

**Evaluating the Impact of Conditional Cash Transfers on Schooling in  
Honduras: An experimental approach**

Paul Glewwe (University of Minnesota)

Pedro Olinto (IFPRI-FCND)

Priscila Z. de Souza (EPGE-FGV)

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## **I. Introduction**

The Programa de Asignacion Familiar (PRAF) is one of the largest government social welfare programs in Honduras, the third poorest country in Latin America and the Caribbean (in terms of per capita GNP). PRAF was initiated in 1990 as a social safety net to compensate the poor for lost purchasing power brought about by macroeconomic adjustment. It was restructured in 1998, and now includes a reformulated project known as PRAF/IDB - Phase II (henceforth referred to as PRAF II). The objective of this project is to encourage poor households to invest in their family's education and health by providing incentives to increase primary school enrollment, the use of preventive health care services, and the quality of both education and health-care services.

An unusual and important feature of the PRAF II project is that it includes a monitoring and evaluation component managed by the International Food Policy Research Institute (IFPRI) to assess the program's impact over time. This paper is the first draft of a study that will evaluate the impact of PRAF II on education outcomes in poor communities in Honduras.

This paper has the following outline. The next section explains the PRAF II program and the data that have been collected so far. Section III provides some descriptive statistics from data collected in 2000 from 70 poor municipalities in Honduras. An analysis of the impact of the program on school attainment is presented in Section IV. Finally, Section V provides brief concluding remarks.

## **II. Description of PRAF II and Data**

Honduras's Family Allowances Program (PRAF) began operation in 1990. The program was initially intended to be a temporary program to ease the burden of macroeconomic adjustment on poor households. PRAF was originally a cash transfer program, distributing cash grants from health centers and schools. Continued widespread poverty in Honduras extended the time horizon of PRAF beyond the initial macroeconomic adjustment period. In addition, the objective of the program was changed; instead of only alleviating poverty it was now given the task of eradicating the root causes of poverty. The root causes were seen as lack of human capital among poor

families, so the program was modified to provide assistance that would enable those families to increase their human capital, particularly the health and education of children in poor families.

### **A. The PRAF II Program**

In 1998, the Honduran government modified PRAF to redirect it toward these new objectives. This modified program will be referred to as PRAF II in the rest of this paper. PRAF II has the following specific objectives: (i) boost the demand for education services; (ii) encourage the “education community” to take part in children's learning development; (iii) instruct mothers in feeding and hygiene practices; (iv) ensure that sufficient money is available for a proper diet; (v) promote demand for, and access to, health services for pregnant women, nursing mothers and children under age 3; and (vi) ensure timely and suitable health care for PRAF beneficiaries. More generally, the objective of PRAF II is to increase the health and education of Honduran children in poor rural communities.

PRAF II has the following distinctive features: (i) a new system for selecting beneficiaries; (ii) interventions to stimulate the demand for and the supply of education, nutrition and health services; (iii) baseline data collection and subsequent annual data collection to measure outcomes and progress under the program; (iv) assessment of the program based on randomized treatment and control groups. PRAF II is being piloted in 70 of the poorest municipalities in Honduras. The municipalities were selected in October, 1999, and the interventions began in 2000.

To estimate the impacts of both the supply side and the demand side interventions, IFPRI designed an evaluation procedure in which the 70 municipalities were assigned randomly to four different groups, designated as G1 – G4:

G1 = Demand side intervention only (20 municipalities)

G2 = Demand and supply side interventions (20 municipalities)

G3 = Supply side intervention only (10 municipalities)

G4 = Control group without intervention (20 municipalities).

This design allows for measurement of the impact of the demand side interventions, the supply side interventions, and of both interventions.

In each municipality in groups G1-G3, both health and education projects are implemented. Both health and education interventions take two distinct forms: a) demand side incentives (cash transfers) conditioned on school attendance and frequent health clinic visits by the recipient; and b) supply side investments, aimed at improving the quality of schooling and health services supplied in poor rural areas. On the health side, the demand side consists of monetary transfers to pregnant women and to mothers of children under three years of age. The voucher is provided only for women who have visited health clinics every month as required by the program. Each family may receive up to three health vouchers per month, each worth approximately US\$4.

The supply side of the health component consists of monetary transfers to primary health care teams, which is formed by members of the community and local health care workers (nurses and doctors when available). To receive the transfers, each team needs to prepare a plan with specific tasks and a budget specifying what equipment and medicine will be purchased for the health center. Each team receives in average US\$6,000 per year, but the amount varies between US\$3,000 and US\$15,000, depending on the size of the population served by the health center.

To promote education, the demand incentive is generated using monetary payments that are received only if children age 6-12 enrolled in the first four years of primary school are actually attending school. A maximum of up to three children per family are eligible. Each receive approximately US\$5 per month; therefore a maximum of US\$15 per month is given to families. To be eligible, children need to be enrolled in school by the end of March, and need to maintain an attendance rate of 85%.

The supply side intervention is payments to Parent Teacher Associations organized around each primary school. The associations need to obtain legal status, and need to prepare plans to improve the quality of the education provided by their respective schools. The plan needs to include a budget for the list of equipment and materials selected from a menu pre approved by PRAF II. Each PTA receives in average US\$4,000 per year, with the amount varying between US\$1,600 and US\$23,000 depending on the size of the school.

## **B. Data**

After the 70 poor municipalities were chosen, baseline data were collected from all 70 before the PRAF II program was implemented in the municipalities in groups G1, G2 and G3. The baseline data were collected in 2000 from mid-August to mid-December. The follow up survey was conducted in 2002, from mid May to mid-August. From each municipality, eight communities (“clusters”) were randomly selected, and from each cluster 10 dwellings were randomly selected. Assuming one household per dwelling, this implies a total baseline sample of 5600 households. In fact, some of these dwellings had more than one household, so the total number of households selected in the baseline was 5784. In most cases, each group of 10 dwellings is found within a different village (aldea) of the municipality, but in some cases two or more groups of 10 are from the same village. These 5784 households contained a total of 30,588 members.

In the follow up 2002 survey, an attempt was made to interview households that moved away from where they were first interviewed in 2000. Also, new households derived from original baseline households were interviewed whenever they contained members that were in the initial survey and who belonged to the program’s targeted population (pregnant women, lactating mothers and children 0-16). As seen in table 7, approximately 84% of the households interviewed in 2000 were interviewed again in 2002, and approximately 3.6% of the original households spun off at least one new household. These figures are very similar across the four treatment groups. Therefore, we do not expect severe attrition bias in our impact estimates below.

Three kinds of data were collected. First, a household questionnaire was administered to all households from August to December, 2000. That questionnaire collected data on: a) housing; education and employment of all household members; b) education (very detailed) of all children age 6-16; c) the health of all women who were pregnant in the past 12 months; d) the health of all children below three years of age (and height, weight and hemoglobin information for all children under five years); e) consumption expenditures on food and non-food items; f) access to credit; g) remittances received from household members who have moved away; h) receipt of assistance from various government and private agencies (including from PRAF II); i) ownership of livestock and durable goods; j) time spent by the “woman of the house” and by children

age 6-12 doing various activities; and k) households' evaluation of the quality of local health centers and primary schools.

Second, data were collected on community characteristics in each of the 560 clusters. The data include: a) whether the community has a primary school, a public hospital or public transport; b) daily wage rates for local agricultural and non-agricultural work; c) the availability of work away from the community; d) a small amount of information on local crime; and e) prices for a large number of food items and the going daily wage rate.

Third, questionnaires were administered to primary schools in each of the 70 municipalities. Three schools were randomly selected from each municipality, totaling 210 schools. The school questionnaire collected the following data: a) general information on the school (days open, number of grades, etc.); b) characteristics of teachers; c) pedagogical aids (library books, dictionaries, paper etc.); and d) school organizations (PTA, teachers association, etc.).

### **III. Education Outcomes from the 2000 Baseline Data**

This section presents some basic data on the education outcomes of children of primary school age, age 6-12. This is done to set the stage for the analysis in Section IV of the impact of the program on years of schooling for children in this age group.

Table 1 shows the ages by which children of different ages have started primary school. Although the official policy of Honduras' Ministry of Education is that children should begin school at age 6, only 21% of 6-year-old children from these 70 poor municipalities have started schooling by that age. Yet by the time they are seven years old, most children have started primary school; 72% have done so by that age. By age 10, 95% of these children have started primary school, and this percentage peaks at about 96% at age 12. Thus only about 4% of the children in these poor municipalities never attend school, but delayed enrollment in primary is a common phenomenon. The table also provides this figures by sex; girls are slightly more likely to start on time, but the difference is not very large.

Table 2 shows current enrollment for all children from ages 6-16. The older children are included to show the ages at which children drop out. For all children in

these 70 municipalities, enrollment peaks at 86-87% at ages 8-10, after which it steadily declines so that only about 15% of 16-year-olds are enrolled in school. (Note, ages 6 and 7 include some children who are still enrolled in pre-school..) Even by age 13 only about half of these children are still enrolled in school. Girls are more likely to be enrolled in school at all ages, but the difference is not very large.

