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2013

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Ryan F. Boone
Claremont McKenna College

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CLAREMONT McKENNA COLLEGE

CONDITIONAL CASH TRANSFERS AND CHILD HEALTH: THE CASE OF MALAWI

SUBMITTED TO

PROFESSOR HEATHER ANTECOL

AND

DEAN GREGORY HESS

BY

RYAN FURAY BOONE

FOR

SENIOR THESIS

SPRING 2013
APRIL 29, 2013

Acknowledgments

I would like to start by thanking Professor Antecol for her numerous edits, her patience with my inability to focus my topic, and all of her helpful advice. Professor Helland also needs to be thanked for his invaluable econometric help. Of course I am very appreciative of my family who always support me, believe in me, and encourage me in whatever I do. I also owe a debt to the entire CMC community; all of my friends who helped me with edits, listened to me complain, gave me advice, and brought me late night coffees at the Rose Institute. Finally, my uncle who encouraged me to study development economics and took me under his wing, as well as everyone I worked with at the FAO in Rome and at IPA/J-PAL in Rabat, have my full gratitude.

Abstract

This paper analyzes the impacts of the Malawi Social Cash Transfer Scheme. The goal of this paper is to help improve the design of cash transfers. First of all, I analyze whether the cash transfer positively affects child health variables despite occurring in a region with poor supply side health institutions. I find significant results for many child level variables, such as frequency of illnesses, but insignificant improvements in anthropometric measurements. Secondly, I examine whether female-headed households invest more in child health than male-headed households. The results show that the impacts of the cash transfer did not depend on the sex of the household head. This result provides some evidence that females do not always have systematically different preferences for expenditure on children than males. The paper uses the imperfect randomization of the cash transfer in combination with difference-in-differences regressions, propensity score matching, and Lee Bounds tests in order to ensure the robustness of the results.

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1. Introduction

The intergenerational transfer of poverty is an especially distressing occurrence. The consequences of poor nutrition and health remain with children for the entirety of their lives. Dr. Martorell (1997) outlined the long term effects of malnutrition for children; reduced IQ, physical growth, increased behavioral problems, decreased attention, and lower educational achievement. These effects persist throughout life and occur even among children without the clinical signs of malnutrition. Poor health even reduces the ‘basic capabilities’ that Amartya Sen (1999) shows are crucial to having an acceptable quality of life and escape poverty.

Many aid programs, such as the UN’s World Food Program, try to address the immediate causes of poor health by increasing food and nutrition intake. These programs might be failing to address some of the underlying causes. Poor health and nutrition are caused by poverty, inequality, food insecurity, and an inability to access supply side institutions. According to Leroy et al. (2009), cash transfer programs could address both the underlying and direct causes of childhood malnutrition. Cash transfers were first popularized in South America and have since expanded throughout the world. The transfers provide regular cash payments, often conditional on certain behaviors. The purpose of these programs is to encourage investment in human capital while at the same time providing aid to the poor. Proponents of these programs view cash transfers as preferable to in-kind transfers because they can cause fewer market distortions and provide the poor with more expenditure flexibility. The majority of the programs are contingent on health and education-seeking behavior, especially in the cases of children and pregnant women.

Mexico’s Progresa program (later renamed Oportunidades) was the first comprehensive national conditional cash transfer program. Oportunidades provides cash transfers to households with additional benefits for children enrolled in school. It also offers nutritional supplements and incentives for preventative care and now serves over six and a half million families. This program has been extensively studied due to the fact that the program began with a staggered rollout that provided economists with an experimental counterfactual. Gertler (2000) finds a reported reduction of sickness in the previous four weeks of 4.2 percentage points for children under the age of three. Gertler (2004) finds that children whose families had received Oportunidades were taller and were 25.5% less likely to be anemic. Van de Gaer et al. (2013) notes positive impacts of the Oportunidades program on anthropometric measures with the results being most pronounced for poor indigenous children.

Many other countries have started similar programs. For example, Brazil implemented its *Bolsa Familia* program, which focuses on incentivizing education. Other South American countries have followed suit as well, such as Chile, Columbia, Honduras, and Peru. The success of these programs has inspired countries outside of the Americas to begin implementing their own cash transfer programs. A key component of many of these programs has been targeting transfers towards females because it is believed that women are more likely to spend the transfers on improving the well-being of their children (Quisumbing et al., 1995).

Lagarde et al. (2009) conducted a comprehensive review of the literature on many of these cash transfer programs and their impact on the health status of household members. The surveys that they included in their reviews were of a high standard. Most were performed under either experimental or quasi-experimental conditions and the resulting papers contained minimal methodological flaws. They find that the cash transfers increased the use of health services and improved nutritional status as well as anthropomorphic health indicators. However, they do not find evidence of effects on health care expenditure. They conclude that it is unclear as to whether these programs would produce similar results without conditional elements or whether most of the beneficial effects are simply a result of the additional spending power from the cash transfer. According to Davis et al. (2012), there still remain significant unanswered questions as to what the effect of the conditional elements of cash transfers are. Lagarde et al. (2009) end their review by stating that the replicability of their results needs to be tested under different conditions; specifically in deprived settings such as in areas of sub-Saharan Africa. They note that the lack of effective primary care facilities might alter any health outcomes of conditional cash transfers. Cash transfers help households by providing cash to generate demand, but supply side issues might prevent improvements in health if there are not adequate health facilities.

Recently, several African countries have created their own cash transfer programs and begun research to evaluate whether or not the programs are effective. These programs are fundamentally demand side solutions. They encourage the use preventative medicine and the accumulation of education, but if effective supply side institutions do not exist then they might prove to be ineffective. Many countries lack the proper infrastructure, especially in rural communities where the need is most dire. Monitoring the conditionality of the programs creates further constraints because it imposes additional costs and requires a professionalized bureaucracy.

The goal of this paper is to bridge this gap in the literature by examining the effect of cash transfers in Malawi, a country that lags the Americas in terms of infrastructure. The Malawi Social Cash Transfer Scheme (SCTS) provides an ideal opportunity to access the efficacy of cash transfers without strong conditional elements. Approximately 22% of the population lives in ultra-poverty, spend almost all of their income on food, and fail to consume an acceptable minimum caloric intake. These people constitute the poorest of the poor in an underdeveloped country. The SCTS is targeted at the neediest 10% of the population with the goal of reducing ultra-poverty rates in the country to 10% by 2015. The program is effectively unconditional, though its recipients are encouraged to use the cash transfer for educational purposes. They also receive additional funds based on whether or not the household contains school-aged children. These cash transfers are also not specifically targeted towards females.

