Estimating Participation and Spill-Over Effects in Conditional Cash Transfer Programs.

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Abstract

This research intends to compare the impact of different conditional cash transfer programs on key outcome variables for human capital accumulation. In particular, we will investigate whether there is just an income effect of the transfer on the outcome variables or whether the other components which aim to change the beneficiary behavior do have an additional impact on those key outcome variables. We call the latter, the participation effect. This issue is related to the efficiency and efficacy of the imposition of conditionalities on beneficiaries and their follow-up as those components are due to be a considerable chunk of the administrative cost of the programs. Finally, we will also investigate whether the program has (spill-over) impacts on non-beneficiary families who live in the same neighborhood as beneficiary families.

A. Aims

Conditional Cash Transfer programs have the double objective of alleviating poverty in the short-run and of breaking the intergenerational transmission of poverty. For the latter, it assumes that the transfer itself and the conditionalities attach to it will be able to increase the families' investments in the human capital of their children. According to this reasoning, improvements in human capital will be triggered by the relaxation of the budget constraints of

the family (income effect) and by a change in behavior that the program requires for the family to keep receiving the transfer (participation effect).

The objective of this study is to use the information available in the baseline surveys of evaluation with an experimental design and/or in the administrative registry for the selection of beneficiaries as well as in the follow-up household surveys for impact evaluations to test whether there is a participation effect on the top of the expected "income effect" and whether the program can have any spill-over effect on non-beneficiaries living in the same neighborhood. The outcomes to be assessed are those related to the human capital objectives of the program: 1) school attendance; 2) child health care and 3) education, health and total expenditures.

This study will rely on the data from household surveys and/or administrative records from Paraguay (*Tekoporã*), Mexico (*Progresa*), Nicaragua (*Red de Protecctión Social*) and Brazil (*Bolsa Família*). This selection of program/countries encompasses not only different designs – that we will discuss in detail below – but different modalities of evaluation. All surveys were planned to estimate the impact of the program on key outcomes of interest and have been used to inform government and civil society on the impacts of the program. Our intention is to go beyond the estimation of the average treatment on the treated and exploit the different mechanisms that the program can have an impact in order to better understand how the impacts of the CCT programs are generated. We will focus on this these three mechanisms: 1) the direct impact of an increase in income, the so-called "income effect"; 2) the impact caused by changes in the behavior of the household – the "participation effect"; 3) the impact that the program may have on non-beneficiaries living in the same neighborhood of benecificiaries -who may have changed their behavior due to the existence of the program - which can be named externality or spill-over effect.

B. Background and Policy Relevance

Most well-known evaluation such as the ones undertaken for *Progresa* in Mexico and *Familias en Acción* in Colombia have shown a positive impact of the transfer on school attendance, nutritional status, immunization rates, anthropometric measures and food consumption (Hoddinot et al. 2000; Attanasio et al., 2004; Soares et al., 2007). These results tell us a success story in terms of the impact of the program on those human capital inputs. However, the black box of most ex-post impact evaluation does not disentangle what has been triggered by the transfer itself – relaxing the budget constraint of the families – and what has been caused by the activities that would stimulate families to change their behavior.

Ex-ante simulations of the impact of the programs show very low impacts of the income effect (Bourguignon et al., 2002), indicating that the program's other components are crucial to guarantee the positive impact observed in the ex-post impact evaluations. Moreover, if the participation in the program and its behavior change effects and/or requirements are really effective, the program may have a spill-over effect on non-beneficiary families whose neighbors take part into the program.

The issue of conditionalities and of complementary activities such as the family support, training, priority access to income generating program is quite controversial. Many researchers advocate that the transfer itself is enough to change the family's behavior or that conditionalities and complementary activities are not cost-effective (Standing, 2007).

There has been some interest from a public policy point-of-view to disentangle the income-effect from the other components of the program in order to assess the different channels through which CCT programs affect outcomes. This knowledge can offer important inputs to assess the cost-effectiveness of those programs. However, as far as we know there is no experimental evaluation that tries to compare the same program with and without

conditionalities on human capital inputs (school attendance and health check-ups) in order to disentangle the two effects.

Davis et al (2004) compares the impact of Progresa – Mexico's CCT program – and Procampo – another Mexican cash transfer program, but without human capital conditionality – in dimensions such as food consumption, school attendance and investment levels. They conclude that conditionalities do influence longer-term (human capital) and medium term (productive) investment decisions, and the receipt of multiple forms of treatment by beneficiaries can affect the overall impact of each individual program. This is an interesting evidence of the role of conditionalities; however, it is based on the comparison of two programs that are very different regarding other dimensions besides the conditionalities themselves. It would be quite important to have some evidence of the differential impact of the income-effect and of the participation-effect within the same design. This proposal aims to fill in this gap using the information available on the evaluations surveys to disentangle those effects and, in addition, look at the spill-over effects that are, in general, overlooked in the traditional impact evaluation of CCT programs.

Assessing the overall impact of the program, which means including any externality or spill-over effect of the program in the assessment of its impact, and disentangling the importance of the income effect vis-à-vis the participation effect is an important piece of evidence to inform policy-makers on the effectiveness of the CCT programs and on the strengths and failures of the specific design of the programs. It can also inform the debate on the relevance of conditionalities and on the importance of complementary programs. In light of this evidence, a possible future research line, perhaps profiting from an experimental design would be to assess multiple treatments to better inform the best mix of program components to effectively tackle the issues related to the accumulation of human capital in order to break the intergeneration cycle of poverty.

