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Estimating Participation and Spill-over Effects in Conditional Cash Transfer Programs

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PIERI2 - Non-Experimental

Externality and Behavioural Change Effects of a non-randomized CCT programme – Heterogeneous impact on Health and Education Outcomes

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Abstract

This paper explores the existence of externalities effects on health and of the pilot phase of a CCT programme in Paraguay. Externality is assessed through the decomposition of the Average Treatment Effect on the Treated (ATT) into participation and externality effect. This was possible thanks to the existence of two distinct comparison groups, one within the village and exposed to the externality and another in a different district not affected by the programme. Moreover, we investigate whether changes in family's outcomes come from the extra income or from changes triggered by the other non-monetary components of the programme such as the conditionalities and the family support services that can also lead to some behavioural changes. The results indicate that the programme was successful in improving child attendance to school and increasing visits at the health centres. They also suggest that the positive impacts on health service usage are not dependent on the knowledge the families have on the conditionaliteis and on the number of visits they receive from social workers, but the increase in school attendance is only observed for families who receive visits of the social workers.

Keywords: Externality, Income Effect, Behaviour Effect, Conditional Cash Transfer.

JEL Classification: C21, D12, D62, I38.

1. Introduction

This paper is part of a wider research agenda that aims to identify whether Conditional Cash Transfers (CCT) programmes have externalities that affect both beneficiary and non-beneficiary families who live in areas where the programme is implemented. In addition, this research agenda puts forward a methodology to decompose the programme's effects into the effect of the monetary transfer and the effect of the other components of the programme that have the objective to change the behaviour of the family. Both methodologies were first presented in Soares et al. (2009) in the context of the impact of the pilot of Tekoporã – a CCT programme in Paraguay - on consumption. Externatility is assessed through the decomposition of the average treatment effected on the treated (ATT) into participation (direct) effect and externality (indirect) effect. Each of these effects is then further decomposed into the effect due to the increase in the income and the impact of the other components of a CCT programme. In this paper, we use these methodologies to investigate the impact of Tekoporã on education and health outcomes. In addition, we assess the heterogeneity of the impact with regards to the knowledge of the conditionalities among beneficiary families as well as to other features of the design of the programme.

Cash transfers programmes have assumed an important role in the social protection schemes of developing countries. In Latin America they usually have a conditional component combined with monetary transfers which should affect not only family's income, but also their behaviour. Moreover, there is some evidence of spillover effects on non-beneficiary families living in the same communities of those who take part in the programme. Externalities can be caused by different reasons depending on the context and on the outcome of interest. It can be due to the sharing of the transfers between beneficiary and non-beneficiary families and/or local multiplier effects as well as due to some learning process fostered by social interactions between beneficiaries and non-beneficiaries. Understanding the impact of the conditionalities and the existence of externalities is an important step in order to better assess the black-box format of the results of the standard impact evaluations and to better inform policy makers on the adequacy of their CCT design (Handa et al. 2009).

Impact evaluations of CCT programmes in Latin America have showed positive results in several dimensions[1], however, the design of these evaluations, even the experimental ones such as *Progresa* (Mexico) and *Red de Protección Social* (Nicaragua), do not allow us to disentangle in a simple manner what can be attributed to the effect of the transfer itself and what is due to behavioural changes linked to the other (non-monetary) components of the programme. There has been some indirect evidence of the importance of conditionalities and of other non-monetary components of the programmes, e.g. social workers talks and visits. Schady and Araújo (2008) show that the positive effect on school enrolment of the Bono de Desarrollo Humano in Ecuador was restricted to those beneficiaries who believed there were conditionalities attached to the programme. De Brauwn and Hoddinott (2008) show that the enforcement of conditionalities was important for schooling progression from primary to secondary level in Mexico's CCT Progresa, but was not relevant for primary education attendance. Hoddinott and Skoufias (2004) show that the *pláticas* - health talks which are part of Progresa design - contributed to a greater diet diversification among beneficiaries.

Tekoporã is a CCT programme that is being scaled up in Paraguay with the objective of alleviating poverty and building up human and social capital. The programme consists of a monthly grant, that should be conditional to a minimum school attendance and regular visits to health centres and immunization updating and of monthly visits of social workers to help families to comply with the conditionalities as well as to "coach" them on a variety of issues such as obtaining identification cards, budget planning, cultivation of vegetable gardens, health and hygiene tips, etc. The conditionalities have not been verified in the pilot phase although they had been extensively communicated to the beneficiaries, mainly through the visits of the social workers. We use the information on the knowledge of the beneficiaries about the conditionalities as well as on the number of visits of social workers to beneficiary households to assess whether these factors have led to any heterogeneity in the impact of the programme on both health and education outcomes.

This paper contains five sections besides this introduction. The second section discusses the sources of externalities and behavioural changes and reviews the literature. The methodological section briefly presents the two decompositions used in this paper and the empirical strategy. The fourth section contains the main characteristics of Tekoporã Programme and data description. Section five brings some descriptive analysis and the empirical results. The last section concludes this paper.

[1] See Finzbein et al (2009) and Soares et al. (2010) for a review of several impact evaluations of CCT programmes.

2. Conditional Cash Transfer Programmes: externality effects and the role of the other components.

CCTs programmes are designed to have a positive and significant impact on the income of beneficiary families, leading to immediate poverty relief. The monetary transfer affects the budget constraint of beneficiary families and, consequently, their optimum choices. They also aim at increasing the demand for goods and services, particularly the demand for education and health, that is reinforced through conditionalities.

To guarantee the positive impact of the transfers, some programmes have worked with family support activities to help families to comply with conditionalities, to communicate key messages of the programme and/or to link them with complementary programmes and/or initiatives. In the case of Tekoporã, families receive regular visits of social workers who inform them about the importance of conditionalities, besides working with the families in several other dimensions. This component of the programme is also supposed to promote changes in family behaviour. Furthermore, the supply of information can reinforce the changes in the preferences of the families in the long term, if they believe this new behaviour is beneficial to them.

Therefore, change in behaviour can derive from a learning process. This process affects not only direct programme beneficiaries, but can also benefit non-participants families. Peer effects happen when non-participants emulate the behaviour of the ones directly affected by the programme, particularly, with regards to conditionalities and the new flow of information in the community (Manski, 1993). Thus, non-treated individuals can be affected by the treatment through their interaction with treated individuals. The larger the interaction among the groups and the size of the treated group relative to non-treated, the larger the spillover effects (Bobba, 2008)[2].

The population not participating in the CCT program but living in treated districts may experience indirect programme effects due to: 1) direct transfers from treated to non-treated households or the development of a credit market; 2) increase in overall income or prices (Angelucci and De Giorgi, 2009); 3) learning from peer interaction (Bobonis and Finan, 2005, Lalive and Cattaneo, 2009 and Bobba, 2008); and 4) and the desire to behave like the eligible population so that they would become eligible. The latter is only the case when eligibility criteria are not very clear to the whole population of the treated community.

Angelucci and De Giorgi (2009) find a positive impact on food consumption of ineligible households. They argue that the programme increased food consumption due to loans and transfers from eligible to ineligible households. Furthermore, Angelucci et al. (2009) explicitly show that this indirect effect on consumption is significant only among ineligible families who are family related to beneficiary families.

Bobonis and Finan (2005) and Lalive and Cattaneo (2009) show that Progresa has also had positive externality effects on school enrolment and attendance of ineligible families. Their hypothesis is that externalities are generated by endogenous peer effects as a result of social interaction between beneficiaries and non-beneficiaries. In the case of Bobonis and Finan, the

[2] Becker and Murphy (2000) argue that activities, behaviour and consumption choices subject to stronger social pressures are those more likely to take place publicly. Health and education choices are examples of public choices influenced by social interaction, via information diffusion, social and moral norms and custom. The decisions about sending children to school and taking them to health centres, about giving birth at hospital or about female working outside home depend largely on what are the community or family habits, educational level, religion, laws, norms etc

closer the child's household is of the eligibility cut-off point, the higher the peer effect. However, Lalive and Cattaneo highlight that peer effects also affect eligible children, boosting the impact of the programme. Moreover, they claim that peer effects arise because parents learn from each other about the ability of their children.[3]

Bobba (2008) takes advantage of the scaling up of Progresa to assess the difference in outcomes for higher and lower treatment density within-municipality across consecutive years of the programme. The author shows that a higher density of treatment villages in a region leads to greater externalities in school enrolment and attendance rate, attained years of education and child labour. Despite a positive spillover to non-treated in less treatment dense municipalities, a crowding out effect operates in those municipalities where there is more than 75 per cent of treated villages, suggesting constraints in the supply of education.

