, and gross domestic product (GDP) lost to malnutrition runs as high as 2 or 3 per cent" (Shekar, Heaver et al. 2006).

Many of the programmes we consider specifically list improving food consumption and thereby nutritional status as an outcome of interest(Barrientos and Nino-Zarazua 2010). The Mexican CCT PROGRESA (now called Oportunidades) explicitly aimed to improve the nutritional status of poor children (Behrman and Hoddinott 2005). The Nicaraguan *Red de Protección Social* listed increasing the health and nutritional status of children under 5 as an objective (Maluccio 2009). Malawi's Mchinji Social Cash Transfer Pilot Scheme was designed in part to reduce malnutrition as well (Miller, Tsoka et al. 2008).

1.2.1.1 Height for age

The primary outcome of interest in this paper is height for age, a main indicator of nutritional status. Growth patterns of children under age 5 are similar for all ethnic groups (WHO 1995, WHO 2006) and growth charts allow the conversion of child height into z-scores based on observed means and standard deviations for children of a given age and sex based on a reference population. These height for age z-scores reflect long-term health. While adverse health events are known to affect growth in the short term, most people are able to recover once they return to health (Tanner 1986). However, repeated insult such as frequent illness or malnutrition may limit the capacity for "catch-up growth" (Tanner 1986) leading to diminished height. Growth is affected by diet, physical activity, and health status (Johnston 1986). Data from Gambia to Guatemala have shown that height deficits are established early in life and often persist into adulthood (Coly, Milet et al. 2006).

Height for age is also one of the most comprehensive and widely used measures of overall long-term health. Height for age is often described as an indicator of long-term nutritional status among children (Waterlow, Buzina et al. 1977; Strauss and Thomas 1998). It is also an indicator of a child's underlying health status, and children showing lower levels of physical development for their age are often delayed mentally as well (Hoddinott and Kinsey 2001; Grantham-McGregor, Cheung et al. 2007). Many studies have evaluated children's growth with reference to such a standardised population in order to estimate the health effects of natural disasters and various policy interventions, (see e.g. (Hoddinott and Kinsey 2001; Balk, Storeygard et al. 2005; Goncalves-Silva, Valente et al. 2005). After evaluating a number of measures, one study concludes "Height for age at 2 years was the best predictor of human capital…." (Victora, Adair et al. 2008).

1.2.1.2 Weight for age

Another measure of nutritional status is weight for age. Weight for age indicates short-term nutritional status, where height for age reflects the longer term. It is useful in evaluating undernutrition, though not as predictive of human capital as height for age (Victora, Adair et al. 2008).

Weight for age and weight for height are also widely used but reflect only short term nutritional status. Also, unlike height for age, they are relatively easy to influence if beneficiaries perceive that programme qualification may be tied to having underweight. (Morris et al.'s (2004b) study suggests that this may have been the case in Brazil.) On the other hand, with short-term interventions such as emergency cash transfers, weight for age is more appropriate than height for age.

1.2.2 Types of Conditionalities

In section 1.1.3 we reviewed the arguments around the importance of conditionality in cash transfer programmes. Here we clarify one further distinction worth noting. One type of conditional cash transfer programmes requires participants to make investments in developing human capital, usually by getting regular check-ups at a health clinic and/ or sending children to school. Other programmes require participants to make different types of investments, such as meeting savings requirements or work programmes.

1.2.2.1 Human Capital-related Conditionalities

Mexico's PROGRESA led the way for this group of programmes by intervening simultaneously in health, education and nutrition (Skoufias 2005). Since these characteristics are mutually reinforcing, the hope is that investing in all three at once will yield greater benefits for the poor themselves and for society as a whole. Other similar programmes followed later, including Brazil's Bolsa Alimentação, Colombia's Familias en Acción, Honduras' Programa de Asignación Familiar, and Nicaragua's Red de Protección Social.

1.2.2.2 Non- Human Capital Conditionalities

Three programmes in Bangladesh, Food for Asset Creation, Food Security Vulnerable Group Development Programme, and the Rural Maintenance Plan, required recipients to work. The first also required households to meet savings goals (Ahmed et al. 2009). Sri Lanka's Samurdhi programme likewise has a labour requirement. Ecuador's Bono de Desarollo Humano programme is something of a hybrid, as it had no official conditionality associated with it but many respondents assumed it did and acted accordingly (Bouillon and Tejerina 2007).

1.3 Policy and research background

Since data collection for evaluation purposes has been incorporated into many CCT programs, there is abundant data that has been analysed by a variety of researchers. Fiszbein & Schady (2009) point out that just one CCT, the Mexican programme Oportunidades, has been the basis for "hundreds of papers." Other reviews of the literature, such as Glassman, Todd, and Gaarder (2007) and Rawlings and Rubio (2003) include more citations on the topic of CCTs in general.

Several papers have reviewed the evidence across multiple programmes, evaluating impacts across CCT's. None have focused exclusively on nutritional status, none have used meta-analysis, and few have included unconditional cash transfers. With respect to nutritional status, no study has looked at more than five programmes, and none have looked outside of Latin America. We more than triple that total, in part by looking in other regions and including unconditional programmes.

Our paper is designed to address why and how cash transfer programmes have had differential effects on nutritional status rather than to make broader claims about the links between these programmes and other outcomes. With our narrower scope, basis, we are able to a) find more previously disseminated research on this precise topic than previous efforts, and b) quantitatively evaluate the correlations between various programme characteristics and this important outcome.

The below systematic reviews contribute to the theoretical basis of this paper. They highlight some of the factors that intervene between cash transfers and efficacy on nutritional status.

1.3.1 Fiszbein and Schady (2009)

Fiszbein and Schady's 383 page book summarizing the experiences of CCTs around the world begins with a review of the economic rationale behind CCTs and then describes the design and implementation process. They devote two subsequent chapters to surveying the evidence of programme effects on a variety of outcomes, including the use of education and health services, infant mortality, and the physical and cognitive development of children (including nutritional status). For example, they list the assessed effects of 13 programmes on school enrolment and attendance (p. 128-9). They do not describe a systematic search mechanism but they do cover a large amount of ground. They observe that CCTs are effective at reaching some goals but are no panacea; for example CCTs may be associated with increasing health care utilization, but that does not always translate into improved nutritional status. Their summary states that CCTs are likely more effective when combined with improvements in the quality of the supply of health, education, and other supporting services.

1.3.2 Gaarder, Glassman and Todd (2010) (and their earlier work, Glassman, Gaarder, and Todd (2006) and Glassman, Todd, and Gaarder (2007))

The most recent iteration of this work is the most systematic of the reviews that we have seen. They describe their work as meeting the Campbell Collaboration's criteria for a systematic review with two exceptions: they do not always have two trained reviewers examine each article, and they do not systematically survey the grey literature. They do systematically comb through the literature, with two people collecting and calculating the impact estimates reported in their study. They list a clear set of search terms and use ten databases, ultimately collecting and summarizing 41 studies linked to 11 programmes/ interventions (p. 7) looking at a variety of outcomes including clinic visits, DPT immunization, full immunization, and nutritional status (just five interventions, all CCTs, are described in terms of impacts on nutritional status). In attempting to isolate the

pathways through which CCTs are effective, they lay out a number of assumptions implicit to the CCT intervention, discussed in 1.2.2 above. They conclude that the mixed results on the nutritional status outcome may indicate supply side problems or problems with unanticipated pathways from programme to observed outcomes, such as the role of men in purchasing food (since only women are targeted for education) and the effects of transfers on mental health.

1.3.3 Hoddinott(2010), Hoddinott and Bassett (2008), and Bassett(2008)

These papers evaluate only Latin American CCTs. The first two look at four programs, while the last adds a fifth. The first two begin with information on stunting and micronutrient deficiencies across Latin America and the Caribbean. They note that CCTs seek to improve outcomes in three ways: through the provision of financial resources to households, through educating mothers, and via food supplements provided directly to children. They describe the details of implementing the programmes and the evaluations. As far as results, they conclude that PROGRESA shows an apparent positive treatment effect on child height for age. Nicaragua's RPS shows stronger effects, while Honduras' and Brazil's programmes show none. They also evaluate effects on anaemia, finding that only in Mexico did the CCT achieve any alleviation of iron deficiency. They call attention to the difficulty of identifying pathways through which the programmes are effective, and note that the task is important.

Bassett (2008) considers ways of improving CCT effectiveness as far as nutritional outcomes. She notes that good nutrition is achieved through a combination of assuring micronutrient intake (including iron, vitamin A, and iodine), exclusive breastfeeding for 6 months followed by appropriate complementary feeding up to the 24th month of age, and appropriate nutritional care of sick and malnourished children. Assuring good nutrition can be achieved through education, provision of supplements, and a reduction of the disease burden, and CCTs are in a good position to provide these services. She next reviews the specifics of five CCTs and concludes that although disentangling the causative mechanisms is difficult, it appears that larger transfers are more effective, the quality of the supply side factors is important, and education in smaller groups as well as frequent supplement distribution help improve outcomes. Best practices include growth monitoring and promotion (education), nutrition education, micronutrient supplementation, and delayed umbilical cord clamping. She notes that focusing CCTs on nutritional issues may require a shift in strategy particularly related to targeting, conditionality, and provision of services. Some new CCTs in Panama, Peru, and Bolivia include a nutritional component larger than past programmes.

1.3.4 Lagarde, Haines, and Palmer (2007)

This paper, titled a "systematic review," is based upon a search of 23 databases using the following terms, their combinations, or both- "cash transfer," "conditional cash transfer," "monetary incentive," "social protection," "safety nets," "health services," "health," and "demand" (p. 1901). Articles were accepted if they utilized one of five study designs. Two qualified people independently carried out the search and evaluated the study quality using standards based on those provided by the Cochrane collaboration. Ultimately over 24000 articles were examined.

The paper examines the link between two inputs, CCTs and health services, and outcomes including anthropometrics. They find ten articles on six studies which they summarise in a series of informative tables. Like others they conclude that indications are positive but that barriers to successfully improving outcomes should

be clarified and pathways to improved effectiveness disentangled through further research.

1.3.5 Leroy, Ruel, and Verhofstadt (2009)

These authors review the evidence on nutritional outcomes in CCTs, searching EconLit and Index Medicus as well as the websites of organisations such as the World Bank, the Institute for Fiscal Studies, and the International Food Policy Research Institute using "conditional cash transfer" as their search term. Finally, they also reviewed five previous synthesis papers, including an early version of Gaarder, Glassman, and Todd and Lagarde, Haines, and Palmer (2007). They do not specify whether the search involved multiple reviewers.

Ultimately the authors summarize findings from Brazil, Colombia, Honduras, Mexico, and Nicaragua (the same five programmes evaluated in Gaarder, Glassman, and Todd (2010)). They find that impact evaluations looking at micronutrient status found a variety of problems with the interventions and identified only limited impacts on iron status and anaemia. With respect to anthropometry, they find effects in Mexico, Nicaragua, and Colombia, but none in Honduras or Brazil. They find an association between larger transfer sizes and greater effects, and also a tendency for younger age groups to benefit more. They also investigate some possible pathways through which the CCTs may have affected nutritional status, noting that all CCTs for which information was available were effective in increasing use of health care services and decreasing child illness. The article features a useful diagram showing the links between transfers and child health. They conclude that CCTs address the underlying causes of poor nutritional status, but note that to maximise programme impact, attention must also be paid to the immediate causes such as provision of adequate micronutrients and alleviation of the burden of childhood illnesses such as diarrhoea.

1.3.6 Bouillon and Tejerina (2007)

In a report called a "systematic review" for the Inter-American Development Bank, the authors began by snowballing references from five previous synthetic summaries that each examined the effectiveness of a given type of intervention. They also looked through databases of interventions kept by the World Bank, one kept by the Chilean Ministerio de Hacienda, and databases kept by the ILO, IFPRI, the Office of Evaluation and Oversight at the Inter-American Development Bank, and the q-squared initiative website. They sought evaluations that were carried out in Latin America using a control group and implementing one of a set of four statistical methodologies. Although they refer to their work as a systematic review, it is not clear whether two qualified personnel performed each search separately.

These authors review impact analyses for 82 social programmes in 15 Latin American countries. They look at many types of schemes ranging from early childhood nutrition interventions to social investment funds, youth training, and water and sanitation improvements. They consider also a variety of outcomes from health to education and child labour. In Latin America, they conclude, conditionality is important; it affects the behaviour of beneficiaries, though of course the value of a conditionality is contingent upon the quality of the institution into which people are brought. To this end, multi-sector interventions addressing both supply-side and demand-side factors "seem to have greater impact and contribute to the sustainability of benefits after the programme has been completed" (p. 96).

1.3.7 Barrientos and Nino-Zarazua (2010)

Summarizing the state of the literature for the ILO, this paper extols the virtues of cash transfers. According to this work, transfers are an inexpensive solution to problems including poor nutrition, poor general health, child labour, unemployment and underemployment, and inequality. Transfers increase education, raise and protect consumption levels, inspire social cohesion, and leave men and women alike feeling empowered.

1.3.8 Yablonski and O'Donnell (2009)

This review done for Save the Children UK looks at the effectiveness of cash transfers in improving child mortality rates. They conclude that transfers are an inexpensive and effective component of efforts to decrease such rates. Transfers are shown to work by improving sanitation conditions, health care access, and food quantity and quality. Like Bouillon and Tejerina (2007), they note that transfers work best when combined with supply-side policies such as improving health care. Their coverage is not as broad but it is useful in calling attention to the size of transfers and to the centrality of supporting pregnant women and children under age 2. Also, they sketch an informative diagram of the means by which transfers affect child health.

The various systematic reviews cover different issues and enable us to assemble lists of potential proximate and distal causes and of interventions. Below we combine their various contributions into an overarching theory from which we derive testable hypotheses. The next section reviews some of the important conclusions reached by previous analyses and sets up hypotheses that we take to the data below.

1.4 Theories linking CCTs to nutritional status

Nutritional status, including height for age and weight for age, directly depends on two factors: sufficient nutrition and the body's ability to absorb it (Agüero, Carter et al. 2007). In other words, what matters are the quantity and quality of food coupled with the health status of the person consuming it. Numerous underlying factors are behind these two directly relevant factors. In the next subsection we summarise some of the literature on the topic, and then we outline the methodological approach of our study.

1.4.1 CCT-nutritional status links in the literature

Previous works have described pathways linking CCTs to health. Diagrams in Leroy, Ruel, and Verhofstadt (2009)and Gaarder, Glassman, and Todd (2010)show the relationship by means of a total of 26 mediating factors including education, feeding care and practices, supply of health services, and cash.

Fiszbein and Schady (2009) note that consumption increases are a commonly observed outcome of CCTs, but they also note that increases in nutritional status are not as common. A variety of potential proximate and ultimate barriers may play roles. Nutritionists note that in addition to needs for calories and protein, a variety of micronutrients including iron, zinc, and vitamin A can constrain growth, as can frequent infections (Rivera, Hotz et al. 2003).

In their meta-analysis, Charmarbagwala et al. (2004) note that although income and/ or consumption expenditures are strong predictors of nutritional status, the final determination of nutritional status also depends on child and household characteristics such as child age and maternal education and supply side issues

such as local water quality and the availability and quality of health services. Thus, we must track whether programme evaluations consider these potential mediators of the income/ nutritional status relationship.

Bassett (2008) draws attention to some specific factors that may limit CCT effectiveness as regards height for age. She notes that:

> many critical behaviour changes that lead to sustainable improvements in nutritional status – such as exclusive breastfeeding, appropriate pregnancy rest, or hand-washing after defecation- are intimate, complex, and difficult to change, and therefore CCTs are not set up to address these behaviours directly. (p. 9)

Hoddinott and Bassett (2008) advocate enhancing CCTs by adding counselling on improved hygiene and sanitation and the provision of nutrient supplements for pregnant women and young children.

In an earlier review, Gaarder, Glassman and Todd (2010) identify some assumptions undergirding CCT effectiveness. They include:

- CCTs improve health by increasing utilization of health services, particularly preventive health services
- Cash improves health by ensuring service utilization and improving food consumption
- Providing information to poor women will induce behavioural changes
- Conditionality is key to increasing service utilization
- Some programmes also add a food supplement
- Poor health status is attributable to demand side factors; supply is sufficient or will increase to meet an increased quantity of demanded care
- The outcomes evaluated are relevant

One input worth highlighting is the importance of care practices. As illness inhibits the body's acquisition of nutrition, access to health care and the quality of available health care play an important role in keeping children on track. Cash transfer programmes that improve food consumption may not be effective in locales lacking accessible, quality health care.

Finally, some heterogeneity in reported outcomes may stem from different study techniques. Lagarde, Haines, and Palmer (2007) report that five different studies of the same data (from Mexico's PROGRESA/ *Oportunidades* program) reported different analyses and results and failed to cite each other. They note that unplanned subgroup analyses of trials can lead to spurious conclusions.

1.4.2 Model of the CCT-nutritional status relationship

Figure 1.1 depicts the factors affecting child nutritional status. Household wealth is subject to a variety of demands, only two of which are food and health care for family members. We use the economic concept of an "efficient input" to characterise household resources. (Just as only a limited share of water applied to a field reaches the roots of the plants for which it is intended, only a limited share of household resources can be spent on inputs that lead to improvements in nutritional status.) Relevant inputs include health care utilization, food quantity, and food quality. Effective food is that portion of household consumption that contributes to child growth, and in addition to the inputs noted it is modified by the size and demographic characteristics of the household in which the child

receives the food. Health care utilization, modified by health care quality, helps prevent child illness. Illness comes about in part due to the local disease environment, though that too is modified by any number of health-related behaviours which can range from smoking in the household to choices about water, sanitation, and breastfeeding. Effective food combines with child illness in the context of child-specific characteristics such as age and maternal height to produce nutritional status. Thus, care-givers' influence is included two ways: as health-related behaviours and as food distribution. Finally, the nutritional status observed in data and the outcomes analysed in research are themselves subject to any number of biases from measurement bias to study characteristics. We denote this bias by the difference between "measured nutritional status" and "nutritional status."

Figure 1.1 Origins of Child Nutritional Status

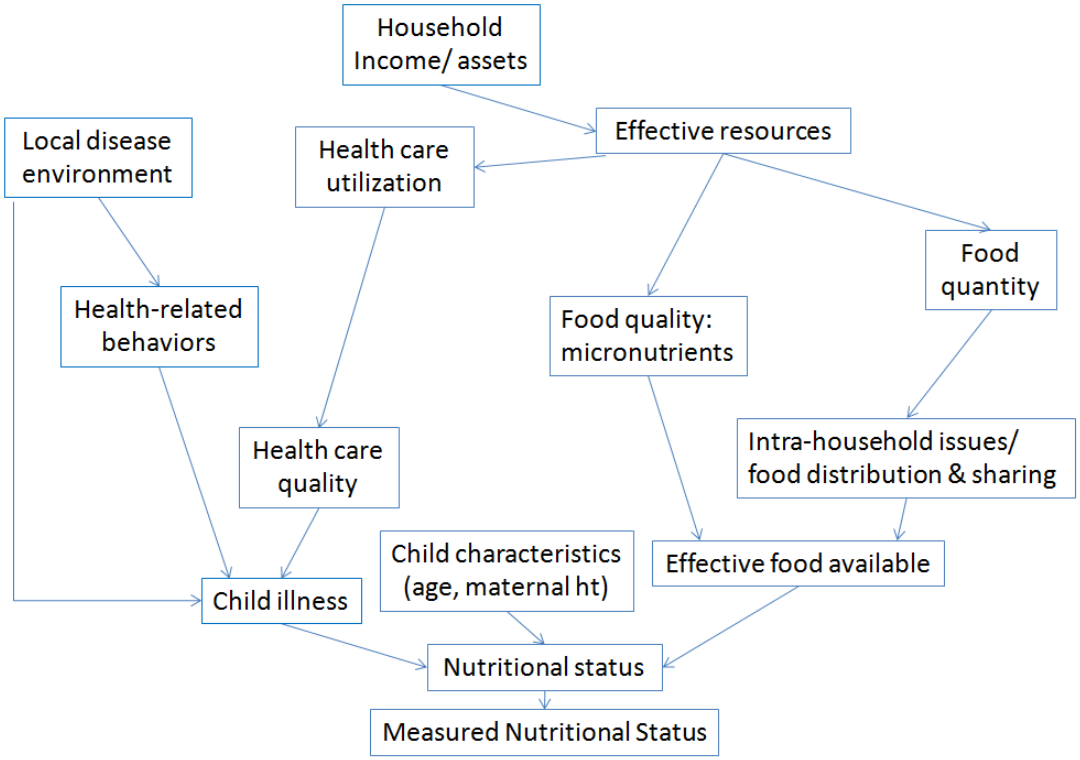
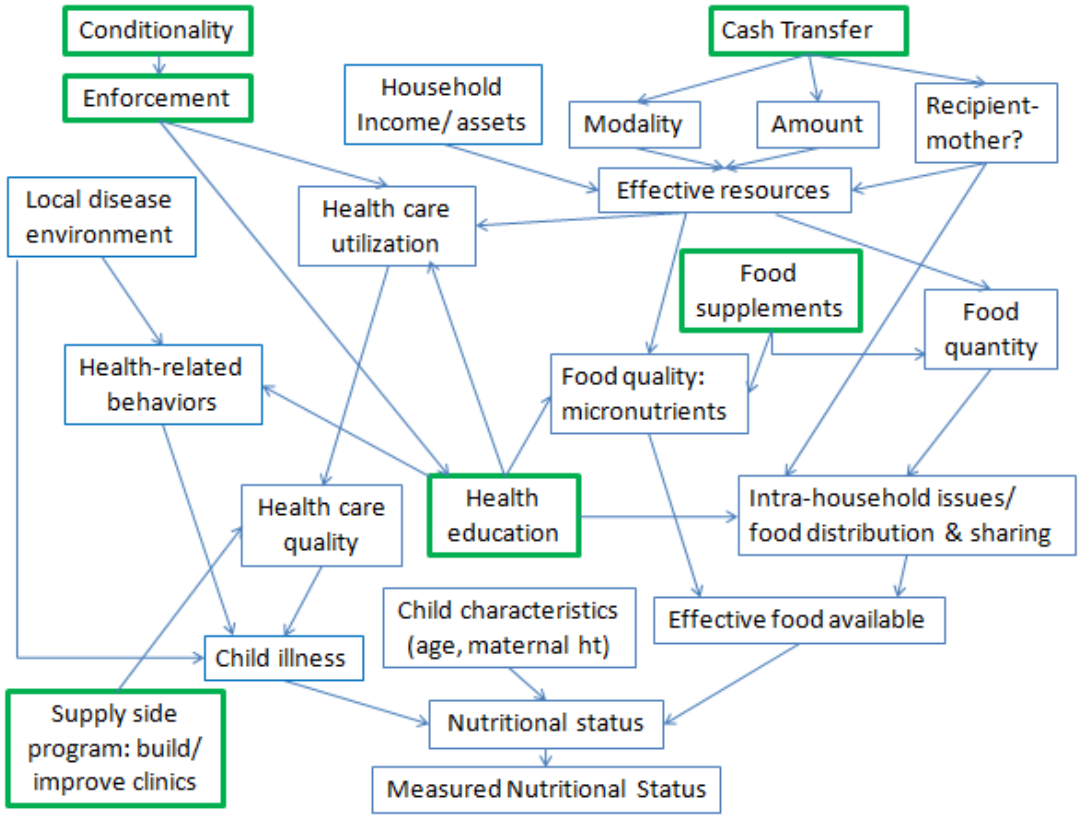


Figure 1.2 expands Figure 1.1 to show the roles of several potential interventions. First, child nutritional status might be improved by providing households with cash transfers. This transfer may become effective resources depending on the amount, modality of the transfer, and which household member receives the transfer. Conditionality, depending on enforcement, can have a few effects. It can contribute to nutritional status by increasing utilization of health care, or it can do so via health education. Health care utilization is effective if the quality is there. Health education can contribute to improved child nutritional status by improving health-related behaviours, whether hand-washing, breastfeeding, or choices about fuel use, drinking water, or sanitation. Health education can also affect children's food consumption by improving food quality or quantity. Bonvecchio et al. (2007) show that education improves the effectiveness of a nutritional supplement in the context of a CCT in part by decreasing consumption of the supplement by other family members. The final means of improving nutritional status is the provision of food supplements. Bhutta et al. (2008) find that in food secure areas, education on

complementary feeding improves height for age scores by 0.25 while among food insecure populations, supplements increase height for age by 0.41.

