International Poverty Centre

IPCevaluationnote

Number 3 March, 2008

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Tekoporã beneficiary family and school children in Paraguay. Photos by Fábio Veras Soares (IPC).

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EVALUATION NOTE PUBLISHED BY THE INTERNATIONAL POVERTY CENTRE

Achievements and Shortfalls of Conditional Cash Transfers: Impact Evaluation of Paraguay's Tekoporã Programme*

Fábio Veras Soares,** Rafael Perez Ribas** and Guilherme Issamu Hirata**

1 INTRODUCTION

The International Poverty Centre (IPC), with the support of GTZ and UNFPA, has recently undertaken an impact evaluation of the pilot of *Tekoporã*, a Conditional Cash Transfer (CCT) programme in Paraguay. Previously, IPC analysed the logical framework of this programme and its implementation challenges, and assessed its targeting mechanisms.[1] This Evaluation Note presents a summary of the impacts of the programme on household behaviour and well-being, as reported in Soares et al. (2008).

The evaluation of a pilot project can offer important inputs into the decision-making process on the feasibility of the scaling-up of the programme, the effectiveness of its design and the assessment of problems that limit its potential. The evaluation of *Tekoporã* shows positive impacts on per capita income and consumption, poverty reduction, school attendance, investment in agricultural production, access to credit, savings and social participation. Indeed, these results easily justify its scaling-up. However, the pilot has not been successful in reducing child labour or increasing child immunizations.[2] Thus, addressing these aspects needs to be a key part of any redesign of the programme when it is scaled up.

Tekoporã seeks to reduce extreme poverty by using direct cash transfers to poor households with children and diminish the potential for future poverty by encouraging investment in human and social capital. The programme intends to break the intergenerational cycle of poverty through investments in the health and education of children. The transfers are conditional on school attendance, regular visits to health centres and updating of immunizations. The programme also includes a family support initiative that, among having other effects, should increase the productive potential of the household and its social participation.[3]

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* The authors gratefully acknowledge the participants of the seminars that took place in Paraguay in November 2007 where the first results of this impact evaluation were presented, in particular, members of the Technical Committee of the Social Cabinet and of the Social Protection Network of the Secretary for Social Action (SAS). We also would like to thank the managers of programme Tekoporã for their openness and support throughout this evaluation, especially the Minister Judith Andraschko, Jorge Centurion, Ofelia Insaurralde and Jorge Castillo. We also acknowledge the financial and logistical support of GTZ Paraguay, in particular Horst Steigler, Irene Ocampos and Patricia Franco, and UNFPA. We are grateful to Terry Mckinley, former IPC director, Katie Fawkner-Corbett of the UK Department for International Development (DfID) and Luca Tiberti for their comments and suggestions. This study is part of a larger research project: "Growth, Inequality, Poverty and Cash Transfers: A comparative study of Poverty Impacts and Institutional Aspects in Selected Countries in Africa and Latin America", funded by DFID, GTZ and UNDP.

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Households are eligible for the programme if they fulfill all the following conditions: 1) have children under 15 years of age or pregnant women; 2) live in the priority areas of the programme, namely, the poorest districts in the country; and 3) have a low Index of Quality of Life (ICV). ICV is a non-monetary measure varying from 0 to 100 that synthesises several quality of life dimensions, such as access to public services, health and education outcomes, occupation of the household head, housing conditions and household assets. The programme provides the transfer to those households that are classified as extremely poor (having an ICV below 25) or moderately poor (having an ICV between 25 and 40).[4]

The database used in our evaluation comes from a household survey that took place between January and April 2007 in districts where the pilot of the programme started and in two other districts where the programme was *not* initiated.[5] In addition, we used data from administrative records—which include the payment register and data from '*Ficha Hogar*', a small questionnaire used to gather information for the selection of households.

The pilot of *Tekoporã* started in five districts: Buena Vista and Abai in the Department of Caazapá, and Santa Rosa del Aguaray, Lima and Union in the Department of San Pedro. The first payments took place in Buena Vista in September 2005. By August 2006, the pilot covered 4,324 beneficiary households in those five districts.

The rest of this Evaluation Note is organised as follows. Section 2 discusses the database available for the evaluation, the rationale for building appropriate control groups based on untreated observations, as well as the evaluation strategy, i.e., how we tried to circumvent the so-called 'fundamental evaluation problem'. Special emphasis is given to the assumptions used in the definition of each estimator implemented in this evaluation, namely, the Difference-in-Differences and the cross-sectional average difference, and the Propensity Score techniques that were adopted to ensure that both treated and untreated households were similar. Section 3 presents the most important results, with a focus on the outcomes of widest interest. Section 4 provides a summary and a discussion of the main results as well as our future research agenda regarding the evaluation of *Tekoporã*.

2 METHODOLOGY

This section explains the evaluation design adopted to investigate the impacts of *Tekoporã* and describes the main shortcomings faced in such an investigation. The major challenge of this evaluation was the lack of proper baseline information that could inform us about the differences between the 'treated' (beneficiary) groups and the 'untreated' (non-beneficiary) groups *before* the programme started. Another challenge was to deal with the problem that the programme's implementation did not follow a randomized selection of beneficiaries (i.e., in its 'placement strategy'), neither for households nor for districts.

2.1 DATABASE

In the absence of a baseline survey, we had to gather from administrative data information on household characteristics and on the criteria for selecting beneficiary households, namely, the values of the ICV and its disaggregated components. The *Ficha Hogar*, which has been the instrument of registry and selection of beneficiary households, allows us to approximate the

condition of the population of interest before the programme started. Thus, the *Ficha Hogar* database is used in this evaluation as the source of baseline information.

In the pilot phase of the programme, the *Ficha Hogar* was fielded through a census that took place in the poorest areas of selected districts. Nevertheless, potentially eligible households that did not live in the poorest areas of those districts could also be included in the programme registry as a result of the so-called 'demand process', namely, based on their demand to have information on their conditions provided to the *Ficha Hogar*.

The *Ficha Hogar* database comprises information on all households interviewed during the census or during the demand process in the five districts that participated in the pilot and in the two other districts—Moises Bertoni in Caazapá and Tacuati in San Pedro—that did not take part in the pilot but were covered by the census.

The evaluation survey was conducted in the field between January and April 2007 based on a sample derived from the Ficha Hogar that contained 1,401 households. The questionnaire of the evaluation encompassed all of the information available in the *Ficha Hogar*. In addition, it also included important questions related to outcome indicators that were not covered by the *Ficha Hogar*[6] and included a module on the perceptions of the beneficiaries about the programme.

Therefore, the evaluation covered several dimensions of the potential impacts of the programme. It examined health and education outcomes; child and adult labour supply; general consumption expenditures and the composition of food consumption; per capita income and poverty (measured both monetarily and non-monetarily); access to credit for consumption and savings; investment in agricultural activities and productive assets; social participation; and the obtaining of official identify cards.[7]

2.2 COMPARISON (OR CONTROL) GROUP

We could use the information on beneficiaries before the programme (from *Ficha Hogar*) and after the programme (from the evaluation survey) to estimate the impact of the programme. However, it is well-known that this 'before-after' estimator, also called 'a naïve estimator', is severely biased. For instance, a drought or a flood could have occurred during the implementation of the programme and made the follow-up indicators appear much worse than the baseline ones.