Gertler (2004) and Attanasio et al. (2005) find significant impact of CCT programmes on health. Health impacts might also be spilt over neighbouring non-treated households. Such externality affects the outcome of non-beneficiary households through the reduction of epidemics in the whole population. Miguel and Kremer (2004) show, for instance, that deworming treatment reduces worm burdens and then increases school attendance of both treated and untreated children in Kenya.

The present paper is a follow-up of Soares et al. (2009) and reproduces their methodology in order to assess the income and behaviour components with regards to their effect on educational and health. It is expected that peer effects influence non-treated households in treated districts through a learning process and interactions with the treated. If non-beneficiary individuals are positively affected by the programme, its impact when we do not consider the possibility of externality can be underestimated.[4]

Average treatment effect on the treated (ATT) is decomposed into participation and externality effects and further decomposed into income effect and other components effect. It is expected that the transfers would have a minor contribution to the effects on health and education since those are more likely influenced by the message attached to the conditionalities and communicated via the other component of the programme. The following section presents the empirical method for implementing the decomposition of the ATT into participation and externality effects as well as their further decomposition into income effect and non-monetary components effect.

3. Methodology

3.1. Decomposing the Average Treatment Effect on the Treated (ATT) into Average participation and externality effects on the treated

A key assumption of the programme evaluation literature to attribute a casual effect to a policy intervention is the absence of spillover effects on the chosen comparison group. The estimation of the average treatment effect is only possible under the assumption known as

[3] Barrera-Osorio et al. (2008) presents other evidence on positive peer effects of CCT programmes on schooling in Colombia.

[4] However, we should be aware of negative effects of the increase in demand for public services, such as crowding-out effect. If the supply of public service is not enough to fulfil the increase in demand generated by conditionalities and information diffusion, the priority access of beneficiary individuals would imply reduction in non-beneficiary access.

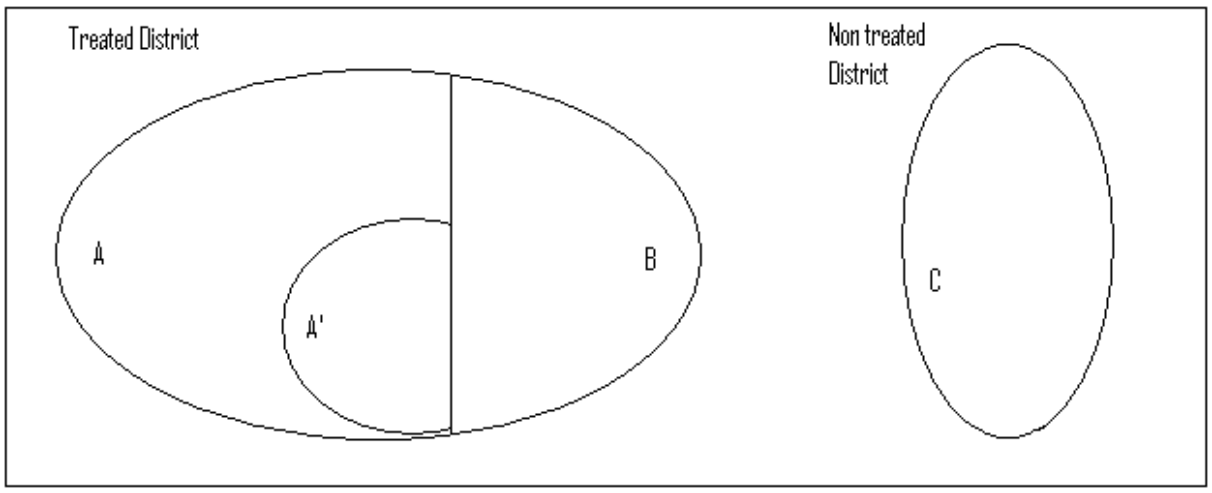
Single Unit Treatment Value Assumption - SUTVA (Rubin, 1980) which states that treatment does not affect directly or indirectly the comparison group. If the comparison group is also affected by the programme, outcomes differences between the treatment and comparison groups would potentially underestimate the impact. (Heckman et al., 1999 and Miguel and Kremer, 2004)

When spillover effects are due to social interaction, we can assume that it will affect the population who live in the same communities or districts, but will not affect significantly individuals from other districts. Thus, while SUTVA fails within districts, it holds between districts. If we accept this hypothesis, we can obtain an unbiased impact estimate of the ATT from the differences between treated and non-treated (or comparison) groups in the non-treated districts. Similarly, the decomposition of ATT into the sum of the differences between treated and non-treated groups in treated districts and between non-treated group in treated districts and the comparison group in the non-treated districts results unbiased estimates (Sobel, 2006 and Hudgens and Halloran, 2008).

The existence of a comparison group within treated districts can help to disentangle the joint effect of the income and behaviour effects over the treated. In order to do so, it is necessary to analyze the differences in outcomes for three groups: treated (group A), non-treated from treated districts (group B) and non-treated from non-treated districts (group C).

In group A, those who claim to be aware of the conditionalities were also identified as group A' for the purpose of analysing heterogeneity in our empirical strategy discussed in section 3.3.

Figure 1



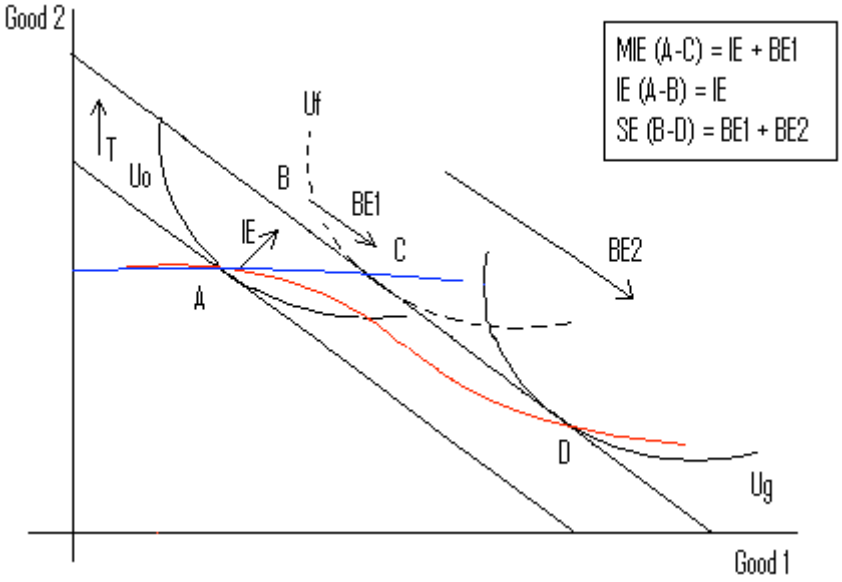
The program affects A directly through the monetary transfers and conditionality compliance. The transfer incurs an income effect (IE) as well as a behaviour effect (BE) as can be observed in the picture below. The treated respond by shifting from the indifference curve Uo to Uf that allows an increase in the consumption basket of two hypothetical goods according to their new preferences.

The conditionality in turn forces the families to change their preferences from indifference curve Uf to Ug. Note that Uf is not feasible because in order to receive the transfer the family must adopt the government indifference curve Ug. The blue curve is the original income expansion path of the families, while the red curve is the path desired by the government.

The behaviour effect is the sum of the change in preferences originated from transfers and from conditionality. In order to isolate the conditionality effect it would be necessary to have a comparison group that receives unconditional transfers only. In that design Uf would be

feasible and thus observed. Only then the difference between household's and government's preferences could be assessed.

