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The Effect of Conditional Transfers on School Performance and Child Labor: Evidence from an Ex-Post Impact Evaluation in Costa Rica

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Abstract[1]

Conditional transfer programs are becoming a common approach to influence household decisions. The evidence to date is that these programs are good at promoting certain outcomes such as school attendance, but that other outcomes such as reducing child labor are more difficult to achieve. This study examines the impact of Superémonos, a conditional transfer program in Costa Rica, which provides poor families with a subsidy for the purchase of food conditional upon children regularly attending school. Using three different empirical techniques—simple comparison of mean outcomes, regression analysis and propensity score matching—we examine the program's impact on school attendance, performance in school and child labor. We find strong evidence that the program achieves its goal of improving school attendance and much weaker evidence regarding school performance. The program does not reduce the likelihood that youth will work. These findings are discussed in the context of the results from impact evaluations of other conditional transfer programs.

Key words: conditional transfer programs, human capital, child labor, Costa Rica

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1. Introduction

Conditional cash transfer (CCT) programs are rapidly populating the landscape of Latin American social programs. CCT programs transfer resources to poor families on the condition that the families engage in some behavior: sending their children to school, taking children to health clinics for check-ups, or some other action. Most transfer programs have two principal components: an education component and a health component (Rawlings and Rubio, 2003). The former is usually focused on primary and secondary-age students, while the latter focuses on infants and very young children. CCT programs have been initiated in Brazil (Bolsa Escola and PETI), Colombia (Familias en Acción), Mexico (PROGRESA, recently renamed Oportunidades), Honduras (PRAF), and Nicaragua (Red de Protección Social).[2] The initial wave of evaluations of these programs has revealed promising results with regards to improved levels of consumption, reduced incidence of poverty, and increased school attendance (Rawlings and Rubio, 2003; Skoufias, 2001).[3]

At the same time, it is not clear that cash transfer programs, without the addition of complementary activities such as after-school programs, can affect the problem of child labor. The two cases in which CCT programs have produced documented reductions in child labor in the Americas—PROGRESA in Mexico and PETI in Brazil—are precisely those cases where complementary activities such as after-school programs were part of the education component (Yap, Sedlacek and Orazem, 2002; Pianto and Soares, 2003; Skoufias, 2001). In only one case,

[2] Bolsa Escola offers cash transfers to families conditional upon school attendance. PETI (Programa de Eliminaçao do Trabalho Infantil—Program for the Eradication of Child Labor) provides cash stipends to poor families living in areas of high rates of child labor, on the condition that their children attend school and participate in an after-school program. Familias en Acción (Families in Action) provides two types of cash transfers to low-income families: i) to families with children under seven years old, conditional upon children receiving health and wellness checks, and ii) to families with grade school and high school-aged children, conditional upon school attendance. PRAF is the Programa de Asignación Familiar (Family Allowance Program). This program involves two transfers to families living in areas of concentrated poverty: a nutrition and health transfer for families with pregnant women and children 3 years old and younger, and a school cash transfer for households with children between the ages of 6 and 12. It also contains two "supply-side" transfers to organizations in the target areas: i) to parent and teacher associations and for teacher training, and ii) to health clinics. PROGRESA is the Programa de Educación, Salud y Alimentación (Education, Health and Nutrition Program), which is now called Oportunidades. It offers cash transfers, nutritional supplements, and preventive health care as well as educational programs about health and hygiene. The Red de Protección Social (Social Protection Network) is a pilot project which offers cash transfers conditional upon families participating in child growth monitoring and immunization programs, as well as regular adult attendance to training sessions on health and nutrition.

[3] Oportunidades, the more recent CCT program in Mexico, expanded Progresa to urban areas and included other modifications to the original program. Oportunidades has not yet been evaluated; thus this paper restricts the discussion of results to the rural program Progresa.

from Bangladesh, have researchers documented that a stand-alone transfer program (in this case an in-kind transfer of food) led to a decline in child labor (Ravaillon and Wodon, 1999).[4]

This paper contributes to this expanding literature on conditional transfer programs by evaluating a program that to date has not been evaluated, Costa Rica's Superémonos ("We will overcome" in Spanish). The program provides food coupons to poor families, conditional upon a commitment of these families to enroll all their children in school. The program does not have as an explicit goal the reduction of child labor.

We gauge the program's impact on three outcomes of interest: school attendance, school performance and child labor, using three different methodologies to ensure a triangulation of results. These methodologies include a "naïve" comparison of means of program participants with non-participants, a regression approach which attempts to identify the determinants of child labor, school attendance and school performance (including among these factors participation in the programs of interest), and a rather new statistical methodology called "propensity score matching," which compares the outcomes of program participants to those of non-participants who are carefully matched so that they have similar ex-ante probabilities of participating in the programs. The fact that Superémonos imposes no requirements on children's *pre-program* work or school behavior is a particular part of the program design that lends itself well to ex-post evaluation.

Three levels of results are of interest. First, of course, is the impact of the program on the school attendance, educational performance and child labor in Costa Rica. A second interesting issue is whether in-kind transfers of food (as provided by Superémonos) are as effective as transfers of cash (as provided by the majority of conditional transfer programs in the region) in improving school attendance and performance. Finally, the paper will contribute to the debate about whether conditional transfers by themselves (i.e., without complementary interventions) are able to influence household decisions on labor force participation of children.

