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How Effective are Cash Transfers at Improving Nutritional Status?

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Abstract

Cash transfer programs have not always affected children's nutritional status. We reviewed 30,000 articles relating cash transfer programs and height for age, finding 21 papers on 17 programs. Applying meta-analysis we examine the overarching relationship, finding that the programs' average impact on height-for-age is positive, but small and not statistically significant. We evaluate many program, child and local characteristics' correlation with estimated outcome. Conditional programs statistically accomplish the same as unconditional. However, conditionalities not related to health or education strongly inhibit child growth. We see girls benefiting more than boys and more disadvantaged areas benefiting more.

Keywords: cash transfers; social assistance; nutritional status; height for age; multi-country

JEL codes: O12, I11, I18, I38

I. Introduction

One of the most widely implemented development policies over the past years has been the Cash Transfer (CT) program, implemented in as many as 48 countries as of 2008 (see Barrientos, Niño-Zarazúa and Maitrot 2010 for a list). Targeted toward the poor, these programs distribute cash payments. A common variant, the Conditional Cash Transfer program (CCT) distributes cash if recipients meet conditions typically including sending children to school and/or getting regular health care.

Although achieving successes on other fronts, such as improving consumption levels and school attendance and decreasing child labor (Fiszbein and Schady 2009) these programs have not consistently improved recipient children's height for age, a common measure of nutritional status. This paper analyzes the state of the evidence regarding the relationship between cash transfer programs 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.

While previous works have mentioned the issue as part of broader surveys of the relationship between CTs and health, none have focused on the issue per se, none have included unconditional cash transfer programs as a comparison, and none have looked at anthropometrics beyond five programs in Latin America. This paper accomplishes all of these aims.

In the next section we discuss our two focal points: cash transfer programs and nutritional status. Following that we review the links between the two, including a short summary of the theoretical relationship. Following the theory we describe our methodology, describe our results, and conclude.

II. Literature Review

A. Cash Transfer programs

CTs are targeted interventions providing cash to selected beneficiaries. Mexico and Brazil established CCTs in the late 1990s, but programs are now found in many countries. Programs in Mexico and Ecuador 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). Many states within the United States have begun implementing CCTs in an attempt to improve educational outcomes (Bassett 2008; Fryer 2010).

CTs have expanded in part because they improved recipients' consumption level. 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 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 2007).

While some CT programs such as pensions involve unconditional distributions, many impose conditions on recipients. Many of these CCTs require participants to make investments in human capital by getting check-ups at a health clinic and/ or sending children to school. Mexico's PROGRESA led this group by intervening simultaneously in health, education and nutrition (Skoufias & McClafferty 2001). The hope is that investing in all three at once will yield greater benefits for the poor and for society. Other similar programs followed, including Brazil's Bolsa Alimentação, Colombia's Familias en Acción, Honduras' Programa de Asignación Familiar, and Nicaragua's Red de Protección Social.

Other programs require participants to meet savings requirements or work. Three programs in Bangladesh (Food for Asset Creation, Food Security Vulnerable Group Development Program, and the Rural Maintenance Plan) and Sri Lanka's Samurdhi program required recipients to work. The first also set mandatory savings goals (Ahmed et al. 2009).

B. Nutritional Status

Though social assistance programs seek to achieve a number of ends, nutritional status 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. "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 programs 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).

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 (HAZ) based on observed means and standard deviations for children of a given age and sex. These scores reflect long-term health (Waterlow, Buzina et al. 1977; Strauss and Thomas 1998). 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 used HAZ 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). One study concludes "Height for age at 2 years was the best predictor of human capital…." (Victora, Adair et al. 2008).

C. Summary of CT programs' effects on nutritional status

Cash transfer programs' effects on child nutritional status are unclear. Barrientos and Nino-Zarazua (2010) claim that, "A range of studies on low and middle income households finds that social transfer programs are effective in improving nutrition… and health status among beneficiaries." (p. vii) On the other hand, 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) and others[1] document a "mixed result" of CCTs on nutritional status in five programs (p. 27). As Leroy, Ruel, and Verhofstadt (2009) also note:

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

Some programs have found success in improving nutritional status. Studies show that the Mexican program *Oportunidades* (formerly known as PROGRESA) improved child growth 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 after 6-10 years when impacts were evaluated in terms of transfers received rather than time on the program (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 programs in Ecuador (Paxson and Schady 2010) and Colombia (Attanasio, Battistin et al. 2005). In Nicaragua Maluccio and Flores (2005) showed a CCT reduced stunting not affecting overall height-for-age. A program in Nicaragua also showed no improvements in height (Macours et al. 2008). No impacts were shown in Honduras (Moore 2008), and negative impacts on height were shown in Brazil (Morris, Olinto et al. 2004).

The idea behind conditionality is clear, but there may be unintended consequences. A recent book (Hanlon, Barrientos et al. 2010) questions the importance of conditionalities, noting that unconditional programs have improved welfare. In countries lacking sufficient health infrastructure, unconditional transfers may be the only realistic alternative.

More ominously, CCT conditions may overburdens households. 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 labor 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).

D. Theories linking CTs 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, it depends on the quantity and quality of food coupled with the health status of the person consuming it.

Diagrams in Leroy, Ruel, and Verhofstadt (2009) and Gaarder, Glassman, and Todd (2010) show a total of 26 mediating pathways linking CCTs to health including parental education, feeding care and practices, supply of health services, and cash. Nutritionists note that in addition to needs for calories and protein, a variety of

[1] Hoddinott(2010); Hoddinott and Bassett (2008); Bassett(2008); Lagarde, Haines, and Palmer (2007); Bouillon and Tejerina (2007) see mixed results in transfers, while Yablonski and O'Donnell (2009) and Barrientos and Nino Zarazua (2010) see unmitigated success in the programs they review.

micronutrients including iron, zinc, and vitamin A can constrain growth, as can frequent infections (Rivera, Hotz et al. 2003).

Hoddinott and Bassett (2008) note that improving food consumption may be necessary but not sufficient to improve child development in locales lacking accessible, quality health care. Bassett (2008) notes specific factors that may limit CCT effectiveness as regards height for age:

> many critical behavior 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 behaviors directly. (p. 9)

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 analyzes and results and failed to cite each other. They note that unplanned subgroup analyzes of trials can lead to spurious conclusions.

From these, we identified key factors to test. Program characteristics such as conditionality, nutritional supplements, and payment size may be important. Child age and sex could matter, and community characteristics such as access to sanitation and health care are likely significant. Means of analysis too could influence results.

III. Search Methods

We gathered data by systematically searching for all existing studies which examine the impact of a cash transfer program 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 was a direct cash transfer. In addition, the relevant studies had to focus on the program effect on height for age or weight for age, another measure of nutritional status.

Our search started with an examination of the references of four systematic review papers on the effect of various cash or/and in-kind transfer programs on health outcomes (Gaarder et al. 2010, Leroy et al. 2009, Lagarde et al. 2007, and Bouillon & Tejerina 2007). These 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 led us to 23 articles containing original impact estimates with standard errors.