This could lead one to conclude that the programme did not have the intended effect on the beneficiary population. However, it is quite possible that in the absence of the programme, the condition of the beneficiaries would have been much worse than that actually found in the follow-up.

To avoid this sort of bias caused by the coincident impact of non-programme factors, it is important to have a comparison group, also called a control group, which is as similar as possible to the beneficiary, or treated, group except that it does not participate in the programme. In our example, the control group would have also suffered the effects of the hypothetical drought or flood, so that the difference between the two groups, on average, could be attributed solely to the programme.

For *Tekoporã*, it was possible to identify two comparison groups: a) non-beneficiaries who live in the other two districts covered by the census but not by the pilot project;

b) non-beneficiaries who live in the same five districts as the beneficiaries. This latter group is comprised of households that would qualify to receive the transfer but were 'overlooked' by the programme in its implementation.

Table 1 shows the reasons for not receiving the transfer for the 'comparison group' of eligible households in the *Ficha Hogar* database. The largest group is comprised of households located in the two control districts (44 per cent). This is followed by the households that lived in the same districts as beneficiaries but were 'overlooked' by the programme (30 per cent).

The other reasons provided in the table refer to 1) rejections by the selection committees, whose function was to check the list of potential beneficiaries yielded by the ICV ranking, 2) potential beneficiaries in settlements controlled by the landless movement who were waiting for permission from their leaders to take part in the programme; and 3) households that were identified as non-poor (because of their degree of economic autonomy) in later check-ups.

TABLE 1

Reasons for Non-Receipt of Treatment

	Eligible but not treated	Per cent
Two control districts	1,160	44.24
No reason (overlooked)	776	29.60
Rejected by selection committee[8]	542	20.67
Waiting for landless movement permission	127	4.84
Graduated due to economic autonomy	17	0.65
Total	**2,622**	**100**

Source: Soares and Ribas (2007).

We chose to use households in the control districts and the overlooked households in treated districts as our two control groups in this evaluation. However, since there was neither a randomized selection of the overlooked households nor of the two districts that were left out of the pilot, it has been necessary to use some techniques to render the potential control group as similar as possible to the treated group. These techniques are discussed below.

2.3 THE EVALUATION PROBLEM

Since some outcomes are observed before and after the programme implementation, while others are observed only after the programme took place, two kinds of estimators were adopted in our evaluation, namely the 'Difference-in-Differences' (DD) estimator and the Cross-Sectional (CS) estimator with selection based on observables. These estimators, as well as their implied assumptions, are described below.

Formally, in order to measure the impact of the programme on an observable outcome, we need to estimate the 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. When these differences are calculated for all treated households, we can obtain the *average* effect of the treatment (i.e., the programme benefits) on the treated households (the *ATT*).

Assume that $D(i,t)=1$ indicates that household *i* in period *t* has received the programme benefit and $D(i,t)=0$ indicates that household *i* has not received it. Before the treatment

$(t=0)$, $D(i,0)=0$ for both treated and untreated households; whereas after the treatment $(t=1)$, $D(i,1)=1$ for treated households and $D(i,1)=0$ for untreated households. Similarly, $Y^0(i,t)$ would indicate the outcome of interest for household *i* in the absence of treatment whereas $Y^1(i,t)$ would be the outcome for household *i* receiving the treatment.

Therefore, the average effect of the treatment on treated households can be defined as:

$$ATT = E\left[Y^1(i,1) - Y^0(i,1) \mid D(i,1)=1\right], \tag{1}$$

where $E[.]$ is the expectation (average) function of the difference between the two outcomes—one with the treatment, $Y^1(i,1)$, and the other without the treatment, $Y^0(i,1)$—over the whole population participating in the programme $D(i,1)=1$.

However, we cannot observe the same household in both states (with and without the treatment) at same time. That is, we cannot observe $Y^1(i,1)$ and $Y^0(i,1)$ for the same household. Therefore, we have a problem of missing data that has to be solved through the estimation of a counterfactual.

In order to identify the *ATT* impact of the programme, we can adopt the following estimator when both baseline and follow-up information is available (see Heckman et al., 1997):

$$\begin{aligned} ATT_{DD} = \; & \left\{E\left[Y(i,1) \mid X(i), D(i,1)=1\right] - E\left[Y(i,1) \mid X(i), D(i,1)=0\right]\right\} \\ & - \left\{E\left[Y(i,0) \mid X(i), D(i,1)=1\right] - E\left[Y(i,0) \mid X(i), D(i,1)=0\right]\right\}, \end{aligned} \tag{2}$$

where $Y(i,1) = D(i,1)Y^1(i,1) + (1-D(i,1))Y^0(i,1)$ and $Y(i,0) = Y^0(i,0)$ are the outcomes actually observed.

The first term in braces is the difference in the outcome, *Y*, between treated and untreated groups after the programme implementation and the second term in braces is the difference in outcome, *Y*, between treated and untreated groups before the programme took place. The difference between the two terms is the so-called 'Difference-in-Differences' (DD) estimator.

However, the identification of this estimator requires the assumption that, conditioned on a set of observable characteristics, $X(i)$, the average difference between treated and untreated groups would have followed the same trend in the absence of the programme:

$$E\left[Y^0(i,1) - Y^0(i,0) \mid X(i), D(i,1)=1\right] = E\left[Y^0(i,1) - Y^0(i,0) \mid X(i), D(i,1)=0\right]. \tag{3}$$

However, when we do not have baseline observation and, therefore, have to rely on differences *after* the programme, we have to make an additional assumption:

$$E\left[Y^0(i,0) \mid X(i), D(i,1)=1\right] = E\left[Y^0(i,0) \mid X(i), D(i,1)=0\right], \tag{4}$$

which basically means that the outcome before the treatment, $Y^0(i,0)$, is the same for both treated, $D(i,1)=1$, and untreated groups, $D(i,1)=0$.

Combined with assumption (3), assumption (4) implies that:

$$E\left[Y^0(i,1) \mid X(i), D(i,1)=1\right] = E\left[Y^0(i,1) \mid X(i), D(i,1)=0\right]. \tag{5}$$

Therefore, the average outcome conditioned on $X(i)$ for the treated group, $D(i,1)=1$, in case that it had not received the treatment, would be the same as that of the untreated group, $D(i,1)=0$, after the programme implementation. This assumption implies that selection into the programme is completely based on observable variables, $X(i)$.

Whereas assumption (3) entails only that the bias due to unobservable differences between treated and untreated groups is 'balanced' and can be eliminated if we take the difference before and after the programme, assumption (4) implies that the bias due to unobserved factors is zero (non-existent). Therefore, the CS estimator is based on stronger assumptions than the DD estimator.

