Effect of a Conditional Cash Transfer Program on Early Childhood Nutrition: the Experience of Urban Mexico

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Abstract

Conditional cash transfers programs are becoming increasingly popular as policy tools to bring desired changes in education and health outcomes both in developing and developed countries. While their effects on academic achievement, school enrollment and health status have been studied extensively, their impact on chilhood nutrition is not well understood. Also, it is not clear whether the problem of child nutrition is intrinsically economic or it is more related to cultural and/or educational factors. In this thesis, I estimate the effect of a Conditional Cash Transfer program (Oportunidades) on early childhood (aged 0 to 47 months) nutrition, using a panel data, with two waves: 2002 and 2004, on representative households from urban Mexico. Contrary to the conventional wisdom, we find no statistical association between program participation and improvement in early childhood nutrition. One possible explanation might lie in the initial nutrition knowledge of the beneficiaries, although the program may induce to a greater calorie consumption, it does not follow a better diet. This result might be useful for the future design of programs featuring a cash transfer with similar target populations.

1 Introduction

Millions of people are beneficiaries of conditional cash transfers programs around the world. In Latin America, many countries have already some experience with this kind of programs or are in the process of developing programs of this type (Handa and Davis, 2006). One of the primary objectives of any Conditional Cash Transfer (CCT) program is to improve children's human capital. According to the U.S. National Library of Medicine, malnutrition is a significant health concern all over the world, especially among children. Nutritional losses in the first two years of life has been shown to be an important determinant of school performance and cognitive achievement, which are very important factors in later earnings and social mobility (Alderman et al., 2001; Glewwe et al., 2001). As far as the causes of malnutrition, poverty is one of the most widely documented factors. Therefore, CCT programs are particularly important for people living in poverty, where improved quantity and quality of food, are needed urgently.

According to Hoddinott and Skoufias (2004), an increase in the income of poor people does not necessarily result in a proportionate change in caloric acquisition. That is, CCT does not automatically reduce hunger. Furthermore, the goal should not be to reduce hunger but to improve the nutritional status. Malnourishment is not only related to the quantity of food, but also with quality. Dietary quality, i.e. food intake high in proteins, fruits and vegetables, is equally important. Poor people's diets can often fulfil or even exceed energy requirements; but, the food they consume might be nutritionally deficient (Tanumihardjo et al, 2007). The effect of CCT programs on household food consumption has been widely studied (Hoddinott and Skoufias, 2004; Maluccio and Flores, 2005; Attanasio and Mesnard, 2006; Angelucci and Attanasio, 2009, Leroy et al., 2010). These studies documented significant effect on beneficiary households' food consumption. However, a mere increased in food consumption does not necessary translate into improvements in childhood nutrition. Leroy et al. (2010) find that a CCT in rural zones leads to excess energy consumption. Thus, the focus on CCT's impact on quantity of food consumed alone is misleading.

The magnitude of the effects of CCT on nutritional status is not well-known. The number of empirical studies is rather small (Barber and Gertler, 2008; Behrman and Hoddinott, 2005; Leroy et al., 2008; Maluccio and Flores, 2005), and some of the evidence presented is inconclusive. Also, some of the studies overlook the link between poverty and obesity focusing separately on height and weight increments; both increments seeing as positive outcomes. Furthermore, those studies do not measure the nutrition status in a comprehensive way, and some of them have serious methodological and/or data flows. For instance, Barber and Gertler (2008) focused on birthweight, and used data from rural communities. Behrman and Hoddinott (2005) also used data from rural communities and they only used one dimension measurement as an outcome variable of child nutrition (child height). There are very strong reasons to believe that urban households might behave differently in terms of consumption than rural households. For instance, the availability of junk food in urban areas is greater than in rural ones, so the relative prices for healthy food and junk food might be different. For example, Attanasio and Mesnard (2006) found that the effect of the CCT on household food consumption in Colombia is quite different in rural and urban zones. However, Attanasio and Mesnard (2006) do not provide children anthropomorphic measures, and they have some issues with the estimation: since the program was not randomly assigned, it would probably be better to implement the evaluation in a similar fashion as Angelucci and Attanasio (2009).

Leroy et al. (2008) is an important effort to evaluate the urban Oportunidades program. However, it suffers from methodological and data issues. As extensively discussed in Angelucci and Attanasio (2009), in the case of urban Oportunidades, the Average Treatment Effect (ATE) is not defined under the assumption of heterogeneous effects, very likely the case. This will be discussed in more detail bellow. Since Leroy et al. (2008) ignored that the program was not random; they tried to estimate the ATE. Furthermore, their sample consists of only 432 observations. However, it is feasible to increase the sample almost by a factor of four (the z-scores have to be calculated manually). The study by Maluccio and Flores (2005) is very similar to the present study. They do not only study the improvement on the household's diet but also analyze whether if the diet improvement results in the nutrition status (measured in a comprehensive way) of children aged less than five years. They found positive results in underweight and stunning. However, the CCT that they study (RPS in Nicaragua) was random assigned and only implemented in rural areas.

This thesis contributes to this literature, but differs significantly in terms of its focus and methodology. We focus our attention in an urban CCT, Oportunidades. However, the urban expansion of Oportunidades was not random. Therefore, a more appropriate technique to estimate the treatment effect is the one used in Angelucci and Attanasio (2009). Unlike the present study, Angelucci and Attanasio (2009) estimated the effect of urban Oportunidades on household consumption. In this thesis, we explore two hypotheses about how beneficiaries respond to an increase in income. In the standard utility maximization model, consumers face prices and maximize their utility subject to a budget constraint. An increase in the family budget of people who live in poverty may lead to increase their food consumption. One possible option is that those families allocate resources to satisfy just certain level of calorie ingestion. Alternatively, beneficiaries may allocate resources to increase healthy food consumption. The latter may happen if healthy nutrition plays a role in their utility function.