The tables examined so far show that almost all (95%) of Hondurans children in these poor municipalities do enroll in primary school, yet they often start their schooling 1-2 years late and they do not stay in school very long. In particular, about half have dropped out by age 13 and about 85% have dropped out by age 16. This implies that these children complete only a few years of schooling before leaving school. This is shown in Table 3, which shows years of schooling completed for children who are 16 years old. About 15% have completed a single year of primary school, and about one fourth completed only 1-3 years of schooling. A common rule of thumb is that four or more years of schooling are needed before someone acquires and retains basic literacy skills, which implies that about 40% of the children in these communities will be illiterate when they become adults. About half (48%) attain 4-6 years of schooling, and only 12% attain seven or more years (i.e. secondary school). Clearly, school attainment in these Honduran communities is quite low and fully justifies programs to increase it.

The next three tables examine patterns that may explain why children are not enrolled in school. Table 4 examines, for children age 6-16, whether children have started primary school and whether they are currently enrolled in school, by income levels. More precisely, children were divided into four groups of equal size according to household consumption expenditures per capita. The poorest 25% are called quartile 1, the next poorest 25% are quartile 2, and so on. For the poorest quartile, only 79% have started primary school and only 55% are currently enrolled in school. These numbers increase steadily for wealthier groups, until at the fourth quartile (wealthiest 25%) 89% of these children have started primary school and 74% are still in school. This suggests, as one would expect, that better off households are more likely to enroll their children in school.

Another factor which could have a strong impact on children's schooling outcomes is their parents' level of education, especially mothers' education. Table 5

looks at starting primary school and current school attendance when children are classified according to their mothers' level of education. About one third (36%) of children age 6-16 have mothers with no education at all. About 80% of those children had started primary school, and only 57% were currently attending school. Children whose mothers had some primary school education (1-5 years) fared better; 85% had started primary school and 65% were currently attending school. Children whose mothers had completed primary schooling (about 12% of the sample) performed best of all, with 87% starting primary school and 82% currently in school. Somewhat surprisingly, the small number of children whose mothers report higher than primary education (0.4% of the sample) did not do as well as mothers who had some or complete primary education; this could reflect the small sample size or it may simply indicate that these women were misclassified.

Another potentially important determinant of school progress is the distance to the nearest primary school. All households were asked how long it would take them to walk to the primary school that is nearest to their homes. Two thirds (68%) of the children live in households where this travel time (one way) is 15 minutes or less. About one fourth (23%) live in households where the travel time is 16-30 minutes, while another 6% have travel times of 31-60 minutes and 2% report travel times of more than one hour (the largest was five hours). Table 6 examines the relationship between travel time, starting primary school and current school attendance. About 85% of children who live within 15 minutes of a primary school have started primary school. This figure drops to 82% for children with travel times of 16-30 minutes and 74% for children with times of 31-60 minutes, and then increases slightly to 78% for children with times exceeding one hour. Current school enrollment has a clear monotonic relationship, ranging from 66% for children within 15 minutes of a primary school to 43% for children whose travel times exceed one hour.

#### **IV. Empirical Methods: Estimating the impact of PRAF II on Schooling**

The central problem in the evaluation of any social program is the fact that households participating in the program cannot be simultaneously observed in the alternative state of no treatment. To illustrate, let  $Y_i$  be the outcome for a given individual

or household in the treated state (i.e., during or after participation in the program) and  $Y_0$  is the outcome in the untreated state (i.e., without participating in the program). Then the gain for any given individual or household from being treated by the program is  $\Delta = (Y_1 - Y_0)$ . However, at any time a person is either in the treated state, in which case  $Y_1$  is observed and  $Y_0$  is not observed, or in the untreated state, in which case  $Y_1$  is unobserved and  $Y_0$  is observed. Given that missing  $Y_1$  or  $Y_0$  preclude measurement of this gain for any given individual, one has to resort to statistical methods as a means of addressing this problem (e.g., see Heckman, LaLonde and Smith, 1999). The statistical approach to this problem replaces the missing data on persons using group means or other group statistics, such as medians.

For example, the majority of the studies on evaluation of social programs focus on the question of whether the program changes the mean value of an outcome variable among participants compared to what they would have experienced if they had not participated. The answer to this question is summarized by one parameter called the “the mean direct effect of treatment on the treated.” Using formal notation, the mean effect (denoted by the expectation operator  $E$ ) of treatment on the treated (denoted by  $T=1$ ) with characteristics  $X$  may be expressed as

$$E(\Delta | T = 1, X) = E(Y_1 - Y_0 | T = 1, X) = E(Y_1 | T = 1, X) - E(Y_0 | T = 1, X). \quad (1)$$

The term  $E(Y_1 | T = 1, X)$  can be reliably estimated from the experience of program participants. What is missing is the mean counterfactual term  $E(Y_0 | T = 1, X)$  that summarizes what participants would have experienced had they not participated in the program.

The variety of solutions to the evaluation problem differ in the method and data used to construct the mean counterfactual term  $E(Y_0 | T = 1, X)$ . For example, one approach used frequently to evaluating social programs is based on the notion that all that is needed is repeated observations on a set of households before and after the start of a program. Thus observations on the same households before the implementation of a social program can be used to estimate  $E(Y_0 | T = 1, X)$ . Another approach is that of social

experimentation or randomization of individuals into treatment and control groups. Experimental designs use information from individuals or households in the control group to construct an estimate of what participants would have experienced had they not participated in the program, i.e., the term  $E(Y_0 | T = 1, X)$ .<sup>1</sup>

The empirical framework adopted by the PRAF II administration for the purposes of evaluating the program's impact offers a very flexible approach to solving the evaluation problem. Its advantages are derived from two key features. Firstly, it is an experimental design with randomization of municipalities, rather than households or individuals, into treatment and control groups.<sup>2</sup> Secondly, data are collected from all households in both treated and control localities before and after the start the treatment. The combination of these two features permit researchers to evaluate the “mean direct effect of treatment on the treated” or in other words the impact of the program on program participants using any of the estimators available in the evaluation literature, including the before-after estimator, the difference-in-differences estimator, and the first-difference or (cross-sectional) estimator discussed in more detail below.

Given the experimental design one may then construct all of the estimators commonly used in program evaluation. These are:

- i) the cross-sectional difference estimator (CSDIF) that compares differences in the means of the outcome variable Y between groups A and B during the periods after the implementation of the program (i.e.,  $t=1,2,3,\dots$ )

$$CSDIF = [E(Y(t) | T = 1, E = 1)] - [E(Y(t) | T = 0, E = 1)] \text{ for } t=1,2,3,\dots \quad (2)$$

and,

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<sup>1</sup> For a more thorough discussion of the various solutions to the evaluation problem see Heckman, LaLonde, and Smith (1999).

<sup>2</sup> Valadez and Bamberger (1994) provide a detailed description of the elements of a quasi-experimental approach to program evaluation. For a review of evaluations of social sector programs using randomized control designs in Mexico and other countries see Newman et al. (1994).

- ii) the double differences or difference-in-differences estimator (2DIF) that measures program impact by comparing differences in the means of the outcome between group A and B in post survey rounds with the differences in the mean the means of the outcome between group A and B in the pre –program round. Formally,

$$2DIF = [E(Y(t = 1) | T = 1, E = 1)] - [E(Y(t = 1) | T = 0, E = 1)] - [E(Y(t = 0) | T = 1, E = 1)] + [E(Y(t = 0) | T = 0, E = 1)]. \quad (4)$$

Each of these estimators has some advantages and shortcomings associated with it. However, the 2DIF estimator in comparison to the CSDIF estimators is the preferred estimator for program evaluation. For example, one major advantage of the 2DIF estimator over CSDIF in evaluating the mean direct effect of treatment on the treated is that the former controls for any pre-existing differences in the expected value of Y between households in treatment and control localities. Measuring program impact based exclusively on post-program difference in the mean level of the outcome indicator between treatment and control localities, as done by the first difference estimator, may lead to potentially misleading conclusions about program impact.

Ultimately, the extent to which the CSDIF estimator may lead to biased results depends critically on whether the selection of treatment and control localities was indeed random. Pure and proper randomization of the selection of localities would ensure that there are no significant pre-program differences in the outcome variable of interest between treatment and control localities, i.e.,

$$[E(Y(t = 0) | T = 1, E = 1)] = [E(Y(t = 0) | T = 0, E = 1)]. \quad (6)$$

Satisfaction of condition (5) also ensures that CSDIF=2DIF. In other words, randomization implies that focusing exclusively on post-program comparisons between treatment and controls yields unbiased conclusions about the impact of the program.

In practice which of these two estimators is feasible depends on whether data on an outcome indicator is available not only after but also before the start of the program.

For most of the key outcome indicators of interest such as school enrollment and attendance, data are available before and after the start of the program that permit implementation of the 2DIF estimator. For some indicators, however, such as school desertion and labor force participation, observations in the baseline were affected by severe seasonality due to the coffee harvest that starts in October and November. For these outcome indicators, the CSDIF estimator provides the best available option for evaluating PRAF II.