This assumption yields the following estimator of the *ATT* effect in a cross-sectional framework:

$$ATT_{CS} = E[Y(i,1) \mid X(i), D(i,1)=1] - E[Y(i,1) \mid X(i), D(i,1)=0]. \quad (6)$$

The DD estimator (2) is our preferred estimator since it relies on weaker assumptions than the CS estimator. However, we can calculate the DD estimator only when outcome indicators of the variables of interest are available from the baseline survey. When this is not the case, we report only the CS estimator. See the Summary Table in the Appendix to have a clearer picture of when each estimator is used.

2.4 PROPENSITY SCORE TECHNIQUES

Abadie (2005) states that the expectations in (2) and (6) can be calculated non-parametrically. However, the main challenge in estimating these estimators is to guarantee the correct balance in the observed characteristics, $X(i)$, in order to avoid selectivity bias since placement into the programme is not random.

We could balance comparison and treated groups according to their observed characteristics, but this would require a very large sample (or the use of a very small set of observable characteristics). Instead, as shown by Rosembaum and Rubin (1983), we can rely on the Propensity Score (*PS*) to balance the two groups according to their observed characteristics. Therefore, the *PS*, $p(X(i))$, can replace $X(i)$ in the expectations (2) and (6).

The *PS* can be defined as the probability of receiving the transfer conditional on the observed characteristics, $X(i)$:

$$p(X(i)) = \Pr[D(i,1)=1 \mid X = X(i)]. \quad (7)$$

This probability can be calculated parametrically, using, for instance, a logit model. The balance mentioned above requires that the households in the treated and comparison groups that have similar Propensity Score have, on average, similar observed characteristics.[9]

$$E[X(i) \mid p(X(i)), D(i,1)=1] = E[X(i) \mid p(X(i)), D(i,1)=0]. \quad (8)$$

The *PS* can then be applied to estimate (2) and (6) through two techniques. We can use a non-parametric matching approach, the so-called 'Propensity Score Matching' (*PSM*), or the parametric 'Propensity Score Weighting Regression' (*PSW*).

The specific *PSM* technique that we implemented was the Nearest Neighbour Matching (*NNM*), in which we basically matched each treated observation with the closest comparis on-group observation in terms of propensity score.

In the *PSW*, we weight the untreated observations in the regression analysis according to their propensity score. Then, the observations with a high propensity score—i.e., those more similar to the treated ones—will have a larger weight in the regression analysis. In accordance with Hirano and Imbens (2001), the weight for estimating the *ATT* effect can be represented as:

$$\omega_{ATT}\left(D(i,1), X(i)\right) = D(i,1) + \left(1 - D(i,1)\right)\frac{p\left(X(i)\right)}{1 - p\left(X(i)\right)}. \quad (8)$$

Note that the higher the *PS* of an untreated observation, $D(i,1) = 0$, the larger its weight, while the weights of the treated observations, $D(i,1) = 1$, are always equal to one.[10]

When analysing the results, we consider a result consistent when it is both significant at the level of five per cent probability for one of those estimators and significant at least at the level of 10 per cent for the other estimator.

2.5 ESTIMATING THE PROPENSITY SCORE

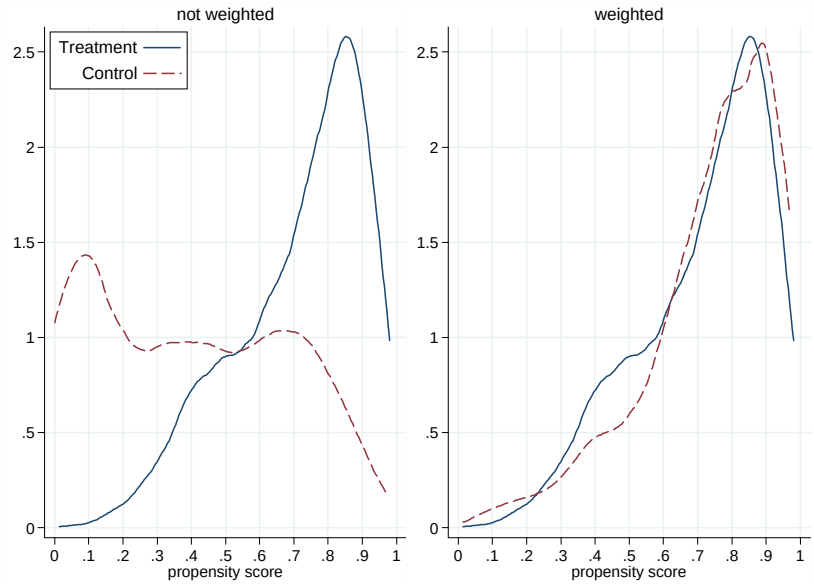
The best model to estimate the propensity score through a logit regression is one that includes only balanced variables in accordance with assumption (8). Such an assumption can actually be tested by checking whether the differences in observable characteristics between treated and comparison groups are significant. In our case, we tested the differences between groups for distinct intervals of *PS*, taking into consideration a confidence interval of 97.5 per cent.

The balanced model includes, in addition to the ICV, several of its components in disaggregated form and other indicators derived from the *Ficha Hogar*. The set of observable variables includes, for instance, demographic characteristics, housing conditions, education of the household head and of his/her spouse, their occupations, household assets, and poverty status in monetary terms.

Figure 1 shows how the weight ω_{ATT} changes the format of the kernel density distribution of the propensity score in the comparison group. It gives a higher weight to the observations at the top of the distribution (those more likely to have taken part in the programme) and a lower weight to those observations at the bottom of the distribution (those less likely to have taken part in the programme), so that the representative distribution of the comparison group mimics the distribution of the treated group (shown in the right panel).

FIGURE 1

Kernel Density of the Propensity Score for Treated and Untreated Groups



Note: Epanechnikov kernel.

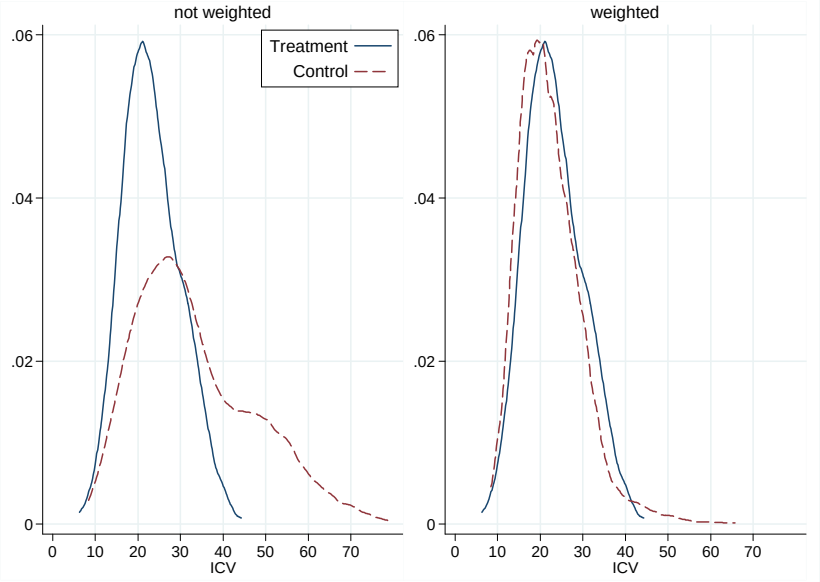
Source: Own calculations based on *Ficha Hogar*.

