Externality and Behavioural Change Effects of a non-randomized CCT programme – Heterogeneous impact on the Demand for Health and Education*

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Abstract

This paper investigates the impact of the pilot phase of Paraguay's Conditional Cash Transfer Program, Tekoporã, on the demand for health and education, and how much of this impact was due to the cash transfers and/or due to behaviour/preferences changes possibly as an effect of other non-monetary components of the programme such as the conditionalities and the family support visits. Moreover, it explores the existence of externalities effects through the decomposition of the Average Treatment Effect on the Treated (ATT) into participation and externality effect. This decomposition was possible thanks to the existence of two distinct comparison groups, one within the village and possibly exposed to the externality and another in a different district not affected by the programme. 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 do not reach non-beneficiary families (no externality effect). At the pilot phase, with no conditionality enforcement in place, the role of conditionality and social worker visits is not yet clear. No differential effect was found for those who were aware of the conditionalities and/or were visited by social workers, although the message of the importance of education and health care have somehow did reach the households changing their preferences towards a greater consumption of health care and education services.

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

JEL Classification: C21, D12, D62, I38.

1. Introduction

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Cash transfers programmes have assumed an important role in the social protection schemes of many developing countries. In Latin America, some of the cash transfer programmes usually have a conditional component combined with monetary transfers which should affect not only family's income – short run effects associated to the poverty alleviation goal –, but also their preferences or behaviour – long run effects aiming to break the intergenerational cycle of poverty actually promoting the exit of poverty for future generations. These programmes are known as Conditional Cash Transfers programmes (CCT hereafter). The conditionalities usually comprise school attendance, health checkups and updating immunisation cards. Some programmes also add periodical visits of social worker visits and complementary programmes with the intention to broader the impact of the programme and to guarantee a more effective change.

There is some evidence of spillover effects on non-beneficiary families living in the same communities of those who take part in the programme. Such externalities can be caused by different reasons depending on the context and on the outcome of interest, such as learning processes fostered by social interactions, general equilibrium effects that influence local prices, transfers and loans among households and even a unexplored hypothesis that ineligible individuals might pretend to be treated to prove to be worthy of the transfer. Understanding the existence and nature 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).

This paper is part of a wider research agenda that aims to identify whether CCT programmes have externalities that affect both beneficiary and non-beneficiary families who live in areas where the programme is implemented. In addition, this paper puts forward a methodology to decompose the programme's effects into the effect of the monetary transfer and the effect of the other non-monetary components of the programme that aim to change the behaviour of the beneficiary family[1].

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 habits, etc. The conditionalities have not been monitored in the pilot phase although they had been extensively communicated to the beneficiaries, mainly through the visits of the social workers. The evaluation design allows for comparisons among beneficiary and non-beneficiary households within the areas where the program is under operation, hence exposed to externality; but also between areas with and without program there should not have any externality.

Externality is assessed through the decomposition of the average treatment effected on the treated (ATT) into participation (direct) effect and externality (indirect) effect by comparing beneficiaries and non-beneficiaries, the latter are divided into two groups: 1) those who were exposed to the programmes for living in districts where the programme was implemented, therefore potentially affected by externalities and 2) those who did not live in districts where the programme was implemented and could not be affected by them. Each of these effects is then further decomposed into the effect due to a looser budget constraint and the effect due to changes in family preferences. The first effect is measured by changes along the income

[1] Both methodologies were first presented in Soares et al. (2009).

expansion path (Engel curve) whereas the second effect is identified by shifts in the income expansion path.

In addition to the Soares et al. (2009) methodology, the heterogeneity of the impact with regard to the knowledge about the conditionalities and the number of visits of social workers is assessed in attempt to measure the influence of non-monetary components of CCT on the overall impact on outcomes of interest. Furthermore, an effort is made to use difference-in-differences methodology to control for unobservable factors that might bias the result taking advantage from the retrospective questions on education included in the survey.

In this paper, we use these methodologies to investigate the impact of Tekoporã on education (school attendance and progression) and health outcomes (number of visits to health centres), here also seen as items of a consumption bundle as there are implicit costs to the consumption of these items, such as transportation costs and other costs associated to service provision and time opportunity costs of adults and children. The objective is to assess whether the lower than socially desirable demand for health and education comes from a budget constraint or from a choice involving expected returns of education and health care.

Families may be unsure of the advantages of preventive health care in improving their well-being of their children, particularly, when the quality is low. The objective of the non-monetary components is to raise awareness of the importance of good nutrition, health care and hygiene habits for child development and overall well being. Healthier individuals are more productive and have more time available to work instead of being sick (Grossman, 1972). Thus the families may realize in time that investing in good health pays off.

In the same line, school attendance conditionality may change the perception of the low future returns of investments in education. Often parents with low education attainment do not value schooling as much as others. It is important to emphasise the idea that higher human capital may promote pathways out of poverty. By generating incentives for keeping children in school through cash transfers, the parents may notice the better opportunities of their children for a better standard of life. If they believe that schooling actually promotes higher wages for their children they may value more investments in education in face of the opportunity cost of current child labour income (Kruger et al. 2007).

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.

2. Conditional Cash Transfer Programmes: externality effects and the role of non-monetary components.

CCT 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 along their income expansion path. They also aim at increasing the demand for goods and services through conditionalities, particularly the demand for education and health, as a way to promote mobility out of poverty, changing the preferences out of the income expansion path toward a consumption bundle suggested by the government.

To guarantee the positive impact of the transfers, some programmes have worked with family support activities to help them comply with conditionalities, to communicate key messages of the programme and/or to link them with complementary programmes. This component of the programme is also supposed to promote changes in family behaviour in terms of consumption choices and human capital investment preferences. This information can promote changes beyond that promoted by the income transfer if the families believe this new behaviour is beneficial to them.

Externality[2]:

The population not participating in the CCT programme 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 idea that behaving like the eligible population would prove they are good candidates and increase their chances of participating in the programme.

