# Do School Subsidy Programs Generate Lasting Benefits? A Five-Year Follow-Up of *Oportunidades* Participants

by

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## Abstract

Conditional cash transfer programs link monetary transfers to investment in child and family human capital, in the hopes of alleviating current poverty and reducing the intergenerational transmission of poverty. These programs have rapidly become the main anti-poverty program across Latin America and are now spreading beyond the region. Several studies have evaluated the short term impacts of Mexico's *Oportunidades* program after 12 to 18 months of program participation. The studies of schooling impacts found benefits to schooling in terms of increased enrollment and higher schooling attainment. This paper moves beyond the short-run effects by evaluating the impacts 5.5 years after the program was introduced, not only on schooling attainment but, also on work and on cognitive achievement, as measured by test scores. Our results show positive impacts on grades of schooling attained, but few effects on achievement tests. With respect to work, the estimates show reductions in work for younger youth, consistent with postponement of work because of longer time attending school, and an increase in work for older girls. The results are also suggestive of a shift out of agricultural employment towards non-agricultural employment.

## 1. Introduction

In recent years, a number of countries in Latin America and elsewhere have implemented large-scale conditional cash transfer programs that provide monetary incentives to poor families for investing in the human capital of their children. The *Oportunidades* program (formerly called *PROGRESA*) has been operating since 1997 in rural Mexico. An initial rigorous experimental evaluation of the program, based on data from the first two years after the program was initiated (1998-2000), demonstrated significant short-run impacts in increasing school enrollment, as well as some other effects.[1] Some of the early evaluation studies also generated estimates of longer-run programs effects, but only under assumptions such as stability in schooling transition matrices or on the functional forms of structural relations underlying family behaviors (e.g. Behrman, Sengupta, Todd, 2005, Schultz, 2004 and Todd and Wolpin, 2007).

This paper uses a newly available follow-up dataset to analyze impacts of the *Oportunidades* program on schooling attainment, labor force participation and cognitive achievement of youth 5.5 years after the experimental treatment group first began receiving benefits. We focus on the group of youth who were age 9 to 15 in 1997, just prior to the program intervention, and who were age 15 to 21 in 2003. In 1997, when the program began, this group was at or near the age of the transition from primary to secondary school, a critical juncture in rural Mexico when many children drop out of school (see Figure 1, which also illustrates the inverse labor market-schooling enrollment relation, particularly for boys).[2] By 2003, many of these youth had entered the labor market, which allows for examination of program effects on labor market outcomes. This paper goes beyond the previous evaluations on schooling by considering program exposure times of about three times as long as previous studies, impacts on achievement tests in addition to time in school, and by impacts on work in addition to those on education.

The original short-term evaluation results were based on the experimental program design, which randomly assigned 320 communities to a treatment group and 186 to a control group. In 2000,

1 The overall evaluation of the initial years of *PROGRESA* is summarized in Behrman and Skoufias (2006), Skoufias (2004) and Skoufias and McClafferty (2001).
2 Previous evaluations have, in fact, demonstrated that the largest effects of the program were precisely at this transition between primary and secondary school (see Behrman, Sengupta, Todd, 2005, Schultz, 2004 and Todd and Wolpin, 2007).

approximately 18 months after the start of the experiment, the control group communities were also incorporated into the program.[3] Thus, comparisons of the experimental treatment and control groups are informative about the effects of differential exposure to the program, but the experimental control group can no longer be used to evaluate impacts against a benchmark of no program. For this purpose, we use an alternative source of comparison group data, consisting of households in 152 communities where the program had not yet been introduced as of 2003.[4] Our principal evaluation strategy is based on difference-in-difference matching comparisons of the original treatment group and the new 2003 comparison group to examine the effect of exposure to the program for 5.5 years versus no exposure. Other comparisons, e.g. of the original control group and the new 2003 comparison group as well as of the original treatment and control group, provide a useful robustness check for the main results. Our data sources are the new follow-up round of the Evaluation Rural Survey (ENCEL2003), linked with earlier data, particularly the 1997 pre-program Survey of Household Socio-economic Characteristics (ENCASEH) data.

Section 2 provides a brief description of the program and presents a simple economic model of expected impacts on school and work. Section 3 introduces the basic sample design and data. Section 4 describes the methodology. Section 5 presents the main results on education and schooling. Section 6 provides estimates of the program on achievement. Section 7 provides estimates of the benefit/cost ratio of the program and Section 8 concludes.

## Section 2. Program Background and a Simple Economic Model of Subsidy Effects

*Oportunidades* (previously called *PROGRESA*) began operating in small rural communities in 1997. Since then, it has gradually expanded to urban areas and now covers a total of 5 million families, or about one quarter of all families in Mexico. The program provides cash payments to families that are conditional on children regularly attending schools and on family members visiting health clinics for checkups.[5] The program has attracted substantial international attention and a number of other countries

3 During the period of the experiment, the control group was not told of the intention of eventually giving them benefits.

4 The comparison group communities are comparable to the communities in the experimental group in terms of community-level characteristics, such as availability of schools and health clinics and geographic location.