Note again that our *PS* model includes the ICV in its set of covariates. Employing assumption (5) on the selection of households based on observables, we can achieve consistent results since the ICV is actually the main determinant of participation in the programme. Indeed, almost all baseline analyses did not reveal significant differences between treated and untreated groups after the *PS* weighting.

Figure 2 shows the kernel density distribution of the ICV for treated and control groups. We can see in the panel on the left the unweighted ICV distribution, which shows that the control group has, on average, a larger ICV value than the treated group—since its distribution is more skewed to the right. The panel on the right shows how the weighting scheme given by the propensity score changes the distribution of the control group so that it resembles the distribution of the treated group. Now, the two groups have means that are not statically different. The primary function of this weighting scheme is to make sure that the control group is as similar as possible to the treated group in observed dimensions before the treatment.

FIGURE 2

Kernel Density of the ICV for Treated and Untreated Groups



Note: Epanechnikov kernel.

Source: Own calculations based on *Ficha Hogar*.

3 MAIN RESULTS OF THE EVALUATION[11]

In this section, we present the results of the impact of the programme on well-being indicators, namely, the ICV (the non-monetary poverty index), per capita household income, and monetarily measured poverty. We also examine the impact on human capital indicators for education and health, especially those related to the conditionalities of the programme: school attendance, possession of vaccination cards, and the proportion of updated vaccinations.

Moreover, we discuss the impacts of the programme on consumption and the composition of the household food basket, as well as on credit for consumption, savings and investment in agricultural activities (livestock and physical capital). We also investigate the impacts on child labour and adult labour supply to see whether the programme has discouraged children from working and has generated any disincentive to work among economically active adults. Finally, we show the impacts on household members with regard to social participation and obtaining official identity cards.

In addition to presenting the average impacts of the programme on the whole sample, this section presents the results for specific groups. For instance, some impacts might be more substantial for households in rural areas, or for the extremely poor (with ICV less than 25) than for the moderately poor (with ICV between 25 and 40).

The possibility of some spill-over effects of the programme is considered as well. This would occur when the impacts of the programme affect the untreated population in the districts participating in the programme but not the untreated population in the two districts not covered by the programme. The particular results affected by spill-over effects will be mentioned, however, only when they are relevant and add some useful information to the evaluation results.

As mentioned before, we report the results based on the techniques of Nearest Neighbour Matching (NNM) and Propensity Score Weighting (PSW). Thus, we will have two estimates for a single indicator, which will be reported as a range. However, when there is no difference between them, we report only one value for the estimated impact.

3.1 THE IMPLEMENTATION OF THE PROGRAMME ACCORDING TO THE BENEFICIARIES

Before we analyse the main results of the impact evaluation, we examine the perceptions of the beneficiaries regarding programme implementation. These perceptions were captured by a specific module of the questionnaire for the evaluation survey. Bear in mind that the majority of the beneficiaries were in the programme for one year and a half when the evaluation survey took place.

As mentioned in Soares and Britto (2007), conditionalities have not been monitored in the pilot of *Tekoporã*. Thus, no family has been dropped from the programme for non-compliance. Nevertheless, the family guides have apparently been effective in communicating the message about conditionalities to households: only nine per cent of the beneficiaries either replied that they did not know the conditionalities of the programme or they stated that the conditionalities did not exist.

About 85 per cent of the beneficiaries correctly stated that attendance at school is a conditionality, while 70 per cent mentioned regular visits to a health centre and 60 per cent mentioned the updating of the immunization card. It would be interesting to investigate in the future whether differences in the knowledge of conditionalities have affected the estimated impacted of the programme on some dimensions, particularly for health and education.

The great majority of beneficiaries have received at least one visit per month of the family guide. When asked about the activities of the family guides, the beneficiaries mentioned mostly monitoring activities: about 69 per cent mentioned the monitoring of children's school attendance, 46 per cent mentioned the monitoring of visits to a health centre, and 37 per cent the monitoring of the updating of the immunization card.

In contrast, 36 per cent said that the family guides organised seminars and kept them informed about the programme. Interestingly, 24 per cent said that the guides monitored their purchases—an activity that is not officially listed as a family guide responsibility, but that might have had an impact on the consumption expenditures of families.

Beneficiaries were not very well informed about the duration of the programme. About 71 per cent said that they did not know how long they could participate in the programme; while only 14 per cent gave the correct answer of three years. Finally, the great majority, namely, 85 per cent, said that the programme payment has often been delayed.

3.2 IMPACT ON THE ICV, HOUSEHOLD PER CAPITA INCOME AND MONETARY POVERTY

Although both treated and control groups witnessed, on average, an improvement in their ICV value, the programme did not have a significant positive impact, in aggregate, on this multidimensional indicator according to the DD estimators. Indeed, such a general improvement might have been caused by an overall enhancement in the quality of life of households or by the use, in fact, of better-quality information in the evaluation survey than in the *Ficha Hogar*.

The ICV is a structural targeting indicator, comprised mostly of long-term household characteristics. Thus, one should not expect the programme to have a significant impact on this composite index for the treated group in the short run.

With regard to monetary measures,[12][13] we found that the programme increased per capita income of the treated households between 31 and 36 per cent; this impact was due mainly to the cash transfer. Such an increase led to a reduction of at least 17 percentage points in the incidence of extreme poverty among beneficiaries (from an initial level of over 52 per cent).

Even when we used household per capita income *net of the transfer*, we found a reduction in the incidence of extreme poverty among beneficiaries of between five and 10 percentage points. This impact indicates that the programme might have encouraged autonomous income generation among beneficiary households by enabling some of them to raise their income above the level of extreme poverty even when the value of the programme transfer is not considered.

In addition to the impacts on increasing per capita income and reducing extreme income poverty, we observed a diversification of household income sources, particularly in rural areas. In such areas, there was a positive impact of five percentage points in the proportion of treated households that reported earnings from both agricultural and non-agricultural activities.

3.3 IMPACTS ON EDUCATION AND CHILD LABOUR

We do not have a baseline survey that would enable us to employ the DD estimator for the education indicators. However, the evaluation survey asked retrospective questions on school outcomes in order to generate some descriptive information, which could provide a rough idea of the changes in attendance rates.[14]

According to the retrospective data, the average attendance rate increased from 92 to 94 per cent. However, the bulk of this change was due to the increase in attendance rates of children in beneficiary households, which rose from 93 to 95.5 per cent.

