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Externality and behavioural change effects of a non-randomised CCT programme: Heterogeneous impact on the demand for health and education

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EXTERNALITY AND BEHAVIOURAL CHANGE EFFECTS OF A NON-RANDOMISED CCT PROGRAMME:

HETEROGENEOUS IMPACT ON THE DEMAND FOR HEALTH AND EDUCATION

Working Paper number 82

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EXTERNALITY AND BEHAVIOURAL CHANGE EFFECTS OF A NON-RANDOMISED 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 programme, *Tekoporã*, on the demand for healthcare and education, and how much of this impact was due to the cash transfers and/or due to changes in behaviour/preferences, possibly as an effect of other, non-monetary programme components such as the conditionalities and family support visits. It also explores the presence of externalities effects through a decomposition of the average treatment effect on the treated (ATT) into participation and externality effect. This decomposition was possible thanks to the use 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 children's attendance at school and increasing visits to the health centres. They also suggest that the positive impacts do not reach non-beneficiary families (no externality effect). In 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 healthcare somehow did reach the households, altering their preferences towards a greater consumption of healthcare and education services.

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

JEL Classification: C21, D12, D62, I38.

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1 INTRODUCTION

Cash transfer programmes have assumed an important role in the social protection schemes of many developing countries. In Latin America, some of these programmes have a conditional component combined with monetary transfers that should affect not only families' income (short-run effects associated with the poverty alleviation goal) but also their preferences or behaviour (long-run effects that aim to break the intergenerational cycle of poverty, thereby promoting future generations' escape from poverty). These initiatives are known as conditional cash transfer programmes (CCTs). The conditionalities usually comprise school attendance, health checkups and the updating of immunisation cards. Some schemes also add periodic visits by social workers and complementary programmes with the aim of broadening the impact and guaranteeing more effective change.

There is some evidence of spillover effects on non-beneficiary families living in the same communities as the programme beneficiaries. There can be various reasons for such externalities, depending on the context and 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 an unexplored hypothesis that ineligible individuals might act as if included in the programme in order to prove themselves worthy of the transfer. Understanding the existence and nature of externalities is an important step towards reaching a better assessment of the results of standard impact evaluations, and in providing policymakers with better information on the adequacy of their CCT design (Handa et al., 2009).

This paper is part of a wider research agenda that seeks to identify whether CCT programmes have externalities that affect both beneficiary and non-beneficiary families living in areas where the programme is implemented. The paper also 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 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, which should be conditional on a minimum rate of school attendance, regular visits to health centres, periodic immunisation, and monthly visits by social workers to help families 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, and so on. The conditionalities have not been monitored in the pilot phase, although they had been extensively communicated to the beneficiaries, mainly through the social worker visits. The design of the evaluation allows for comparisons to be made between beneficiary and non-beneficiary households in the areas where the programme is operating, and hence where there is exposure to externality; but it also allows comparisons between areas with and without the programme, where there should not be an externality.

Externality is assessed by means of the decomposition of the average treatment effect on the treated (ATT) into participation (direct) effect and externality (indirect) effect, by comparing beneficiaries and two groups of non-beneficiaries: (i) those who were exposed to the programme because they lived in districts where it was implemented and thus potentially were affected by externalities; and (ii) those who did not live in districts where the programme was implemented and could not be affected by externalities. Participation and externality effects are then further decomposed into the effect caused by a looser budget constraint and

the effect caused by changes in family preferences. The first effect is measured by changes along the income expansion path (Engel curve), whereas the second effect is identified by shifts in the income expansion path.

In addition to the Ribas et al. (2010) methodology, the heterogeneity of the impact with regard to knowledge of the conditionalities and the number of social worker visits is assessed, in an attempt to measure the influence of the non-monetary components of the 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 of the retrospective questions on education included in the survey.

In this paper, we use these methodologies to investigate the impact of *Tekoporā* on education (school attendance and progression) and health outcomes (number of visits to health centres). These outcomes are here seen as items of a consumption bundle since there are implicit costs involved in the consumption of these items, such as transport costs and other costs associated with service provision, as well as time opportunity costs for adults and children. The aim is to assess whether the lower-than-socially-desirable demand for health and education comes from a budget constraint or from a choice involving the expected returns to education and healthcare investments.

Families may be unsure of the advantages of preventive healthcare in improving the well-being of their children, particularly when the quality of the service is poor. The objective of the non-monetary components is to raise awareness of the importance of good nutrition, healthcare 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 realise over time that investing in good health pays off.