5 Regular school attendance (85% of time) is required to continue receiving the monthly grant payments as is

in Latin America and elsewhere now have similar governmental programs (see Behrman and Skoufias, 2006 and Parker, Rubalcava and Teruel, 2006). [6]

Table 1 shows the monthly grant levels for children between the third grade and the twelfth grade in the second semester of 2003 (the exchange rate was about 11 pesos per US dollar). Originally, the program provided grants only for children between the third and ninth grades, but in 2001, the grants were extended to high school or upper-secondary school (grades 10-12). At grades seven and above, the grants are slightly higher (by about 13%) for girls than boys. Program rules allow students to fail each grade once, but if a student repeats a grade twice, the education benefits are discontinued permanently.[7] In terms of magnitude, the school subsidies constitute the majority of the program benefits. However, the program also provides some additional subsidies for school supplies and a fixed transfer linked to regular visits to health clinics.[8]

The school subsidies provided under the program reduce the shadow wage (or relative value) of children's time in activities other than school. The theoretical effect of the program on time spent working is ambiguous. To see why, consider a simple two-period model, where in the first period, individuals are children and can allocate their time between leisure, work and school. In the second period, they are adults and can choose between leisure and work, with a wage that depends on the time spent in school in the first period. Let $C_1$ and $C_2$ denote consumption of the individual as a child and as an adult, $L_1$ and $L_2$ denote leisure, $S$ amount of schooling, $W_1$ the child wage rate, $W_2(S)$ the adult wage rate, and $A$ transfers to the individual from parents and other family members. Also, let $t_1$ denote amount of time

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attendance at a health talk once a month for high school students.

6 Latin American countries include Colombia, Nicaragua, Costa Rica, Brasil, Peru, Jamaica, Honduras, among others. The Brasilian Bolsa Escola program predates the Mexican program, but the information available on its program participants is not detailed enough to permit systematic empirical evaluation of its impacts, although see de Janvry, Finan and Sadoulet (2006) for an analysis of program impacts on dropout and failure rates using adminstrative data from schools.

7 After three years in the program, all households are subject to recertification, a process by which households receive a visit and their household characteristics are evaluated to see if they continue to be eligible for the program. Those found to no longer be eligible for benefits are transitioned to a modified version of the program (Esquema Diferenciado de Apoyos-EDA), which continues to include secondary and high school schooling grants, but excludes primary school scholarships and cash transfers for food. In practice, however, very few households in our sample have been transitioned to the modified version of the program, so that our sample includes all households initially eligible for the program.

8 All monetary grants are given to the mother of the family with the exception that scholarships for upper-secondary school can be received by the youth themselves, if authorized by their mothers.

spent working in period 1 and $t_2$ the amount of time spent working in period 2. β denotes the discount rate and $p_s$ the subsidy paid for amount of schooling attended. We assume diminishing marginal utility of consumption and of leisure in each period and diminishing marginal productivity of schooling on the second-period wage. We also assume that consumption and leisure are normal goods in both periods. There is no direct utility from schooling, which solely provides a technology for transferring resources from the first to the second period and increasing the second-period wage.

Individuals maximize the objective function

$$U_1(C_1, L_1) + \beta U_2(C_2, L_2)$$

subject to the constraints

$$S + t_1 + L_1 \leq T$$

$$t_2 + L_2 \leq T$$

$$C_1 + C_2/(1+r) = A + p_s S + (T - L_1 - S)W_1 + (T - L_2)W_2(S)/(1+r)$$

An optimality condition that holds in any interior solution of the problem is

$$MU_{L1} - MU_{C1} p_s = \beta MU_{C2} t_2 W'_2(S)$$

or equivalently,

$$MU_{C1}(W_1 - p_s) = \beta MU_{C2} t_2 W_2^{'}(S)$$

The left side of the equation is the cost of time spent in school, in term of monetary direct costs and foregone leisure (or equivalently, foregone earnings from work, since another optimality condition equates the marginal utility from leisure with the marginal utility in added consumption derived from work). The right side is the marginal benefit of spending additional time in school, i.e. higher earnings as an adult.

The optimality conditions show that the subsidy affects the marginal costs of schooling in the same way as would a decrease in the child wage rate: the subsidy reduces the shadow wage (or relative value) of children's time in activities other than school. The benefit to schooling in terms of earnings (as opposed to full income) depends on how much time the individual as an adult spends working. Because adult

males usually have higher wages than adult females, the marginal benefit from boys' schooling in terms of earnings is likely to be higher than that of girls' at any given level S. [9] This may help explain lower average schooling level choices for girls than for boys.[10] If this is the case and child wages are the same for boys and girls, then a higher subsidy may be required to induce girls to get as much schooling as boys. (Note, however, that if full income rather than earnings is the relevant measure of well being, there is no reason to give different subsidy rates to boys and girls.)

A decrease in the direct costs of schooling, resulting from the subsidy, has both substitution and income effects. The substitution effect decreases the amount of time spent in leisure and working as a child and increases the amount of time spent in school. The income effect (an increase in life-time earnings) increases consumption of all normal goods, namely of leisure and consumption in the first and second periods. Thus, the net effect on childhood time spent in leisure in the first period is ambiguous. The net effect on time spent working and time spent in school is also, in principle, ambiguous. However, if the substitution effect on leisure dominates the income effect, then leisure in the first period will go down and time spent in non-leisure activities will go up. Because the subsidy increases the relative benefit of school relative to work, we expect that time spent in school will go up and time spent working will go down.