Despite the already high attendance rates observed in the general population, it was possible for the evaluation to identify a positive impact of the programme.[15] Attendance rates increased by between five and eight percentage points for the treated group. Consequently, grade progression also increased by four to seven percentage points.

However, no evidence was found that the increased grade progression was due to the reduction in the probability of failure to progress among the children attending school. It was explained almost fully by the reduction in the probability of children's dropping out. This result is similar to that found in Brazil (Oliveira et al., 2007). It suggests that CCT programmes might, indeed, have a limited impact on child proficiency, while having a broad impact on improving school attendance (Soares et al., 2007).

The impact on attendance was stronger among boys (i.e., between six and 11 percentage points) and among children aged 11 to 15 years (i.e., between nine and 15 percentage points). The larger impact for boys and older students indicates that the programme could have discouraged dropping out and/or brought back to school students who had previously dropped out. Indeed, older children, mainly boys, are more inclined to give up school in order to work. Since school attendance among children six to nine years old was already very high, it was more difficult for the programme to have a significant impact on this age group.

In our analysis, we examined the evolution of child labour before and after the programme for children between four and 14 years of age. The incidence of child labour for the whole sample increased by almost five percentage points—namely, from 8.5 per cent to 13 per cent. The increase was larger for the untreated group (from 5.4 to 11.6 per cent) than for the treated group (from 10.4 to 14.1 per cent). These statistics suggest that without the programme, the labour supply of treated children could have been, in fact, higher.

Only for children between four and nine years old could we identify a significant impact of the programme on reducing labour. Nevertheless, for children younger than ten years old, most labour activity is family work.

We also modelled school attendance and child labour taking into consideration their interdependence. We investigated differences in the probability of being in one of the following four states: 1) not at school and not at work; 2) both at school and at work; 3) only at work; 4) only at school. This analysis showed that treated children were less likely than untreated children to neither attend school nor do work (namely, less likely by between 3.5 and 2.2 per cent) and less likely to be involved only in work (by between one and two per cent).

Moreover, beneficiary girls were more likely to only study than non-beneficiary girls (by between 2.5 per cent and 5 per cent). Finally, there was no negative effect of the programme on the combination of study and work: treated children were not less likely to study and work than non-beneficiary children.

With regard to spill-over effects, it was only when we compared treated and untreated groups in different districts that the results reported in the last paragraph were significant. There was not a significant difference between treated and untreated households in the same districts.

With regard to the heterogeneity of these impacts, we observed that in the sample from the census, treated children were more likely to only study and less likely to only work because of the programme. In fact, this reduction in the probability of only working was stronger in rural areas and among children 10 to 14 years of age.

Among extremely poor households, treated children became less likely to neither study nor work (by between 4.5 and 5.5 per cent) and more likely to be both working and studying (by six per cent) due to the programme. Indeed, the impact on the transition from the former condition to the latter condition occurred mainly among extremely poor children.

Overall, these results show that the programme has been markedly effective in keeping children in school, but has not been effective enough in ensuring that they would also not be involved in work activities.

3.4 IMPACTS ON HEALTH

For the proportion of children younger than 60 months who had vaccination cards, there was an increase from 77.5 per cent to 90 per cent for the whole population. However, this increase might not have been very significant because only 58 per cent of the cards were actually shown when the person responsible for the children was asked to do so in the follow-up survey. Unfortunately, this type of information was not gathered in the *Ficha Hogar* so we could not compare results on this indicator before and after the programme.

In fact, we have not been able to identify a positive impact of the programme on the proportion of children with vaccination cards and on the proportion of updated vaccinations

for those who showed their cards. Indeed, the DD estimator shows a negative and statistically significant impact of the programme on the possession of vaccination cards.

Interestingly, when those interviewed were asked to show the children's vaccination card (in the evaluation survey), only 56 per cent of the non-beneficiaries managed to do so, whereas 95 per cent had said that they had vaccination cards. For the beneficiaries, 60 per cent showed their cards, but only 87 per cent had said that they had it. Nevertheless, the difference between treated and comparison groups, with regard to the proportion of those who showed the cards, was not statistically significant. Hence, the increase in the possession of vaccination cards, if true, cannot be attributed to the programme.

With regard to updating vaccinations, only 50 per cent of cards were updated among treated children and only 45 per cent among untreated children. However, the impact analysis based on the CS estimator did not find this difference to be statistically significant. In any case, it is worth noting the low level of updated vaccinations for both groups. This might indicate that even the access to the cards has not guaranteed a corresponding access to vaccines. This condition highlights a problem that is likely due to a supply-side deficiency.

With regard to the visits of children younger than 60 months to a health centre, treated children attended, on average, almost one more time per year than untreated children. The results also show that beneficiaries were six per cent less likely than non-beneficiaries to *not* attend a health centre at all or to attend *less than* twice per year. The treated children were one per cent more likely to attend at least three times a year, four per cent more likely to attend 4-5 times and seven per cent more likely to attend six times or more.

It is important to note that this impact is driven basically by the comparison between districts participating in the programme and those not participating. Thus, there might have been a positive spill-over effect on untreated neighbours in the same district, leading all children of the districts where the programme was implemented to be more likely to attend a health centre.[16]

Therefore, the conditionality of visiting a health centre regularly seems to be the easiest with which a beneficiary household could comply. An important question for the programme is why, despite complying with the conditionality of health clinic visits, beneficiary children were not obtaining their vaccination card and having their vaccinations updated. Only among moderately poor children was it possible to identify a positive impact on the proportion of updated vaccinations.

This appears to represent an important supply-side bottleneck that prevents households from complying with the conditionality of maintaining their vaccinations updated. The lack of coverage and the low quality of public services have already been mentioned by Soares et al. (2007) as factors that might have inhibited the impact of CCT programmes on health outcomes.

3.5 IMPACT ON CONSUMPTION AND THE FOOD BASKET

Due to the programme, treated households have had a level of per capita consumption (both monetary and in-kind)[17] that was between nine per cent and 15 per cent higher than that of untreated households. Moreover, the programme caused a relative reduction of four percentage points in the expenditures on food,[18] in contrast to a relative increase of three percentage points in expenditures on child clothing. The latter result was found for all groups that we investigated. This robust result suggests that treated households increased their

relative consumption of some specific goods related to the welfare of their children after having received the transfer.

Among *extremely poor* households, the positive impact of the programme on per capita consumption was larger than in the whole sample. Namely, it led to an increase of between 13 and 21 per cent (compared to 9-15 per cent for all treated households). For the extremely poor, the relative reduction in expenditures on food consumption was not found. Interestingly, there was a decrease in both the probability of consuming alcoholic beverages and the relative amount of money spent on such items. Finally, there was a small increase in the relative expenditures on the acquisition of a house or its maintenance (which includes refurbishments).

Comparing households within the same districts leads to other interesting results. In this case, there was an increase not only in total per capita consumption (between 19.5 per cent and 21 per cent), but also in monetary per capita expenditures (between 14 and 34 per cent), and in per capita food consumption (14 per cent).