C. Background Methods

In order to evaluate the effectiveness of conditional cash transfers on several outcomes, Latin-American countries have undertaken several specific household surveys covering both beneficiary and non-beneficiary households. In this study we will focus on the household surveys for impact evaluation of CCT programs carried out in Paraguay, Brazil, and Mexico and Nicaragua. These programmes were chosen because their CCT programs are very different in terms of coverage, value of the transfer, enforcement of conditionalities and complementary activities[1]. Moreover, there have been evaluation surveys for all those programs.

The objective of breaking the intergeracional cycle of poverty by means of the improvement of human capital of beneficiary children and the targeting on poor households are common features of those programs.

The selection of beneficiary households in those four programs has not followed necessarily a randomized design; however, in the case of Mexico and Nicaragua there has been a randomization of the municipalities that would enter first the program. Similarly in the case of Tekoporã in Paraguay an index of prioritization based on poverty measures were used to rank districts. In Brazil, such an index was not calculated, but priority was given to regions with highest poverty rates, notably the Northeast region. Below we describe the selection process for each program. In all cases, even for the countries where there has been randomization of municipalities, we will use quasi-experimental methods to match the beneficiary sample with a comparable sample of households. This control sample could be obtained both from areas

[1] For instance, in the case of Paraguay there is a strong component of family support through the monthly visits of family guides, whereas in Brazil – the largest CCT in Latin America – there is nothing that resembles this component.

where the programs have not taken place yet and from the neighborhood of the beneficiary households. Given that the selection of beneficiaries or communities in all programs has been made through observed variables that are correlated with their socioeconomic condition, which can be estimated by the administrative registers and/or baseline surveys, the most suitable evaluation strategy involves methods of selection on observables, like Propensity Score Matching (PSM) and Propensity Score Weighting (PSW).

Paraguay, *Tekoporã*

Tekoporã has followed an approach of first using geographical targeting in order to rank districts according to their poverty level and unsatisfied basic needs. This targeting was based on a Geographical Prioritization Index (IPG), which was composed of both monetary and non-monetary indicators. Within selected districts, households are eligible for the program if they: 1) have children under 15 years of age or pregnant women; 2) live in the priority areas of the program, namely, the poorest districts in the country; and 3) have a low Index of Quality of Life (ICV). ICV is a non-monetary measure varying from 0 to 100 that synthesizes several quality of life dimensions, such as access to public services, health and education outcomes, occupation of the household head, housing conditions and household assets. The program provides the transfer to those households that are classified as extremely poor (having an ICV below 25) or moderately poor (having an ICV between 25 and 40).

In Paraguay, the *Tekoporã* program is characterized not only by the transfer of income conditioned to households' compliance with co-responsibilities, but also by a strong intervention of community agents (*guias familiares*) on them (Soares and Britto, 2007).

In July of 2005, *Tekoporã* started its pilot in five districts of seven that were previously selected and screened by a census. The control group can be draw from the two districts that were not selected to participate into the pilot (due to the restricted budget for the pilot phase)

as well as from eligible households in the treated districts that were not included into the program due to mistakes made in the implementation. This mistake was basically the overlook of some neighborhoods, and there is no evidence that it is correlated with any observable or unobservable variable that could raise concerns about skim-creaming from the side of the managers (Soares and Ribas, 2007).

The evaluation survey of the pilot took place between January and April of 2007 and its sample was designed to ensure that the following groups would be self-representative: extremely poor treated households; moderate poor treated households; extremely poor control households; moderate poor control households; control households which live at selected districts; and control households which live at districts not selected. The total number of households fielded is 1,102, which represents a population of 6,423 households. Within this sample, 319 households are treated, 435 households are not treated but live in the selected districts, and 348 do not live in a selected district.

The survey questionnaire has the following sections where the outcome of interest can be obtained: education (school attendance and schooling progress); health (child health care); employment (child labor); food consumption; and other expenditures. In addition, there are other sections which provide us a wealthy set of control variables: housing conditions; demographic characteristics; characteristics of participation in the program; social participation; and socioeconomic shocks.

Since the sample was drawn based on the screening census, we already have a data that can be used as the baseline information. This baseline is useful not only to make a consistent matching of households but also to take the initial condition of some outcome variables and to allow the estimates of differences-in-differences estimators using PSM or PSW.

Other additional data that may be used are the presence of health centers and schools in the communities within the districts, which allows us to control the supply of social services, and

the administrative register of payments, which let us to analyze both the duration and dosage effects of the program.

Brazil, *Bolsa Família*

Bolsa Família is the largest CCT program in the world, benefiting roughly 11 millions of both extreme and moderate poor families. The program started in 2003 with the merge of the existing conditional and unconditional cash transfer programs of the Federal Government. Specifically, it unified four major programs: *Bolsa Escola*, a minimum income grant related to primary education; *Fome Zero* and *Bolsa Alimentação*, two income grants related to food security; and *Vale Gás*, a subsidy to help poor households buy cooking gas.

Families are eligible to take part into the program with their per capita income are below R$ 120 (around US$ 60) – poor – and there are children younger that 15 years (or pregnant women) in the household or if their per capita income is below R$ 60, – extremely poor – regardless of the household composition.