The next step was to search 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, and Worldcat.org.

Next, we searched the websites of 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.

Each of our searches combined a term that referred to the program with another that referred to anthropometrics. One term identified interventions: "cash transfer," "social safety net," family allowance program," "child grant," "child support grant," "social transfer" or "social assistance." Another denoted anthropometrics: "height and child," "nutritional status," "child growth," "anthropometric," or "weight and child.[2]" This resulted in 35 combinations of phrases, which were used for the search of each database. When the original stipulation yielded more than 1000 references, The search was limited to citations dated after 1990. No other restrictions were imposed. After compiling the results from each of 35 searches of each search engine we used a simple inclusion criterion: "Does the article evaluate a cash transfer program and does it report numerical anthropometric impacts?"

The full search was carried out independently by two reviewers between July 29, 2010 and September 2, 2010. All searches were carried out on a computer running Windows Vista and Google's Chrome browser. Findings were then carefully compared, and in the case of discrepancies with respect to the located number of articles, the search was repeated to establish the cause.

The next step of the process involved hand searching 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 programs as well. No new quantitative studies were added to the previous findings.

Based on feedback from reviewers we decided to do an additional search of the above resources in Spanish using translations of relevant terms. The search in Spanish was performed in August of 2011 with the same databases above along with Spanish language sources such as Centro de Estudios sobre Desarrollo Económico of Universidad de los Andes, Literatura Latinoamericana y del Caribe en Ciencias de la Salud and Mexico's INSP. The search yielded four additional articles, including two with new data on programs already in our database and two on additional programs (Paraguay Tekopora and Peru's Juntos). The inclusion or exclusion of the articles from the Spanish search does not substantially change the results of the meta-analysis.

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

IV. Search Results

Through this process, we examined over 30,000 articles, not including journals hand-searched. The set of final usable articles is summarized in Table 1. The PRISMA flow diagram (slightly modified from Moheret al. 2009) in the Appendix also summarizes the search. Database searches identified 16,467 records, and an additional 13,740 were located on the institutional websites. Citations were screened by title and abstract to isolate empirical studies of cash transfer programs on children's anthropometric outcomes. 1,477 references appeared relevant, but only 360 remained after removing repetitive entries. The full text of these was assessed for eligibility, ultimately resulting in the addition of seven new studies to those identified in the literature snowballing.

We found studies on 13 countries, but our sample includes data from 12 countries with program impacts measured in terms of HAZ. (A program in Malawi was dropped since impacts are measured in centimeters.) The 12 remaining countries contain 18 programs, but we analyzed only 17 programs because the South Africa Child Support grant study did not contain standard errors and was dropped. (South Africa has a separate program, the Old Age Pension, so we still include that country and have a total of 12 countries.) We have a total of 27 program - study combinations, which includes some programs with multiple estimates and studies examining multiple programs. Table 1 summarizes the various studies and interventions by country of implementation. Some studies reported impacts on other anthropometric outcomes such as weight for age and weight for height z-scores as well as height for centimeters. The last column reports the outcomes analyzed in the respective evaluation paper. Besides height for age, no nutritional outcome appeared in all of the listed studies.

Table 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
		Vera-Hernandez et al. (2010)	HAZ, WAZ, WHZ
Ecuador	BDH	Paxson and Schady (2007)	HAZ
		Younger et al (2009)	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
Nicaragua	RPS	Behrman et al. (2008)	HAZ
		Maluccio and Flores (2005)	HAZ
		Maluccio (2005)	HAZ
		Gitter et al. (2010)	HAZ
	Atención a Crisis	Macours, Schady, et al. (2008)	HAZ, WAZ
Paraguay	Tekopora	Barrios et al. (2008)	HAZ, WAZ
Peru	Juntos	Perova and Vakis (2009)	HAZ, WAZ
South Africa	Old Age Pensions	Duflo (2003)	HAZ, WHZ
		Case (2001)	Height in cm
Sri Lanka	Samurdhi	Himaz (2008)	HAZ, WHZ

Table 2A lists the programs with some of the covariates on which we collected data. Most studies do not report information on all of the listed variables. Additional papers, not listed, were used to obtain more qualitative information on cash transfer programs, such as impacts on food consumption, food diversity, and health care utilization. Even after this supplemental search, we were still unable to find all the information on covariates we had sought. This further restricts the number of observations for the covariate analysis. For example, baseline income was rarely reported, and when it was, it was hardly ever broken down by group when additional subgroup analysis was carried out. Other common omissions are program take up rate and participation in the conditionality.

Table 2B summarizes the findings of many studies on food consumption and food diversity. One positive outcome shown in Table 2B is that only one of the programs studied (Honduras' PRAF) was found to have no impact on food consumption and/or variety, while ten showed increases in recipient expenditures on food, calorie consumption, and/ or food diversity.

The Appendix provides greater details on the identified programs.

Table 2A. Program Listing

Country	Program Name	Years Evaluated	Nutritional supplement?	Educational condition?	Health care condition?	Enforced?	Transfer % 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
Paraguay	Tekopora	2008-2009	Yes	Yes	Yes	SP	17%	Women
Peru	Juntos	2005-2007	Yes	Yes	Yes	SP	13%	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. * An education-conditional portion of this program has not yet been evaluated.

Table 2B. Program Listing and Food Consumption

Country	Programs	Impact on (food) consumption	Impact on Food Diversity	Source
Bangladesh	FSVGD	Each of these three programs has positive, significant per capita impact on calories & food exp		Ahmed et al. (2009)
	FFA			Ahmed et al. (2009)
	RMP			Ahmed et al. (2009)
	Primary Education Stipend	Positive, significant increase in adult equivalent food exp. & kcal	Positive, significant increase in protein consumption	Baulch (2010)
Brazil	Bolsa Alimentação		Positive, sig. 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.		Black, Allen et al. (2008)
	Bono Solidario	not measured	not measured	
India	Apni Beti Apna Dhan	not measured	not measured	
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)
Paraguay	Tekopora	Negative for households not extremely poor	Improved consumption of milk and fresh fruit	Soares et al (2008)
Peru	Juntos	Positive, significant effect on per capita food consumption	Positive, significant effect on per capita consumption of a variety of foods	Perova and Vakis (2009)
South Africa	Old Age Pensions	not measured	not measured	
	Child Support Grant	not measured	not measured	
Sri Lanka	Samurdhi	not measured	not measured	

V. Data Analysis

Statistical consideration provides a more objective means of identifying trends (Mann 1990) though small number of subgroups makes clean identification difficult. We first use a forest plot to evaluate the overall trend toward effectiveness. Next, we compare some theory-based covariates with observed anthropometric outcomes.

A. Weighted Average Overall Impact

Our first task is to describe in general terms the relationship between cash transfer programs and child nutritional status. Several factors make combining these diverse estimates a complex proceeding.