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 the externality on consumption is only significant 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. Bobonis and Finan (2005) also show that the closer the child's household is of the eligibility cut-off point, the higher the peer effect. However, Lalive and Cattaneo (2009) highlight the fact that peer effects also affect eligible children, boosting the impact of the programme. Peer effects occur 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 are more than 75 per cent of treated villages, suggesting constraints in the supply of education.

Morris, Flores et al. (2004) find that households increase their demand for preventive care due to PRAF, CCT programme in Honduras. Gertler (2004) find significant impact of Progresa on health outcomes such as illness rate and anaemia. Behrman and Hoddinott (2005a) also evaluate the impact of PROGRESA but on children's nutritional status. They find important impacts on the height of children aged 12–36 months. Attanasio et al. (2005) analysed

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[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

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

Familias en Acción in terms of chronic undernourishment, diarrhoea symptoms and symptoms of respiratory disease, as well as health inputs such as children's intake of protein and vegetables and compliance with the Growth and Development (G&D) programme and with the DPT vaccination schedule and find significant improvements in all indicators.

Health impacts might also be spilt over neighbouring non-treated households. Such externality affects the outcome of non-beneficiary households through, for example, social interaction (learning from beneficiaries or emulating beneficiaries behaviour), better food consumption or 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.

Income and substitution effect:

Impact evaluations of CCT programmes in Latin America have showed positive results in several dimensions[4], however, the designs 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 evidence of the importance of conditionalities and of other non-monetary components of the programmes, e.g. social worker visits.

Evidence suggests that CCT programmes have had significant impacts on the Engel curve of beneficiary households. Specifically, these households have been encouraged by the programmes to change their behaviour in terms of consumption patterns. Such evidence may distinguish CCT programmes from other types of targeted cash transfers, whose benefit—according to Case and Deaton (1998) and Edmonds (2002)—is used like any other income by households.[5]

Hoddinott and Skoufias (2004), for instance, state that the income effect itself explains about 50 per cent of the total positive impact on consumption found in the PROGRESA evaluation. The remaining impact might be attributed to one of the conditionalities of the programme: attendance at talks on health issues (pláticas). They also estimate that 69 per cent of the increase in calories from vegetables is due to the *platicas*, while the remaining effect is due to the transfer itself.

Like Hoddinott and Skoufias in Mexico, Attanasio and Mesnard (2006), Maluccio and Flores (2005), Schady (2006) and Oliveira et al. (2007) show that CCT programmes have changed the consumption basket of households in Colombia, Nicaragua, Ecuador and Brazil, respectively. In Colombia and Nicaragua, Attanasio and Mesnard, and Maluccio and Flores, find that the food consumption of beneficiary households grew as much as their aggregate consumption, which may be more than the Engel curve predicts. In Ecuador and Brazil, Schady, and Oliveira et al., show that the programmes have affected the expenditure share of households, even though there is no significant impact on aggregate level of consumption.

By estimating similar models, Handa et al. (2009) seem to contradict the findings of Rubalcava et al. (2004). Handa et al. show that the PROGRESA benefit has no effect on education spending and makes no difference in terms of child clothing with respect to other

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

[5] Examples of effective results promoted by unconditional cash transfers are given by Duflo (2003), Agüero et al. (2007), León and Younger (2007), and Paxton and Schady (2007).

earnings. Hence they conclude that the PROGRESA transfer is treated as general income by the households and its effects would not differ from those of an unconditional transfer, while the opposite is stated by Rubacalva et al (2004). It is worth mentioning, however, that there are some critical differences between the models of Handa et al. and Rubalcava et al. The former authors use instrumental variables to predict both the per capita transfer and per capita income,[6] estimate the effects on expenditure levels, and adopt a linear model with constant elasticities. The latter authors are not concerned about any source of endogeneity, estimate the effects on expenditure shares, and adopt a non-linear model with a flexible spline. Regarding this latter point, Rubalcava et al. actually show that ignoring non-linearities in the income effect could lead to misleading results as there would be no distinction between movements along the Engel curve from shifts of the curve.

Heterogeneity of conditionality enforcement or knowledge:

Two other studies assessed heterogeneous effects for beneficiaries based on their knowledge about the programme's conditionalities or on actual conditionality monitoring. 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.

It is important to highlight possible negative effects of the increased demand for public services in facing supply side constraints, such as crowding-out effect. If the supply of public service is insufficient to provide for the increase in demand generated by conditionalities and information diffusion, the priority access of beneficiary individuals would imply reduction in non-beneficiary access.

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

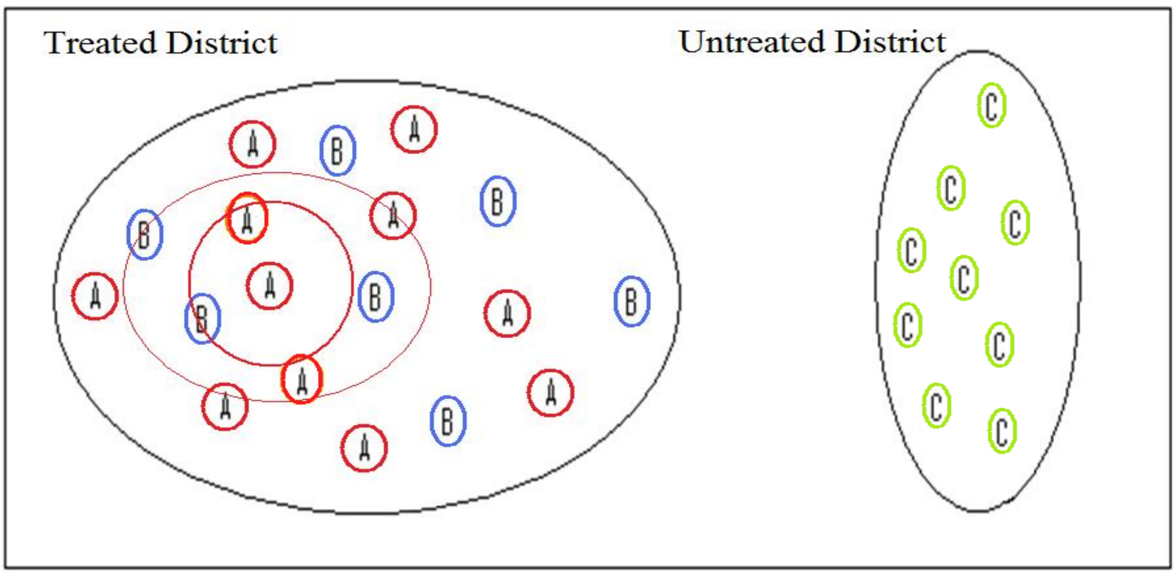
The absence of spillover effects on the comparison group is a key assumption of the programme evaluation literature in order to identify a casual effect with a specific policy intervention. The estimation of the average treatment effect is only possible under the assumption known as Single Unit Treatment Value Assumption - SUTVA which states that treatment does not directly or indirectly affect the comparison group (Rubin, 1980). 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)