Figure 1.2 Origins of Child Nutritional Status and Potential Interventions



1.5 Objectives

To answer these questions we scan the literature and perform meta-analysis using appropriate quantitative means including meta-regression analysis. We gather reports of impact (and a large set of covariates) from the programme evaluation literature and use regression analysis to identify correlations. A list of relevant hypotheses is below.

1) Do community characteristics (such as supply side service availability and quality, food availability and quality, and water availability and quality) constrain programme effectiveness? Is it possible to identify which characteristics are the most relevant?
2) Do demographic covariates such as household size and child age constrain effectiveness? Are programmes less helpful to large families? Do they help younger and older children equally?
3) Does programme effectiveness in encouraging use of health care correlate with programme effectiveness in improving child nutritional status?
4) Does programme effectiveness in increasing food diversity or at least in increasing household food consumption expenditures translate to improved child nutritional status?
5) Does conditionality of any kind predict increases in child height for age?

6) If conditionality matters, what types of conditions matter? Does conditioning payments on maternal participation in educational talks correlate with increases in child height for age?
7) Does the provision of food supplements correlate with improved child nutritional status?
8) Does total payment size matter? How about payment timing (i.e. every 2 months, during certain seasons only, etc.) or modality (i.e. cash, bank transfer, etc.). Programmes that pay at a flat rate or which impose a broadly binding constraint on household payments may be less adequate for larger households (Such constraints were deliberately imposed in some cases to disincentivise fertility (Bouillon and Tejerina 2007; Stecklov, Winters et al. 2007)).
9) How do the means of analysis affect the estimated impact?
10) Bassett (2008) concludes that CCTs are a best response to issues of poor nutrition if and only if they can be introduced along with high quality and accessible services AND if such services would be underutilised in the absence of cash incentives and conditionalities (p. 44). Are programmes more successful if they improve access to high quality services?

To analyse the importance of these cofactors, we take data on outcomes from impact analyses and compare it with programme characteristics and characteristics of the impact analyses themselves. We gather information on programme characteristics from the impact analyses as well as from survey articles. Once all of this material is together, we are able to statistically analyse the relationships between estimated effectiveness and programme characteristics as well as between estimated effectiveness and study methods.

2. Methods used in the review

2.1 Approach and user involvement

Our advisory group includes aid providers, systematic review experts, and econometricians from 3ie and DFID. In addition to distributing our final product to development practitioners, we hope to publish some version of this review in a field journal, facilitating access by a greater proportion of the development community, including academia, research-oriented non-governmental organizations such as ODI and IFPRI, and government researchers. We are open to other means of disseminating our work, which would benefit us as well as DFID and 3ie.

We will also contact development professionals in countries with CCT programmes. One of us worked at Mexico's National Institute for Public Health, and has contacts there to whom we will send our finished product. We will also share our work (by sending the report and offering to present) with relevant non-governmental organizations such as Save the Children. Finally, we will endeavour to identify and contact relevant agencies in each country implementing a CCT to share with them the results of our inquiry once it becomes available.

2.2 Identifying and describing studies

2.2.1 Defining relevant studies: inclusion and exclusion criteria

We gathered data by searching for all existing studies which examine the impact of a cash transfer programme on child anthropometric outcomes. In particular, included studies had to report authors' original estimates of the impact of an intervention, at least one component of which had to be a direct cash transfer to selected beneficiaries. In addition, the relevant studies had to focus on the programme effect on anthropometric outcomes, such as height for age and weight for age.

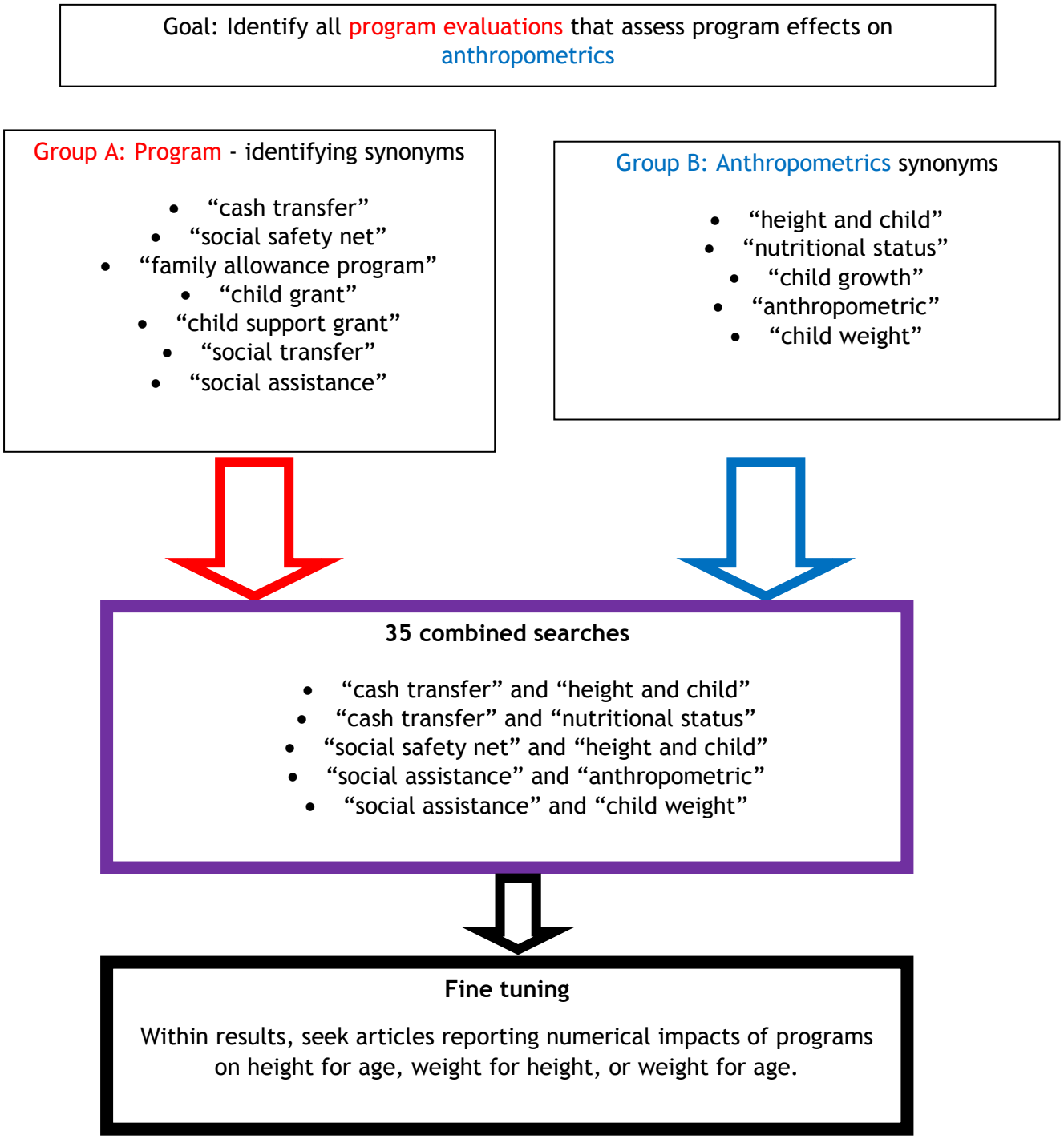
No studies were excluded on the basis of how the beneficiaries were selected, i.e., whether beneficiaries were randomly chosen or not (We did control for study quality later—see section 2.3.1 below). Articles using all study identification strategies, whether experimental design, cross sections, difference-in-difference, prospective trials with matched groups, propensity score matching, regression discontinuity, or even simple interrupted time series methods could be included in the current analysis. In other words, the inclusion criteria had two main components: I) the relevant study had to be an impact evaluation of programme with a cash transfer component, and ii) it had to contain original numerical impact estimates on children's height for age and/ or weight for age.

A clear outline of the search process for articles which report original impact estimates of cash transfer programmes on children's anthropometric outcomes follows below. The search of all the sources was carried out independently by two reviewers. The findings of the two were then carefully compared, and in the case of discrepancies with respect to the located number of articles, the search was carried out again in order to establish the cause of the variation.

Table 2.1 Examples of Excluded Articles with Reason for Exclusion

Excluded article	**Reason for exclusion:**
de Janvry et al. (2006)	does not use height for age or height in cm as a dependent variable of interest, although it may report on cash transfer programmes
Goncalves-Silva et al. (2005)	examines height for age or height in cm but not in the context of a cash transfer programme
Strauss and Thomas (1998)	surveys the literature and does not provide original impact estimates
Stifel and Alderman (2006)	evaluates the impact of an in-kind programme such as Vaso de Leche in Peru on height for age or height in cm
Behrman, Cheng and Todd (2004), Attanasio and Vera-Hernandez (2004)	evaluates the impact of a nursery programme such as Proyecto Integral de Desarrollo Infantil (PIDI) in Bolivia and Hogares Comunitarios de Bienestar Familiar in Colombia: no cash transfer component
Hoddinott and Skoufias (2004)	evaluates the impact of a cash transfer intervention on education, consumption and/or health but not specifically on height for age or height in cm
Fernald, Gertler and Hou (2008)	evaluates the impact of a cash transfer programme on adult anthropometric status rather than on children

Figure 2.1 Search strategy for research databases



2.2.2 Identification of potential studies: Search strategy

The goal of the search was to identify analyses of cash transfer programmes that included anthropometric information. Our search started with an examination of the sources listed in the protocol of the present study. In addition, the references of four systematic review papers on the effect of various cash or/and in-kind transfer programmes on health outcomes (Gaarder et al. 2010, Leroy et al. 2009, Lagarde et al. 2007, and Bouillon et al. 2007) were screened and "snowballed," meaning that in addition to looking up cited articles, we searched the cited articles' references, and those references' references, and so on as long as the names of referenced articles seemed appropriate. A total of 410 references (including repetitions) were examined which resulted in 19 different studies containing original impact estimates of either conditional or unconditional cash transfer programmes on children's height for age or height in centimetres.

The next step was to conduct an extensive search of the following bibliographic databases: EconLit, PsycInfo, PubMed, Google Scholar, Eldis (and ID21, which has merged with it), Inter-Science, Science Direct, Medline, IDEAS (REPEC), the Cochrane Central Register of Controlled Trials, the Database of Abstracts of Reviews of Effectiveness, JOLIS, POPLINE, CAB Direct, Ovid.com (AKA Healthcare Management Information Consortium and FRANCIS), WHOLIS (World Health Organization Library Database), British Library for Development Studies (BLDS), Journal Storage (JSTOR), Latin American and Caribbean Health Sciences Literature (LILACS), MEDCARIB, Virtual Library in Health (ADOLEC), Pan American Health Organization (PAHO), the Social Science Research Network (SSRN), Social Sciences Citation Index plus Conference Proceedings Citation Index, ProQuest Dissertations & Theses Database, the System for Information on Gray Literature in Europe (SIGLE), the ntis.gov search engine of U.S. Government documents, and the Effective Practice and Organization of Care Group (EPOC) Register. Although library shelves were not hand searched for books, Worldcat.org was explored for information on books to complement our searches for articles.

Each of our searches combined a term that referred to the programme with another that referred to anthropometrics. Figure 2.1 depicts our final search strategy. In the left block are 7 terms used to identify interventions: "cash transfer," "social safety net", "family allowance program", "child grant", "child support grant", "social transfer" or "social assistance." The right box lists 5 alternative terms which denote anthropometrics: "height and child", "nutritional status", "child growth", "anthropometric", or "weight and child".[2] Matching terms on the left side with those on the right results in 35 combinations of phrases, which were used for the search of each database. The complete set of search terms is in Appendix 2.2. The search was limited to citations dated after 1990 only when the original stipulation yielded more than 1000 references. No other restrictions were imposed. The full search was carried out between July 29, 2010 and September 2, 2010, on dates listed in Appendix 2.1. All searches were carried out on a computer running Windows Vista and Google's Chrome browser.

An example of the complete search of one database, EconLit, may better illustrate the process. The first search was "'cash transfer' and 'height and child'".(The text typed into the search engine was just so: ***"cash transfer" and "height and child"***, resulting in a Boolean search based on those two compound terms.) We did not use any wildcard markers such as *. It yields two references which happened to be two

[2] Only in the search of Google Scholar was "child weight" substituted for the Boolean combination "weight and child" due to the unmanageably huge number of citations procured by the latter phrase.

versions of the same article – Duflo (2000) and Duflo (2003). In such a case only the newest version was considered. This article was relevant and contained original impact estimates but it had been located during the first step of the search process; hence, in the spreadsheet next to "'cash transfer' and 'height and child'" and under EconLit it was recorded that two references were found, both of which were relevant, yet they already had been included. The next search phrase is "'cash transfer' and 'nutritional status'" which resulted in 3 citations, all of which contained numerical estimates, yet they were also already coded. "'Cash transfer' and 'anthropometric'" and "'cash transfer' and 'weight and child'" each resulted in 2 citations, which were also repetitive. "'Cash transfer' and 'child growth'" did not identify any citations. This process was repeated for the other 30 phrases. The full search of EconLit procured a total of 18 citations (with repetitions) 15 of which (including repetitions) were identified as potentially relevant based on title and abstract, yet after removing the recurring citations, only 2 new references were reviewed. The same search process was followed with all bibliographic databases. While the exclusion of the articles that have already been coded is straightforward, the exclusion of the recurring articles which are not directly relevant to this meta-analysis is imperfect. Although careful attempts were made to identify articles which appear multiple times, given the large number of articles surveyed, there were unavoidably still some minimal repetitions.

The results of the search were carefully recorded in separate files by database and term combination. The titles and abstracts were reviewed and potential useful references were noted for further examination of the full document. In addition, a spreadsheet was maintained which summarized the number of references procured by the respective phrase combination, the number of potentially useful references as suggested by the title and abstract, and their record number in the database so that they could be easily accessed.

After compiling the results from each of the 35 searches of each search engine, the titles and abstracts were screened to see if the article cited was likely to contain numerical anthropometric information on the impact of a cash transfer program. Full reports were obtained for those studies that appeared to meet the criteria or where there was insufficient information in the abstracts. In practice, this became a simple exclusion criterion: "Does the article evaluate a cash transfer programme and does it report numerical anthropometric impacts?" The criteria were re-applied to the full reports and those that failed to meet it were excluded. The data from reports that meet the criteria were compiled initially into an Excel spreadsheet before that became too cumbersome. (The final data structure is based on Appendix 3.1.) Publication bias was addressed by the inclusion of unpublished works and later tested using a funnel plot (see Data Analysis below).

Two of us independently carried out each search, and met regularly to discuss the search process, results, and to assess study eligibility. All searching was tracked, and a unified database of reviewed publications maintained. Titles and abstracts of reviewed publications were imported or entered manually into EndNote.

The next step of the process involved hand searching key journals such as the Journal of Nutrition, IDS Bulletin, Journal of Development Studies, Journal of Development Economics, World Development, the Journal of Development Effectiveness, Economic Development and Cultural Change, Economic Development, and Social Science and Medicine from the present back through 1995 or the earliest available, whichever was later. Since PROGRESA, the first CCT, began in 1997 this should ensure some degree of homogeneity in world economic conditions when considering unconditional programmes as well. (Since Worldcat is more comprehensive than any individual library's collection of books, it is expected

that its search would accomplish a "virtual" bookshelf search.) No new quantitative studies were added to the previous findings. Section 3.1 below summarizes the search findings.

Lastly, around a dozen experts were directly contacted via email for additional suggestions. About half of the experts responded, with many suggesting articles. No new material was uncovered.

2.2.3 Screening studies: applying inclusion and exclusion criteria

Since the goal of the current study is to evaluate systematically the effect of cash transfer programmes on children's health status as measured by height for age or weight for age, the search is also naturally defined in terms of the outcomes height for age or weight for age. Following Charmarbagwala et al. (2004) the primary inclusion criterion was the use of height for age, height in centimetres, or weight for age as an outcome. Yet, once the articles which reported height for age or height in centimetres as a dependent variable were located, they were also coded for other anthropometric outcomes, if they did report any. Papers reporting only impacts on education were not considered. Papers reporting "instrumental" outcomes such as increased use of health care, i.e. programme effects that might have contributed to improved nutritional status, were not directly included in the quantitative analysis, but they were considered in the qualitative discussion and the programme summary if they contributed to the general summation of evidence. Similarly, papers reporting only instrumental nutritional outcomes such as increases in food diversity or calories consumed were excluded from the meta-regression analysis, but were included in the analysis of programme impact beyond anthropometric outcomes.

In addition, the scope of the current study is limited to cash transfer programmes, i.e. programmes of which at least one major component is the provision of cash. Both unconditional cash transfer programmes and conditional cash transfer programmes are examined in order to address the importance of conditionality for intervention effectiveness with respect to children's nutritional status. In-kind transfer programmes, worker training or educational programmes were not considered. Papers providing information about height for age outside the context of some type of social assistance programme were also not included. The data came from impact evaluations, not from summary articles. Examples of excluded papers include de Janvry et al. (2006) which reports on a CCT but does not include height for age; Goncalves-Silva et al. (2005) which reports on height for age but not in the context of a cash transfer program; and Strauss and Thomas (1998) is a survey article rather than an impact evaluation. Table 2.1 provides additional examples of excluded articles.

There were no restrictions with respect to identification strategy used in the papers. Randomized control trials, quasi-experimental designs such as difference-in-difference and propensity score matching, non-experimental single difference studies and instrumental variable methods are all accounted for in the papers from which the data was compiled. Papers with viable estimates were labelled as to whether or not they are published in peer reviewed journals. Additionally, the quality of the studies was evaluated in accordance with the specifications of the Cochrane handbook. See section 2.3.1 below for our quality evaluation.

2.2.4 Characterising included studies

The main data for this study come from impact evaluations of cash transfer programmes on children's height for age or height in centimetres. The reported impact estimates were compiled in an Excel spreadsheet. In addition, we took data

on two sets of factors to help statistically control for impact heterogeneity. First are the specific features of the programme, including the country of intervention, size of the transfer, the type of conditionalities, whether a nutritional supplement was provided, programme duration at time of measurement, and design characteristics of the evaluation sample. Second are the characteristics of the study such as the identification strategy; sample size; sample characteristics such as age, household size, and baseline wealth; the explanatory variable of interest (i.e. transfer funds vs. overall programme participation); and an identifier for whether the study is a peer-reviewed publication or not. The data from the chosen articles were also extracted by two independent reviewers. Any discrepancies in the coded numbers were discussed in person and resolved by examining the conflicting entries from the respective study again. If any other questions or concerns with respect to the data persisted, they were discussed and resolved by the whole team during the approximately weekly meetings.

Since not all of the studies included in the quantitative analysis described the respective interventions in detail, it was necessary to supplement the knowledge of the programmes with respect to implementation, operations and reality specifics through ad hoc searches of additional published studies. Appendix 3.1 is a compilation of the details of each intervention including both information from the impact evaluations and the supplemental literature. This information is used to fill in the gaps on programme characteristics needed to perform the quantitative analysis. For example, the Mexican conditional cash transfer programme PROGRESA/ Oportunidades requires women in transfer-receiving households to attend educational talks, but this information was not included in each impact evaluation. After finding this in one paper, we coded all articles assessing impacts of that programme appropriately as it pertains to this programme characteristic.

2.2.5 Identifying and describing studies: quality assurance process

As two team members independently carried out searches, they checked with each other periodically to reconcile their work. Later, when the searches were done, the same two team members independently extracted data from the accumulated articles and then compared notes on the extraction process. After the search was completed and all the relevant articles which met the inclusion criteria identified, the data from these primary studies were coded. As with the search, the data coding was carried out by two independent researchers. Each used the data coding sheet in Figure 2.2, a checklist of the variables on which data were collected from the primary studies. All the coded data was compared and the differences were resolved primarily through going back to the particular study and comparing the copied results with the original.

As we compiled data from various sources, we kept track of it in a spreadsheet that unfortunately became too cumbersome for us to present in paper form. Appendix 3.1 summarizes the information about each programme evaluated. The information there is from both the impact evaluation studies themselves and secondary studies. Some part of the table was compiled before the completion of the search described earlier. The purpose was to create a table of existing cash transfer interventions around the world in order to get a better idea of their characteristics. In fact, similar tables but for fewer programmes are provided in most known systematic reviews including Gaarder et al. (2010), Handa and Davis (2006), Lagarde, Haines et al. (2007), Hoddinott and Bassett (2008), Fiszbein and Schady (2009), and Leroy, Ruel et al. (2009). Table 3.2A gives an idea about the similarities and differences across the various programmes. In addition, important information from the Appendix is included in the data set compiled from the

primary studies, in cases where the primary studies do not report the relevant data necessary for the quantitative analysis.

Figure 2.2 Data Entry Checklist

- General Information
  - Study code
  - Programme name
  - Country

- Sample characteristics
  - Baseline anthropometric outcome (height for age z-score, weights for age z-score, weight for height z-score, body mass index z-score, or height in cm)
  - Impact of the particular programme
  - Statistical significance of the impact (standard errors, t-statistics, or p-value)
  - Number of observation in the sample or the respective subsamples
  - Whether the sample consisted of all male or female children
  - Mean age in months of the children included in the sample of interest
  - Age range of the children in the sample
  - Mean household monthly income at baseline, and/or total annual income at baseline, and/or per capita income at baseline, and/ or total consumption at baseline
  - Household size

- Programme characteristics
  - Programme conditionality
  - Level of participation in conditionality
  - Transfer recipients' gender
  - Whether the programme had a nutritional component

- Study methodology
  - Baseline year for the survey
  - Study identification strategy
  - Whether the explanatory variable of interest is a binary indicating whether the child is in a treated household or whether the explanatory variable is the cumulative transfer amount.
  - Size of the total transfer and/ or size of monthly transfer
  - Currency of transfer (for exchange rate conversion purposes)
  - Whether the article is peer-viewed or not
  - Programme duration at measurement
  - Whether the programme was randomized
  - Income fixed effects
  - Age fixed effects
  - Additional control variables

2.3 Methods for synthesis

2.3.1 Assessing quality of studies

The Cochrane handbook delineates a number of criteria which can be used to objectively evaluate the quality of a given piece of research. Based in part on these criteria[3], we developed two sets of criteria on which to evaluate each study and implemented a scoring system, shown in Figure 2.3. The first set of ten questions looks at study design, including the randomisation and data collection processes carried out in each intervention. The second part, also ten questions, evaluated the ex post methods used by those analysing the programme impact. For each question, a score of zero, one, or two was given. Two points were assigned for questions which were convincingly addressed in the particular study and were clearly not problematic. One point was given to questions which mention the potential issue but their discussion is not perfectly convincing. Zero points were assessed to questions which were not explicitly addressed in the paper or which clearly pointed out as problematic in the study itself.

Since summarizing study quality using an additive approach may hide problematic details of particular studies, we consider separately several facets of study quality, such as randomisation and peer review, to determine whether these characteristics affected the estimated outcome. In addition, we do consider the summed total quality score, though this method has its shortcomings. The scores were tallied for a maximum score of 40 points. Specifics on how each programme scored follow in section 3.2.6.

2.3.2 Overall approach to and process of synthesis

Our goal was to build a matrix listing the assessed programme impacts along with the numerous covariates identified in chapter one, including programme characteristics, participant characteristics, and study characteristics. Thus, our first step was to compile a set of assessed programme effects and fill in as many covariates as possible from the impact evaluations. Next, we added data on other covariates from the supplemental literature. Finally, when the matrix was as complete as possible, we analysed the resulting data statistically using meta-analysis. We looked for evidence of publication bias using funnel plots, evaluated overall effects with a forest plot, and finally used both pooled estimates and meta-regression analysis to identify covariates significantly affecting programme efficacy and to estimate their marginal effects. This enabled us to address the questions raised above in section 1.5. Details of our data analysis follow in section 4.

We chose to analyse our data statistically. While the relatively small number of subgroups makes clean identification difficult, it also provides a more objective means of identifying trends in the data we do have (Mann 1990). We first use a forest plot to evaluate whether an overall trend toward effectiveness is apparent, and conclude that considerable heterogeneity exists, and that considering a variety of covariates is appropriate. Thus, we chose a set of covariates from our theory to match with observed anthropometric outcomes, and we collected data on as many of these as we could find. Since we ended up with 113 data points from 17 studies on our most frequently reported outcome, height for age, we were able to generate both pooled estimates and do meta-regression analysis. This process is reported in detail in section 4.