Figure 2



Similarly, group B can also indirectly experience income and behaviour changes due to spillover effects. As mentioned before, the transfers can be shared between the two groups or non beneficiaries may benefit from an increase in credit and/or local multiplier effect due to the increase in the consumption of beneficiary families. Similarly, due to social interaction the sharing of the knowledge acquired through other components of the CCT programme can also change preferences of the non beneficiary families.

The ATT is the programme impact on an observable outcome. It is obtained through the estimation of the average difference between the outcome with the treatment (participating in the programme) and the outcome without the treatment (not participating in the programme) for the same household. The ATT effect can be defined as:

$$\tau = E\left[Y_i(T_i = 1) - Y_i(T_i = 0) \mid T_i = 1\right], \qquad (3.1)$$

where $E[.]$ is the expectation function, Y the outcome measure and T the treatment indicator for each person i.

The approach is similar to what is proposed by Hudgens and Halloran (2008). In order to identify participation and externality effects, we need to make a distinction between two comparison groups: those living in treated districts and those living in untreated districts.Let $D_i = 1$ indicate that household i living in the area where the programme took place, and $D_i = 0$ otherwise. Thus, $(D_i = 1, T_i = 0)$ indicates the within-community comparison group, while $(D_i = 0, T_i = 0)$ indicates the between-community comparison group. For all treated households, D_i is certainly equal to one, which leads to $(D_i = 1, T_i = 1)$. Note that there are no treated households in the non treated districts.

An underling assumption is that SUTVA holds for untreated districts, comparison group C do not suffer contamination or interference from groups A and B, thus there is no externality affecting those households.

We may define the Average Participation Effect on the Treated (*APT*), within-community effect, as follows:

$$\tau_p = E\left[Y_i\left(D_i = 1, T_i = 1\right) - Y_i\left(D_i = 1, T_i = 0\right) | T_i = 1\right], \tag{3.2}$$

and the Average Externality Effect on the Treated (*AET*), between-community effect, may be defined as:

$$\tau_e = E\left[Y_i\left(D_i = 1, T_i = 0\right) - Y_i\left(D_i = 0, T_i = 0\right) | T_i = 1\right]. \tag{3.3}$$

Then, we may rewrite the *ATT* effect as the sum of both effects 3.2 and 3.3:

$$\tau = \tau_e + \tau_p = E\left[Y_i\left(D_i = 1, T_i = 1\right) - Y_i\left(D_i = 0, T_i = 0\right) | T_i = 1\right]. \tag{3.4}$$

3.2. ASSESSING INCOME AND BEHAVIOURAL CHANGE EFFECT

Besides looking at externalities, this paper also applies a methodology to disentangle the effects of transfers and the changes induced by the characteristics of the programme (conditionalities and learning process) on treated households. We basically propose to decompose each of the *ATT, APT and AET* effects into income and behaviour 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 $Y_i\left(D_i = 1, T_i = 1\right) = Y_{i,1,1}$, $Y_i\left(D_i = 1, T_i = 0\right) = Y_{i,1,0}$, and $Y_i\left(D_i = 0, T_i = 0\right) = Y_{i,0,0}$. Then, consider the outcome $Y_{i,D,T}$ as a function of the income level of household *i*, $W_{i,D,T}$, as follows:

$$Y_{i,D,T} = g_{D,T}\left(W_{i,D,T}, u_{i,D,T}\right), \tag{3.5}$$

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 (1993), 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 $u_{i,D,T} = F_{D,T}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)$.

If we define $\overline{g}(.,.)$ as a counterfactual function representing what would the effect be if only the income changed while outcome preferences remained equal for every group; $g_{D,I}(.,\ .)$ is a pooled function in which each group has its mean consumption preferences – income elasticity; and $\overline{F}(.)$ is the counterfactual cumulative distribution for residuals. We can rewrite the equation (3.5) as:

$$Y_{i,D,T} = Y_{i,D,T}^{W} + Y_{i,D,T}^{g} + Y_{i,D,T}^{u}, \tag{3.6}$$

where

$$Y_{i,D,T}^{W} = \overline{g}\left(W_{i,D,T}, \overline{F}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)\right), \tag{3.7}$$

$$Y_{i,D,T}^{g} = g_{D,T}\left(W_{i,D,T}, \overline{F}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)\right) - \overline{g}\left(W_{i,D,T}, \overline{F}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)\right), \text{ and} \tag{3.8}$$

$$Y_{i,D,T}^{u} = g_{D,T}\left(W_{i,D,T}, F_{D,T}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)\right) - g_{D,T}\left(W_{i,D,T}, \overline{F}^{-1}\left(\theta_{i,D,T} | W_{i,D,T}\right)\right). \tag{3.9}$$

Through equation (3.7) we estimate a counterfactual in which outcomes are the result from income variation exclusively – it assumes mean fixed income elasticity. Equation (3.8), in turn, provides outcomes estimated through group specific income elasticity. Finally, Equation (3.9) assesses the observed residual differences.

Based on those counterfactual consumption outcomes, the *ATT* effect (3.1) can be rewritten, without loss of generality, as:

$$\tau = E\left[\left(Y_{i,1,1}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \tag{3.10}$$

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:

$$\tau_p = E\left[\left(Y_{i,1,1}^{W} - Y_{i,1,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,1,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,1,0}^{u}\right) \mid T_i = 1\right], \text{ and} \tag{3.11}$$

$$\tau_e = E\left[\left(Y_{i,1,0}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,0}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,0}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \tag{3.12}$$

Additionally, by using exclusively the income of between district comparison group in the non-parametric function, an estimate of the income path if the programme were unconditional. This gives us the marginal income effect:

$$Y_{i,D,T}^{MW} = \bar{g}\left(W_{i,D=0,T=0}, \bar{F}^{-1}\left(\theta_{i,D=0,T=0} \mid W_{i,D=0,T=0}\right)\right) \tag{3.13}$$

3.3 EMPIRICAL STRATEGY FOR APPLICATION OF DECOMPOSITION METHODOLOGY – THE WEIGHTING PROPENSITY SCORE METHOD

In practice, for the ATT estimation, it is not possible to observe the same household in both states (with and without the treatment) at same time. If the groups were defined randomly, we could assume that the average outcome is the same among the treated, $T_i = 1$, and a comparison group, $T_i = 0$ in the absence of treatment. However, although Tekoporã was not randomly assigned[5], the identification of eligible households was based on a non-monetary quality of life index (ICV). Thus, the effect can be adequately estimated conditioning on the X_i variables determinants of programme participation. The underlining assumption is that unobservable characteristics do not determine treatment assignment, i.e., selection into the programme is completely based on observable variables, X_i. Under randomization, or alternatively, conditioning to X_i, one can estimate unbiased *ATT* effect by comparing treated and comparison group outcomes in the presence of treatment for the cause is adequately identified (Rubin, 1978 and Rosenbaum and Rubin, 1983).

The identification of these effects requires the following unconfoundness assumption:

$$T_i \perp \left(Y_i(T_i = 0), Y_i(T_i = 1)\right) \mid X_i, D_i. \tag{3.14}$$

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

[5] See section 4 for programme description and evaluation design.