The evaluation survey of *Bolsa Família* took place in October of 2005 and was conducted by the Center of Development and Regional Planning (Cedeplar) of the Federal University of Minas Gerais (UFMG), in cooperation with the researchers from the National School of Statistics (ENCE) of the Brazilian Institute of Geography and Statistics (IBGE). This survey fielded 14,022 households and around 30% of them were beneficiaries. Its representativeness allows inferences not only for the whole country but also for three macro-regions, namely South and Southeast, North and Central-West, and Northeast. However, the microdata of this survey has not been made public yet. It is expected to be made public by the first semester of 2008.

The surveys questionnaire resembles the well-known Living Standard Measurement Survey. Therefore, it provides us a very rich set of data on household conditions, especially related to

health, education and consumption. Nonetheless, there is no baseline information that could be used in this evaluation.

A manner to match beneficiary households with comparables households could be the using of Demographic Census of 2000. In that case, we will assume that the selection process of *Bolsa Família* was mainly based on the Brazilian Poverty Map. Indeed, Andrade, Chein and Ribas (2007) confirm such assumption, showing that local poverty headcount in 2000 is the main determinant in the probability of treatment, in relation to other household characteristics. Finally, another data that will be merged is the statistics of beneficiaries by municipality, available at MDS website.

Mexico, *Progresa*

The selection process of beneficiary families in *Progresa* presents three stages. The first stage consists in a geographical targeting. From the information provided by 1990 population census and the 1995 population count, a Basic Index of Marginalization is constructed to identify communities with high proportion of households in extreme poverty condition. To develop this index, it was applied the method of principal components based on share of illiteracy adults (older than 14 years) in the locality and six other variables of household conditions (like access to water and number of occupants for room). About 30% of the localities did not have data for all the seven variables, so it was applied regression techniques to estimate the marginality index in these cases. The index has five groups according to the degree of marginalization, and those with very high and high classifications were considered priorities. Nevertheless, features related to financial and logistical considerations and to the availability of services in the locality (health and education) were also taken into account in the selection process. In the end of this first stage, localities with less than 50 and more than 2500 inhabitants were excluded.

The second stage consists in the selection of beneficiaries. It was carried out a census of all the households residing in the selected localities (ENCASEH survey) to collect socio-economic information. The distinction between poor and non-poor starts with the construction of a per capita household income variable net from the children's income. That constructed income is compared with the cost of a basic food basket to define poor households. Finally, for each region, a discriminant analysis takes place to identify the variables that best distinguishes poor from non-poor. These variables are used to construct another index with a multidimensional perspective of poverty condition. This index is ranged between 0 and 100, with low values attached to poorer households.

The final stage consists in a "validation process" in which a list of beneficiary families (containing the name of the person who receives the benefits) is made public in community assemblies for review and discussion. In some cases, a new visit to the household may apply.

Due to the experimental design of the evaluation a subset of communities eligible to receive Progresa were randomly assigned to a treatment (320 communities) or control (186 communities) group.

Nicaragua*, Red de Protección Social*

In the case of the pilot of the *Red de Protección Social* there has been only geographical targeting. The municipalities within the two departments chosen to be part of the pilot were selected according to their capacity to implement the supply side of the RPS. However, the selection of the rural *comarcas* that would take part into the program was done through a marginality index whose components are known: family size, access to potable water, access to latrines and illiteracy rates.

The 59 *comarcas* were grouped into four priority levels, and only the 42 *comarcas* of the priority scores 1 and 2 were eligible for the pilot's phase's first stage. Almost all households

in the eligible *comarcas* were eligible for the food security transfer whereas only those with children ages 7-13 who had not yet completed the fourth grade of primary school were also eligible for the education component of the program. Half of the eligible *comarcas* were selected into the programme whereas the other half served as control group.

At the moment we have access to the microdata for Paraguay (whose impact evaluation was undertaken by ourselves), Mexico and Nicaragua datasets have been made available to the public by IFPRI. Only in the case of Brazil, we are still waiting for the Ministry of Social Development to release the microdata. Therefore, we run the risk of not getting one of the surveys.

Estimation Strategy

In order to measure the impact of the programs on an observable outcome, we need to estimate the difference between the outcome with the treatment (participating in the program) and the outcome without the treatment (not participating in the program) for the same household. When these differences are calculated for all treated households, we can obtain the Average Effect of the Treatment on the Treated (the *ATT* effect).

Formally, assume that $T_i = 1$ indicates that household *i* has received the program benefit and $T_i = 0$, otherwise. Similarly, $Y_i(T_i = 0)$ would indicate the outcome of interest for household *i* in the absence of treatment, whereas $Y_i(T_i = 1)$ would be the outcome for household *i* receiving the treatment. It is important to mention that Y_i can be either the level of outcome in

a cross-section setting or a difference in outcome, $Y_i = Y_{i1} - Y_{i0}$, in a difference-in-difference framework[2] (Abadie, 2005).

Then, the *ATT* effect can be defined as:

$$\tau = E\left[Y_i(T_i = 1) - Y_i(T_i = 0) \mid T_i = 1\right], \quad (1)$$

where $E[.]$ is the expectation function.