[3] The Cochrane criteria list others more relevant to medical trials, such as whether treatment/ control status was adequately concealed. It is unlikely that payment information could be secret, particularly in the context of a rural community in which many CCTs are implemented.

Figure 2.3 Quality Evaluation Instrument

Impact Evaluation Quality Checklist

2 points per criterion: 0 = problem; 1 = unclear; 2 = paper addresses adequately/ no problem

IQ	Pts (of 2)
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	

Study Design

1. Is allocation clearly random, i.e. does paper mention how the choice of treatment and control group was carried out?
2. Do participants know which group they'll be in before they're randomized? Surveys tip people off unless people know that they may be involved as controls.
3. Is data collection process laid out clearly?
4. Do treatments and controls match on covariates at baseline?
5. Do treatments and controls match in terms of outcomes at baseline?
6. Is there any possibility of self-selection bias? In other words, do only the go-getters participate, or does everyone offered the programme take up?
7. Is there any possibility that treatments spill over to the comparison group?
8. Are there other interventions going on simultaneously that might mess things up?
9. Are other confounding variables obvious?
10. Was knowledge of the designated outcomes prevented during the study or are outcomes objective and difficult to manipulate?

SQ	Pts (of 2)
1	
2a	
2b	
2c	
3	
4	
5	
6	
7	
8	

Assessment Methods

1. How was the evaluation sample chosen? Is randomization clearly described?
2. Are attrition and exclusion from the evaluation sample addressed?
    a. Are the numbers of people staying and attritting listed?
    b. Are reasons for attrition listed?
    c. Are people who are not in the final evaluation survey similar to those who remain?
3. Are missing observations equally likely among controls and treatments?
4. Do any subgroup analyses test whether they have power to detect the difference in question? (see below)
5. Is there any evidence of selection in terms of outcomes? In other words, do authors report height and not weight, or seem to include anthropometry at one point and not at another? Is any outcome mentioned in the methods section and not in the results section?
6. Are evaluators independent of funders? Any possible conflicts of interest?
7. Are analysed subgroups so designated prior to the analysis?
8. Is seasonality of the intervention an issue?

3. Search results

3.1 Studies included from searching and screening

For this review, we examined over 30,000 articles, not including journals we hand-searched. From these the set of final usable articles is summarized in Table 3.1. The PRISMA flow diagram (suggested by Moheret al. (2009), but slightly modified in this paper) in Figure 3.1 summarizes step by step the search results from all bibliographic databases as well as the websites of the following organizations: the World Bank, Inter-American Development Bank, the Center for Global Development, the International Food Policy Research Institute, the ILO social transfer impacts database on the ILO GESS website, the DFID's research4development.info website, the Overseas Development Institute's website, and chronicpoverty.org. A total of 16,467 records were identified through database searching, and an additional 13,740 documents were located in the above listed institutional websites. All of these citations were screened by title and abstract to isolate potential empirical studies of cash transfer programmes on children's anthropometric outcomes. 1,477 references appeared relevant to the current study, but after removing the repetitive entries, only 360 studies were left to be reviewed. The full text of all articles was obtained and assessed for eligibility. The search ultimately resulted in the addition of 7 new studies to those identified in the literature snowballing, the first step of the search process.

In the end, 24 papers on 18 programmes in 11 countries were located through this search. 18 of the articles (on 15 programmes in 10 countries) report height for age as an outcome variable. The other six studies use only height in centimetres as a dependent variable. Due to the relative paucity of articles reporting results in terms of height in centimetres, our quantitative analysis focuses on height for age z-scores (HAZ). For this outcome, we have data from 18 studies on 15 programmes in 10 different countries. Table 3.1 summarizes the various studies and interventions by country of implementation. The last column also reports the outcomes analysed in the respective evaluation paper. Besides height for age, there was no other nutritional outcome which appeared in all of the listed studies. We found no papers analysing the use of cash transfers in short-term or emergency settings, which had been our reason for including weight for age.

Table 3.2A lists all the programmes with some of the covariates on which we collected data. Most studies do not report information on all of the listed variables. Some additional papers, not listed, were retained in the interest of obtaining more qualitative information on cash transfer programmes, such as their impact on non-anthropometric outcomes like food consumption, food diversity, and health care utilization. Even after adding information from this supplemental search, we were still unable to find all of the information on covariates we had sought. This further restricts the number of observations for the covariate analysis. For example, while most studies reported the number of observations of the whole sample, not all reported the number of observations for subsamples for which a programme impact was estimated. Baseline income was probably the least often reported variable in all of the studies. Even when some measure of baseline earnings was included for the whole sample, it was hardly ever broken down for the various subgroups in the cases when additional subgroup analysis was carried out. Another common omission was programme take up rate or participation in the conditionality.

Table 3.1 Data resources by country and study

	Programs	Reference	Outcomes Studied
Bangladesh	FSVGD	Ahmed et al. (2009)	HAZ, WAZ, WHZ
	FFA	Ahmed et al. (2009)	HAZ, WAZ, WHZ
	RMP	Ahmed et al. (2009)	HAZ, WAZ, WHZ
	Primary Ed. Stipend	Baulch (2010)	HAZ, BMIZ
Brazil	Bolsa Alimentação	Morris et al. (2004)	HAZ, WAZ
Colombia	Familias en Acción	Attanasio, Gomez et al. (2005)	HAZ
		Attanasio, Battistin et al. (2005)	Height in cm
Ecuador	BDH	Paxson and Schady (2007)	HAZ
	Bono Solidario	Leon and Younger (2007)	HAZ, WAZ
India	Apni Beti Apna Dhan	Sinha and Yoong (2009)	HAZ, WAZ
Honduras	PRAF II	Gitter et al. (2010)	HAZ
Malawi	Social Cash Transfer	Miller et al. (2009)	Height in cm
Mexico	PROGRESA/ Oportunidades	Rivera et al. (2004)	Height in cm
		Fernald et al. (2008)	HAZ
		Behrman and Hoddinott (2005)	Height in cm
		Hoddinott and Bassett (2008)	HAZ, WAZ
		Gertler (2004)	Height in cm
		Leroy et al. (2008)	HAZ, WHZ, Height in cm
		Fernald et al. (2009)	HAZ, BMIZ
		Gitter et al. (2010)	HAZ
		Behrman et al. (2008)	HAZ
Nicaragua	RPS	Maluccio and Flores (2005)	HAZ
		Maluccio (2005)	HAZ
		Gitter et al. (2010)	HAZ
	Atención a Crisis	Macours, Schady, et al. (2008)	HAZ, WAZ
South Africa	Old Age Pensions	Duflo (2003)	HAZ, WHZ
		Case (2001)	Height in cm
Sri Lanka	Samurdhi	Himaz (2008)	HAZ, WHZ

Figure 3.1 Prisma Flow Diagram

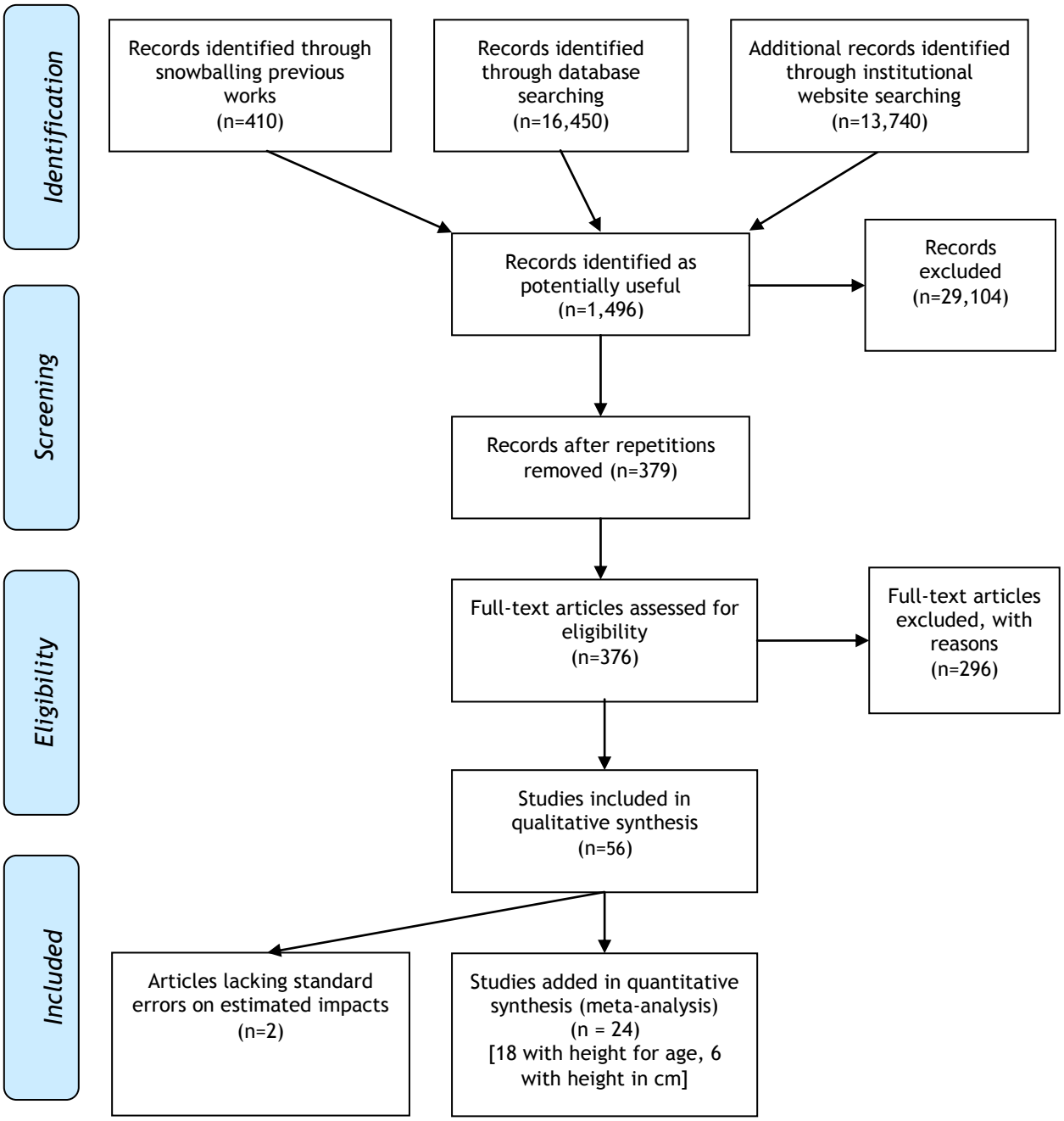


Table 3.2A Programme Listing

Country	Programme Name	Years Evaluated	Nutritional supplement?	Educational condition?	Health care condition?	Enforced?	Amount: % of base income	Recipient
Bangladesh	Food Security Vulnerable Group Development	2004-6	Yes	No	No	No	Unclear	Women
Bangladesh	Food For Asset Creation	2005-6	No	No	No	N/A	Unclear	Household
Bangladesh	Rural Mtnce Program	2003-6	No	No	No	N/A	Unclear	Women
Bangladesh	Primary Educ. Stipend	2002	No	Yes	No	No	Unclear	Women
Brazil	Bolsa Alimentação	2001	No	No	Yes	No	8%	Unclear
Brazil	Bolsa Familia	2003	No	Yes	Yes	No	Unclear	Household
Colombia	Familias en Acción	2002	No	Yes	Yes	No	24-30%	Women
Ecuador	Bono de Desarrollo Humano	2003	No	No	No	N/A	10%	Women
Ecuador	Bono Solidario	1998	Yes	No	No	N/A	11%	Women
India	Apni Beti Apna Dhan	1992-3	No	No*	No	No	Unclear	Women
Honduras	PRAF II	1998-2000	No	Yes	Yes	No	3-10%	Women
Malawi	Malawi Social Cash Transfer	2006	No	No	No	N/A	Unclear	Household
Mexico	PROGRESA/ Oportunidades	1997 – 2007	SP	Yes	Yes	Yes	20-25%	Women
Nicaragua	Red de Protección Social	2000-2	Yes	Yes	Yes	SP	18-20%	Women
Nicaragua	Atención a Crisis	2005-6	Yes	Yes	Yes	SP	15%	Women
South Africa	Old Age Pensions	1993	No	No	No	N/A	250%	Elderly
Sri Lanka	Samurdhi	1999-2000	No	No	No	N/A	25%	Household

Unclear = different sources say different things. (Left blank for quantitative analysis.) SP = some problems: implemented but with noted difficulties. See Appendix for programme details. * An education-conditional portion of this programme has not yet been evaluated.

Table 3.2B Programme Listing and Food Consumption

	Programmes	Impact on (food) consumption	Impact on Food Diversity	Source
Bangladesh	FSVGD	Positive, significant per capita impact on calories & food exp		
	FFA	Positive, significant per capita impact on calories & food exp		
	RMP	Positive, significant per capita impact on calories & food exp		
	Primary Education Stipend	Positive, significant increase in adult equivalent food exp. & kcal	Positive, significant increase in protein consumption	(Baulch 2010)
Brazil	Bolsa Alimentação		Positive, significant impact on number of food items	(Morris, Olinto et al. 2004)
Colombia	Familias en Acción	Positive, significant increase in food& total consumption.	Positive, significant increases in meat, fruit & vegetables, cereals, fats & oils	(Attanasio, Battistin et al. 2005)
Ecuador	BDH	Positive increase in food exp.		in (Black, Allen et al. 2008)
	Bono Solidario	n/m	n/m	
India	Apni Beti Apna Dhan	n/m	n/m	
Honduras	PRAF II	No impact on consumption.	No impact on dietary diversity.	(Maitra, Rammohan et al. 2010), (Bouillon and Tejerina 2007)
Malawi	Social Cash Transfer			
Mexico	PROGRESA/ Oportunidades	Positive, significant effect on total calories	Positive, significant effect on dietary diversity	(Hoddinott, Skoufias et al. 2000; Hoddinott and Skoufias 2004)
Nicaragua	RPS	Positive, significant effect on per capita food expenditures	Positive, significant effect on shares of meat & vegs in diet	Maluccio and Flores (2005)
	Atención a Crisis	Positive, significant effect on per capita food consumption	Positive, significant effect on per capita consumption of meat, fruits & vegs	Macours, K. et al. (2008)
South Africa	Old Age Pensions	n/m	n/m	
	Child Support Grant	n/m	n/m	
Sri Lanka	Samurdhi	n/m	n/m	

n/m: not measured

Table 3.2B summarizes the findings of many such studies on food consumption and food diversity. One positive outcome shown in Table 3.2B is that of the programmes studied in the analysis only one (Honduras' PRAF) was found to have no impact on food consumption and/or variety, while 10 programmes showed positive increases in recipient expenditures on food, calorie consumption, and/ or food diversity.

3.2 Details of included programmes

Our fullest exposition of the characteristics of the various programmes is saved for Appendix 3.1. More concise summaries of the data are in Tables 3.1-3.3. The following describes a few central features of the programmes.

3.2.1 Conditionalities across programmes

The present programme list contains 6 conditional cash transfer programmes with very similar conditionalities – the Mexican PROGRESA/Oportunidades, the Honduran PRAF II, the Nicaraguan Atención a Crisis and Red de Protección Social (RPS), the Colombian Familias en Acción, and the Brazilian Bolsa Alimentação (which is part of the Bolsa Família). All these interventions require regular health and growth monitoring for infants and often pre-school age children as well, prenatal classes for pregnant women and/or attendance of discussions on recommended health practices, hygiene, sanitation, and family planning, and educational stipends for primary and/or secondary grades[4]. Atención a Crisis includes also some rather different components because of the context in which it was created. It was targeted at rural municipalities which had been "affected by a drought the previous year and hav[e] a high prevalence of extreme rural poverty based on the national poverty map" (Macours et al. 2008). Besides the health and education based CCT component, it also included a scholarship for one family member to participate in a vocational training course or a productive investment grant for recipients to start a small non-agricultural business.

In addition to the Latin American CCTs, the sample also contains 5 unconditional cash transfer programmes including Malawi's Social Cash Transfer Scheme (SCTS), the Ecuadoran Bono de Desarrollo Humano (BDH) and Bono Solidario, the South African Old-Age Pensions and Child Support Grant (CSG). (The study of Malawi's SCTS uses only height in cm as an outcome variable, and hence this programme is not included in the main quantitative analysis.) Some of these programmes have undergone significant changes from their original design. For example, BDH was intended as a transfer programme conditional on certain health and educational practices, but this conditionality was never implemented. On the other hand, Bono Solidario started as an unconditional transfer scheme, but was eventually made conditional on prescribed health and education-seeking behaviour. However, all the studies for the present analysis are based on a timeline when the programmes were fully unconditional.

The rest of the programme list is occupied by some rather idiosyncratic interventions such as the Bangladeshi Primary Education Stipend (PES). This programme is aimed at 6-10 year-old children who must attend 85% of classes, yet there are no health-related requirements attached. Probably the most unique intervention is the North Indian Apni Beti Apna Dhan (Our Daughter, Our Wealth) which is designed to encourage better care for girls. Within 15 days of the birth of a daughter, mothers are given a monetary grant (of approximately $11) to cover

[4] Bolsa Alimentação, which was merged into Bolsa Família in 2003, does not include an educational transfer, yet Bolsa Família does.

birth-related fees and expenses; the new-born girl is also entitled to government fixed-deposit securities worth about $55, redeemable for a guaranteed sum of Rs 25,000 (approximately $550) on her 18th birthday provided she is unmarried. Additional monetary incentives aim at encouraging higher educational attainment for girls such as 5,000 Indian rupees for completion of primary education, and 1000 Indian rupees more for completion of secondary education. Other programmes which do not fit neatly in any intervention group are the Bangladeshi Food Security Vulnerable Group Development (FSVGD) programme, the Food for Asset Creation (FFA) and the Rural Maintenance Programme (RMP). The common features of the three programmes are that a fraction of the transferred money must be saved and that beneficiaries have to attend workshops on skill development and awareness raising training. Among the differences are the values of transfers and the compositions of the benefits: both FSVGD and FFA beneficiaries receive half of their transfers in food and half in cash, while RMP participants receive only cash transfers in the form of wages. Also, FSVGD's only conditionality is the saving requirement, while both FFA and RMP include a work component as well. The Sri Lankan Samurdhi includes a cash transfer, a group saving and credit component and a low-budget rural infrastructure development component. It is close to being unconditional but recipients contribute a few "voluntary" days of labour to the community depending on the size of the transfer.

3.2.2 Transfer size across programmes

It is important to note that the amounts of cash transfer vary significantly across programmes from 4 per cent of pre-intervention income level in Honduras to about 250 per cent of pre-programme per capita household income in South Africa. Most of the transfers are in the range of 10 to 25 per cent of initial household income. We lack information on baseline consumption information for most programmes, so we use USD($) conversions if presented in the papers or else dated exchange rates.

3.2.3 Nutritional supplementation

Four of the analysed programmes provide micronutrient supplements, though in many cases their consumption was not strictly enforced. In Mexico, for example, the micronutrient supplement or "papilla" was targeted at pregnant and lactating women, children between 4 months and 2 years, and children between 2 and 5 years if there were any signs of malnutrition (Behrman and Hoddinott 2005: 552). However, these supplements were also given to children residing in the control communities if signs of malnutrition were identified (552). This contamination of the control group may bias downward the impact estimates. Furthermore, the same study reports difficulties in making available adequate quantities of the food supplement (553). In Nicaraguan Atención a Crisis, the allocation of micronutrients was done at the discretion of the health providers, and hence their distribution was endogenously determined. This creates a negative correlation between the consumption of micronutrient supplements and health status and impedes assessment of the importance of supplements on children's health status.

Two programmes in Bangladesh (FSVGD and FFA) include not only cash transfers but also food transfers. Almost all of the food distributed by FSVGD was micronutrient-fortified *atta* (whole-wheat flour), while the food provided by FFA consisted entirely of rice. No discussion is provided on the comparability between micronutrient supplements in powdered form as in Mexico and those added to whole-wheat flour as in Bangladesh.

3.2.4 Programme duration at time of measurement

Information on programme duration at the time of measurement is also collected from the primary studies. Most impact evaluations are conducted 2 to 4 years after the start of the programme or after the baseline survey, but there are evaluations conducted as early as 6 months after the start (such as the evaluations of FFA and Bolsa Alimentação) and as late as 10 years after the intervention inception (PROGRESA). The duration of exposure to programme benefits is expected to influence the impact estimates of height for age or height in centimetres, especially since height for age is a measure of long-term nutritional status.

3.2.5 Pilot versus nationwide interventions

The Mexican CCT Oportunidades is the largest programme of its kind in the world. In fact, many programmes are currently modelled after it. It started in 1997 as a small scale intervention in rural areas but in 2002 it was expanded to cover urban areas as well. In 2004 its budget was $2.2 billion for a coverage of 3.7 million families; in 2007 the budget was expanded to $3.7 billion and more than 5 million families (Fernald, Gertler et al. 2008). Before the start of the programme the Mexican government had committed funds for an external evaluation of the Oportunidades (then PROGRESA). Positive results led to a gradually expansion to include the whole targeted population. Evaluations of the programme performance in both the rural and urban areas are used in this meta-analysis.

Bolsa Alimentação, part of the larger government programme Bolsa Família, is a national-wide intervention with a focus on nutrition. No ex-ante stipulations were made for a programme evaluation; however, a few unintended mistakes lead to the exclusion of a random group of qualifying beneficiaries, enabling the evaluation (Morris, Olinto et al. 2004).

Modelled after Oportunidades, the pilot phase of the Nicaraguan RPS was launched in 2000 with a budget of $11 million. After an external impact evaluation was carried out, at the end of 2002 the government of Nicaragua doubled the budget for RPS and expanded it for another three years (Maluccio and Flores 2005). The evaluation of the pilot phase of RPS is used in the current analysis.

In 2005-06 another pilot programme was launched in Nicaragua, Atención a Crisis. It was designed for extremely poor, drought affected areas. Randomization was built into the design of the programme which enabled evaluation.

Similarly to RPS, the included evaluation of PRAF is from the pilot phase of the program. Due to political considerations, PRAF was implemented very hastily and the original evaluation design was not followed. This compromised the evaluation of the programme and possibly of the intervention as well.

The CCT in Colombia, Familias en Acción, followed the design of PROGRESA and hence was rolled out randomly. Thus, some eligible households received benefits, while others did not and served as a counterfactual.

From the two Ecuadorian programmes, BDH was slowly rolled out which enabled a randomized evaluation, while Bono Solidario was implemented hastily on a nationwide scale; no ex-ante provision for evaluation was made (León and Younger 2007). As many as 1.2 million households, that is, 45 per cent of all households, benefitted from Bono Solidario (León and Younger 2007).

In Bangladesh FSVGD, FFA and RMP were all nation-wide programmes, yet the number of beneficiaries varied with FSVGD having more than 100,000 recipients, while both FFA and RMP covering about 40,000 participants in 2006.

Apni Beti Apna Dhan was implemented only in the Indian state Haryana, which was considered one of the worst with respect to female disadvantage, despite being one of the richest states . The programme was carried out simultaneously in all the districts within the states, which created substantial limitations with respect to identifying a proper counterfactual. Samurdhi in Sri Lanka, the Bangladeshi PES, the South African Old Age Pensions and Child Support Grant are all country-wide programmes, which posed problems with respect to evaluation. In short, the 17 programmes included in the current analysis vary substantially in their scale of implementation. Some included a pilot phase to enable impact evaluations and were expanded after that; some such as PRAF did not live past their pilot phase. Others were simultaneously rolled out country-wide.

3.2.6 Study quality

Nine studies evaluate programmes which were randomized at implementation. Most of them were designed as randomized interventions, while one turned into an experiment due to accidental administrative mistakes (Bolsa Alimentação in Brazil). The quality points assigned to each study differ based on how convincingly the authors have addressed the potential issues with randomizations and how clearly they have described their data and data problems. Gitter et al. (2010) and Hoddinott and Bassett (2008) lost most of their points because in analysing multiple programmes, they leave out the discussion of attrition and/ or the baseline comparison of characteristics between control and treatment. Table 3.3 illustrates that the studies using experimental methods have the highest quality scores.