Similarly, we should also assume that, the average outcomes conditioned on X_i of both comparison groups would be the same in the absence of treatment. Formally, we assume that the distinction between comparison groups and the potential outcomes conditional on X_i are orthogonal:

$$D_i \perp \left(Y_i(D_i = 0), Y_i(D_i = 1)\right) | X_i, T_i = 0 . \tag{3.15}$$

Since one actually only observes:

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

assumptions (3.14) and (3.15) yield the following estimators of the *APT*, *AET* and *ATT*, respectively:

$$\hat{\tau}_p = E[Y_i \mid X_i, D_i = 1, T_i = 1] - E[Y_i \mid X_i, D_i = 1, T_i = 0], \tag{3.16}$$

$$\hat{\tau}_e = E[Y_i \mid X_i, D_i = 1, T_i = 0] - E[Y_i \mid X_i, D_i = 0, T_i = 0], \text{ and} \tag{3.17}$$

$$\hat{\tau} = E[Y_i \mid X_i, D_i = 1, T_i = 1] - E[Y_i \mid X_i, D_i = 0, T_i = 0]. \tag{3.18}$$

Without the distinction between the two comparison groups, one may assess the following confounded *ATT* estimator:

$$Y_i = \alpha_0 + \tau_c \cdot T_i + \alpha_1' X_i + \varepsilon_i$$

$$\begin{aligned} \hat{\tau}_c = \; & E[Y_i \mid X_i, D_i = 1, T_i = 1] \\ & - P[D_i = 1 \mid X_i, T_i = 0] \cdot E[Y_i \mid X_i, D_i = 1, T_i = 0] \\ & - P[D_i = 0 \mid X_i, T_i = 0] \cdot E[Y_i \mid X_i, D_i = 0, T_i = 0], \end{aligned} \tag{3.19}$$

where $P[.]$ is a probability function. It just tells us how much the treated outcomes differ, on average, from their comparables in the whole untreated group. Since it depends on the composition of the untreated group, nothing can be concluded from this estimator in terms of programme impact on the treated, if there are externality effects on the untreated group living in the treated districts.

Decomposition of ATT into participation and externality effects

An empirical alternative to estimate participation and externality effects proposed in section 3.2 is to approximate the conditional means by estimating the following linear functions (Rubin, 1977):

$$Y_i = \alpha_0 + \tau_p \cdot T_i + \tau_e \cdot D_i + \alpha_1' X_i + \varepsilon_i \tag{3.20}$$

$$\tau = \tau_p + \tau_e \tag{3.21}$$

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 0, 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:

$$E[x_i \mid p(X_i), T_i = 1] = E[x_i \mid p(X_i), D_i, T_i = 0] \quad \forall x_i \in X_i \tag{3.22}$$

This assumption is called 'balancing property' and can be empirically verified. In our case, we tested the differences in means for observables characteristics between treated and comparison groups are significant for distinct intervals of propensity score, taking into consideration an interval of confidence of 95 per cent. 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 (3.15) 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. These probabilities can be estimated using a multinomial or multivariate regression model, 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. Weighting on the propensity score is a means to make comparison group's characteristics closer to treated observations. The objective is to eliminate differences in mean X_i.

Hirano and Imbens' estimator is based on weighted least square estimation of the regression functions (3.19), where the control variables in the RHS of equations are a subset of X_i. The estimated weight, applied in these regressions, is given by:

$$\hat{\omega}(T_i, D_i, Z_i) = T_i + \frac{\hat{p}(Z_i)\cdot(1 - T_i)\cdot D_i}{\hat{e}(Z_i)} + \frac{\hat{p}(Z_i)\cdot(1 - D_i)}{1 - \hat{p}(Z_i) - \hat{e}(Z_i)}, \tag{3.23}$$

where Z_i is a subset of balanced variables of X_i.[6]

Difference in Differences

As some outcome variables were available in both baseline and follow-up datasets, it was possible to use the difference in differences (DD) methodology in addition to the cross section single difference approach described so far[7].

The use of DD helps to control for baseline differences that can contaminate the result with selection bias. Once the sample has being balanced through weights, the baseline difference due to treatment should not be significant. For those outcomes of interest with available baseline information, we chose to use difference in differences to make sure the results are robust.

First we pool baseline and follow-up data. Then we estimate the following equation weighted by expression (3.23):

[6] Wooldridge (2002; 2007) demonstrate the properties of this estimator.
[7] See Woodridge and Imbens (2008) and Bertrand, Duflo and Mullainathan (2003)

$$Y_{ij} = \alpha_0 + \alpha_t \cdot S_{ij} + \alpha_{ba} \cdot T_{ij} + \alpha_{bb} \cdot D_{ij} + \tau_p \cdot T_{ij} \cdot S_{ij} + \tau_e \cdot D_{ij} \cdot S_{ij} + \alpha_1' X_{ij} + \varepsilon_{ij} \text{ , where:} \quad (3.24)$$

$$\tau = \tau_p + \tau_e \quad (3.25)$$

S_i indicates the year. It equals 0 for year j = 2005 and 1 for year j = 2006; α_t is the time trend; α_{ba} is the baseline difference between groups A and B (baseline within- district differences); α_{bb} is difference between groups B and C (baseline between- district differences); τ_p is the participation effect, that is, how much of the baseline difference between group A and B changed over the year the programme was implemented; τ_e is the externality effect, that is, how much of the baseline difference between group B and C changed over the year the programme was implemented.

Heterogeneity of ATT: awareness of conditionalities and social workers visits

For the heterogeneity analysis, a dummy K_i identifying treated individuals aware of the need to comply with conditionalities or who received monthly visits of social workers was added to the model as follows:

$$Y_i = \alpha_0 + \tau_{p'} \cdot T_i + \tau_{p''} \cdot K_i + \tau_e \cdot D_i + \alpha_1' X_i + \varepsilon_i \quad (3.26)$$

Thus, $\tau_{p'}$ is the estimated APT for treated unaware of the need to comply and $\tau_{p''}$ is how much knowing about compliance changes the effect. The overall APT is the sum of both:

$$\tau_p = \tau_{p'} + \tau_{p''} \quad (3.27)$$

ATT for the aware (visited) group is:

$$\tau = \tau_{p'} + \tau_{p''} + \tau_e \quad (3.28)$$

ATT for the unaware (non visited) group is:

$$\tau = \tau_{p'} + \tau_e \quad (3.29)$$

Income and Behaviour Effects Decomposition

For the decomposition of income and behaviour effects we first estimate the smoothed polynomial relationship between kernel densities of outcomes and household income, which is a non-parametrical estimation. This estimation is made for (i) the household income of whole sample as shown in equation (3.7), (ii) the household income of treated, comparison group within and between districts separately and then pooled as in equation (3.8), and (iii) the household income of comparison group between districts as in (3.13). The predicted results are used as dependent variables in equation (3.20) weighted by expression (3.23).

The non-parametric estimation of the outcome using the household income of the whole sample (i) is $Y^W_{i,D,T}$, which allows for the estimation of the income effect component in ATT, APT and AET. When (ii) is used, the estimation provides $Y^g_{i,D,T}$ which allows for the estimation of the substitution effect component. Following (3.9), the unobserved effect is calculated by the residual difference between $Y^W_{i,D,T}$ and $Y^g_{i,D,T}$, resulting in $Y^u_{i,D,T}$. When (iii) is used, the non-parametrical estimation provides $Y^{MW}_{i,D,T}$ enabling to assess the marginal income effect associated to ATT, APT and AET.

4. Programme Description and Evaluation Design[1]

The pilot of *Tekoporã*, the Paraguayan Conditional Cash Transfer (CCT) programme, started in September 2005 with 3.452 beneficiary households mostly from rural areas, and is being scaled up by the new government. *Tekoporã* has been gradually expanded reached 15 districts from 5 departments by 2009. The pilot phase ran between 2005-2006, and covered five districts of two departments. These districts were selected from a pool of 66 districts considered to have the bulk of the vulnerable population, according to a scoring index called Geographical Prioritization Index (IPG), which is composed by both monetary and non-monetary indicators.

This programme has two main components. The first one is a monetary transfer that aims to alleviate immediate family's budget constraints that encompasses a benefit of 30,000 Guaraníes (US$ 6) per child or pregnant woman up to a limit of four children per household; in addition to the basic transfer of 60,000 Guaranies (US$ 12) per month. Thus, eligible households could receive between 90,000 Guaranies to 180,000 Guaranies per month (US$ 18 and US$ 36). The second component encompasses conditionalities related to school attendance, regular visits to health centres and updating of immunizations as well as family support provided by social workers (*Guias Familiares*). This second component has the goal of strengthen the long term-effect of the programme, granting changes in attitude and behaviour of family members through education, health and training.