The public policy implication is very clear: if society has more families that maximize their utility based just on calorie ingestion instead of a healthy diet, adverse outcomes will likely occur when the budget of these families is increased. From a public health perspective, several questions arise: Does the program have a positive impact on childhood nutrition? Do the beneficiaries spend the transfer money in a "smart" way?

The purpose of this thesis is to examine the effect of urban CCT in children's nutrition status under the context of non-randomization. The expansion of Oportunidades in urban areas of Mexico is the particular case chosen for study. Section 2 of this thesis provides a utility maximization model with welfare determination under an expansion of budget constraint. In Section 3, the program, the data used and the empirical approach is explained. The theory predicts that if the head of households have enough information and nutrition status is part of their utility function, beneficiaries are likely to buy more food, especially healthy ones. However, in Section 4, I find strong empirical evidence that the program does not has a nutritional effect. In particular, the kids of the beneficiaries of the program do not boost their nutrition status. Why the kids that are beneficiaries do not quite improve their nutrition status? One possible explanation is that beneficiary households do not have enough information about which food is nutritionally good for their child. Finally, in Section 5, I offer some concluding remarks. This study contributes to the empirical literature on CCT in two ways. First, this is the only urban study that measures the nutrition status in a comprehensive way, taking into account the link between poverty and obesity. Secondly, I deal with some data related problems that have been overlooked by some previous studies.

2 Conceptual Framework

2.1 A model of utility maximization with human capital

This section outlines a simple utility maximization model with human capital, especially child nutrition (Behrman and Deolalikar, 1988; and Becker, 1981). While it is impossible to capture the full process of child nutrition, this model serves to clarify the concepts of decision making, it embodies household information about nutrition, and also identifies the situations in which cash and in-kind transfers will induce different household consumption behavior.

The model presented here is adapted from Currie (2000), Smith et al. (2003), and Smith and Haddad (2000). In these papers the household maximizes utility:

$$U_i = U_i \{NF_{jt}, L_{jt}, N_{jt}\} \tag{1}$$

$\forall i \epsilon (1, 2, ..., n)$, and $\forall j \epsilon (1, 2, ..., m_i)$, where n are the number of households, m_i the number of individuals in each household, U_i is household utility, NF consumption of non-food and non-health items, L leisure, and N nutritional status. Utility maximization is inter-temporal, but the same decision is taken at each period so the time subscript can be dropped.

Assume households maximize utility over all these goods. These goods are strictly increasing and concave in all arguments, and are maximized with respect to the budget constraint $p_{NF}q_{NF} + p_L q_L + p_N q_N \leq Y$, where p_{NF}, p_L, p_N are the prices for non-food consumption, leisure, and nutrition status.

The household maximizes utility subject to the total income constraint and nutrition production functions. Nutritional status is viewed as a household provisioning process with inputs of food and non-food. The nutrition provisioning function for child i is as follows:

$$N_i^{ch} = N\left\{\left[N_{t-1}^{ch}, F_{jt}, HC_{jt}, TIME_{jt}, INF_i, ...\right]\right\} \tag{2}$$

where N_{t-1} is nutritional status in the previous period by the same child, F is food consumption, HC is the amount spent on health-care for individual j, $TIME$ is the time household members devote to the healthcare for that individual, and INF is the amount and quality of information that each household has about nutrition. A great assumption in our study is that utility preferences are constant over the period of study. Only the platicas component of Oportunidades can increase the amount of INF in beneficiaries households. It is clear that a lack of knowledge about nutrition plays a crucial role.

Suppose that a household utility function is represented in Figure 1, the horizontal axis is the quantity of nutrition consumed and the vertical axis represents the non-nutrition consumption. Also assume that the preferences of this household remain the same. The line AB represents the budget constraint (Figure 1). Now consider two scenarios: a cash transfer versus a in-kind transfer. If a cash transfer of amount T occurs, the budget constraint shifts upwards to CD, corresponding to $p_{NF}q_{NF} + p_L q_L + p_N q_N \leq Y + T$, while an in-kind transfer (the dietary supplement) leads to a kinked budget constraint.

As expected, the household shown is weakly worse off under the in-kind transfer[1] because it is forced to consume at III' (the kink) if the supplement cannot be sell, and at III" if the

[1]

supplement can be sell but its price is lower than the market price. The best scenario is III"', the situation under a cash transfer of the amount of the market value of the supplement.

One common paternalistic fear is that households may spend some portion of the cash transfer on "sin items", such as alcohol and tobacco; rather than spent this money on healthy food. This situation may suggest that a paternalistic approach is preferable, as it is good for the household (in terms of nutrition) to consume all the supplement. If the program consists of in-kind transfers, households are more likely to consume a higher amount of nutritious food (at least part of the supplement).

However, on the other hand, with an in-kind transfer households can respond by adjusting the expenditure on healthy food. The existence of the in-kind intervention may induce parents to expend less on food such as fruit and vegetables. Furthermore, these households might not have the information necessary to reach an acceptable level of nutrition, due to the majority of these households being poorly educated. What is important for the empirical portion of our study is that we have a testable hypothesis: controling for other external income shocks, urban Oportunidades improve the nutrition status of beneficiaries' children?

Oportunidades is a CCT that combines an in-kind transfer, a cash transfer, and a supply of nutritional information. This afford us the opportunity to observe the impact of a CCT that combines an in-kind and a cash transfer and affects, nutrition information, one of the most important variables in the nutrition provisioning function.

As established, the primary purpose of this paper is to estimate the impact of Oportunidades on the change of nutrition status in beneficiaries' children. Since urban Oportunidades is not a random program, a main problem is that simple mean comparisons between beneficiaries and non-beneficiaries are not credible estimates of the impact of the program. This is because most of the non-beneficiaries come from not treatment areas (blocks that have less poverty), and

weakly cause it can be the case that the in-kind transfer is the same that what the household would decide under utility maximization therefore there are valid reasons to believe that there exists unobserved differences between beneficiaries and non-beneficiaries.