[6] Since Handa et al. (2009) do not present the result obtained without using instrumental variables, the effect of such variables on the estimation is unknown.

It is a feasible assumption that spillover effects affect the population who live in the same communities or districts, but do 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.

Thus the externality effect is basically handled with a multiple-treatment approach, like those proposed by Sobel (2006), Imbens (2000), Lechner (2001), and Hudgens and Halloran (2008).[7] 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 (A), non-treated from treated districts (B) and non-treated from non-treated districts (C).

Figure 1 – Sample design



The program affects A directly through the monetary transfers and conditionality compliance. Similarly, group B can also indirectly experience income and behaviour changes due to spillover effects. As depicted in figure 1, externality reaches both A and B within treated community – treated families may learn from one another and price changes and increased liquidity affect all. The assumption that A and B react similarly to externality was adopted. It is a strong assumption, but necessary for using B to represent the externality effect on A.[8]

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:

$$\tau = E[Y_i(T_i = 1) - Y_i(T_i = 0) \mid T_i = 1], \tag{3.1}$$

[7] See Flores and Mitnik (2009) for a discussion on the consistency of these multiple treatment estimators.
[8] It does not hold for example in the presence of crowding out effects in which the effect on B would have the opposite direction of A. In face of crowding out the externality and participation estimates would be biased.

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:

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

That is, the participation effect taking away the externality effect. This is not observed, and is estimated by

$$\tau_p = E\left[Y_i\left(D_i = 1, T_i = 1\right) - Y_i\left(D_i = 1, T_i = 0\right) | T_i = 1\right] \quad (3.3)$$

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

$$\tau_e = E\left[Y_i\left(D_i = 1, T_i = 0\right) - Y_i\left(D_i = 0, T_i = 0\right) | T_i = 1\right]. \quad (3.4)$$

We can represent the outcome by the following linear function:

$$Y_i\left(D_i, T_i\right) = \alpha + \tau_p T_i + \tau_e D_i + \varepsilon_i \quad (3.5)$$

This functional form assumes that there is no specific effect stemming from the interaction between participation and externality. That is, the externality effect is equal in both cases when the household is treated and when it is untreated; likewise, the participation effect is the same regardless of the existence of externalities. This assumption facilitates the decomposition of the ATT effect, because it implies that

$$Y_i\left(D_i = 1, T_i = 1\right) = Y_i\left(D_i = 0, T_i = 1\right) + Y_i\left(D_i = 1, T_i = 0\right) - Y_i\left(D_i = 0, T_i = 0\right). \quad (3.6)$$

Then, we may rewrite the ATT effect as the sum of both effects (3.3) and (3.4):

$$\tau = \tau_e + \tau_p = E\left[Y_i\left(D_i = 1, T_i = 1\right) - Y_i\left(D_i = 0, T_i = 0\right) | T_i = 1\right]. \quad (3.7)$$

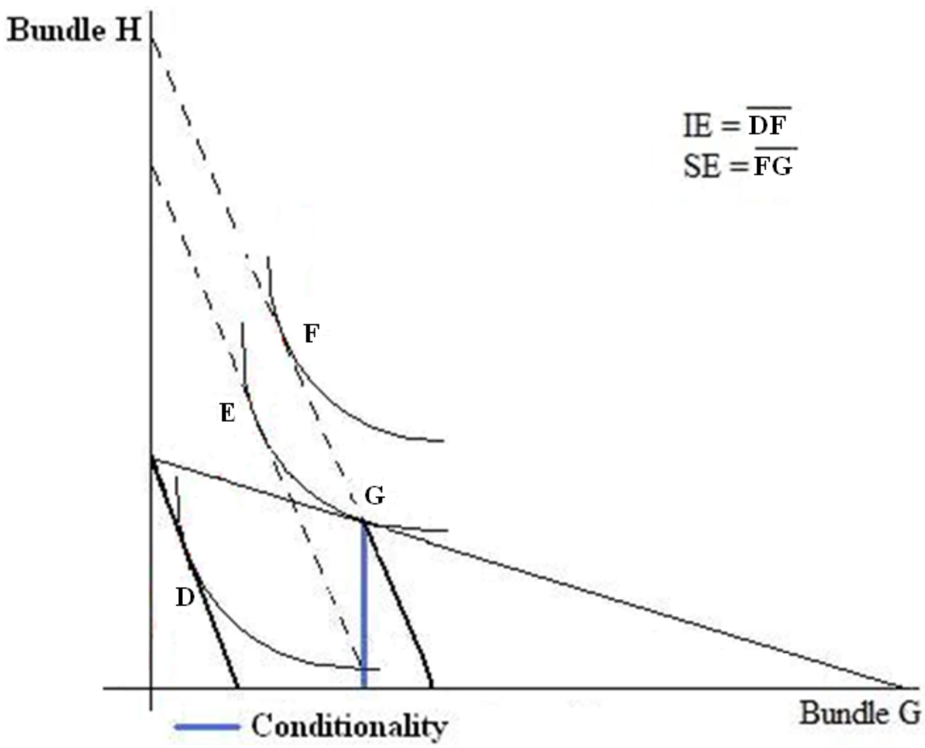
3.2. ASSESSING INCOME AND BEHAVIOURAL CHANGE EFFECT

Besides looking at externalities, this paper also applies a methodology to disentangle the effects of monetary transfers and other non-monetary components of the programme (e.g. conditionalities and social worker visits) on treated households.