To identify eligible households it was adopted a non-monetary quality of life index (ICV) as the targeting tool. Such an approach has been common throughout Latin American, where monitoring of poverty often rely on using a composite index of Unsatisfied Basic Needs. The ICV varies between 0 and 100 and is comprised of variables related to: housing condition; access to public services and utilities, such as water, electricity, garbage collection and telephone; health care and insurance; the education of the head of household and spouse; years of schooling "lost" by children aged between 6 and 24 years; the occupation of the head of the household; ownership of durable goods; and the household demographic composition. Unlike the IPG, ICV does not use any monetary variables.

Households are eligible for the program if they fulfil all the following conditions:

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 according to the Index of Geographical Prioritaziation (IPG);

3) have an ICV below 40 points.[8]

The pilot of *Tekoporã* started in five districts: Buena Vista and Abaí in the Department of Caazapá, and Santa Rosa del Aguaray, Lima and Unión in the Department of San Pedro. In this pilot phase, the *Ficha Hogar* was fielded through a census that took place in the poorest areas of the selected districts, in addition to the poorest areas of other two districts -- Moises Bertoni in the department of Caazapá and Tucuati in the department of San Pedro --that did not take part in the pilot.

Furthermore, potentially eligible households that did not live in the poorest areas of the districts of the pilot could also be included in the program registry as a result of the so-called 'demand process', namely, based on their demand to have information on their living

[8] Initially the program intended to target only households with an ICV below 25 points, but the realization that the number of predicted beneficiaries was below the expected numbers per district and due to some complaints at the local level, the eligibility threshold was increased to 40 points.

conditions provided to the *Ficha Hogar*. In total, 7,990 households were screened by the census and 1,827 by demand.

4.1 DATABASE

In the absence of a baseline survey, information on household characteristics before the programme started comes from the database originated by Ficha Hogar[9], which was the instrument used to collect information on the variables used to calculate the ICV – the main indicator for the selection of beneficiary households. The follow-up survey was fielded between January and April of 2007. It contains all information available in Ficha Hogar and additional questions necessary to capture the outcomes of interest that were missing in the baseline (e.g. consumption data, school attendance, visit to health centres, etc).

We have information of 1,093 households. Among the 1093 households with complete interviews at baseline, 316 (28.91%) are treated, 430 (39.34%) are control from treated district (within district comparison group) and 347 (31.74%) are control from non-treated districts (between district comparison group). In terms of individuals these figures correspond to 2,002 (31.26%), 2,320 (36.23%) and 2,082 (32.51%), respectively, adding up to 6,404 observations.

Both comparison groups of households have eligible households, which had children and ICV less than 40, and ineligible households, which also had children but with ICV equal to or greater than 40. Households that do not have children or pregnant women were automatically excluded from the dataset, as well as those households registered with an incomplete interview.[10]

The within district comparison group is comprised of both ineligible and eligible households (ICV lower than 40), the latter were 'overlooked' or 'forgotten' by the program. They represent 66 percent of the within district comparison group. Unlike the between district comparison group, the within group have better living standard indicators since a reasonable share, 34 percent, has ICV over 40.

In the sample of the between district comparison group more than 90 percent of the households with children had ICV below 40. It is worth to mention the untreated districts were meant to be included in the pilot, but due to budget restrictions the programme could only afford five districts. In order to keep the geographical balance between departments, one district from each department was excluded from the pilot.

Our outcomes of interest are related to education and health. To assess the impact on education outcomes, we created dummies for school attendance and grade promotion for 7 to 15 year-old children. We also included two dummies related to school attendance in 2006: one that indicates if children attended school more than 2 days a week and another that indicates an attendance of 5 days or more per week.

Since there is very limited information on school indicators in baseline, retrospective questions were added in the follow-up questionnaire. The retrospective information is not very precise because the person that answered the survey might not recall the exact facts from past years.

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[9] The information provided by *Ficha Hogar* will be used to estimate the propensity score to match treated and comparison households.

[10] About 8% of the households registered in the *Ficha Hogar* had an incomplete interview (752 of 9,817). Nonetheless, 98% of these cases (736) were registered by demand and 88% (6 from the census and 653 by demand) have been treated (Soares and Ribas, 2007).

With regards to child health indicators, we are mostly interested in children's vaccination records and regular visits to health centres to monitor weight and height, which are key conditionalities of the programme. As with vaccination, we use three variables: a) possession of vaccination card; b) showed the vaccination card and c) child have at least 70% of vaccines updated. As with visits to the health centres regular visits we use a) at least one visit in the last year, b) 3 visits or more per year.

In the estimation of the impact of the programme on education outcomes we use as control variables the following set: sex, age, age squared, number of years lost in schooling for children under 14 years old, number of school years for household head, number of household members aged under 5 years, number of household members aged between 6 and 14 years. In the estimation of child health outcomes we used the following: size of the family, child/adult ratio for children under 5 years old, child/adult ratio for children between 6 and 14 years and age in months for children under 5 years old and its square.

5. Empirical Results

5.1 DESCRIPTIVE STATISTICS

This subsection presents some descriptive results of the sample and allows for an initial contrast between treated and comparison groups. Means for individual and family demographic characteristics are described in Table 1. Table 2 brings some evidence on how effective the programme has been in making beneficiaries aware of the programme's conditionalities and on ensuring that beneficiaries receive monthly visits from social workers. The descriptive tables for education and health outcomes are shown in Table 3 and 4, respectively.

According to Table 1, the average size of the household is seven people and the average age is 22 years. There are 2.5 children per household and 2/3 of them are between 6 and 14 years old. Children on average are 1.5 years behind in school and the average household head has no more than 4 years of schooling. Household per capita income is on average 133,000 *guaraníes* per month (USD 26.6/month) and the average ICV is 24, about half the cutoff point of 40.

Note that the controls from treated districts are better off than the two other groups. It is likely that they were not randomly overlooked/forgotten. They have higher ICV and average household per capita income. One possible reason for this administrative mistake refers to the change in the cut-off point of the eligibility criteria. As the cuf-off point was increased from 25 to 40 when the registration process into the program had already begun, it is possible that in some neighbourhoods, potential beneficiaries whose ICV was in the range between 25 and 40 did not receive the invitation to register.

Table 1 – Descriptive analysis of individuals and families' demographic characteristics

	Total (A+B+C)	Treated group (A)	Comparison groups from: within district (B)	between district (C)	Difference in means significance (A–C)	(A–B)	(B–C)
Household size	7.070	7.106	6.704	7.261		***	***
	(0.035)	(0.058)	(0.067)	(0.063)			
% Male	0.527	0.528	0.524	0.528			
	(0.007)	(0.011)	(0.011)	(0.011)			
Age	21.626	21.088	23.230	21.951	**	***	**
	(0.234)	(0.404)	(0.405)	(0.424)			
Age in months for children under 5 years old	35.734	36.048	37.019	33.851			**
	(0.648)	(1.134)	(1.071)	(1.132)			
Number of children under 5 years old	1.210	1.212	1.033	1.350		***	***
	(0.014)	(0.025)	(0.021)	(0.026)			
Number of children between 6 and 14 years old	2.232	2.338	1.850	2.222	***	***	***
	(0.019)	(0.034)	(0.030)	(0.033)			
Dependence ratio for children under 5 years old (children/adults)	0.164	0.166	0.151	0.168		***	***
	(0.002)	(0.004)	(0.003)	(0.003)			
Dependence ratio for children between 6 and 14 years old (children/adults)	0.303	0.316	0.268	0.289	***	***	***
	(0.002)	(0.004)	(0.004)	(0.004)			
Number of years lost in schooling	1.522	1.570	1.244	1.579		***	***
	(0.034)	(0.057)	(0.056)	(0.060)			
Number of school years of Household Head	3.943	3.787	4.873	3.658		***	***
	(0.030)	(0.048)	(0.061)	(0.051)			
Household per capita income	132,937	133,975	141,143	123,086	***	*	***
	(1,412)	(2,251)	(2,921)	(2,624)			
ICV - Indice de Calidad de Vida	23.667	21.863	31.405	22.825	***	***	***
	(0.103)	(0.142)	(0.200)	(0.172)			

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

Table 2 describes shows that 86 per cent of treated households were visited by a social worker at least once per month. As the social worker should inform them about conditionalities, one would expect that at least those who were visited would be aware of the need to comply with them. In fact, 92% claims to know about the conditionalities, but a smaller share know about each one of them requisite.