2.2 Empirical Framework

According to the causal model of Rubin (Holland, 1986)., for each household i in the sample, let D_i indicate whether the treatment of interest was received, with $D_i = 1$ if household i received the treatment of interest, and $D_i = 0$ if household i did not received the treatment. This treatment is not randomly assigned but rather households have to apply for it at time $t = t_0$, and we observe outcomes at time $t = t_1$. Let $Y_{it_1}(1)$ be the potential outcome (nutrition status) of household i at time t_1 when $D_i = 1$, $Y_{it_1}(0)$ the potential outcome when household i did not receive the treatment. Finally, let $\Delta Y_i(0) = Y_{it_1}(0) - Y_{it_0}(0)$ denote the change in outcome (change in nutrition) for household i under control, and $\Delta Y_i(0) = Y_{it_1}(0) - Y_{it_0}(0)$ the change in nutrition status under treatment. In our case of study, Oportunidades in urban zones, individuals either belong to a treatment zone ($Z = 1$) or control zone ($Z = 0$), these zones are not randomly assigned.

For unit i the treatment effect is the difference in change in nutrition with and without the treatment. Then, the parameter of interest can be described as the Average Treatment Effect (ATE) in the population, $ATE = E[\Delta Y_i(1) - \Delta Y_i(0)] = \tau$, i.e., what the change in nutrition would be if the kid receives Oportunidades, and what the change in nutrition would be if the kid is not a beneficiary of the program. The difficulty in estimating the average treatment effect is that treatment levels are mutually exclusive, e.g. we only observe either $\Delta Y_i(0)$ or $\Delta Y_i(1)$; but never both. Then we can only compare individuals that receive the treatment with individuals that do not receive the treatment. However, if the treatment is not randomly assigned, and specially if beneficiaries have to apply for the treatment, the individuals who apply for the treatment and get it may be quite different from the individuals who do not receive the treatment; and these differences might be related with the outcome. This is the so call self-selection problem.

One of the most common assumptions useful to avoid the self-selection problem, is the Unconfoundedness assumption (Rubin, 1978; Rosenbaum and Rubin, 1983). This assumption establishes that in observational studies where treatment D is not randomized, an identifying assumption for the ATE is that the treatment assignation is independent of the potential outcomes of interest conditional on some covariates, i.e.

$$D_i \perp (Y_i(1), Y_i(0)) \mid X_i \tag{3}$$

In simple words, this assumption implies that for subpopulations with $X = x$ we essentially have a randomized experiment. Therefore, we can compare treated and control subgroups that share $X = x$. However, in Oportunidades, no matter which variables we use to control for, D_i is determined endogenously. This is due to the presence of unobserved confounders. In urban Oportunidades, being enrolled in the program not only depends on characteristics of the individuals of the household, but also on whether the household is in a treatment or control zone. That is, D_i is determined in part by the zone in which the household lives. In this case D_i is correlated with the disturbances, this problem is commonly called "endogeneity of treatment status", and it is the fundamental problem we need to solve. Under the presence of endogeneity of treatment status, i.e. when the treatment is confounded due to unobservables, we can use an instrument. Such instrument must have two main characteristics: it should be relevant, and it must not have a direct effect on the outcome.

This concept is what Imbens and Angrist (1994) introduced as local average treatment effect or LATE. It basically means that using an Instrumental Variable (IV) one can identify a causal effect under the potential outcomes approach but just for the treatment group. Our instrument in this case is the treatment zone($Z = 0, 1$). Relevance of the instrument means that the $E[D_i|Z = 1] > 0$, that is, the instrument can actually induce some individuals to participate. This is obviously the case in our study. Also the instrument has no direct effect on the outcome, but only an indirect effect through the treatment. This is not that clear in urban Oportunidades since the area of residence may have some direct effect on the nutrition status of the children, e.g. some areas may have different information about nutrition and this would definitely influence directly the nutrition status.

In the very well-known papers of Angrist (1998), Angrist and Evans (1998), and Angrist and Krueger (1994), the instrument is randomly assigned. However, in our case, treatment zones are not randomly assigned, even more, since the treatment zones are selected based on block poverty scores, it is perfectly reasonable to suppose that $Cov\left(Z,D\right)\neq 0$ and maybe also $Cov\left(Z,\Delta Y\right)\neq 0$. Thus, the instrument may be confounded with the outcome. Therefore, Z may only be a proper instrumental variable after conditioning on some covariates, this covariates might include household and block characteristics. So the Unconfoundedness assumption in this case is

$$Z_i \perp \left(\Delta Y_i\left(1\right),\Delta Y_i\left(0\right),D_i\left(1\right),D_i\left(0\right)\right) \mid \gamma_i \tag{4}$$

where $D\left(1\right)$ is the treatment status if $Z=1$, and $D\left(0\right)$ is the treatment status if $Z=0$. Unconfoundedness of LATE is plausible given that we have all the variables used to determine treatment and control zones. Also, if 10 holds, we can use a version of the propensity score presented first by Rosenbaum and Rubin (1983); so instead of conditioning 10 on γ_i, we will conditioning on the estimated propensity score $\hat{p}_i\left(\gamma\right)$. Then, for LATE to be identified there are other assumptions to be satisfied: monotonicity, exclusion restriction, and common support.