The analysis of the impacts of cash transfer programs on height for age uses 20studies looking at 17 programs in 12 countries. Two studies examine multiple programs: Ahmed et al. (2009) evaluate three cash transfer programs in Bangladesh, and Gitter et al. (2010) evaluate programs in Mexico, Nicaragua, and Honduras. Likewise some programs, such as Mexico's *Oportunidades*, were examined by many different studies. Table 1 in the previous section lists the studies and the programs they examine.

To complicate things further, most studies generate multiple estimates of a given program's effect. For example Duflo (2003) tests the effects of the South African Old-Age Pension Program on child height for age in twenty different ways. She looks at the program's effect on boys and on girls separately, and she estimates each many times using different econometric specifications. Other studies use different types of estimators (e.g. OLS, 2SLS, PSM) as robustness checks. Below we describe how we pool multiple estimates to reach a single point estimate for each program and paper.

Table 3 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 3 lists the number of estimates generated by each study. Weighting each article equally the mean impact was 0.06 with an average standard error of .16 and with on average 6.4 estimates per study.

Table 3. 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
Barrios et al.	Paraguay	-0.01	0.14	1
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
Perova and Vakis	Peru	0.07	0.19	3
Sinha and Yoong	India	0.34	0.27	3
Vera-Hernandez et al.	Colombia	0.12	0.09	12
Younger et al (2009)	Ecuador	-0.11	0.02	8
Unweighted Averages		0.06	0.16	6.4

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

There is considerable heterogeneity in estimates of program impact, due at least in part to the considerable heterogeneity in the programs 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 Bangladesh's Rural Maintenance Program, which decreased the HAZ 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.

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 analyzing 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. The second issue is that multiple observations of the same program 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, but at the same time we must control for the correlation between estimated impacts for different estimators or treatment groups in the same program. 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 program 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 cases such as comparisons between boys and girls, the individuals in the treatment and control groups are different but the program is the same, so we would expect limited correlation. Following 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). We calculate the variance for each program and study following Borenstein et al. (2009, Chapter 24):

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

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.

A forest plot, Figure 1, graphically depicts the impact estimated by each study and summarizes the overall estimated effect based on the sixteen observations in the data. The estimated treatment effect (θ_{pooled}) for the pooled sample of "k" studies is equal to a ratio shown in equation 1. 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. 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 program. This seems unlikely in our data as cash transfer programs 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 treatment effect 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

$w_i = 1/[v_i + \tau^2]$

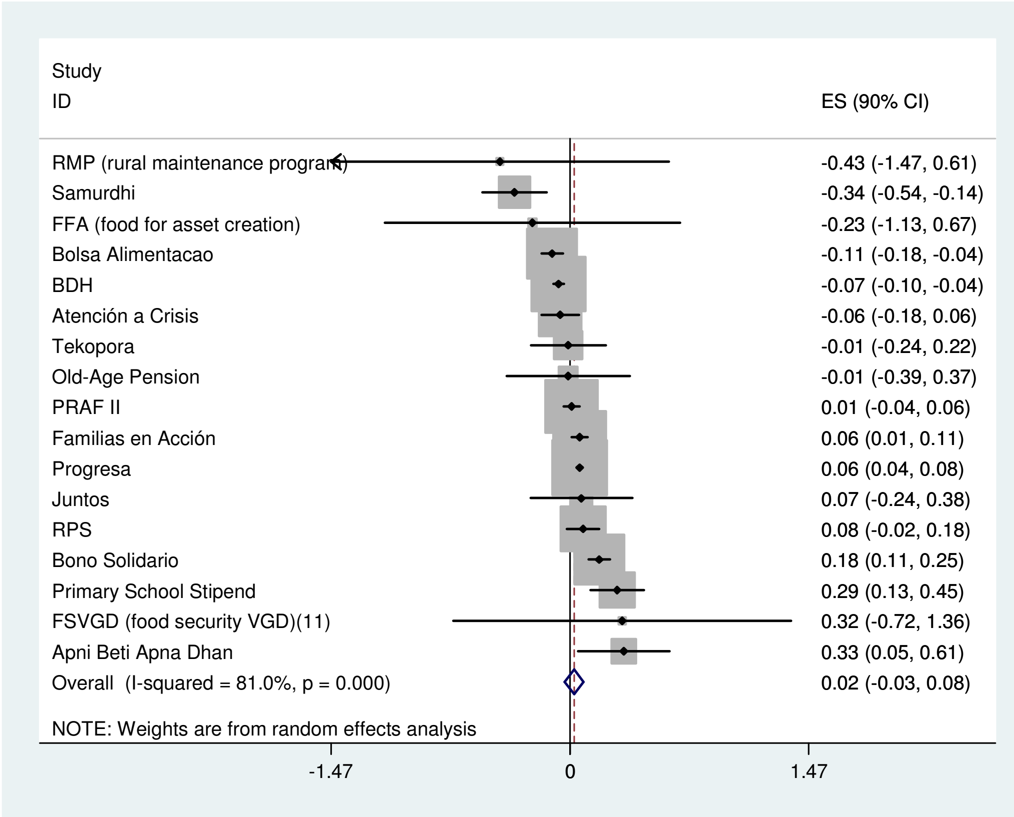
We use the standard DerSimonian and Laird estimate of τ^2. Assuming that each program has its own impact on children's height for age z-scores, we identify random effects at the program level, reporting τ^2 for each pooled analysis.

We also report I^2, 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 of 50% is a moderate level of heterogeneity, while 75% is high. For the whole sample and most subgroups we have a moderate to high level of heterogeneity among studies. This suggests that as expected programs have different effect sizes. We also report the estimate of the variance between programs (τ^2), which for most estimates is around 0.01 z-scores.

Grouping by programs rather than studies and weighting effect sizes by the inverse of their standard error, the 15 programs increased HAZ by an average of 0.04 with a p-value of 0.16. Figure 1 shows the estimated mean effect of each of the 15 programs as well as the confidence interval and weight based on the inverse of the estimated variance.

The forest plot in Figure 1 illustrates our finding that on average CT programs increased height, although the size of the effect is small and statistical significance is weak. When effect sizes are weighted by the inverse of their standard error on average the 17 programs increased HAZ by 0.025 with a p-value that the effect is different from zero of 0.38. The forest plot below shows the estimated mean effect of each of the 17 programs as well as the confidence interval and weight based on the inverse of the estimated variance (this weight is represented by the grey boxes). The program effects are sorted from smallest (including negative) to largest.

Figure 1. Forest plot showing estimated overall program effect on HAZ



B. Covariate Analysis

The next stage is to examine the influence of covariates on HAZ using meta-regression, which provides marginal effects while providing a simple statistical test of the effect. Due to the limited sample size we examine one covariate at a time using weighted least squares (where programs are weighted by the inverse of their variance) via 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.