Table 2 - Descriptive statistics on the awareness of the treated about need to comply with conditionalities

	Average
Has received at least 1 visit per month from *social workers*	86%
Is aware of programme conditionalities	92%
Is aware of school attendance conditionality	83%
Is aware of visits to child height and weight control conditionality	67%
Is aware of vaccination conditionality	58%

Source: Own calculation based on the Evaluation Survey.

Table 3 shows that 93% (92%) of children attended school and 90% (92%) graduated to the next level in 2006 (2005). Over 96% of children attended school 5 days/week or more. The groups are similar to each other on average. In terms of attendance and progression, the control group from untreated districts had a worse perform as the averages are significantly lower in 2006 in comparison to the groups living in treated districts.

Table 3 - Descriptive statistics of educational outcomes

			Comparison groups from:		Difference in means significance		
	Total (A+B+C)	Treated group (A)	within district (B)	between district (C)	(A–C)	(A–B)	(B–C)
Attendance 2006	0.939	0.953	0.931	0.897	***		**
	(0.006)	(0.009)	(0.011)	(0.013)			
Attendance 2005	0.921	0.925	0.919	0.907			
	(0.007)	(0.011)	(0.012)	(0.013)			
Progression 2006	0.904	0.911	0.909	0.873	**		*
	(0.007)	(0.011)	(0.012)	(0.014)			
Progression 2005	0.916	0.918	0.928	0.901			
	(0.007)	(0.012)	(0.012)	(0.014)			
Attendance of 3 days/week or more	0.985	0.986	0.965	0.995		**	***
	(0.003)	(0.005)	(0.008)	(0.003)			
Attendance of 5 days/week or more	0.966	0.970	0.926	0.982		***	***
	(0.005)	(0.007)	(0.012)	(0.006)			

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

According to Table 4, the treated group had a higher probability of showing the vaccination card, 65 per cent as well as higher percentage of updated vaccines 51 per cent when compared to the within-district comparison group, 45 per cent and the between-district comparison group, 44 per cent. As for visits to the health centres, the treated group has a lower probability than attending more than once the health centre, but have a higher probability of attending more than twice and more than four times than the between-group comparison group.

Table 4 - Descriptive statistics of health outcomes

	Total (A+B+C)	Treated group (A)	Comparison groups: within district (B)	Comparison groups: between district (C)	Difference in means significance (A–C)	(A–B)	(B–C)
Vaccination card	0.624	0.652	0.613	0.552	**		
	(0.017)	(0.029)	(0.029)	(0.030)			
% updated vaccines	0.491	0.516	0.455	0.442	*	*	
	(0.015)	(0.025)	(0.025)	(0.026)			
More than 70% updated vaccines	0.408	0.435	0.374	0.353	*		
	(0.017)	(0.031)	(0.029)	(0.029)			
At least one visit to child development control	0.818	0.808	0.843	0.828			
	(0.014)	(0.024)	(0.022)	(0.023)			
More than 1 child development control visit	0.731	0.734	0.755	0.705			
	(0.016)	(0.027)	(0.026)	(0.028)			
More than 2 child development control visits	0.608	0.643	0.575	0.530	**		
	(0.017)	(0.029)	(0.030)	(0.030)			
More than 3 child development control visits	0.400	0.414	0.413	0.349			
	(0.017)	(0.030)	(0.030)	(0.029)			
More than 4 child development control visits	0.315	0.342	0.283	0.259	*		
	(0.016)	(0.029)	(0.027)	(0.027)			

Source: Own calculation based on the Evaluation Survey.

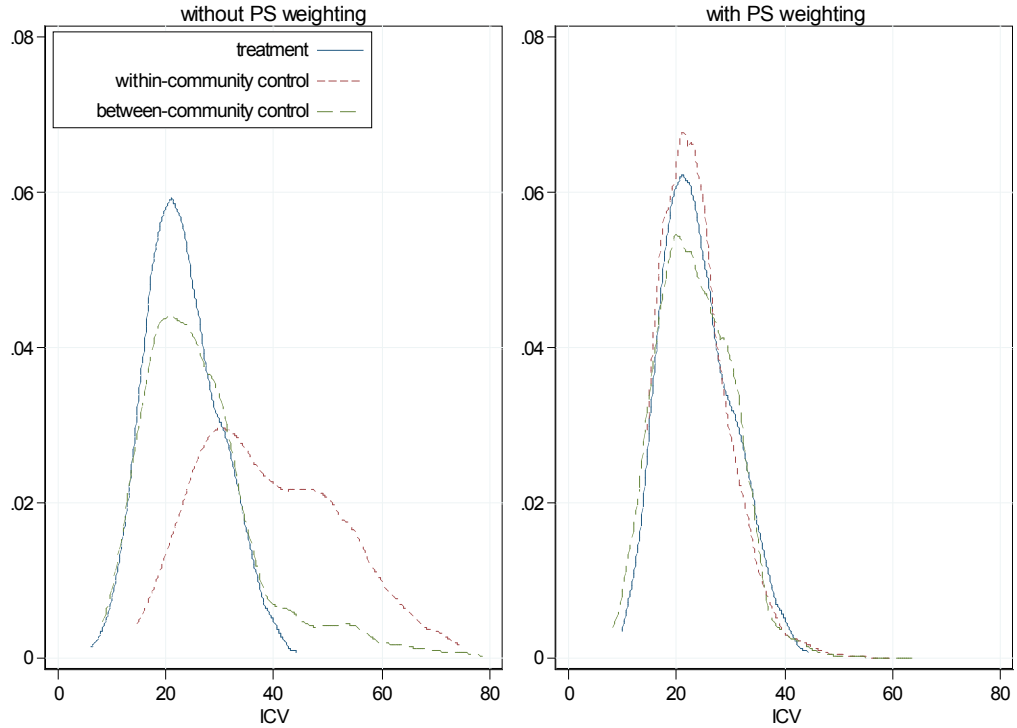
Note: Significant different from treated group at *10%, **5% and ***1%.

In an effort to even out the means of relevant variables to make the groups comparable, as stated in the methodology, within and between-district observations were re-weighted based on a propensity score calculated using baseline information. The propensity score manage to balance the distribution of the characteristics between the three groups.

Figure 1 shows how the weighting changes the format of the kernel density distribution of the *Indice de Calidad de Vida* (ICV) in the comparison groups. It gives a higher weight to the observations with ICV below 40, so that the distributions of the comparison groups mimic the distribution for the treated group (shown in the right panel). [11]

[11] Further information about multiple propensity score is available in Soares et al. (2009).

Figure 1 -Kernel Density of the Probabilities of Being Treated for Treated and Comparison Groups



Source: own elaboration based on *Ficha Hogar*.

5.2 PARTICIPATION AND EXTERNALITIES, INCOME AND SUBSTITUTION EFFECT AND HETEGONEITY

The tables below contain the estimates for the coefficients of the ATT, APT and AET estimates according to equation 3.19 (columns 5 to 7). When considering heterogeneity, equation 3.25 was used (columns 8 to 13); and when difference in differences methodology was applied, equation 3.23 was used (columns 2 to 4).

The decomposition into marginal income effect, MIE, income effect, IE, substitution effect, SE, and unexplained effect, UE, were obtained following equations 3.13, 3.7, 3.8 and 3.9, respectively.

5.2.1 EDUCATIONAL OUTCOMES

Four estimates of the Tekoporã effect on school attendance are shown in Table 5 and on progression in Table 6: difference in differences estimates, single difference estimates, heterogeneity taking into account social worker visits and heterogeneity taking into account the knowledge of conditionalities. Table 7 depicts Tekoporã effect on attending school more than 4 days/week. This table only has three out of the four estimates; the lack of 2005 information prevented the estimation of difference in differences.