Monotonicity requires that any beneficiary who would get the program if not zone eligible, would also get the program under zone eligible. In our setting, monotonicity seems a very reasonable assumption, we expect that the households that find their ways to receive Oportunidades would apply and get the program if those households now belong to the treatment zone. The exclusion restriction means that there is no direct effect of the instrument on the outcome. Without conditioning on γ_i, this assumption may be invalid. However, because we have all the variables that determines our instrument and plenty of variables that influence household participation we have reasons to believe that this assumption is satisfied. The last condition, common support, is probably the most controversial in our study. Since the zones were selected based on poverty scores, it is hard to believe that this condition would be easily satistified. Indeed, Figure 3 shows us that even though this condition is not violated (we do not have propensity score equal one or zero for any individual, and the proponsity score is continuous), it might be problematic [2] (See Angrist (1990) and Angrist, Imbens, and Rubin (1996) for discussions of potential violations, and Angelucci and Attanasio (2009) for a more detailed discusion of the common support of urban Oportunidades).

As discused above, we can only identify the average effect on the subpopulation called "compliers" (Angrist, Imbens and Rubin, 1996). "Compliers" are the households who were in a treatment area and receive Oportunidades. These individuals comply with assignment no matter what their eligibility. That is, if they are assigned to the control group, they would not participate, and if they are assigned to the treatment, they do participate. Therefore, we want to estimate the LATE $\tau_{LATE} = E\left[\Delta Y\left(1\right) - \Delta Y\left(0\right) \mid D\left(1\right) = 1\right]$.

Under this conditions, the LATE estimator [3]is simply the Wald or the IV estimator when Z and D are both binary[4]:

$$\tau_{LATE} = \frac{E[\Delta Y|Z=1,p(\gamma)]-E[\Delta Y|Z=0,p(\gamma)]}{P(D=1|Z=1,p(\gamma))-P(D=1|Z=0,p(\gamma))}$$

Then, following Frolich (2007), define the conditional mean functions $m_z(x) = E[Y|X = x; Z = z]$ and $\mu_z(x) = E[D|X = x; Z = z]$, and instead of $Z = z$ we can replace it by the estimated propensity score $\hat{p}\left(\gamma\right)$. Lets call $\hat{m}_z(x)$ and $\hat{\mu}_z(x)$ the estimated conditional means, estimated by a linear regression and a logit regression respectively.

$$\hat{\tau}_{LATE} = \frac{\sum_{Z_i=1}(Y_i-\hat{m}_0(X_i))-\sum_{Z_i=0}(Y_i-\hat{m}_1(X_i))}{\sum_{Z_i=1}(D_i-\hat{\mu}_0(X_i))-\sum_{Z_i=0}(D_i-\hat{\mu}_1(X_i))}$$

[2]However, the results of taking subsamples of the samples with propensity scores between .9 and .1, .8 and .2 were consistent with the results using the all sample.

[3]Please see Appendix

[4]For the standard errors estimation, we will be using the methodology and software proposed by Frolich (2007)

3 Context and Data

Oportunidades is a nationwide CCT program in Mexico. The goal of the program is to assist in overcoming poverty by providing support in matters of education, health, and nutrition in order to promote the permanent development of the basic capacities of men, women, and children who live in extreme poverty (Coordinación Nacional de Programa de Desarrollo Humano Oportunidades, 2005). Beneficiaries of the program must follow a set of requirements in order to receive a Cash Transfer and a nutrition supplement (in case of a household has a child less than five years old). These requirements are school age children must be enrolled in school, one parent needs to attend health and nutrition seminars, and all family members must go to a health clinic for check-ups.

Oportunidades started in 1997 in highly marginalized rural zones as PROGRESA (Programa de Educación, Salud y Alimentación), in 2000 PROGRESA changed its name to Oportunidades. Since 2002 Oportunidades has began operations in urban areas with populations between 50,000 to 1 million of people, as part of its expansion program. In some settings a randomized experiment would have been feasible, due to political and social considerations this is not the case. The program coordinators established priority implementation zones based on poverty scores (those which include more than 500 eligible households based on the 2000 national census). These zones started the program in 2002, the remaining zones will incorporate later as budget restrictions permit. It is important to note that this program is voluntary; i.e. each household decided whether or not to apply to the program.

In the 2002 incorporation zones, the program and its requisites were advertised on various media (subjectively decided by state level coordinators). The steps for households to incorporate to the program are as follows: 1) physical visit to the incorporation module: for each household that voluntarily applied for the program, its "eligibility potential" was evaluated using a "poverty score" based on socioeconomic characteristics, 2) domiciliary visit: the information of those households which passed the first evaluation was corroborated, and a new questionnaire was applied that contains more detailed information, 3) final notification: to receive a final decision, every household was required to return to the module. Each household selected for the program had to pass through these three evaluation steps. The cash amount varies according to the number and grade level of school age children, and it is subject to the satisfactory progress of the previously mentioned conditions. All modules finished operations by August 2002. It is worth noting that the households did not receive any cash transfer until they went to the health clinic and proved that their children were in fact enrolled in school.

In order to evaluate the program's effectiveness, the Federal Government collected a panel data survey (Encuesta de Evaluación de los Hogares Urbanos, ENCELURB) for 2002, 2003, and 2004. The population was a mix sample of intervention (2002 incorporation) and no intervention (2004 incorporation) zones. The sample consisted of 5,647 households participating in the program (treatment). Surprisingly, there were some households in non intervention zones that actually were enrolled in the program[5], Table 1 illustrates this. Also, since the rate of participation was lower than expected, and in order to assure enough representation, a "sweep zone"[6] had to be added. This zone consisted of neighborhoods near by some treatment zones. There are 11,550 control households; 5,461 living in control zones and the rest are from treatment zones.

Different types of questions were asked in the surveys: socioeconomic characteristics, biology, anthropomorphic measures, and food intake frequency. In this study, our population target is children 0-47 months old, whom as explained before are a very susceptible group to malnourishment. For those children, we have socioeconomic household characteristics from 2002, and anthropomorphic measures from 2002 and 2004 (taken by the survey applicant). For the purpose of this study, we are also using block variables from the 2000 census.