The covariates can be divided into four groups: the program characteristics (nutritional supplement offered, conditionality, approximate payment size, and program duration); characteristics of the study (peer reviewed, randomization, study quality, and whether the baseline z-score was measured); 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). The first three sets of characteristics are summarized in Table 5. We identified local health conditions as important above, but lacking local information on disease environment and the quality and utilization of local health services we use the World Bank's country level World Development Indicators.

First we consider program characteristics. A little over half offered a nutritional supplement. Eleven programs were conditional; seven on health check-ups for children and health seminars for mothers. Eight of the eleven enforced their required conditions. We also test the duration of time a household has received transfers. Finally, we approximate monthly payment sizes by converting all figures to US dollars, either based on values reported in the studies or by the prevailing exchange rate in the baseline year. There is a relatively even three-way split of programs 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 and the gender of the recipient of program funds; however, we lacked the data to do so. Only 2 of the programs (Samurdhi and Old Age Pension) targeted funds equally to men and women, while all other interventions targeted women only.

Table 4. Study, Child and Program Covariate Descriptive Statistics

Authors	Country	Study Covariates				Child Covariates			Program Covariates			
		Peer Reviewed?	Randomized?	Baseline Z given?	Average Sample Size	All Male Samples	All Female Samples	% Under 36 Months	Nutr Supplement?	Conditional?	Enforce Conditions?	Payment Size*
Ahmed et al.	Bangladesh	N	N	Y	317	N	N	0	1/2	3/4	1/4	1
Attanasio Gomez et al.	Colombia	N	N	N	--	N	N	33	N	Y	N	2
Barrios et al.	Paraguay	N	N	N	585	N	N	60	Y	Y	Y	2
Baulch	Bangladesh	N	N	N	129	1/3	1/3	0	N	Y	N	1
Behrman et al.	Mexico	N	Y	N	379	N	N	100	Y	Y	Y	3
Duflo	S. Africa	Y	N	Y	1582	1/2	1/2	50	N	N	N	3
Fernald et al. (2009)	Mexico	Y	Y	Y	1710	N	N	0	Y	Y	Y	3
Fernald et al. (2008)	Mexico	Y	Y	Y	2402	N	N	0	Y	Y	Y	3
Gitter et al.	(3**)	N	Y	Y	817	1/3	1/3	0	2/3	Y	2/3	†
Himaz	Sri Lanka	Y	N	N	484	1/5	1/5	20	N	Y	N	1
Hoddinott & Bassett	Mexico	N	Y	Y	--	N	N	50	Y	Y	Y	3
Leon and Younger	Ecuador	Y	N	Y	850	N	N	60	N	N	N	2
Leroy et al.	Mexico	Y	N	Y	432	N	N	29	Y	Y	Y	3
Macours et al.	Nicaragua	N	Y	Y	1622	N	N	0	Y	Y	Y	3
Maluccio	Nicaragua	N	Y	Y	1493	N	N	0	Y	Y	Y	3
Maluccio& Flores	Nicaragua	N	Y	Y	1982	N	N	100	Y	Y	Y	3
Morris et al.	Brazil	Y	Y	Y	915	N	N	25	N	Y	N	2
Paxson& Schady	Ecuador	N	Y	Y	1434	N	N	0	Y	N	N	1
Perova & Vakis	Peru	N	N	N	345	N	N	60	Y	Y	Y	3
Sinha & Yoong	India	N	N	Y	2241	N	Y	100	N	N	N	1
Vera-Hernandez	Colombia	N	Y	Y	1525	N	N	43	Y	Y	Y	2
Younger et al.	Ecuador	N	Y	Y	2646	N	N	100	N	N	N	2
Average (proportion):		.32	.55	.73	1195	.06	.11	.38	.60	.76	.54	2.2

*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 programs in categories2 and 3 both -- denotes value missing from data

Table 4 shows covariate data. 32% of the studies included are peer reviewed. It is important to test for publication bias in studies as published studies may favor a surprising or a positive result. Of the studies 55% use randomized controls to test program effects. About three quarters list the baseline HAZ of program recipients. Sample sizes run from about 100 to over 2,000 with an average of 1,195. Just two papers, Attanasio et al. (2005) and Hoddinott and Bassett (2008), did not report sample size. Nearly all studies added controls for mother's education and household size. Due to this lack of heterogeneity we cannot examine the significance of these controls.

Sex and age could influence the effect of a CT. Duflo (2003) finds evidence that girls may see larger benefits than boys from cash transfers. To test this we compare the six programs (including Duflo's) reporting separate estimates by sex.

Hoddinott and Bassett (2008) note that interventions with the goal of reducing stunting should focus their treatments on pregnant women and children under 2. 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 of younger children are more likely to see effects, but that studies with older children may find effects as well. We test directly for an age effect and we test the difference in estimated effect size for studies including treatment groups with different ages. Finally, we compare estimated effect sizes for the youngest and oldest cohorts tested.

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 national health conditions in the closest available year to the program's baseline (see Table 5). In most cases we find data in the baseline year. When we cannot, we use the closest available year or the average of the two closest years in cases where the 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 households having improved sanitation.

Table 5. Country Level Measures of Health Care Quality and Utilization

Country	Baseline Year	IMR	Beds/1k	ARI %	DPT%	ImprSan%
Bangladesh	2006	48.2	0.4	30.1	91	48
Bangladesh	2000	65.6	0.3	27.2	83	43
Brazil	2002	28.2	2.6	**49.7**	99	36
Colombia	2001	22	1.1	51.3	80	50
Ecuador	2003	23.5	1.7	38.8	75	*75*
Ecuador	1999	28	**1.5**	38.8	79	*68*
Honduras	2000	32.6	1	**55.9**	78	47
India	1992	79	0.787	**67**	56	*8.3*
Mexico	1998	22.1	1.1	Missing	96	46
Nicaragua	2005	26.6	0.9	**57.7**	88	35
Nicaragua	2000	34.2	**1.12**	57.7	83	32
Paraguay	2008	26	1	Missing	92	70
Peru	2005	28	1	67	89	66
South Africa	1993	48.2	**Missing**	**75.3**	81	58.5
Sri Lanka	1999	17.3	2.2	**58**	99	60.5

Program covariate regressions are summarized in Table 6. Again each coefficient represents a separate regression on a single variable. The results do not show an impact of supplements, likely due to confounding. Among studies reporting solely on programs with supplements, all but one found a positive program impact, though three found small impacts (<= 0.06). The pooled effect of supplement provision on HAZ is nearly zero. Even removing a program with a supplement and negative effects, the influence of supplements still has large p-values (0.6).

Table 6. Program Characteristics (Pooled Analysis)+

Impact on HAZ of:	Mean†	SD	Est'd Impact	P(t)	Lower	Upper	I^2	τ^2
Program with Nutritional Supplements	39%		-0.02	0.77	-0.19	0.14	81%	0.015
Conditional Programs	72%		-0.08	0.21	-0.27	0.11	79%	0.015
Conditional: no Education or Health Conditions	22%		-0.35	0.02	-0.65	-0.04	79%	0.009
Health Conditions+	50%		-0.04	0.58	-0.22	0.12	81%	0.01
Less than $10 a month	50%		-0.02	0.79	-0.19	0.14	72%	0.015
Duration	23.1	18.3	0.003	0.18	-0.002	.008	78%	0.012
Baseline HAZ	-1.56	0.44	-0.06	0.5	-0.26	0.13	82%	0.001

A simple meta-regression was performed for each covariate.