### **Program Evaluation Within the Regression Framework**

The discussion so far focused on evaluating PRAF II without making any adjustments for the role of observed characteristics of the individual, the household and the locality on the variation in the observed value of the outcome indicator of interest. Without any adjustment for the role of such confounding factors all the differences in the mean value of the outcome indicator are attributed to the program. As a matter of principle it is preferable to have an estimate of the impact of the program net of the influence of these observed characteristics on the difference in the mean value between treatment and control households.

We also assume that the objective of the regression analysis is to obtain a consistent and unbiased estimate of the effect of the program on households or individuals eligible for the program. WE do not address the issue of take up, although to the extent that take up differs a lot from eligibility biases from selectivity may be at work.

We begin with the case where there are data available for treatment and control households before and after the start of the program.<sup>3</sup> Restricting the sample to eligible households only, the various estimators for program evaluation discussed above that

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<sup>3</sup> We assume only one observation after the start of the program for expositional simplicity only. More than one round of observations after the start of the program can be easily accommodated by including an additional binary variable (say R3) along its interaction with the treatment dummy (R3\*T). Then the coefficient of the (R3\*T) term is an estimate of the 2DIF program impact estimate in the third round of the survey and the can yield information on whether the impact of the program is strengthened or weakened over time (e.g., Schultz 2000; Parker and Skoufias, 2000).

control for individual, household and locality observed characteristics can be obtained by estimating a regressions equation of the form

$$Y(i,t) = \alpha + \beta_T T(i) + \beta_R R2 + \beta_{TR} (T(i) * R2) + \sum_j \theta_j X_j + \eta(i,v,t), \quad (7)$$

where  $Y(i,t)$  denotes the value of the outcome indicator in household (or individual)  $i$  in period  $t$ ,  $\alpha$ ,  $\beta$ ,  $\gamma$  and  $\theta$  are fixed parameters to be estimated,  $T(i)$  is a binary variable taking the value of 1 if the household belongs in a treatment community and 0 otherwise (i.e., for control communities),  $R2$  is a binary variable equal to 1 for the second round of the panel (or the round after the initiation of the program) and equal to 0 for the first round (the round before the initiation of the program),  $\mathbf{X}$  is a vector of household (and possibly village) characteristics and  $\eta$  is an error term summarizing the influence random disturbances.

To better understand the preceding specification it is best to divide the parameters into two groups: one group summarizing differences in the conditional mean of the outcome indicator before the start of the program (i.e.,  $\alpha$ ,  $\beta_T$ ,) and another group summarizing differences after the start of the program (i.e.,  $\beta_R$ , and  $\beta_{TR}$ ). Specifically, the coefficient  $\beta_T$  allows the conditional mean of the outcome indicator to differ between eligible households in treatment and control localities before the initiation of the program whereas the rest of the parameters allow the passage of time to have a different effect on households in treatment and control localities. For example, the combination of parameters  $\beta_R$  and  $\beta_{TR}$  allow the differences between eligible households in treatment and control localities to be different after the start of the program.

One advantage of this specification is that the t-values associated with some of these parameters provide direct tests of a number of interesting hypotheses. For example, the t-value associated with the estimated value  $\beta_T$  provides a direct test of the equality in the conditional mean of  $Y$  between treatment and control before the initiation of the program and serves the role of a test of the randomness in selection of localities. For if there were a truly random selection of localities into control and treatment, then the

conditional mean of the outcome indicator should be the identical across treatment and control households/individuals.

Specifically, given the preceding specification, the conditional mean values of the outcome indicator for treatment and control groups before and after the start of the program are as follows:

$$[E(Y | T = 1, R2 = 1, \mathbf{X})] = \alpha + \beta_T + \beta_R + \beta_{TR} + \sum_j \theta_j X_j \quad (8a)$$

$$[E(Y | T = 1, R2 = 0, \mathbf{X})] = \alpha + \beta_T + \sum_j \theta_j X_j \quad (8b)$$

$$[E(Y | T = 0, R2 = 1, \mathbf{X})] = \alpha + \beta_R + \sum_j \theta_j X_j \quad (8c)$$

$$[E(Y | T = 0, R2 = 0, \mathbf{X})] = \alpha + \sum_j \theta_j X_j \quad (8d)$$

According to the preceding specification, the cross-sectional difference estimator is given by the expression:

$$\begin{aligned} CSDIF &= (a - c) = \\ [E(Y | T = 1, R2 = 1, \mathbf{X}) - E(Y | T = 0, R2 = 1, \mathbf{X})] &= \beta_T + \beta_{TR}. \end{aligned} \quad (9)$$

while the before-and-after difference estimator is given by

$$\begin{aligned} BADIF &= (a - b) = \\ [E(Y | T = 1, R2 = 1, \mathbf{X}) - E(Y | T = 1, R2 = 0, \mathbf{X})] &= \beta_R + \beta_{TR}. \end{aligned} \quad (10)$$

Expression (9) describing the CSDIF estimator highlights the fact that the estimated impact of the program is inclusive of any pre-program differences between treatment and control groups (summarized by the presence of the  $\beta_T$  term). Along similar lines, expression (10) indicates that the BADIF estimator is inclusive of any trend

or aggregate effects in the changes of the outcome indicator  $Y$  (summarized by the presence of the  $\beta_R$  term).

The advantage offered by the difference in differences (2DIF) estimator is that it provides an estimate of the impact of the program that is net of any pre-program differences between treatment and control households and/or any time trends or aggregate effects in changes of the values of the outcome indicator.<sup>4</sup> By comparing before and after differences between treatment and control households (or differences between treatment and control households after and before the program) one is able to get an estimate of the impact of the program (summarized by the single parameter  $\beta_{TR}$ ).

$$2DIF = (a - b) - (c - d) = (a - c) - (b - d) = \beta_{TR} \quad (11)$$

Using the terminology of Heckman et al. (1999), the parameter  $\beta_{TR}$  provides an estimate of the “mean direct effect of treatment on those who take the treatment.” It should also be noted the program effect summarized by the parameter  $\beta_{TR}$  is inclusive of the role that the operational efficiency or inefficiency with which the program operates. It is likely that persistent delays in the processing of forms in some municipalities and other administrative bottlenecks may lead to weaker impacts of the program on households residing in these municipalities relative to that in municipalities where the program is operating more efficiently.

In this paper, the vector  $X$  typically consists of variables characterizing the age and gender, and grade in which students currently attend.. Given that the vector  $X$  does not contain any supply-related variables,  $\beta_{TR}$  is an estimate of the impact of the conditional cash transfers (demand-side effects) and the improvements in the quantity and quality (or supply-side effects) of educational and health services and facilities associated with the PRAF II program.

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<sup>4</sup> It is important to take note, however, that these advantages of the 2DIF estimator are the result of some implicit assumptions.

## Estimating Longer-Term Impact: A Markov Schooling Transition Model

There are many schooling indicators that can be affected by the program, including effects on the initial age of school entry, dropout rates, grade repetition rates and the rate of school reentry among dropouts. To analyze the experimental program impact along these various dimensions, we adopt a Markov schooling transition model. This transition model provides a convenient framework to study the long-term impact of program participation on school attainment.

Let  $f_{g,a}$  be the proportion of children of age  $a$  enrolled in grade  $g$ ,  $f_{ne,a}$  the proportion of children of age  $a$  never enrolled and  $f_{drop,a}$  the proportion of children of age  $a$  that were enrolled at school in the past, but dropped out. For six-year-old children, there are three possible schooling states (not yet enrolled, enrolled in grade one, or enrolled in grade 2) with the majority of six-year-olds enrolled in grade one. For seven-year-old children, there are six possible schooling states (not yet enrolled, dropped out after being enrolled as a six-year-old, enrolled in grade one, enrolled in grade 2, and enrolled in grade three) with the majority of seven-year-olds enrolled in grade two.

At age  $a$ , the probability of moving from state  $i$  to state  $j$  is given by  $p_{ij,a}$ . Therefore, the transition from age 6 to 7 is given by:

$$\begin{pmatrix} f_{3,7} \\ f_{2,7} \\ f_{1,7} \\ f_{drop,7} \\ f_{ne,7} \end{pmatrix} = \begin{bmatrix} p_{11,6} & p_{12,6} & p_{13,6} \\ p_{21,6} & p_{22,6} & p_{23,6} \\ 0 & p_{32,6} & p_{33,6} \\ p_{41,6} & p_{42,6} & 0 \\ 0 & 0 & p_{53,6} \end{bmatrix} \begin{pmatrix} f_{2,6} \\ f_{1,6} \\ f_{ne,6} \end{pmatrix}$$

where the cells set equal to zero impose the restrictions that students cannot regress in grades and once enrolled, they can no longer enter the state of being “never enrolled”.

Thus, we denote the transition rule from age  $a$  to age  $a+1$  by  $f_{a+1}=A_a f_a$ . To simulate the impacts of the program for a synthetic cohort from data on a short panel we require two assumptions:

- i) The transition probabilities at each age depend on the current schooling level and on whether the children are currently participating in the program.

- ii) The transition matrices for an age group do not change over time.