The optimality condition can be rewritten as a tangency condition

$$MU_{C1} / \beta MU_{C2} = t_2 W_2^{'}(S) / (W_1 - p_s)$$

where the left side gives the slope of the indifference curves between present and future consumption for the child and the right side gives us the slope of the frontier of the feasible consumption set. The model so far ignores the indivisibilities built in the program with respect to school subsidies. Consider a version of the model in which there is no proportional school subsidy (i.e. $p_s = 0$) but rather a subsidy $p$ is paid to every child that obtains a level of education larger or equal than $S_{\min}$ and a subsidy of zero to every

9 Assuming the marginal opportunity cost of boy's schooling is not higher than for girls' schooling.

10 Although Behrman, Sengupta and Todd (2000, 2005) show that actual attainment of girls in terms of grades of completed schooling in rural areas pre-program averaged more than that of boys in Mexico so the policy of higher grants for girls does not seem to be justified by these gender differences on average in schooling.

child that does not. Here $S_{\min}$ may represent the minimum mandatory attendance to receive the benefits of the program. The resource constraint of the individual is now

$$C_1 + C_2/(1+r) = A + pI(S) + (T - L_1 - S)W_1 + (T - L_{2)}W_2(S)/(1+r).$$

where

$$I(S) = \begin{cases} 0 & \text{if } S < S_{\min} \\ 1 & \text{if } S \geq S_{\min}. \end{cases}$$

This subsidy scheme will affect children differently according to the decisions regarding schooling before school subsidies were introduced. For children who were already receiving more than $S_{\min}$ units of education before the introduction of school subsidies, the program has only an income effect.[11] For children who were not receiving that level of education and who do not receive it either after the introduction of the subsidies, the program has neither an income nor a substitution effect. And for children who did not receive $S_{\min}$ units of education before the program but who do so after, there are both income and substitution effects. The extent to which the program has a significant impact on the human capital and work of children can only be determined through empirical analysis. [12]

**Section 3. Evaluation Design and Data**

As noted in the introduction, the original evaluation and sample design for *Oportunidades* involved

11 We are ignoring here for simplicity the fact that the frontier of the feasible consumption set may be nonlinear.

12 Note that it is not obvious why a government interested in increasing the welfare of the child would prefer school subsidies over unconditional transfers in the context of the simple model discussed above. Many might argue that conditionality requirements are paternalistic and therefore not necessarily welfare-improving relative to unconditional transfers. One potential motivation for conditioning benefits is that there are some social returns to investing in education that are not reaped by the individual. But a review of the literature (Martinelli and Parker, 2003) found few empirical studies demonstrating that social returns were significantly higher than private returns (one exception is Foster and Rosenzweig, 1995). An alternative motivation to condition transfers is the role of conditionality in implementing outcomes that are favorable to the child in the context of intra household bargaining. For instance, unconditional transfers to the child may be counterweighed by a reduction in the resources transferred by family members to the child. Martinelli and Parker (2003) study conditionality in the context of intrahousehold bargaining and show how it might shift welfare from fathers to mothers and children if mothers have greater interests in child human capital than do fathers.

selecting 506 communities with 320 randomly assigned to receive benefits immediately and the other 186 to receive benefits later. The eligible households in the original treatment localities (we term these T1998), determined by a means test, began receiving program benefits in the spring of 1998 whereas the eligible households in the control group (T2000) began receiving benefits at the end of 1999. Between 1997 and 2000, evaluation surveys (ENCELs) with detailed information on demographics, education, health, income and expenditures were administered every six months to households in both the T1998 and T2000 groups. All households in the 506 communities in the evaluation sample, including both eligible and ineligible households, were interviewed.

In the year 2003, there was a new follow-up round of the rural evaluation survey (ENCEL2003). The sampling frame was augmented to include a new subsample of households from other communities that that had not received the program by 2003. These communities (C2003) were selected to be similar to the communities in the original experiment through a matching procedure that matched the experimental communities to comparison group communities based on locality characteristics, such as geographic location and the availability of schools and health clinics.

The data used in this paper are from the Survey of Household Socio-economic Characteristics (ENCASEH) and the Evaluation Survey of *Oportunidades* (ENCEL2003). The Survey of Household Socio-economic Conditions serves as a baseline survey for the evaluation and is the survey that was originally used to select households in the eligible communities for participation in *Oportunidades*. We link the ENCASEH97 to the ENCEL2003 in order to have longitudinal data on individual children who were 9 to 15 years of age in 1997 and 15 to 21 in 2003. For the new comparison group households, we use recall data on their 1997 characteristics to characterize their eligibility status in 1997. Using both the 2003 and 1997 rounds of data, we can construct difference-in-difference matching estimators of program impacts. This is the case for all the outcomes, except for the achievement tests, which were only collected in the 2003 survey and are described more in Section 6. For the schooling and work indicators, there are a total of 19,586 youth between the ages of 15 and 21 in 2003. Table 2 shows their distribution among the three sample groups of interest.