Similarly, the school attendance conditionality may change the perception of the poor future returns on investments in education. Often, parents who have limited educational attainment do not value schooling as much as others. It is important to emphasise the idea that higher levels of human capital may promote pathways out of poverty. Since the cash transfers offer incentives to keep children in school, parents may notice that their children have a greater prospect of a better standard of living. If they believe that schooling actually promotes higher wages for their children, they may value more investments in education in the face of the opportunity cost of foregoing current income from child labour (Kruger et al., 2007).

This paper has 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 the paper and the empirical strategy. The fourth section contains the main characteristics of *Tekoporā* and the data description. The fifth section offers descriptive analysis and the empirical results. The final section provides the conclusions.

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 the families and, consequently, their optimum choices along their income

expansion path. The programmes also seek to increase the demand for goods and services by means of conditionalities, particularly the demand for education and healthcare, as a way of promoting mobility out of poverty, changing the preferences of the income expansion path toward a consumption bundle suggested by the government.

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

2.1 EXTERNALITY²

The population not participating in the CCT programme but living in “treated” districts (that is, where the programme is operating) may experience indirect programme effects because of: (i) direct transfers from treated to non-treated households or the development of a credit market; (ii) an increase in overall income or prices (Angelucci and De Giorgi, 2009); (iii) learning from peer interaction (Bobonis and Finan, 2005; Lalive and Cattaneo, 2009; Bobba, 2008); and (iv) the idea that if they behave like the eligible population this would prove that 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 in ineligible households. They argue that the programme increased food consumption as a result of 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 Mexico’s *Progresa* CCT programme has also had positive externality effects on school enrolment and attendance among 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 to 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 rates, attained years of education and child labour. Despite a positive spillover on the non-treated in less treatment-dense municipalities, there is a crowding-out effect in those municipalities where more than 75 per cent of villages are treated, suggesting constraints in the supply of education.

Morris, Flores et al. (2004) find that households increase their demand for preventive care due to the *Programa de Asignación Familiar* (PRAF), a CCT programme in Honduras. Gertler (2004) finds that *Progresa* had a significant impact on health outcomes such as rates of illness and anaemia. Behrman and Hoddinott (2005) 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 Colombia's *Familias en Acción* in terms of chronic undernourishment, symptoms of diarrhoea 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. They find significant improvements in all indicators.

Health impacts might also be spilt over neighbouring, non-treated households. That externality has an effect on 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 among both treated and untreated children in Kenya.

2.2 INCOME AND SUBSTITUTION EFFECT

Impact evaluations of CCT programmes in Latin America have shown positive results in several dimensions.⁴ Because of the way in which these evaluations are designed, however (even experimental assessments such as those undertaken for *Progresa* in Mexico and the *Red de Protección Social* (RPS) in Nicaragua), one cannot disentangle in any simple way what can be attributed to the effect of the transfer itself and what is due to behavioural changes linked to the conditionalities, as well as to other programme components. There has been some evidence of the importance of conditionalities and of other, non-monetary components of the programmes, such as visits by social workers.

Evidence suggests that CCT programmes have had significant impacts on the Engel curve of beneficiary households. Specifically, the programmes have encouraged these households 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 *pláticas*, 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 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 Rubalcava 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, since there would be no distinction between movements along the Engel curve from shifts of the curve.

2.3 HETEROGENEITY OF CONDITIONALITY ENFORCEMENT OR KNOWLEDGE

Two other studies have assessed heterogeneous effects for beneficiaries on the basis of their knowledge of the programme's conditionalities or of actual conditionality monitoring. Schady and Araújo (2008) show that the positive effect on school enrolment of the *Bono de Desarrollo Humano* in Ecuador was confined to those beneficiaries who believed there were conditionalities attached to the programme. De Brauw and Hodinott (2008) show that the enforcement of conditionalities was important for schooling progression from primary to secondary level in Mexico's *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 the 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