Another impact found by comparing the treated and untreated groups of the same districts was the reduction in the *relative* expenditure on food, in contrast to an increase in the *relative* expenditure not only on child clothing but also on adult clothing, education and tobacco.

When the impact analysis is limited to the comparison between districts in which the programme was implemented and districts in which it was not, the only significant result—in addition to the one on child clothing—was the reduction in the probability of purchasing alcoholic beverages.

Comparing the results between the analyses using both control groups (i.e., within the same district or in another district), we have identified that the programme could have had a negative spill-over effect on the consumption of food, tobacco and alcoholic beverages—and even on expenditures as a whole. This effect implies that in the districts where the programme took place, these indicators decreased on average among non-beneficiaries, suggesting a change in the average behaviour of all households living in those areas.

Even if the programme did not affect the level of food consumption, it could have changed the composition of the food basket since treated households would have had more flexibility to choose the items that they wanted to consume, as well as have receiveed advice from family guides on which kinds of food provide a healthier diet. For this purpose, we analysed two types of indicators: a) the probability of a household's consumption of each item; b) the share of each item, measured monetarily, in total food consumption.

The results show that there was a *relative* increase in the consumption of fresh fruits, non-alcoholic drinks, sweets and pastries (normal goods) and a *relative* decrease in the consumption of flour and eggs (inferior goods). There was also an *increase in the probability* of consuming dairy products, non-alcoholic drinks, sweets and pastries and a reduction in the probability of consuming *mate* (a brewed herb), prepared food and spices.

The changes in the food basket reveal a clear preference for dairy products and some child-related products, such as sweets and pastries, when household income rises. Finally, the increase in the consumption of fresh fruits might have been encouraged by the family guides, who try to motivate household members to consume a healthier diet.

3.6 IMPACT ON CREDIT AND SAVINGS

One of the potential impacts of a cash transfer is to relax the budget constraint of a household by providing a regular and predictable transfer of purchasing power. This feature gives more choice to the household on its consumption decisions, but also allows it to be more effectively cushioned against negative shocks (which can be frequent for extremely poor households). In order to evaluate changes in the way that households are able to manage their budget, we assessed their access to credit for consumption and savings as a result of the transfer.

The impact analysis revealed a positive impact of the programme on increasing household access to credit for consumption. Beneficiary households had, on average, seven per cent more access to credit than non-beneficiary households. In rural areas, such an impact was slightly larger—between eight and 10 per cent. However, whereas the moderately poor experienced an increase in access to consumption credit, there was no such impact on the extremely poor.

When comparing the treated with the untreated, we found that the positive results were larger in the comparison between populations in different districts. This might indicate a positive spill-over effect on untreated households for consumption credit in treated districts.

We define the savings rate as the difference between the logarithms of monthly income and expenditures, as in Deaton (1997). The evaluation shows that beneficiary households saved 20 per cent more due to their participation in the programme. Before the programme, those households had a negative savings rate of 17 per cent. So, after the programme, they had achieved a positive savings rate of three per cent of their income.

This positive effect was also found for the non-beneficiaries living in districts where the programme had been implemented. This finding is consistent with the negative spill-over effect of the programme on consumption for this group. Non-beneficiary households in the same district did not experience an increase in their income, but they did increase their savings through a reduction in consumption. In this sense, they might have imitated the precautionary saving behaviour of beneficiaries.

Among extremely poor households, the savings rate for beneficiary households was 20 to 26 per cent higher than for non-beneficiary households, whereas among the moderately poor, this impact was not robust. Another difference observed between extremely poor and moderately poor households was that the former had reduced their debts attributable to the purchase of food by between six and eight percentage points, whereas the latter had increased their debt by five percentage points

3.7 IMPACT ON AGRICULTURAL ACTIVITIES

Since the majority of participants in *Tekoporã* live in rural areas and are occupied as self-employed workers in agriculture, it is important to evaluate the impact of this programme on agricultural activities. In addition, family guides are instructed to motivate households to engage in income-generating activities.

The main impact that we identified is that beneficiary households invested between 45 to 50 per cent more in production than non-beneficiary households in the 12 months prior to the survey. The programme had also increased the probability that households would acquire livestock by six per cent. This mainly took the form of acquiring fowl (by an increased

probability of four to five per cent) and pigs (by an increased probability of 10 per cent). Among beneficiaries who had such animals, the average number of fowls and pigs also increased. But no impact was found on the acquisition of cattle, a larger form of livestock.

Particularly in rural areas and among the extremely poor, there was not only an expansion in the amount invested in production but also an increase in the proportion of beneficiaries doing such investment. In rural areas this increase was between eight and 13 per cent and among the extremely poor, about 20 per cent.

There was an overall decrease in both the cultivation of vegetable gardens (from 77 per cent to 65 per cent) and in crop diversification (from 32 to 22 per cent) for the entire sample. But there were 'positive' impacts of the programme on these indicators for treated households: the reductions were about 24 per cent and between 13 and 20 per cent, respectively. Thus, these impacts took the form of dissuading beneficiary households from reducing their cultivation of vegetable gardens and reducing crop diversification *as much as* the drastic way that the untreated households had done.

3.8 IMPACT ON LABOUR SUPPLY

The impact of cash transfers on labour supply is one of the main concerns about CCT programmes. Public opinion has often been swayed to fear that transfers might cause dependency on public assistance. For *Tekoporã*, we assessed the impact on the labour supply of the entire adult population and on men and women separately, according to two definitions of 'economic activity'. In the first definition, we included all people either working or looking for a job and in the second definition, we added temporary workers who were seasonally unoccupied at the time of the survey.

With regard to the first definition, we found a negative impact (amounting to between three and 10 percentage points) on the labour supply of men. This negative impact on men was also pronounced among the moderately poor and in rural areas. Nevertheless, such an impact was identified neither for the whole population nor for women. For the latter group, the effect was positive, but lacked statistical significance.

Using the second definition, we did not find robust impacts on any of the groups analysed. The negative impacts on men's labour supply turned out not to be significant, except for moderately poor men.[19]

An explanation for the observed negative impact on beneficiary males in the first definition is that temporary workers, who usually work according to the seasonality of agriculture, might have refrained from seeking casual jobs during their period of lay-off due to the receipt of the cash transfer.

Using a qualitative approach, Guttandin (2006) corroborates this hypothesis by pointing out that men participating in *Tekoporã* could afford not to engage in casual work (*changas*) due to the transfer. This qualitative result is in line with the quantitative differences that we obtained from using the different definitions discussed above. In this sense, the programme transfers could be functioning as informal unemployment insurance for workers who rely on temporary jobs for their livelihoods.

3.9 IMPACT ON SOCIAL PARTICIPATION

In the impact evaluation of *Tekoporã*, social participation was defined as the involvement of the household head or his/her spouse in political, religious, leisure or labour associations. Moreover, when one of them worked, made decisions or voted in some such social group, his or her participation was defined as 'active'.