## Section 4: Methodology

To estimate medium-term program impacts against the benchmark of no program, we compare the original treatment group (T1998) with the new comparison group (C2003) that was drawn from rural areas that had not yet been incorporated into the program in 2003. Because the C2003 group was not selected randomly, we use matching methods to take into account differences in observed characteristics between the T1998 and C2003 samples. For each individual in the treatment group (T1998), these methods select comparable individuals from the outside comparison group (C2003).[13]

The households living in localities where the program was not yet available were unlikely to have been affected by the existence of the program. However, because they lived in different geographic areas from the treatment group sample, they may have experienced different local area effects (labor market conditions, quality of schooling, quality of health clinics, prices) that may be also be relevant determinants of outcomes of interest (schooling, work, achievement scores). To take such differences into account, we make use of "difference-in-difference matching," which has been developed in the recent econometric literature (Heckman, Ichimura and Todd, 1997). The approach is analogous to the standard DID regression estimator, but does not impose any functional form restriction in estimating the conditional expectation of the outcome variable and reweights the observations according to the weighting functions implied by the matching estimators. Unlike cross-sectional matching estimators, DID matching estimators allow for selectivity into the program to be based on unobserved fixed attributes (analogous to a fixed effects). Fortunately, we were able to assure that the households in the C2003 sample were asked retrospective questions with regard to their demographics, poverty, schooling and work in the baseline year 1997. The preprogram information for the comparison group is crucial as we use it both as pre-program information for our variables of interest and as a source of matching variables. We use local linear matching and bootstrapping to calculate standard errors for the main estimates presented here.

13 The localities that were included in the sampling frame for C2003 were initially selected by matching on locality characteristics. This first matching procedure to determine the comparison group localities from which households were sampled is distinct from the finer matching that we perform to obtain our estimates, which uses both household and individual level data in selecting the matches.

To describe the estimators that we use, we first introduce some notation. Let $Y_1$ denote the outcome for persons who receive the treatment (T1998 persons) and $Y_0$ the outcome without treatment (C2003 persons). Let $D = 1$ if persons participate in the program and $D = 0$ if not. Denote as $\Delta$ the gain to the individual of moving from $Y_0$ to $Y_1$. Let $X$ denote other characteristics used as conditioning variables and let $P(X) = \Pr(D = 1 \mid X)$ denote the conditional probability of participating in the program, conditional on X. The matching literature refers to $P(X)$ as the propensity score.

The key parameter of interest in our evaluation is the *mean impact of treatment on the treated (TT)* (where the treated are defined as those persons participating in the program that we observe in the data in 2003).

$$TT = E(\Delta \mid X, D = 1) = E(Y_1 - Y_{0)} \mid X, D = 1) = E(Y_1 \mid X, D = 1) - E(Y_{0)} \mid X, D = 1)$$

A *cross-sectional matching estimator* assumes:

$$(CS.1) E(Y_{0t} \mid P(X), D = 1) = E(Y_{0t} \mid P(X), D = 0)$$

$$(CS.2) 0 < \Pr(D = 1 \mid X) < 1)$$

at some post-program-initiation time period t and for some subset of characteristics X. Under these conditions, an estimator for TT is

$$\Delta_{D=1} = (1/n_1) \sum_i Y_{1i}(P(X_i)) - E(Y_{0i} \mid P(X_i), D = 0)$$

where the sum is over $n_1$, the number of treated individuals with $X$ values that satisfy CS.2.14 $E(Y_{0i} \mid P(X_i), D = 0)$ represents the matched outcome for each treated individual, which can be estimated nonparametrically by nearest neighbor, kernel or local linear regression.

14 The CS.2 condition that insures that matches can be found for the treated individuals. See Heckman, Ichimura and Todd (1997) for a discussion of the relevance of common support restrictions for matching estimators.

The *difference-in-difference matching estimator* requires longitudinal or repeated cross-section data on program participants and nonparticipants. Let t and t′ be two time periods, one before the program start date and one after. $Y_{0t}$ is the outcome observed at time t. Conditions needed to justify the application of the estimator are:

$$(DID.1) E(Y_{0t} - Y_{0t'} \mid P(X), D = 1) = E(Y_{0t} - Y_{0t'} \mid P(X), D = 0)$$

$$(DID.2) 0 < \Pr(D = 1 \mid X) < 1)$$

where t is a post-program-initiation time period and t' a pre-program time period. The *DID* matching estimator, based on longitudinal data, is

$$\Delta_{D=1} = (1/n_{1t}) \sum_i [Y_{1i}(P(X_i)) - Y_{0it'} P(X_i) - E(Y_{0it} \mid P(X_i), D = 0) - E(Y_{0it'} \mid P(X_i, D = 0))]$$

Where $n_{1t}$ denotes the number of treated observations in the two time periods.

The propensity score matching estimators are estimated in two stages. In the first stage, $P(X)$ is estimated using a logistic model and a set X consisting of pre-program (1997) household and locality level characteristics.[15] In the second stage, the matched outcomes are constructed, i.e. $E(Y_{0t} \mid P(X, D = 0)$ for the cross-sectional estimator and, additionally, $E(Y_{0t'} \mid P(X), D = 0)$ for the difference-in-difference estimator.