Table 3.3 Study quality assessment

Authors	Country	Peer Reviewed	Randomized	Total Quality Score
Ahmed et al.	Bangladesh	NO	NO	12
Attanasio Gomez et al.	Colombia	NO	NO	8
Baulch	Bangladesh	NO	NO	18
Behrman et al.	Mexico	NO	YES	21
Duflo	South Africa	Yes	NO	14
Fernald et al. (2008)	Mexico	Yes	YES	21
Fernald et al. (2009)	Mexico	Yes	YES	29
Gitter et al.	(3*)	NO	YES	14
Himaz	Sri Lanka	Yes	NO	12
Hoddinott and Bassett	Mexico	NO	YES	16
Leon and Younger	Ecuador	Yes	NO	14
Leroy et al.	Mexico	Yes	NO	19
Macours et al.	Nicaragua	NO	YES	26
Maluccio	Nicaragua	NO	YES	19
Maluccio and Flores	Nicaragua	NO	YES	22
Morris et al.	Brazil	Yes	YES	19
Paxson and Schady	Ecuador	NO	YES	26
Sinha and Yoong	India	NO	NO	11
Total Average		41%	53%	18

* Gitter et al. look at Honduras, Mexico, and Nicaragua.

Among the programmes that were not randomized at implementation are Familias en Acción, Bono Solidario, FSVGD, RMP, FFA, ABAD, PES, Samurdhi, the extension of Oportunidades in urban areas, the South African Old Age Pensions, and the Child Support Grant. The authors use various techniques to solve the placement and self-selection biases. To start with, Leroy et al. (2008) and Baulch (2010) estimate programme effects with difference-in-difference propensity score matching (PSM) because they have access to both baseline and follow-up survey data. These are carefully performed studies which have clearly outlined the methodology, the data and the advantages of the matching approach.

Many studies lost quality points due to study design. Ahmed et al. (2009) and Himaz (2008) use PSM for the evaluation of the specific programmes; however, the authors have access to only one cross section of data. Since the matching is done based on pre-programme characteristics, pre-existing differences between the control and treatment are accounted for, but time trends are not addressed. The quality checklist is designed to give the best scores to experimental studies, and accordingly the last two studies score relatively poorly (12 each).

Another way to evaluate non-randomized programmes is to use instrumental variables. That is the approach taken by Duflo (2003) and León and Younger (2007). Based on the quality checklist, the two articles are ranked equally because they clearly justify their evaluation methodologies, choice of instrumental variables, and data sources. The missing quality points are mostly due to the fact that the programmes were not randomized at implementation.

The last three last studies use somewhat different approaches: Attanasio, Gomez et al. (2005) use matching at the community level in order to create a counterfactual to the treated communities. The availability of baseline and follow-up data allows the use of the difference-in-difference PSM, but the authors never refer to the method as PSM and provide little information on the matching process and the counterfactual. Moreover, the quality of the study is compromised by the simultaneous existence of a large-scale nurseries programme, *Hogares Comunitarios*, which also targets child nutrition and hence the estimates may be downward biased. These are the main reasons for this study's low quality score. Sinha and Yoong (2009) also use the difference-in-difference approach since they have panel data. However, the programme was not randomized at implementation. The authors are forced to estimate the intent-to-treat effect because they do not observe actual participation; moreover, the counterfactual that they use is not similar eligible households who are not offered the transfer, but rather ineligible households. This raises serious questions as to whether placement bias has truly been resolved. Finally, Agüero et al. (2009) analyses the impact of the Child Support Grant in South Africa. The slow roll out of programme benefits created variations in coverage period and treatment dosage was used for identification. Agüero et al. (2009) also include a variable to control for "parents' eagerness" to obtain the benefits, an attempt to capture part of the self-selection bias that often plagues programme evaluations. It is not clear whether this approach leads to unbiased estimates.

4. Synthesis results

Outline of chapter

In this chapter we use meta-analysis to investigate the links between the outcome of interest and 17 covariates. We devote the most attention to height for age (HAZ) since that is the outcome for which we have the most data. We estimate the effects of programme, study, and child characteristics on HAZ and narrow the list down to a few showing statistically significant relationships.

Several factors show some association with nutritional status. We find that girls see greater increases in HAZ, and that when evaluated as a group, programmes with conditionalities do no better at improving nutritional status than do programmes without. In particular, programmes with conditions unrelated to health outcomes appear to have a negative influence on HAZ, while conditions related to health have positive effects. Also programmes in countries with initially poor health indicators tend to see more of an impact. However, in our relatively small sample, many of these factors were correlated, making it difficult to isolate the specific factor responsible for an observed difference.

4.1 Summarizing reported outcomes

Height for age is the most frequently used anthropometric outcome in the data we collected, but there was some use of weight for age and weight for height z-scores as well as of height in centimetres. Half of the programmes showed positive effects and half negative effects on weight for age, and the same is roughly true for weight for height z-scores. Table 4.1 summarizes the data we collected on these outcomes for various cash transfer programmes.

The analysis of the impacts of cash transfer programmes on height for age uses 18 studies looking at 15 programmes in 10 countries. Two studies examine multiple programmes: Ahmed et al. (2009) evaluate three cash transfer programmes in Bangladesh, and Gitter et al. (2010) evaluate programmes in Mexico, Nicaragua, and Honduras. Likewise some programmes, such as Mexico's *Oportunidades*, were examined by many different studies. Table 3.1 in the previous section lists the studies and the programmes they examine.

To make things more complicated, most studies generate multiple estimates of a given programme's effect. For example Duflo (2003) tests the effects of the South African Old-Age Pension Program on child height for age 20 different ways. She looks at the programme's effect on boys and on girls separately, and she estimates each many times using different econometric specifications. Other studies report multiple estimates because they use different types of estimators (e.g. OLS, 2SLS, PSM) as robustness checks on their outcomes.

Table 4.1 summarizes the findings of each study. Descriptive statistics including an estimated mean impact and the standard error of that impact are listed. The final column in Table 4.1 lists the number of estimates generated by each study. Table 4.1 does not include studies that did not report HAZ as an outcome.

Table 4.1 Average Impact on Height for Age by Study

Authors	Country	Mean Impact HAZ	Mean SE	Number of estimates
Ahmed et al.	Bangladesh	-0.04	0.64	4
Attanasio Gomez et al.	Colombia	0.06	0.06	3
Baulch	Bangladesh	0.40	0.23	6
Behrman et al.	Mexico	-0.01	0.06	4
Duflo	South Africa	-0.01	0.35	20
Fernald et al. (2008)	Mexico	0.20	0.06	1
Fernald et al. (2009)	Mexico	0.04	0.03	2
Gitter et al.	(3*)	0.03	0.08	9
Himaz	Sri Lanka	-0.33	0.29	30
Hoddinott and Bassett	Mexico	0.15	0.10	2
Leon and Younger	Ecuador	0.21	0.12	10
Leroy et al.	Mexico	0.17	0.17	7
Macours et al.	Nicaragua	-0.06	0.12	3
Maluccio	Nicaragua	0.06	0.14	2
Maluccio and Flores	Nicaragua	0.13	0.09	1
Morris et al.	Brazil	-0.11	0.10	4
Paxson and Schady	Ecuador	0.04	0.07	6
Sinha and Yoong	India	0.34	0.27	3
Unweighted Averages		0.08	0.17	117

* Gitter et al. look at Honduras, Mexico, and Nicaragua.

There is considerable heterogeneity in estimates of programme impact, due at least in part to the considerable heterogeneity in the programmes themselves. The largest estimated effect is of the Apni Beti Apna Dhan program in southern India, which improved child height by over 0.3 standard deviations, a result that was statistically significant. The lowest effect is associated with a programme in Bangladesh called the Rural Maintenance Program, which decreased the height for age of children in participating households by as much as 0.4, though broad variation in estimated impacts and a relatively small sample size rendered that result statistically indistinguishable from zero.

Authors such as Barrientos and Nino-Zarazua (2010) claim that, "A range of studies on low and middle income households finds that social transfer programmes are effective in improving nutrition, access to health care, and health status among beneficiaries" (p. vii). Does giving money to the poor work out, on average, regardless of time, place, and location? To evaluate that claim, we pool our data using standard meta-analytical techniques. As Bravata and Olkin (2001) point out, pooling data while ignoring weights can lead to biased results. Higgins et al. (2008) and Waddington et al. (2009) identify two main problems associated with analysing studies when at least some provide multiple estimates. The first issue is that studies with multiple estimates would receive more weight than studies with a single estimate. In our data set the number of such estimates ranges from 1 to 30. The second issue is that multiple observations of the same programme are likely

correlated, so if these correlations are not taken into account we will underestimate the true variance. For example, in our data Himaz (2008) uses different matching estimators on the same sample and provides separate estimates by gender and age. Each estimate contains useful information so we want to include all of them, but at the same time we must control for the correlation between estimated impacts for different estimators or treatment groups in the same programme. Failure to do so underestimates the variance of the impacts (Borenstein et al. 2009).

Following Waddington et al. (2009) we calculate an average effect for each programme and study, where each observation is weighted by the sample size. Next we calculate the variance of this mean estimate by accounting for the correlation between comparison groups. When the same control and treatment group is used in different estimators we assume that the correlation is 1. In some cases such as comparisons between boys and girls or between younger and older children, the individuals in the treatment and control groups are different but the programme is the same. In these cases we would expect some correlation between outcomes. In this case, like Waddington et al. (2009) we estimate the correlation using the midpoint of 0.5 (with a correlation of 0, as with two independent observations, being an underestimate and 1, perfectly correlated, an overestimate of the variance). Borenstein et al. (2009, Chapter 24) show that the variance can be calculated with the below equation, with which we calculate the variance for each programme and study. Here Y_i is the measured impact on HAZ of a study or program, m is the number of measured impacts, and V is the estimated variance of the outcome.

$$var(\frac{1}{m}\sum_{i=1}^{m} Y_i) = \left(\frac{1}{m}\right)^2 var(\sum_{i=1}^{m} Y_i) = \left(\frac{1}{m}\right)^2 (\sum_{i=1}^{m} V_i + \sum_{i\neq j}^{m} r_{ij} * \sqrt{V_i * V_j}$$

If we treat each estimate of each programme separately and combine them with equal weights, the mean programme effect is a positive impact of 0.04 z-scores, with an average standard error of 0.18. Of 18 studies, 12 found on average positive impacts of programmes, while 6 found negative impacts. Since some programmes such as Mexico's PROGESA/ Oportunidades CT were analysed by multiple papers, another way to look at the same data is to group those that examined the same programmes. When we summarize estimated effects by programme rather than by study, weighing all 15 programmes equally the average impact is just 0.03 with a standard error of 0.22. 10 of the 15 programmes see positive effects.

Ideally we would also analyse the relationship between the covariates of interest and each measure of child nutrition, including weight for age, BMI, and height in centimetres. However, we have limited data on these three other measures. As Table 4.2 shows, only 6 studies estimated programme impacts on weight for age z-scores (WAZ) and only 5 on weight for height (WHZ) or BMI for age scores. The limited number of studies does not permit substantive covariate analysis. However, it is worth noting that the correlation between impacts on height for age and the other potential measures is quite high. In the six studies with data on both height for age and weight for age, the correlation between the average impact on these two outcomes is 0.90.The correlation between height for age and weight for height (or BMI) z-scores for the 5 studies with data on both is 0.78. Thus, findings on HAZ are somewhat likely to apply also to WAZ, WHZ, and/ or BMIZ.

Table 4.2 Average Impact on Various Outcomes by Study

Authors	Country	Mean Impact WAZ	Mean Impact BMIZ/WHZ	Height in cm	Mean Impact HAZ
Ahmed et al.	Bangladesh	-0.02	0.16		-0.04
Attanasio, Gomez et al.	Colombia				0.06
Attanasio, Battistin et al.	Colombia			0.18	
Baulch	Bangladesh		0.37*		0.40
Behrman et al.	Mexico				-0.01
Behrman & Hoddinott 2005	Mexico			0.51†	
Case	South Africa			6.24	
Duflo	South Africa				-0.01
Fernald et al. 2008	Mexico				0.20
Fernald et al. 2009	Mexico		-0.02*		0.04
Gertler	Mexico			1.0	
Gitter	(3**)				0.03
Himaz	Sri Lanka		-0.17		-0.33
Hoddinott and Bassett	Mexico	0.09			0.15
Leon and Younger	Ecuador	0.74			0.12
Leroy et al.	Mexico		0.07	0.64	0.17
Macours et al.	Nicaragua	-0.04			-0.06
Maluccio	Nicaragua				0.06
Maluccio and Flores	Nicaragua				0.13
Miller et al.	Malawi			5.28	
Morris et al.	Brazil	-0.14			-0.11
Paxson and Schady	Ecuador				0.04
Rivera et al.	Mexico			1.1	
Sinha and Yoong	India	0.46			0.34

* indicates BMIZ; others in this column are WHZ

** Gitter et al. look at Honduras, Mexico, and Nicaragua.

† Behrman and Hoddinott provide other estimates without child fixed effects that show negative results. These results are not included in the average estimated impact as the authors suggest these estimates are biased.

Table 4.2 reports weight for age and weight for height z-scores as well as height for centimetres for those studies that report those outcomes. Again, the figures presented are weighted by sample size. In the search we also found 7 studies that utilized height in cm as the measure of interest. These studies examined four programmes, three of which we were able to include. Those three were included since, in addition to the work examining height in centimetres, an analysis of HAZ was also performed. (SCTS in Malawi is not included, since data appear only in the form of height in cm rather than z-scores.)

Analyses using height in centimetres come to similar conclusions as those using HAZ. Of the 7 studies using centimetres four examine PROGRESA and, consistent with the findings using z-scores, each finds an increase of roughly ½ to 1 cm. The analysis of Familias in Acción shows a small increase in height in centimetres, also roughly equivalent to the study that uses z-scores. (The two studies share lead authors.) The exception is Case (2001), who has larger impacts on height in centimetres than Duflo (2003) does for the same programme using height for age z-scores. However, Case focuses on the disadvantaged population and has a sample size under 50 in many cases.

4.1.1 Descriptive Statistics of Covariates by Study

The covariates can be divided into four groups: the programme characteristics (nutritional supplement offered, conditionality, enforcement of conditions, approximate payment size, and programme duration); characteristics of the study (peer reviewed, randomization, study quality, whether the baseline z-score was measured, and average sample size); of the child (sex and age); and country characteristics (infant mortality rate, hospital beds per 1000 persons, share of children with acute respiratory infections receiving health care, percentage of children receiving the DPT vaccine, and the share of the population with improved sanitation). We identified local health conditions as important in section 1.5, but lacking local information on disease environment and the quality and utilization of local health services we use the World Bank's World Development Indicators, which are country level measures. A more detailed description of each of the four types of covariates follows.

4.1.1.1 Programme Covariates

The programmes studied vary in several ways. A little over half offered a nutritional supplement, while 11 were conditional. Of the 11 conditional programmes, 7 were conditional on health check-ups for children and health seminars for mothers. 8 of the 11 actually enforced their required conditions. We also test the relationship between the duration of time that a household has received transfers on effect size. Finally, we approximate monthly payment sizes by converting all figures to US dollars. We do this either based on values reported in the studies themselves or by utilizing the prevailing exchange rate in the baseline year. There is a relatively even three-way split of programmes between those that paid under $10 a month, those paying $10-$20 a month, and those paying over $20. We considered also testing the share of baseline income comprised by the transfer; however, we lacked the data from many studies to do so (see Table 3.2A). We were likewise unable to test the impact of the gender of the recipient of programme funds, because only 2 of the programmes (Samurdhi and Old Age Pension) targeted funds equally to men and women, while all other interventions targeted women only.

Table 4.3 Study, Child and Programme Covariate Descriptive Statistics

		Study Covariates					Child Covariates			Programme Covariates			
Authors	Country	Peer Review	Random	Quality	Baseline Z given?	Average Sample Size	All Male Samples	All Female Samples	% Under 36 Months	Nutr Suplmt	Cond'l?	Enforce Cond.'s?	Payment Size*
Ahmed et al.	Bangladesh	NO	NO	12	YES	317	0%	0%	0%	50%	75%	25%	1
Attanasio Gomez et al.	Colombia	NO	NO	8	NO	Missing	0%	0%	33%	0%	100%	0%	2
Baulch	Bangladesh	NO	NO	18	NO	129	33%	33%	0%	0%	100%	0%	1
Behrman et al.	Mexico	NO	YES	X	NO	379	0%	0%	100%	100%	100%	100%	3
Duflo	S. Africa	Yes	NO	14	YES	1582	50%	50%	50%	0%	0%	0%	3
Fernald et al. (2009)	Mexico	Yes	YES	29	YES	1710	0%	0%	0%	100%	100%	100%	3
Fernald et al. (2008)	Mexico	Yes	YES	21	YES	2402	0%	0%	0%	100%	100%	100%	3
Gitter et al.	(3**)	NO	YES	14	YES	817	33%	33%	0%	67%	100%	67%	2, 3†
Himaz	Sri Lanka	Yes	NO	24	NO	484	20%	20%	20%	0%	100%	0%	1
Hoddinott & Bassett	Mexico	NO	YES	16	YES	Missing	0%	0%	50%	100%	100%	100%	3
Leon and Younger	Ecuador	Yes	NO	12	YES	850	0%	0%	60%	0%	0%	0%	2
Leroy et al.	Mexico	Yes	NO	19	YES	432	0%	0%	29%	100%	100%	100%	3
Macours et al.	Nicaragua	NO	YES	24	YES	1622	0%	0%	0%	100%	100%	100%	3
Maluccio	Nicaragua	NO	YES	19	YES	1493	0%	0%	0%	100%	100%	100%	3
Maluccio& Flores	Nicaragua	NO	YES	22	YES	1982	0%	0%	100%	100%	100%	100%	3
Morris et al.	Brazil	Yes	YES	19	YES	915	0%	0%	25%	0%	100%	0%	2
Paxson& Schady	Ecuador	NO	YES	26	YES	1434	0%	0%	0%	100%	0%	0%	1
Sinha & Yoong	India	NO	NO	11	YES	2241	0%	100%	100%	0%	0%	0%	1
Total Average		41%	53%	18	82%	1227	8%	14%	27%	54%	75%	47%	2.20

*1: under $10 a month; 2: $10-20; 3: over $20 a month **Gitter et al. look at Honduras, Mexico, and Nicaragua. † This paper considers programmes in both categories

4.1.1.2 Study Covariates

Table 4.3 shows the covariate data for each of the 18 studies. As noted, we examine five study characteristics. First is peer review. Of the studies we identified, 41% are peer reviewed. It is important to test for publication bias in studies as published studies may favour a surprising result, and we do this below. Next, the "Gold Standard" in programme evaluation is a randomized control group. By using randomized experiments researchers hope to limit bias. Of the studies 53% use randomized controls to test programme effects. Next we present our own constructed measure of study quality, described above in section 2.3.1. The average quality score is 18 with a range from 8-29. Of the studies 14 of 18 had listed the height for age z-scores of their programme recipients at baseline. Having baseline scores allows for the addition of controls of pre-programme conditions. Finally we report the average sample sizes for each study, which run from just over 100 to over 2,000. The average sample size in the studies used was 1,227. Of the 18 papers two did not report sample size (Attanasio et al. (2005) and Hoddinott and Bassett (2008)). Nearly all studies (16 of 18) added controls for mother's education and household size. Due to this lack of heterogeneity we cannot examine differences between studies that add and do not add these controls.

4.1.1.3 Child Covariates

We identified sex and age as two child characteristics that could influence the effect of a cash transfer programme. There is some evidence that girls may see larger benefits than boys from cash transfers (Duflo 2003). To test for this effect we compare the 6 programmes (including Duflo's analysis) with separate estimates by sex to test for Duflo's finding.

Hoddinott and Bassett (2008) note that interventions with the goal of reducing stunting should focus their treatments on pregnant women and children under 2, and some reviewers have suggested that we limit our review to those examining changes in height for age among children no older than three. Others such as Grillenberger et al. (2006) argue that, "Nutrition and health in the first 2 to 3 years of life can affect the growth and development of children, and most growth faltering occurs during this time. However, the growth of older children is also important for their normal development and some studies have shown that catch-up growth is possible in school-aged children and even in adolescents…." (p. 379) This suggests that studies with estimated influences on younger children are more likely to see effects, but that studies with older children may viably find effects as well. We test directly for an age effect on estimates across studies. Additionally, we test the difference in estimated effect size for the 8 studies that include treatment groups with different ages. Finally, we also compare estimated effect size for the youngest and oldest cohorts tested.

4.1.1.4 Country Covariates

Disease environment, health care quality, and health care utilization are important pathways for child nutrition. Ideally we would have local measures of these variables, but these are not available in most studies. In their place we use five measures of national health conditions in the closest available year to the programme's baseline, which are summarized in Table 4.4. In most cases we are able to find data in the baseline year. When we cannot do so, we use the closest available year or the average of the two closest years in cases where the closest years are equidistant from the baseline. The five measures we report are infant mortality rate, hospital beds per 1,000 persons, share of children with acute respiratory infections that receive health care, the percentage of children receiving the DPT vaccine and finally the percentage of household nationally that have improved sanitation.

Table 4.4 Country Level Measures of Health Care Quality and Utilization

Country	Baseline Year	IMR	Beds/1k	ARI %	DPT%	ImprSan%	# High Quality
Bangladesh	2006	48.2	0.4	30.1	91	48	1
Bangladesh	2000	65.6	0.3	27.2	83	43	0
Brazil	2002	28.2	2.6	**49.7**	99	36	3
Colombia	2001	22	1.1	51.3	80	50	4
Ecuador	2003	23.5	1.7	38.8	75	*75*	3
Ecuador	1999	28	**1.5**	38.8	79	*68*	3
Honduras	2000	32.6	1	**55.9**	78	47	2
Mexico	1998	22.1	1.1	Missing	96	46	4
Nicaragua	2005	26.6	0.9	**57.7**	88	35	3
Nicaragua	2000	34.2	**1.12**	57.7	83	32	2
India	1992	79	0.787	**67**	56	*8.3*	1
South Africa	1993	48.2	**Missing**	**75.3**	81	58.5	3
Sri Lanka	1999	17.3	2.2	**58**	99	60.5	5

Italics = interpolated from 2 nearest years, Bold = data from > 2 yrs away from baseline
IMR = infant mortality rate
Beds/ 1k = hospital beds available per 1000 people of population
ARI % = share of children with acute respiratory infections who get health care
DPT % = share of children receiving DPT vaccine
Impr San % = share of rural population with access to improved sanitation
High Quality = number of preceding five variables in which the country in question is above the median, i.e. better off, than the others

Table 4.5 Programme Characteristics (Pooled Analysis)[+]

Impact on HAZ of:	Mean[†]	SD	Estimated Impact	P(t)	Lower	Upper	I^2	τ^2
Programme with Nutritional Supplements	38%		0.011	0.895	-0.172	0.195	72.87%	0.016
Conditional Programmes	69%		-0.114	0.206	-0.299	0.070	69.78%	0.013
Conditional, but no Education or Health Conditions	25%		-0.372	0.022	-0.681	-0.063	66.49%	0.007
Health Conditions[+]	44%		-0.081	0.286	-0.243	0.080	72.33%	0.008
Programmes Enforced	25%		-0.000	0.998	-0.199	0.198	72.49%	0.016
Less than $10 a month	56%		-0.003	0.970	-0.187	0.181	71.97%	0.016
Duration	25.1	19.4	0.003	0.142	-.0013	.00814	71.58%	0.012

A simple meta-regression was performed for each covariate.
[†]Means for dummy variables shown as a percentage.
+ Analysis compares programmes with conditionalities related to health against unconditional programmes, dropping programmes with conditions unrelated to health.

Table 4.6 Study Characteristics (Pooled Analysis)

Impact on HAZ	Mean[†]	SD	Estimated Impact	P(t)	Lower	Upper	I^2	τ^2
Published Studies	41%	51%	-0.023	.686	-.145	.098	67.71%	.0097
Quality	16.54	5.35	-0.0012	.795	-0.011	0.009	66.04%	.0094
Randomized	53%	51%	-0.064	.279	-0.184	.056	61.69%	.00727
Sample Size In 100s of Observations	10.58	7.56	0.004	.379	-0.006	0.013	68.74%	.011
Baseline Z-Score	-1.55	0.40	-0.075	.340	-0.237	0.087	63.0%	0.0065

[†] Means for dummy variables shown as per cents.
A simple meta-regression was performed for each covariate.