The paper is structured as follows. The following section contains a description of the Superémonos program. Section 3 describes the data and methodologies used in the evaluation of the program's impact. Section 4 presents the statistical and econometric results, and Section 5 contains the conclusions.

[4] Ravaillon and Wodon find that receiving an extra 100 kg of rice decreases the probability of child labor by 4 percent, evaluated at the mean.

2. Superémonos: A Conditional Transfer Program

Superemónos is a program developed by the Instituto Mixto de Apoyo Social (IMAS), the institution responsible for leading and coordinating anti-poverty programs in Costa Rica. Superémonos provides a monthly food coupon to poor households on the condition that *all* children in the household between the ages of 6 and 18 attend school. Participating households receive a coupon worth 10,000 colones per month (approximately US $30) for the 10 months comprising the school year, which can be redeemed for food in any supermarket. In 2001, 12,234 families participated in the program (IMAS, 2001).

Parents or guardians are required to sign a commitment agreement ("compromiso de participación"), in which they agree to: i) keep all of their children in school; ii) not use coupons to buy liquor, cigarettes, drugs or luxury items; iii) report abuse of the program by other beneficiaries; and iv) not transfer the coupon to third parties. Support can be discontinued if these and other conditions are not met. Two progress reports per year document whether children are regularly attending school.[5] It is important to note that the program does not require that the children pass the school year or stop working, but simply that they attend school.

As is the case for other IMAS programs, households must pass a means test in order to participate in Superémonos. Information on potential beneficiaries is collected in a questionnaire and entered into a database called SIPO (Sistema de Información sobre la Población Objetivo—Information System on the Target Population), which contains data on more than 250,000 households. Eligibility is determined by a household's SIPO score, which is based upon five factors: i) occupation of household head; ii) quality of housing stock; iii) household income; iv) educational level of household head; and v) net worth of the household.

[5] Other reasons for discontinuing support are: i) children do not continue to reside with parents or guardians; ii) coupons are not picked up by beneficiaries within 20 days of their being available; or iii) false information was given to IMAS in order to qualify for the program.

3. Data and Methodology

Data were collected on program participants and non-participants by a custom-designed sample survey. Basic contact information for program participants came from lists of beneficiaries for the year 2001 provided by IMAS. We conducted the survey of beneficiaries and non-participants in three neighboring urban areas of Costa Rica: Alajuela, Cartago and San Jose. In all, 746 participating families and 1,042 non-participating families were surveyed.[6]

The most rigorous way to examine the effectiveness of a program is by random assignment of individuals to treatment and control groups before program participation begins; this approach allows evaluators to attribute the differences in outcomes for these two groups, with a great degree of confidence, to participation in the program (or non-participation). Unfortunately, such random assignment frequently is not feasible in social programs. Intentionally excluding eligible individuals from a program to form an ideal control group presents an ethical dilemma. In addition, random assignment is often politically intractable: individuals excluded initially from participating (assigned to the control group) may lobby successfully to be included from the outset.[7]

Random assignment did not take place in Superémonos program. Since random assignment did not occur, we constructed the control group after the fact. This control group was drawn from families from the same communities and neighborhoods as program participants. Our goal in constructing the control group was to ensure that non-participants resembled our treatment group of participants as closely as possible along a key set of demographic and socio-economic variables that would not be affected by program participation. Care was taken to ensure that, as a group, the age and sex of children in the control group matched those of Superémonos participants. In addition, non-participating families were selected so that their neighborhood, educational level of the mother, and access to electric service mirrored that of Superémonos participants.[8] Table 1 indicates that, according to these variables, the

[6] Responses to questions in the survey revealed that some of these families identified as non-beneficiaries (i.e., not on the list of program beneficiaries) identified themselves as program beneficiaries. These families were excluded from the control group.

[7] This is sometimes dealt with, albeit imperfectly, by allowing those in the control group to participate in the program at some future date (termed "cross-over evaluation design").

[8] Access to electric service was broken into two categories: 1) having metered electricity or 2) having non-metered electricity or no electricity at all. The second category represents about 4 percent of the households in the sample.

demographics and socio-economic status of beneficiary families (as indicated by the list provided by IMAS) are quite similar to those of non-participating families.

The survey instrument collected detailed information pertaining to one randomly selected child in the household, as well as information about other family members and the dwelling. The mother or father was asked whether the child regularly attended school during the 2002 school year and whether the child regularly attended school during the previous school year (2001). We also included a question about whether the child attended school in the week prior to the survey in 2002, which served as a consistency check for the more general question on attendance.[9]

Among children aged 10-16, 90 percent were reported to be regularly attending school in 2002, while 88.7 percent were reported to have attended school last week. The parents were also asked if their children worked in the week prior to the survey in 2002, with the definition of work including all types of activities that generate income (in-cash or in-kind) for the family.[10]