I use the quasi-experimental conditions of the SCTS program to test if cash transfers significantly improve the health of children even in the absence of strong conditional elements. Propensity score matching and differences-in-differences techniques allow me to correct for any flaws in the experimental design. Moreover, I examine whether female household heads have systematically different expenditure preferences than male household heads. I generally find positive impacts on child health. For example, the percentage of children who were sick in the last month decreased by 9.4

percentage points and I find a decrease in the number of respondents who said they were unable to seek care by 6.4 percentage points. Unfortunately, I do not find improvements in anthropometric measurements (such as height for age, weight for age, and bmi for age). There also seems to be no systematic difference in expenditure preferences for female and male headed households. This finding does not prove females in general do not have systematically different preferences. Instead, it provides evidence that it is important to consider the cultural context rather than assuming that it is always better to provide the cash transfers solely to females.

The paper is structured as follows. Sections 2 and 3 present the theory and data, respectively. The methodology and results are discussed in Sections 4 and 5, respectively. Finally, conclusions are presented in Section 6.

2. Theory

2.1 Program Theory Framework

There are multiple channels through which cash transfers might improve the health and nutrition of children. Leroy et al. (2009) outline the major pathways through which cash transfers can improve the health of children. They begin by outlining the most basic mechanisms through which the cash transfer programs can work; increased purchasing power for the household, the provision of fortified products, education of household decision makers about health and nutrition, and conditional elements, such as having to attend school or visit health clinics. Leroy et al. also include various intermediary pathways. For example, the increased purchasing power might free additional time to care for household members or it might allow the household to purchase food with higher nutritional content.

These are all elements common to many cash transfer programs and therefore it has been difficult determining what mechanism is the most effective. The cash transfer in Malawi is particularly helpful in this debate because it does not include conditional elements and it does not provide any nutritional supplements. Therefore the Malawi cash transfer should only affect the nutrition and health of children through the increased spending power that it gives households. It is important to discover if the cash transfers are primarily effective through this pathway because monitoring requirements add additional costs to the program as well as decreasing the uptake rate. If there are significant impacts on the nutrition and health of children in the Malawi SCTS then it shows that providing poor households with increased purchasing power might be an effective way to improve child health outcomes in countries with poor supply side infrastructure.

2.2 Household Decision Making

There is also the theoretical question about whether a household should be treated as a single unitary decision making entity. If this assertion is true then the recipient's gender should not be a factor, but there have been both theoretical reasons and empirical reasons to reject his model of household decision making. Manser and Brown (1980) developed a model of intra-household bargaining. They recognized that different individuals in the household have different preferences and that household decisions are bargained results. The distribution of power in a household (whether power from income or from social norms) effects what decision is reached. If males and females in a household have systematically different preferences then the gender of the recipient of the cash transfer might be pertinent. Giving the cash transfer to the female in a household theoretically increases her bargaining power. If the females systematically prefer expenditures on the well-being of children in comparison to men, then the most effective solution to increase the human capital accumulation for children is to give the transfers to females.

Chiappori (1988) created a theoretical model for shocks to non-labor income, such as a cash transfer. The household is composed of H members. Every household member receives non-labor income in the current period of y_t^h. They can purchase at p_t a vector of goods (q_1,…., q_N). His model assumes that there is no borrowing or saving from one period to the next for the sake of simplicity. The utility of each member of the household depends on the consumption bundles purchased by every other member in the current period and on an individual specific shock ϵ_t^h with mean ϵ^h.

$$V^h(q) = E \sum_{t=1}^{T} \delta^t u^h(q_t, \epsilon_t^h)$$

The household will maximize $\sum_{h=1}^{H} \theta^h V^h(q)$ such that $\sum_{h=1}^{H} q_t^h p_t = \sum_{h=1}^{H} y_t^h = y_t$ for t=1,…,T. Duflo (2000) uses this model as a basis to examine whether the gender of recipients of a South African pension scheme affects the impact on grandchildren. Duflo shows how a permanent exogenous shock in non-labor income affects the household member's weight in the optimization problem by increasing the member's outside options. If women have systematically different preferences than men, an exogenous shock to the non-labor income of a female should have different effects on child health than an exogenous shock to the non-labor income of a male.

There has been research suggesting that females do have systematically different preferences. Thomas (1990) finds that income from assets owned by women significantly and positively affects child nutrition and health as well as leading to larger expenditure shares for health, nutrition, and housing (Thomas 1994). Lundberg et al. (1997) use a natural experiment created by a change in the mode of allocating child benefits in the United Kingdom from a tax credit to a direct payment to the mother. They find that the change was associated with an increased share of expenditure on women's and children's clothing relative to men's clothing. Duflo (2000) also finds that the gender of the cash transfer recipient in the South Africa pension scheme affected outcomes. When the recipient was female, the anthropometric measurements of grand-daughters of the recipients improved. Duflo warns that care must be taken when generalizing the results; difference in social and cultural norms could influence outcomes.

If the gender of the recipient does matter, then it does not necessarily mean that cash transfers to females produce more desirable results. There have been many papers showing that giving cash transfers to women has positive impacts on the well-being of household members[1], but many of these studies lack a proper male counterfactual (Gutierrez et al., 2011). Gutierrez et al. examine an unconditional cash transfer to older adults in Mexico City and find that the gender of the recipient created different effects. Transfers to females resulted in higher household expenditures on children while giving the transfer to males increased school enrollment rates.

The hypothesis that females have systematically different preferences is tested in this paper by examining if the impact of the cash transfers on child health is systematically different for similar male and female headed households. If the impact is significantly different, then it provides evidence that the hypothesis is correct. The fact that ultra-poor female headed households might be systematically different than ultra-poor male headed households for unobservable reasons does complicate the answer. However, even if they are the results could still provide evidence that it is more effective for the cash transfers to be targeted at female-headed households. It is important to note

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[1] See for example Thomas (1990), Rubalcava and Contreras (2000), Glewwe and Olinto (2004), Maluccio and Flores (2005), and Attanasio et al. (2009).

that this paper is not testing whether the cash transfer affects intra-household bargaining; rather it is testing the underlying assumption that females have systematically different preferences than men.

3. Data

3.1 Program Description

The Malawi SCT program began in 2006 in the Mchinji district of Malawi. The goal of the program was to eventually expand to the national level. The Mchinji district pilot program allowed the government to assess the design of the program as well as to conduct an impact evaluation. The impact evaluation relies on the randomized phase-in of the program. In order to qualify for the program households had to be labor constrained, which was defined as the household lacking able-bodied adults between the ages of 19 and 64 or having a dependency ratio[2] worse than three. It was also necessary for the households to be ultra poor, which was defined as consuming one meal or fewer per day and lacking valuable or productive assets. Several factors influenced the size of the cash transfer received. Households received a base transfer of $4.30 per month and an additional $2.85 per household member up to four total members. An additional $1.42 for every primary school aged youth and $2.85 for every secondary school aged youth was provided to recipient households. These cash transfers lacked explicit conditions; that is, there were no monitoring requirements like those found in many South American cash transfer programs. Recipients of the cash transfers did receive some social marketing promoting the use of the cash transfers for health and education. The average recipient household received approximately $14 a month. In the Mchinji district, this transfer was large enough to raise the income of recipients from the lowest decile to above average

[2] For the purposes of the SCT the dependency ratio was defined as the sum of children younger than 19, the elderly older than 64, and the number of chronically ill or disabled adults aged between 19-64, all divided by the number of able-bodied adults aged 18-64 (Miller et al., 2011)

(Miller et al., 2008). Therefore this transfer represents a very significant infusion of cash into recipient households. While many cash transfer programs have larger absolute transfers, this program has a much larger transfer relative to typical earnings in the community compared to other programs in Africa (Ghana, Mozambique, Kenya, and Zambia) and cash transfer programs in other parts of the world (Jamaica, Mexico, and Columbia) (Osei et al., 2012).