Let  $s_a$  be the schooling level at age  $a$ ,  $T$  a variable indicating the whether the child participates in the program and  $H$  a vector of treatment and schooling level history before age  $a$ . Formally, assumption (i) can be expressed as:

$$P(s_{a+1}/s_a, T_a, H_a) = P(s_{a+1}/s_a, T_a)$$

Under assumptions (i) and (ii), and given an initial vector of state proportions at some age, the predicted schooling state distribution at a later age can be obtained by:

$$\hat{f}_{T-t}^a = \left( \prod_{s=a}^{a-1} \hat{A}_{T-t}^s \right) \hat{f}_{T-t}^a$$

where  $a$  is an age later than  $a_s$ , and  $T=1$  if the child participates in the program, and  $T=0$  otherwise.

## V. Empirical Results

In this section we present the empirical results obtained from applying the two methods of estimating program impact explained above. We use the *2DIF* estimators for most schooling indicators. For indicators that were likely affected by seasonality during the baseline survey (i.e., the coincidence of the coffee harvest season with the survey of the control group), we adopt the *CSDIF* estimator for the 2002 data.

Tables A.1 to A.5 in the appendix present *2DIF* estimates of impact for several outcome indicators of interest for the program's target population, children aging 6-12. These estimates were obtained from the following regression equation:

$$Y_{it} = b_0 + b_1.G1_i + b_2.G2_i + b_3.G3_i + b_4.D2002_{it} + b_5.G1_i.D2002_{it} + b_6.G2_i.D2002_{it} + b_7.G3_i.D2002_{it} + e_{it}, \quad (12)$$

where  $G1_i$  is an indicator variable determining whether household  $i$  lives in demand side-only municipality,  $G2_i$  determines whether household  $i$  lives in a demand+supply-side municipality, and  $G3_i$  determines whether household  $i$  lives in a supply side-only municipality. The dummy for households living in control municipalities ( $G4_i$ ) is excluded. Therefore,  $b_0$  gives the mean outcome for the control households in 2000, and  $b_4$  gives the estimated change in this mean between 2000 and 2002.  $b_1$ - $b_3$  give the average differences between the respective treatment groups ( $G1$ ,  $G2$  and  $G3$ ), and the control group  $G4$  in the baseline in 2000. The *2DIF* estimates of impact for each treatment are therefore given by parameters  $b_5$ - $b_7$ .

Tables A.1 to A.5 also present statistical tests for the existence of synergetic effects between supply-side and demand-side interventions. That is, we test the null hypothesis  $H_0: b_6 = b_5 + b_7$ , against the alternative  $H_a: b_6 \neq b_5 + b_7$ . As one can see, we cannot reject the null hypothesis at any conventional significance level, and for any of the outcome indicators. Therefore, in the rest of the paper we focus on the analysis of impact via the following regression model

$$Y_{it} = c_0 + c_1.D_i + c_2.S_i + c_3.D2002_{it} + c_4.D_i.D2002_{it} + c_5.S_i.D2002_{it} + e_{it}, \quad (13),$$

where  $D_i$  indicates that the municipality was treated with demand-side interventions, and  $S_i$  indicates supply-side interventions. Therefore, observations from municipalities  $G2$  will exhibit  $D_i=1$  and  $S_i=1$ , while observations from the control municipalities  $G4$  will exhibit  $D_i=0$  and  $S_i=0$ . Accordingly,  $G1$ ( $G3$ ) observations will exhibit  $D_i=1$  and  $S_i=0$  ( $D_i=0$  and  $S_i=1$ ).

Table 9 presents *estimates* of impact for several schooling outcome indicators for the program's target population (children aging 6-12) utilizing the regression framework in (13). As it can be seen, the results suggest that there is no impact of supply-side interventions for any of the outcome indicators. This is consistent with anecdotal evidence suggesting that the supply-side components of the program have been badly implemented or not implemented at all. Henceforth we focus on the estimates of demand-side impact only.

As it can be seen in table 9, there appears to be no impact of the program in enrollment rates for the 6-12 age group as a whole. This disappointing result can perhaps be explained by the graph in figure 2. The graph shows that enrollment rates for the ages 7-12, the main target of the program, are relatively high and therefore not likely to respond sharply to any demand-side cash transfers. That is, perhaps the opportunity cost of time for children aging 7-12 is still not elevated enough to permit a high response to the program. The graph shows that the program could have a greater impact if it had also targeted children aging 13-15.

Table 9 also shows strong and positive estimates of impact of the demand-side interventions on school dropouts. As discussed above, these unexpected results are likely due to the seasonality problems experienced by the baseline survey in 2000. Therefore, we shift attention to the *CSDIF* estimates of program impact on dropout rates. These indicate that the demand-side cash transfers have reduced dropout rates by 1.4 percentage points from a 3.2% basis, that is, a higher than 50% reduction in the rate of school dropout.

Table 9 also presents estimates of the impact of the PRAF II on school attendance. As shown, the results point to a 4.6 percentage point increase (from an average of approximately 91%) in attendance rates due to the program's demand-side interventions.

Therefore, while there is no evidence of increased enrollment due to the program, kids in the program appear less likely to dropout before the end of the school year and do seem to attend more classes. However, this increased contact with the school does not appear to boost school performance. As indicated in Table 9, approval rates do not seem to be higher for kids participating in program.

Finally, the impact of the program on child labor is also examined in Table 9. As seen, the *2DIF* estimate suggests a 7.4 percentage point increase in the incidence of child labor up from approximately 16%. This substantial and counterintuitive result is also explained by the effect of seasonality during the baseline survey. That is, in 2000, the incidence of child labor for the control and supply-side treatment groups was significantly higher than the incidence for the demand-side groups since the former were interviewed during the coffee harvest. The *CSDIF* estimates are less suspicious and

suggest that the program has no impact whatever in reducing the prevalence of child labor.

### **Estimating the schooling transition model**

As observed above, there are very many channels via which the demand-side cash transfers may affect schooling. We next present the results of the estimation of the schooling transition model that was described above, and can be used to summarize the impact of the program via several indicators into the impact of education attainment at age 13. We follow a procedure suggested in Behrman, Sengupta and Todd (2002) to estimate the probability transition matrices for ages 6 to 12, and then to simulate the resulting distribution of education attainment at age 13. The results are presented in table 18 and are graphically illustrated in figure 6.

Table 7 gives the simulated pdf and cdf values for treatments and controls, where treatment now refers to being a beneficiary of PRAF for 7 years from age 6 to 13. Figure 6 plots the corresponding histograms and cdf's which reveal a slight difference between the treatment and control groups. Our simulation predicts an average education attainment of 5.42 for program participants at age 13, as compared to 5.28 years for controls. A comparison of the predicted cdf for treatments and controls shows that the program induces about 12.3% more children to continue schooling into secondary school grades. These estimates can be combined with estimates of return to schooling in Honduras to assess the cost-benefit ratio of the program.

## **VI. Conclusion**

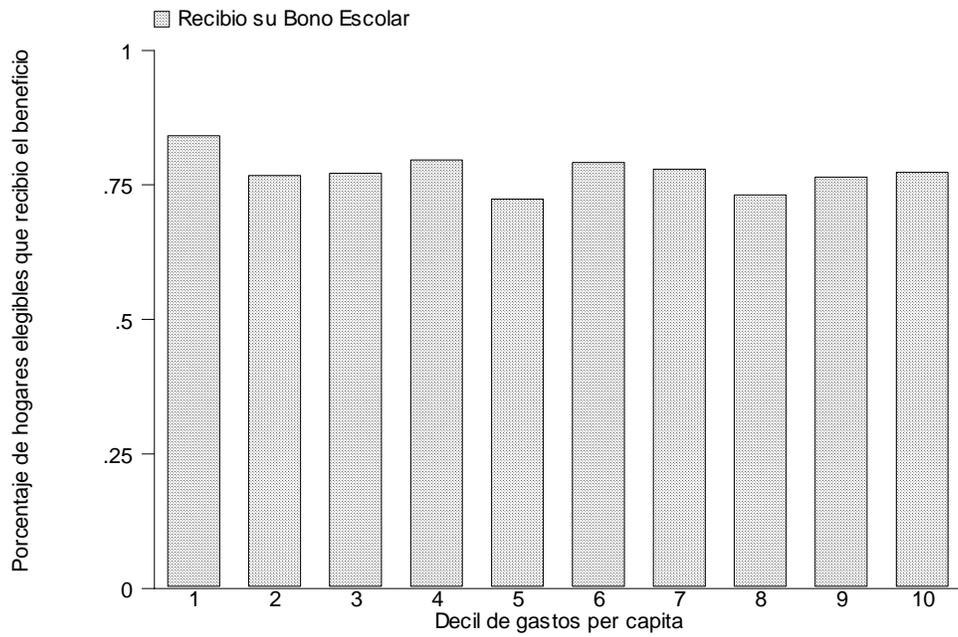
This paper uses data collected from a social experiment in Honduras to estimate the impact of a conditional cash transfers program, PRAF-II, on schooling and education attainment. The experiment provides demand-side cash transfers to families under the condition that they enroll and keep their school age children in school. It also provides block grants to parent-teacher associations to help them improve the quality of their schools. Seventy municipalities were assigned randomly to four treatment groups: G1, where only demand-side transfers were made; G2, where both demand-side and supply-side transfers were made; G3, where only supply side transfers were made, and G4, the

control group, where no transfers were made. Baseline data were collected from a random sample of 5,600 households living in these seventy municipalities before the program was implemented. A second round of surveys of the same families was conducted two years after the start of the program.