However, we cannot observe the same household in both states (with and without the treatment) at same time. Then, one may assume that the average outcome conditioned on observable characteristics, X_i, for the treated group, $T_i = 1$, in case that it had not received the treatment, would be the same as that of the untreated group, $T_i = 0$. This unconfoundedness assumption allows one to estimate the *ATT* effect comparing the treated group with the comparison group and implies that selection into the program is completely based on observable variables[3], X_i (Rubin, 1978; Rosenbaum and Rubin, 1983).

On the other hand, if there exists spill-over effect on the comparison group ($T_i = 0$), outcomes differences between the treatment and comparison groups will potentially underestimate the *ATT* effect. In this case, effects of the program may be doubly understated because spill-over effects to the comparison group may be neglected and then the benefits for the treated may be also underestimated. This identification problem is closely related to the issue of contamination in experimental studies, where programs have externality effects on

[2] In the empirical application whenever baseline information on outcomes of interest are available, we will use the difference-in-differences estimator. This is important, because whereas the cross-sectional estimators does not allow for unobserved determinants of participation, the difference-in-differences estimates allows them as long as it lies on separable individual and/or time specific components of the error term.

[3] If $Y_i = Y_{i1} - Y_{i0}$, the unconfoundedness assumption will imply that the treatment assignment is orthogonal only to the average differences in outcomes (pre- and post-treatment) conditioned on observables and not to the potential outcomes (pre- or post-treatment). This assumption implies that in the absence of the treatment, the average outcome of the treated would have experienced the same variation as the average outcome for the untreated.

population not participating in the program (Heckman et al., 1999; Miguel and Kremer, 2004).

Assessing Treatment and Spill-Over Effects

First, we should consider a distinction in the comparison group to assess the spill-over effect. Let $D_i = 1$ indicates that household *i* living in the area where the program took place, and $D_i = 0$ otherwise. Thus, $(D_i = 1, T_i = 0)$ indicates the within-community comparison group, while $D_i = 0$ indicates the between-community comparison group. For all treated households, D_i is certainly equal to one.

Without the distinction between comparison groups, one may actually assess the following confounded *ATT* effect:

$$\tau_c = E\left[\begin{array}{l} Y_i(D_i = 1, T_i = 1) \\ -P(D_i = 1)\cdot Y_i(D_i = 1, T_i = 0) \\ -P(D_i = 0)\cdot Y_i(D_i = 0, T_i = 0) \end{array}\middle| T_i = 1\right] \quad (2)$$

where $P(.)$ is a probability function. It just tells us how much the treated distinguish, on average, from the combination of counterfactual possibilities. Since it depends on the probability of being in other states, nothing can be concluded from this estimator in terms of program impact on the treated.

Indeed, we can define three kinds of program effect. The first one, called the Average Participation Effect on the Treated (*APT*), may be written as follows:

$$\tau_p = E\left[Y_i(D_i = 1, T_i = 1) - Y_i(D_i = 1, T_i = 0) \mid T_i = 1\right], \quad (3)$$

The second effect, called Average Externality Effect on the Treated (*AET*), may be defined as[4]:

$$\tau_e = E\left[Y_i\left(D_i = 1, T_i = 0\right) - Y_i\left(D_i = 0, T_i = 0\right) \mid T_i = 1\right]. \quad (4)$$

Then, we may rewrite the *ATT* effect (1) as the sum of both effects (3) and (4):

$$\tau = \tau_e + \tau_p = E\left[Y_i\left(D_i = 1, T_i = 1\right) - Y_i\left(D_i = 0, T_i = 0\right) \mid T_i = 1\right]. \quad (5)$$

The identification of these effects requires the following unconfoundedness assumption:

$$T_i \perp \left(Y_i\left(T_i = 0\right), Y_i\left(T_i = 1\right)\right) \mid X_i, D_i. \quad (6)$$

It means that, under an environment with or without externality effect, treatment assignment and the potential outcomes are independent conditional on the pre-treatment variables, X_i.

Similarly, we should also assume that, the average outcomes conditioned on X_i of both comparison groups will be the same if they do not receive the treatment and are exposed to the same externality environment. Formally, we assume that the distinction between comparison groups and the potential outcomes conditional on X_i are orthogonal:

$$D_i \perp \left(Y_i\left(D_i = 0\right), Y_i\left(D_i = 1\right)\right) \mid X_i, T_i = 0. \quad (7)$$

Since one actually only observes the following outcome for each observation:

$$Y_i = T_i \cdot Y_i\left(D_i = 1, T_i = 1\right) + \left(1 - T_i\right) \cdot D_i \cdot Y_i\left(D_i = 1, T_i = 0\right) + \left(1 - D_i\right) \cdot Y_i\left(D_i = 0, T_i = 0\right),$$

assumptions (6) and (7) yield the following estimators of the *APT*, *AET* and *ATT*, respectively:

$$\hat{\tau}_p = E\left[Y_i \mid X_i, D_i = 1, T_i = 1\right] - E\left[Y_i \mid X_i, D_i = 1, T_i = 0\right], \quad (8)$$

[4] The Average Spill-Over Effect of the Program on the Treated Community is similarly defined as $\tau_s = E\left[Y_i\left(D_i = 1, T_i = 0\right) - Y_i\left(D_i = 0, T_i = 0\right) \mid D_i = 1\right]$.

$$\hat{\tau}_e = E[Y_i \mid X_i, D_i = 1, T_i = 0] - E[Y_i \mid X_i, D_i = 0, T_i = 0], \text{ and} \tag{9}$$

$$\hat{\tau} = E[Y_i \mid X_i, D_i = 1, T_i = 1] - E[Y_i \mid X_i, D_i = 0, T_i = 0]. \tag{10}$$

One way to implement the estimators (8)-(10) is to estimate those conditional expectations non-parametrically and then integrate the estimates to the desired level of aggregation. However, this estimation is feasible only if we are handling a very large sample and/or a small set of covariates.