Once data were collected on program participants and a socio-economically similar group of non-participants, three different methodological approaches were used to gauge the effect of Superémonos on the outcome variables of interest (work or not, attend school or not, pass school year or not). First, the mean values of the outcome variables were compared between the treatment and control groups. Second, regression analysis was used to estimate reduced form equations, with the outcome variable of interest as the dependent variable. A variable measuring participation in Superémonos is included as one of the explanatory variables, in order to determine whether program participation affects the outcomes of interest after controlling for other factors that might influence the outcome. Third, a matching methodology was used in which participants are explicitly matched with an individual in the non-participating group, in order to ensure that the outcomes are being compared between individuals who have similar a priori propensities of participating in the program.[11] Essentially, this methodology models the determinants of program participation and estimates a "propensity score" for each individual

[9] There were two waves of the survey. The first wave was in April and May of 2002, and the second wave in August 2002. The cost per survey was approximately US $13. While the availability of the high quality database of beneficiaries (SAB) at IMAS greatly facilitated the implementation of this study, the fact that Costa Rica does not have a standard system of addresses, relying instead on descriptive characteristics of locales, greatly increased the cost of fielding the survey.

[10] Thus, unpaid work in family businesses was captured work. Time spent on unpaid domestic activities such as babysitting and cleaning was measured in another question.

[11] This propensity score is modeled as a function of a large number of explanatory variables. Clearly, these variables must not themselves be affected by program participation.

(both participants and non-participants) that predicts the probability of participation. Using this propensity score, the group of participants (treatment group) is matched with a comparison group (control group), and the mean values of the outcome variables are compared between the treatment and control groups in order to gauge program effectiveness.[12]

Each approach has advantages and disadvantages. A comparison of means is the simplest possible approach. But while its simplicity is a virtue, it has quite serious disadvantages. Most important among them is that there is no way to control for other factors which affect the outcomes of interest and which potentially may vary between the treatment and control groups. Regression analysis has the advantage of being widely used and easily understood. Since program participation is only one of many variables that may affect the outcomes of interest, using this approach allows us to gauge the importance of program participation in comparison to a large number of other variables, some of which can be directly targeted by public policy. But regression analysis rests on strong assumptions about the parametric distribution of the error term.

Propensity score matching has one key advantage: the methodology only compares observations that are very similar in terms of the probability of program participation. Treatment observations that do not have a sufficiently close match in terms of the probability of program participation are dropped from the comparison. At the same time, propensity score matching has a serious disadvantage: in order to find statistically significant differences between treatment and control groups, the sample size must be larger than for the regression approach. Given that each methodology has advantages and disadvantages, we opt to present results from all these approaches in this paper.

4. Empirical Results

Unadjusted Differences in Mean Outcomes

Before presenting the results from more sophisticated techniques, it is useful to consider the differences in the mean outcomes across the groups without controlling for other characteristics of the household or child. Table 2 displays differences in mean outcomes between beneficiaries

[12] See Annex 1 for a more detailed description of this methodology.

(treatment group) and non-beneficiaries (control group) for children between ages 13 and 16.[13] There are two outcome measures for each year: attending school and passing the grade for the 2001 school year, and attending school and working in the week prior to the survey for the 2002 school year.[14]

Measurement error produces slight inconsistencies between the list of beneficiaries provided by IMAS for 2001 and the families who report in the surveys having received Superémonos in 2001. We define the treatment group for the 2001 outcomes according to whether the family has reported receiving Superémonos in 2001. The control group for 2001 was similarly defined as any family who reported not receiving Superémonos that year.[15]

Note that in defining the treatment group for 2002, a new survey was *not* drawn from a beneficiary list for 2002. The treatment group for 2002 consists of those who report they have received Superémonos benefits in 2002. The difference in sample size between the treatment groups in 2001 and 2002 largely reflects that families are less likely to participate in Superémonos as children enter their late teenage years and drop out of school.

Mean attendance rates for Superémonos beneficiaries in 2001 were approximately four percent higher than for non-beneficiaries. While the mean passing rates for beneficiaries was also about 4 percent higher, this difference was not quite statistically significant. In terms of outcomes for 2002, neither attendance rates nor the probability of working in the week prior to the survey were statistically different between beneficiaries and non-beneficiaries.

Regression Analysis

The comparison of unadjusted means is valid if the distributions of characteristics (observed and unobserved) are the same for the treatment group and control group. While great care was taken

[13] We limit our sample to children ages 13 to 16 for two reasons. First, we have few observations for youth over the age of 16. Second, for ages 10-12, while the sample size is sufficient, the probability of school attendance is close to the maximum of 100 percent; with 98 percent of children attending school, there is little scope for the program to increase this percentage. There is some scope for the program to have an impact on the age group 13-16 since the percentage attending school in both comparison groups is lower at 89 percent.

[14] Data on work experience was not collected for the preceding year due to potentially serious problems with recall bias. Data on passing is not available for the current year, since the school year had not yet ended at the time of the survey. We also performed analysis with a second, more "pure" control group whose members not only did not receive Superémonos last year, but also did not receive a monetary scholarship through the National Fund for Scholarships (Fondo Nacional de Becas, FONABE). The results from this analysis were quite consistent with those reported here.

[15] I.e., those who were on the IMAS list of beneficiaries for 2001 but responded that they did not receive benefits in 2001 were included in the control group.

to draw the control group from a similar population (see Section 3), the populations may differ in some characteristics. In this sub-section we report the results of a regression analysis that attempts to control for these potential differences.[16]

Tables 3-6 provide the results of multiple regression analyses of the effect of receiving Superémonos on school attendance, promotion, and child labor, controlling for other factors that might influence these outcomes.[17] The outcomes are binary (e.g., worked last week or not, attended school last year or not, etc.), and thus the probit specification is used. Marginal effects of the explanatory variables are reported, since these effects cannot be inferred directly from the regression coefficients.