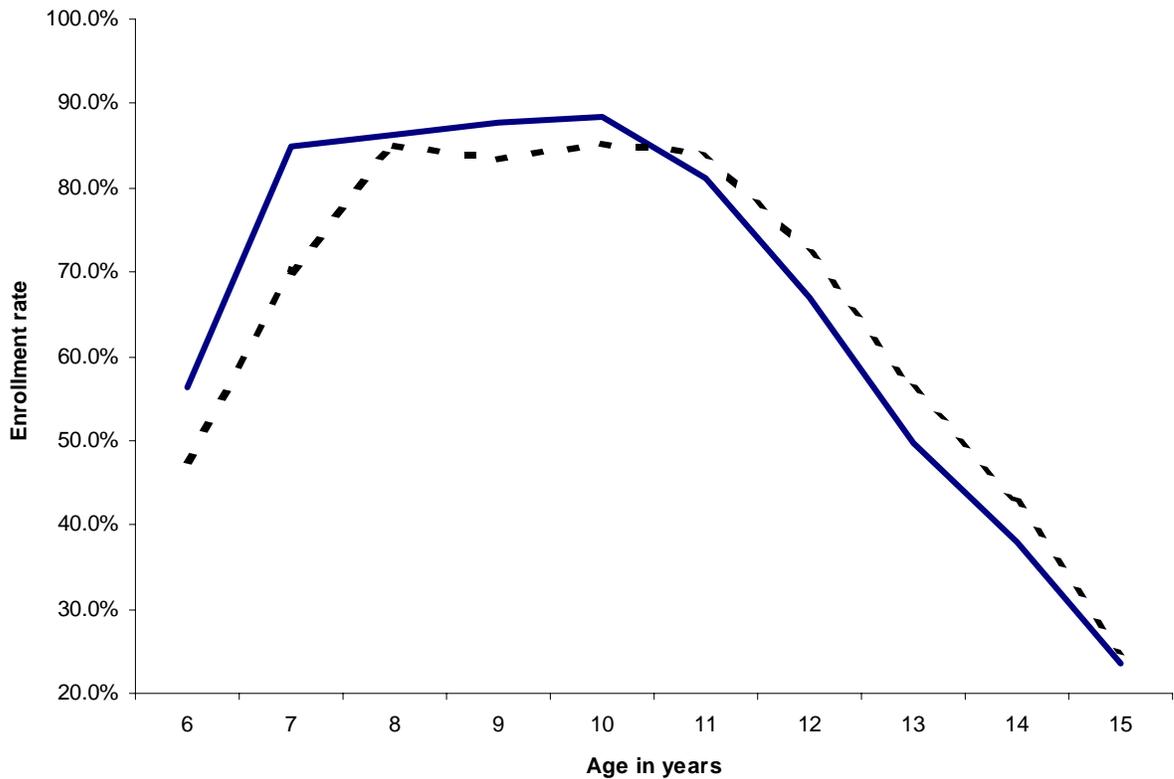
The results from the regression analysis and the simulated schooling transition model show a very weak impact of the demand-side component of the program, and no impact whatsoever of the supply-side interventions. The data suggest that the weak impact of the demand-side component is perhaps associated with poor targeting. Enrollment rates for children aging 6-12 were relatively high before program implementation, thereby weakening the potential for impact. The design of the program should be revised to allow participation of older children (13-16).

The results also confirm anecdotal evidence that the supply-side component of the program has been badly implemented, and sometimes not implemented at all in some municipalities.

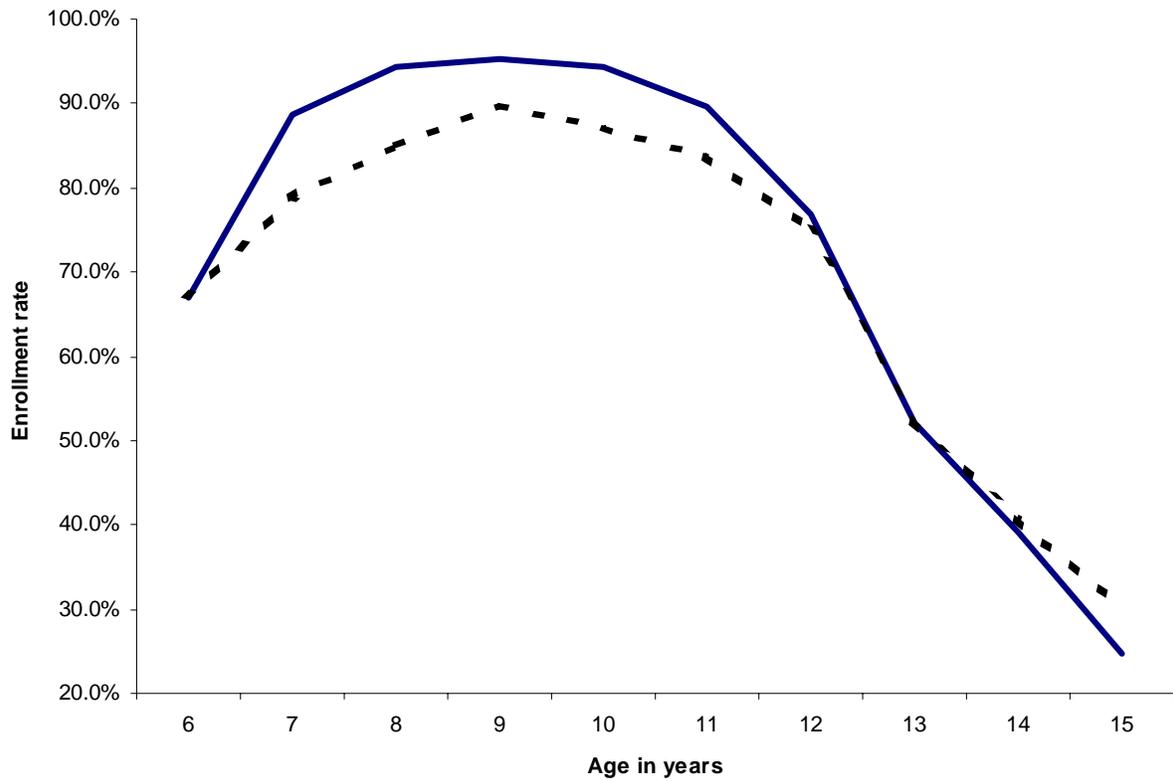
**Figura 1: Cobertura del Bono Escolar por decil de gastos per cápita.**



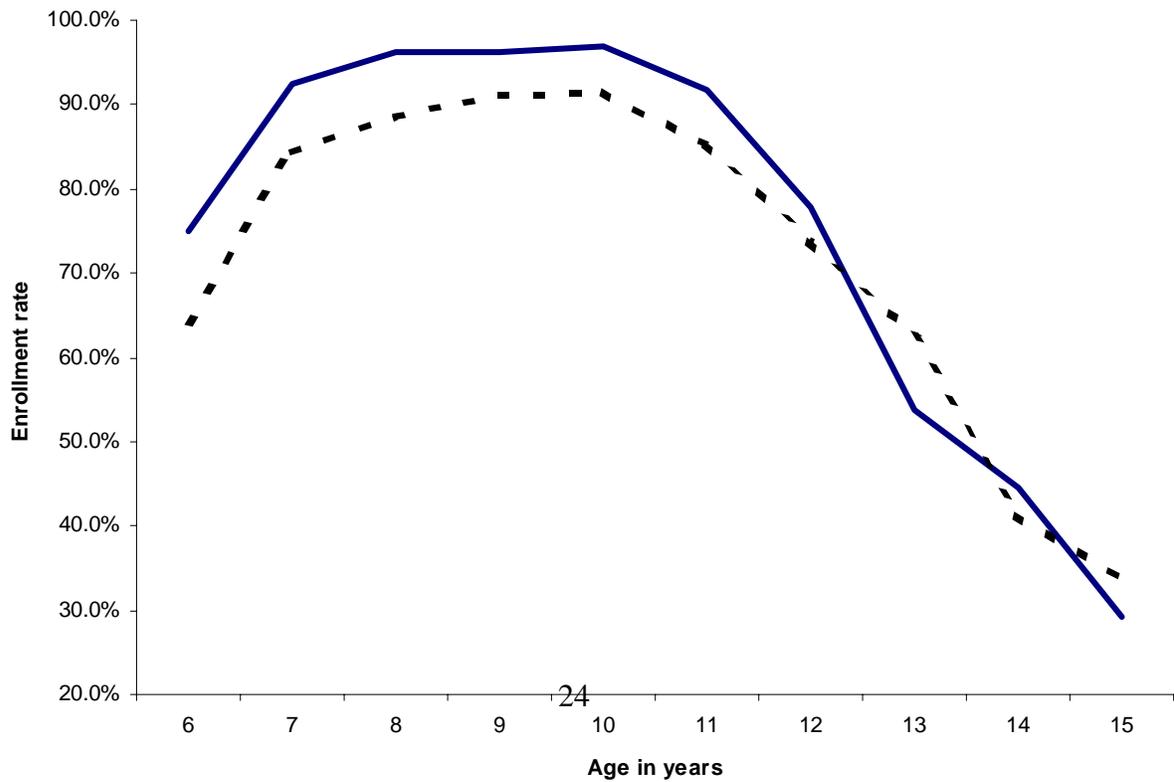
**Figure 2: Enrolment rates by age in 2000**