An alternative to estimate those effects is to approximate the conditional means by estimating the following linear functions (Rubin, 1977):

$$\begin{aligned} Y_i = \; & \alpha_0 + \tau_p \cdot T_i + \tau_e \cdot D_i + \alpha_1' X_i \\ & + \alpha_2' [X_i - E(X_i \mid T_i = 1)] \cdot T_i + \alpha_3' [X_i - E(X_i \mid T_i = 1)] \cdot D_i + \varepsilon_i, \end{aligned} \tag{11}$$

$$\tau = \tau_p + \tau_e. \tag{12}$$

When the dimension of X_i is large and some critical covariates are correlated with the residuals of the equations above, it may be difficult to estimate accurately those regressions functions.

The well-known solution to control for treatment selection on many observable characteristics is to reduce the set of covariates, X_i, to a scalar by means of a parametric estimation in the first step. Namely, we may estimate a Propensity Score, $p(X_i) = P[T_i = 1 \mid X_i]$, that represents the probability of the household i being treated conditional on X_i. Given the unconfoundedness assumption (6), treatment assignment and the potential outcomes will be independent conditional on $p(X_i)$ (Rosenbaum and Rubin, 1983).

The implementation of the propensity score requires, however, an additional assumption:

$$E[x_i \mid p(X_i), T_i = 1] = E[x_i \mid p(X_i), D_i, T_i = 0] \quad \forall x_i \in X_i \tag{13}$$

This assumption is called 'balancing property' and can be empirically verified. Yet in the case of distinct comparison groups, the balancing property is not as simple as the conventional. It is needed that the treated sample is balanced to the within-community comparison group, as well as to the between-community comparison group.

Moreover, assumption (7) requires that one estimates not only the probability of each unit sample being treated but also the probabilities of belonging to the between- and within-community comparison groups. It can be achieved estimating a multinomial or multivariate model of probability regression, where the chances of being in within-community comparison group, $e(X_i) = P[D_i = 1, T_i = 0 \mid X_i]$, are also calculated.

In the second step, adjusting for the propensity score removes the bias associated with differences in the observed covariates in the treated and comparison groups. One approach, derived from Horvitz and Thompson (1952) and Hirano et al. (2003), consists in weighting treated and comparison observations to make them representative of the population of interest—in our case, the treated group—, as follows:

$$E[Y_i(D_i = 1, T_i = 1) \mid T_i = 1] = E\left[p(X_i) \frac{Y_i \cdot T_i}{p(X_i)} \right] = E[Y_i \cdot T_i], \tag{14}$$

$$E[Y_i(D_i = 1, T_i = 0) \mid T_i = 1] = E\left[p(X_i) \frac{Y_i \cdot (1 - T_i) \cdot D_i}{e(X_i)} \right], \text{ and} \tag{15}$$

$$E[Y_i(D_i = 0, T_i = 0) \mid T_i = 1] = E\left[p(X_i) \frac{Y_i \cdot (1 - D_i)}{1 - p(X_i) - e(X_i)} \right]. \tag{16}$$

The estimators of the *APT*, *AET* and *ATT* effects are then given respectively by:

$$\hat{\tau}_p = \left[\sum_i (Y_i \cdot T_i) \Big/ \sum_i T_i \right] - \left[\sum_i \frac{\hat{p}(X_i) \cdot Y_i \cdot (1 - T_i) \cdot D_i}{\hat{e}(X_i)} \Big/ \sum_i \frac{\hat{p}(X_i) \cdot (1 - T_i) \cdot D_i}{\hat{e}(X_i)} \right], \tag{17}$$

$$\hat{\tau}_e = \left[\sum_i \frac{\hat{p}(X_i)\cdot Y_i\cdot(1-T_i)\cdot D_i}{\hat{e}(X_i)} \Big/ \sum_i \frac{\hat{p}(X_i)\cdot(1-T_i)\cdot D_i}{\hat{e}(X_i)}\right] - \left[\sum_i \frac{\hat{p}(X_i)\cdot Y_i\cdot(1-D_i)}{1-\hat{p}(X_i)-\hat{e}(X_i)} \Big/ \frac{\hat{p}(X_i)\cdot(1-D_i)}{1-\hat{p}(X_i)-\hat{e}(X_i)}\right], \text{ and} \quad (18)$$

$$\hat{\tau} = \left[\sum_i (Y_i\cdot T_i) \Big/ \sum_i T_i\right] - \left[\sum_i \frac{\hat{p}(X_i)\cdot Y_i\cdot(1-D_i)}{1-\hat{p}(X_i)-\hat{e}(X_i)} \Big/ \sum_i \frac{\hat{p}(X_i)\cdot(1-D_i)}{1-\hat{p}(X_i)-\hat{e}(X_i)}\right], \quad (19)$$

where $\hat{p}(X_i)$ and $\hat{e}(X_i)$ are probability estimates of being treated and being in the within-community comparison group, respectively. Both estimates derive from a multinomial propensity score model.