Receiving Superémonos in 2001 had a significant effect on school attendance (Table 3) and on passing the grade (Table 4). Program participation is associated with a 2.94 percentage point increase in the probability of attending school and a 4.83 percentage point increase in the probability of passing the grade for these children who were ages 12-15 during the 2001 school year. It is interesting to compare these marginal effects with those of other explanatory variables. In the case of school attendance, the marginal effect of Superémonos is equivalent to increasing the mother's educational level by six years. On the other hand, an increase in one year of the child's age reduces the likelihood of attendance roughly 1.7 times as much as participation in Superémonos.[18] In the case of passing the school year, the result is similar: an increase in the child's age of one year has a larger (negative) marginal impact than the (positive) marginal effect of participating in Superémonos.

In terms of effect in 2002 when the children are ages 13-16 years of age, Superémonos increases the probability of attending school by 4.45 percentage points. The marginal effect is equivalent to roughly four additional years of mother's education, but only one-half year of additional age of the child. The probit analysis, however, does not detect any impact of Superémonos on the probability of working in the week prior to the survey.

[16] In the next sub-section, we report results produced by propensity score matching, another technique for controlling for these potential differences.

[17] These other variables must be not be affected by program participation in order not to produce multicollinearity among the regressors.

[18] This is the average marginal effect of one additional year. Clearly, the effect will be larger the older is the child, since the opportunity cost of continuing schooling rises with age. Thus, the size of the marginal effect of Superémonos versus the marginal effect of an additional year may constitute evidence that the amount of the food coupon is insufficient to offset the higher opportunity costs, especially at older ages.

These regression results support the differences in mean attendance rates reported in Table 2 on the basis of naïve comparison of means, but suggest a smaller marginal effect of the program. The regression results provide some evidence that the program has a positive effect on passing the grade, an effect that did not quite achieve statistical significance in the naïve comparison of means.

Propensity Score Matching

In this section we present the results from a technique called propensity score matching (henceforth, PSM). This technique is gaining increasing acceptance as a tool for program evaluation; it is appealing because it provides a way to ensure that treatment and control groups are similar not only in observable characteristics, but also in individuals' likelihood of participating in the program that is being evaluated.[19] A detailed description of the steps in implementing this methodology is provided in Appendix A; here, we note only that:

i) Each individual in the treatment group is "matched" with one or more individuals in the control group who have similar probabilities of participating in the program based on observable characteristics;[20]
ii) Matching is accomplished by estimating a limited dependent variable regression for program participation, in which both participants and non-participants are included. In this paper, the probability of participating in Superémonos was modeled using a probit equation with the following explanatory variables: types of walls, potable water, working electricity, canton, crowding of residence, home ownership, age of child, mother's years of education and the presence/absence of the mother from the residence. The predicted values for each individual (both participants and non-participants) are the a priori probabilities of participating in the program.

[19] By definition, individuals in the control group do not participate in the program. "Likelihood" in this sense is the a priori probability of participation before a participation decision is made, based on observable characteristics.
[20] If each individual in the treatment group is matched with one individual in the control group, it is termed "one-to-one" matching. It is possible to match each individual in the treatment group to more than one individual in the control group; this is termed "*n*-to-one matching", where *n* is the number of individuals in the control group paired with each observation in the treatment group. We report results from one-to-one matching in this paper, but results from five-to-one matching were quite similar.

iii) The methodology requires that the range of a priori probabilities be similar for treatment and control groups. Appendix B provides evidence that areas of common support exist for our treatment and control group.

iv) The distribution of propensity scores (probability of participation) for the treated and untreated groups have areas in common which allows for the construction of an appropriate control group. This is what is known in the PSM literature as the "areas of common support."

v) The number of observations from the control group effectively used in the analysis is smaller than in the regression analysis because the methodology matches each observation in the treatment group with the observation(s) from the control group that has or have the most similar a priori probability of participating in the Superémonos program. In other words, some observations from the control group are not utilized, and this accounts for the smaller sample size.

vi) Any observations from the treatment group that did not find a match within a five percentage-point window were thrown out; this resulted in the loss of only one observation from the treatment group for 2001.

Before presenting the results from this methodology, it is important to note one caveat: PSM requires much larger sample sizes than standard parametric methods, and our sample sizes may not always be sufficiently large enough to demonstrate significant differences across groups.

Table 7 presents the results of the one-to-one PSM analysis. Superémonos is shown to have had a statistically significant impact on attending school in 2001, raising the probability of attendance by five percentage points. No significant effect was found on the probability of passing the grade. For the 2002 school year, the matching methodology finds a significant effect on the probability of attending school—an increase of 8.7 percent—but no impact on the probability of working in the week prior to the survey. The results of the PSM analysis confirm the findings of the regression analysis regarding the impact of Superémonos on school attendance. The marginal impacts for 2001 and 2002, however, are approximately twice as large using the PSM methodology. Also consistent with the regression analysis is the finding that

participation in Superémonos has no impact on child labor. Unlike the regression analysis, however, the PSM does not find any statistically significant impact on the probability of passing the grade in 2001.