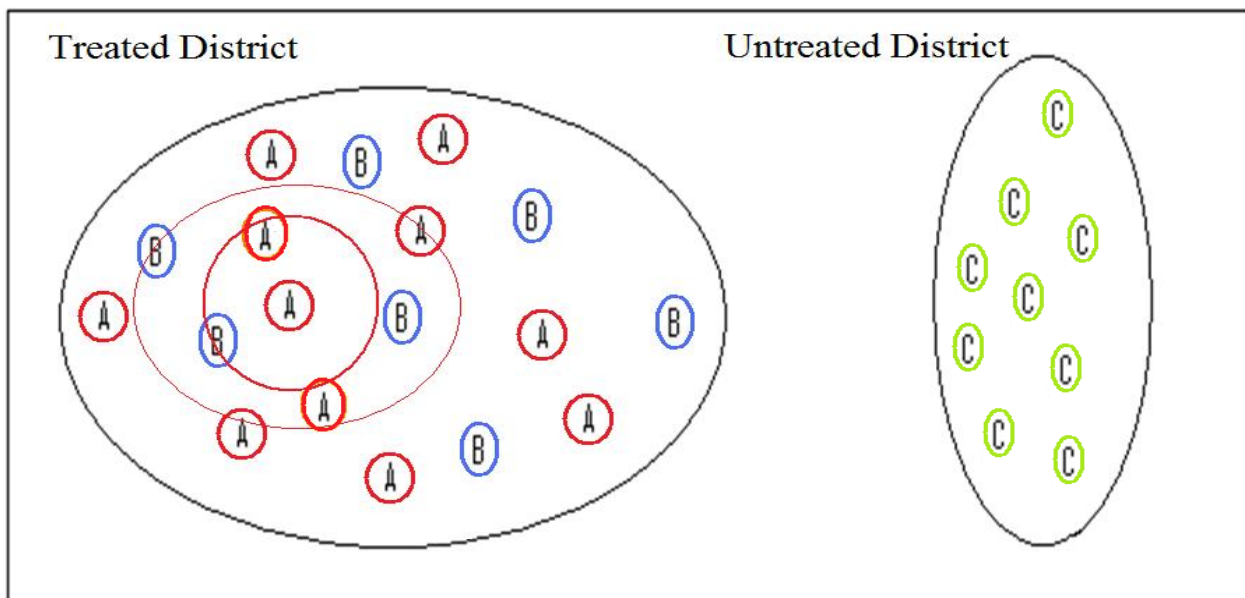
The absence of spillover effects on the comparison group is a key assumption of the programme evaluation literature, with a view to identifying a casual effect with a specific policy intervention. Estimation of the average treatment effect is only possible under what is known as the Single Unit Treatment 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, differences in outcomes between the treatment and comparison groups would potentially underestimate the impact (Heckman et al., 1999; and Miguel and Kremer, 2004).

It is a feasible assumption that spillover effects affect the population who live in the same communities or districts, but do not significantly affect 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 the non-treated group in treated districts and the comparison group in the non-treated districts, results in 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 in treated districts can help disentangle the joint effect of the income and behaviour effects over the treated. To that end, we must analyse 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 programme affects group A directly through the monetary transfers and conditionality compliance. Similarly, group B can also indirectly experience income and behavioural changes as a result of spillover effects. As depicted in Figure 1, externality reaches both A and B within the treated community: treated families may learn from one another, and price changes and increased liquidity affect all. It is assumed that A and B react similarly to externality. 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 by estimating 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:

$$\tau = E[Y_i(T_i = 1) - Y_i(T_i = 0) | T_i = 1], \quad (3.1)$$

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

The approach is similar to that proposed by Hudgens and Halloran (2008). In order to identify participation and externality effects, we have 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 is 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 underlying assumption is that SUTVA holds for untreated districts, comparison group C does not suffer contamination or interference from groups A and B, and thus there is no externality affecting those households.

We may define the average participation effect on the treated (APT), within-community effect, as follows:

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

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

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

The average externality effect on the treated (AET), between-community effect, may be defined as:

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

We can represent the outcome by the following linear function:

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

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

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

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

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

3.2 ASSESSING INCOME AND BEHAVIOURAL CHANGE EFFECT

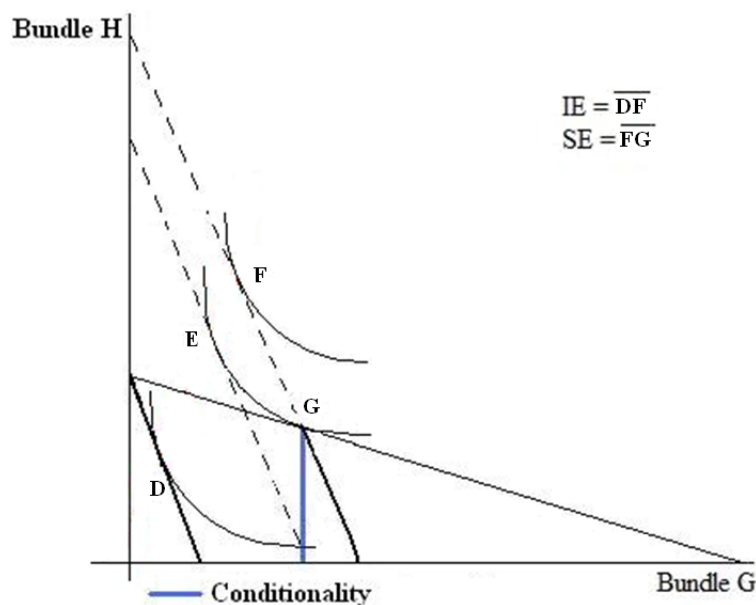
Besides looking at externalities, this paper also applies a methodology to disentangle the effects of monetary transfers and other non-monetary components of the programme (such as conditionalities and social worker visits) on treated households.