Table 5 – Effect of Treatment on Treated for School Attendance

	Difference in Differences			Single Difference			Heterogeneity over visits from social worker			Heterogeneity over school attendance conditionality		
	Coef.	Std.		Coef.	Std.		Coef.	Std.		Coef.	Std.	
ATT	0.033	0.030		0.069	0.021	***						
MIE	-0.005	0.003		0.001	0.008							
IE	-0.004	0.004		-0.003	0.005							
SE	0.057	0.014	***	0.054	0.021	**						
UE	-0.020	0.030		0.018	0.010	*						
ATT – OC							0.072	0.019	***	0.071	0.021	***
MIE							0.000	0.009		0.000	0.010	
IE							-0.004	0.006		-0.004	0.006	
SE							0.053	0.019	***	0.053	0.020	***
UE							0.024	0.012	**	0.021	0.012	*
ATT – Transf.							0.048	0.029		0.060	0.031	*
MIE							0.008	0.009		0.003	0.007	
IE							0.003	0.005		0.001	0.004	
SE							0.060	0.020	***	0.057	0.021	***
UE							-0.015	0.021		0.001	0.022	
APT	0.019	0.074		0.050	0.039		0.053	0.038		0.052	0.037	
MIE	-0.002	0.005		0.001	0.011		-0.001	0.011		0.000	0.012	
IE	-0.002	0.005		-0.001	0.004		-0.002	0.005		-0.002	0.005	
SE	0.065	0.056		0.072	0.060		0.071	0.058		0.071	0.056	
UE	-0.045	0.055		-0.021	0.029		-0.015	0.029		-0.017	0.028	
AET	0.014	0.079		0.019	0.041		0.019	0.038		0.019	0.038	
MIE	-0.003	0.003		0.000	0.007		0.000	0.007		0.000	0.008	
IE	-0.003	0.004		-0.002	0.005		-0.002	0.005		-0.002	0.005	
SE	-0.008	0.057		-0.018	0.061		-0.018	0.059		-0.018	0.058	
UE	0.025	0.058		0.039	0.034		0.039	0.034		0.039	0.033	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

The ATT estimate given by difference in differences methodology was not significant. But, the substitution effect component was significant and robust, indicating an increase of six per cent in school attendance. However, as mentioned in database description, there is little information related to school indicators in baseline; thus, we had to resort to retrospective information, about attendance and progression, which is not very reliable.

According to the single difference estimates, the program increased the proportion of children who attended school in 2006 by seven per cent (ATT). The heterogeneity analysis shows that the same effect is observed for those who knew about the school attendance conditionality and/or were visited by the social worker (ATT-OC). From the income and substitution effect decomposition it may be concluded that most of the effect mentioned above comes from the substitution effect, that is, the change in the preferences, and not from the alleviation of the budgetary restriction.

For those not visited by social workers there was no significant net effect. This could highlight the effectiveness of social workers in disseminating the importance of school attendance. But, taking a closer look, the substitution effect is of six per cent and significant, meaning that if it was not for the effect of unobservables, the ATT would be the same for the two treated groups. The AET does not indicate any evidence of potential externality.

Furthermore, the impact of the programme on school attendance for those who were aware of conditionalities seems to be slightly more robust than the impact for those who were not aware of them. The former is significant at 1% whereas the latter is significant only at 10%. However, the point estimates is quite similar, seven per cent for those who knew about the conditionalities and six per cent for those who did not know it. Again, as in the case of the visits of the social workers, the result is totally driven by the substitution effect and not by the income effect. The results also show that there is no externality related to school attendance for the within district comparison group as the AET estimates were not significant.

Table 6 – Effect of Treatment on Treated for Progression

	Difference in Differences			Single Difference			Heterogeneity over visits from social worker			Heterogeneity over school attendance conditionality		
	Coef.	Std.		Coef.	Std.		Coef.	Std.		Coef.	Std.	
ATT	0.050	0.036		0.076	0.026	***						
MIE	-0.002	0.004		0.022	0.016							
IE	-0.006	0.005		-0.001	0.009							
SE	0.043	0.017	**	0.070	0.027	***						
UE	0.013	0.034		0.006	0.013							
ATT – OC							0.083	0.026	***	0.079	0.027	***
MIE							0.024	0.017		0.025	0.019	
IE							-0.003	0.011		-0.002	0.011	
SE							0.069	0.027	**	0.070	0.028	**
UE							0.017	0.016		0.011	0.016	
ATT – Transf.							0.032	0.046		0.060	0.044	
MIE							0.008	0.020		0.005	0.018	
IE							0.008	0.008		0.004	0.009	
SE							0.076	0.029	***	0.075	0.030	**
UE							-0.052	0.034		-0.019	0.033	
APT	0.055	0.078		0.057	0.057		0.065	0.053		0.060	0.053	
MIE	0.002	0.007		0.019	0.016		0.021	0.018		0.022	0.018	
IE	-0.002	0.007		0.004	0.007		0.002	0.007		0.003	0.007	
SE	0.097	0.070		0.094	0.074		0.093	0.071		0.093	0.072	
UE	-0.040	0.054		-0.040	0.027		-0.030	0.028		-0.035	0.028	
AET	-0.005	0.082		0.019	0.058		0.018	0.058		0.018	0.059	
MIE	-0.005	0.005		0.003	0.016		0.003	0.017		0.003	0.018	
IE	-0.004	0.005		-0.005	0.009		-0.005	0.008		-0.005	0.009	
SE	-0.054	0.069		-0.023	0.075		-0.023	0.075		-0.023	0.076	
UE	0.053	0.060		0.046	0.031		0.046	0.033		0.046	0.031	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

The effects over progression are in line with those of school attendance described above. Difference in differences net effect was not significant. The substitution effect, in turn, indicates a four per cent increase in progression.

According to the single difference estimates for the impact on the treated households who either were visited by the social workers or were aware of the school attendance conditionality suggest an increase of about eight per cent on the proportion of children who attended school in 2006 and were approved. Most of the effect can be explained by a substitution effect of seven per cent. The substitution effect is also present for those who claim to only have received the transfers. The substitution effect component is robust in the four estimates. Again there is no evidence of externality effect.

The results show a much clearer effect of both social workers visit and knowledge of conditionalities in the case of progression than attendance, since the estimates are only significant for this group and the point estimates are much higher for them.

Table 7 – Effect of Treatment on Treated for School Attendance higher than 4 days/week

	Single Difference			Heterogeneity over visits from social worker		Heterogeneity over school attendance conditionality		
	Coef.	Std.		Coef.	Std.	Coef.	Std.	
ATT	0.017	0.023						
MIE	0.015	0.014						
IE	0.010	0.009						
SE	0.017	0.023						
UE	-0.010	0.010						
ATT - OC				0.029	0.022	0.026	0.023	
MIE				0.019	0.017	0.018	0.015	
IE				0.012	0.010	0.012	0.009	
SE				0.021	0.022	0.020	0.022	
UE				-0.004	0.011	-0.005	0.011	
ATT – Transf.				-0.050	0.047	-0.029	0.040	
MIE				-0.009	0.015	-0.001	0.013	
IE				-0.005	0.007	0.000	0.006	
SE				-0.001	0.028	0.006	0.026	
UE				-0.043	0.031	-0.036	0.025	
APT	-0.009	0.013		0.002	0.011	0.000	0.012	
MIE	0.010	0.010		0.014	0.011	0.013	0.011	
IE	0.007	0.005		0.009	0.005	0.009	0.005	*
SE	-0.024	0.009	***	-0.021	0.009	-0.022	0.009	**
UE	0.008	0.009		0.014	0.011	0.013	0.011	
AET	0.026	0.025		0.026	0.026	0.026	0.026	
MIE	0.005	0.012		0.005	0.012	0.005	0.012	
IE	0.003	0.008		0.003	0.008	0.003	0.008	
SE	0.042	0.023	*	0.042	0.023	0.042	0.023	*
UE	-0.018	0.011		-0.018	0.011	-0.018	0.011	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

In terms of the number of days/week the child goes to school, no impact has been identified as can be seen in Table 7. Some changes in preferences were significant, for instance, i) a four per cent increase in the proportion of treated children attending school more than 4 days/week is observed as a result of externality and ii) a two per cent decrease for the treated children net of the externality. The two effects cancel each other and do not stand out when the other components are considered.