†Means for dummy variables shown as a percentage.

+ Analysis compares programs with conditionalities related to health against unconditional programs, dropping programs with conditions unrelated to health.

The provision of nutritional supplements shows a 0.59 correlation with the mean age. Programs providing supplements more often tracked older children. Since older children might show less program effect, supplements may show less effectiveness. Note that this does not mean that supplements were provided ineptly: if supplements are provided only to younger children, we code the program as providing supplements. Researchers tracking program effects on children of all ages would see supplement effects only insofar as the supplement recipients dominate the sample. For example, if only the youngest 1/3 of children in the sample receive supplements, overall program 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 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.

The second row of Table 6 shows that conditional programs have weaker effects than unconditional programs by a factor of 0.08. However, the effect is not statistically distinguishable from zero (p-value = 0.21). To investigate the negative effect of conditionality we split conditionalities into two groups. Programs with non-health conditions (e.g. requiring work or savings quotas) show negative impacts on HAZ, decreasing child height by 0.35 (p-value = 0.02). Finally we compare just unconditional and programs with health conditions. Conditional programs' effect size was 0.04 smaller than unconditional and statistically insignificant (p-value = 0.2)

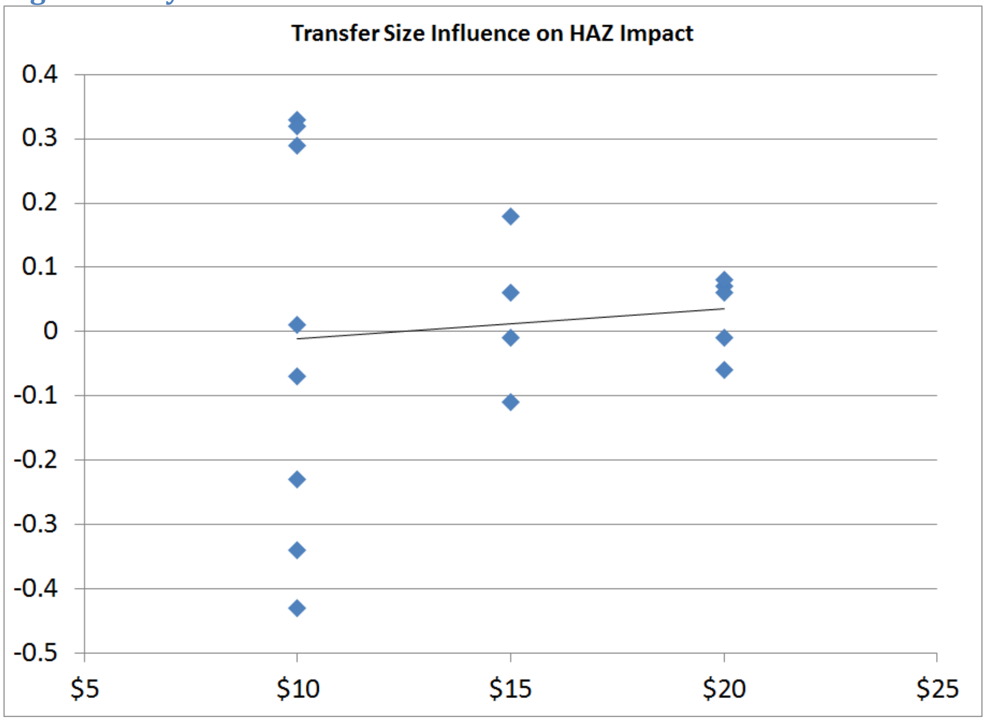
To test this result we reran the regression 16 times, each time dropping one program to test for the influence of any one program. In 15 of the regressions we find similar results, near -0.37. Dropping Samurdhi, a program with non-health conditions that showed the largest negative impact, gives a coefficient of -0.19, still statistically insignificant.

The group of interventions with requirements unrelated to education or health were highly (negatively) 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. Since we show later that these low health countries experience higher returns to transfers, having a negative sign on this relationship is a further indictment of these conditionalities.

We do not find a relationship between payment size and program effects (see Figure 2). We divided the sample into three monthly payment sizes (under $10, $10-$20, and over $20). Effect sizes for the three groups minimally increase as payment size goes up: the smallest payments see an effect size of 0.01 while both other groups see 0.04. A regression of HAZ on a dummy for programs with payments under $10 shows nearly zero program effect, but similar results obtain when using a dummy for programs paying over $30 a month.

Confounding this relationship is a correlation between payment size and local health conditions (0.6 between infant mortality and probability of payment under $10 a month). If higher impacts are observed in areas with worse health conditions, estimated effects of lower payments will be biased downward. Thus, this evidence shows that policymakers did well at fitting the transfer size to the locality. No transfers showed outsize returns to their transfer amount.

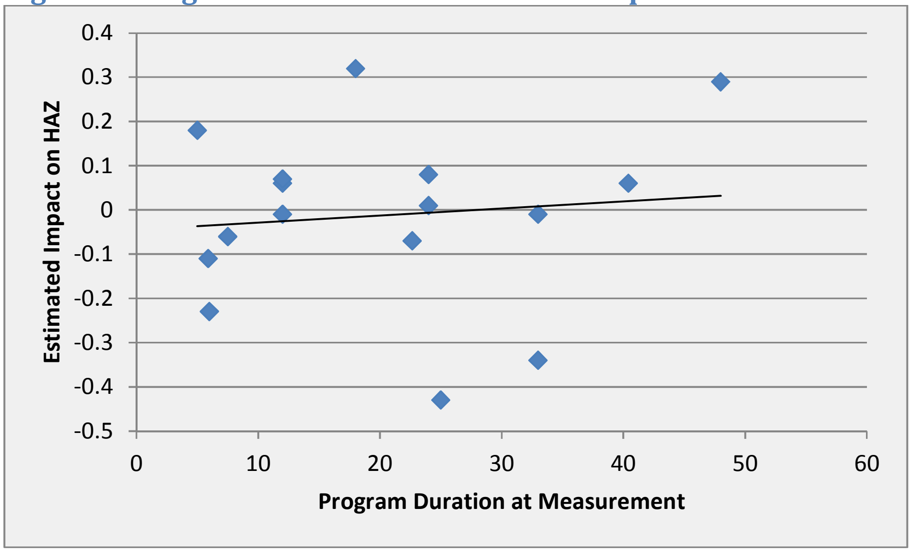
Figure 2. Payment size and estimated effect on HAZ



*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.