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

The conditionality in turn forces the families to change their preferences from point F to point G, which is a less optimal option in face of the new budget constraint (lower indifference curve passing through point E). Note that F is not feasible because, in order to receive the transfer, the family must consume a minimum amount of healthcare and education—that is, consume on 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 healthcare 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 Hodinott 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:

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

where $g_{D,T}(\cdot, \cdot)$ is a non-parametric function and $u_{i,D,T}$ represents the unobservable components. As in Juhn et al. (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, $F_{D,T}(\cdot)$. Thus, $u_{i,D,T} = F_{D,T}^{-1}(\theta_{i,D,T} | W_{i,D,T})$.

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

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

where

$$Y_{i,D,T}^W = \bar{g}(W_{i,D,T}, \bar{F}^{-1}(\theta_{i,D,T} | W_{i,D,T})), \quad (3.10)$$

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

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

Through equation (3.10) we estimate a counterfactual in which outcomes are the result of 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.

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

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

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:

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

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

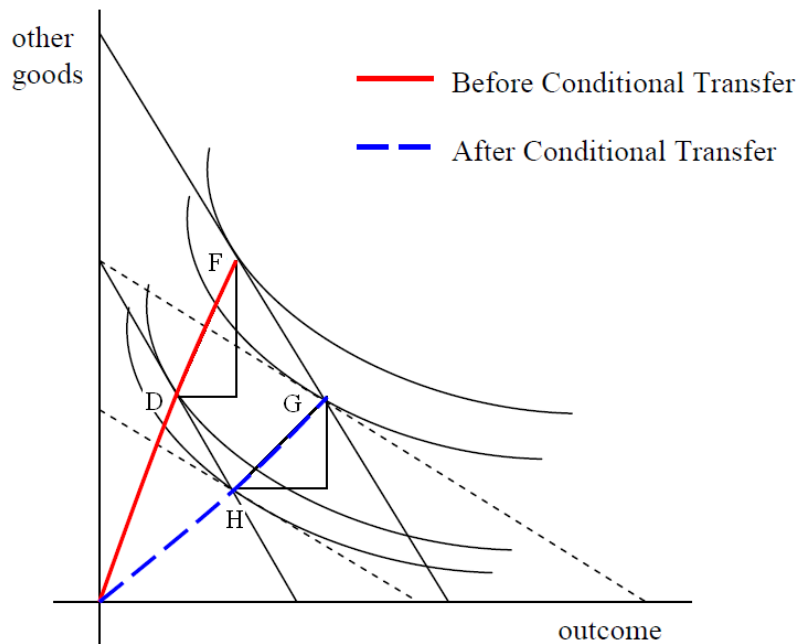
Additionally, by using exclusively the income of the between-district comparison group in the non-parametric function, an estimate of the income path if the programme were unconditional. This gives us the marginal income effect:

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

If instead of the cash transfer the government subsidised health and education, for instance by giving the individuals some money each time the child went to school or they held 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. 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 illustrate the effect, although it is not precisely 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

3.3.1 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. Since it is not possible to observe the same household in both states, the alternative is to work with counterfactuals. 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 counterfactual 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 components. The underlining assumption is that unobservable characteristics do not determine treatment assignment—that is, selection into the programme is completely based on observable variables, X_i . Under randomisation, or alternatively, conditioning to X_i , one can estimate an unbiased ATT effect by comparing treated and comparison group outcomes in the presence of treatment, for the cause is adequately identified (Rubin, 1978; Rosenbaum and Rubin, 1983).

The identification of these effects requires the following unconfoundedness assumption:

$$T_i \perp (Y_i(T_i = 0), Y_i(T_i = 1)) \mid X_i, D_i \quad (3.17)$$

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 in 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 differentiate treated and control districts—if households are in a 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:

$$D_i \perp (Y_i(D_i = 0), Y_i(D_i = 1)) \mid X_i, T_i = 0 \quad (3.18)$$

3.3.2 Estimating the Decomposition of ATT into Participation and Externality Effects

Given the groups surveyed, we observe 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:

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

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

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

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

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

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

where $P[\cdot]$ 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 identify participation in the absence of externality, τ_p , externality, τ_e , and ATT, τ :

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

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

3.3.3 Difference in Differences

The follow-up evaluation survey of *Tekoporā* included a retrospective question on education, and thus 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, and that can contaminate the result with selection bias. Once the sample has been

balanced by means of weighting, the baseline difference due to treatment should not be significant. For those outcomes of interest with available baseline information, we chose to use DD to ensure 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 the 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.