As can be observed in figure 2 below, the transfer leads to an income effect by displacing the lower indifference curve from point D to point F that allows an increase in the consumption basket according to their new preferences correspondent to the more flexible budget constraint.

The conditionality in turn forces the families to change their preferences from point F to point G that is a less optimal option in face of the new budget constraint (lower indifference curve that passes through point E). Note that F is not feasible because, in order to receive the transfer, the family must consume a minimum amount of health and education, i. e. consume in the right side of the blue line representing the conditionality. The movement from D to F follows the original income expansion path of the families, while the transition from F to G indicates the change in consumption preferences – increased demand for health and education – desired by the government.

Figure 2 – Indifference Curves and Consumption Choices with Conditional Transfer



A decomposition of each of the ATT, APT and AET effects into income and behaviour components is proposed, using a methodology analogous to those presented by Juhn, Murphy and Pierce (1993) and Firpo, Fortin and Lemieux (2007). The decomposition of the average treatment effect is based on a semi-parametric approach, as in DiNardo et al. (1996), which avoids biases caused by linearity assumptions. In particular, and in contrast to Hoddinott and Skoufias (2004), Rubalcava et al. (2004), Gitter and Barham (2007), and Handa et al. (2009), the income expansion path is estimated nonparametrically and it guarantees that the identified behavioural-change effect does not come from the change in household income along the income expansion path (non-homothetic preferences).

First, 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:

$$Y_{i,D,T} = g_{D,T}\left(W_{i,D,T}, u_{i,D,T}\right), \tag{3.8}$$

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

If we define $\bar{g}(.,.)$ as a counterfactual function of the average income expansion path (Engel Curve) representing what would the effect be if only the income changed; $g_{D,I}(.,\ .)$ is the observed income expansion path function for each group following their respective mean income elasticity (consumption preferences); and $\bar{F}(.)$ is the counterfactual cumulative distribution for residuals. We can rewrite the equation (3.8) as:

$$Y_{i,D,T} = Y_{i,D,T}^{W} + Y_{i,D,T}^{g} + Y_{i,D,T}^{u}, \tag{3.9}$$

where

$$Y_{i,D,T}^{W} = \bar{g}\left(W_{i,D,T}, \bar{F}^{-1}\left(\theta_{i,D,T} \mid W_{i,D,T}\right)\right), \tag{3.10}$$

$$Y_{i,D,T}^{g} = g_{D,T}\left(W_{i,D,T}, \bar{F}^{-1}\left(\theta_{i,D,T} \mid W_{i,D,T}\right)\right) - \bar{g}\left(W_{i,D,T}, \bar{F}^{-1}\left(\theta_{i,D,T} \mid W_{i,D,T}\right)\right), \text{ and} \tag{3.11}$$

$$Y_{i,D,T}^{u} = g_{D,T}\left(W_{i,D,T}, F_{D,T}^{-1}\left(\theta_{i,D,T} \mid W_{i,D,T}\right)\right) - g_{D,T}\left(W_{i,D,T}, \bar{F}^{-1}\left(\theta_{i,D,T} \mid W_{i,D,T}\right)\right). \tag{3.12}$$

Through equation (3.10) we estimate a counterfactual in which outcomes are the result from income variation exclusively. Equation (3.11), in turn, provides outcomes estimated through group specific income elasticity. Finally, Equation (3.12) assesses the observed residual differences.

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

$$\tau = E\left[\left(Y_{i,1,1}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \tag{3.13}$$

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:

$$\tau_p = E\left[\left(Y_{i,1,1}^{W} - Y_{i,1,0}^{W}\right) + \left(Y_{i,1,1}^{g} - Y_{i,1,0}^{g}\right) + \left(Y_{i,1,1}^{u} - Y_{i,1,0}^{u}\right) \mid T_i = 1\right], \text{ and} \tag{3.14}$$