5. Conclusions

This paper examines the impact of the Superémonos program on children's school and work outcomes in Costa Rica. Table 8 provides a summary of the results from the various methodologies used in the paper. The first important conclusion is that the program is quite successful in achieving its stated goal of increasing school attendance. Depending on the methodology used, program beneficiaries are between 2.9 and 8.7 percentage points more likely to be attending school than non-beneficiaries. This impact is all the more impressive in Costa Rica, a country with quite high enrollment figures to begin with.

There is much less evidence that the program increases the likelihood that students pass the grade. In fact, only the probit regression analysis detected such an impact. Thus, we are left to conclude that Superémonos beneficiaries are just about as likely to pass the grade as similar individuals attending school but not participating in the program. We must be careful in interpreting this result; it emphatically is *not* evidence of the ineffectiveness of Superémonos in increasing the educational attainment of children; rather, it means that the increase in years of completed schooling of program beneficiaries will come from their attending school at higher rates, rather than progressing through grades at a faster pace.

While the program appears to be achieving its primary objective of raising school attendance and educational attainment among poor children, there is no evidence that the program decreases child labor. As was made clear in the introduction, reducing child labor is not an explicit objective of the Superémonos program.

How effective is Superémonos, an *in-kind* transfer program, compared to other conditional *cash* transfer programs in increasing school attendance? PROGRESA raised enrollment (not attendance) rates by between 0.74 and 1.07 percentage points for boys, and from 0.96 to 1.45 percentage points for girls at the primary level. At the secondary level, the effects ranged from 3.5 to 5.8 percentage points for boys and 7.2 to 9.3 percentage points for girls. In Nicaragua, the Red de Protección Social produced even more dramatic results: an increase in enrollment rates for primary school students of almost 22 percentage points (Rawlings and

Rubio, 2003). Our estimates for the attendance impacts of Superémonos range from 2.4 to 5.0 percent percentage points for students aged 13 to 16, corresponding in Costa Rica to the first four of the five years of secondary school for students with normal age-to-grade progression. These are slightly lower percentage point gains than reported for PROGRESA and significantly lower than for Red de Protección Social.

Does this mean that in-kind transfers are inherently less effective than cash transfers? Not necessarily. Levels of school attendance in Costa Rica are significantly higher than in either Mexico or Nicaragua; if one assumes, as is plausible, that the remaining non-attendees in Costa Rica have either higher opportunity costs or lower returns to education than their peers who are already enrolled, then it is not surprising that it becomes increasingly difficult—and expensive—to register further gains in school attendance. Any serious comparative analysis would also need to take into account the amount of the transfers and the fidelity of program execution. But more fundamentally, the most fertile laboratory for comparing in-kind to cash transfers will be within countries, rather than among countries. Inter-country comparisons of programs are simply fraught with too many confounding effects.

Can conditional transfer programs affect child labor without complementary activities such as after school programs? As the cliché goes, the jury is still out. The evidence reported here from Costa Rica is that this in-kind transfer program does not affect child labor at all. Evidence from PROGRESA in Mexico and PETI in Brazil suggest that cash transfers, combined with complementary initiatives, can significantly affect rates of child labor.[21] Might stand-alone transfer programs have some impact in rural areas and in countries where the prevalence of child labor is significantly higher than in urban Costa Rica? Perhaps, but more research will need to be conducted to document these impacts.

[21] In the case of PETI, attending after-school sessions was required which effectively limited the available time for working. Hours of work were halved, and the probability of working was reduced by at least 5 percentage points (Yap, Sedlacek and Orazem, 2002). In the case of PROGRESA, the additional interventions came primarily in the form of home visits by community promoters, nutritional interventions and health seminars. The probability of working fell by approximately 10-15 percent for beneficiaries ages 8 to 17, with larger effects found for children ages 12-15.

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Table 1. Mean Values for Treatment and Control Groups

	Superémonos participants*	Non-participants**
Number of total observations	746	1032
Average age of child	12.90	12.95
Percentage female	50.27	48.55
Percentage in San Jose	60.19	56.88
Percentage in Alajuela	6.03	4.94
Percentage in Cartago	33.78	38.18
Percentage of mothers with incomplete primary education	36.46	35.76
Percentage of households lacking working electricity	4.29	3.88

Notes:
* from IMAS list of participants.
** not on IMAS list.

Table 2. Differences in Outcomes across Treatment and Control Groups in Urban Areas of San Jose, Alajuela and Cartago: No Controls across Samples

	number of observations in control group	Number of observations In treatment group	mean of observations in control group	mean of observations in treatment group	difference in means (treatment -control)
Treatment: Received Superémonos in 2001 School Year[1]					
Attend last year (2001)	691	419	0.899	0.938	0.039**
Pass last year (2001)	615	390	0.777	0.818	0.041
Treatment: Received Superémonos Again in 2002 School Year[2]					
Attend this year (2002)	904	206	0.8385	0.8689	0.030
Work last week (2002)	904	206	0.0841	0.0922	0.008

Notes:
* Difference in mean between treatment and control group is significant at .01 level.
** Difference in mean between treatment and control group is significant at .05 level.
*** Difference in mean between treatment and control group is significant at .10 level.
[1] Students aged 12-15; control group composed of individuals who did receive Superémonos in 2001.
[2] Students aged 13-16; control group composed of individuals who did not receive Superémonos this year; they may have participated in 2001.