For the T1998 communities, we use only those households initially eligible for the program under the original eligibility criteria, as these are the only households in the sample that received benefits for more than five years. For the C2003 group, we restrict the analysis to those eligible under the eligibility definition in effect in 2003. [16]

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15 For matching on X to be valid, the distribution of X should be unaffected by the receipt of treatment. Using 1997 preprogram characteristics ensures that this requirement is satisfied, because, in 1997, none of the respondents had any knowledge of the program. Post-program-initiation characteristics such as age are also valid, as they vary in a way that does not depend on treatment receipt.

16 Unfortunately, the original discriminant model used to determine who was eligible in 1997 could not be located or reproduced; therefore it has thus far not been possible to carry out the original model for the C2003 new comparison group. This would be advantageous as it would allow us to begin with more similar samples before the

The variables used for the matching include demographic characteristics of the households in 1997, grades of schooling completed of household head and spouse in 1997, whether the household head and spouse spoke an indigenous language in 1997, whether the household head and spouse were employed in 1997, a number of household characteristics and consumer and production durables in 1997, the *Oportunidade*s' *puntaje* score for program eligibility in 1997, income in 1997 and state of residence in 1997. Table 4.2 gives the estimated propensity score model, for which the variables are jointly significant at the 0.1% level (according to a $Chi^2$ test) and which has fairly good predictive power. Figures 3a and 3b show the distribution of propensity scores in the original treatment group (T1998) and the distribution of propensity scores in the C2003 comparison group. Although the distributions between the two groups are clearly different, there is adequate support in the sense of a number of households in C2003 that have propensity scores similar to those in T1998, although some comparison households with very high propensity scores are likely to be used a number of times as matches. To avoid matching children of different ages and genders, we implement the matching conditioning on age, sex, and the household's propensity score.

To check the specification of our propensity score model, we implemented several variants of balancing tests. First, we implemented a procedure suggested by Dehejia and Wahba (2002) in which we stratify treatment and control observations into stratum based on the estimated propensity score (in our case, quintiles). We then tested for significant differences between the covariates of the propensity score within each stratum. The vast majority of the covariates showed no significant differences by quintile; for the few with significant differences, we added some higher order terms to the propensity score model. We also carried out balancing tests summarized in Todd and Smith (2005). In particular, we carry out a test based on Rosenbaum and Rubin (1985) that calculates standardized differences for each covariate between the treatment and matched comparison group. Less than 5 percent of the covariates have standardized differences above the value of 20 percent, above which Rosenbaum and Rubin (1985) consider a "large bias".

An important issue for those indicators where we have pre-program information is the "before" program data. Traditionally, difference-in-difference matching uses longitudinal data, where the same individuals

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matching for the two groups, e.g. all households who would have been classified as eligible in 1997 according to the old eligibility criteria. The eligibility criteria in 1997 were somewhat more stringent than the criteria in 2003.

before and after the program are used. However, in the current context of a span of six years before and after program initiation, it is possible that in some cases using the same individuals may not adequately control pre-program differences between the treatment and comparison group. In particular, because of greater labor force participation with age, pre-program differences may be larger for older youth than younger youth. Graphs 4a through 4c show unadjusted pre-program differences between the T1998 and C2003 group for schooling, work and agricultural work. For schooling and labor force participation, there are relatively low differences before the age of 15, with significantly larger differences appearing after the age of 15. This, however, is not the case for agricultural work, where greater differences are evident before the age of 15. As an alternative to longitudinal difference-in-difference matching, we also carry out difference-in-difference matching using children who were of the same age pre- program, e.g. age 15 to 21. In summary, we present matching estimates using two different "before" groups. In the first, we carry out longitudinal difference-in-difference matching using the same individuals to measure "before" program indicators. In the second, we use as "before" program indicators, the group of youth aged 15 to 21 prior to the program in both the treatment and comparison groups, e.g. those of the same age as our post-program-initiation sample. As is discussed below, the results from both sets of estimators are fairly similar.

The analysis here is restricted to youth in the sample in 1997 and 2003. While migration rates for this group could be considered high (about 40 percent), effective attrition for our purposes is less than 15 percent, because information on outcomes for migrants, including schooling and occupation is provided by the parents or other household informants. [17]

**Section 5: *Oportunidades* Impacts on Schooling and Work**

Tables 4a, 5a and 6a present longitudinal matching results on schooling, work and participation in agricultural work, comparing those with 5.5 years of *Oportunidades* to those never receiving benefits. Tables 4b, 5b and 6b present difference-in-difference matching estimates using the same-age children pre-program for the same indicators.

[17] Behrman, Parker and Todd (2007) show that there are no statistically significant differences in overall attrition rates between the T1998 and T2000 samples although there are a number of significant treatment interactions with individual, parental and housing characteristics. They propose and implement a weighting method that is equivalent to a matching on observables approach to account for possible attrition biases.

*Schooling*

Table 4a shows important effects on school grades completed for both boys and girls, particularly those younger when the program began. Boys age 9 to 10 pre-program (15 to 16 in 2003) accumulate 1.0 additional grades of schooling than comparable boys without the program, while boys age 11-12 pre-program accumulate an additional 0.9 grades. Boys 13 to 15 pre-program accumulate about half a year of additional schooling. Impacts are also significant although slightly smaller for girls. Girls between the ages of 9 and 12 pre-program accumulate 0.7 to 0.8 grades of schooling, with no significant impacts for older girls. Compared with schooling grades completed of the comparison group in 2003 (about 7 grades), these impacts represent significant increases of about 12 to 15% for boys and 9 to 10% for girls in overall schooling grades completed, relative to the comparison group never receiving benefits.