3.3.4 Heterogeneity of ATT: Awareness of Conditionalities and Visits by Social Workers

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

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

Thus, τ_p is the estimated APT for the treated unaware of the need to comply, and τ_{p^n} is the extent to which knowing about compliance changes the effect. The overall APT is the sum of both:

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

And ATT is given by:

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

3.3.5 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 nonparametrical estimation and ensures the possible non-linearity of the Engel curves. This estimation is made for (i) the household income of the whole sample, as shown in equation (3.10); (ii) the household income of the treated comparison group within and between districts, separately and then pooled, as in equation (3.11); and (iii) the household

income of the 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 us to assess the marginal income effect associated with ATT, APT and AET.

3.3.6 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, $p(X_i) = P[T_i = 1 | X_i]$, which 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:

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

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. The treated sample has to be 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 the within-community comparison group, $e(X_i) = P[D_i = 1, T_i = 0 | X_i]$ 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 of 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 of

making the comparison group's characteristics closer to treated observations. The objective is to eliminate differences in mean X_i .

As Robins and Rotnitzky (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's 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:

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

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

4 PROGRAMME DESCRIPTION AND EVALUATION DESIGN

The pilot phase of *Tekoporã* 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, and it covered 15 districts in five departments by 2009. The pilot phase covered five districts in 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 the Geographical Prioritisation Index (IPG), which is composed of both monetary and non-monetary indicators.

The programme has two main components. The first is a monetary transfer that aims to alleviate a family's immediate budget constraints; it amounts to 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 guaraníes (US\$12) per month. Thus, eligible households could receive between 90,000 and 180,000 guaraníes per month (US\$18–36). The second component involves conditionalities related to school attendance, regular visits to health centres and keeping immunisations updated, as well as family support provided by social workers (*guias familiares*).

To identify eligible households, a non-monetary quality of life index (ICV) was adopted as the targeting tool. This approach has been common throughout Latin America, where the monitoring of poverty often relies on using a composite index of unsatisfied basic needs. The ICV varies between 0 and 100 and consists of variables related to: condition of housing; access to public services and utilities, such as water, electricity, garbage collection and telephone; healthcare and insurance; the education of the head of household and spouse; years of schooling "lost" by children aged between 6 and 24; the occupation of the head of the household; ownership of durable goods; and the demographic composition of the household. Unlike the Geographical Prioritisation Index (IPG), ICV does not use any monetary variables.

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

- 1) Presence of children under 15 years of age or pregnant women.
- 2) Located in the priority areas of the programme, namely the poorest districts in the country according to the IPG.
- 3) ICV score below 40 points.¹²

In this pilot phase, the *Ficha Hogar* was fielded by means of a census 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 in districts that did not take part in the pilot formed the between-district comparison group. It is worth mentioning that the untreated districts were meant to be included in the pilot, but because of budget restrictions the programme could only afford five districts. To maintain the geographical balance between departments, one district from each department was excluded from the pilot.

Furthermore, potentially eligible households that were not in the poorest areas of the districts of the pilot could also be included in the programme registry as a result of the so-called “demand process”—that is, based on their demand to obtain 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 were not in the poorest areas of the pilot’s districts 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 for the collection of 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 2007. It contains all information available in the *Ficha Hogar* and additional questions needed to capture the outcomes of interest that were missing in the baseline (such as consumption data, school attendance, visit to health centres and so on).

About 1,093 households were surveyed. Among those which had complete interviews at baseline, 316 (28.91 per cent) are treated, 430 (39.34 per cent) are control from treated districts (within-district comparison group) and 347 (31.74 per cent) are control from non-treated districts (between-district comparison group). In terms of individuals, these figures correspond to 2,002 (31.26 per cent), 2,320 (36.23 per cent) and 2,082 (32.51 per cent), respectively, totaling 6,404 observations.