Table 3. Effect of Receiving Superémonos on Probability of School Attendance: 2001 Results of Probit Analysis, Children Ages 12 to 15 in Urban Areas of San Jose, Alajuela and Cartago

	coefficient	standard error	P value	marginal effect
Superémonos 2001 (d)	0.349 *	0.137	0.011	0.029
child's age	-0.573 *	0.069	0.000	-0.050
female child (d)	-0.135	0.125	0.280	-0.012
child is white (d)	0.038	0.127	0.764	0.003
mother's education	0.059 **	0.028	0.036	0.005
mother is absent (d)	-0.278	0.240	0.248	-0.030
Alajuela (d)	-0.950 *	0.267	0.000	-0.165
Cartago (d)	-0.249 ***	0.140	0.076	-0.023
Crowding	-0.185 *	0.053	0.000	-0.016
potable water (d)	-0.054	0.244	0.825	-0.005
bathroom internal (d)	-0.137	0.132	0.299	-0.012
running electricity (d)	0.395	0.362	0.276	0.048
total rooms	-0.064	0.075	0.390	-0.006
block walls (d)	-0.112	0.190	0.555	-0.010
prefab walls (d)	-0.171	0.200	0.391	-0.016
other type walls (d)	-0.438 **	0.214	0.041	-0.051
home owner (d)	-0.222	0.165	0.179	-0.018
parent has a home business (d)	0.307	0.247	0.213	0.022
minutes to the bus stop	0.021	0.014	0.143	0.110
minutes to reach primary school	-0.001	0.001	0.297	-0.004
minutes to reach secondary school	-0.005 ***	0.002	0.051	-0.025
minutes to bus imputed	0.424	0.758	0.576	0.026
minutes to secondary school imputed	-0.023	0.389	0.953	-0.002
constant	10.615 *	1.318	0.000	

Number of observations	1109
Pseudo R-squared	0.226
Mean of dependent variable	0.913

Notes:
* Significant at .01 level.
** Significant at .05 level.
*** Significant at .10 level.
Observations in which time reported was missing were imputed at average of non-missing variables.
A dummy variable (imputed) captures whether imputed values were used.
Marginal effects for time variables were calculated per hour, rather than per minute.
(d) indicates that the variable is a dummy variable.

Table 4. Effect of Receiving Superémonos on Probability of Passing School Year: 2001 Results of Probit Analysis, Children Ages 12 to 15 in Urban Areas of San Jose, Alajuela and Cartago

	coefficient	standard error	P value	marginal effect
Superémonos 2001 (d)	0.180 ***	0.098	0.067	0.048
child's age	-0.226 *	0.044	0.000	-0.062
female child (d)	0.104	0.094	0.269	0.028
child is white (d)	-0.157 ***	0.094	0.095	-0.043
mother's education	-0.011	0.020	0.578	-0.003
mother is absent (d)	0.020	0.206	0.922	0.005
Alajuela (d)	-0.258	0.229	0.259	-0.077
Cartago (d)	-0.135	0.103	0.189	-0.037
crowding	-0.080 ***	0.044	0.066	-0.022
potable water (d)	-0.005	0.192	0.980	-0.001
bathroom internal (d)	0.019	0.113	0.868	0.005
running electricity (d)	0.715 **	0.284	0.012	0.244
total rooms	-0.009	0.055	0.870	-0.002
block walls (d)	0.090	0.133	0.499	0.024
prefab walls (d)	-0.126	0.139	0.363	-0.035
other type walls (d)	-0.015	0.165	0.925	-0.004
home owner (d)	-0.087	0.116	0.452	-0.023
parent has a home business (d)	-0.158	0.155	0.307	-0.045
minutes to the bus stop	-0.005	0.008	0.487	-0.060
time to reach primary school	-0.001	0.001	0.346	-0.013
time to reach secondary school	0.005 **	0.003	0.030	0.089
time to bus imputed	-0.716	0.719	0.319	-0.246
time to secondary school imputed	0.253	0.360	0.482	0.062
constant	3.712 *	0.854	0.000	

Number of observations	1004
Pseudo R-squared	0.061
Mean of dependent variable	0.913

Notes:
* Significant at .01 level.
** Significant at .05 level.
*** Significant at .10 level.
Observations in which time reported was missing were imputed at average of non-missing variables. A dummy variable (imputed) captures whether imputed values were used. Marginal effects for time variables were calculated per hour, rather than per minute.
(d) indicates that the variable is a dummy variable.