[5]Probably it was because those households provide an wrong address that was part of the intervention zone in order to get the benefits

[6]Hereinafter, we will consider this zone as part of the treatment zone

3.1 Data

After matching the different parts of the survey, the 2002 data contains 4,896 children with 47 months old or less. Out of those, 2,072 are treatment and 2,824 are control children. When these data are matched with 2004, the data consist of 658 treatment and 795 control children. We have no reasons to believe that there is a systematic attrition[7].

Variable definitions are in the Appendix[8]. We control for observable variables both at the individual level and at the block level. Block level variables are intended to measure the level of poverty. Tables 3 and 4 present a short description of the individual level variables used. For each covariate, we calculated the t-statistic for the difference in mean between intervention and comparison group. The majority of the variables differ significantly between groups. Therefore the groups are different in some very important variables such as age of the child, age of the household head, food expenditure, and all of the poverty proxy variables.

3.1.1 Measuring Early Childhood Nutrition

We are proposing three different ways of measuring childhood nutrition: height-for-age, weight-for-height, and the Shukla's Nutrition Index (SNI) (Shukla et al, 1972). The first two are the classic standards established by the World Health Organization (WHO), the SNI is specially used by nutritionist and it has been proved to measure malnutrition especially in developed countries (quote africa study).

Height-for-age reflects linear growth: a short height is correlated with a lack of physical development. As a result of a poor nutrition over time, and especially during the first years of live, there is a permanent deficit in growth. Therefore this measure is useful to detect chronic malnourishment; however it cannot detect a severe malnutrition. According with the WHO, a kid

[7]See Angelucci and Attanasio (2009) for more on this.
[8] For an extended analysis of variable construction, please consult the Appendix

is said to be in deficit of physical develop if his/her height-for age z-score (HAZ) is two or more standard deviations (sd) below the median of the reference tables provided by the WHO. The z-score for a height H_i for the i individual at age t was computed as:

$$HAZ_i = \frac{H_i - Median_t}{sd_t} \tag{5}$$

where sd_t represents the standard deviation at age t. Both median and standard deviation were obtained from the WHO standard tables.

Another indicator is weight-for-height. This index is useful to diagnosis severely malnourished children, however it cannot detect chronic malnutrition. According with the WHO, a kid is said to be wasted if his/her weight-for-height z-score (WHZ) is more than two standard deviations below the median of the reference tables provided by the WHO. Similarly to HAZ, the z-score for a height W_i for the i individual with height j was computed as:

$$WHZ_i = \frac{W_i - Median_j}{sd_j} \tag{6}$$

The Shukla's Nutrition Index (SNI) measures how deteriorated is the relationship of weight and height for a given age. The virtue of this index is that it is clearly able to account for under or over nutrition at every specific age. One is the optimal measure. If the child is overweight $SNI_i > 1$, and the ideals weight and height changes within every month and are obtained for standard tables published by the WHO. Severe obesity and severe starvation occurs at $SNI_i \geqq 1.2$ and $SNI_i \leqq 0.8$. A drawback for this measure is that it is sensitive to the height, so small children might result with overweight and big children with underweight. Nevertheless, this problem is reduced in early childhood where the differences in stature are not that considerable. This index was calculated as follow:

$$SNI_i = \frac{Weight_i / Height_i}{Ideal\,Weight_i / Ideal\,Height_i} \tag{7}$$

Formulas 3, 4, and 5 were calculated for each child in both waves (i.e., 2002 and 2004). With these results it is possible to address in a comprehensive way the impact of the CCT to the nutrition status. Table 2 compares the three different measures used in this study in terms of under and overnutrition. It is worth noting that in 2002 only 6 kids were under/over nourished for all the measures, while in 2004 only 4 kids share this condition. This is an indicator that HAZ, WHZ and SNI clearly measures different things.

3.2 Outcome variable

Our outcome variable should capture the change in nourishment between 2002 and 2004. HAZ and WHZ are centered at zero, i.e., the optimal score for both measures is zero. So, an improvement in nutrition status would happen if $abs\,(HAZ_{i2002}) > abs\,(HAZ_{i2004})$, a decrease in nutrition status is the opposite case, and this is analogously for WHZ. The SNI is centered at one, an improvement would mean that the deviation from 1 is closer in 2004 compare with 2002, and a decrease in the nutrition status means that the deviation from 1 is greater in 2004 compare with 2002.

Unfortunately, and since the nature of the study, in each methodology proposed some information is lost; that implies that we would not be able to establish whether the overall program effect (improvement or deterioration of the aggregate nutrition status) is due to an overnutrition or undernutrition. We would be able to know just the absolute value of the WHZ, HAZ and SNI improved (or unimproved).

We are proposing the following variables that measure the change in nutrition status:

$$YHAZ_i = abs\,(HAZ_{i,2002}) - abs\,(HAZ_{i,2004}) \tag{8}$$

$$YWHZ_i = abs\,(WHZ_{i,2002}) - abs\,(WHZ_{i,2004}) \tag{9}$$

$$YSNI_i = abs\left(SNI_{i,2002} - 1\right) - abs\left(SNI_{i,2004} - 1\right) \tag{10}$$

Formulas 6, 7 and 8 assign equally weight to each proportionate change, no matter the starting point.

3.3 Control variables

As explained in Section 2, we need to control for individual characteristics, as well as for block characteristics. The household level variable definitions are listed in Table 3, in Table 4 we can find the definitions of the block level variables. In Table 5 and 6 the descriptive statistics shows some very important differences between treatment and control group. This is not surprising because we were expecting that those groups would be very different due to the mechanism used to select them.

We use the same set of block level variables that the program coordinators used to define the control and treatment areas. All the individual level variables are from 2002, except work status in 2004. Including both 2002 work status and 2004 work status variables makes identification of the treatment effect more credible, since we are controling for changes in income due to a new job. In sum, we conditioned on all variables that may strongly influence both participation and outcome. We think that by conditioning on these variables the Unconfounded assumption is well justified.