This paper analyzes household, adult, and child level data from an impact evaluation survey administered in 2007-2008. The survey assessed the effect of the pilot SCT program in the Mchinji district of Malawi. Eight Village Development Groups (VDGs) were chosen to participate in the survey. The eight VDGs are comprised of a total of 23 villages and each VDG has roughly 1,000 member households. Four of the VDGs were randomly chosen to be part of the treatment group and began receiving the transfer in 2007 immediately after being administered the baseline survey. The other four VDGs were assigned to the control group with eligible households receiving transfers in 2008 after the final survey was administered.

In each of these VDGs, Community Social Protection Committees (CSPCs) ranked households to determine their eligibility status. The goal of the program was to provide cash transfers to the neediest 10% of Malawian households. Over 10% of the population in these groups met the uniform national eligibility criteria to be included in the program. The ranking process at the local level conducted by the CSPCS played a critical role because only 10% of households in the VDGs could receive the transfer. The total number of households deemed to be eligible ended up being 802 because each VDG group contains roughly 1000 households and there were eight groups. Only eligible households, in both the treatment and control groups, were surveyed and they were visited up to three times to ensure the completion of questionnaires (Miller et al., 2011). Take-up rates were near universal because the SCT was unconditional.

There were three rounds of surveys. The baseline questionnaire was administered[3] in March 2007 which was just prior to commencement of treatment, the midline questionnaire was administered in September 2007, and the endline questionnaire was administered in April 2008 just before the control group received their first cash transfer. The baseline and endline surveys were both administered after the December-March rainy season when food stores have been exhausted in the 'hunger season'. The midline survey was administered during the harvest season when households have a relative abundance of food from the recent harvests. A total of 751 households completed all three rounds of questionnaires. Fifty-one eligible households, 6% of the original sample, were excluded from analysis because they did not complete all three rounds of the survey.[4]

3.2 Differences Between Control and Treatment Groups for Independent Variables

The surveys contain a variety of detailed information on demographics, health, education, income, and expenditures. There are a considerable number of questions

[3] The Boston University Center for International Health and Development in conjunction with the Center for Social Research of the University of Malawi administered the survey and was responsible for the survey design and data entry.
[4] Twenty-three of the households were lost due to death. Therefore only twenty-eight households failed to complete the survey for other reasons, which is only 3.5% of the original sample.

specifically relating to health and nutrition, including data on the types of food consumed, anthropometric measures (e.g., height and weight), expenditures related to health, sickness, disabilities, and general physical well-being. The questions varied slightly across the rounds of the survey with a general trend of adding more detailed questions in later rounds.

There were only eight VDGs that were randomized, which provides only 800 households. The national eligibility criteria were rather strict and straightforward. Unfortunately, due to more than 10% of households meeting the eligibility criteria in the Mchinji district, the final selection took place at the community level. Different communities placed emphasis on slightly different household characteristics when ranking the households. These differences create some baseline differences between the treatment and control households (Miller et al., 2008). If there were a larger sample of VDGs the selection criteria should be on average relatively similar because there is no reason to believe that the randomized treatment and control VDGs employ systematically different selection criteria. The small number of communities means that care needs to be taken to examine the variation in baseline characteristics. Statistical methods are required to control for any significant variations in characteristics between treatment and control households. These statistical methods are discussed in detail in Section 4.

First, this paper establishes the validity of the counterfactual and examines any relevant differences between the control and treatment groups. There are two main types of relevant variables for this analysis, (i) the program eligibility criteria, and (ii) general variables on basic characteristics of the household and the household members. These baseline summary statistics are shown in Table 1. T-tests are used to see if there are statistically significant differences in the baseline characteristics of the control and treatment groups.

Table 1 reveals there are significant differences in baseline characteristics across the treatment and control groups. These differences are especially pronounced for household demographic structures. For the variables related to eligibility criteria, treatment households are significantly more likely to have relied upon begging at some point. They are also much more likely to have a dependency ratio above three. The treatment households are also more likely to have experienced natural shocks in the form of droughts or floods between 2005 and 2007. This susceptibility to natural disaster might explain why treatment households are more likely to have resorted to begging. Control households, on the other hand, tend to be older and less educated.

These differences show that concerns related to the small number of Village Development Groups are valid. The CSPCs in the treatment group seemed to prioritize households with orphans and higher dependency ratios, while those in the counterfactual seemed to prioritize the elderly. Because younger Malawians are more likely to have had access to education, these differences in prioritizations also explain the gap in education levels of the household heads. These differences persist in the adult level data with adults in the treatment group being younger and better educated on average. These differences demonstrate the need to use statistical tools to account for these differences in order to ensure unbiased results. These adjustments are discussed in detail in the methodology section.

Control households anticipating their future receipt of the cash transfer might systematically alter behavior in the control group. These households knew that they were eligible to receive the transfer at a later date. Due to the program being new and not widely known, the exact details of the program are unlikely to be well known. There is also some uncertainty in the households as to whether they would actually receive the cash transfer in the future. Therefore the anticipation effects do not seem likely. Additionally, the main effect of anticipating the cash transfer would be to increase expenditure in these households as a form of consumption smoothing. This increased expenditure would actually decrease observed effects and if positive health benefits are discovered, then the possible existence anticipation effects should increase the robustness of the results.

3.3 Dependent Variables

Table 2 presents the summary statistics for the child level variables being analyzed. A variety of variables related to health and nutrition are used to examine the impacts of the treatment. The first group of variables consists of respondent reported measures of health. An adult respondent would answer the questions for the children in the household. The first question analyzed is whether the child has been sick in the past month. If the child had been sick, the respondent was asked what the child had been sick with. The most common illness was malaria so a variable was included measuring its prevalence. Malaria is also a useful indicator. Because simple measures, such as buying mosquito nets, can be used to reduce its risk, it can serve as an indicator if households are using the cash transfer to invest in precautionary measures.