We regress HAZ impacts on the number of months the average household had received payments to test for the effect of program duration. Figure 3 shows the data. We find the expected positive relationship, although not statistically different from zero (p =.18). A one standard deviation increase in program duration (18.3 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 16 times eliminating one program at a time we find consistent coefficients and p-values (.05 to .4).

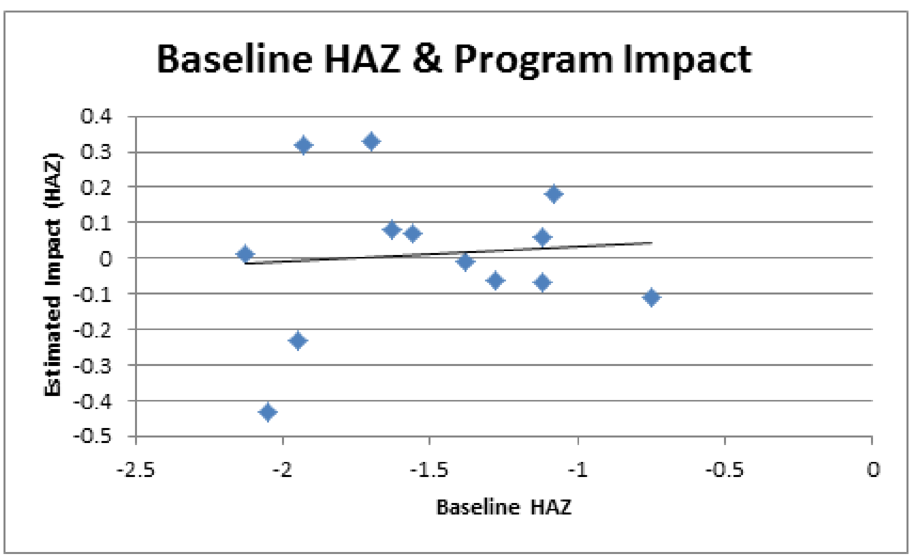
Figure 3. Program duration and estimated impact on HAZ



Note that one outlier, a program measuring impacts after 80 months, was dropped.

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 17 programs we have baseline HAZ scores for 14, shown in Figure 4.6. When those scores are regressed on impacts we do not find a statistically significant impact. However, the estimated relationship is in the expected negative direction. The estimated coefficient is -0.06 (p-value =.50) which indicates 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, though it is at best a weak indication since it lacks statistical significance.

Figure 4. Estimated program impact on HAZ vs. baseline HAZ



Next we turn our attention to study-level covariates, including whether studies are published, whether the study was randomized, and the baseline HAZ. Results are summarized in Table 7.

Table 7. Study Characteristics (Pooled Analysis)

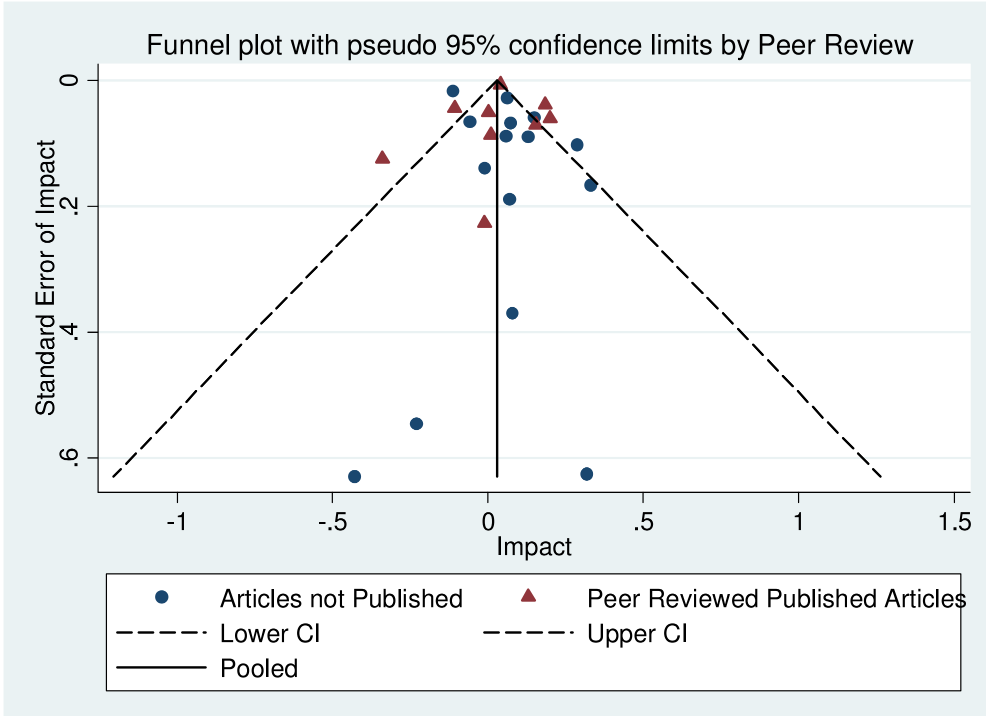
Impact on HAZ	Mean†	Est'd Impact	P(t)	Lower	Upper	I^2	τ^2
Published Studies: Study Averages	33%	0.76	0.30	-0.74	2.25	99.96%	3.12
Published Studies: Program Averages	29%	1.53	0.03	0.14	2.9	99.5%	1.3
Randomized	53%	0.21	.77	-1.26	1.69	99.96%	3.25

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

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 -0.02, 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 5, a funnel plot contrasting peer reviewed and non-peer reviewed work. Bias may not always be positive as cash transfer programs that decrease HAZ might be potentially more publishable results. The two papers with statistically significant negative results are peer reviewed. We regressed the absolute value of the mean impact estimated for each program (the dependent variable) on the percent of papers on the program that were published. The estimated t-stat for peer reviewed works was 1.53 higher than non-peer reviewed works with a p-value of .03.

Figure 5. Funnel Plot Showing Estimated Effects by Peer-Reviewed Status



Next we compare studies with a randomization mechanism and those who utilize non-randomized estimation techniques. There were nearly equal numbers of randomized and non-randomized studies. Papers that used randomized techniques saw slightly large program impacts (0.21); however, this difference was not statistically different from zero (p-value .77).

We now examine the link between CT impacts and the observed child's characteristics, summarized in Table 8. Six programs analyze 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 program. In addition to the pension program we have separate estimates by gender for six programs. On average girls see impacts that are 0.20 HAZ larger. A meta-regression with 12 observations (two each for the six programs) shows that the effect is marginally different than zero (p-value = 0.052) suggesting the result is not unique to Duflo's findings. When we rerun the regression dropping one program at a time, the results are consistent although in some cases the result is fully statistically significant. In no case do p-values exceed 0.15.