Table 4.7 Influence of Child Characteristics

Impact on HAZ	Mean†	SD	Estimated Impact	P(t)	Lower	Upper	I^2	τ^2
All Female Sample	NA	NA	0.202	0.052	-0.002	0.408	52.09%	.011
Mean Age For All Programmes (N = 16)	45.3	33.9	0.001	0.545	-0.002	0.003	72.9%	.015
Mean Age For Programmes with Multiple Age Cohorts (N = 16)	34.0	24.1	-0.002	0.167	-0.005	0.001	29.08%	.000

A simple meta-regression was performed for each covariate.
† Means for dummy variables shown as percentages.

Table 4.8 Country-level Covariates

Impact on HAZ	Mean†	SD	Estimated Impact	P(t)	Lower	Upper	I^2	τ^2
Infant Mortality Rate	38.7	17.2	0.006	.034	0.001	0.01	70.05%	.009
Hospital Beds Per 1,000 People	1.06	0.69	-0.141	.024	-0.26	-0.02	66.93%	.008
% of Children with Acute Respiratory Infections Who See a Doctor	46.5	15.4	-0.005	.195	-0.013	.002	69.53%	.011
% of Households With Improved Water	73.9	6.36	-.0005	.930	-0.013	.012	72.93%	.0163
% of Household with Impr. Sanitation	46.9	15.3	-0.001	.596	-0.007	.004	72.18%	.0163

A simple meta-regression was performed for each covariate.
† Means for dummy variables shown as percentages.

4.2 Synthesis of evidence

In this section we analyse the impact on HAZ of potential covariates. Tables 4.5-4.8 summarize the data and a forest plot by programme is presented in Figure 4.1. The latter graphically depicts each study and summarizes the overall estimated effect.

To create a forest plot and estimate an overall, pooled programme effect of the 16 observations in our data set we use standard meta-analysis techniques. One way to estimate a pooled effect given heterogeneous estimates is to assign weights to studies by their variance. This helps by giving larger weights to studies with larger sample sizes and lower variance in estimates. The estimated treatment effect (θ_{pooled}) for the pooled sample of "k" studies is equal to a ratio shown in equation 1 below. The numerator of the ratio is the sum of product of each study's weight (w_i) and mean effect (θ_i) and the denominator is the sum of the weights.

$$\theta_{pooled} = \frac{\sum_{i=1}^{k} w_i \theta_i}{\sum_{i=1}^{k} w_i} \qquad (1)$$

There are two common methods for calculating the weights used above in equation 1. One method is "fixed effects" in which the weight of a study is the inverse of the estimated variance ($w_i = 1/v_i$). However, this method assumes that if measured correctly, all studies have equal treatment impacts. In other words, fixed effects would assume that Mexico's *Oportunidades* should have the same impact as the South African Pension programme. This seems unlikely in our data as cash transfer programmes differed not only by where they were administered but also by how they were designed. To avoid making this assumption we use a "random effects" model.

A random effects model assumes that the true treatment effect of a given programme is observed as normally distributed with variance τ^2. This measure is the "between studies variance" and it can be used to calculate the appropriate weights. When using random effects the study weight is

$$w_i = 1/[v_{i+} \tau^2]$$

We use the standard DerSimonian and Laird estimate of τ^2. Consistent with the assumption that each of the separate programmes has its own impact on children's height for age z-scores, we use programmes to identify random effects, reporting τ^2 for each pooled analysis.

In the analysis we also report I^2, which is a measure of the portion of the variation that reflects real differences in effect size among studies. Higgins et al. (2003) suggest that an I^2 equal to 25% or less is a low level of heterogeneity, while 50% is moderate and 75% is high. For the whole sample and for most subgroups we have a moderate to high level of heterogeneity among studies. This level of heterogeneity suggests that programmes have different effect sizes, which is likely the result of varying programme and study design. We also report the estimate of the variance between programmes (τ^2), which for most estimates is around 0.01 z-scores.

First an average programme effect is calculated using the methods described in section 4.1. The next stage of the analysis is to examine the influence of the covariates of interest using meta-regression. We use meta-regression on just one variable at a time because small sample sizes limit our degrees of freedom, but regression analysis provides marginal effects while providing a simple statistical test of the effect. Due to the limited sample size we only examine one covariate at a time using weighted least squares (where programmes are weighted by the inverse of their variance) of this equation

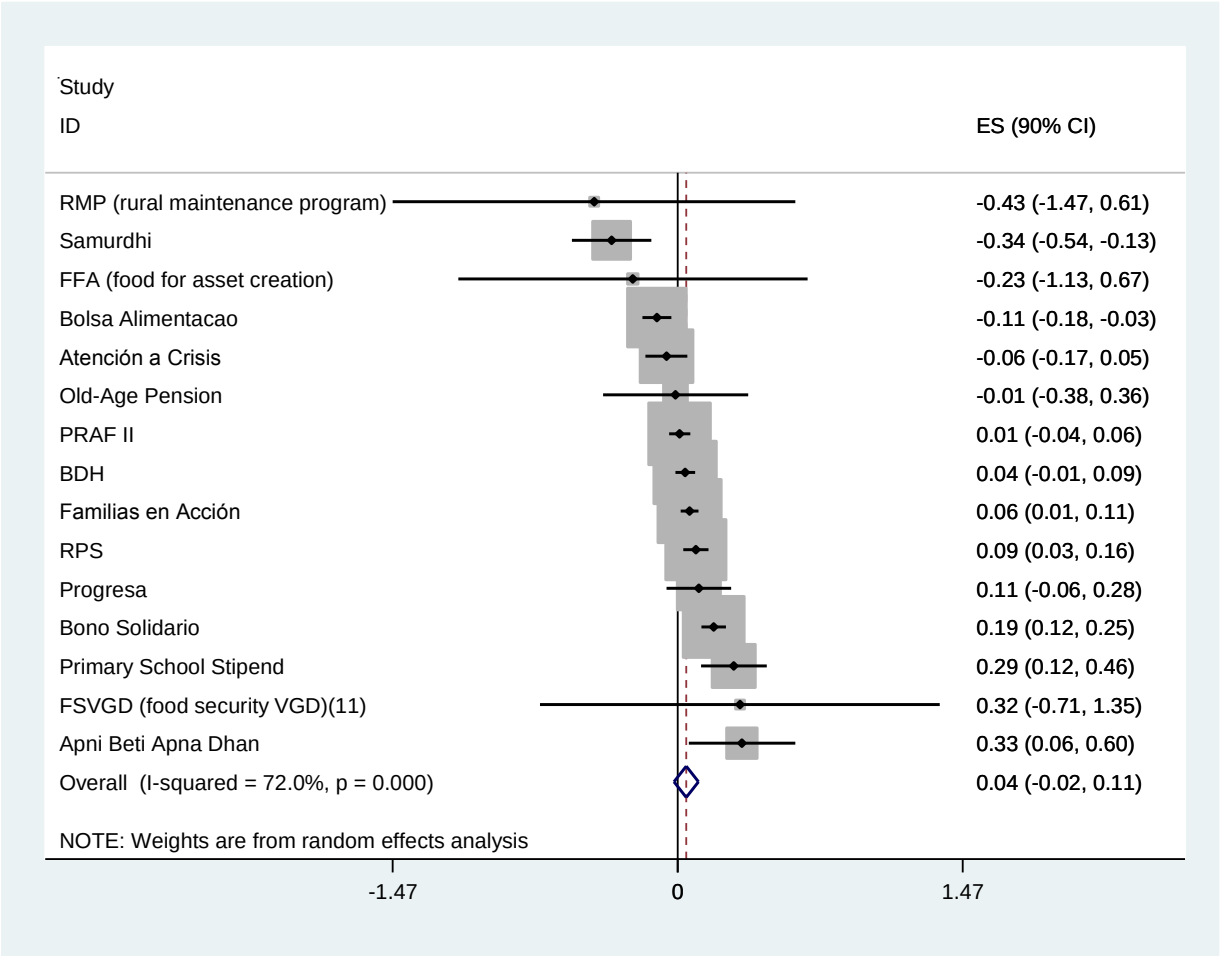
$$Y_i = \beta_0 + \beta_1 X_i + \mu_i$$

We estimate the effect of covariate X_i on the estimated change in HAZ, Y_i, from the program. The estimated influence of covariate X_1 is represented by β_1. Finally the error term is represented by μ.

4.3 Summary of results of synthesis

The forest plot in Figure 4.1 illustrates our finding that on average CT programmes increased height, although the size of the effect is small and statistical significance is weak. Grouping by programmes rather than studies and weighting effect sizes by the inverse of their standard error, the 15 programmes increased HAZ by an average of 0.04 with a p-value that the effect is different from zero of 0.16. The forest plot below shows the estimated mean effect of each of the 15 programmes as well as the confidence interval and weight based on the inverse of the estimated variance.

Figure 4.1 Forest plot showing estimated overall programme effect on HAZ



Sections 4.3.1-4.3.4 analyse the relationship between impact on HAZ and relevant covariates using meta-regression analysis. Below we highlight the most interesting results from each of the four covariate categories.

- Programme: Conditionality is weakly negative, though programmes with conditions unrelated to health show a statistically significant negative

impact on HAZ. There is weak evidence that longer programme duration increases effect size. Nutritional supplements do not show significant effects.

- Study: Although we do not find publication bias in a specific direction, we do find that published studies are more likely to show statistically significant results.
- Child Characteristics: In the subsample of studies with multiple estimations we find larger impacts for girls and younger children (with the second result being weaker)
- Country Characteristics: Higher infant mortality rates and fewer hospital beds are associated with larger marginal programme impacts on HAZ.

4.3.1 Programme Covariates

Programme covariate regressions are summarized in Table 4.5. As noted above, each coefficient represents a separate regression on a single variable and an intercept. Sections 4.3.1.1 – 4.3.1.4 summarize the four programme covariates of interest.

4.3.1.1 Nutritional Supplements

The results do not show an impact of supplements, likely due to confounding. Among studies reporting solely on programmes with supplements, all but one found a positive programme impact, though three found small impacts (<= 0.06). Our estimation finds that the pooled effect of supplement provision on HAZ is nearly zero with an estimated standard deviation of 0.01(a p-value of 0.895). Even removing Atención a Crisis, the one programme with a supplement and negative effects, the influence of supplements still has large p-values (0.6).

One complicating factor is that the provision of nutritional supplements shows a 0.59 correlation with the mean age of the sample. In other words, programmes that provided supplements were also programmes that tracked older children. We would expect older children to show less evidence of programme effect, and since the two are correlated, that reduction in observed efficacy translates to apparent ineffectiveness of the supplements.

Note that this does not mean that supplements were provided ineptly: if supplements were provided only to younger children, we still coded the entire programme as providing supplements. If researchers then tracked estimated programme effects on children of all ages, the combined effect would include the effects of the supplements only insofar as the supplement-receiving children dominated the sample. For example, if only the youngest 1/3 of children in the sample receive supplements, overall programme effects on the whole sample will show at best 1/3 of the true effect caused by supplements and possibly less if supplement provision was sub-optimal (as in PROGRESA for example).

The non-finding therefore likely stems from the broad level of aggregation needed to perform this analysis rather than from improper application of supplements. Nonetheless, it tells a cautionary tale that the provision of supplements alone is no guarantor of success in improving nutritional status.

4.3.1.2 Conditionality

Our first look at conditionality, shown in the second rows of Table 4.5, shows that conditional programmes have weaker effects than unconditional programmes by a factor of 0.11. However, the effect is not statistically distinguishable from zero (p-value = 0.21).

To investigate the negative effect of conditionality we split up conditionality into two groups based on the type of condition. When we ran a pooled analysis on the impacts of the seven programmes with health conditions such as required visits to clinics and seminars, we find these programmes on average increase HAZ scores by 0.04. However, the impact is not statistically significant (p-value = .20). The five unconditional programmes increase HAZ 0.12 with a p-value of 0.04. When we run a regression with a binary dummy equal to one when programmes have non-health conditions (e.g. requiring work or savings quotas) we find that those programmes show negative impacts on HAZ, decreasing child height by 0.37 (p-value = 0.02). This result suggests that conditionality demanding other sorts of behaviour from beneficiaries may impede child development. (We obtain similar results whether a single covariate is used for programmes with non-health conditions or an additional variable is added to control for programmes with health conditions.) Finally we ran a regression with just unconditional and programmes with health conditions. Conditional programmes' effect size was 0.08 smaller than unconditional although the difference was not statistically significant (p-value = 0.2)

To test the robustness of this result we reran the regression 15 times, each time dropping one programme to test for the influence of any one programme. In 14 of the regressions we find similar results, near -0.37. When the Samurdhi programme is dropped, the coefficient is -0.19, although not statistically significant. Samurdhi had the largest negative impact and also had non-health conditions, so dropping it from the estimate would have the biggest influence on the estimation.

The group of conditional interventions with requirements unrelated to education or health were highly correlated with two indices of poor health. The number of hospital beds per 1000 people and the share of children with acute respiratory infections who have access to a doctor are both negatively associated with non-health conditionality. Since we show later that these low health countries in general experience higher returns to transfers, having a negative sign on this relationship is a strong indictment of these other types of conditionality.

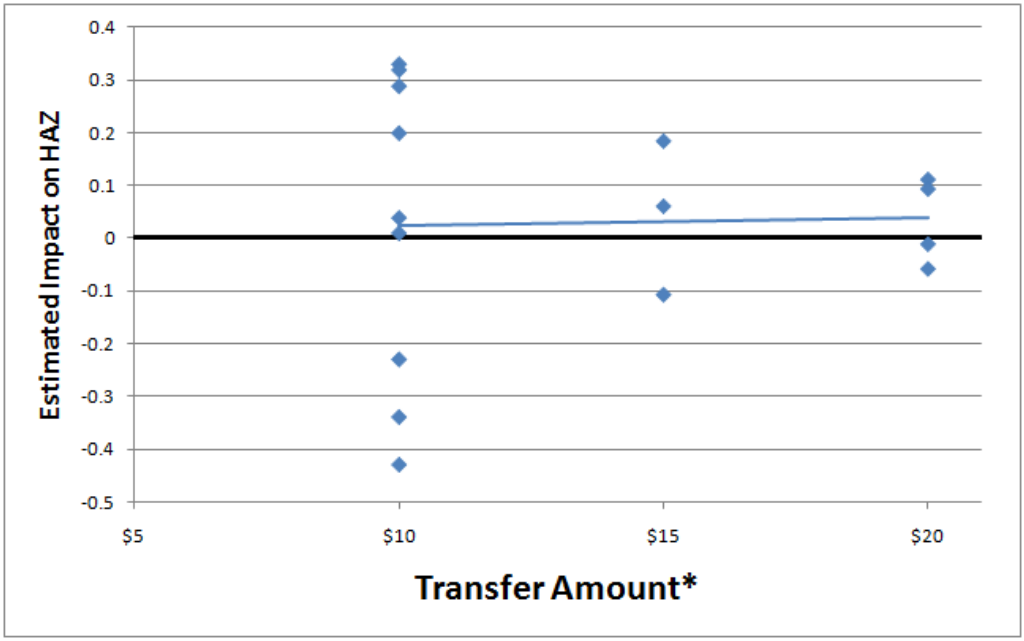
An additional characteristic of conditionality we examine is enforcement. Only four programmes with conditionalities actually enforced the conditions. That is, only for the minority of alleged "conditions" was there evidence that benefits were withdrawn for those who did not meet the "requirements." Programmes with enforcement did not show different impacts from those without enforcement. Interactions between enforcement and type of conditions yield similar results.

4.3.1.3 Payment Size

We do not find a relationship between payment size and programme effects. We divided the sample into three payment sizes (under $10, $10-$20, and over $20). Estimated impacts minimally increase as payment size goes up with effect sizes of .043, .048 and .064 for the three pooled groups respectively. A regression using a dummy for programmes with payments under $10 shows nearly zero effect on the impact on HAZ scores, and similar results obtain for a regression with a dummy for programmes that pay over $20 a month.

One major caveat must be noted. We observe a relatively strong positive correlation between payment size and local health conditions (0.6 between infant mortality and probability of payment under $10 a month). To the extent that we observe higher impacts in areas with worth health conditions, the estimated effects of lower payments will be biased downward. Thus, this evidence can be taken to show that policymakers did well at fitting the transfer size to the locality. Transfers fit the relative income level of their environment, and none showed outsize returns to their transfer amount.

Figure 4.2 Payment size and estimated effect on HAZ

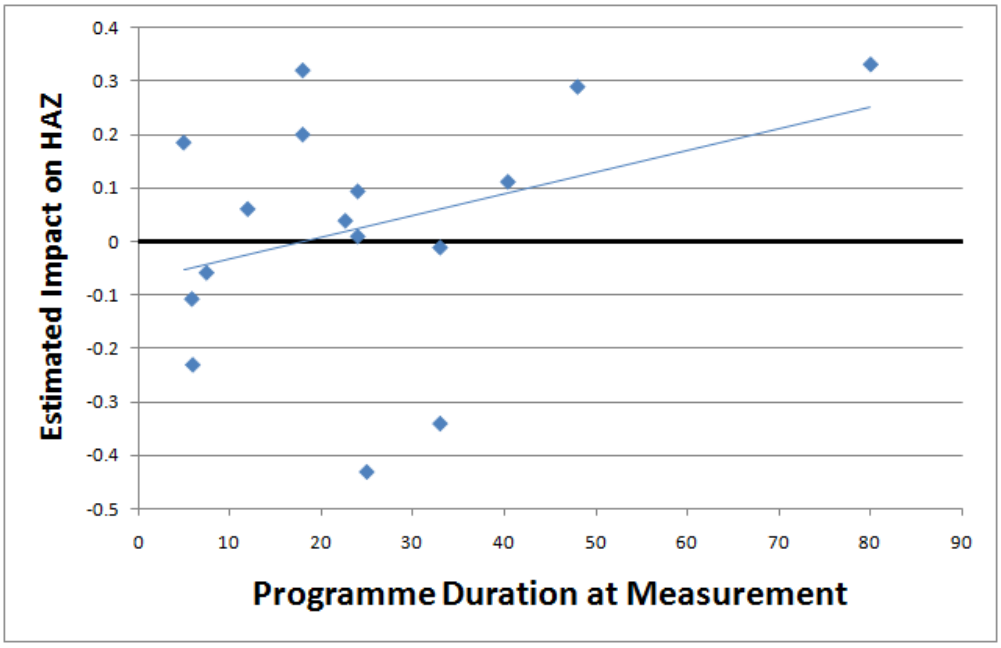


*Note that "$10" on the chart refers to any transfer amount less than USD$10, "$15" is anything between $10-$20, and $20 is anything greater. More detail was not available.

4.3.1.4 Programme Duration

We regress HAZ impacts on the number of months the average household had received payments to test for the effect of programme duration. Figure 4.3 shows the data. We find the expected positive relationship, although not statistically different from zero (p =.14). A one standard deviation increase in programme duration (19.4 months) would lead to estimated increase in z-scores of just over 0.06. Duration squared and log duration provide similar results, with slightly lower p-values. When we rerun the regression 15 times eliminating one programme at a time we find consistent coefficients and p-values (.05 to .4).

Figure 4.3 Programme duration and estimated impact on HAZ



4.3.2 Results of Study-level Covariates Analysis

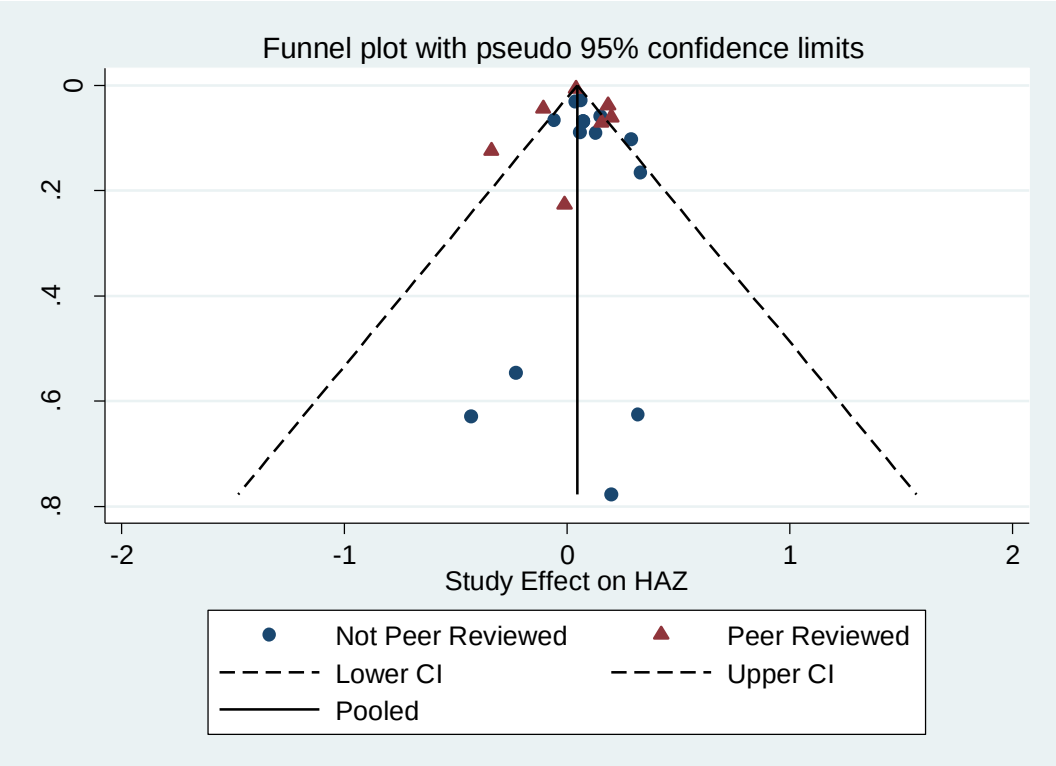
The results of the study-level covariate analysis are summarized in Table 4.6. The results of the five covariates of interest are summarized in sections 4.3.2.1-4.3.2.5.

4.3.2.1 Peer review

In their analysis of conditional cash transfers Gaarder at al. (2009) note the potential for biases that arise from publication, reporting, and censorship. As with their analysis we note the potential and test for publication bias. We begin our analysis of covariates by separating those studies published in peer reviewed journals from those that are not. When we run a regression with a binary dummy = 1 for a published study, the estimated relationship is negative. Peer reviewed studies have a pooled HAZ impact of -0.04, although the relationship is not significantly different from zero (p-value = 0.520)

A closer examination between effect size and publication can be seen in Figure 4.4, a funnel plot contrasting peer-reviewed and non-peer-reviewed work. Although peer-reviewed papers are found on both sides of the triangle, two papers with negative results that are statistically significant are both peer reviewed.

Figure 4.4 Estimated effects on HAZ by peer-reviewed status



We considered using the method suggested by Formann (2008) to address publication bias, but that approach is designed to cope with truncation in reported outcomes. A priori we would expect that positive programme results would be more likely to be submitted for publication, so the presence of evidence on the negative side is what we would have assumed had been truncated. Having published articles more or less equally represented on both sides of the diagram, we see no way to apply the Formann algorithm. Thus, we cannot correct for censorship in the observed distribution of data. This result implies that censorship is likely not a powerful force in this context other than the possible suppression of statistically insignificant results.