The next question captures whether the child has been sick over a month. The purpose of this variable is to see if the household tries to get treatment for recurrent illnesses after receiving the cash transfer when they previously have not. A relevant follow-up is if the household failed to seek any form of treatment for their previous illness (any type of medicine or help whether 'Western' or traditional) and another variable indicating if the household failed to get treatment because of a lack of money.

The next several variables try to assess the impact of illness by asking if the child had to stop normal activities because of illness or if others had to stop activities to care for the sick child. Both of these questions are followed up by questions asking the number of days that activity was stopped. Ideally, the cash transfer should reduce the necessity of stopping activities by preventing disease or increasing resilience, but there might be other effects as well. The cash transfer might reduce the opportunity cost of stopping regular activities, which could result in longer periods of inactivity due to illness. The results will tell whether either of these effects dominates the other.

The next set of variables consists of anthropometric variables. The survey team conducted several types of measurements for the members of the household. First, they weighed every child in kilograms. Next for children under the age of two, the survey team measured the length of the child laying down in centimeters. Finally, for children over the age of two, the survey team measured the child's standing height. There are slight differences between measuring the length of a child and measuring the height of a child (on average around .7 cm according to the WHO reference population), but this discontinuity is easily corrected. Henceforth, height is used to refer to both height and length measurements.

The height of a child represents the long run health and nutrition of a child. Height at any given age for a child represents years of accumulated investments that the household has made in nutrition and health (Martorell and Habicht 1986). Therefore, stunting and other height related problems represent years of undernourishment. Weight is much more variable in the short run and is most likely to provide evidence of improvement with the intervention. There are several issues with simply using weight to analyze the impact of the program. Weight is heavily dependent on height and age as well as nutrition. Therefore, this paper primarily uses a constructed Body Mass Index (BMI) calculated by weight in kilograms divided by height in meters squared.

The analysis utilizes a reference population developed by the WHO. This reference population represents what child growth should be under ideal circumstances, i.e., those with minimal constraints to growth. The WHO recently undertook a major project in order to create a set of growth standards for children under the age of five. This was because of criticism that the old NCHS study from 1977 represented children of only one country with a limited ethnic background. Therefore, the WHO set out to answer these criticisms. They studied numerous children raised in six different countries under optimal conditions.[5] After they determined that the ethnicity was not a crucial factor for

[5] They studied 8,440 children under the age of five in Ghana, the United States, Brazil, India, Norway and Oman to check if results were consistent across ethnicities using advanced statistical techniques, such as. All of the children came from optimal conditions; breastfeeding, good diets, prevention and control of infections, the mother did not smoke, and healthcare was provided (WHO Child Growth Standards 2006).

the countries chosen, they merged the data set with the 1977 NCHS study to create consistent standards from birth to age nineteen. To ensure the validity and continuity of the combined sample, the Box-Cox Power Exponential method is applied along with appropriate diagnostic tools.[6]

The reference population is used to estimate how far these children are from ideal growth in height, weight, and BMI. Following the lead of other researchers, such as Duflo (2000), height for age, weight for age, and BMI for age *z*-scores are constructed. These scores are constructed by subtracting the median of the reference population for the relevant age and gender and then dividing the result by the standard error of the relevant reference population.[7] Unfortunately, weight for age z-scores could only be constructed for children younger than ten years old. The age groups used are by age in months and gender which allows relatively granular and specific assessment. The primary issue with this approach is that the results are based on changes in *z*-scores, which makes interpreting the exact level of impact difficult. But, the main concern is whether there is an effect since the beneficial effects of nutrition and health on weight and height are cumulative and therefore increase over time.

[6] For more details please see pages and articles at http://www.who.int/growthref/growthref_who_bull/en/index.html.
[7] For data and a detailed guide of how to replicate this process along with useful STATA tools please use http://www.who.int/growthref/tools/en/ for children aged 5-19 and http://www.who.int/childgrowth/software/en/ for children from birth until five years of age.

4. Empirical Approach and Results for Child Health Outcomes

While there are, on average, some differences in targeting methods between the treatment and control groups, the random assignment of communities along with the application of national eligibility criteria suggests that the groups do not radically differ from each other. They do not have productive or valuable assets, they lack food security, and they live in ultra-poverty. Despite these strong similarities, econometric techniques are used in order to create a more valid counterfactual. This paper utilizes two approaches; a difference-in-difference estimator and matching approaches. Because the variation between the counterfactual and treatment groups are due to variations in targeting criteria, rather than due to self-selection or other primarily unobservable characteristics, the variation should be linked to observable demographic characteristics. This paper uses data on these observable characteristics in order to create a valid counterfactual. I also use a Lee Bounds test in order to ensure the robustness of the results.

This section is structured as follows; first I outline theoretical basis of the difference-in-difference and propensity score matching methodology. I then apply these techniques to the general child health results. Next, I outline the difference-in-difference and linear combination framework used for determining if results differ by household gender; followed by the results. Finally, I discuss Lee Bounds tests and present the results from these tests.

4.1 Difference-in-difference

First, a difference-in-difference, or double difference (DD), method is used. The purpose of the DD method is to control for time-invariant unobservable differences at the baseline in case there are problems with the experiment's random design (Ravallion, 2005; Gertler et al., 2011). The DD estimator regression framework is as follows:

$$ch_{it} = \alpha_0 + \partial_1 post_t + \partial_2 treatment_i + \beta_1 post_t * treatment_i + \alpha_1 X_i + \alpha_2 Z_{it} + \varepsilon_{it}$$

where ch_i is the child health indicator variable of interest for child *i* in time *t*, $post_t$ refers to whether the data is from pre (t=0) or post (t=1) treatment (post treatment is used exclusively to signify data from round three[8]), $treatment_i$ indicates that the child *i* belongs to a household in the treatment group, X is a vector of baseline observable characteristics of the household that the child *i* belongs to, as well as time invariant demographic characteristics of the child. I control with baseline characteristics because the communal selection process was conditioned on these baseline characteristics. Z is a vector of time variant demographic characteristics of the child, such as age. ε_{it} is the error term.

The community selection processes resulted in a counterfactual that differed in statistically significant manner from the treatment group. For instance, the treatment

[8] Round one and round three are one year apart and are therefore more directly comparable than round two which occurred six months after the treatment began. Seasonality plays an important role in the results because round one and round three are during the 'hunger' season directly before the harvest while round two is after the harvest and occurs when food is relatively plentiful. Additionally, Bertrand et al. (2004) note issues with standard error consistency for DD estimations using many time periods of data so this problem is circumvented by relying solely on a pre and post treatment measurement.

communities prioritized households with orphans and higher dependency ratios while the counterfactual prioritized the elderly. These differences should be controlled by X_i which captures baseline observable characteristics of the household. The regression also includes the conditioning variables in X_i (such as meals per day and the lack of valuable or productive assets), which should improve both the accuracy and the precision of the results (Stock and Watson, 2003).