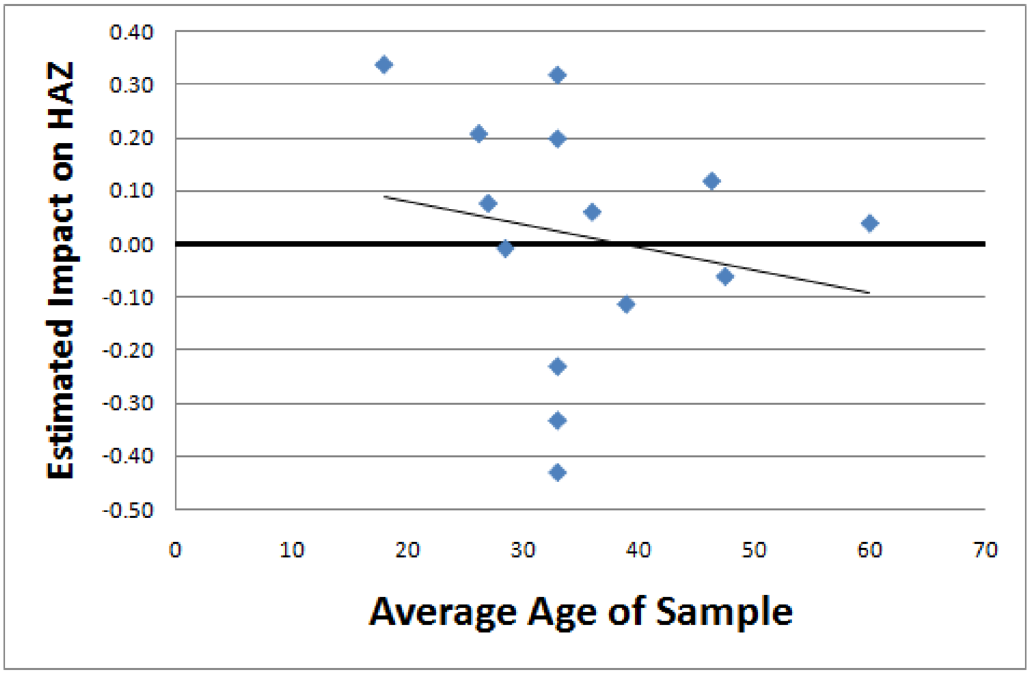
Next we estimate the influence of the age of participating children (see Figure 6 and Table 8). For this analysis we remove one outlier, the Primary Education Stipend, with its average age of about 150 months, more than 2.5 times our second highest. Including the outlier the relationship between age of measured children and impacts is positive although not significant. Dropping the program we see the expected negative sign with a one standard deviation increase in the mean age of measured children decreasing HAZ by .05 z-scores. Next we construct a sample of 18 observations by taking the oldest and youngest age cohorts from the nine studies with multiple age cohorts listed. Here we find that as the mean age increases one month, the impact falls by .002 z-scores. The result is small and statistically insignificant (p-value = .17) but consistent with a negative relationship between the age of treated children and program impacts.

Table 8. Influence of Child Sex and Age

Impact on HAZ	Mean	SD	Est'd Impact	P(t)	Lower	Upper	I^2	τ^2
All female sample	NA	NA	0.202	0.052	-0.002	0.408	52%	0.011
Mean age (N=17)	41.7	39.9	0.001	0.287	-0.001	0.004	73%	0.015
Mean age (removing outlier: N=16)	34.6	9.9	-0.005	0.115	0.115	0.001	73%	0.009
Mean age (Programs with multiple age cohorts: N=18)	34.0	24.1	-0.002	0.187	0.187	0.001	24%	0.000

A simple meta-regression was performed for each covariate.

Figure 6 Age of sample and estimated impact on HAZ



The final covariates of interest are the WDI variables showing national health conditions. Table 9 summarizes our results. In each case, worse health conditions and lower health care utilization correlate with larger program effects on height for age z-scores. "Worse health conditions" in these countries includes higher infant mortality, fewer hospital beds, a lower treatment percentage for children with respiratory infections, and a lower prevalence of sanitation. Both infant mortality and hospital beds per 1,000 show significantly greater impacts in countries with poorer health conditions. In both an improvement of one standard deviation is associated with a decrease of 0.09 HAZ. The results are reasonably robust to eliminating any one program, as the average effect size stays relatively constant and the maximum p-value is 0.12. The other three measures are consistent with these, but the measures are not statistically different from zero.

Table 9. Country-level Covariates

Impact on HAZ	Mean	SD	Est'd Impact	P(t)	Lower	Upper	I^2	τ^2
Infant Mortality Rate	38.7	17.2	0.006	0.03	0.001	0.01	70%	0.009
Hospital Beds per 1,000 People	1.06	0.69	-0.141	0.02	-0.26	-0.02	67%	0.008
% of children with Acute Respiratory Infections who see a doctor	46.5	15.4	-0.005	0.19	-0.013	0.002	70%	0.011
% with Improved Sanitation	46.9	15.3	-0.001	0.60	-0.007	0.004	72%	0.0163

A simple meta-regression was performed for each covariate.

One concern is that covariates might be strongly correlated with each other. For example, 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 include control variables in regressions, but we can identify variables that are related. We ran correlations between the covariates, and most variables showed correlations below 0.6. We highlighted larger correlations above, including sample age and supplement use, conditionality types and country health indicators, and transfer size and health conditions. One other pair of variables could bias the results.

Somehow peer reviewed studies are correlated with positive local health conditions, perhaps because data sufficient to support more rigorous analysis is more available in more developed countries. The correlation with infant mortality is -0.45 and that with hospital beds is 0.58. Thus, peer reviewed articles may be indirectly linked to smaller observed impacts.

VI. Discussion

By systematically going through the literature on cash transfer programs, we identified many studies assessing impacts on nutritional status. Meta-analysis shows that on average programs have a positive but insignificant effect on nutritional status, verifying previous researchers' observation that such programs have inconsistent effects on child nutritional status. (It bears repeating that the programs have achieved success on other fronts such as improving education or decreasing child labor). Third, we have identified several program, child and study characteristics that are correlated with improved nutritional status:

- Conditionality is weakly negative, though programs with conditions unrelated to health show a statistically significant negative impact on HAZ.
- 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.
- In the subsample of studies with multiple estimations we find larger impacts for girls and younger children
- Higher infant mortality rates and fewer hospital beds are associated with larger marginal program impacts on HAZ.

These last findings fit with the UNICEF (2010) finding that returns to investments in child health are highest in remote rural areas. They note that remote populations generally have a larger proportion of children than other groups due to higher fertility rates. Also 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. Investments may be most cost effective in these areas.

We find that health-based conditions do lead to increased gains in nutritional status, though we also find that unconditional programs could be effective in increasing nutritional status. This also highlights a potential danger: making transfers conditional on non-health related behavior such as pushing parents out to work or forcing them to meet savings requirements has a clear, negative effect on child development.

Economic models would suggest that parents are already optimizing, and pushing them to work at regularly available rates of return may be detrimental to their children. Others find that conditions can limit household coping responses to shocks (de Janvry et al. 2006). A meta-analysis of workfare programs in the US and Canada (Duncan, Morris, and Rodrigues 2011) finds that programs pushing parents to work do not improve child development, but when such programs are supplemented with added income, the results are there. 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 programs are more effective at getting kids to school. However, conditional programs also have negative side effects, and they recommend unconditional programs for some purposes.