As Robins and Rotnizky (1995) point out, if either the model of conditional means (11)-(12) or the model of propensity score (14)-(16) are correctly specified, the resulting estimator will be consistent. For this reason, Hirano and Imbens (2001) propose a flexible approach combining both models. Their estimator is relatively robust compared to estimators that rely on parsimonious specifications of one of the two components.

Hirano and Imbens' estimator is based on weighted least square estimation of the regression functions (11)-(12), where the control variables in the RHS of equations are a subset of X_i. Based on the estimators (17)-(19), the estimated weight, applied in these regressions, is given by:

$$\hat{\omega}(T_i, D_i, Z_i) = T_i + \frac{\hat{p}(Z_i)\cdot(1-T_i)\cdot D_i}{\hat{e}(Z_i)} + \frac{\hat{p}(Z_i)\cdot(1-D_i)}{1-\hat{p}(Z_i)-\hat{e}(Z_i)}, \quad (20)$$

where Z_i is a subset of balanced variables of X_i.[5]

[5] Wooldridge (2002; 2007) demonstrate the properties of this estimator for M estimation in general.

Assessing Income Effect and Behavioural Change Effect

Another issue of this research proposal refers to the distinction between the effects of income change and induced behavioural change in the average effects of the program on treated households. We basically propose to decompose the *APT*, *AET* and *ATT* effects in these components, using a methodology analogous to those presented by Juhn, Murphy and Pierce (1993) and Firpo, Fortin and Lemieux (2007).

First, in order to simplify, let $Y_i(D_i = 1, T_i = 1) = Y_{i,1,1}$, $Y_i(D_i = 1, T_i = 0) = Y_{i,1,0}$, and $Y_i(D_i = 0, T_i = 0) = Y_{i,0,0}$. Then, consider the outcome $Y_{i,D,T}$ as a function of the income level of household i, $W_{i,D,T}$, as follows:

$$Y_{i,D,T} = g_{D,T}(W_{i,D,T}, u_{i,D,T}), \tag{21}$$

where $g_{D,T}(.,.)$ is a non-parametric function and $u_{i,D,T}$ represents the unobservable components. As in Juhn, Murphy and Pierce's, it is useful to think of $u_{i,D,T}$ as two components: the percentile in the residual distribution, $\theta_{i,D,T}$, and the distribution function of the outcome equation residuals, $F_{D,T}(.)$. Thus, we have $u_{i,D,T} = F_{D,T}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})$.

If we define $\overline{g}(.,.)$ to be a counterfactual function and $\overline{F}(.)$ to be the counterfactual cumulative distribution, we can rewrite the equation (21) as:

$$Y_{i,D,T} = Y_{i,D,T}^{W} + Y_{i,D,T}^{g} + Y_{i,D,T}^{u}, \tag{22}$$

where

$$Y_{i,D,T}^{W} = \overline{g}(W_{i,D,T}, \overline{F}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})),$$

$$Y_{i,D,T}^{g} = g_{D,T}(W_{i,D,T}, \overline{F}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})) - \overline{g}(W_{i,D,T}, \overline{F}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})), \text{ and}$$

$$Y_{i,D,T}^{u} = g_{D,T}(W_{i,D,T}, F_{D,T}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})) - g_{D,T}(W_{i,D,T}, \overline{F}^{-1}(\theta_{i,D,T} \mid W_{i,D,T})).$$

For instance, the *ATT* effect (5) can be rewritten, without loss of generality, as:

$$\tau = E\left[\left(Y_{i,1,1}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \quad (23)$$

The first parenthesis within the brackets contains the income effect component, while the second parenthesis represents the average behavioural change component and the last one represents the change in idiosyncratic behaviour.

Similarly, the *APT* and *AET* effects may be written respectively as:

$$\tau_p = E\left[\left(Y_{i,1,1}^{W} - Y_{i,1,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,1,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,1,0}^{u}\right) \mid T_i = 1\right], \text{ and} \quad (24)$$

$$\tau_e = E\left[\left(Y_{i,1,0}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,0}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,0}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \quad (25)$$

In practice, after we calculate each component of equation (22) using a Kernel estimation weighted by expression (20), we can estimate the average effects of the program on each component of (23)-(25) through the propensity score weighted regression (PSW) described early.

D. Consultation and Dissemination

The focus of this research is one of the main unresolved questions regarding impact evaluation of CCT programmes: separating out income effect from "programme participation" effect (to be more general than reducing to conditionality effect) as well as the issue of externality of the programme. In that sense, it may interest a broader audience than only policy makers of a specific country.

Nevertheless, the team has close contact with policy-makers from two of the countries being evaluated: Paraguay and Brazil. In the case of Paraguay, results from previous impact evaluation have been disseminated in internal meetings with the managers of the program as well as in fora with the participation of civil society. During the internal discussions with policy makers, the issue of which component -- transfers or conditionalities and

complementary activities -- was the main factor leading to positive impacts of the programme as reported in the impact evaluation[6] often came up. Evidence of the dissemination work done by team members can be found in GTZ Paraguay webpage where the presentation made during a week-long mission to the country have been posted: http://www.gtzparaguay.org/docpobreza.htm. See specially the links under the following titles: *Evaluación de Impacto del Piloto de Tekoporâ, Resumen de la Evaluación de Tekopora* and this Working Paper: this link: http://www.gtzparaguay.org/Library/libros/doc_pobreza/WP%20%2038%20IPC%20ago%2007%20english.pdf.