A variety of household level controls are used, including: eligibility criteria, household head years of schooling, age of household head, whether the household head is elderly, an interaction between a household head's educational attainment and the elderly household head dummy, and an interaction term for additional elderly household members when the household head is elderly. I also use some basic demographic characteristics, such as age in months and gender, though there is a lack of child level specific conditioning variables. A probit regression model is used on all dummy variables.[9] For all count variables I use a linear regression model. Lastly, all standard errors are adjusted for heteroskedasticity.[10]

The coefficient δ_1 controls for aggregate factors that would cause a change in ch_{it} even in the absence of treatment. δ_2 captures possible differences between the treatment

[9] Ai and Norton (2003) raised concerns over the inclusion of interaction terms in nonlinear regression models. A recent paper by Puhani (2012) addressed these concerns by noting that it is appropriate to focus on the sign of interaction terms for nonlinear difference-in-difference models and he notes that the coefficient provides an estimate of treatment effects with the nonlinear transformation rule. I checked whether using an OLS regression provides similar results in order to ensure the robustness of our results and they are similar both qualitatively and quantitatively.

[10] Because there are eight VDGs, estimating cluster-robust standard errors at the VDG level may seem to be appropriate. However, Cameron et al. (2008) find that for small numbers of clusters (between five and thirty) clustered standard errors are too large. This leads to a greater likelihood of rejecting the hypotheses. Further, it is not possible to use cluster robust standard errors with the matching procedures. In order to be consistent across models, and because the possible overestimation issues with cluster-robust standard errors, I use robust standard errors.

group and the counterfactual pre-treatment. Finally, β_1 is our DD estimate of the impact and should capture the effect of the cash transfer upon treatment households.

4.2 Propensity Score Matching

DD estimates should be able to create statistically reliable results because the regressions have adequate controls and the difference between treatment and control groups are based on observable characteristics. However, in order to ensure the robustness of the results this paper also relies on matching techniques. Propensity score matching (PSM) with kernel weighting is used to test the robustness of the overall results. Kernel weighting means that closer neighbors are weighted more than those neighbors with more distant PSM Values.[11]

PSM uses probit or logit models in order to calculate the predicted probability of a household receiving treatment based on observable characteristics;

$$e(x) = Pr\,(Treatment = 1|\mathrm{x})$$

As explained by Rosenbaum and Rubin (1983), the function e(x) is the propensity score and it gives the propensity towards exposure to treatment given the baseline observed covariates x. These probabilities are used to match households with similar propensities (e(x)) in order to create a valid counterfactual. This technique is most effective when groups differ solely based on observable characteristics (x) that can be used to match them. For testing overall results, the matched households have their results

[11] A variety of matching specifications were performed in order to check the accuracy and robustness of the results, including using five nearest neighbor matching. These results were broadly consistent with those from kernel weighting.

compared (for gender-related results households without adequate matches are simply excluded rather than matching households and examining the differences in results). PSM has been shown to provide experimental conditions by Heckman et al. (1997, 1998), as well as Dehejia and Wahba (1999, 2002), as long as the matching is accurate (no unobservable differences). Because the Malawi data has a variety of covariates on observable differences and differences are based on observable characteristics, the matching should effectively create close to experimental conditions and therefore an accurate indicator of the effects of treatment. Since village development groups are randomly assigned to control and treatment groups, PSM should be especially effective at creating experimental. The results of the probit regression that was used to calculate propensity scores are presented in Annex 1. All of the estimates use bootstrapped standard errors with fifty repetitions in order to ensure the reliability of the standard errors. This approach should also account for the fact that the propensity score is estimated (Diaz and Handa 2006).

The procedures developed by Abedie and Imbens (2011) are used in the PSM process that allow for biased-corrected estimates of average treatment effects. The matching procedure and the PSM procedure use the same set of confounders and the standard errors are adjusted for heteroskedasticity.[12]

For general results of impact, matching is used in combination with DD meaning that the matching is capturing differential trends over time rather than final differences in levels. This approach should control for any time-invariant characteristics at baseline that

[12] The PSM procedure using the Stata command psmatch2 with standard errors calculated using bootstrapping . The five nearest neighbor matching used the nnmatch command developed by Abedie et al (2004) and includes the bias adjustment and calculation of robust standard errors.

are not adequately controlled for. These results are generally consistent with the standard DD results with only slight changes in magnitude.

Table 1 shows that the baseline characteristics of the treatment and control groups after adjustment using kernel weighted propensity score matching. No statistically significant differences remain for baseline characteristics after PSM. The scores are also divided into five blocks so that balancing tests could be performed in order to check for consistency across covariates in each block. This additional step checks to make sure that households with similar characteristics are matched together. Otherwise it might be argued that households with similar propensity scores might have the same score (since all of the controls are distilled into one number) for systematically different reasons and therefore present an invalid counterfactual. Without significant balancing issues, it seems as though the matching creates a valid counterfactual.

4.3 General impact of treatment on child health outcomes

Table 4 contains the results of the DD and Kernel PSM regressions. The results are generally consistent across both approaches. The results for other PSM specifications are consistent as well and are available upon request. There seems to be somewhere between a nine percentage point and thirteen percentage point decrease in the number of respondents reporting that their child has been ill in the last month. Unfortunately, there is no corresponding decrease in malarial rates. Malaria can be recurrent so there might be residual effects of previous illnesses before the cash transfer even if the household has taken precautionary measures. There is a slight decrease in the number of children ill for longer than a month, but it is only statistically significant at the 10% level and only for the DD specification. If a child is sick for over a month, that might indicate more severe health problems that could require better supply side institutions to fix.

Treatment households are more likely to access medical care of any form. The results are significant at the 1% level across specifications. The DD method calculated a 6.4 percentage point decrease in respondents not seeking care for a child when sick and the Kernel PSM approach calculated a 9.2 percentage point decrease in the same variable. According to the Kernel PSM approach this decrease might be due to better being able to afford preventative care, but the DD results are inconclusive.

There is also a statistically significant decrease in children reporting that they have to stop regular activities because of illness, as well as a decrease in the number of days they stopped regular activities for when sick. Missing fewer days to sickness also decreases the number of days missed of school. There is also a highly statistically significant decrease in the likelihood that other members of the household have to stop their regular activities to care for a sick family member. Both regression specifications calculated effects of similar magnitude with roughly 22 percentage point decreases.