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 programs.

We find only limited support for the importance of increasing payment size, though our data did not permit us to compare payment size to baseline income. This finding does square with the only program we are aware of that randomized transfer sizes. Baird et al. (2009) 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 program "relatively insensitive" to payment size.

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.

Environmental issues matter for nutritional status, as Table 2B shows that almost every program was associated with increased food consumption and/ or food diversity, a positive development. However, we see no consistent effects on nutritional status. Clearly improved access to food alone is not sufficient to improve nutritional status.

Finally, we call for more data. Bouillon and Tejerina (2007) 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 programs through a deep engagement with the qualitative literature, there is also a need for more quantitative impact evaluation. Dozens of programs are currently operating, but we were only able to identify impact evaluations for a small minority, and data on nutritional status for an even smaller subset. Given the relative ease of implementing anthropometric evaluation, this should change. Hopefully the availability of new NGOs such as 3ie and the increasing accessibility of impact assessment methodologies will increase the share of programs that are evaluated both quantitatively and qualitatively.

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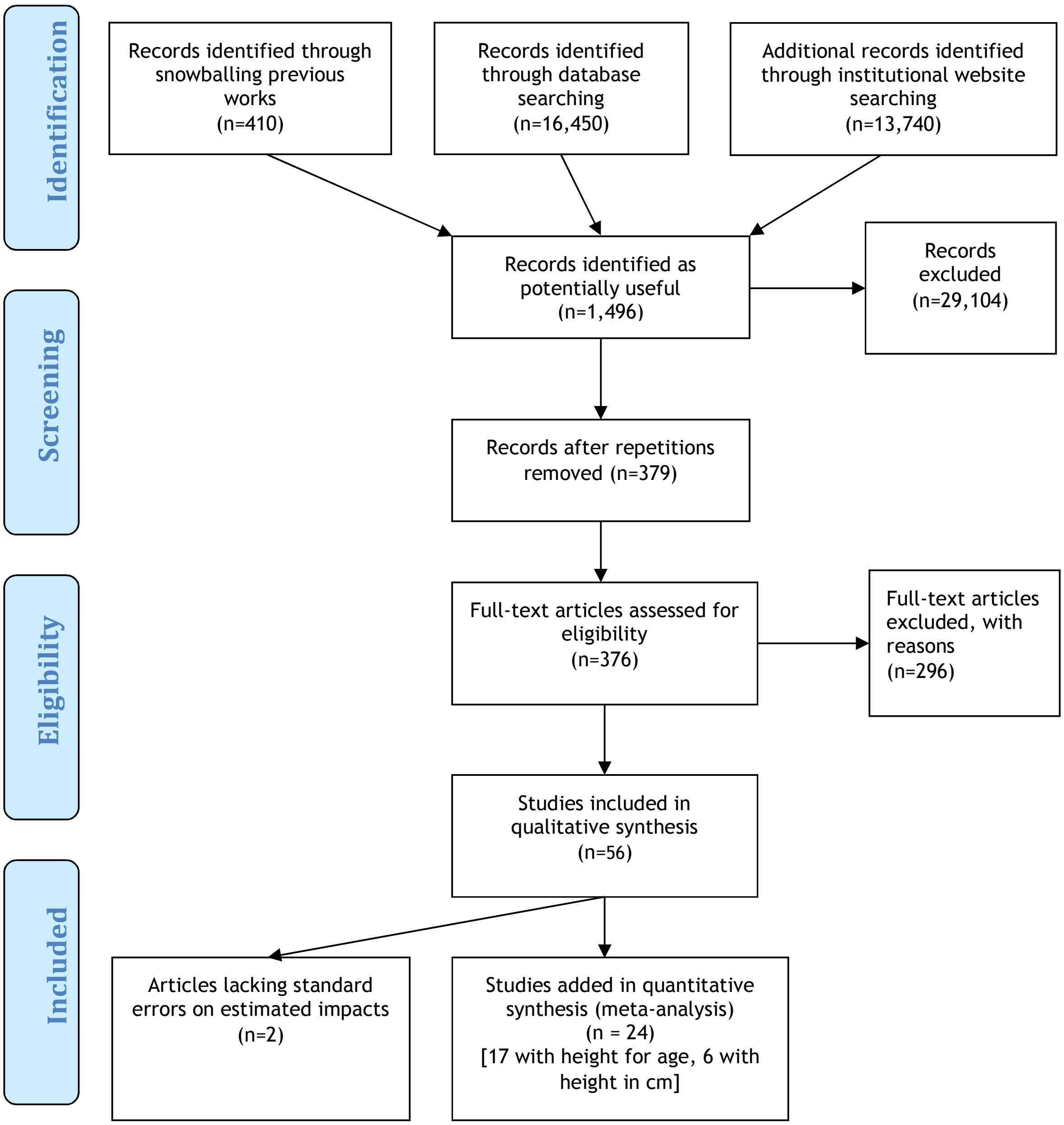
Appendix: Summary of Search and Included Programs

Prisma Flow Diagram



The present program list contains 6 conditional cash transfer programs 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 five unconditional cash transfer programs 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 program is not included in the main quantitative analysis.) Some of these programs have undergone significant changes from their original design. For example, BDH was intended as a transfer program 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 behavior. However, all the studies for the present analysis are based on a timeline when the programs were fully unconditional.

The rest of the program list is occupied by some rather idiosyncratic interventions such as the Bangladeshi Primary Education Stipend (PES). This program 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 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 programs which do not fit neatly in any intervention group are the Bangladeshi Food Security Vulnerable Group Development (FSVGD) program, the Food for Asset Creation (FFA) and the Rural Maintenance Program (RMP). The common features of the three programs 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

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

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 labor to the community depending on the size of the transfer.

It is important to note that the amounts of cash transfer vary significantly across programs from 4 per cent of pre-intervention income level in Honduras to about 250 per cent of pre-program 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 programs, so we use USD($) conversions if presented in the papers or else dated exchange rates.

Four of the analyzed programs 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 programs 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.

Information on program 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 program 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 program benefits is expected to influence the impact estimates of height for age or height in centimeters, especially since height for age is a measure of long-term nutritional status.

The Mexican CCT Oportunidades is the largest program of its kind in the world. In fact, many programs 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 program 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 program performance in both the rural and urban areas are used in this meta-analysis.

Bolsa Alimentação, part of the larger government program Bolsa Família, is a national-wide intervention with a focus on nutrition. No ex-ante stipulations were made for a program 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 program was launched in Nicaragua, Atención a Crisis. It was designed for extremely poor, drought affected areas. Randomization was built into the design of the program 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 program 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 programs, 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 programs, 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 program 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 programs, which posed problems with respect to evaluation.

In short, the 17 programs 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.