5.2.2 HEALTH OUTCOMES

Three estimates of the Tekoporã effects on health outcomes are shown in Table 8 to 10: single difference estimates for the whole sample, heterogeneity taking into account social worker visits and heterogeneity taking into account the awareness of the need to comply with health conditionalities as a prerequisite of Tekoporã. The health outcomes analysed here are possession of vaccination cards, proportion of children with more than 70 per cent of updated vaccines, and number of visits to the health centres for child height and weight control.

Table 8 – Effect of Treatment on Treated for Vaccination Card Possession

	Single Difference			Heterogeneity over visits from social worker			Heterogeneity over vaccitation conditionality		
	Coef.	Std.		Coef.	Std.		Coef.	Std.	
ATT	0.058	0.049							
MIE	0.018	0.015							
IE	0.031	0.014	**						
SE	0.051	0.049							
UE	-0.025	0.020							
ATT – OC				0.059	0.050		0.073	0.053	
MIE				0.022	0.018		0.021	0.017	
IE				0.038	0.015	**	0.033	0.016	**
SE				0.057	0.049		0.054	0.048	
UE				-0.036	0.023		-0.014	0.031	
ATT – Transf.				0.053	0.093		0.028	0.065	
MIE				-0.002	0.015		0.013	0.016	
IE				-0.011	0.016		0.027	0.017	
SE				0.015	0.054		0.046	0.049	
UE				0.048	0.074		-0.045	0.047	
APT	0.034	0.063		0.035	0.064		0.049	0.065	
MIE	0.013	0.015		0.016	0.016		0.016	0.016	
IE	0.014	0.014		0.020	0.015		0.016	0.016	
SE	0.050	0.071		0.055	0.070		0.052	0.073	
UE	-0.030	0.032		-0.041	0.036		-0.020	0.043	
AET	0.024	0.066		0.024	0.066		0.024	0.067	
MIE	0.005	0.015		0.005	0.014		0.005	0.014	
IE	0.017	0.017		0.018	0.015		0.017	0.018	
SE	0.002	0.072		0.002	0.070		0.002	0.074	
UE	0.005	0.034		0.005	0.034		0.005	0.036	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

There is no effect over the possession of vaccination card. The only component that is significant is the income effect but it does not stand out when the other components are considered. The same goes for the proportion of children with more than 70 per cent updated vaccines as shown in table 9 below.

Table 9 – Effect of Treatment on Treated for Children with more than 70 per cent updated vaccines

	Single Difference			Heterogeneity over visits from social worker			Heterogeneity over vaccination conditionality		
	Coef.	Std.		Coef.	Std.		Coef.	Std.	
ATT	0.052	0.048							
MIE	0.008	0.011							
IE	0.022	0.012	*						
SE	0.027	0.048							
UE	0.004	0.024							
ATT – OC				0.054	0.054		0.060	0.053	
MIE				0.009	0.014		0.010	0.015	
IE				0.025	0.014	*	0.024	0.014	*
SE				0.027	0.052		0.024	0.051	
UE				0.001	0.029		0.012	0.032	
ATT – Transf.				0.040	0.094		0.036	0.063	
MIE				0.007	0.013		0.005	0.013	
IE				-0.002	0.013		0.018	0.013	
SE				0.024	0.057		0.033	0.052	
UE				0.019	0.079		-0.014	0.049	
APT	0.047	0.076		0.048	0.072		0.054	0.072	
MIE	0.010	0.012		0.010	0.014		0.011	0.014	
IE	0.021	0.013		0.024	0.014	*	0.023	0.013	*
SE	0.072	0.083		0.073	0.078		0.069	0.077	
UE	-0.046	0.037		-0.049	0.039		-0.038	0.040	
AET	0.006	0.081		0.006	0.077		0.006	0.073	
MIE	-0.001	0.012		-0.001	0.014		-0.001	0.013	
IE	0.001	0.015		0.001	0.016		0.001	0.015	
SE	-0.046	0.087		-0.046	0.085		-0.046	0.079	
UE	0.050	0.037		0.050	0.042		0.050	0.038	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

As for the visits to the health centres for child height and weight control the estimates varies according to the number of visits being considered. Estimates for at least three visit are shown in Table 10.

Table 10 – Effect of Treatment on Treated for Children with at least 3 visits to the health centres for height and weight control

	Single Difference			Heterogeneity over visits from social worker			Heterogeneity over visits to child height and weight control conditionality		
	Coef.	Std.		Coef.	Std.		Coef.	Std.	
ATT	0.163	0.053	***						
MIE	0.011	0.015							
IE	0.015	0.011							
SE	0.148	0.055	***						
UE	0.000	0.021							
ATT – OC				0.153	0.055	***	0.164	0.052	***
MIE				0.016	0.017		0.016	0.017	
IE				0.017	0.012		0.018	0.012	
SE				0.150	0.052	***	0.153	0.051	***
UE				-0.014	0.025		-0.007	0.031	
ATT – Transf.				0.226	0.090	**	0.158	0.071	**
MIE				-0.018	0.020		0.001	0.016	
IE				-0.003	0.014		0.007	0.011	
SE				0.140	0.056	**	0.138	0.052	***
UE				0.089	0.080		0.013	0.048	
APT	0.176	0.072	**	0.167	0.072	**	0.178	0.077	**
MIE	0.017	0.014		0.021	0.015		0.021	0.015	
IE	0.014	0.017		0.017	0.016		0.018	0.015	
SE	0.195	0.074	***	0.197	0.078	**	0.200	0.076	***
UE	-0.034	0.036		-0.047	0.037		-0.040	0.038	
AET	-0.014	0.079		-0.014	0.079		-0.014	0.079	
MIE	-0.005	0.017		-0.005	0.017		-0.005	0.016	
IE	0.000	0.017		0.000	0.017		0.000	0.015	
SE	-0.047	0.079		-0.047	0.084		-0.047	0.082	
UE	0.033	0.034		0.033	0.033		0.033	0.034	

Source: Own calculation based on the Evaluation Survey.

Note: Significant different from treated group at *10%, **5% and ***1%.

All three estimates show an increase of 15 to 17 per cent in the proportion of children visiting height and weight control more than 3 times. Most or all of this effect comes from the substitution effect component, indicating a change in preferences. The heterogeneous analysis of the social workers visits and knowledge about conditionality for health outcomes do not indicate any significant difference within the treated group. No externality has taken place for the visits to health centres for child height and weight control.

6. Conclusion

In this paper we evaluated the impact of Tekoporã Cash Transfer Programme taking into account interaction and behaviour changes over education and health outcomes among treated and non-treated groups in districts benefited by that programme. We followed the methodology proposed in Soares et al. (2009) and also found that SUTVA does not hold

within-district. However, our results differ from theirs in two ways: a) there are no externality effects either for education or health outcomes; and b) the main contributor for ATT significance is the substitution effect that captures behavioural changes. This latter result suggests that whereas relaxing the budget constraint alone is key to improve family consumption (Soares et al., 2009), the changes in preferences are key to improve the family's demand for health and education as proxied by school attendance and progression as well as visits to health centres. As with the inexistence of externality effects, these results are at odds with recent papers based on Progresa (Mexican CCT programme) that found positive impacts on education outcomes.

From the results the programme is accomplishing its objectives by improving child attendance to school and to health centres. Heterogeneity with respect to the knowledge about the need to comply with conditionalities as well as the visits of the social workers only shows a stronger impact for the education outcome, similar to the results reported by Schady and Araujo (2008) on the impact of school enrolment of the CCT programme in Ecuador. For health outcomes the impacts are very similar for both groups suggesting that the awareness of the conditionalities and the visit of the social workers did not lead to a differential impact among the treated households.

Given the costs that the social workers component represents for the programmes who adopt them and the findings mentioned above, it is advisable to have more research done on the contribution different components to have a clearer idea on what is essential to guarantee the positive impacts of the programme.

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