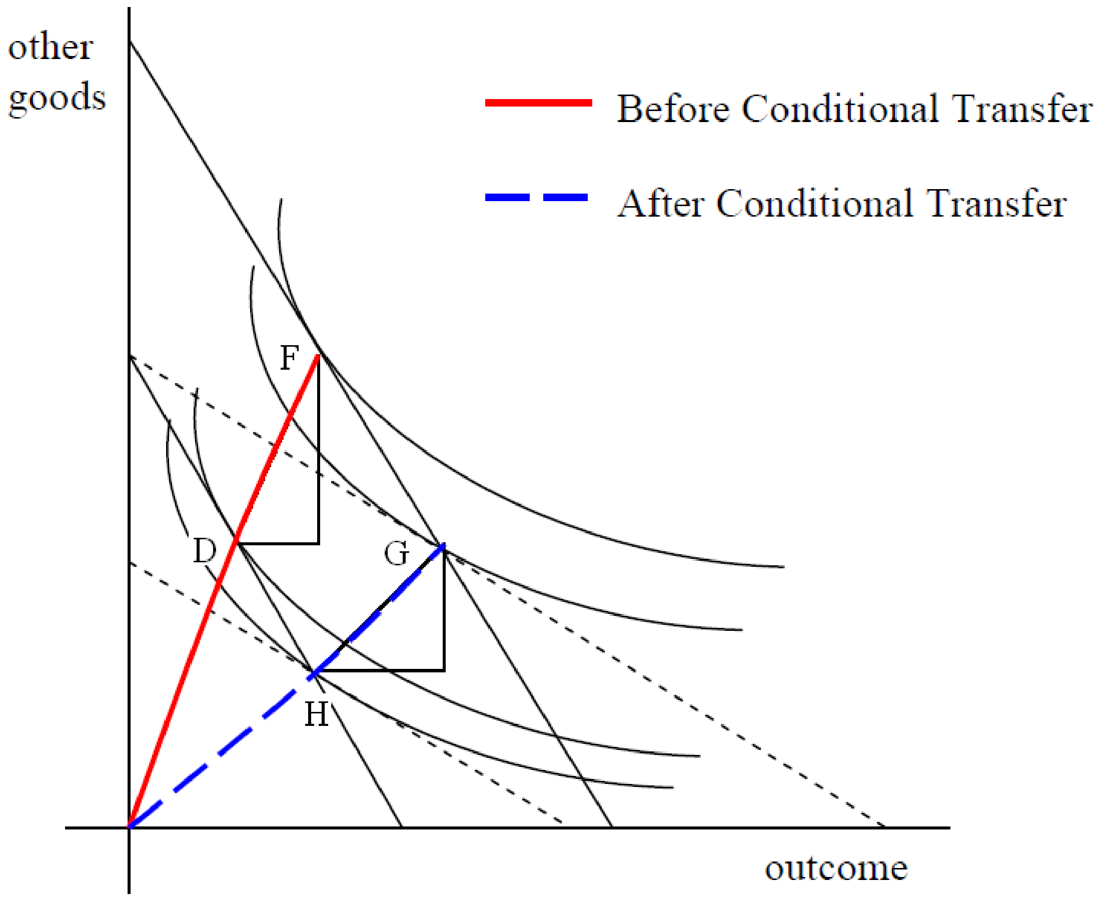
$$\tau_e = E\left[\left(Y_{i,1,0}^{W} - Y_{i,0,0}^{W}\right) + \left(Y_{i,1,0}^{g} - Y_{i,0,0}^{g}\right) + \left(Y_{i,1,0}^{u} - Y_{i,0,0}^{u}\right) \mid T_i = 1\right]. \tag{3.15}$$

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:

$$Y_{i,D,T}^{MW} = \bar{g}\left(W_{i,D=0,T=0}, \bar{F}^{-1}\left(\theta_{i,D=0,T=0} \mid W_{i,D=0,T=0}\right)\right) \tag{3.16}$$

If instead of the cash transfer the government subsidised health and education, for instance, by giving to the individuals some money each time they went to school or to the child development visit, the change in the income expansion path would occur gradually for all eligible households. The graph would look like Figure 3 below. Bear in mind that the Engel curve is not necessarily linear. The subsidy would alter the price relation and change the income elasticity of those goods – our chosen variable for preferences. This is just an exercise to better visualise the effect although it is not exactly what happens because the cash transfer as it occurs depicts an abrupt change in the Engel Curve.

Figure 3 – Income Expansion Paths before and After the Conditional Transfer



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

Causal Identification

For the ATT estimation, it is ideal to compare the same treated households with and without the treatment at a given point in time. As it is not possible to observe the same household in both states the alternative is to work with contrafactuals. 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, and the control group would be a natural contrafactual in the presence of treatment.

Although Tekoporã was not randomly assigned[9], 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 – ICV

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

components. 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:

$$T_i \perp \left(Y_i(T_i = 0), Y_i(T_i = 1)\right) \mid X_i, D_i . \qquad (3.17)$$

It means that treatment assignment is independent of the potential outcomes conditional on the pre-treatment variables that determine the treatment assignment, X_i, and on every district, D_i.

Similarly, we should also assume that, the average outcomes conditioned on X_i of control groups from both treated and control districts would be the same in the absence of treatment. For this to happen it is necessary to include in X_i the available observable environmental characteristics that differentiates treated and control districts – if households are from rural or urban area, if the area is flooded, insanitary or hard to access. Formally, we assume that the distinction between comparison groups of different districts and the potential outcomes conditional on X_i are orthogonal:

$$D_i \perp \left(Y_i(D_i = 0), Y_i(D_i = 1)\right) \mid X_i, T_i = 0 . \qquad (3.18)$$

Estimating the decomposition of ATT into participation and externality effects

Given the groups surveyed, one actually observes as average outcome:

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

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

$$\hat{\tau}_p = E[Y_i \mid X_i, D_i = 1, T_i = 1] - E[Y_i \mid X_i, D_i = 1, T_i = 0], \qquad (3.19)$$

$$\hat{\tau}_e = E[Y_i \mid X_i, D_i = 1, T_i = 0] - E[Y_i \mid X_i, D_i = 0, T_i = 0], \text{ and} \qquad (3.20)$$

$$\hat{\tau} = E[Y_i \mid X_i, D_i = 1, T_i = 1] - E[Y_i \mid X_i, D_i = 0, T_i = 0]. \qquad (3.21)$$

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

$$Y_i = \alpha_0 + \tau_c \cdot T_i + \alpha_1' X_i + \varepsilon_i \qquad (3.22)$$

$$\begin{aligned} \hat{\tau}_c = \; & E[Y_i \mid X_i, D_i = 1, T_i = 1] \\ & - P[D_i = 1 \mid X_i, T_i = 0] \cdot E[Y_i \mid X_i, D_i = 1, T_i = 0] \\ & - P[D_i = 0 \mid X_i, T_i = 0] \cdot E[Y_i \mid X_i, D_i = 0, T_i = 0], \end{aligned} \qquad (3.23)$$

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.

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) that correctly identifies participation in the absence of externality, τ_p, externality, τ_e, and ATT, τ:

$$Y_i = \alpha_0 + \tau_p \cdot T_i + \tau_e \cdot D_i + \alpha_1' X_i + \varepsilon_i \quad (3.24)$$

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

Difference in Differences

The follow up evaluation survey of Tekoporã included a retrospective question on education so it was possible to use the difference in differences (DD) methodology in addition to the cross section single difference approach described so far[10].