The results based on difference-in-difference matching (Table 4b) using children of the same age, pre-program, show overall very similar impacts with the exception of boys aged 13 to 15 pre-program, where results suggest a significantly higher impact than that observed using longitudinal difference. Overall, the results show an increase in schooling levels attained because of the program.

*Probability of working*

The theoretical effect of *Oportunidades* on the probability of working is ambiguous. On the one hand, as shown in previous evaluations (Parker and Skoufias, 2000), the program should reduce work for children spending more time in school. On the other hand, for youth who have already left school, but have more schooling as a result of the program, one might expect to find a positive effect on work.

The matching results in Tables 5a and 5b, for younger boys, e.g. those age 9 to 10 pre-program show a negative and significant impact on the probability of employment, consistent with the point that at this age, e.g. 15 to 16 after-program, many boys are still attending school and thus less likely to be working. The magnitude of the impact is large, corresponding to a reduction of almost 30 percent in the probability of working. For the older age groups, there is no significant impact of the program on working, which may mask negative impacts from some boys continuing to study and positive impacts from those having

completed their studies and entering the work force. For girls, who in these traditional communities have much lower wage labor force participation rates than boys, there is no significant impact on working for younger girls. However, for those girls aged 13 to 15 pre-program (19-21 post), there is an important and significant increase in the proportion working, of 6 percentage points corresponding to an increase of 20 percent for the longitudinal matching (Table 5a) and 9 percentage points for the same age difference-in-difference matching (Table 5b). Note however this older group of girls showed no significant increase on schooling, suggestive that the program may influence work through other channels. One possibility is that older girls substitute in the labor market for their younger siblings, who do show increases in their schooling and work (in the case of boys).

*Probability of working in agriculture:*

Schooling is often claimed to have higher returns in non-agricultural than in agricultural work, so if *Oportunidades* were effective it might induce some shift out of the agricultural sector. We look at the unconditional probability of individuals participating in agricultural work.[18] Note that overall, girls have much lower participation in agricultural work than boys. For boys, the reductions in agricultural work for boys aged 9 to 10 pre-program are similar under both sets of matching estimates to the percentage reductions in work, implying similar reductions in agricultural as non-agricultural work. (Tables 6a and 6b). For older boys, e.g. age 13 to 15 pre-program, however, on which there were no overall significant program effects on working, there are significant reductions in agricultural work, implying some substitution from agricultural work to non-agricultural work. For girls, there is no overall effect on participation in agricultural work for younger girls, who also showed no effect on working. For older girls, for which there was a significant increase in the probability of working, there is no significant increase in the probability of participating in agricultural work, again suggestive of some substitution away from agricultural work.

In summary, the results here demonstrate large increases in schooling due to *Oportunidades* after 5.5 years of benefits for both boys and girls, implying increases in their overall level of schooling of between

18 We do not condition on working as this itself is an impact indicator, subject to changes with the program, as demonstrated above. Thus, the impacts on the proportion of individuals in agriculture might change under the program both as a result of working individuals shifting in or out of agriculture as well as new individuals entering/exiting work.

10 and 15 percent. For younger children, the program continues to reduce working, mainly for boys. Older boys do not show increases in work, but this likely conflates reductions in work of youth still in school with increases in work of those finishing school. Older girls, on the other hand, show significant increases in working, although no increase in schooling, potentially suggestive of substitution for the labor market participation of younger siblings. For older youth, the program is also suggestive of substitution out of agricultural employment to non-agricultural opportunities.

How reasonable are the estimates presented here? First, note that in previous work Behrman, Parker and Todd (2007), we have estimated impacts of longer durations of program exposure by comparing the T2000 and T1998 group. The comparison is between individuals with 5.5 years of benefits versus 4 years, that is, the program impacts represent differential exposure estimates of 1.5 years. Behrman, Parker and Todd show that the 1.5 years of additional program receipt translate into an additional 0.2 to 0.3 grades of schooling, very consistent with the matching estimates here that report 0.8-1.0 grades of additional schooling after 5.5 years of benefits if schooling impacts are approximately cumulative as seems plausible.

As an additional check, we also carry out estimates comparing the T2000 group with the C2003 group on schooling. This compares individuals receiving 4 years of benefits with those never receiving benefits. Again, one would expect proportionately lower estimated impacts on schooling from this comparison than that based on 5.5 years. These results are presented in Table 7 and effectively demonstrate somewhat smaller impacts of the program, on the order of 25 to 30 percent as would be expected.

Finally, Appendix Table 1 presents longitudinal matching estimates on schooling varying the bandwidth for the local linear matching estimates as well as provides kernel matching estimates. The results are similar across these different matching estimators.