Among beneficiary households, only 28 per cent took part in the beneficiary committee, which has been one of the activities organised and encouraged by the family guides. However, since only beneficiaries could participate in this committee, this aspect was not taken into consideration in evaluating any impact on social participation.

The impact analysis shows that the programme was responsible for an increase of six to 10 percentage points in social participation in general and for an increase of five percentage points in *active* participation. The increase in social participation was due mainly to participation in trade unions, cooperatives or productive associations—as well as in religious groups. It is also striking that social participation among the extremely poor increased by seven to nine percentage points, whereas participation by the moderately poor did not change significantly.

There was no impact on participation in political groups, leisure organisations or community committees. A possible explanation for the lack of impact on community committees is that there was some substitution effect between participation in beneficiary committees and participation in community committees since their functions and activities have often overlapped.

Finally, when we compared the treated households with their untreated neighbours in the same district, no significant programme impact was found on social participation. Thus, there might have been a spill-over effect of the programme in promoting social participation. That is, social participation improved as a whole in districts where *Tekoporã* was carried out. But since we do not have adequate baseline information, we cannot present stronger evidence for such an impact.

3.10 IMPACT ON ACQUIRING ID CARDS

Another responsibility of the family guides is to motivate all beneficiaries to obtain the official identification card. For this reason, we evaluated the impact of the programme on the probability that different age groups would show an identity card (an I.D.).

During the period between the programme implementation and the evaluation survey, there was a reduction in the number of people *without* an I.D. card. This happened for both treated and untreated groups. However, only for adults older than 25 years was it possible to identify a positive impact of the programme. Such an impact amounted to a reduction of three percentage points in the proportion of adults *without* an I.D. card. This result represents, in fact, a reduction of 40 per cent in the absolute number of adults *without* such an I.D.

The limitation of this impact to only adults older than 25 years of age could be explained by the conditionality that the adult responsible for receiving the transfer has to present his/her I.D. card in order to collect the benefit. Because of the difficulties and costs involved in obtaining the I.D. in Paraguay, particularly in areas distant from large urban centres, this result can be interpreted as a significant achievement of the programme. Nevertheless, much more work is still needed to successfully register the younger population.

Please refer to Table 2 appended at the end of this Note for a convenient summary description of all of the results that we have reported in this section. Such a summary is useful background information for our final remarks.

4 FINAL REMARKS

This Evaluation Note has highlighted the main results of the impact evaluation of a CCT pilot in Paraguay called *Tekoporã*. Impact evaluations play a very important role in the design of social programmes, especially in the pilot phase. After an *ex-post* impact analysis, policy makers can develop a better idea of the areas that will need more attention in the future scaling up of the programme. Such an analysis helps them not only to justify the maintenance and expansion of a successful programme but also to rethink its design, if necessary, in order to mitigate unanticipated effects and enhance its beneficial features.

When analysing the results presented here, one should bear in mind that they refer to a pilot in which a series of adjustments and modifications were still taking place when the evaluation survey was conducted. Thus, it is remarkable that some positive impacts could still be clearly discerned, particularly regarding education, consumption, income, social participation and productive investment, and especially among the extremely poor.

Tekoporã has had a positive effect on school attendance, particularly among those groups more likely to drop out. However, there has been no significant impact on child labour, which, in general, has been combined with school attendance instead of being abandoned. This effect occurred mainly among the extremely poor. This is an important issue that should be carefully considered in the future design of the programme, since an increase in child labour could negatively affect education outcomes.

The programme has also had a sizeable impact on consumption, mainly among the extremely poor, and it has had a marked impact on reducing extreme poverty. This reduction is also evident even if we consider the household income *net of the transfers*. This indicates that beneficiaries have been able to autonomously generate income rather than relying solely on the transfers to exit extreme poverty.

This result is in line with the findings that a) beneficiaries have increased their investment in production and b) a higher proportion of extremely poor households have started investing in production. Other indicators such as the acquisition of small livestock also corroborate this interpretation.

The negative impact on men's labour supply is the only result that seems to run counter to the results described earlier in this note. Nevertheless, one should not overlook the possibility that the transfer could have increased the reservation wage of 'temporary workers' so that they did not need to accept low-quality 'casual jobs' (*changas*). Thus, such an outcome does not necessarily represent a negative impact of the programme.

Despite the large positive impact on consumption, there has been also an increase in savings, which is another remarkable outcome of the programme. In rural areas, this effect has a critical importance, since beneficiary households become more able thereby to manage the risks of adverse shocks, such harvest losses.

The evaluation also revealed some bottlenecks in the implementation of the programme. For instance, the lack of impact on obtaining vaccination cards and updating vaccinations,

despite the increase in the number of visits to health centres, calls for a closer examination in order to isolate and identify the problem.

This finding should alert programme managers to the risk of not having the desired impact on health outcomes after the scaling-up of the programme because the necessary adjustments were not being made concurrently in the provision of health services. Clearly, if the supply of health services, including the quality of their delivery, does not improve along with the scaling up, families will not be able to comply with some of the health-related conditionalities of the programme.

In this evaluation we have focused on the average treatment effect on the treated and assumed that the treatment could be represented as a binary variable: treated or untreated households. An agenda for further research would involve the use of other measures of the scope and scale of treatment. For instance, we intend to investigate the impact of the period of exposure to the programme as well as the size of transfers on some key outcomes.

Moreover, the distinction between the effect on household well-being due solely to the transferred income or to the effect of the condionalities on household behaviour requires a deeper investigation. Also requiring more research are the spill-over effects of programme benefits on non-beneficiaries.

In addition, designing a module of the questionnaire that is specifically addressed to programme implementation could help us investigate the role of conditionalities. This would help understand, for instance, whether the fact that the updating of vaccinations was the least known conditionality of the programme had any connection with the lack of impact on this outcome. This research agenda would likely shed light on some of the key issues in the global debate about the effectiveness of Conditional Cash Transfer programmes and of their various components.