The use of DD helps to control for baseline unobservable differences that are constant over time 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:

$$Y_{ij} = \alpha_0 + \alpha_t \cdot S_{ij} + \alpha_{ba} \cdot T_{ij} + \alpha_{bb} \cdot D_{ij} + \tau_p \cdot T_{ij} \cdot S_{ij} + \tau_e \cdot D_{ij} \cdot S_{ij} + \alpha_1' X_{ij} + \varepsilon_{ij} \text{ , where:} \quad (3.26)$$

$$\tau = \tau_p + \tau_e \quad (3.27)$$

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:

$$Y_i = \alpha_0 + \tau_{p'} \cdot T_i + \tau_{p''} \cdot K_i + \tau_e \cdot D_i + \alpha_1' X_i + \varepsilon_i \quad (3.28)$$

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:

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

And ATT is given by:

$$\tau = \tau_{p'} + \tau_{p''} + \tau_e \quad (3.30)$$

Income and Behaviour Effects Decomposition

[10] See Woodridge and Imbens (2008) and Bertrand, Duflo and Mullainathan (2003)

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 and ensures the possible non-linearity of the Engel Curves. This estimation is made for (i) the household income of whole sample as shown in equation (3.10), (ii) the household income of treated, comparison group within and between districts separately and then pooled as in equation (3.11), and (iii) the household income of comparison group of untreated districts as in (3.12). The predicted results are used as dependent variables in equation (3.24).

The non-parametric estimation of the outcome using the household income of the whole sample (i) is $Y_{i,D,T}^{W}$, which allows for the estimation of the income effect component for an average Engel Curve in ATT, APT and AET. When (ii) is used, the estimation provides $Y_{i,D,T}^{g}$ which allows for the estimation of the substitution effect component, the differences in the outcome due to different Engel Curves. Following (3.9), the unobserved effect is calculated by the residual difference between $Y_{i,D,T}^{W}$ and $Y_{i,D,T}^{g}$, resulting in $Y_{i,D,T}^{u}$. When (iii) is used, the non-parametrical estimation provides $Y_{i,D,T}^{MW}$ enabling to assess the marginal income effect associated to ATT, APT and AET.

Causal Identification: Applying the Propensity Score Matching

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 (3.17), 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:

$$E[x_i \mid p(X_i), T_i = 1] = E[x_i \mid p(X_i), D_i, T_i = 0] \quad \forall x_i \in X_i \tag{3.31}$$

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.

Assumption (3.18) 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.

As Robins and Rotnizky (1995) point out, if either the model of conditional means or the model based on the propensity score are correctly specified, the resulting estimator will be consistent. For this reason, Hirano and Imbens (2001) propose a flexible approach combining both models. Hirano and Imbens' estimator is based on weighted least square estimation of the regression functions (3.24), (3.26) and (3.28) above, where the control variables in the right hand side are a subset of X_i. The estimated weight, applied in these regressions, is given by:

$$\hat{\omega}(T_i, D_i, Z_i) = T_i + \frac{\hat{p}(Z_i)\cdot(1-T_i)\cdot D_i}{\hat{e}(Z_i)} + \frac{\hat{p}(Z_i)\cdot(1-D_i)}{1-\hat{p}(Z_i)-\hat{e}(Z_i)}, \qquad (3.32)$$

where Z_i is a subset of balanced variables of X_i.[11]

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 covered five districts of two departments: Buena Vista and Abaí in the Department of Caazapá, and Santa Rosa del Aguaray, Lima and Unión in the Department of San Pedro. 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*).

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:

[11] Wooldridge (2002; 2007) demonstrate the properties of this estimator.

1) Presence of children under 15 years of age or pregnant women;

2) To live in the priority areas of the program, namely, the poorest districts in the country according to the Index of Geographical Prioritization (IPG);

3) ICV score below 40 points.[12]

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. Surveyed households of districts that did not take part in the pilot formed the between district comparison group. 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.

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 conditions provided to the *Ficha Hogar*. In total, 7,990 households were screened by the census and 1,827 by demand. Those potentially eligible households that did not live in the poorest areas of the districts of the pilot and others that were overlooked formed the within district comparison group[13].

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[14], 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).

About 1,093 households were surveyed. Among those 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

[12] Initially the program intended to target only households with an ICV below 25 points, but 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.

[13] Selection on unobservables might have taken place due to these two events: self-selection as a result of the demand process and overlooked households. It would be ideal to use instrumental variables to correct for the consequent potential bias, however, the dataset does not provide any good candidate to be an IV.

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

excluded from the dataset, as well as those households registered with an incomplete interview.[15]

The within district comparison group is comprised of both ineligible households (ICV higher than 40) and eligible households that were 'overlooked' by the program. They represent 60 percent of the within district comparison group. The within group observation have better living standard indicators since a smaller share of the between district comparison group, 9 percent, has ICV over 40. Furthermore, while 74 per cent of treated households have ICV below 25 (the original target), the share is of 61 per cent in between district comparison group and only 16 per cent in within district comparison group. It is likely that households were not randomly overlooked. One possible reason for this administrative mistake refers to the change in the cut-off point of the eligibility criteria. As the cut-off point was raised from 25 to 40 when the registration process 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.

5. Empirical Results

5.1 DESCRIPTIVE STATISTICS

This subsection presents some descriptive statistics of the sample and allows for an initial assessment of the differences between treated and comparison groups. Means for individual and family socio-demographic characteristics are described in Table 1. Table 2 brings some evidence on conditionality knowledge and on 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 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. They have higher ICV and average household per capita income due to the reasons stated above.

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

	Total	Treated	Comparison groups from: within	between	Difference in means significance		
	(A+B+C)	group (A)	district (B)	district (C)	(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			

[15] 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).