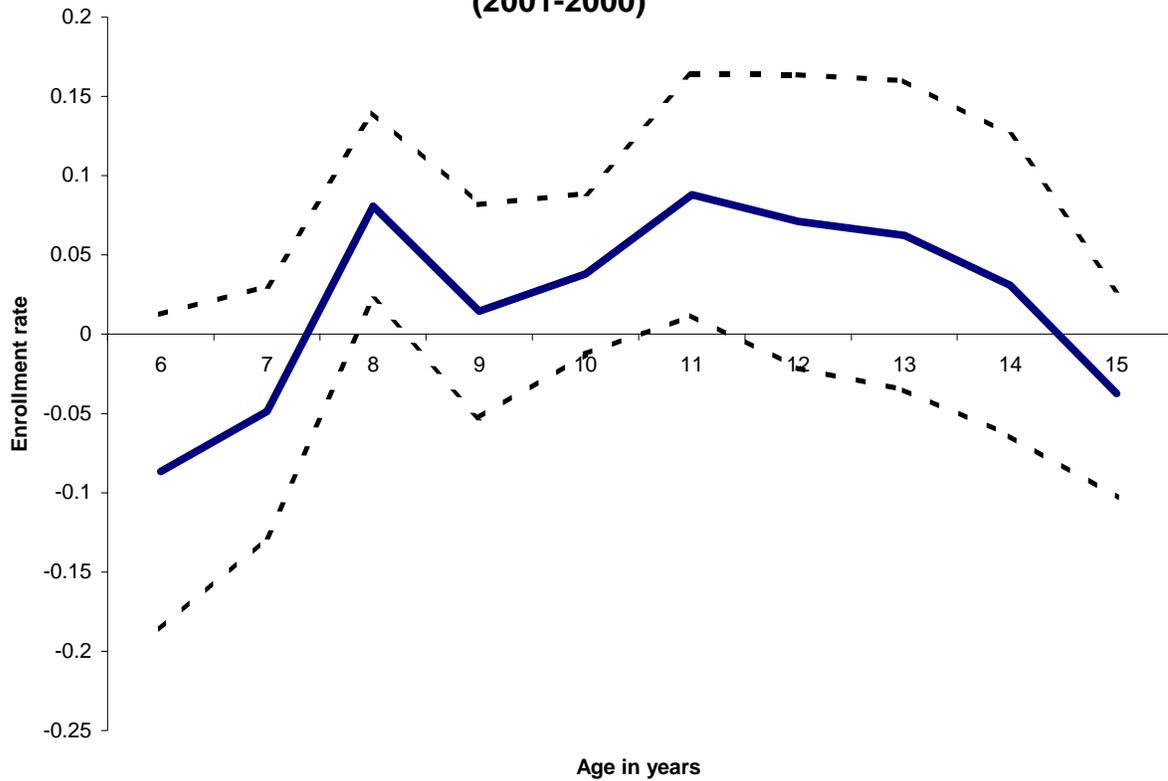
**Figure 3: Enrolment rates by age in 2001**



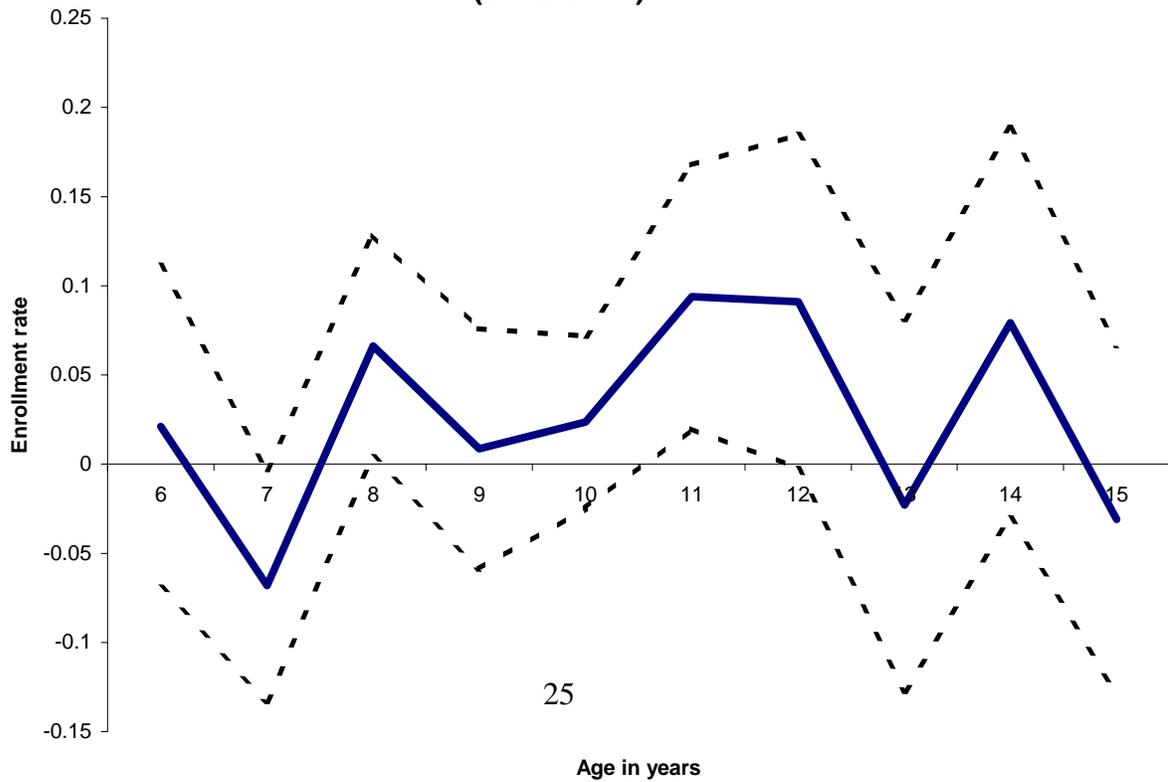
**Figure 4: Enrolment rates by age in 2002**



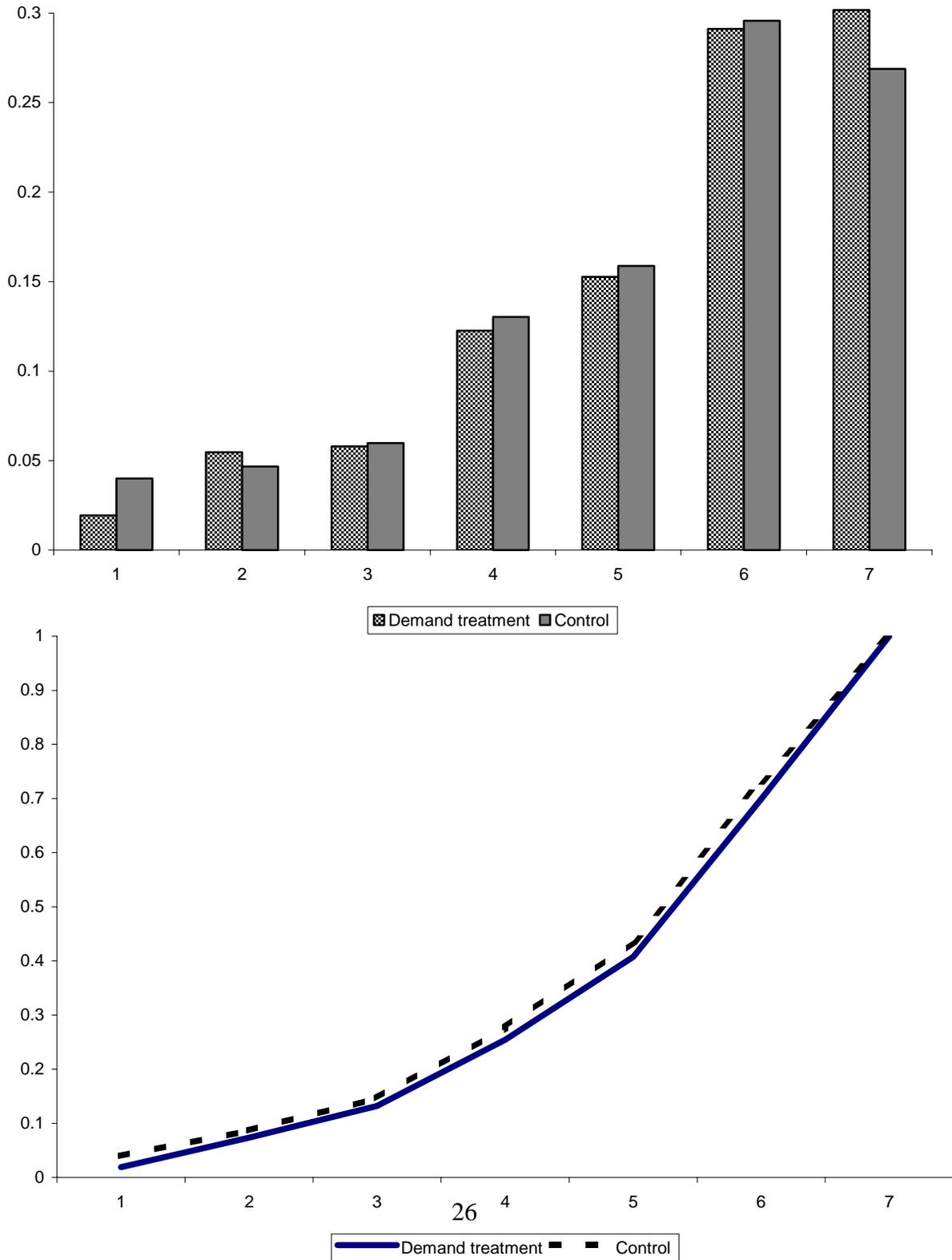
**Figure 5: Average Demand Side Treatment Impacts on Enrollment by Age (2001-2000)**