4 Results

As explained in Section 3.2, if urban Oportunidades actually improves nutrition status we would expect a positive coefficients in each one of the three outcome variables after controlling for a large number of individual, family, and community characteristics. Table 7 shows the descriptive statistics for the outcome variables. To show the sensitivity of the results we estimates the LATE for different in-between brackets of propensity scores and the results were consistent with the ones shown in Table 8. As we can see, in all three cases we find that the estimates are not statistical different from zero. That is, the effect of the CCT urban Oportunidades has not a great impact in nutrition improvement in the population of study.

It is important to interpret carefully this result. We are presented evidence that for the compliers population that participated in urban Oportunidades, their children between 0-47 months olds had not improvement in nutrition status due to the program. These findings are very different in magnitude than results shown in a study of Leroy et al. (2008), however, it is important to note that they actually tried to estimate the ATE and not the LATE. So, it is very interesting that when the more appealing econometric approach is used the effect of the CCT actually change driving to a different conclusion.

5 Policy Implications

Although the average effect for the full sample of urban Oportunidades is not identified, we were able to identify for a particular subpopulation what the average effect is. We claim that the effect of urban Oportunidades in nutrition status of beneficiary children that live in treatment areas and that had participated for 2 years is not statistically meaningful. Neither of the channels (platicas, nutritional complement or cash transfer) was efficient enough to make a statistical improvement on those children, although they are likely to be important in the long run.

In spite of the statistical null effect seen, one may consider, however, that the program was evaluated only 2 years after it started. One reason that urban Oportunidades does not has the intended nutrition outcome in the short-run is perhaps the fact that nutrition status is also highly related with cultural/educational factors. Therefore, probably the platicas effect may take a little longer to persuade household and social norms about food intake and activity. Further studies might analyze the impact of the nutritional talks on the behavior of the beneficiaries after the first year of implementation of the program.

A second potential explanation is that Oportunidades increase the financial capability of beneficiaries to afford more fat and sugar because of their increased income. However, since we found neither an improvement or a deterioration of the nutrition status in the intervention areas, this suggests that the educational component and the in-kind transfer of the program could be counterbalancing the income effect. This may suggests that a better design of these kind of policies might require a more paternalistic approach.

In summary we encountered a very interesting and unique CCT. However, the evaluation that we proposed has many limitations. Since the program is not randomly designed and given also that we have control and treatment areas, our scope has to be limited only to the beneficiaries that reside on treatment areas. Another major limitation of the analysis is that we have 3 effects confounded: the cash transfer, the nutritional talks, and the in-kind transfer. Therefore, we can not identify each effect separately. However, LATE for the program is perfectly identified for the population of study; also, the nonparametric approach provide us a more flexible approach in the function specification. This study make it clear why any public policy has to be rigorously evaluated econometrically speaking.

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Tables and Figures

Table 1: Participation in Oportunidades by Survey Zones

Type of Household	Control Zone	Treatment Zone	Sweep Zone	All Zones
Treatment	177	4,574	896	5,647
Control	5,461	5,937	152	11,550
Total	5,638	10,511	1,048	17,197

Table 2: Comparison across different measures

	Intervention group			Comparison group		
Under/Overnutrition	HAZ	WHZ	Shukla	HAZ	WHZ	Shukla
2002	184	42	71	201	52	59
2004	162	34	31	184	50	33

Table 3: Individual level variables definition

Group	
Variable name	Detailed definition
Age02	Age of the child in months in the 2002 wave
Weight02	Weight of the child in kilograms in the 2002 wave
Height02	Height of the child in centimeters in the 2002 wave
Age04	Age of the child in months in the 2004 wave
Weight04	Weight of the child in kilograms in the 2004 wave
Height04	Height of the child in centimeters in the 2004 wave
Sex	Sex of the Child
Hsize	Number of members per household
HHage	Household head age
Foodexp	Food expenditure in pesos per week
kfloor	Dummy variable, describes if the household has firm floor
toilet	Dummy variable, describes if the household has a toilet facility
sexhh	Dummy variable, describes if the head of household is a men
educhh	Dummy variable, describes if the head household has at least middle school
wshh02	Dummy variable, describes if the head of household has a remunerated work in 2002
wshh04	Dummy variable, describes if the head of household has a remunerated work in 2004
media	Dummy variable, describes if the household has a television or a radio.

Table 4: Block level variables definition

Group	
Variable name	Detailed definition
bao	Proportion of households without a bathroom
piso_tie	Proportion of households with dirt floor
luz_elec	Proportion of households with electricity
escj	Head of household mean of years of education
lee_esc	Proportion of households without literacy
percpod	Proportion of poor households
h_pobre	Number of poor households
h_cpobre	Number of almost poor households

Table 5: Descriptive Statistics (continuous variables)

Group	Intervention group		Comparison group		
Characteristic	mean	sd	mean	sd	t-stat
Age02	12.59	7.35	15.58	7.42	7.68
Weight02	8.79	2.18	9.58	2.11	6.99
Height02	71.64	8.60	74.36	8.01	6.22
Age04	35.41	7.03	36.07	7.13	1.78
Weight04	13.45	4.00	13.41	2.13	-0.20
Height04	91.63	35.98	90.87	6.11	-0.58
Hsize	5.83	2.65	5.96	2.41	0.96
HHage	34.5	11.39	35.57	12.78	1.67
Foodexp	375.68	339.18	405.90	272.94	1.88
bao	0.17	0.17	0.10	0.12	-9.48
piso_tie	0.41	0.23	0.21	0.19	-18.09
luz_elec	0.11	0.19	0.04	0.11	-8.87
escj	5.02	0.99	5.70	1.31	11.02
lee_esc	0.18	0.08	0.12	0.08	-14.53
percpod	0.00	0.01	0.00	0.01	2.95
h_pobre	67.26	28.32	27.15	23.22	-29.66
h_cpobre	25.48	12.45	14.40	12.58	-16.77
Sample size	658		795		