Both comparison groups of households have eligible households, which had children and an ICV of less than 40; and ineligible households, which also had children but an ICV equal to or greater than 40. Households without children or pregnant women were automatically excluded from the dataset, as were households registered as having an incomplete interview.¹⁵

The within-district comparison group consists of both ineligible households (ICV higher than 40) and eligible households that were “overlooked” by the programme. They account for

60 per cent of the within-district comparison group. The within-group observations have better living-standard indicators, since a smaller share of the between-district comparison group (9 per cent) has an ICV of over 40. Furthermore, while 74 per cent of treated households have an ICV of below 25 (the original target), the share is 61 per cent in the between-district comparison group and only 16 per cent in the within-district group. It is likely that households were not randomly overlooked. One possible reason for this administrative mistake is the change in the cut-off point of the eligibility criteria. Since the cut-off point was raised from 25 to 40 after the registration process had already begun, it is possible that in some neighbourhoods, potential beneficiaries whose ICV was 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 offers some evidence on conditionality knowledge and on monthly visits from social workers. The descriptive tables for education and health outcomes are Table 3 and Table 4, respectively.

According to Table 1, the average size of the household is seven persons and the average age is 22 years. There are 2.5 children per household and two-thirds of them are between six and fourteen years old. Children on average are 1.5 years behind in school, and the average household head has no more than four years of schooling. Household per capita income averages 133,000 guaraníes per month (US\$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 a higher ICV and average household per capita income for the reasons stated above.

TABLE 1

Descriptive Analysis of Individuals and Families' Demographic Characteristics

	<u>Comparison groups from:</u>				Difference in means significance		
	Total (A+B+C)	Treated group (A)	Within district (B)	Between district (C)	(A-C)	(A-B)	(B-C)
Household size	7.070 (0.035)	7.106 (0.058)	6.704 (0.067)	7.261 (0.063)		***	***
% male	0.527 (0.007)	0.528 (0.011)	0.524 (0.011)	0.528 (0.011)			
Age	21.626 (0.234)	21.088 (0.404)	23.230 (0.405)	21.951 (0.424)	**	***	**
Age in months for children under five years old	35.734 (0.648)	36.048 (1.134)	37.019 (1.071)	33.851 (1.132)			**



	<u>Comparison groups from:</u>				Difference in means significance		
	Total (A+B+C)	Treated group (A)	Within district (B)	Between district (C)	(A-C)	(A-B)	(B-C)
Number of children under five years old	1.210 (0.014)	1.212 (0.025)	1.033 (0.021)	1.350 (0.026)		***	***
Number of children between six and fourteen years old	2.232 (0.019)	2.338 (0.034)	1.850 (0.030)	2.222 (0.033)	***	***	***
Dependence ratio for children under five years old (children/adults)	0.164 (0.002)	0.166 (0.004)	0.151 (0.003)	0.168 (0.003)		***	***
Dependence ratio for children between six and fourteen years old (children/adults)	0.303 (0.002)	0.316 (0.004)	0.268 (0.004)	0.289 (0.004)	***	***	***
Number of years lost in schooling	1.522 (0.034)	1.570 (0.057)	1.244 (0.056)	1.579 (0.060)		***	***
Number of school years of household head	3.943 (0.030)	3.787 (0.048)	4.873 (0.061)	3.658 (0.051)		***	***
Household per capita income	132,937 (1,412)	133,975 (2,251)	141,143 (2,921)	123,086 (2,624)	***	*	***
ICV—Quality of Life index	23.667 (0.103)	21.863 (0.142)	31.405 (0.200)	22.825 (0.172)	***	***	***

Source: Authors' calculations based on the Evaluation Survey.

Note: Significantly 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. Since 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 enforced during the pilot phase. In fact, 92 per cent of the families claims to know about the conditionalities, but a smaller share know about each one of them.

TABLE 2

Descriptive Statistics on Awareness of the Treated about Need to Comply with Conditionalities

	Average
At least one visit per month from social workers	86%
Aware of programme conditionalities	92%
Aware of school attendance conditionality	83%
Aware of visits for child height and weight control conditionality	67%
Aware of vaccination conditionality	58%

Source: Prepared by the authors on the basis of the Evaluation Survey.

Table 3 shows that 93 per cent (92 per cent) of children attended school and 90 per cent (92 per cent) graduated to the next level in 2006 (2005). Over 96 per cent of children attended school five days a 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 performance, as the averages are significantly lower in 2006 compared to the groups in treated districts.

TABLE 3

Descriptive Statistics of Educational Outcomes

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

Source: Authors' calculations based on the Evaluation Survey.

Note: Significantly 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, and a higher percentage of updated vaccinations (51 per cent) than the within- and between-district comparison groups. As for visits to the health centres, the treated group had a higher probability of attending more than twice, and more than four times than the between-group comparison group.

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 manages to balance the distribution of the household and ICV components between the three groups.