	(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 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, although conditionalities were not enforce during the pilot phase. In fact, 92% of the families claims to know about the conditionalities, but a smaller share know about each one of them.

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

	Average
At least 1 visit per month from *social workers*	86%
Aware of programme conditionalities	92%
Aware of school attendance conditionality	83%
Aware of visits to child height and weight control conditionality	67%
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	Treated	within	between			
	(A+B+C)	group (A)	district (B)	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, than the between district comparison group; as well as higher percentage of updated vaccines, 51 per cent, when compared to the within- and between-district comparison groups. As for visits to the health centers, the treated group has 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	Treated	Comparison groups: within	between	Difference in means significance		
	(A+B+C)	group (A)	district (B)	district (C)	(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.

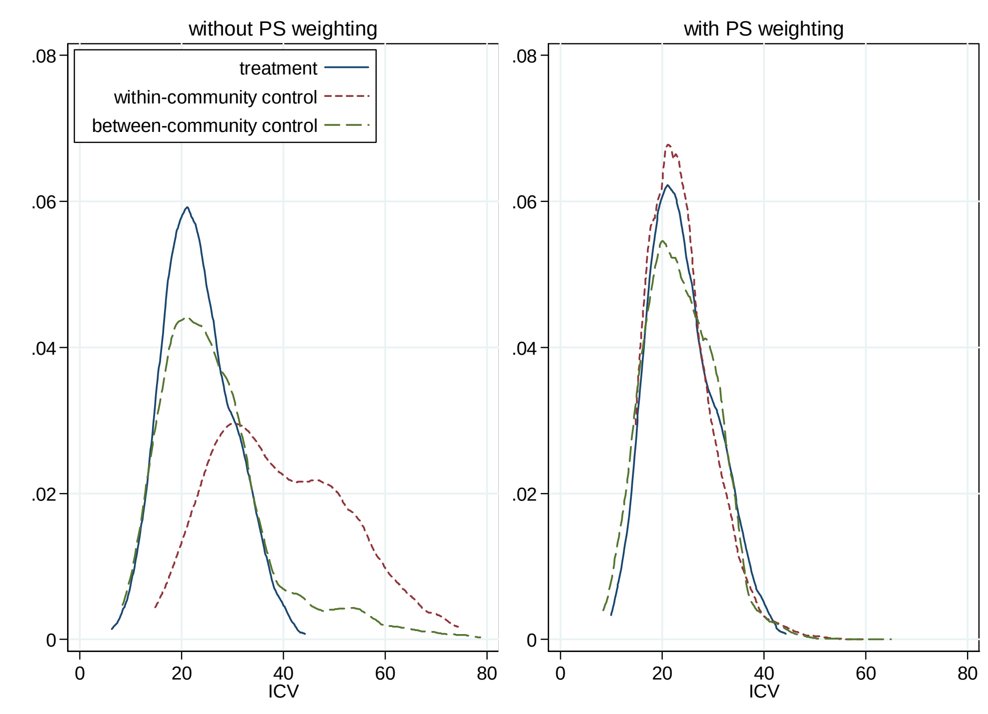
Note: Significant different from treated group at *10%, **5% and ***1%.

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 the propensity score calculated using baseline information. The propensity score manage to balance the distribution of the household and ICV components 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). [16]

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

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



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. The decomposition into marginal income effect, MIE, income effect, IE, substitution effect, SE, and unexplained effect, UE, were obtained following equations 3.16, 3.10, 3.11 and 3.12, respectively. When considering heterogeneity, equation 3.28 was used; and when difference in differences methodology was applied, equation 3.26 was used.

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 progression for 7 to 15 year-old children.

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 (variable used in the ICV calculation), 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.

Since there is very limited information on school indicators in the 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.

5.2.1 EDUCATIONAL OUTCOMES

Estimates of the Tekoporã effect on school attendance are shown in Table 5 and 6: difference in differences estimates, single difference estimates, heterogeneity taking into account social worker visits and heterogeneity taking into account the knowledge about the existence of conditionalities and their different components.

The ATT estimate given by the difference in differences methodology is not significant. But according to the single difference estimates, the program increased the proportion of children who attended school in 2006 by seven per cent (ATT). It is possible that unobservable variables are generating a biased result in single difference estimate, but it is also likely that the retrospective information about attendance and progression was not very reliable. In any case, the substitution effect component was significant and robust across methods indicating an increase of around five per cent in school attendance. Thus, at least for the single difference, the overall impact is positive and mostly due to substitution effect. This implies that change in the preferences, and a looser budgetary constraint, are the main determinants of the result.

Table 5 – Effect of Treatment on Treated for School Attendance

Impact decomposition over school attendance

	Difference in Differences			Single Difference		
	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	*
APT	0.019	0.074		0.050	0.039	
MIE	-0.002	0.005		0.001	0.011	
IE	-0.002	0.005		-0.001	0.004	
SE	0.065	0.056		0.072	0.060	
UE	-0.045	0.055		-0.021	0.029	
AET	0.014	0.079		0.019	0.041	
MIE	-0.003	0.003		0.000	0.007	
IE	-0.003	0.004		-0.002	0.005	
SE	-0.008	0.057		-0.018	0.061	
UE	0.025	0.058		0.039	0.034	

Source: Own calculation based on the Evaluation

Survey.

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

The AET estimate does not indicate any evidence of potential externality on school attendance. So it seems that the change in behaviour only affects treated households. The heterogeneity analysis in Table 6 shows no differential effect for social worker visits or for conditionality awareness. This suggests that the message about conditionalities was passed along somehow understood by treated households. However, the message has not been restricted to those visited by social workers or to those who were aware of the need to comply with the school attendance conditionality.