**Figure 5: Average Demand Side Treatment Impacts on Enrollment by Age (2002-2000)**



**Figure 6: Simulated Effects of Demand Treatment on Educational Attainment at 13**



**Table 1: Started Primary School, by Age**

<u>Age (years)</u>	<u>Boys</u>	<u>Girls</u>	<u>All</u>
6	20.0%	22.7%	21.3%
7	69.4	74.0	71.6
8	86.9	88.7	87.8
9	91.1	91.7	91.3
10	93.8	95.6	94.7
11	94.7	96.4	95.5
12	95.5	96.5	96.0

**Table 2: In School This Year, by Age**

<u>Age (years)</u>	<u>Boys</u>	<u>Girls</u>	<u>All</u>
6	49.6%	53.7%	51.6%
7	76.9	79.5	78.2
8	84.3	87.3	85.8
9	85.3	87.0	86.1
10	85.0	88.8	86.9
11	77.1	84.1	80.6
12	66.0	72.4	69.1
13	52.0	52.6	52.3
14	36.8	39.6	38.1
15	20.9	27.9	24.1

**Table 3: Years of Schooling Completed at Age 16**

<u>Years of Schooling</u>	<u>Boys</u>	<u>Girls</u>	<u>All</u>
0	14.3%	14.7%	14.5%
1-3	29.8	21.5	25.9
4	12.6	15.0	13.8
5-6	32.4	35.3	33.8
7+	11.0	13.4	12.0

**Table 4: Starting Primary and In School This Year, by Expenditure Quantile**

<u>Expenditure Quantile</u>	<u>Started Primary</u>	<u>In School This Year</u>
1	79.2%	54.8%
2	82.1	60.7
3	83.7	64.0
4	88.8	73.9

**Table 5: Starting Primary and In School This Year, by Mother's Education**

<u>Mother's Years Schooling</u>	<u>Started Primary</u>	<u>In School This Year</u>
0	80.2%	56.8%
1-5	84.5	64.9
6	87.2	81.9
7+	80.7	61.3

**Table 6: Starting Primary and In School This Year, by Distance to School**

<u>Travel Time (walking) to Nearest Primary School</u>	<u>Started Primary</u>	<u>In School This Year</u>
0-15 minutes	85.0%	65.9%
16-30 minutes	81.7	61.2
31-60 minutes	73.5	50.1
> 60 minutes	77.8	43.4



**Table 7: Sample evolution from 2000 to 2002 by random treatment group.**

	<b>Grupo de asignacion aleatoria</b>				
	<b>Demanda</b>	<b>Demanda+oferta</b>	<b>Oferta</b>	<b>Control</b>	<b>Total</b>
Ubicacion en el 2002					
<i>Hogar original</i>	85.4%	84.5%	85.6%	82.5%	84.3%
<i>Hogar derivado</i>	3.9%	3.3%	3.6%	3.4%	3.6%
<i>Murió</i>	0.6%	0.7%	0.6%	0.8%	0.7%
<i>Movio, no se pudo localizer</i>	5.7%	6.0%	4.5%	7.0%	6.0%
<i>Ignorado</i>	0.7%	0.7%	0.9%	0.8%	0.8%
<i>Fracaso (Rechazo/Ausente)</i>	1.6%	2.2%	2.3%	2.5%	2.1%
<i>Fracaso (Desocupada/No encontrada)</i>	2.1%	2.6%	2.5%	2.9%	2.5%
Total de personas	8823	8797	4197	8749	30566

**Table 8: Coverage and leakage of the cash transfers in 2001.**

	<b>Treatment Group</b>			
	<b>Demand (G1)</b>	<b>Demand +supply (G2)</b>	<b>Supply (G3)</b>	<b>Control (G4)</b>
Coverage	726/959 75.7%	746/942 79.2%		
Leakega	15/689 2.2%	12/669 1.8%	3/798 0.4%	19/1625 1.2%

**Table 9: Estimated impact of PRAF's demand and supply side interventions on schooling indicators**

<i>Dependent variable:</i>	<i>Enrollme nt 2000 &amp; 2001</i>	<i>Enrollme nt 2000 &amp; 2002</i>	<i>Drop-out (2Diif)</i>	<i>Drop-out (CSDiff)</i>	<i>Attendance</i>	<i>Approval</i>	<i>Child Labor (2Diif)</i>	<i>Child Labor (CSDiff)</i>
Explanatory variables:								
Change 2002—2000 in control municipalities	0.058** (0.017)	0.074** (0.017)	-0.150** (0.049)	--	0.026** (0.009)	0.002 (0.011)	-0.057** (0.021)	--
Demand-side municipalities (baseline in 2000)	0.039 (0.031)	0.039 (0.031)	-0.140** (0.045)	--	-0.030** (0.013)	0.019 (0.013)	-0.084** (0.021)	--
Impact of demand- side interventions	0.019 (0.019)	0.029 (0.020)	0.127** (0.047)	-0.014** (0.007)	0.046** (0.012)	-0.009 (0.013)	0.074** (0.025)	-0.010 (0.011)
Supply-side municipalities (baseline in 2000)	-0.010 (0.030)	-0.010 (0.030)	-0.003 (0.018)	--	0.018 (0.014)	-0.008 (0.014)	0.020 (0.019)	--
Impact of supply-side interventions	0.019 (0.018)	0.015 (0.020)	-0.002 (0.019)	-0.005 (0.006)	-0.011 (0.013)	-0.005 (0.012)	-0.012 (0.024)	0.008 (0.011)
Intercept (control baseline)	0.751** (0.026)	0.751** (0.026)	0.182** (0.047)	0.032** (0.007)	0.913** (0.011)	0.868** (0.010)	0.157** (0.017)	0.100** (0.009)
Observations:	12,451	12,741	9,139	5,442	9,515	8,948	12,722	6265

**Table10: Impacto en la tasa de matricula escolar por edad (2001-2000)**

Edad	Control			<i>Doble-Diferencia</i>	
	2000	2001	<i>Cambio</i>	Demanda-Control	valor-P
<b>6</b>	47.6%	66.8%	19.2%	-8.7%	0.086
<b>7</b>	70.0%	78.7%	8.8%	-4.9%	0.224
<b>8</b>	85.1%	85.2%	0.1%	8.1%	0.008
<b>9</b>	83.5%	89.7%	6.2%	1.4%	0.668
<b>10</b>	85.4%	87.3%	1.9%	3.8%	0.139
<b>11</b>	84.3%	83.8%	-0.5%	8.8%	0.024
<b>12</b>	72.2%	75.0%	2.7%	7.1%	0.13
<b>13</b>	56.1%	52.4%	-3.7%	6.2%	0.207
<b>14</b>	42.7%	40.7%	-2.1%	3.1%	0.52
<b>15</b>	25.0%	29.9%	5.0%	-3.7%	0.257

**Table 11: Impacto en la tasa de matricula escolar por edad (2002-2000)**

Edad	Control			<i>Doble-Diferencia</i>	
	2000	2002	<i>Cambio</i>	Demanda-Control	valor-P
<b>6</b>	47.6%	64.2%	16.6%	2.1%	0.642
<b>7</b>	70.0%	84.3%	14.4%	-6.8%	0.040
<b>8</b>	85.1%	88.6%	3.4%	6.6%	0.037
<b>9</b>	83.5%	91.3%	7.7%	0.9%	0.801
<b>10</b>	85.4%	91.6%	6.2%	2.4%	0.334
<b>11</b>	84.3%	85.3%	1.1%	9.4%	0.014
<b>12</b>	72.2%	73.9%	1.7%	9.1%	0.056
<b>13</b>	56.1%	62.4%	6.3%	-2.3%	0.660
<b>14</b>	42.7%	41.2%	-1.5%	7.9%	0.153
<b>15</b>	25.0%	33.7%	8.8%	-3.1%	0.526