**6. Achievement Tests**

The observed impacts on schooling indicate that *Oportunidades* substantially increases the average amount of time students spend in school, which might suggest that the program would positively impact achievement tests. As part of the ENCEL2003 fieldwork, achievement tests in reading, math and written

language skills were administered to a sub-sample of adolescents 15 to 21 years of age in 2003. The tests are a Spanish version of Woodcock Johnson tests, which are commonly applied in the United States to measure academic achievement. [19] Figures 3a through 3b show density histograms of math and writing, where the sum of the area of the bars equals one. [20, 21]

The achievement tests were applied several months prior to the ENCEL2003 in a separate operation focused on collecting biological samples of mothers, small children and adolescents. Achievement tests were applied in the household so they cover youth regardless of whether they are attending school. The original sample design called for the achievement tests to be applied to 3000 adolescents in each of the three groups (T1998, T2000 and T2003). However, only about 7000 youth took the tests and, of those taking the tests, many were youth not originally eligible for the program and/or were new arrivals to the household. For this analysis, we focus on the subset of the approximately 3000 observations of youth who were program-eligible, who were in the household pre-program in 1997, and were in either the T1998 and C2003 group. This sub-sample may not be representative of all the eligible youth, e.g. for instance they are presumably more likely to have been in the household at the time of the interview, which may be correlated with activities such as work. To gauge how different this sample is from the entire sample, we also carried out difference-in-difference estimates of the impact of *Oportunidades* on grades of schooling for this sub-sample. The results (Table 8) for boys are similar as to those shown in Tables 4a and 4b with the exception that the sample of older boys (13-15 pre-program) taking the tests show no significant impacts on schooling. For girls, the impacts on younger girls are similar to those with the full sample; however, older girls (13 to 15 pre-program) who showed no significant impacts in the whole sample, now show large and important program impacts of an additional grade of schooling. For older girls, taking the tests appears to be predictive of program impacts, perhaps because being in the household correlates with still being in school.

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[19] Note that the scores of the tests cannot be compared with the levels of achievement of Mexican children outside of the ENCEL sample where they were applied, because these tests have not been more broadly utilized in Mexico. They can, however, be roughly compared with the average levels of attainment of children in the United States or in other Latin American countries where the tests have been utilized.

[20] The graph of reading scores shows many test takers are bunched at the right tail of the distribution, implying that they scored near the maximum raw score. This is problematic for the analysis because, for the individuals who are at the top without the program, we cannot identify positive impacts of the program. We thus restrict attention to math and writing tests that show much greater dispersion.

[21] For the analysis, we use the raw scores reported on each test. The tests also could be standardized by age to facilitate comparing the scores to a US-based population. For our analysis in which we always control for age, it is sufficient to use the raw scores.

A potentially serious problem for the evaluation of cognitive achievement effects is that we have no baseline information on achievement tests. If pre-program schooling levels are higher in the C2003 group (see Table 4b), and assuming that schooling affects the level of test scores,[22] the C2003 group would also presumably have pre-program higher scores on the achievement tests, thus we may underestimate program impacts using only post program information.[23]

To get an idea of the potential for such a bias, we estimate cross-sectional matching estimates of schooling impacts in 2003 for the sub-sample of eligible youth who took the achievement tests, e.g. ignoring information on pre-program levels. Table 8 shows that without using pre-program information, we would underestimate the program's impact on schooling, particularly for male youth. Impact estimates based only on post-program-initiation data are in the range of only 3 to 5 percent (and none are statistically significant) for boys aged 9 to 12 pre-program, compared with their DID estimate of 10 to 16 percent. For girls, the post–program cross-sectional estimates underestimate impacts by somewhat less and for girls aged 11-15 pre-program, continue to be significant. The overall implication is that the impacts on achievement test scores are also likely to be similarly underestimated because of the lack of pre-program test score data. Still, if the increased schooling from *Oportunidades* results in increased learning, some positive impacts on achievement tests should be observed.

Table 9 present the matching impacts of *Oportunidades* on test scores in mathematics and in writing. For boys, the results are all insignificant, and 4 of 6 coefficients are negative. For girls, the oldest group e.g. 13 to 15 pre-program shows positive, marginally significant at the 10 percent level, coefficients on math and writing. This is also the group for which we observed the largest schooling impacts on for the sub-

22 Parker, Behrman and Todd (2004) show in a regression analysis that the Woodcock Johnson test scores are highly and positively correlated with completed grades of schooling, suggestive that children with higher years of schooling do better on achievement tests, although omitted variables such as ability might explain both higher grades of schooling and achievement tests results.

23 A possible solution would be to use the ineligible group with test score information in the T1998 and C2003 groups to difference out the "pre-program" difference under the assumption that these differences would be similar for eligibles and ineligibles. A limitation is that most of the households originally ineligible for the program began to receive benefits over time due to changing definitions of program eligibility, so there are few non-beneficiary youth with information on test scores in the treatment groups. We did however use this strategy to estimate difference-in-difference matching restricting analysis of ineligibles in T1998/T2000 to those never receiving benefits. These results (available upon request) are not substantially different than those presented here.

sample of those applied achievement tests. The magnitude of the impact (6 percent) is only about half the size of the estimated impact on schooling though.