The next section is the anthropometric results. These measures would provide the strongest indicator of improvements in health and nutrition because they are definitively measured and therefore not susceptible to respondent error. All of these variables are measured by z-scores created using a WHO ideal growth reference population (see data section for details). Unfortunately, it seems as though none of these variables have any statistically significant changes. This is perhaps not unexpected given the sample under consideration. In particular, height represents accumulated investment in nutrition and health. Skeletal growth is most variable for those under five years old. Therefore, a sample containing older children is unlikely to manifest any change. Ideally, there would be a large sample of very young children. Unfortunately, this paper has to rely on a larger sample that includes older children.[13]

There might be several reasons for these results. First of all, it might be that the time period of one year is too short. This might be especially true because other papers (Covarrubias et al. 2012) have shown that these households invested heavily in their productive output. The final round of surveying was before the year's harvest and therefore the households have not reaped the returns of their productive investments yet. The new crops harvested might improve their dietary diversity and caloric intake significantly. The additional investment in their land plots might also have required extra household work because of barriers to outside hiring. This extra work might have drained extra calories from the household. Secondly, the household might simply not have invested heavily enough in the health and nutrition of the children to produce statistically significant results. Without conditions and monitoring, the adults who received the transfer might have elected to spend more of it on their own well-being. Finally, there might be supply side constraints on the quality of food and nutrition. While the household might be able to purchase larger quantities of food, they might not have access to food with higher nutritional quality. Maize is overwhelmingly the crop and food of choice in Malawi. Therefore it might be harder to improve the nutrition and health of children.

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[13] The height and weight variables were not only tested by using constructed z-scores in order to ensure that the lack of results was not due to a specific methodology. I also ran DD regressions on weight, height, and BMI without relying on z-scores, as well as creating measurements of thinness and severe thinness based on WHO standards and running the regressions on these variables. The results were similarly not statistically significant.

Overall it seems as though there are some positive results of the cash transfer. There is a sharp reduction in the number of children sick in the past month, a large reduction in the number of children who do not receive care, a reduction in the number of children forced to stop regular activities, and a major drop in the number of respondents reporting that others had to stop regular activities to care for children.

4.4 Linear combination and DD framework for gender specific results

This section seeks to answer whether different genders have systematically different preferences for expenditure on children. Unlike many other cash transfers, the Malawi SCTS was not targeted at a specific gender. Around 65% of the households containing children are female headed. It is possible to see if female-headed households have systematically different preferences by using linear combination tests on the impact of the cash transfer. The exact DD specification I use is as follows:

$$ch_{it} = \alpha_0 + \partial_1 treatment_i + \partial_2 femhhh_i + \partial_2 femhhh_i * treatment_i + \beta_1 post_t + \beta_2 post_t * treatment_i$$
$$+ \beta_3 post_t * femhhh_i + \beta_4 post_t * femhhh_i * treatment_i + \alpha_1 X_i + \alpha_2 Z_{it} + \varepsilon_{it}$$

where femhhh is a dummy variable with one indicating that the household head is female and 0 indicating the household head is male. The linear combination used was (β_4 - β_3)-(β_2 - β_1). (β_4 - β_3) captures the difference between treated (β_4) and untreated (β_3) female headed households post treatment. (β_2 - β_1) captures the difference between treated (β_2) and untreated (β_1) male headed households post treatment. The impact for male-headed households (β_2 - β_1) is then subtracted from the impact for female-headed households (β_4 - β_3) in order to determine which gender of household heads had the largest impact on the health of children in the household. Finally, tests are run to determine if the difference in impacts is statistically significant.

4.5 Results for impact differences between male and female headed households

The results of these linear combination tests are presented in Table 5. The only statistically significant result was the fact that male headed households had a larger reduction in malarial rates than female-headed households. The lack of clear results is an interesting finding. Most cash transfers in South America are targeted towards females because they are assumed to have systematically different preferences and they are assumed to prefer investing in their children. In this case, it seems as though female-headed households are not specifically investing more in their children.

There might be several reasons for this lack of a difference. First of all, the transfers are being given to female headed households rather than females within a household. Females as part of a larger household might be seen as primarily responsible for domestic tasks in more traditional societies. Therefore, the role that they have might shape their expenditure preferences. If they are seen to be primarily responsible for domestic tasks then this perceived societal responsibility might shape their expenditure preferences. These female headed households have to handle the full range of household responsibilities, including production, and therefore might have preferences shaped by these more general responsibilities.

This result highlights the cautious approach that Duflo (2000) encourages in regards to making assumptions about an individual's preferences solely based on gender. She highlights the fact that care must be taken when generalizing her results that cash transfers to grandmothers improved the anthropometric measures of their grand-daughters. The cultural context plays a large role in shaping these preferences.

4.6 Lee Bounds

This paper also uses Lee Bounds tests in order to further ensure the robustness of the results. It is used for treatment evaluation problems where problems such as non-response, sample attrition, or other structural problems might skew the results. It helps the researcher make sure his or her results are valid by creating upper and lower bounds on the possible treatment effect. As long as the treatment effect is still statistically significant for the lower bound then the results remain valid even in the worst case scenario.

This technique was developed by Lee (2009) for analyzing the impact of the Job Corps program. He wanted to assess whether the program improved the wage rate of participants. Unfortunately, the issue of sample selection arises. The same issue can arise with the cash transfer. The health benefits are only observed for children who remain in the household and the cash transfer incentivizes households to keep children by increasing benefits for each child. The transfer might also decrease the risk of attrition through child death or household dissolution from the household head dying. Those who are induced to stay are the 'marginal' group while those children who would have been in the household anyways are the ‘infra-marginal’ group. By examining the selection probabilities of the control and treatment groups one can determine the possible size of the marginal group (*n*). Assuming a worst case scenario that all members with the highest health benefits are members of the marginal group (*n*) an upper bound on the child health effect is created. By assuming that all members with the smallest health benefits are members of *n*, a lower bound on the child health effect is created.[14] As long as the upper bound on the effects of the intervention in the Malawi SCTS is still statistically significant then the results should be accurate regardless of any attrition or non-response effects. It must be remembered though that this bound represents a ‘worst case’ scenario. Additionally, there are inferential reasons to believe that the attrition might actually cause the sample to understate the impact of the cash transfer. The households most likely to dissolve, the children most likely to die, and the households least likely to retain children are all likely to be associated with poor health.

4.7 Results of Lee Bounds Testing

Two different specifications for the Lee Bounds test were used. First, the tests were done with weights generated from propensity scores. Secondly, the Lee Bounds tests were performed without any weights. The results are broadly similar across both specifications and are found in Table 6 and Table 7.

[14] The Lee Bounds test relies on only two assumptions. The first assumption is independence or random assignment. This assumption is fulfilled by the Malawi SCTS. The second assumption is monotonicity which means that the treatment should only affect sample selection in one direction.

The only variable that retains its significance even under the ‘worst case’ scenario is the reduction in children who did not seek treatment because they lacked money. The Lee Bounds test does undermine the robustness of the results presented earlier, but there are several important factors to keep in mind. First of all, there is supposed to be random assignment for Lee Bounds tests. The Malawi Social Cash Transfer was imperfectly randomized and therefore this will introduce additional ‘noise’ to the Lee Bounds results. For Lee Bounds testing, it is not possible to use controls. These controls might eliminate some of the noise, especially since multiple econometric techniques and specifications were used. Secondly, it must be remembered that the bounds generated represent best and worst case scenarios and even under the worst case scenario several other of the results were close to achieving statistical significance.