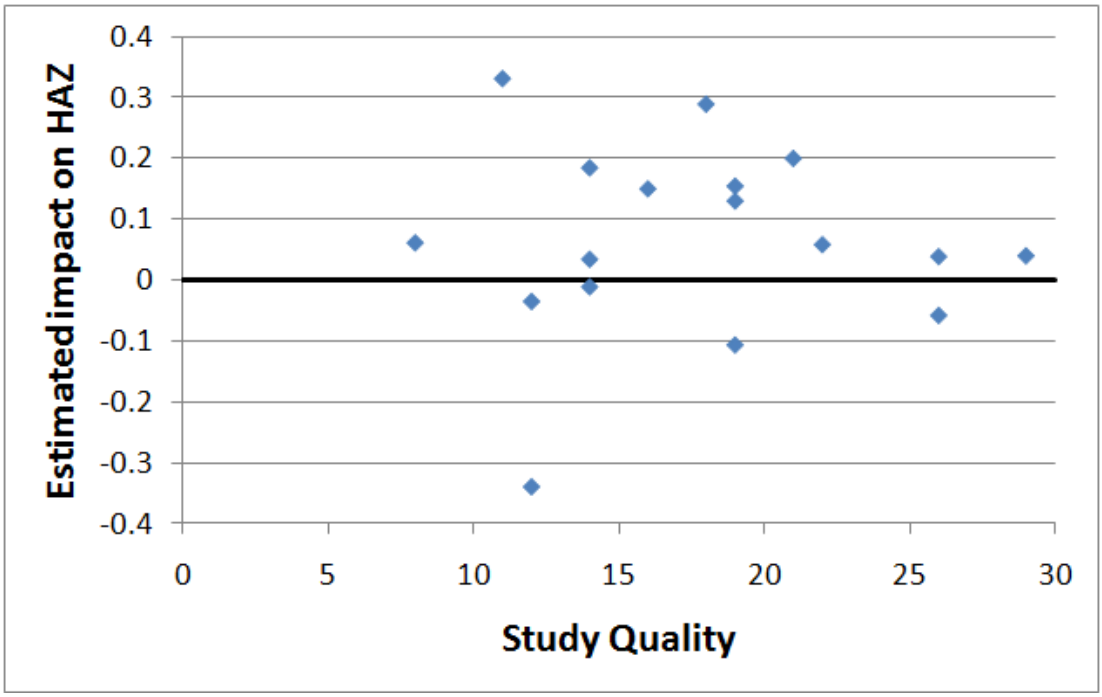
4.3.2.2 Randomization in programme design

Next we compare studies with a randomization mechanism and those who utilize non-randomized estimation techniques. There were equal numbers of randomized and non-randomized studies. Papers that used randomized techniques saw slightly more negative programme impacts (-.07); however, this difference was not statistically different from zero (p-value .27).

4.3.2.3 Study quality

Quality and estimated effect size have a very weak inverse relationship, and a regression does not show statistically significant impacts. We find that for an increase of 1 point on the quality score, effects fall -0.006. A one standard deviation increase is associated with a decrease of about -0.03. Looking at the scatterplot in Figure 4.5, we see that higher quality studies tend to be closer to a zero estimated impact level, while lower quality studies are more variable in their estimated impact.

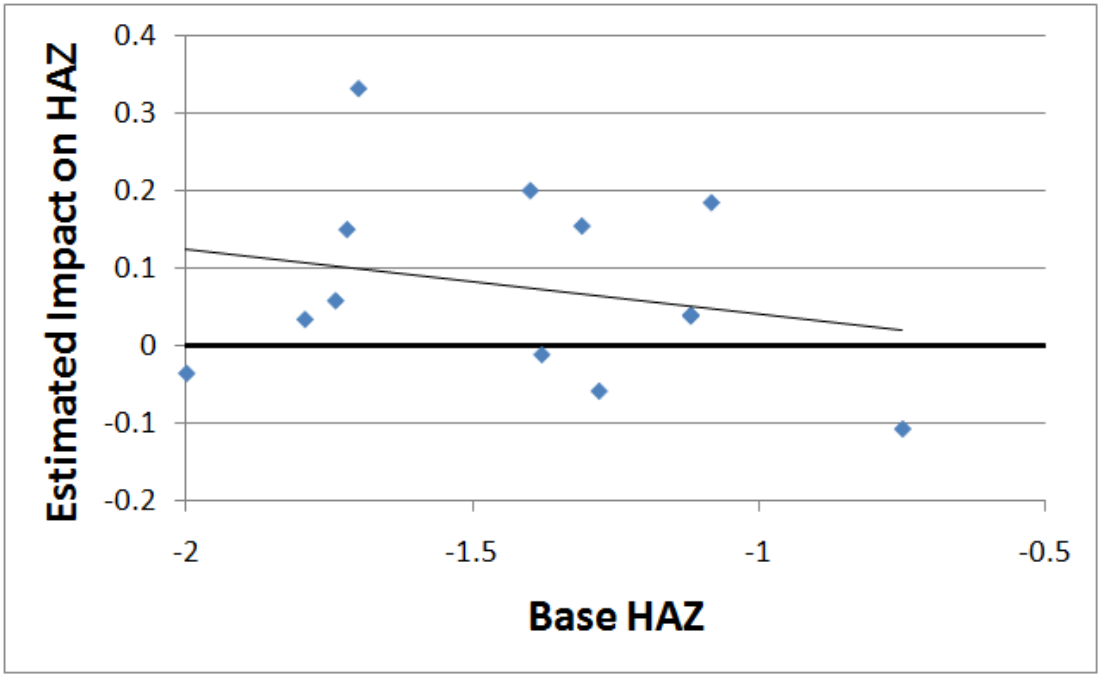
Figure 4.5 Estimated programme impact on HAZ vs. Study Quality



4.3.2.4 Baseline conditions

Baseline conditions have the potential to influence outcomes. Ideally we would have liked to compare sample populations by a baseline income or consumption measure, but limited data and differences in currency make such analysis difficult. Instead, a child's baseline z-score is a good indicator of pre-treatment conditions and it is also easily comparable across nations, as z-scores are built by comparing with international norms. Of the programmes we have baseline HAZ scores for 12, shown in Figure 4.6. When impacts are regressed on baseline scores we do not find a statistically significant link. However, the estimated relationship is in the expected negative direction. The estimated coefficient is -0.05 (p-value =.50) which indicates that a 1 point increase in baseline HAZ would decrease effects by .05 z-scores. This matches well with our finding below that people who are initially worse off benefit more. Lacking statistical significance makes it at best a weak indication.

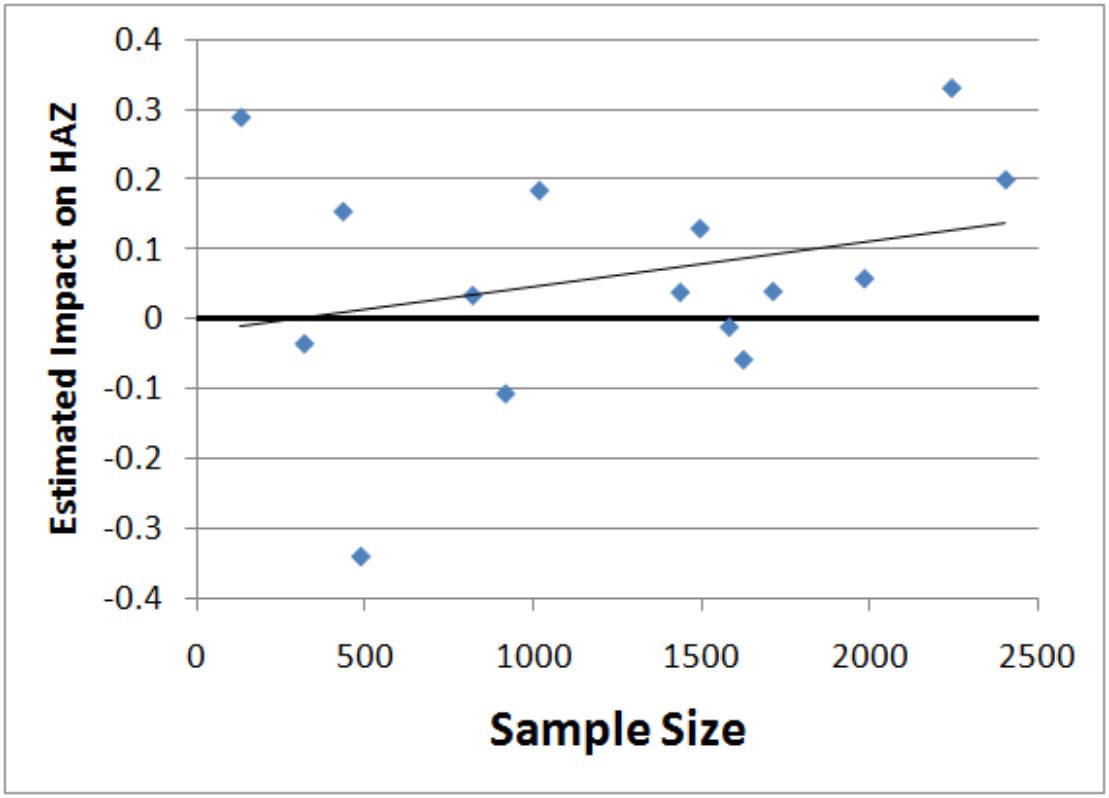
Figure 4.6 Estimated programme impact on HAZ vs. baseline HAZ



4.3.2.5 Sample size in impact evaluation

When we gathered 20 sample sizes from 18 studies and regressed the estimated impact on the average sample size for the estimate, we found a positive but not statistically significant relationship. Figure 4.7 shows the relationship between sample size and mean impact. The regression shows a weak positive relationship between sample size and effect on HAZ.

Figure 4.7 Sample size and estimated effect on HAZ

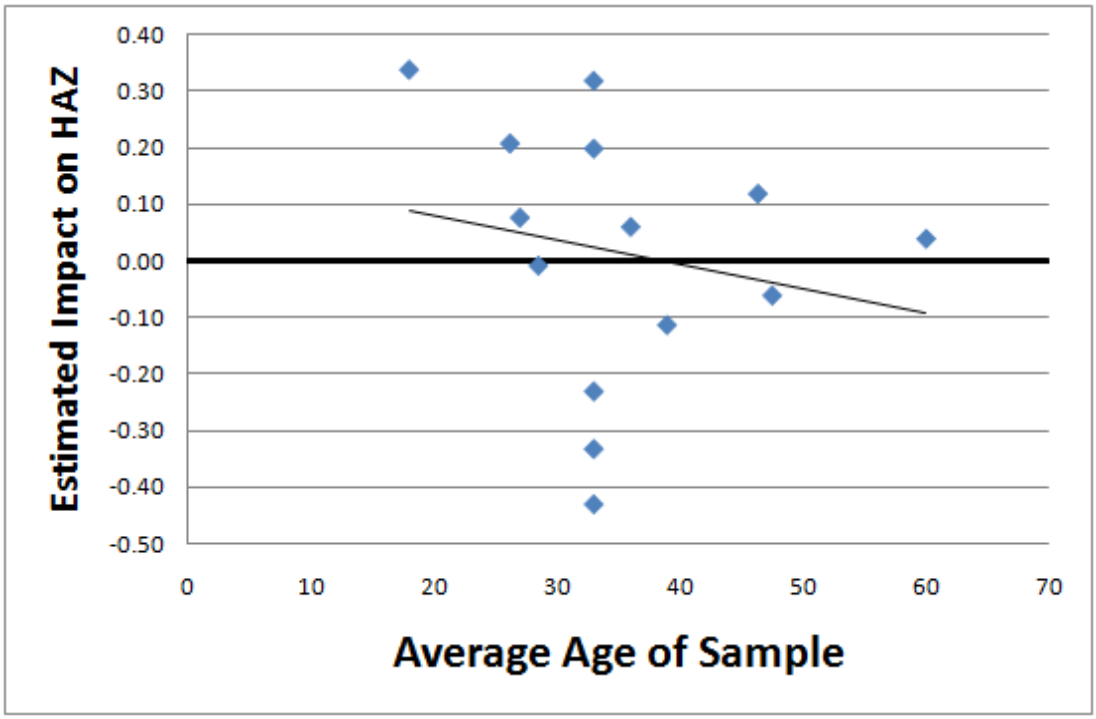


4.3.3 Child Covariates – Sex and Age groupings

We now examine the impact of cash transfer programmes based on the observed child's characteristics, summarized in Table 4.7. Not all studies of impacts examine how child characteristics interact with programme effects. However, six programmes analyse the sexes separately, while two more break out analysis just for girls. Most famously Duflo (2003) found larger impacts for girls from the South African pension programme. In addition to the pension programme we have separate estimates by gender for 6 programmes. On average girls see impacts that are 0.20 HAZ larger. A meta-regression with 12 observations (2 each for the six programmes) shows that the effect is marginally significantly different than zero (p-value = 0.052) suggesting the result is not unique to Duflo's findings. When we rerun the regression dropping one programme at a time, the results are consistent although statistical significance varies, strictly speaking. In no case do p-values exceed 0.15.

To estimate the influence of the age of sampled children we run two regressions. (In both cases we removed one outlier, the Primary Education Stipend. The average age in that sample is about 150 months, more than 2.5 times our second highest, and inclusion strongly affects all results.) In the first we include all programme impacts and use the average age of the sample to test the relationship between age and effect. Figure 4.8 shows the expected negative relationship between age and effect size but the slope is very slight (-0.004) and insignificantly different from zero. In the second regression we created a subsample of the 8 studies with multiple age cohorts. We compare the oldest and youngest cohort, finding that as the mean age rises one month it reduces the impact by .002 z-scores, although the result is not statistically significant (p-value = .17).

Figure 4.8 Age of sample and estimated impact on HAZ



4.3.4 Country Covariate Results

The final covariates of interest are the five WDI variables showing national health conditions. Table 4.8 summarizes our results. In each case, countries with worse health conditions and lower health care utilization have larger effects from cash

transfer programmes on height for age z-scores. “Worse health conditions” in these countries are defined as those with higher infant mortality, fewer hospital beds, a lower treatment percentage for children with respiratory infections, lower vaccine rates, and a lower prevalence of sanitation. Regressions on infant mortality and hospital beds per 1,000 both show statistically significant relationships consistent with countries with poorer health conditions seeing greater impacts. In both cases an improvement of one standard deviation is associated with a decrease in impacts of roughly 0.09 HAZ. The results are reasonably robust to eliminating any one programme, as the average effect size stays relatively constant and the maximum p-value is 0.12. The other three measures show results consistent with the hypothesis of larger effects in countries with poorer health, but the measures do not show signs statistically different from zero.

4.3.5 Correlation Between Covariates

One concern with limited data is that our measured covariates might be strongly correlated with other measured covariates. In this case we may incorrectly interpret the influence of a covariate when another covariate is influencing the effect. For example, we find no effect of health conditions or supplements, but if health conditions and supplements are more frequently implemented where better health infrastructure exists, this may bias the results downward. Due to our limited data we cannot control for correlation by multivariate analysis, but we can identify variables that are related. We ran correlations between all of the covariates, and for the most part most variables showed correlations below 0.6. We highlighted larger correlations in the appropriate sections, including the preceding discussions in 4.3.1.1 of sample age and supplement use, in 4.3.1.2 of conditionality types and country health indicators, and in 4.3.1.3 of transfer size and health conditions. One other pair of variables could bias the results.

Somehow a study’s being peer reviewed and local health conditions also have a positive correlation (the correlation with infant mortality is -0.45 and hospital beds per 1,000 people is .58). If impacts decline as local health conditions improve then this may diminish our estimate of peer reviewed articles.

5. Strengths and limitations

In this paper we analyse the impact of cash transfer programmes on height for age. Height for age is an important summary measure of health that reflects more than just food insecurity. It is reflective of access to food, the quality of that food, of access to health care, and of the health environment in which children live. In reflecting a variety of determining factors, it provides a larger picture view on an important issue: are cash transfers improving overall child health? Looking at height for age in this case is like looking at the bottom line. It tells donors investing in aid programmes whether children are seeing improvements in their health as a result of their investments, or whether other problems need to be solved before their investments can make a difference. The above section 1.2.1 has additional information and references detailing at greater length why nutritional status, particularly height for age, is an important lens through which to examine programme effectiveness.

That said, we recognize that programmes are designed with multiple ends in mind. Some programmes may not have achieved objectives as far as nutritional status, but they may have increased participant education rates and cut child labour, for example, other worthy goals.

This study has achieved several ends. First, by systematically going through the literature on cash transfer programmes, we identified many studies linking cash transfer programmes to nutritional status. This on its own is a useful accomplishment as it provides a foundation for further research. Second, we have applied meta-analysis to get a rough average of CT programme impacts. We found that on average programmes have a positive but insignificant effect on nutritional status, verifying previous researchers' observation that such programmes have inconsistent effects on child nutritional status. Third, we have identified several programme, child and study characteristics that are correlated with improved nutritional status. While any programme must consider first and foremost the specific circumstances under which it will go into effect, there are several indications from this analysis that may provide useful guidance.

Under the guidance of some of our reviewers and in fact contravening a suggestion by another reviewer that we further limit our scope, we went beyond the boundaries set by our initial proposal in numerous ways. We doubled the number of people-hours spent doing the search; we considered a greater variety of programmes, including the unconditional; we expanded the set of search terms; and we undertook a tightly focused review of the qualitative literature in areas that would enhance our dataset.

Even so, our focusing on the link between cash transfers and nutritional status necessarily prevented us from addressing some larger issues. Considering cash transfers as the cause and anthropometric outcomes as the effect ignores the context in which the cash transfer programme comes into existence, i.e., the causes that lead to the effect of a programme's implementation and sustainability. Some members of our advisory team had hoped we could address questions about the politics of programme adoption and continuation, including the roles of "fiscal space, political stability, political will and leadership, policy space, public acceptability, institutional capacity and co-ordination" in contributing to the feasibility and success of cash transfer programmes but with our chosen focus we could not oblige them. Leroy et al. (2009) note the difficulty and lack of data to address some of these issues.

We are similarly unable to answer other long-term questions posed by another reviewer, such as whether the improvements in educational participation caused by such programmes will result in improved child health in the next generation or whether communities as a whole will be enabled by these transfers to get out and stay out of poverty. Although the accumulation of human capital is a necessary step toward escaping poverty, it is likely not a sufficient condition. An enumeration of the set of other factors necessary to making that escape are certainly central concerns to the development community, but that is unfortunately beyond the scope of the current project. A systematic review would be dependent on the existence of primary research linking such issues to programme impact, duration, and sustainability and we have seen little of the type of long-term analysis that would make this possible. Perhaps only Fernald, Gertler, and Neufeld's (2009) work looking at the Mexican programme Oportunidades after ten years has a sufficient perspective to enable the analysis in question.

Our study constitutes a vital first step toward this type of analysis, however. If we wish to understand the relationship between cash transfer programmes and nutritional outcomes, we must first identify cases in which clear impacts have been found, and the circumstances which contribute to high marginal returns on investment. In particular, identifying the conditions under which cash transfers result in quantifiable improvements in nutritional status is an important starting point. Qualitative studies observing increases in consumption or improvements in dietary diversity may note that steps are being taken in the right direction, but it is only by looking at the key outcome of nutritional status that we see if potentially lasting change is being effected. For example, we have seen that although transfers are often linked to consumption of more and better food, this increased consumption does not always translate to improved growth. In these cases, increasing consumption alone is not sufficient since another constraint on child development appears to be binding. Getting data on the bigger picture outcome of height for age allows us to make more confident statements about programme effectiveness, a necessary precondition to evaluating the factors associated.

The electronic search strategy was limited to a range of short phrases, without using wild cards, so could not guarantee to capture all relevant studies. This limitation may have been offset by the wide range of other sources searched, however the overall strategy cannot be described as exhaustive or as definitely identifying a comprehensive body of literature. The findings of this review are therefore indicative rather than conclusive.

A final strength and limitation is the type of analysis we adopted. Our choice to use statistical analysis as our main means of summarising the information we gathered has the advantage of limiting the subjectivity of the undertaking to some degree (Mann 1990). It also requires grouping of somewhat disparate programmes under one label. This is a strength in that grouping programmes for joint analysis enables us to see broad trends. It is a limitation in that a qualitative review might reveal idiosyncratic quirks that limited programme effectiveness in specific cases: every quantitative study of human activity ever performed has by necessity relegated some factors to the error term in its analysis, and ours is no exception. Qualitative analysis might do a better job telling a causal story than we can with our correlation and speculation. Again, though, we see this paper as laying important groundwork upon which qualitative analysis may be able to productively build in the future.

6. Conclusions and recommendations

We began by noting the diversity of previous researchers' observations on the link between cash transfer programmes and the improvement of child nutritional status, a fundamental component of human capital. While we cannot claim to disentangle all the causal pathways, we are able to statistically tease out a few key pieces for evaluation. Some of our observations such as apparent evidence of enhanced effects on girls, the counter-effectiveness of conditionalities such as work program, and signs of high effectiveness in areas with less developed health infrastructure provide useful hints.

6.1 Review of and response to hypotheses

Chapter 1 began by summarizing the background literature including nine systematic reviews looking at the relationship between CCTs and a variety of outcomes. Based upon those and other contributions, we synthesized that information into ten hypotheses listed in section 1.5. In this chapter we will move through the list, discussing the results of our analysis as pertains to each point, before drawing some final conclusions and summarizing policy implications.

1) Do community characteristics (such as supply side service availability and quality, food availability and quality, and water availability and quality) constrain programme effectiveness? Is it possible to identify which characteristics are the most relevant?

   We were able to find information on community characteristics for only a relatively small subset of our programmes, so we brought in contemporaneous data from the World Bank's World Development Indicators. Using this data we looked at five factors: infant mortality rates, sanitation, and three indicators of access to health care (hospital beds per unit population, share of children with respiratory infections who get treated, and the share of children receiving the DPT vaccine). While these country-level measures are necessarily imprecise, two were statistically significant. Households in programme areas with high infant mortality rates benefitted more, as did households in areas with fewer hospital beds.

   These findings fit with the recent (September 2010) findings published by economic modellers at UNICEF (UNICEF 2010), who find that returns to investments in child health are highest in remote rural areas for three reasons. First, remote populations generally have a larger proportion of children than other groups due to higher fertility rates. Second, in remote areas a higher proportion of children die due to preventable or treatable conditions. Third, people in these locations tend to have lower coverage levels of highly cost effective interventions. Thus, investments may be most cost effective in these areas.

2) Do covariates such as household size and child age constrain effectiveness? Are programmes less helpful to large families? Do they help younger and older children equally?

   We find that impact evaluations looking at younger subgroups of children in recipient households tend to observe greater increases in height for age, though the effect is not significant. Identification was made difficult by a correlation between programme use of nutritional supplements and having an older average age in the sample. We were not able to distinguish the

role played by household size, as virtually all studies we found had controlled for that in their analysis, removing the effects of the variable.

3) Does programme effectiveness in encouraging use of health care correlate with programme effectiveness in improving child nutritional status?

   Fiszbein and Schady (2009) say, "CCTs have increased the likelihood that households will take their children for preventive health check-ups, but that has not always led to better child nutritional status…." (p. 3). We found that health-based conditions did indeed lead to increased gains in nutritional status, though we also found that unconditional programmes could be effective in increasing nutritional status. This also highlights a potential danger: making transfers conditional on non-health related behaviour has a clear, negative effect on child development.

4) Does programme effectiveness in increasing food diversity or at least in increasing household food consumption expenditures translate to improved child nutritional status?

   While cash transfers necessarily imply an increased level of income and make increases in overall consumption expenditures all but certain, again it is clear that this alone has not translated to improvements in nutritional status. Taking this a step further, Table 3.2B shows that almost every programme was associated with increased food consumption and/ or food diversity, a positive development. However, we see no effects on nutritional status. Clearly improved access to food alone is not sufficient to improve nutritional status.

5) Does conditionality of any kind predict increases in child height for age?

   While comparison of conditional to unconditional programmes is difficult given the many other programme characteristics that vary simultaneously with the conditionalities, we found that overall conditional programmes were not much different from unconditional programmes. Unconditional programmes on average significantly improved child nutritional status by a small margin, while conditional programmes on average had a small, statistically insignificant negative effect. Nonetheless we cannot rule out the possibility that the effects of conditional and unconditional are not significantly different, as the 95% confidence intervals for the two estimates overlap. The type of condition is very important, as conditions not related to health or education can have negative effects on child nutritional status.

   Others have found that conditions can hurt food availability or nutrition, such as Gitter et al. (2010) and de Janvry et al. (2006). Also, misunderstanding conditionalities can have adverse implications as described by Gaarder, Glassman and Todd (2010)regarding cases in Honduras, Turkey and Brazil. Finally, a recent study by Baird et al. (2011) is one of the first to compare conditional to unconditional transfers directly. They do not consider anthropometrics but show that conditional programmes are more effective at getting kids to school. However, conditional programmes also have negative side effects, and they recommend unconditional programmes for some purposes.

6) If conditionality matters, what types of conditions matter? Does conditioning payments on maternal participation in educational talks correlate with increases in child height for age?