Table 6 – Heterogeneous Effect of Non-Monetary Components on School Attendance

	Single difference			Social Worker		Conditionality	
	Coef.	Std. Err.		Coef.	Std. Err.	Coef.	Std. Err.
ATT	0.069	0.021	***	0.025	0.027	0.040	0.047
MIE	0.001	0.008		-0.009	0.014	0.003	0.018
IE	-0.003	0.005		-0.007	0.008	-0.007	0.012
SE	0.054	0.021	**	-0.007	0.008	-0.003	0.009
UE	0.018	0.010	*	0.039	0.026	0.049	0.043

Source: Own calculation based on the Evaluation Survey.

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

The impacts on school progression are in line with those on school attendance described above (tables are in annex A.1 and A.2). Difference in differences estimate shows that the overall ATT effect is again not significant but the substitution effect indicates a four per cent increase in progression. The ATT estimates provided by single difference are significant and slightly higher, indicating an increase of about seven per cent in school progression. This result is also entirely due to the substitution effect. However, the heterogeneity analysis did not show any evidence of a differential effect of social worker visits and conditionality awareness to the substitution effect. Again, there is no evidence of externality.

5.2.2 HEALTH OUTCOMES

With regard 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) was able to show the vaccination card and c) child has 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. No significant result was found for vaccination variables so results are not displayed here. As for visits to health centres, 3 visits or more per year was the variable of choice due to significance and robustness of the results.

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.

Estimates for at least three visits to the health centres for child height and weight control are shown in Table 7. As data did not allow for the use of difference in difference methodology, only single difference is shown below. Results show a significant impact of the visit to a health centre to monitor the child development of 16 per cent. About 14 per cent alone was due to substitution effect and the remaining 1.5 per cent due to income effect. Once again it does not seem that a looser budgetary constraint is driven the results, which seem to be triggered by changes in preferences.

Table 7 – Effect of Treatment on Treated on Number of Visits to Child Height and Weight Control (at least 3 in the last 12 months)

	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	
APT	0.176	0.072	**
MIE	0.017	0.014	
IE	0.014	0.017	
SE	0.195	0.074	***
UE	-0.034	0.036	
AET	-0.014	0.079	
MIE	-0.005	0.017	
IE	0.000	0.017	
SE	-0.047	0.079	
UE	0.033	0.034	

Source: Own calculation based on the Evaluation Survey.

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

Even larger impacts were found for the treated community for the APT, i.e., in the absence of externality effects (AET). There is an increase of 18 and 19 per cent for the APT and its correspondent substitution effect (SE). Again the externality estimate was not significant. Similar to the educational estimation, the heterogeneity estimates do not suggest that conditionality awareness or social workers visit made a difference in terms of the substitution effect.

Table 8 – Heterogeneous Effect of Non-Monetary Components on Number of Visits to Child Height and Weight Control (at least 3 in the last 12 months)

	Single difference			Social Worker			Conditionality	
	Coef.	Std. Err.		Coef.	Std. Err.		Coef.	Std. Err.
ATT	0.163	0.053	***	-0.074	0.088		0.152	0.118
MIE	0.011	0.015		0.034	0.021	*	0.059	0.039
IE	0.015	0.011		0.020	0.017		0.039	0.027
SE	0.148	0.055	***	0.010	0.026		0.052	0.054
UE	0.000	0.021		-0.104	0.088		0.062	0.117

Source: Own calculation based on the Evaluation Survey.

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

6. Conclusion

In this paper we evaluated the impact of Tekoporã Cash Transfer Programme taking into account interaction and behavioural changes related to 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 added a heterogeneity analysis of the impact with regard to the awareness of the existence of conditionalities and the number of visits of social workers. This latter component is assessed in order to check the influence of non-monetary components of CCT on its impact. Furthermore, an effort is made to use difference-in-differences methodology to control for unobservable factors that might bias the result taking advantage from the retrospective question on education included in the survey.

However, our results differ from Soares et al. (2009) 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 fundamental to improve family consumption, the changes in preferences are necessary 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 existence of externality effects, the lack of externality in all indicators analysed are at odds with recent papers based on Progresa (Mexican CCT programme) - Bobonis and Finan (2005) and Lalive and Cattaneo (2009) - that found positive externalities on education outcomes.

In any case the ATT estimates suggest that the programme is achieving 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 do not add any extra impact on the education outcome, at odds to the results reported by Schady and Araujo (2008) on the impact of school enrolment of the CCT programme in

Ecuador, suggesting that at the pilot phase, with no conditionality enforcement in place, the role of conditionality awareness and social workers is not yet clear. In any case the message of the importance of education and health care seems to have reached the households and changed their preferences towards a greater consumption of health care and education.

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. The following step in the research agenda is to replicate this methodology in a randomized data, to apply difference-in-difference methodology in more variables or use regression discontinuity design in order to obtain better causal identification.

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8. Annex

Table A.1 – Effect of Treatment on Treated for Progression

	Difference in Differences			Single Difference		
	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	
APT	0.055	0.078		0.057	0.057	
MIE	0.002	0.007		0.019	0.016	
IE	-0.002	0.007		0.004	0.007	
SE	0.097	0.070		0.094	0.074	
UE	-0.040	0.054		-0.040	0.027	
AET	-0.005	0.082		0.019	0.058	
MIE	-0.005	0.005		0.003	0.016	
IE	-0.004	0.005		-0.005	0.009	
SE	-0.054	0.069		-0.023	0.075	
UE	0.053	0.060		0.046	0.031	

Source: Own calculation based on the Evaluation Survey.

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

Table A.2 – Heterogenic Effect of Non-Monetary Components on School Progression

	Single difference			Social Worker			Conditionality		
	Coef.	Std. Err.		Coef.	Std. Err.		Coef.	Std. Err.	
ATT	0.076	0.026	***	0.051	0.041		0.071	0.058	
MIE	0.022	0.016		0.017	0.023		0.066	0.036	*
IE	-0.001	0.009		-0.011	0.011		-0.004	0.021	
SE	0.070	0.027	***	-0.006	0.010		-0.003	0.012	
UE	0.006	0.013		0.068	0.039	*	0.078	0.058	

Source: Own calculation based on the Evaluation Survey.

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