Similarly, in the case of Brazil, results from evaluation of targeting and impacts of *Bolsa Familia* have also been disseminated in seminars and through IPC publications (Soares et al, 2007). IPC has been involved with the Ministry of Social Development in providing Technical Assistance in the design of cash transfer programme in African countries. This proximity has allowed IPC to have a very good dialogue with the managers responsible for the implementation of the different components of the programme in Brazil, where the issue of conditionality and the impact of complementary programmes is also very much debated.

The team would need to contact both the governments of Mexico and Nicaragua to raise their interest regarding the results of this evaluation. In addition, organizations involved in the evaluation of *Progresa* and *Red de Protección Social* such as the Inter-American Development Bank and IFPRI will also be contacted in order to increase the potential for consultation regarding previous studies as well as for the dissemination of the results of the current research.

[6] See Soares et al (2008).

The team has an interesting springboard to disseminate its work which is the dedicated webpage of the International Poverty Centre to cash transfer and social protection http://www.undp-povertycentre.org/CCT.do. We use this page to disseminate key findings for specific countries through a series of evaluation notes, country studies, One Pagers and Working Papers so that it can inform a broader audience that is comprised of policy-makers, practitioners, NGOs as well as the academia. This webpage has a monthly average of 19,000 accesses, 16,000 downloads, and a mailing list of 16,988 addresses all over the world.

Moreover, given the general interest that this research may arise, we would use all available opportunities to share the results and to discuss the suggested methodology in academia meetings as well as in conferences and seminars dedicated to the improvement of CCT programs.

Participation in Seminars, Workshops and Courses on CCTs:

Source: http://www.undp-povertycentre.org/mission.htm

Fabio Veras Soares and Tatiana Britto presented their working papers and country studies in a workshop to discuss the future of Red Solidaria: 06 May 2008.

http://www.redsolidaria.gob.sv/content/view/810/

Fabio Veras Soares took part in the Conference "Social Policies in Argentina: Results and Experiences" and delivered a talk on "The Impact Evaluation of Cash Transfers in Brazil". Buenos Aires, Argentina. 9-10 April 2008.

Fábio Veras Soares, Rafael Perez Ribas and Guilherme Issamu Hirata presented the preliminary results of the Impact Evaluation of the Tekoporã Conditional Cash Transfer programme to the Paraguayan Government in Asunción, Paraguay. 19-23 November. 2007.

Rafael Perez Ribas analysed the pilot results of the evaluation survey of the Solidariedad Conditional Cash Transfer programme, carried out by the Dominican Central Bank, in Santo Domingo, Dominican Republic. 25 September – 1 October. 2007.

Fábio Veras Soares gave presentations on "An Assessment of the Impact of Cash Transfers in Brazil" and "Impact Evaluation: an Overview" at the workshop "Designing and Implementing Social Transfer Programmes" in Cape Town, South Africa.
16-29 September 2007.

Fábio Veras Soares took part in missions to Ghana as part of the cooperation agreement between the Brazilian Ministry of Social Development and Fight against Hunger, the Government of Ghana, the IPC and the DfID for the design and implementation of the Ghanaian Cash Transfer Programme: Livelihood Empowerment against Poverty (LEAP).
16-27 August 2007.

Fábio Veras Soares and Rafael Perez Ribas advised the Social Cabinet of the Dominican Republic on building a framework for impact evaluation of the Solidariedad Conditional Cash Transfer programme in Santo Domingo, Dominican Republic. 31 July – 3 August 2007.

Fabio Veras Soares made a presentation on "Conditional Cash Transfer in Brazil, Chile and Mexico: Impacts upon Inequality" at the Charlotte Maxeke Conference on The Economics of Social Protection in South Africa 12 - 15 June 2007.

Fabio Veras Soares and Rafael Perez Ribas participated in the workshop on "Experiences and Lessons Learned in Conditional Cash Transfers Programmes in Latin America" in San Salvador, El Salvador. Presentations were made to country delegations from Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Mexico, Nicaragua, Panama, and Paraguay 16 May - 18 May. 2007.

Tatiana Britto took part in a mission to El Salvador as part of a GTZ-sponsored research programme on conditional cash transfers in Latin America. She met with Red Solidária

programme directors and managers at the Presidency's Technical Secretariat (STP), the Social Investment Fund for Local Development (FISDL) and related ministries. She also undertook field visits to the municipalities of Jicalapa, San Agustín, Jutiapa and Masahuat to meet with local stakeholders and beneficiaries and observe and report on the processes involved in the programme's implementation 19-28 March.2007.