Table 5. Effect of Receiving Superémonos on School Attendance: 2002 Results of Probit Analysis, Children Ages 13 to 16 in Urban Areas of San Jose, Alajuela and Cartago

	coefficient	Standard error	P value	Marginal effect
Superémonos 2002 (d)	0.250 ***	0.137	0.068	0.044
child's age	-0.422 *	0.050	0.000	-0.082
female child (d)	-0.123	0.101	0.225	-0.024
child is white (d)	-0.066	0.102	0.521	-0.013
mother's education	0.050 **	0.023	0.029	0.010
mother is absent (d)	-0.281	0.201	0.162	-0.063
Alajuela (d)	-0.302	0.256	0.240	-0.068
Cartago (d)	-0.516 *	0.110	0.000	-0.108
crowding	-0.120 *	0.044	0.007	-0.023
potable water (d)	-0.177	0.178	0.322	-0.034
bathroom internal (d)	-0.070	0.109	0.524	-0.014
running electricity (d)	0.382	0.290	0.187	0.091
total rooms	-0.023	0.060	0.699	-0.004
block walls (d)	-0.055	0.149	0.713	-0.011
prefab walls (d)	-0.136	0.155	0.380	-0.027
other type walls (d)	-0.310 ***	0.172	0.071	-0.068
home owner (d)	-0.171	0.131	0.193	-0.032
parent has a home business (d)	0.016	0.179	0.928	0.003
minutes to the bus stop	0.008	0.006	0.226	0.090
minutes to reach primary school	0.001 ***	0.001	0.069	0.016
minutes to reach secondary school	-0.005 *	0.002	0.008	-0.001
minutes to bus (imputed)	-0.504	0.669	0.451	-0.128
minutes to secondary school (imputed)	-0.634 **	0.285	0.026	-0.168
constant	7.874 *	0.953	0.000	

Number of observations	1109
Pseudo R-squared	0.15 9
Mean of dependent variable	0.84 4

Notes: * Significant at .01 level; ** Significant at .05 level; *** Significant at .10 level. Observations in which time reported was missing were imputed at average of non-missing variables. A dummy variable (imputed) captures whether imputed values were used. Marginal effects for time variables were calculated per hour, rather than per minute. (d) indicates that the variable is a dummy variable.

Table 6. Effect of Receiving Superémonos on Probability of Child Working in Reference Week: 2002 (Results of Probit Analysis, Children Ages 13 to 16 in Urban Areas of San Jose, Alajuela and Cartago)

	coefficient	standard error	P value	marginal effect
Superémonos 2002 (d)	0.076	0.152	0.619	0.009
child's age	0.318 *	0.059	0.000	0.035
female child (d)	-0.515 *	0.128	0.000	-0.057
child is white (d)	0.241 ***	0.124	0.052	0.026
mother's education	-0.066 **	0.027	0.014	-0.007
mother is absent (d)	-0.055	0.258	0.831	-0.006
Alajuela (d)	-0.042	0.332	0.898	-0.005
Cartago (d)	0.376 **	0.131	0.004	0.045
crowding	0.015	0.057	0.789	0.002
potable water (d)	0.373 **	0.186	0.044	0.041
bathroom internal (d)	-0.013	0.135	0.924	-0.001
running electricity (d)	-0.541	0.336	0.108	-0.088
total rooms	0.054	0.073	0.459	0.006
block walls (d)	0.042	0.171	0.805	0.005
prefab walls (d)	-0.029	0.180	0.872	-0.003
other type walls (d)	-0.177	0.221	0.424	-0.018
home owner (d)	0.049	0.155	0.753	0.005
parent has a home business (d)	0.575 *	0.173	0.001	0.091
minutes to the bus stop	-0.008	0.007	0.249	-0.054
time to reach primary school	-0.001	0.001	0.327	-0.006
time to reach secondary school	0.006 *	0.002	0.010	0.038
time to bus imputed	1.502 ***	0.776	0.053	0.402
time to secondary school imputed	0.420	0.353	0.233	0.063
constant	-6.150 *	1.106	0.000	

Number of observations	1109
Pseudo R-squared	0.163
Mean of dependent variable	0.086

Notes: * Significant at .01 level; ** Significant at .05 level; *** Significant at .10 level.
Observations in which time reported was missing were imputed at average of non-missing variables. A dummy variable (imputed) captures whether imputed values were used. Marginal effects for time variables were calculated per hour, rather than per minute.
(d) indicates that the variable is a dummy variable.

Table 7. Effect of Receiving Superémonos on Attendance, Passing Grade and Child Labor: Urban Areas of San Jose, Alajuela and Cartago Results of One-to-One Propensity Score Matching

	number of observations in treatment group	number of unmatched observations	mean of matched obs. In control group	marginal effect of Superémonos	standard error	T test
2001 [1]						
Attend last year	419	1	0.888	0.050**	0.025	2.036
Pass last year	419	1	0.782	0.035	0.036	0.987
2002 [2]						
Attend this year	206	0	0.782	0.087**	0.042	2.103
Work last week	206	0	0.073	0.019	0.030	0.642

Notes: ** Difference in mean between treatment and control group is significant at .05 level.
[1] Beneficiaries aged 12 to 15; Control group composed of individuals who did not receive Superémonos last year.
[2] Beneficiaries aged 13 to 16; Control group composed of individuals who did not receive Superémonos this year; they may have participated in 2001.

Table 8. Marginal Impacts of Superémonos on Outcomes of Interest

	Attend school 2001	2002	Pass 2001 school year	Work last week
Naïve means comparison	3.9 ppts	--	--	**--**
Regression analysis	2.9 ppts	4.4 ppts	4.8 ppts	**--**
Propensity score matching	5.0 ppts	8.7 ppts	--	**--**

Notes:

Impacts are difference in means (treatment minus control) in the case of naïve means comparison and propensity score matching. For the probit regression analysis the impacts are the predicted marginal effects. All impacts are measured in terms of percentage points.