TABLE 4

Descriptive Statistics of Health Outcomes

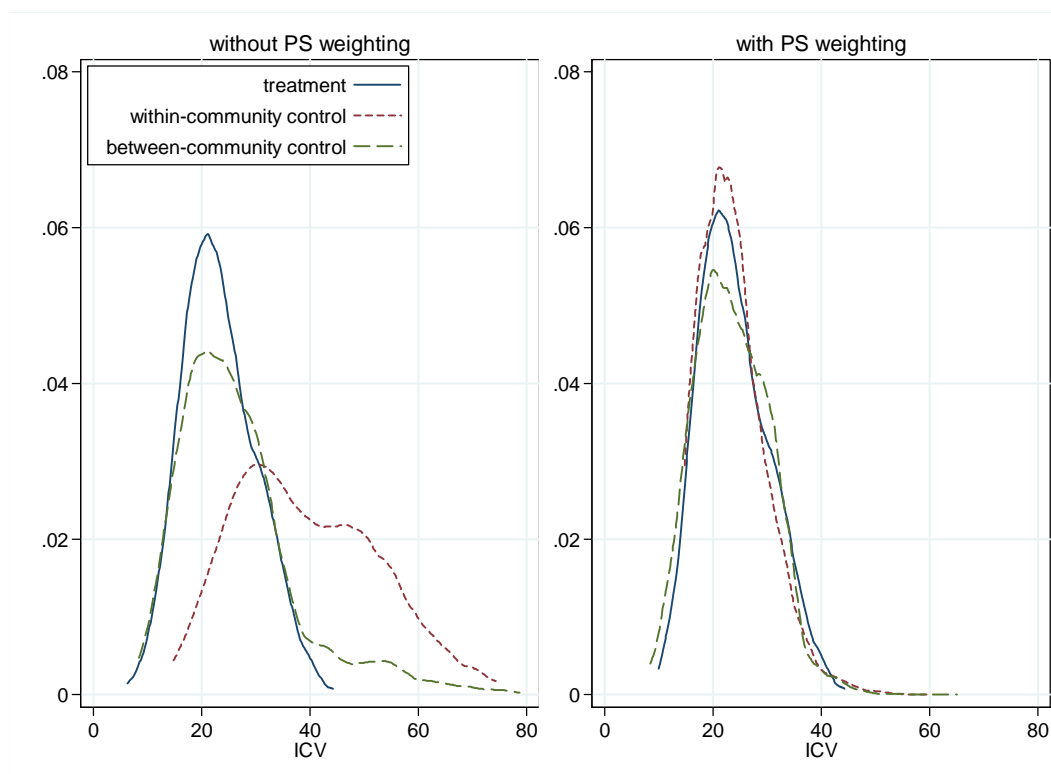
	Total (A+B+C)	Treated group (A)	Comparison groups:		Difference in means significance		
			Within district (B)	Between district (C)	(A-C)	(A-B)	(B-C)
Vaccination card	0.624 (0.017)	0.652 (0.029)	0.613 (0.029)	0.552 (0.030)	**		
% updated vaccinations	0.491 (0.015)	0.516 (0.025)	0.455 (0.025)	0.442 (0.026)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

Source: Authors' calculations based on the Evaluation Survey.

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

Figure 4 shows how the weighting changes the format of the kernel density distribution of the ICV in the comparison groups. It gives a higher weight to the observations with an 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 4

Kernel Density of the Probabilities of Being Treated for Treated and Comparison Groups

Source: Prepared by the authors on the basis of *Ficha Hogar*.

5.2 PARTICIPATION AND EXTERNALITIES, INCOME AND SUBSTITUTION EFFECT AND HETEROGENEITY

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 the 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 children aged between seven and 15.

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 five years, number of household members aged between six and 14.

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 previous years.

5.2.1 Educational Outcomes

Estimates of the *Tekoporā* effect on school attendance are shown in Tables 5 and 6: difference-in-differences estimates, single-difference estimates, heterogeneity taking into account social worker visits, and heterogeneity taking into account knowledge of 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 programme increased the proportion of children who attended school in 2006 by 7 per cent (ATT). It is possible that unobservable variables are generating a biased result in the 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 5 per cent in school attendance. Thus, at least for the single difference, the overall impact is positive and mostly due to the 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	*	
<hr/>						
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		
<hr/>						
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: Authors' calculations based on the Evaluation Survey.

Note: Significantly 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 somehow passed along and was understood by treated households. However, the message has not been confined 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: Authors' calculations based on the Evaluation Survey.