Finally, there are inferential reasons to believe that the worst case scenario is unlikely. As previously noted, the cash transfer incentivizes households to keep children and improvements to health might make other forms of attrition more likely. On the other hand, children in the counterfactual who are lost due to attrition might be more likely to have poor health. Poor health increases the likelihood of attrition due to death. Constraints on the resources of a household increase the likelihood of children leaving due to lack of resources (for example possibly moving into a relative’s household). These hypotheses need to be empirically examined, but they do provide prima facie evidence that the worst case scenario is unlikely.

5. Conclusion

As cash transfers become more and more prevalent, it is increasingly important to understand exactly how they work. This paper tries to answer two main questions. First of all, I examine whether the increased purchasing power of the cash transfer can improve the health outcomes for children even when there are weak supply side institutions. Secondly, I check the hypothesis that women have systematically different preferences than men for investment in the human capital of their children. I use several different econometric techniques (Difference-in-difference, propensity score matching, and Lee Bounds) in order to ensure the robustness of my results. By answering these two questions, I am trying to help provide additional evidence about how to best design cash transfer programs and the conditions under which they are most effective.

It is important for policy makers to know if cash transfers can be effective even in the absence of supply side institutions as cash transfer become increasingly prevalent. This paper shows that cash transfers can be effective even in the absence of adequate supply side institutions. The cash transfer reduced the frequency of illnesses significantly and reduced the impact of sicknesses on the households. Additionally, it removed barriers to accessing care. On the other hand, the lack of anthropometric results hints that other approaches should be taken in combination with cash transfers. Luckily, many programs already do this by distributing fortified products along with the cash transfer. Oportunidades is one prime example. The program provides nutritional supplements to infants between six and twenty three months of age as well as to undernourished children between twenty-four and fifty-nine months old. Van de Gaer et al. (2012) and Gertler

(2004) both found significant positive impacts of the Oportunidades program on anthropometric measurements.

Additionally, this paper shows that care must be taken when deciding who receives the transfer. In Latin America, most of the programs are targeted towards females. However, in the Malawi SCTS, female-headed households produced similar results to male-headed households. There are costs associated with targeting the transfers exclusively towards females; it means some male families are excluded and it might reinforce traditional gender standards that view the female as being in charge of the domestic sphere. The female-headed households in Malawi have to manage more general household responsibilities including production and do not seem to have systematically different preferences than the male headed households. This result provides evidence that the culture and societal conditions must be taken into consideration rather than assuming that females automatically have different expenditure preferences centered on traditional domestic roles.

Even though these results hold for multiple specifications that these issues are far from resolved. The results pertain to certain cultural (gender norms) and societal conditions (supply side institutions and governance). A fruitful area for future research would be to examine how strong of a role culture plays in determining gender preferences. The Lee Bounds results also show that the effect of cash transfers on household stability should be examined. With so many people living in poverty worldwide, it is critical to continue to rigorously assess poverty alleviation programs in order to determine the most effective approaches.

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Tables

TABLE 1: Baseline Characteristics

		Overall sample			*Propensity score adjusted sample*		
	Overall	**Control**	**Treat**	**Test (p-val)**	**Control**	**Treat**	**Test (p-val)**
Eligibility Criteria							
0-1 Meals per Day (0/1)	0.527	0.508	0.548	0.271	.0.568	0.546	0.645
HH Member Begs (0/1)	0.382	0.329	0.438	**0.002**	0.453	0.435	0.704
Monthly Exp. (Kw/capita)	192.434	193.304	191.513	0.938	155.412	191.913	0.135
0-1 Assets (0/1)	0.503	0.523	0.482	0.261	0.510	0.485	0.572
Dep. Ratio Over 3 (0/1)	0.393	0.326	0.463	**0.000**	0.470	0.462	0.850
Orphans (number)	0.551	0.383	0.729	**0.000**	0.605	0.702	0.409
Household characteristics							
Head education (years)	1.587	1.205	1.992	**0.000**	2.032	1.953	0.740
Over 60 (0/1)	0.597	0.650	0.540	**0.002**	0.537	0.538	0.984
Household size (number)	4.091	3.541	4.671	**0.000**	4.643	4.593	0.742
Children (number)	2.463	1.979	2.975	**0.000**	2.941	2.928	0.933
Members aged 0-5 (number)	0.425	0.376	0.477	**0.062**	0.461	0.476	0.808
Members aged 5-10 (number)	0.800	0.630	0.981	**0.000**	0.928	0.964	0.637
Members aged 11-15 (number)	0.951	0.759	1.153	**0.000**	1.160	1.133	0.794
Members aged 15-59 (number)	1.136	0.933	1.351	**0.000**	1.340	1.320	0.855
Members aged 60+ (number)	0.779	0.845	0.710	**0.005**	0.691	0.699	0.890
Natural Shock (0/1)	0.609	0.573	0.647	**0.038**	0.602	0.649	0.680
N	751	386	365				
Child Characteristics							
Age (months)	114.342	113.105	115.286	0.398	115.45	115.64	0.939
Male (0/1)	0.506	0.530	0.489	**0.093**	.49285	.52941	0.119
Orphan (0/1)	0.219	0.185	0.245	**0.005**	.22635	.22911	0.893
N	1732	750	982				

TABLE 2-Pre-Treatment Dependent Variables

	Before Treatment			After Treatment		
	Control	Intervention	Ttest (p-value)	Control	Intervention	Ttest (p-value)
Child Health						
Sick in the Past Month (0/1)	0.669	0.650	(0.420)	0.541	0.422	(0.000)
Malaria in Last Month (0/1)	0.095	0.094	(0.945)	0.069	0.061	(0.490)
Ill over a Month (0/1)	0.082	0.075	(0.637)	0.039	0.021	(0.022)
Sought Treatment (0/1)	0.051	0.101	(0.000)	0.105	0.042	(0.000)
No Treatment b/c Money (0/1)	0.010	0.039	(0.000)	0.044	0.000	(0.000)
Stopped Reg. Activ. b/c Illness (0/1)	0.498	0.457	(0.106)	0.386	0.281	(0.000)
# of Days Stopped (Days)	5.012	5.204	(0.583)	5.292	4.681	(0.133)
Others Stopped Reg. Activ. To Provide Care (0/1)	0.430	0.462	(0.310)	0.628	0.523	(0.011)
# of Days Stopped (Days)	5.346	5.638	(0.561)	5.265	4.946	(0.578)
Weight in Kg	25.653	25.670	(0.977)	28.268	28.687	(0.503)
Height in Cm	122.720	122.252	(0.713)	126.588	128.104	(0.194)
N	750	982		750	982	