-- indicates no statistically significant effect.

Appendix A. Steps in Propensity Score Matching

Propensity score matching (PSM) is a technique used to form control groups that closely resemble treatment groups. Rather than match on just one or two key characteristics (e.g., age, sex), PSM attempts to form a control group in which the a priori likelihood of participating in the evaluated program is similar between the two groups.

The main steps in PSM are the following:

- Collect a representative sample of program participants and non-participants. The sample of non-participants is typically significantly larger than that of participants, in order to facilitate finding good matches.
- Pool the two samples (participants and non-participants) and estimate a logit or probit model of program participation, using all relevant explanatory variables.
- Create the predicted values of program participation; these are called "propensity scores." There will be propensity scores for all individuals in the data set, whether participant or non-participant.
- Exclude any individuals from the non-participant sample whose predicted probability of participation is outside the range of probabilities found in the participant sample. (Typically, observations are excluded because the estimated participation probabilities are too low.)
- For each individual in the treatment sample (participants), identify the individual or individuals in the control sample (non-participants) with the closest estimated probabilities. If one individual is selected in the control sample for each individual in the treatment sample, this is termed "one-to-one matching." If more than one individual in the control sample is matched to each individual in the treatment sample, this is termed "*n*-to-one matching." The most common value of *n*-to-one matching is five-to-one. The matched observation(s) in the control sample are termed "nearest neighbor(s)".
- Calculate the mean value of the outcome indicator or indicators (e.g., attend school, pass grade, work) for the nearest neighbor(s). The

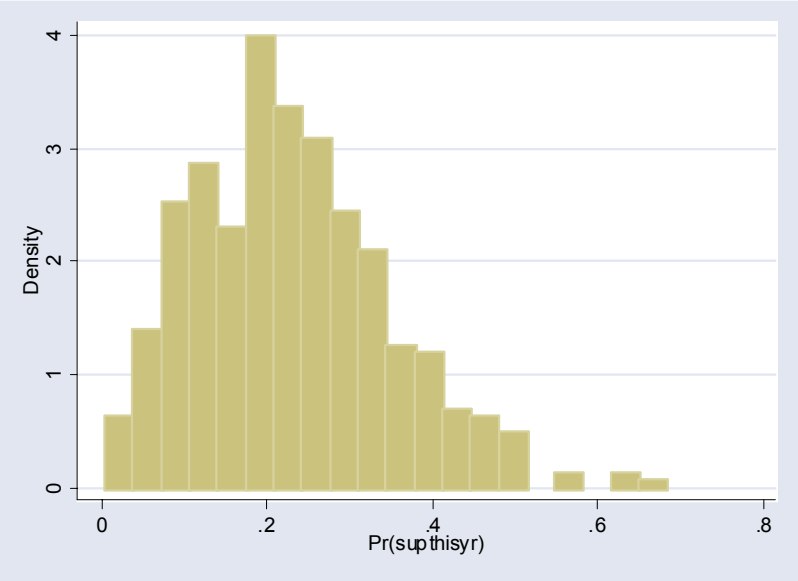
difference between this mean and the corresponding value for the individual in the treatment sample is the estimate of the gain or loss due to program participation for that individual.

Calculate the mean of all these individual gains or losses to obtain the overall mean gain or loss.

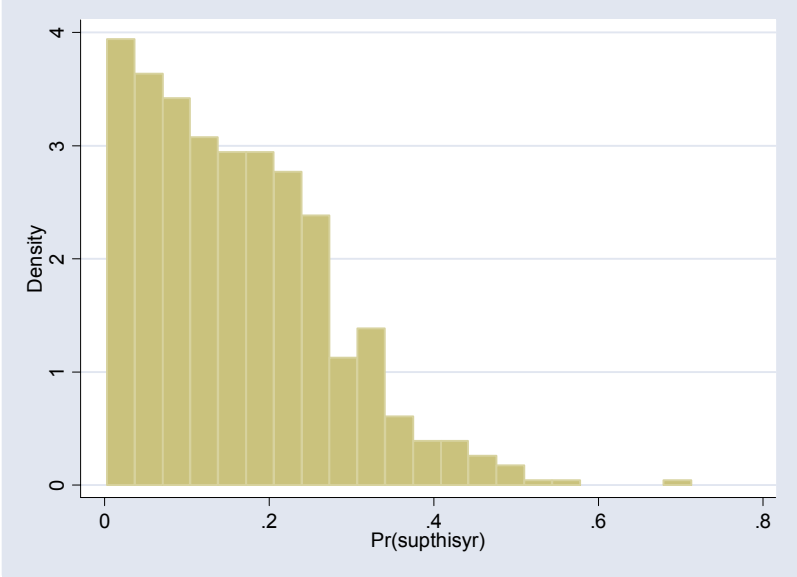
Source: World Bank (2000).

Appendix B. Areas of Common Support for Treatment and Control Groups

Distribution of Propensity Score Among Treated (received Superémonos in 2001)
(The propensity score is the probability of receiving Superémonos in 2001.)



Distribution of Propensity Score Among Untreated (did not receive Superémonos in 2001)



Note: The matched control group is drawn from this group.