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

The impacts on school progression are in line with those on school attendance described above (see Tables A.1 and A.2 in the annex). The difference-in-differences estimate shows that the overall ATT effect is again not significant but the substitution effect indicates a 4 per cent increase in progression. The ATT estimates provided by single-difference are significant and slightly higher, indicating an increase of about 7 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: (i) possession of a vaccination card; (ii) ability to show the vaccination card; and (iii) child is up to date with at least 70 per cent of vaccines. As with regular visits to the health centres we use (i) at least one visit in the last year; and (ii) three visits or more per year. No significant result was found for vaccination variables, so the results are not displayed here. As for visits to health centres, three visits or more per year was the variable of choice because of the 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 five years old, child/adult ratio for children between six 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 the data did not allow for the use of the difference-in-difference methodology, only single difference is shown below. The results show that the visit to a health centre to monitor child development had a significant impact 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 driving 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 three 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: Authors' calculations based on the Evaluation Survey.

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

Even larger impacts were found for the treated community for the APT—that is, in the absence of externality effects (AET). There is an increase of 18 and 19 per cent for the APT and its corresponding substitution effect (SE). Again, the externality estimate was not significant. As with the educational estimation, the heterogeneity estimates do not suggest that conditionality awareness or visits by social workers 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 three in 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
IE	0.015	0.011		0.020	0.017		0.039
SE	0.148	0.055	***	0.010	0.026		0.054
UE	0.000	0.021		-0.104	0.088		0.117

Source: Authors' calculations based on the Evaluation Survey.

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

6 CONCLUSION

In this paper we evaluated the impact of the *Tekoporã* CCT programme, taking into account interaction and behavioural changes related to education and health outcomes among treated and non-treated groups in districts that benefited from the programme. We followed the methodology proposed in Ribas et al. (2010) and added a heterogeneity analysis of the impact with regard to awareness of the existence of conditionalities and the number of visits by social workers. This latter component is assessed in order to check the influence of the scheme's non-monetary components 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 of the retrospective question on education included in the survey.

However, our results differ from Ribas et al. (2010) in two ways: (i) there are no externality effects, neither for education nor health outcomes; and (ii) 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 improving family consumption, the changes in preferences are necessary to improve the family's demand for healthcare 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 Mexico's *Progresa* (Bobonis and Finan, 2005; Lalive and Cattaneo, 2009), which found positive externalities on education outcomes.

In any case, the ATT estimates suggest that the programme is achieving its objectives by improving children's attendance at school and at health centres. Heterogeneity with respect to knowledge of the need to comply with conditionalities, as well as the visits by social workers, do not add any extra impact on the education outcome. This is at odds with the results reported by Schady and Araujo (2008) on the impact on 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. The message of the importance of education and healthcare seems to have reached the households and has changed their preferences towards a greater consumption of healthcare and education.

Given the costs of the social workers component for the programmes that use them and the findings mentioned above, it is advisable to have further research on the contribution of different components, so as to have a clearer idea of what is essential to guarantee the programme's positive impacts. The next step in the research agenda is to replicate this methodology with randomised data, to apply the difference-in-difference methodology to more variables, or to use regression discontinuity design in order to obtain better causal identification.

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: Authors' calculations based on the Evaluation Survey.

Note: Significantly 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: Authors' calculations based on the Evaluation Survey.

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

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NOTES

1. Both methodologies were first presented in Ribas et al. (2010).
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 the home depend largely on the community or family habits, educational level, religion, laws, norms and so on.
3. Barrera-Osorio et al. (2008) presents other evidence on positive peer effects of CCT programmes on schooling in Colombia.
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).
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.
7. See Flores and Mitnik (2009) for a discussion of the consistency of these multiple treatment estimators.
8. It does not hold in the presence of crowding out effects in which the effect on B would have the opposite direction to A. For example, if there is service supply constraint and treated individuals are awarded preference in service provision, controls from the same area will experience crowding out even though they were willing to increase service use due to social interaction. Due to crowding out the externality and participation estimates would be biased because B will react differently from A in face of externality. So a second implicit hypothesis is that there is no service constraint.
9. See Section 4 for programme description and evaluation design.
10. See Woodridge and Imbens (2008) and Bertrand, Duflo and Mullainathan (2003).
11. Wooldridge (2002 and 2007) demonstrate the properties of this estimator.
12. Initially, the programme 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 because of some complaints at the local level, the eligibility threshold was increased to 40 points.
13. Selection on unobservables might have taken place because of 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, but 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.
15. About 8 per cent of the households registered in the *Ficha Hogar* had an incomplete interview (752 of 9,817). Nonetheless, 98 per cent of these cases (736) were registered by demand and 88 per cent (six from the census and 653 by demand) have been treated (Soares and Ribas, 2007).
16. Further information about multiple propensity score is available in Ribas et al. (2010).



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