   Splitting out conditional programmes based on their types of conditions led to an important observation. Programmes imposing health-based conditions

are on average associated with improved nutritional status, though not significantly and less effectively than the unconditional programmes we found information on, while non-health-based conditions are associated with significant declines.

In-depth qualitative research might be able to tell a more complete story or address the issue of maternal participation in educational talks. It is worth noting that Fernald et al. (2009) find in Mexico that money received rather than length of participation explains child development outcomes like nutritional status.

A final note here is that although the issue of unconditional vs. conditional transfers has dominated much of the discussion of the value of CTs, conditionality appears to be much less important than a number of other issues, such as the age and sex of the children in the household and access to health care. More effort should be made to identify other correlates of successful programmes.

7) Does the provision of food supplements correlate with improved child nutritional status?

   Evidence on this point is ambiguous. A small positive association is found, but it is statistically indistinguishable from zero. One probable reason for this result is a relatively high correlation with an older sample.

8) Does total payment size matter? Is per capita payment size the most relevant way to analyse this? How about payment timing (i.e. every 2 months, during certain seasons only, etc.) or modality (i.e. cash, bank transfer, etc.)

   Rather than clear effects of transfer size, we see only reduced variation. Overall we do see a positive trend, implying that more is better, but it is not statistically different from zero. The effect of increasing transfers is not strong, but less variation in effects is observed with larger payments. We have not yet been able to analyse timing or modality.

   One important factor is the correlation between transfer size and health conditions. Areas with lower infant mortality rates and more hospital beds received higher transfer amounts. This effectively confounds our analysis: since marginal impacts are higher in areas of poorer health infrastructure, where there are also lower payments, it appears as though transfer amounts do not matter. We lack sufficient variation in the data to address the topics separately.

   This lacuna is one of the areas crying out for further examination. The only programme we are aware of that randomised transfer sizes is Baird et al. (2009), and they found that transfer size made at best a limited impact when it was targeted to young women rather than to households. They deem response to the programme “relatively insensitive” to payment size.

9) How do the means of analysis affect the estimated impact?

   We examine several possible means through which study type might affect the observed outcome. We assess study quality using an index we created (and which was later endorsed by 3ie) to look at potential for various biases, and find that lower quality works by this measure tended to be more variable in their estimations of effect. Other measures, such as peer reviewed studies, baseline z-scores, and sample sizes do not have statistically significant effects in our sample. We conclude that due

attention should be paid to study quality to increase confidence in observed outcomes.

10) Are programmes more successful if they improve access to high quality services? Our estimates show high marginal effects in the least developed settings. This tells us only that these areas need the most help, and benefit from it the most. Our analysis is not designed to answer the larger question of whether cash transfers are more or less effective than the provision of large scale public goods such as improved water or access to medical care.

6.2 Policy recommendations

The first crucial realization we hope policymakers will take from this study is that although cash transfer programmes are a valuable tool for reaching the poor, they are not guaranteed to achieve any particular goal. Previous studies have shown that using cash transfers to incentivize education and health works in that investments in education and health increase, but outcomes on child nutritional status from cash transfer programmes vary widely depending on circumstances. On average effects are positive but statistically indistinguishable from zero, and the conditions, including the country characteristics, recipient population characteristics, and the programme characteristics all matter. Thus, careful tracking and impact evaluations should continue to be part and parcel of such investments.

Poorly designed interventions can be damaging: in particular, making payments conditional on activities other than health and education can be costly in terms of child nutritional status. On the other hand, the unconditional programmes we found had on average a significant, positive effect. Programmes conditioning payments on health-related activities fall somewhere in between: lack of precision in our estimates means that effects could be comparable to those of unconditional programmes, a finding similar to that of Baird et al. (2009).

Next, we observe that targeting remains an important task. People in unfortunate disease environments and with poor access to health care, particularly as measured by poor infant mortality rates and lower access to hospital beds, are more likely to make strong gains from cash transfers. Improving the quality of health care is no doubt a complementary intervention, but waiting for clinics to appear or improve before beginning transfers seems unnecessary. As to whether cash transfers or investments in clinics are preferable, we cannot say.

Previous works have shown that CCT programmes improve vaccination rates and health care utilization, which may be behind improvements in nutritional status. We believe that this observation is an important counterpoint to the contention made by many such as Fiszbein and Schady (2009) and Bassett (2008) that a high quality infrastructure on the supply side is a necessary complement to an effective CT programme. A combination of both supply side and demand side interventions sounds like a sure recipe for success particularly in places that have already met basic challenges such as immunization and sanitation.

Finally, as noted by a reviewer it is important to note that the ambiguity of many of our conclusions stems from the paucity of evidence from which we have drawn our data. It is difficult to identify effective practices when few relatively few impact evaluations are available. More pilot programmes are beginning: Barrientos et al. (2010) identify over 100, and opportunities for data collection are legion. Once more data is available, precision of estimates in papers like this one will improve, making evidence clearer.

6.3 Final word and areas highlighted for future research

The potential for success of unconditional cash transfers and the potential for heightened improvements in nutritional status in environments with inadequate health care is particularly compelling in spite of some doubt about the extent to which conditionalities would be, in an anonymous reviewer's words, "operationally feasible in LIC contexts given administrative capacity, supply of services, and risk of increasing costs and programme complexity." The efficacy of CCTs in bringing families in for health care and children into school is well established (Fiszbein and Schady 2009) and we find that conditioning on health improves nutritional status.

Perhaps more importantly, CCTs may be more palatable politically, though they are of course more costly economically. As the reviewer concludes, "There are many other reasons (not related to impact) for and against the use of conditions and their appropriateness is likely to be very context specific." It is likely that at least some implementation of conditionality in cash transfer programmes is for political economic reasons. Many donors (including taxpayers) may find it more palatable to support programmes if they believe that programme recipients are constrained not only by material resources but also by extremely high discount rates or a dysfunctional ideology that requires intervention.

We also echo the findings of Bouillon and Tejerina (2007) who remind us that, "Timely and well-designed impact evaluations of key interventions are indispensable to inform policy" (p. 97). Much work has already been done, and this paper has relied upon the efforts of those who have designed and performed previous such evaluations. While we mentioned above the possibility for further research investigating current programmes through a deep engagement with the qualitative literature, there is also a continued need for more quantitative impact evaluation. Dozens of programmes are currently operating, but we were only able to identify impact evaluations for a small minority of them. Of the impact evaluations that we found, many failed to include nutritional status. Given the relative ease of implementing anthropometric evaluation, this should change. The more programmes that are analysed, the deeper the evidence base we will have from which to proceed as we formulate future policies. Hopefully the availability of new NGOs such as 3ie and the increasing accessibility of impact assessment methodologies will increase the share of programmes that are evaluated both quantitatively and qualitatively.

7. References

7.1 Studies included in review

(note that Agüero et al. and IFPRI are not in the main body of the text as they did not report standard errors)

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2. Ahmed, A.U., et al., *Comparing Food and Cash Transfers to the Ultra Poor in Bangladesh*. 2009, International Food Policy Research Institute, Research Monograph 163: Washington, DC. p. 1-250.

3. Attanasio, O., E. Battistin, E. Fitzsimmons, A. Mesnard, and M. Vera-Hernández, *How Effective are Conditional Cash Transfers? Evidence from Colombia, Briefing Note No. 54*. 2005, The Institute for Fiscal Studies: Washington, DC. 10 p.

4. Attanasio, O., L. Gómez, P. Heredia, and M. Vera-Hernández, *The Short-Term Impact of a Conditional Cash Subsidy on Child Health and Nutrition in Colombia*, Centre for the Evaluation of Development Policies Report Summary: Familias 03, The Institute for Fiscal Studies: Washington, DC. 15pp.

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7. Behrman, J. and J. Hoddinott (2005). "Programme Evaluation with Unobserved Heterogeneity and Selective Implementation: The Mexican PROGRESA Impact on Child Nutrition." Oxford Bulletin of Economics and Statistics 67(4): 547-569.

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Appendices

Appendix 1.1: Authorship of this report

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Conflicts of interest

Both primary reviewers have published on the topic. In addition, Manley was for four years involved with data analysis of the Oportunidades programme that culminated in the Fernald, Gertler, and Neufeld papers published in 2008 and 2009. Manley and Gitter have a working paper looking at the effectiveness of the Honduran, Nicaraguan, and Mexican CCTs in protecting children's nutritional status from the coffee shock in 2000-2002, a paper that is included in the review.

Appendix 2.1: Summary of the database searches by date

All searches were carried out on a computer running Windows Vista and Google's Chrome browser.

Database	Date
EconLit	07/29/10, 08/03/10
PsychInfo	07/30/10, 08/03/10, 08/04/10
PubMed, Science Direct, JSTOR, Eldis	07/31/10, 08/04/10
Google Scholar	08/06/10, 08/11/10, 08/13/10, 08/14/10, 08/15/10, 08/16/10
Inter-Science, JOLIS	08/02/10, 08/04/10
Medline	07/30/10, 08/04/10
SIGLE, Cochrane Central Register of Controlled Trials	08/03/10
The Database of Abstracts of Reviews of Effectiveness, IDEAS [Repec]	08/04/10
SSRN	08/02/10, 08/03/10, 08/05/10
Dissertation: proquest.umi.com	08/05/10
WorldCat	08/26/10
World Bank	08/26/10, 08/28/10
Popline, Cab Direct, Ovid.com, WHOLIS, British Library for Development Studies, LILACS, MEDCARIB, ADOLEC, PAHO, EPOC, SSCI	08/16/10 - 08/26/10
DFID	09/01/10
IFPRI, Chronicpoverty.com, Odi.org.uk, Ntis.org, ILO, Center for Global Development	09/02/10

Appendix 2.2: Complete list of search terms

In each of the above databases, the following 35 searches were performed. Each line contains the complete and exact text entered into the search field of the database.

"cash transfer" and "height and child"

"cash transfer" and "nutritional status"

"cash transfer" and "child growth"

"cash transfer" and "anthropometric"

"cash transfer" and "child weight"

"social safety net" and "height and child"

"social safety net" and "nutritional status"

"social safety net" and "child growth"

"social safety net" and "anthropometric"

"social safety net" and "child weight"

"family allowance program" and "height and child"

"family allowance program" and "nutritional status"

"family allowance program" and "child growth"

"family allowance program" and "anthropometric"

"family allowance program" and "child weight"

"child grant" and "height and child"

"child grant" and "nutritional status"

"child grant" and "child growth"

"child grant" and "anthropometric"

"child grant" and "child weight"

"child support grant" and "height and child"

"child support grant" and "nutritional status"

"child support grant" and "child growth"

"child support grant" and "anthropometric"

"child support grant" and "child weight"

"social transfer" and "height and child"

"social transfer" and "nutritional status"

"social transfer" and "child growth"

"social transfer" and "anthropometric"

"social transfer" and "child weight"

"social assistance" and "height and child"

"social assistance" and "nutritional status"

"social assistance" and "child growth"

"social assistance" and "anthropometric"

"social assistance" and "child weight"

Appendix 3.1: Details of studies included in the review

Appendix 3.1.1 Agüero, Carter and Woolard: South Africa

Author: Jorge Agüero, Michael Carter and Ingrid Woolard

Title: "The Impact of Unconditional Cash Transfers on Nutrition: The South African Child Support Grant"

Objective: The objective of this paper is to examine the impacts of South Africa's unconditional Child Support Grant (CSG) on the nutritional status of children as measured by height for age. The authors use the slow programme roll-out which led to an exogenous variation in exposure to the intervention to analyse the impact of various transfer "dosages" on child growth.

Programme details: In 1998 CSG officially replaced State Maintenance Grants which were ineffective in reaching the eligible children. CSG transfers were made fully unconditional and did not include any in-kind transfers either. The take up rate in 1999 was low, and hence means tested eligibility requirements were changed. By September 2004 about half of age-eligible children were receiving the grant. The initial monthly benefit was R100 in 1998, yet it is currently R180. During the time of the survey used in the paper, the monthly transfer was R170 which was approximately equal to US$25 using the market exchange rate (or, PPP US$50). Since the grant must be paid to an adult and in particular, the person to whom the grant is paid must be the primary care giver of the child, this programme indirectly targets mothers.

Evaluation Methodology: CSG is a nation-wide programme which was unintentionally rolled slowly but did not follow a random design. The authors suggest that some eligible households in urban areas may have received the information earlier that households in rural areas. At the time of the survey, some of the children under 36 months have been in receipt of the benefits for their whole lives while others have been covered only for a fraction of their lives. The data used for the analysis includes both baseline survey data from 1993 and follow-up data from the KIDS survey for 2004.

Findings: Controlling for caregivers' "eagerness" to apply for the benefits as well as other family and child characteristics, the authors find that large dosages of CSG treatment early in life have significant positive impact on child height for age.

Appendix 3.1.2 Ahmed, Quisumbing et al.: Bangladesh

Author: Akhter Ahmed, Agnes Quisumbing, Mahbuba Nasreen, John Hoddinott, and Elizabeth Bryan

Title: "Comparing Food and Cash Transfers to the Ultra Poor in Bangladesh"

Objective: This lengthy study examines the relative effectiveness of food and cash transfers in Bangladesh with a particular focus on four interventions: Income-Generating Vulnerable Group Development programme (IGVGD), Food Security Vulnerable Group Development programme (FSVGD), Food for Asset Creation (FFA) component of the Integrated Food Security (IFS) program, and the Rural Maintenance Program (RMP). The authors look at how well both food and cash transfers were delivered; which type of transfer was preferred by beneficiaries; how well transfers were targeted; the effects of the transfers on food security, livelihoods, and gender-related outcomes; and the cost effectiveness of the transfers. IGVGD was fully in-kind transfer programme and hence it is not in the scope of the current study.

FSVGD details: The FSVGD project started in July 2001 and ended on December 31, 2006. It included both cash and in-kind transfers. The food provided by FSVGD was almost entirely micronutrient-fortified *atta* (whole-wheat flour).The benefits constituted of15 kg *pusti atta* plus Tk150 per month. The food rations varied, mainly because of the irregularities in the *atta* milling and fortification process. This programme targeted women. The benefits were not unconditional but they were distributed irregularly. One requirement for programme participation was that 32 Tk/month per beneficiary had to be saved. In addition, beneficiaries had to take part in a skill development and awareness raising training. There was no work requirement.

FFA details: FFA also distributed a half of the designed benefits in food and half in cash as wage payments to workers in labour-intensive public works programs. During the work season the transfers constituted of 2 kg rice or wheat plus Tk15 per work day. During the training season the benefits were 20 kg rice or wheat plus Tk100 per month. However, cash transfers were not regularly distributed. Food and cash for work were usually provided during the months of December to May, which was the appropriate time for moving earth. Training in awareness-raising and was carried out from June to November. Both men and women could participate in this programme, yet at least 70 per cent of the participants had to be women. The work under FFA normally focused on building of community infrastructure and assets. FFA had two additional requirements: attendance of skill development and awareness raising training and mandatory savings of Tk25 per month.

RMP details: Participants receive cash wages for maintaining rural roads. The wage consists of Tk 51 per day for 30 days a month and hence the participant is entitled to receive a monthly salary of Tk 1,530, or Tk 1,230 after the deduction of mandatory savings. The cash transfers were irregularly distributed. The programme was targeted at women only. RMP had three main conditions: work requirement; mandatory saving of Tk 10 per day, or 300 Tk/month; women had to receive counselling to help them understand and establish their rights and improve their health and nutrition and that of their families.

Evaluation Methodology: Since programme beneficiaries were not randomly selected into treatment and control, the authors use propensity score matching to evaluate the impact. There is no baseline information on the participant.

Findings: An important conclusion of the study is that most of the programmes provided only temporary poverty-alleviation impacts. Using PSM techniques, the authors find that children 6-60 months belonging to the IGVGD, FSVGD, and RMP households have better nutritional outcomes, as assessed by height for age, weight for height, and weight for age, than do those from matched control households. However, these differences are not statistically significant.

Appendix 3.1.3 Attanasio, Gómez et al.: Colombia

Author: Orazio Attanasio, Luis Carlos Gómez, Patricia Heredia, Marcos Vera-Hernández

Title: "The short-term Impact of a Conditional Cash Subsidy on Child Health and Nutrition in Colombia"

Objective: The purpose of the study was to evaluate the impact of the Colombian CCT Familias en Acción. In particular, the authors look at how the programme affected the nutrition and health-related outcomes for children in the short term, about a year after start of the intervention.

Programme details: Familias en Acción started in 2001/02, but the study reports that at baseline data June-Sept. 2002 none of the treated households had yet received transfers. This programme was conditional and required that: 0-6 year old children have regular growth and development check-ups; mothers attend courses on hygiene, vaccination and contraception; and children aged 6–17 at least 80% of school lessons. Beneficiary families with children under the age of 5 were entitled to receive a basic nutritional subsidy (about US$15 per month); eligible households with children aged 6–17 received a separate monthly grant per child approximately US$8 for children attending primary schools and about US$16 for children attending secondary school. The transfer was equivalent to about 24-30 per cent of baseline household income according to various studies (Leroy et al. (2007), Lagarde et al. (2007)). The take up of the programme was roughly 90 per cent. It is unclear from the existing studies whether monitoring was effective and conditionalities strictly enforced, although Gaarder et al. (2010: 24) does suggest that compliance was between 94 and 99 per cent. As most similar conditional cash transfer programmes, FA was targeted at women.

Evaluation methodology: A treatment level was assigned at the municipality level, not at the household level. That is why the effect estimated in the paper is interpreted as the intent to treat. Since the authors have access to baseline and follow-up data, they utilize difference-in-difference methods. To qualify as a beneficiary community, that community needed to have a bank (Attanasio, Battistin et al. 2005); however, in selecting the control communities for the evaluation, most of the controls were similar to the treated communities except that they did not have a bank. This may actually be a potential source of difference and affect the estimates.

Findings: The study finds out that the programme improved significantly the height for age z-score for children under 24 months (0.161 [standard error 0.085]). This is equivalent to 0.43 centimetres for a 12-month-old child. The authors also conclude that probability of being chronically undernourished for the same group of children was reduced by 6.9 per cent (-0.069 [standard error 0.034]). However, the study concludes that the programme had no statistically significant effect on older children (more than 24 months of age) in terms of nutritional status.

Appendix 3.1.4 Baulch: Bangladesh

Author: Bob Baulch

Title: "The Medium-Term Impact of the Primary Education Stipend in Rural Bangladesh"

Objective: The purpose of this paper was to estimate the long-term impact of Bangladesh's Primary Education Stipend (PES) programme on a variety of individual and household welfare measures. The study used a unique longitudinal dataset spanning the years 2000 to 2006.

PES details: In mid-2002 the PES program replaced the existing Food for Education (FFE) intervention in Bangladesh. The programme remained an education related intervention focusing on primary-school age children who had to attend 85% of classes in order to receive the transfers. The transfer included consisted of BDT100 per month for one child (about US$1.76) and BDT125 per month for more than one child. An important problem of the programme was that the transfer payments were not adjusted for inflation and hence the purchasing value of the transfers has decreased substantially since 2002. The stipends were disbursed on a quarterly basis to authorized parents/guardians on predetermined dates at the local bank branches or at temporary distribution points ("camps") established within 5 kilometres of the school. The transfers were given to students' parents or legal guardians on presentation of bank-issued identity cards (mothers were preferentially issued these identity cards) (Baulch 2010).

Evaluation Methodology: Since PES was implemented on a nationwide scale, experimental estimation techniques could not be used. For that reason, the author uses a quasi-experimental or observation approach such that comparable treatment and comparison groups are constructed using covariate and propensity score matching. Some problems with the evaluation are explicitly noted by the author: first, for households to be eligible for the transfer, they had to have at least one primary-aged child and hence in the process some households shifted from eligible to ineligible; second, PES replaced the rather similar FFE and thus the impact of PES was likely to be measured against the impact of the previous programme, rather than against not having PES or FFE; yet, the study attempts to resolve these problems.

Findings: Using covariate and propensity score matching and difference-in-difference techniques, the author finds that the programme had small impacts on school enrolment, household expenditures, calorie consumption, and protein consumption. However, PES had a significant and positive effect on girls' height for age and boy's body mass index. Yet, these impacts are deemed rather small given the size of the programme. The author attributes the inadequate programme effects on poor targeting and the failure to adjust the grant for inflation which led to the substantial decline in its real value.

Appendix 3.1.5 Duflo: South Africa

Author: Esther Duflo

Title: "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa"

Objective: This study evaluates the impact of the old-age pension in South Africa on children's nutritional status. The author assesses whether the gender of the recipient affects the results.

Programme details: The old-age pension scheme in South Africa became fully racially non-discriminatory in 1993, although its expansion started at the end of 1991. The pensions are given to women over age 60 and men over age 65, subject to a means test. These transfers are unconditional. In 1993, 60 % of eligible men in (the studied sample) and 77% of eligible women were receiving pensions. Monthly benefits in 1993 were R370 (1993 Rands), while monthly per capita household income of Africans in the sample in 1993 was about R149. The variety of payment methods are not discussed in detail, but it is mentioned that in some provinces (such as KwaZulu Natal) the pensions are distributed through mobile pay points equipped with automated teller machines with a fingerprint recognition system.

Evaluation Methodology: Duflo's (2003) evaluation of the impact of the pensions on child growth is based on a national survey carried out in the last five months of 1993. Since the programme is country-wide, experimental approaches are unfeasible. However, the author uses both OLS and instrumental variable approach to identify the effect of the transfers on child growth.

Findings: The study finds that pensions received by grandmothers have a significant positive effect on girls' nutritional outcomes as assessed by height for age and weight for height, but little effect on boys' nutritional outcomes. Pension transfers received by men were not associated with any impact on children's nutritional status. Thus, the gender of the recipient was a significant determinant of children's nutritional status.

Appendix 3.1.6 Fernald, Gertler, and Neufeld 2008: Mexico

Author: Lia Fernald, Paul Gertler, Lynnette Neufeld

Title: "Role of cash in conditional cash transfer programmes for child health, growth, and development: an analysis of Mexico's *Oportunidades*""

Objective: This study examines the impact of the cash component of Oportunidades on children's health, growth and development. The authors use the doubling of cash transfers as the explanatory variable of interest. The study is restricted to a sample of children who had been *Oportunidades* beneficiaries their whole lives, however the household's length of time in the programme varies and hence the cumulative amounts of the transfers that households have received are different. The data for this study includes both a baseline survey from 1997 and follow-up survey carried out September-December 2003.

Programme details: PROGRESA (later renamed to Oportunidades) was instituted in 1997 in rural areas, and in 2002 it was expanded to the urban areas. This paper focuses on the CCT impact in the rural communities. PROGRESA/Oportunidades transfers were conditional on regular health clinic visits for growth monitoring, preventive care and regular immunizations for children 0-5 year old; on all household members' having annual health check-ups; and pregnant and lactating women's visits to the health clinics for pre- and post-natal care. In addition, women had to attend monthly meeting (platicas) at which health and nutrition issues and practices were discussed by trained physicians and nurses. About 90 per cent of eligible households enrolled in the programme and only about 1 per cent were denied benefits for failing to comply with the conditions. PROGRESA distributed both cash transfers and nutritional supplements. Micronutrient-fortified supplements in different amounts were given to all 6-24 month old children, to all 24-60 month olds with low weight for age, and to pregnant and lactating women. However, some studies have raised the concern that the allocation of these in-kind transfers was not random and children in control areas could also receive the

nutritional supplements (Lagarde et al. 2007). Lagarde et al. (2007) reports that the mean transfer amount was $20 US/month, $13 per family, $8-$17 per primary school child, $25-$32 per secondary school child, $12-$22/year for school supplies. This constituted about 25% of baseline income for rural areas and about 15-20% of baseline income in urban areas. Fiszbein and Shady (2009) report that the main payment modality in Mexico is a fairly low-tech "Brinks truck," yet, in urban areas payments through banks and electronic cards are becoming common. Payments are distributed to women every two months.

Evaluation Methodology: The implementation of PROGRESA in the rural areas followed a random design; that is, some households were randomly assigned to receive the benefits while others were placed as controls and hence delayed the benefits for about 18 months. Data at both baseline and later in the programme was collected. However, the purpose of this study was to evaluate the cumulative impact of the transfers on child growth and development, and hence the authors restrict the sample to children who have been beneficiaries their whole lives. Thus, the study does not examine the programme effect between control and treatment groups. The authors use linear and logistic regressions, adjusted for sampling design and clustering and regress the outcome variables against the cumulative cash transfers and other covariates.