E. The study team

Capacity building for the study team: This research will contribute to the improvement of the knowledge and research capacity on impact evaluation that the team responsible for the research programme on Conditional Cash Transfer has been developing. Among those activities we would like to emphasise the course on Econometric methods for Impact Evaluation that IPC researchers and team members: Fabio Veras Soares, Rafael Ribas, Guilherme Hirata and Bruno Araújo (from the Institute for Applied Economic Research – IPEA) are given this semester and that those from team who are not teaching are attending (Please check link below to see with topics are been reviewed and studied by the team:

http://www.undp-povertycentre.org/evaluation/IETC_Programme.pdf

1. Team Leader: **Fabio Veras Soares** will be the team leader. He has a PhD in Economics from University College London where he became familiarized with program evaluation techniques. His PhD dissertation is on labor economics, but he has used program evaluation methodologies – such as propensity score matching – to address the impact of minimum wage on employment transitions in one of the chapters of the dissertation. He has been working with part of the team on specific researches on the issue of conditional cash transfers and impact evaluation both at the Institute for Applied Economic Research (IPEA) and at the International Poverty Centre.

Task: Overall supervision and guidance.

2. Other team members:

Tatiana Britto (female): 32 years – works on the political economy of CCT programs

Task: summarizing the design and major features of the programme.

Rafael Ribas (male): 25 years – strong background in survey design and impact evaluation

Task: Methodology refinement and data analysis.

Elydia Silva (female): 25 years – strong background in econometrics and microsimulation

Task: data analysis.

Joana Costa (female): 28 years – strong background in labor economics and econometrics

Task: data analysis.

Guilherme Hirata (male): 27 years – strong background in labor economics and econometrics.

Task: methodology refinement and data analysis.

3. Other related researches.

1) Comparative research on Cash transfer among Latin American and African countries: simulations, needs assessment and institutional analysis. Financed by Dfid/GTZ and RBA-UNDP (Rafael Ribas, Fabio Soares, Tatiana Britto and Guilherme Hirata).

2) Impact Evaluation of the Pilot of Tekoporã. Financed by GTZ-Paraguay. (Rafael Ribas, Fabio Soares and Guilherme Hirata).

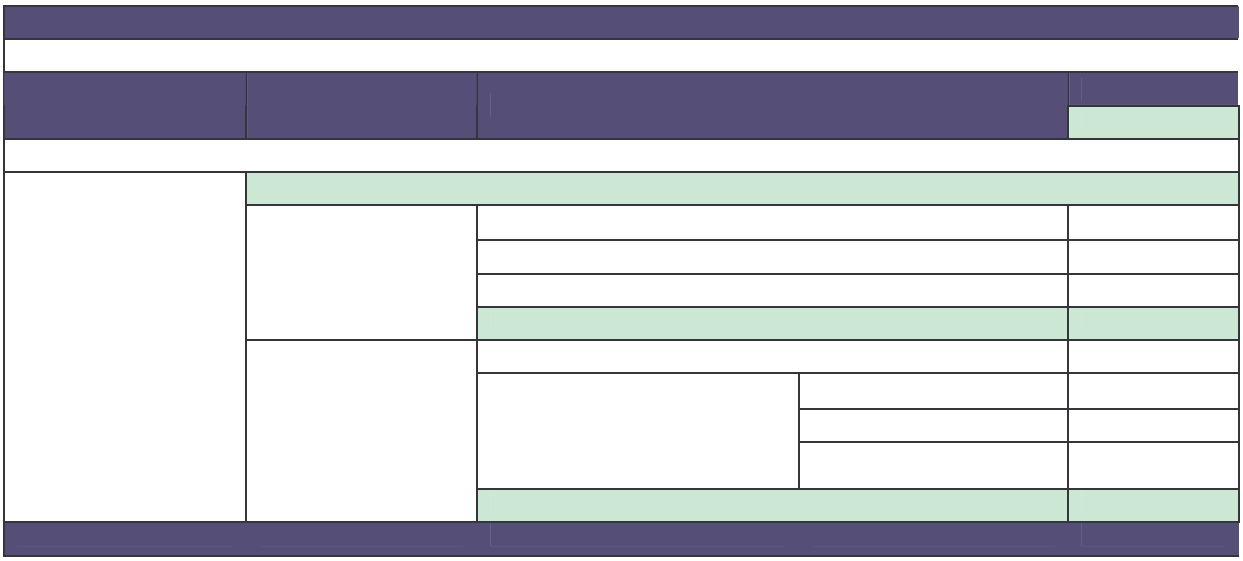
3) Impact Evaluation of the *Solidariedad* Program in Dominican Republic. Financed by Government of Dominican Republic and UNDP-Dominican Republic (Rafael Ribas, Fabio Soares and Guilherme Hirata).

4) Impact Evaluation of PROAP (*Favela-Bairro*). Financed by BID. (Fabio Soares)

5) Evaluation of *Bolsa Familia*. Financed by the Brazilian Government (Rafael Ribas)

F. Timeline

	2008						2009												2010											
Task	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Methodology development	X	X																												
Methodology review						X	X																							
Data collection		X	X	X			X	X		X	X																			
Data analysis				X	X	X		X	X	X	X	X																		
Interim report elaboration	X	X	X	X	X	X																								
Presentation of interim report at PEP general meeting						X																								
Draft Final Report							X	X	X	X	X	X	X	X																
Study Visit for consultation and dissemination															X	X	X	X												
Final Report															X	X	X	X	X	X	X	X	X							
Presentation of Final Report at PEP general meeting																								X						
Working Papers and academic dissemination																								X	X	X	X	X	X	X



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