**Table12: Impacto en la deserción escolar  
en 2002 para grados de 1 a 6**

<b>Grado</b>	<b>Control</b>	<b>Demanda-Control</b>	<b>Valor-P</b>
<b>1</b>	5.3%	-4.3%	0.000
<b>2</b>	2.6%	-0.7%	0.508
<b>3</b>	3.0%	-1.0%	0.368
<b>4</b>	2.3%	0.2%	0.817
<b>5</b>	3.6%	-0.7%	0.697
<b>6</b>	3.3%	1.5%	0.349

**Table 13: Impacto en la deserción escolar  
en 2002 por edad**

<b>Edad</b>	<b>Control</b>	<b>Demanda-Control</b>	<b>Valor-P</b>
<b>6</b>	5.6%	-2.5%	0.103
<b>7</b>	4.1%	-3.2%	0.015
<b>8</b>	2.2%	-0.6%	0.549
<b>9</b>	2.9%	-2.3%	0.020
<b>10</b>	2.3%	-0.8%	0.444
<b>11</b>	2.0%	-0.2%	0.847
<b>12</b>	3.6%	0.2%	0.904
<b>13</b>	3.5%	1.0%	0.581
<b>14</b>	6.8%	-2.4%	0.364
<b>15</b>	8.5%	2.5%	0.567

**Table 14: Impacto en la tasa de asistencia escolar  
por grado de la primaria**

<b>Grado</b>	<b>Control</b>			<b>Doble-Diferencia</b>	
	<i>2000</i>	<i>2002</i>	<i>Cambio</i>	Demanda-Control	valor-P
<b>1</b>	88.8%	93.4%	4.7%	3.0%	0.145
<b>2</b>	91.4%	93.4%	2.0%	6.5%	0.002
<b>3</b>	90.5%	94.3%	3.8%	3.9%	0.033
<b>4</b>	93.5%	94.1%	0.6%	6.2%	0.000
<b>5</b>	93.2%	95.3%	2.1%	3.3%	0.049
<b>6</b>	93.9%	95.4%	1.5%	2.7%	0.107

**Table 15: Impacto en la tasa de asistencia escolar por edad**

<b>Edad</b>	<b>Control</b>			<b>Doble-Diferencia</b>	
	<i>2000</i>	<i>2002</i>	<i>Cambio</i>	Demanda-Control	valor-P
<b>6</b>	90.6%	93.3%	2.7%	4.2%	0.040
<b>7</b>	90.8%	93.3%	2.4%	5.6%	0.010
<b>8</b>	91.7%	94.2%	2.5%	5.0%	0.012
<b>9</b>	91.4%	94.5%	3.1%	2.8%	0.113
<b>10</b>	91.1%	93.9%	2.8%	4.8%	0.017
<b>11</b>	91.3%	92.7%	1.4%	5.9%	0.001
<b>12</b>	91.8%	95.8%	4.1%	3.1%	0.087
<b>13</b>	92.9%	94.7%	1.8%	3.4%	0.089
<b>14</b>	93.6%	95.2%	1.6%	4.1%	0.187
<b>15</b>	92.5%	96.4%	3.9%	-1.1%	0.744

**Table16: Impacto en la tasa de aprobación por grado**

<b>Grado</b>	<b>Control</b>			<b>Doble-Diferencia</b>	
	<i>2000</i>	<i>2001</i>	<i>Cambio</i>	Demanda-Control	valor-P
<b>1</b>	72.0%	73.3%	1.3%	-5.4%	0.114
<b>2</b>	91.4%	90.2%	-1.2%	-0.8%	0.777
<b>3</b>	94.2%	92.3%	-2.0%	0.5%	0.796
<b>4</b>	95.3%	91.6%	-3.7%	1.6%	0.624
<b>5</b>	98.3%	96.2%	-2.1%	3.9%	0.046
<b>6</b>	97.9%	98.0%	0.1%	-0.3%	0.879

**Table 17: Impacto en la tasa de aprobación por edad**

<b>Edad</b>	<b>Control</b>			<b>Doble-Diferencia</b>	
	<i>2000</i>	<i>2002</i>	<i>Cambio</i>	Demanda-Control	valor-P
<b>6</b>	92.5%	94.2%	1.7%	-0.5%	0.914
<b>7</b>	88.9%	83.2%	-5.7%	6.9%	0.110
<b>8</b>	79.9%	80.1%	0.2%	-8.2%	0.050
<b>9</b>	83.4%	86.0%	2.5%	-8.4%	0.028
<b>10</b>	86.8%	86.7%	-0.1%	0.1%	0.975
<b>11</b>	89.1%	89.5%	0.4%	3.2%	0.318
<b>12</b>	91.2%	92.0%	0.8%	3.7%	0.212
<b>13</b>	92.2%	93.1%	1.0%	-1.0%	0.790
<b>14</b>	96.0%	95.9%	-0.1%	3.7%	0.284
<b>15</b>	93.2%	92.3%	-1.0%	5.8%	0.219

**Table 18: Simulated Education distribution ast age 13 for Treatment and Control ChildrenAfter Exposure of treatment for 8 years, Age 6-13**

<b>Grade</b>	<b>Treatment %</b>	<b>Control %</b>	<b>Treatment cdf</b>	<b>Control cdf</b>
1	1.94%	4.01%	1.94%	4.01%
2	5.47%	4.67%	7.41%	8.68%
3	5.79%	5.98%	13.20%	14.65%
4	12.26%	13.03%	25.46%	27.68%
5	15.26%	15.87%	40.72%	43.55%
6	29.11%	29.57%	69.83%	73.13%
7	30.17%	26.87%	100%	100%

## Appendix

**Table A.1: Impacto en la tasa de matrícula escolar para niños y niñas de 6 a 12 años, 2001-2000**

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**Grupo de asignacion aleatoria**

	<b>Demanda</b>	<b>Demanda+Oferta</b>	<b>Oferta</b>	<b>Control</b>
2000 N	1,892		1,852	869
2001	1,772		1,724	805
2000 %	79.5%		77.5%	75.1%
2001	86.7%		87.6%	81.7%
Cambio	7.2%		10.1%	6.6%
Doble diferencia	0.8%		3.7%	0.2%

Prueba para la existencia de efectos de sinergia:  
 $F(1, 65) = 0.62, \text{Prob} > F = 0.4357$

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**Table A.2: Impacto en la tasa de matrícula escolar para niños y niñas de 6 a 12 años, 2002-2000**

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**Grupo de asignacion aleatoria**

	<b>Demanda</b>	<b>Demanda+Oferta</b>	<b>Oferta</b>	<b>Control</b>
2000 N	1,892		1,852	869
2002	1,867		1,818	845
2000 %	79.5%		77.5%	75.1%
2002	89.7%		89.4%	83.8%
Cambio	10.2%		12.0%	8.6%
Doble diferencia	2.7%		4.4%	1.1%

Prueba para la existencia de efectos de sinergia:  
 $F(1, 65) = 0.03, \text{Prob} > F = 0.8738$

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**Table A.3: Impacto en la tasa de deserción escolar para niños y niñas de 6 a 12 años**

<b>Grupo de asignacion aleatoria</b>						
		<b>Demanda</b>	<b>Demanda+Oferta</b>	<b>Oferta</b>	<b>Control</b>	
2000	N	1,503		1,435	239	520
2002		1,675		1,626	708	1,433
2000	%	4.5%		3.4%	20.1%	17.1%
2002		1.9%		1.2%	2.8%	3.1%
Cambio		-2.6%		-2.2%	-17.3%	-14.0%
Doble diferencia		11.4%		11.8%	-3.2%	

Prueba para la existencia de efectos de sinergia:  
 $F(1, 65) = 0.15, \text{Prob} > F = 0.6973$

**. Table A.4: Impacto en la tasa de asistencia escolar para niños y niñas de 6 a 12 años**

<b>Grupo de asignacion aleatoria</b>						
		<b>Demanda</b>	<b>Demanda+Oferta</b>	<b>Oferta</b>	<b>Control</b>	
2000	N	1,435		1,383	411	965
2002		1,643		1,606	688	1,384
2000	%	88.5%		89.8%	94.0%	91.0%
2002		95.7%		95.9%	95.3%	93.6%
Cambio		7.2%		6.1%	1.4%	2.7%
Doble diferencia		(p=0.004)4.6%		(p=0.046)3.4%	-1.3%	

Prueba para la existencia de efectos de sinergia:  
 $F(1, 65) = 0.00, \text{Prob} > F = 0.9486$

**Table A.5: Impacto en la tasa de aprobación para niños y niñas de 6 a 12 años**

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**Grupo de asignacion aleatoria**

		<b>Demanda</b>	<b>Demanda+Oferta</b>	<b>Oferta</b>	<b>Control</b>	
2000	N	1,294		1,289	558	1,156
2002		1,433		1,450	570	1,198
2000	%	88.9%		87.7%	86.6%	86.6%
2002		88.3%		86.6%	86.5%	86.7%
Cambio		-0.6%		-1.2%	-0.1%	0.1%
Doble diferencia		-0.7%		-1.3%	-0.2%	

Prueba para la existencia de efectos de sinergia:

$F(1, 65) = 0.02, \text{Prob} > F = 0.8883$