TABLE 2

Summary of the Main Findings of the Impact Evaluation

Indicators	Average Impact	Heterogeneity of Impact	Spill-over Effects	Estimator
1. Education	Positive impacts on school attendance of 5 to 8pp and on grade progression of 4 to 7pp. However, there was no impact on the failure to progress. The joint analysis of education and child labour shows a decrease in the probability of both 'only working' (1.2 to 1.8pp) and neither working nor studying (3.5 to 2.2pp). No impact was found on the probability of 'study only' and of 'study and work'.	The positive impacts on school attendance and grade progression were higher for boys: 6 to 11pp and 5 to 10pp, respectively; and for 11-15 years old: 9 to 15pp and 7 to 11pp, respectively. The decrease in the probability of 'only working' was larger for boys (1.2 to 2pp) and for 10-14 years old (2.1 to 3.2pp). The decrease in the probability of 'neither working nor studying' was larger for 10-14 years old (4.2 to 5.4pp) and for the extremely poor (3.1 to 5.1pp). There was an impact on the probability of 'study only' for girls (6.5 to 12.6pp) and for the whole census sample (6.2 to 12.7pp); and a positive impact on the probability of 'study and work' for the extremely poor (6pp).	Some evidence of positive spill-over effects on the probability of 'neither working nor studying' and on the probability of 'only work'.	Cross-sectional
2. Health	The only positive impact identified was the average number of visits to the health centre in the past 12 months for children younger than 60 months: treated children make one more visit per year. There was a negative impact on the possession of vaccination cards and no impact on vaccination updating.	There was a positive impact on vaccination updating for moderately poor treated children: 10 to 15pp higher.	Some evidence of positive spill-over effects on the average number of visits to the health centre.	DD: possession of vaccination card CS: number of visits to the health centre and vaccination updating
3. Consumption	Main findings: 1) Positive impacts on per capita consumption: nine to 15 per cent; 2) Relative reduction in food expenditures: four per cent; 3) Relative increase in the expenditures on child clothing: three per cent	Positive impact on per capita consumption was higher for the extremely poor: 13-21 per cent. For this latter group there was no reduction in relative food expenditure.	Some evidence of negative spill-over effects on per capita consumption.	Cross-sectional
4. Food Basket	Main findings: 1) Increase in the probability of consuming dairy products, non-alcoholic drinks, sweets and pastries; 2) Reduction in the probability of consuming *mate* (herbal tea), processed food and spices; 4) Increase in the consumption of fresh fruits, non-alcoholics drinks, sweets and pastries; 4) decrease in the relative consumption of flour and eggs			Cross-sectional
5. Poverty	Main findings: 1) No impact on the ICV; 2) increase of 31 to 36 per cent on household per capita income of beneficiary families; 3) reduction of 17 percentage points in extreme poverty; 4) reduction of five to 10 percentage points in extreme poverty even when not taking into consideration the value of the transfer.	In rural areas there was an increase in the diversification of sources of income: a five percentage points increase in the proportion of households in rural areas that report income from both agricultural and non-agricultural activities.		Cross-sectional

Indicators	Average Impact	Heterogeneity of Impact	Spill-over Effects	Estimator
6. Credit for Consumption	Main finding: beneficiary families have, on average, seven per cent more access to credit for consumption than non-beneficiaries.	Beneficiaries in rural areas have an 8 to ten percentage point higher access to credit than non-beneficiaries.	Some evidence of negative spill-over effects on access to credit	Cross-sectional
7. Savings	Main finding: beneficiary families save 20 per cent more than non-beneficiary families.	Among extremely poor beneficiaries, the positive impact on savings was higher than for all beneficiaries: 20 to 26 per cent higher and for the census sample, 30 per cent higher.	Some evidence of positive spill-over effects on savings.	Cross-sectional
8. Agricultural Activity	Main findings: positive impacts on: 1) amount invested in production: 45 to 50 per cent more; 2) the probabilityof livestock acquisition: 6pp higher for treated households; 3) number of fowl and pigs: at least 17 per cent higher for both animals. There has also been positive impacts on 'vegetable garden' cultivation (24 per cent) and on 'crop diversification' (between 14 and 20 per cent). But for these indicators there was a general downward trend so the programme prevented treated households from reducing both as much as untreated households did.	In rural areas and among the extremely poor—in addition to the increase in the amount invested in production—there has been an increase in the probability of investing in production: between 8-13pp in rural areas and between 20-22pp among the extremely poor.		Difference-in-differences (in general)
9. Social Participation	Main findings: positive impacts on: 1) social participation in general: six to 10pp; 2) with this increase being driven by the participation in trade unions, cooperatives or productive associations, as well as in religious groups; 3) there were no impacts on participation in political groups, leisure organisations or community committees	Among the extremely poor, social participation increased by seven to nine percentage points, whereas social participation by the moderately poor did not change significantly	Some evidence of positive spill-over effects on social participation in general	Cross-sectional
10. Acquiring ID Cards	Main finding: positive impact on ID card acquisition among adults, but no impact for younger household members.			Difference-in-differences
11. Child Labour	Main findings: No impact on child labour, except the negative impact on child labour (family work) for children aged 4-9.			Difference-in-differences
12. Adult Labour Supply	Main findings: 1) No overall impact; 2) Negative impact for men depending on the way temporary workers who are laid-off are treated	When our analysis includes temporary workers who are laid-off as economically active, only for the moderately poor is it possible to find a negative impact on men's labour supply.		Difference-in-differences

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NOTES

1. See Soares and Britto (2007) and Ribas et al. (2008).

2. The reduction of child labour was not listed as one of the objectives of the programme, but this evaluation shows that an increase in attendance at school does not necessarily lead to a reduction in child labour. Assuming that this is a problem that the government would like to address, some changes in the design of the programme would be necessary. The lack of impact on immunisations should raise concerns about the awareness-raising work of the family guides as well as about the provision and quality of health services.

3. This family support approach is inspired largely by the 'psycho-social support' of *Chile Solidario*.

4. For more details on the targeting mechanism, see Ribas et al. (2008).

5. This survey was financed by UNDP Paraguay.

6. Bear in mind that for the indicators not covered by the *Ficha Hogar*, all reported results refer to the cross-sectional estimators--based on the conditional differences between treated and untreated observations after the programme--rather than to the Difference-in-Differences estimators, which will be discussed later in this note.

7. Another dimension that was evaluated was the impact of the programme on household composition. However, this dimension is not presented in this Evaluation Note because we did not find any relevant results from our analysis.

8. This statistic is over-estimated since it includes hundreds of rejections of households that were located in indigenous communities that could not take part in the programme as it was designed. A specific programme is due to be designed for these communities.

9. In addition to guaranteeing the balance between the two groups, the propensity score is important for determining the 'common support' for the two groups. Thus, comparisons are made only between households within the same range of propensity scores.

10. In fact, we also have sampling weights. The final weight used in the regression analysis is the sampling weight multiplied by ω_{ATT}.

11. In order to compare these impacts with those achieved by other CCT programmes in Latin America, see Soares et al. (2007).

12. Due to the poor quality of the income data of the *Ficha Hogar,* we do not use the DD estimator. All results refer to the CS estimators, which compare treated and comparison households after the treatment.

13. We used the poverty lines calculated by the National Statistical Office of Paraguay (DGEC). The poverty line is Gs 250,074 for urban areas and Gs 151,315 for rural areas, whereas the extreme poverty line is Gs 143,152 for urban areas and Gs 98,517 for rural areas.

14. We did not use this retrospective information in our impact analysis since it would introduce bias into our results.

15. Here we use the CS estimator. Thus, this impact corresponds to the difference between treated and untreated groups after the programme, such as in equation (6).

16. Of course, this might also be due to differences between districts in the supply of health services.

17. We imputed the monetary value of in-kind consumption, which includes self-consumption and donations.

18. The programme has not caused, however, any absolute reduction in food consumption.

19. It is important to note that the negative results for men were found mostly when we used the comparison group in the same district. Using this latter group, it was also possible to identify a positive impact on women's labour supply.

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