TABLE 3-Pre-Treatment Child Variables by HH Head Gender

	Control		Intervention	
	Male HH Head	Female HH Head	Male HH Head	Female HH Head
Sick in the Past Month (0/1)	0.589	0.704	0.626	0.668
Malaria in Last Month (0/1)	0.113	0.087	0.102	0.09
Ill over a Month (0/1)	0.053	0.094	0.058	0.086
Didn’t Seek Treatment (0/1)	0.043	0.055	0.1	0.102
No Treatment b/c Money (0/1)	0.005	0.013	0.021	0.049
Stopped Reg. Activ. b/c Illness (0/1)	0.44	0.523	0.457	0.459
# of Days Stopped (Days)	4.133	5.328	5.231	5.202
Others Stopped Reg. Activ. To Provide Care (0/1)	0.478	0.412	0.511	0.436
# of Days Stopped (Days)	3.897	5.992	5.435	5.784
Weight in Kg	25.685	25.639	25.95	25.518
Height in Cm	121.861	123.084	122.375	122.159
N	230	520	363	613

TABLE 4- General Results for Impact on Child Health

	DD	Kernel PSM
Sick in the Past Month (0/1)	-0.092**	-0.134***
	(0.043)	(0.034)
Malaria in Last Month (0/1)	-0.018	-0.030
	(0.020)	(0.023)
Ill over a Month (0/1)	-0.027*	-0.028
	(0.014)	(0.019)
Did not Seek Treatment (0/1)	-0.064***	-0.092***
	(0.015)	(0.027)
No Treatment b/c Money (0/1)	0.028	-0.081***
	(0.020)	(0.016)
Stopped Reg. Activ. b/c Illness (0/1)	-0.064	-0.131***
	(0.041)	(0.037)
# of Days Stopped (Days)	-0.710**	-1.025***
	(0.291)	(0.313)
Others Stopped Reg. Activ. To Provide Care (0/1)	-0.131***	-0.152***
	(0.03)	(0.032)
# of Days Stopped (Days)	-0.716***	-0.988***
	(0.254)	(0.306)
Weight for age (z-score)	-0.065	-0.038
	(0.052)	(0.044)
Height for age (z-score)	0.002	-0.001
	(0.028)	(0.027)
BMI for age (z-score)	-0.014	0.001
	(0.027)	(0.021)
N= 1732		

* =p<0.1   ** =p<0.05   ***= p<0.01

TABLE 5- Differences in Impacts between Female and Male Headed Households

	DD
Sick in the Past Month (0/1)	0.070
	(0.055)
Malaria in Last Month (0/1)	0.071**
	(0.029)
Ill over a Month (0/1)	0.006
	(0.023)
Did not Seek Treatment (0/1)	-0.023
	(0.029)
No Treatment b/c Money (0/1)	0.009
	(0.032)
Stopped Reg. Activ. b/c Illness (0/1)	-0.001
	(0.053)
# of Days Stopped (Days)	-0.174
	(0.426)
Others Stopped Reg. Activ. To Provide Care (0/1)	.014
	(0.046)
# of Days Stopped (Days)	-0.088
	(0.367)
Weight for age (z-score)	-0.022
	(0.213)
Height for age (z-score)	-0.265
	(0.169)
BMI for age (z-score)	0.046
	(0.118)

N= 1732

* =p<0.1 ** =p<0.05 ***= p<0.01

TABLE 6- Lee Bounds Results with Weighting

	Lower Bound	Upper Bound
Sick in the Past Month (0/1)	-0.184***	-0.049
Malaria in the Last Month (0/1)	-0.082**	0.035
Ill over a Month	-0.071***	0.015
Did not Seek Treatment	-0.170***	-0.019
No Treatment b/c Money (0/1)	-0.098***	-0.037***
Stopped Reg. Activ. b/c Illness (0/1)	-0.176***	-0.019
# of Days Stopped (Days)	-1.616***	0.134
Others Stopped Reg. Activ. To Provide Care (0/1)	-0.200***	-0.043
# of Days Stopped (Days)	-1.616***	-0.028
Weight for Age (Z-Score)	-0.017	0.000
Height for Age (Z-Score)	-0.002	0.032
BMI for Age (Z-Score)	-0.041	0.023
N=	1747	

* =p<0.1 ** =p<0.05 ***= p<0.01

TABLE 7- Lee Bounds Results without Weighting

	Lower Bound	Upper Bound
Sick in the Past Month (0/1)	-0.174***	-0.046
Malaria in the Last Month (0/1)	-0.064***	0.044**
Ill over a Month	-0.068***	0.020
Did not Seek Treatment	-0.168***	-0.039
No Treatment b/c Money (0/1)	-0.089***	-0.031***
Stopped Reg. Activ. b/c Illness (0/1)	-0.143***	0.006
# of Days Stopped (Days)	-1.553***	0.203
Others Stopped Reg. Activ. To Provide Care (0/1)	-0.171***	-0.022
# of Days Stopped (Days)	-1.467***	0.104
Weight for Age (Z-Score)	-0.105	-0.029
Height for Age (Z-Score)	-0.048	0.041
BMI for Age (Z-Score)\	-0.052*	0.028
N=	1747	

* =p<0.1 ** =p<0.05 ***= p<0.01

Annex 1: Probit Results for Propensity Score Estimation

	Child
0-1 Meals per Day (0/1)	0.128
	(1.87)
HH Member Begs (0/1)	0.0825
	(0.61)
Monthly Exp. (Kw/capita)	0.000285
	(1.53)
0-1 Assets (0/1)	-0.198**
	(-2.74)
Dep. Ratio Over 3 (0/1)	-0.340**
	(-2.61)
Orphans (number)	0.210***
	(4.53)
Head education (years)	0.0442**
	(2.75)
Head over 60 (0/1)	0.476*
	(2.55)
Household Size (number)	0.239
	(1.57)
Children (number)	-0.0567
	(-0.64)
Members aged 0-5 (number)	-0.142
	(-0.95)
Members aged 5-10 (number)	0.175
	(1.14)
Members aged 11-15 (number)	0.00325
	(0.02)
Members aged 15-59 (number)	0.0893
	(0.64)
Natural Shock (0/1)	0.254**
	(2.97)
Head over 60*Members aged 60+	-0.402*
	(-2.14)
Log household size (number)	-0.568
	(-1.16)
Log dependency ratio	0.508***
	(3.44)

Log of age (months)	-0.00113
	(-0.01)
Over 65 yrs. Old*Education	0.0139
	(0.38)
HH begs*Natural Shock	0.132
	(0.83)
Age (months)	0.000128
	(0.07)
Male (0/1)	-0.0865
	(-1.29)
Orphan (0/1)	-0.414*
	(-2.54)
Constant	-1.079
	(-1.87)
N	1573

t statistics in parentheses
* =p<0.05   ** =p<0.01
***= p<0.001