Table 6: Descriptive Statistics (discrete variables)

Group	Intervention	Control
Characteristic	sum	sum
Sex	658	795
kfloor	358	236
toilet	378	566
sexhh	560	698
educhh	387	445
wshh02	526	673
wshh04	16	47
media	588	728

Table 7: Outcome Variables

	Intervention group			Comparison group		
Variable	mean	min	max	mean	min	max
YHAZ	-0.02	-4.24	4.79	0.11	-4	3.46
YWHZ	0.02	-2.49	3.63	0	-2.47	3.48
YSNI	0.02	-0.37	0.42	0.02	-0.44	0.41

Table 8: Results

	Intervention group		
Variable	Estimate	standard error	p value
YHAZ	0.053	0.151	0.726
YWHZ	0.055	0.113	0.629
YSNI	0.029	0.047	0.542

Figure 1: Cash and in-kind Transfer

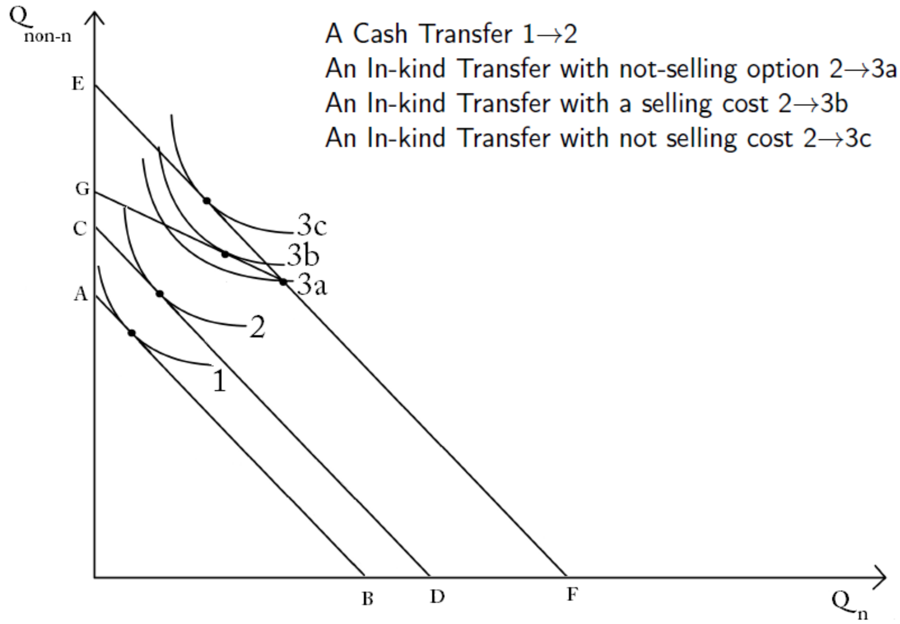


Figure 2: Distribution of Propensity Score by Group

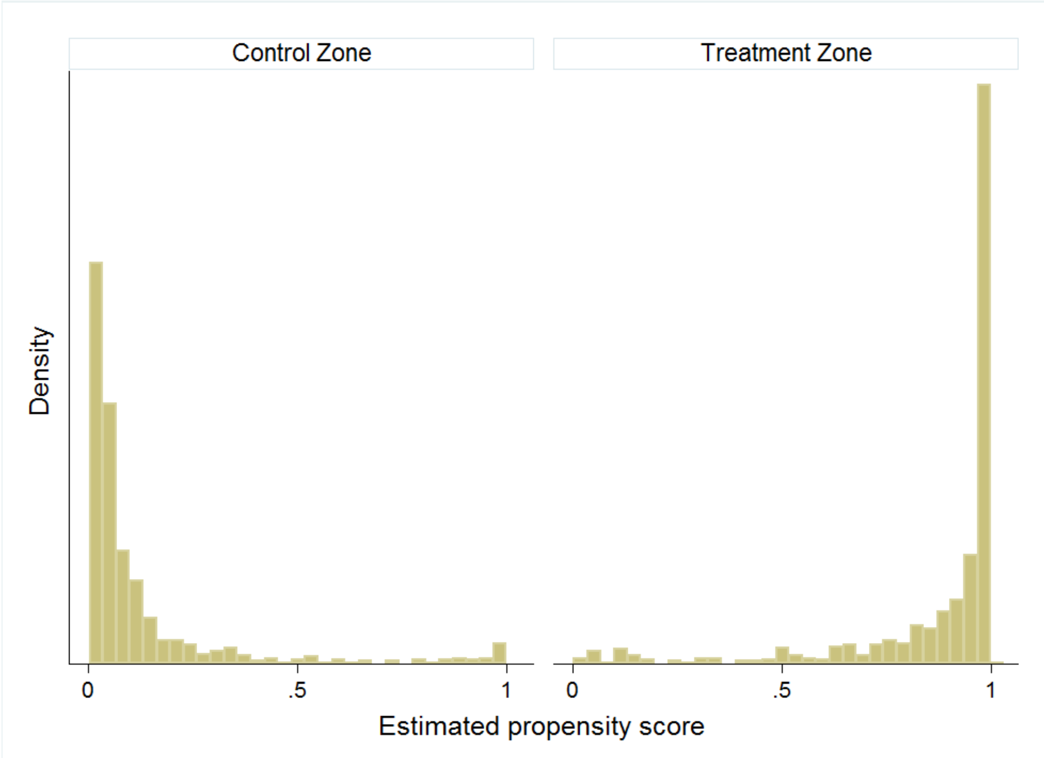
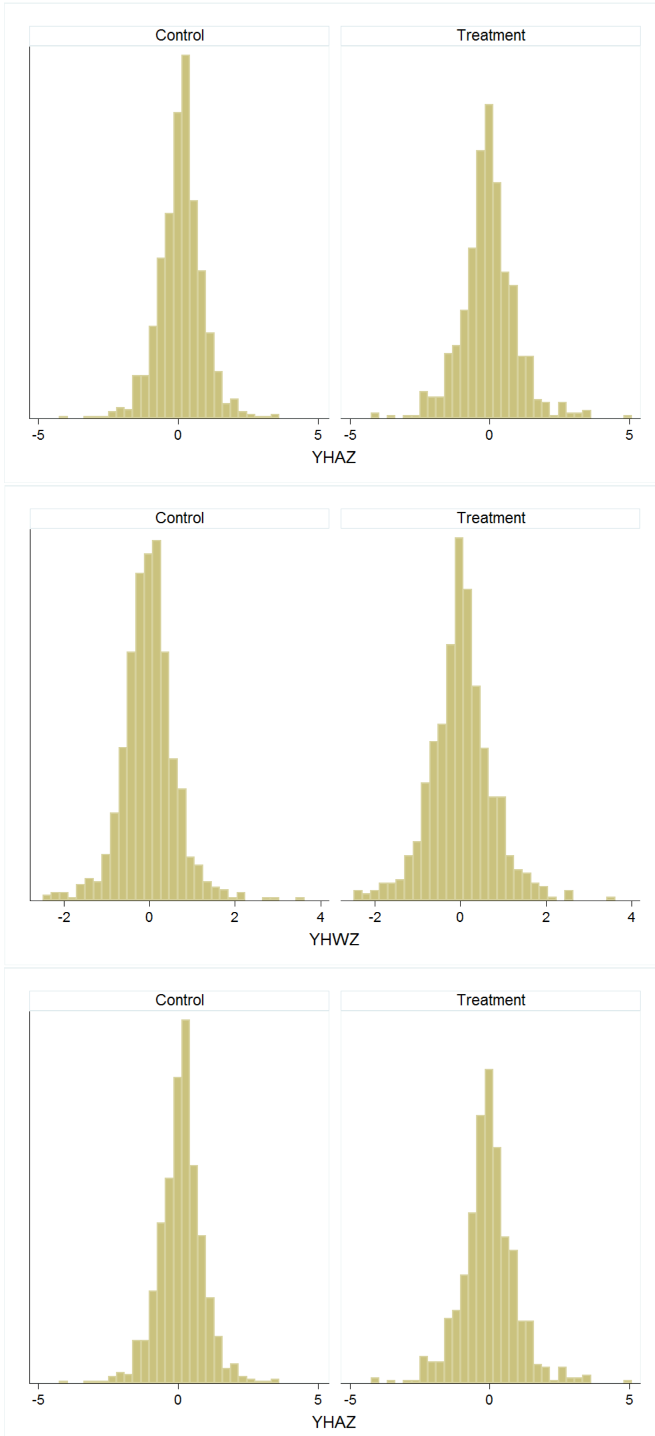