Findings: The study finds that doubling of cash transfers lead to higher height-for-age *Z* score (β 0.20, 95% CI 0.09–0.30; p<0.0001), lower prevalence of stunting (–0.10, –0.16 to –0.05; p<0.0001), lower body-mass index for age percentile (–2.85, –5.54 to –0.15; p=0.04), and lower prevalence of being overweight (–0.08, –0.13 to –0.03; p=0.001). These higher cash transfers were also linked to both better motor development and cognitive development.

Appendix 3.1.7 Fernald, Gertler, and Neufeld 2009: Mexico

Author: Lia Fernald, Paul Gertler, Lynnette Neufeld

Title:"10-year effect of Oportunidades, Mexico's conditional cash transfer programme, on child growth, cognition, language, and behaviour: a longitudinal follow-up study"

Objective: The study examines the impact of Oportunidades on children's growth, cognition, language and behaviour 10 years after the start of the programme.

Programme details: PROGRESA/Oportunidades is described above.

Evaluation Methodology: Using cluster-adjusted t-tests and multivariate regressions, the authors analyse the effects of programme participation on height-for-age, body-mass index (BMI), and cognitive language and behavioural assessment scores on children who were assigned to treatment at the start of the programme (early enrolment) versus children who were initially assigned as control and did not start receiving benefits until 18 months after the start of the CCT (late enrolment). The baseline data for the analysis is from April 1998 and the follow-up survey was carried out in the end of 2007.

Findings: The study finds that early enrolment reduced behavioural problems for children in the early versus late treatment group (mean behaviour problem score –0.09 [SD 0.97] versus 0.13 [1.03]; p=0.0024). However, the authors identify no programme effect on mean height-for-age Z scores (–1.12 [0.96] for early enrolment versus –1.14 [0.97] for late enrolment; p=0.88), no effect on BMI-for-age Z scores (0.14 [0.99] for early enrolment versus 0.17 [1.06] for late enrolment; p=0·58), no impact on assessment scores for language (98.8 [13.8] early enrolment

versus 98.4 [14.6] for late enrolment; p=0.90) and no impact on cognition (98.8 [12.9] versus 100.2 [13.2]; p=0.26). An additional 18 months of treatment before age 3 years for children who were ages 8–10 years at follow-up and whose mothers had no education was associated with an increased in child growth of about 1.5 cm measured as height-for-weight Z score (β 0.23 [0.023–0.44] p=0.029), independently of cash received.

Appendix 3.1.8 Gitter, Manley, & Barham: Mexico, Honduras, & Nicaragua

Author: Seth Gitter, James Manley and Brad Barham

Title: "The Coffee Crisis, Early Childhood Development, and Conditional Cash Transfers"

Objective: The objective of this paper was to analyse the efficacy of the CCTs in Honduras, Mexico, and Nicaragua in mitigating the likely negative effects of an income shock caused by the "coffee crisis" in Latin America. The authors present a theoretical household model which illustrates both the mechanisms through which CCTs can mitigate negative shocks effects on early childhood development and the mechanisms through which CCTs can actually exacerbate the impacts of a negative shock on early childhood development if the conditionality induces households to redistribute resources from younger to older children in order to maintain the school attendance requirement.

Programme details: PROGRESA details are given above, while PRAF and RPS details follow.

Evaluation methodology: All three programmes included randomized control and treatment groups, as well as baseline and follow-up data. As a result, the authors use difference-in-difference estimation technique to assess the programme impact on child height for age. However, the time span of the study is such that an exogenous economic shock affected some of the communities and in fact, the main focus of the paper is the role of CCTs in the context of negative income shocks. Thus, the authors also use triple difference to capture the heterogeneity of programme impact in coffee growing versus non-coffee growing areas.

Findings: The paper concludes that in Mexico the CCT mitigated the negative shock on child height-for-age z-scores, while the opposite was observed in Nicaragua where children in CCT-receiving coffee producing households saw greater declines in their anthropometric outcomes.

Appendix 3.1.9 Himaz: Sri Lanka

Author: Rozana Himaz

Title: "Welfare Grants and Their Impact on Child Health: The Case of Sri Lanka"

Objective: The goal of the paper was to evaluate whether an exogenous increase in income such as from a poverty alleviation program had an impact on child anthropometric outcomes. The study focused on the Samurdhi Program in Sri Lanka.

Programme detail: This programme started in 1995. It consists of three simultaneous components: a monthly income transfer to poor households designed to raise nutritional status, and welfare; a group savings and credit component which is meant to reduce the vulnerability of the poor; and a low-budget rural infrastructure development scheme which should help mitigate village-level

bottlenecks in physical capital. It is important to note that a household which receives Rs 500, for example, is expected to contribute four to five man-days of labour to community projects. This is a type of conditionality since ''voluntary'' labour contribution is a mandatory requirement. The average grant size was Rupees 365.10 a month, which constituted roughly 25% of the monthly per capita household income of a grant-receiving household. The cash transfers are distributed using the existing co-operative system. This programme does not explicitly target women.

Evaluation Methodology: The Samurdhi Program is a national poverty alleviation scheme, and hence randomization was not used for selecting the beneficiaries. As a result, the author uses propensity score matching to evaluate the programme impact on child health. The data for the main empirical analysis is from a one cross-section household survey.

Findings: Using propensity score matching, the author calculates that Samurdhi increased the height-for-age z-score of a child from a beneficiary family by about 0.40 standard deviations compared to a similar child who did not receive the grant. This impact was mainly driven by the programme effect on children between 6 and 36 months of age. Samurdhi was also associated with improved weight-for-height z-scores by around 0.45 standard deviations for children aged 36–60 months.

Appendix 3.1.10 Hoddinott and Bassett: Mexico

Author: John Hoddinott and Lucy Bassett

Title: "Conditional Cash Transfer Programs and Nutrition in Latin America: Assessment of Impacts and Strategies for Improvement"

Objective: This study examines the degree to which four CCT programs, Brazil's Bolsa Alimentação (BA), the Programa de Asignación Familiar – Fase II (PRAF-II) in Honduras, Mexico's Programa de Educación, Salud, y Alimentación (PROGRESA), and Red de Protección Social (RPS) in Nicaragua, have improved the nutritional status of preschool age children. The authors also discuss options for design modifications which could enhance CCT impacts on nutritional status. The study is extensively based on existing quantitative and qualitative impact evaluations of the mentioned CCTs. However, they also include their original estimates of the impact of PROGRESA on children's height for age.

Programme details: PROGRESA/Oportunidades was described above

Evaluation methodology: Most of the reported impact estimates come from existing evaluations. However, the authors also provide their original estimates of the effect of PROGRESA on height for age. The authors use a child fixed effects model similar to Behrman and Hoddinott (2005) except that consumption is not included as a control (some of the controls are age, year dummy, and food prices). Little specific discussion is provided on the new results. They appear only to supplement to the analysis of the impact of CCTs on nutritional outcomes. Yet, the authors provide an extensive discussion of flaws in CCT designs as well as common problems of evaluations such as no explicit accounting for attrition.

Findings: Their original estimates of the impact of PROGRESA on height for age and weight for age for both children under age 3 and children under age 5 are not statistically significant at conventional significance levels. However, the authors do find a significant reduction in the prevalence of stunting among children in both age groups (-7.3 per cent for children under age 3 and -6 per cent for children under age 5).

Appendix 3.1.11 León and Younger: Ecuador

Author: Mauricio León and Stephen Younger

Title: "Transfer Payments, Mothers' Income and Child Health in Ecuador"

Objective: The goal of the study was to evaluate the impact of the Bono Solidario, an unconditional cash transfer programme in Ecuador, on children's nutritional status. Moreover, the authors look at whether mother's income had a stronger effect on children's heights and weights than other household income.

Programme detail: The Bono Solidario transfer scheme was initiated in September 1998. It was a loosely means-tested transfer programme focusing on mothers with non-adult children, the elderly and the handicapped. At the beginning no conditions were attached to the transfer (that includes the period covered in the study), however, eventually the transfers became conditioned on certain health- and education-seeking practices. In 1998 mothers received 100,000 sucres per month, about US$15, but in April 1999, those amounts were increased by 50 per cent, mostly to account for inflation. The transfer constituted about 11 % of total household expenditure as of 1999. The modality for payment distributions was the banking sector.

Evaluation methodology: Since Bono Solidario was national programme, random assignment into treatment and control was not done. Thus, the authors run OLS and 2SLS regressions with child anthropometric outcome as the dependent variable, and log (household expenditure per capita) and Bono share in household expenditure as the explanatory variables of interest (additional controls are also included).

Findings: The study reports that the Bono Solidario had a statistically significant but rather small impact on children's nutritional status as assessed by height for age and weight for age. In addition, the authors conclude that this impact is no different than the ordinary household income effect on height and weight, and hence the gender of the recipient of the transfer was not a distinguishable factor for reducing malnutrition.

Appendix 3.1.12 Leroy, García-Guerra et al.: Mexico

Author: Jef Leroy, Armando García-Guerra, Raquel García, Clara Dominguez, Juan Rivera, and Lynnette Neufeld

Title: "The Oportunidades Program Increases the Linear Growth of Children Enrolled at Young Ages in Urban Mexico"

Objective: After the initial evaluations of Oportunidades for the rural areas, the programme was expanded to urban areas as well. Thus, the objective of the study was to estimate the impact of Oportunidades on the growth of children who were under 24 months of age and living in the urban areas. This impact evaluation was carried out after 2 years of CCT operation in the urban areas.

Programme detail: This paper evaluates the impact of Oportunidades on child health in urban areas. Programme components were not different from those for rural recipients. Leroy et al. (2008) reports that households receive US $32.5 to $41.3, which was equivalent to about 19 to 24% of mean household consumption. Household could stay in the programme for three years, and then their eligibility was evaluated again.

Evaluation methodology: In urban areas, the programme roll-out did not follow a random design. However, baseline and follow up data were still collected. This

allows the authors to use difference-in-difference propensity score matching to assess the programme impact. The models used in the paper control for child age, sex, anthropometric measures at baseline, and maternal health.

Findings: Using difference-in-difference propensity score matching, the authors find that the programme had no impact on growth for children 6 to 24 months of age. However, beneficiary children who were less than 6 months old at baseline grew 1.5 cm ($P<0.05$) more than similar children who did not receive benefits from Oportunidades. Moreover, this same age group of beneficiaries gained an addition 0.76 kg ($P<0.01$). The benefits associated with programme participation were especially pronounced among children of the poorest households.

Appendix 3.1.13 Macours, Schady, and Vakis: Nicaragua

Author: Karen Macours, Norbert Schady, and Renos Vakis

Title: "Can Conditional Cash Transfer Programs Compensate for Delays in Early Childhood Development?"

Objective: This main objective of this paper was to evaluate the impact of a randomized conditional cash transfer program, *Atención a Crisis,* on cognitive development in early childhood in rural Nicaragua.

Programme details: This programme was initiated between November 2005 and December2006. The first requirement was that preschool-aged children visit regularly the health centres to be weighed and administered vaccines, micronutrients, or food supplements, as necessary. The second component included a transfer plus a scholarship that allowed one of the household members to choose among a number of vocational training courses offered in the municipal headquarters. The third component was the transfer plus a productive investment grant, aimed at encouraging recipients to start a small non-agricultural activity in order to create assets and diversify income. During program enrolment and pay-days about there was repeated emphasis on the importance of varied diets, health and education. Such disbursement of information tried to change household investment and consumption patterns. Children's visits to health centres were not monitored because of supply failures, but school enrolment and attendance for 7-15 tear olds were carefully monitored. Program take-up rate was reported at more than 95%. About 5% of households were denied the full amount of cash transfer that they were eligible for because of noncompliance with the school attendance requirements. Households were entitled to a transfer of US $145 even no kids were present. Households with children between 7 and 15 years who were enrolled in primary school received in addition US $90 per household, and an extra US $25 per child. Thus, the transfer constituted a nontrivial 15 per cent of total household expenditure. The payments were distributed every two month to women.

Evaluation methodology: The Atención a Crisis was a pilot programme which included a careful evaluation design. Eligible households were randomly assigned into one of four groups: control, CCT only, CCT plus vocational training, and CCT plus productive investment grant. Thus, the authors take advantage of the random assignment to identify the impact using first difference only, although they have both baseline and follow up survey data. The study attempts to identify the mechanisms through which the programme affected child development.

Findings: The authors show that the program had significant effects on children's cognitive outcome, especially among older pre-school aged children, who also had more delays. In addition, the paper reports that the programme had significant positive impact on key inputs into early childhood development including, but not

limited to, total food consumption, food diversity, healthcare utilization and vitamin A intake. The CCT impact on child height for age and weight for age is negative but not statistically significant.

Appendix 3.1.14 Maluccio: Nicaragua

Author: John Maluccio

Title: "Coping with the "Coffee Crisis" in Central America: The Role of the Nicaraguan *Red de Protección Social*"

Objective: The study examines the role of CCTs during an economic downturn such as the "coffee crisis" in Latin America. In particular, the author focuses on the importance of RPS in Nicaragua as a social safety net especially in coffee growing regions.

Programme details: The pilot phase of RPS was implemented in 2000, and the programmes expanded coverage in 2002. It was a conditional cash transfer modelled after PROGRESA. The first requirement was that children 0-5 years old are taken regularly to health clinics to be administered deworming, vitamin A and iron supplementations, and immunization. Also, on a monthly basis 0-2 year olds and on a bimonthly basis 2-5 year olds had to visit the clinics for monitoring. Mothers had to attend bimonthly workshops that discussed household sanitation and hygiene, nutrition, reproductive health, breastfeeding and related topics. Up to date vaccinations for 0-1 year olds was not enforced due to supply failures, but the other conditions were effectively enforced and hence the reported compliance was between 94 and 99 per cent, according to Gaarder et al. (2010). RPS distributed both cash transfers and micronutrient supplements such as iron supplements for children 0-5 years and antiparasitics and multivitamins. The mean cash transfer was about $25 US/month, $18 per family, $9 per family with school aged child, $20/year for supplies, which constituted about 20 per cent of baseline household expenditure (Lagarde et al 2007). Similarly to most CCTs, RPS was targeted at women.

Evaluation methodology: The author takes advantage of the randomized design of the programme and the availability of both baseline and follow-up data on the households in control and treatment areas in order to resolve problems of econometric estimation and causal inference. The estimation method used is difference-in-difference approach.

Findings: The study reports that RPS protected households against declines in per capita consumption relative to similar households which did not have the program. RPS appeared to have served as an important risk-sharing mechanism in both coffee and non-coffee growing areas, affecting also the probability of children being enrolled in school and/ or working. The programme effect on child height-for-age Z-scores was positive for all RPS areas; however, it was smaller and statistically insignificant in coffee-growing areas (net positive effect of 0.12 in coffee areas compared with 0.36 in non-coffee areas).

Appendix 3.1.15 Maluccio and Flores: Nicaragua

Author: John Maluccio and Rafael Flores

Title: "Impact Evaluation of a Conditional Cash Transfer Program the Nicaraguan Red de Protección Social"

Objective: The purpose of this report was to present the findings of a quantitative impact evaluation of RPS against its primary objectives.

Programme details: RPS was described above.

Evaluation methodology: Since RPS was a randomized intervention and data for both baseline and follow-up for control and treatment communities was available, the authors use difference-in-difference estimation technique.

Findings: The study reports the impact estimates of RPS on a variety of outcomes including education, nutrition, healthcare utilization, consumption and investment. Among the outcomes of interest for the current analysis are the findings that RPS led to a net average improvement of height for age of 0.13, although it is not significant, which the authors attribute to the small sample. In addition, the study reports that in 2 years of operation, RPS has significantly reduced stunting in children younger than age 5 by 5.5 percentage points, which is an annual rate of decline 1.7 times faster than the national trend.

Appendix 3.1.16 Morris, Olinto et al.: Brazil

Author: Saul Morris, Pedro Olinto, Rafael Flores, Eduardo Nilson, and Ana Figueiró

Title: "Conditional Cash Transfers Are Associated with a Small Reduction in the Rate of Weight Gain of Preschool Children in Northeast Brazil"

Objective: The authors took advantage of an unintended natural experiment to evaluate the impact of the national health-related CCT in Brazil, *Bolsa Alimentação,* on children's growth. The experimental design was a result of accidental, random mistakes which led to some eligible children being excluded from receiving the cash transfers.

Programme details: Bolsa *Alimentação* was instituted in 2001, but then it was merged with other existing transfer programmes into Bolsa Família in 2003. Similarly to other CCTs it stipulated that children 0-7 year old must had to have up—to-date immunizations and growth monitoring; pregnant women had to go to prenatal classes; and, children 0-15 years old had to visit health centres regularly for preventive care. The literature contains conflicting reports as to whether this programme provided micronutrient supplements, and whether the conditionalities were strictly monitored and enforced. The cash transfer payment was at most $18.25 US, $6.25 per beneficiary person in the household (pregnant women or children 0-7 y old), which constituted about 8 per cent of household consumption. The transfer payments were credited to a magnetic card that could be used to withdraw cash at offices of a federally owned bank, or with lottery agents in the very isolated municipalities.

Evaluation methodology: The authors take advantage of the quasi-experimental design that resulted due to random, unintended mistakes in beneficiary selection. They use adjusted (for household demographic characteristics and beneficiary status) and unadjusted t-tests, as well as generalized linear regression methods to identify the programme impact on child anthropometric outcomes based on one cross-section of data.

Findings: The authors find that six months after benefits began to be distributed, beneficiary children were 0.13 Z-scores lighter (weight-for-age) than excluded children, after adjusting for confounders (P=0.024). Each additional month of programme participation resulted in a rate of weight gain 31 g lower than that observed among the accidentally excluded children with similar characteristics (P=0.001). Beneficiary children also had lower height for age z-scores than did the

excluded children, but the difference was not statistically significant for any age group. The authors attribute these negative findings to recipients' perception that benefits would be discontinued if the child started to grow normally.

Appendix 3.1.17 Paxson and Schady: Ecuador

Author: Christina Paxson and Norbert Schady

Title: "Does Money Matter? The Effects of Cash Transfers on Child Health and Development in Rural Ecuador"

Objective: The objective of this paper was to evaluate the impact *Bono de Desarrollo Humano* (BDH), a government-run unconditional cash transfer program targeted to poor mothers in rural Ecuador, on the health and development of their children.

Programme details: BDH started in mid-2003 and it actually replaced the existing Bono Solidario. Initially the programme was meant to be a conditional cash transfer, such that beneficiaries would receive $15 per month, but they had to take their children younger than age six for bi-monthly visits to public health clinics and send their school-aged children to school. However, for a variety of logistical reasons, these conditionalities were never carried out. As mentioned, the monetary transfer consisted of $15 per month and constituted a non-trivial 10 per cent increase in family expenditure for the average eligible family. The distribution of the transfers was done through the banking system. Similarly to most of the discussed programmes, BDH also targeted mothers.

Evaluation methodology: The programme was rolled slowly which provided opportunities for randomizing the design, that is the authors randomly placed parishes in treatment and control groups. Then, they estimate the treatment effect using first difference approach but controlling for baseline individual and household characteristics.

Findings: The authors find that the cash transfer program had positive impacts on the physical, cognitive, and socio-emotional development of children. Also, treatment effects were significantly larger for the poorer children compared to the less poor children. Higher impacts were reported for girls and for children with more educated mothers. Moreover, the study examines three mechanisms through which the programme might have influenced child outcomes —better nutrition, greater use of health care, and better parenting. Only the first mechanism was identified as significantly attributing to child development in the case of Ecuador.

Appendix 3.1.18 Sinha and Yoong: India

Author: Nistha Sinha and Joanne Yoong

Title: "Long-Term Financial Incentives and Investment in Daughters: Evidence from Conditional Cash Transfers in North India"

Objective: In 1994 the government of India *Apni Beti Apna Dhan* (Our Daughter, Our Wealth) programme in the state of Haryana. This rather unique intervention aimed at creating financial incentives for eligible parents to give birth to a daughter and subsequently to invest in her human capital development. The incentive consisted of an immediate cash grant at the time of birth and a long-term savings bond which was redeemable on the daughter's 18th birthday provided she was unmarried. Additional bonuses for education were also provided. The purpose of the study was to evaluate *Apni Beti Apna Dhan's* impact on the primary targeted

indicators: sex ratio among live children, mothers' fertility preferences, mothers' use of antenatal care, child survival probability, child nutritional status, immunization and schooling.

Programme details: *Apni Beti Apna Dhan (ABAD)* was initiated in October 1994 in the State of Haryana, Northern India. It provided financial incentives to discourage son preference. Eligible mothers who give birth to daughters are given an immediate cash transfer plus a long-term savings bond redeemable by the unmarried daughter at the age of 18. When a daughter is born (provided that she is the first, second or third born child in the family), mothers are entitled to a monetary grant of Rs. 500 (approximately $11) within 15 days of the birth designed to cover post-delivery needs. The intervention also includes a longer-term investment of Rs. 2,500, (approximately $55) in government fixed-deposit securities, redeemable for a guaranteed sum of Rs 25,000 (approximately $550) on her 18th birthday provided she remains unmarried. Moreover, additional cash bonuses aim at encouraging parents to invest in the human capital of their female children. A bonus of Rs. 5000 is allocated if the girl has received at least primary education, and an additional Rs. 1000 is awarded if she has continued up to grade 8.

Evaluation methodology: ABAD was introduced in all districts of Haryana simultaneously and hence experimental design techniques are inappropriate for this evaluation. In addition, no available survey data contain information on actual programme participation. So, the authors estimate an intent-to-treat effect using the statutory programme eligibility criteria and making use of three cross-sections of survey data that covers the time of the programme implementation. The authors use a standard difference-in-difference approach to account for pre-existing differences between participants and non-participants and to capture time trend changes in outcome of interest.

Findings: The paper reports that the program had a positive impact on girls' birth and survival as assessed by changes in the sex ratio of mother's total living children over time. The programme effects on mothers' preferences for female children and fertility were unclear. However, the financial incentives were associated with increased investment in daughters' human capital. Greater post-natal health investments in girls were observed but the evidence on improved health status in the short and medium term was mixed.

Appendix 3.1.19 IFPRI: Honduras

Author: IFPRI

Title: Sexto Informe. Proyecto PRAF/BID Fase II: Impacto Intermedio

Objective: The objective of this report was to analyse the impact of the Honduran CCT, PRAF II, on the primary targeted indicators including, but not limited to, healthcare utilization, child vaccination, child nutritional status and morbidity, prenatal care, school enrolment and attendance.

Programme details: PRAF II was pilot CCT designed after PROGRESA. The first phase of PRAF started in 1991 and the second phase, PRAF II, in 1998. Transfers are targeted at young children and pregnant women, and there are additional educational incentives. Programme participants have to comply with the designed conditions: 0-3 year olds have to be regularly taken to health clinics for growth monitoring and health check-ups, pregnant women have to attend prenatal classes, and 6-12 year olds (not completed 4th grade) had to be enrolled and attend classes regularly. Attempts were made to monitor compliance with the conditionalities,

for example from late 2001, beneficiaries were required to deposit a certified, bar-coded attendance slip on visits to the health centres, but no one was actually suspended for non-compliance. The education grants were effectively conditioned on enrolment, but not on attendance.

Each beneficiary household received vouchers worth 55 Lempiras (£2·53 according to currency conversion rate in late 2001, or 3.85USD) per month for each pregnant woman or child younger than 3 years of age in the household, up to a maximum of two. In addition, all households with children between 6 and 12 years of age enrolled in primary school in grades 1–4 received, for each child up to a maximum of three, vouchers worth 80 Lempiras (£3·69) per month, for 10 months of the year (Morris et al. 2004), which constitutes about 4 per cent of baseline household consumption. PRAF included both demand-side and supply-side interventions, however, the latter were delayed in implementation. The monetary demand-side vouchers were distributed about three times.

Evaluation methodology: The implementation of PRAF II followed a random design, but the randomization was carried out only at the community level. Data for control and treatment localities was collected at both baseline and follow-up which allowed the use of difference-in-difference impact estimation approach.

Findings: PRAF II was associated with no significant impact on children's nutritional status as measured by height for age.

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