Figure 3: Distribution of Outcome Variable by group



7 Appendix

Detailed derivation of the LATE equation, for simplicity we will assume that equation 10 holds without conditioning:

We can define $\Delta Y = (1-D)\,\Delta Y(0) + D\Delta Y(1)$; also we can define $D = (1-Z)\,D(0) + ZD(1)$, and combining this two equations we have

$\Delta Y = (1 - [(1-Z)\,D(0) + ZD(1)])\,\Delta Y(0) + [(1-Z)\,D(0) + ZD(1)]\,\Delta Y(1)$

$= \Delta Y(0) - D(0)\,\Delta Y(0) - ZD(0)\,\Delta Y(0) + ZD(1)\,\Delta Y(0) + D(0)\,\Delta Y(1) - ZD(0)\,\Delta Y(1) + ZD(1)\,\Delta Y(1)$

agrouping terms$\Delta Y = \Delta Y(0) + D(0)\,(\Delta Y(1) - \Delta Y(0)) + Z\,[D(1) - D(0)]\,[\Delta Y(1) - \Delta Y(0)]$

then, if we take the conditional expectation:

$E[\Delta Y|Z] = E[\Delta Y(0)] + E[D(0)\,(\Delta Y(1) - \Delta Y(0))] + ZE\{[D(1) - D(0)]\,[\Delta Y(1) - \Delta Y(0)]\}$

$E[\Delta Y|Z=1] - E[\Delta Y|Z=0] = E\{[D(1) - D(0)]\,[\Delta Y(1) - \Delta Y(0)]\}$

$= 1 \cdot E[\Delta Y(1) - \Delta Y(0)\,|D(1) - D(0) = 1]\,P(D(1) - D(0) = 1) +$

$0 \cdot E[\Delta Y(1) - \Delta Y(0)\,|D(1) - D(0) = 0]\,P(D(1) - D(0) = 0)$

$-1 \cdot E[\Delta Y(1) - \Delta Y(0)\,|D(1) - D(0) = -1]\,P(D(1) - D(0) = -1)$

$= E[\Delta Y(1) - \Delta Y(0)\,|D(1) - D(0) = 1]\,P(D(1) - D(0) = 1)$

since there are not defiers $P(D(1) - D(0) = -1) = 0$

Also because of not defiers $P(D(1) - D(0) = 1) = E(D(1) - D(0)) = E(D(1)) - E(D(0))$

and also we can represent $E(D|Z) = (1-Z)\,E(D(0)) - ZE(D(1))$, because the othorgonality between Z and D taking the difference

$E(D|Z=1) - E(D|Z=0) = E(D(1)) - E(D(0)) = P[D=1|Z=1] - P[D=1|Z=0]$ this is $\neq 0$ by relevance assumption

then, rearranging the terms, we have shown that

$\tau_{LATE} = E(\Delta Y(1) - \Delta Y(0) | D(1) - D(0) = 1) = \frac{E[\Delta Y|Z=1] - E[\Delta Y|Z=0]}{P(D=1|Z=1) - P(D=1|Z=0)}$