To summarize, the evidence for impacts on achievement test scores is not compelling, although the results are clearly hampered by the lack of a baseline survey on achievement tests. With this caveat, potential explanations for the lack of impact on achievement tests might involve school quality, that is, attendance at a low school quality school might result in a higher level of schooling attainment without much improvement on achievement tests.[24] And the enrollments induced by *Oportunidades* also may have lowered school quality both through congestion and through adding marginal students who may have diverted resources and attention from the students who would have been in school in the absence of the program.[25] Given the extent to which conditional cash transfer programs are spreading throughout the world, these issues are clearly worthy of further investigation and collecting more information on cognitive achievement and schooling quality would certainly be useful in this regard.

**7. Benefit-Cost Estimates**

The relatively large impacts on schooling attainment documented here raise the issue of whether the benefits of the *Oportunidades* program justify the costs. One might inquire as to how *Oportunidades* performs given its cost relative to other types of human capital programs. However, because *Oportunidades* also alleviates current poverty by giving money directly to the poor, it is probably not correct to judge *Oportunidades* as only a human capital investment program. Nevertheless, in this section, we offer a benefit-cost analysis of the *Oportunidades* program as if the only objective of the program were investment in human capital and the only benefits the subsequent impact on future earnings of the increased schooling attainment of students affected by *Oportunidades*.

24 A number of empirical studies emphasize the importance of school quality in Latin American (e.g., Behrman and Birdsall 1983) and elsewhere (e.g., Card and Krueger 1992).

25 On the other hand, the results that we report do not reflect the hypothetical situation in which *average* achievement test scores declined because the marginal students who were induced to enroll school due to the program pulled down the averages through their low scores despite improved performance by students who would have been in school in any case. Because we are following individual students over time, not average scores, our analysis is not contaminated by such compositional changes.

Here we assume the benefits from the program arise from increases in future earnings as a result of the increase in schooling attainment from the program, ignoring other potential impacts, such as that of improved health and nutrition. The estimated benefits will thus critically depend on the level of returns to schooling attainment. While a number of simple Mincer equations have been estimated for the Mexican labor market (e.g., Rodriguez-Oreggia, 2005), there are no endogeneity-corrected estimates reported in the literature. In an ongoing separate project, we have estimated the return to schooling controlling for potential endogeneity using a plausible instrument given by a change in the compulsory schooling law in 1993 from sixth to ninth grade.[26] The estimates are based on data from the Mexican Family Life Survey, which is a broad, nationally representative sample. Here, we use the estimated returns to provide a guide for what the estimated returns to schooling attainment for youth in rural areas are likely to be, and we also provide simulations based on varying levels of returns.

Focusing on the sample of men, a simple OLS estimate of returns to schooling attainment gives a value of 7.5 percent per year for this age group (16 to 24), which is similar across rural (7.2) and urban (7.6) areas. The IV estimate of wages actually increases the value to about ten percent both for those residing in rural and in urban areas. [27] We provide simulations below based on 6 percent, 8 percent and 10 percent returns to schooling attainment.

The resource costs of the program include the administrative costs of the program (costs of transferring benefits, conditionality and targeting) and the private costs associated with participation in the program.[28]

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26 In 1993, lower secondary school (seventh through ninth grade) became obligatory. This was a nationwide change, and the main impact of this change appears to have been a very large increase in the construction of lower secondary schools, the majority (80 percent over the period 1993-2000) of which were of the tele-secondary school type, which is a mode of secondary school provided only in rural areas. Thus the main impact of the change in law and the consequent large construction in schools appears in rural areas. We use the Mexican Family Life Survey 2002 to construct an schooling history and residence/migration history and we link the MXFLS to state level data on tele-secondary school construction from 1990 to 2003, interacted with whether an individual lived in rural areas at the age of 12 as a variable affecting completed schooling in 2002.( (Details provided in Behrman, Parker and Todd, 2007). Since the reform and associated school construction are relatively recent, we are only able to focus on the sample of individuals aged 16 to 24 in 2002, which corresponds well with the age groups we study here in this paper but gives us little information on whether estimated returns to schooling might differ for older age groups.

27 It is perhaps surprising that the instrumental variable estimates increase the estimated return to schooling attainment if there is ability bias in the OLS estimates, although the control for random measurement error means that the estimate might increase if this effect outweighs the ability bias effect. Ashenfelter and Krueger 1994 find similar results for the United States and Duflo 2001 finds similar results for Indonesia.

28 They do not include the budgetary costs of the schooling grants based on the calendar of grants (see Table 1) because these are transfers, not resource costs (see Knowles and Behrman 2004, 2005). We have been able to find

The administrative and private costs of participation in the program including the monetary and time costs of transportation associated with greater school attendance were calculated in Coady (2000). He estimates that for each 100 pesos transferred, administrative and private costs are equivalent to 11.3 pesos. Coady however does not take into account the opportunity costs of spending more time in school or the additional household expenditures, other than transportation, related to schooling which might occur, such as spending on school uniforms. We encompass that an additional year spent in school results in a 0.75 year decrease in time spent in the labor force.[29] We obtain estimates of the additional household expenses incurred because of increased schooling, e.g. school supplies and children's clothing, from an earlier study by Hoddinott, Skoufias and Washburn (2000)[30]. Finally, for each peso transferred we assume a 25% increase in cost (grants plus administrative) from the distortions associated with raising revenues.[31] The resource costs of the education grants is calculated assuming that each youth in our sample receives the grants for 6 years. Youth in the absence of the program are assumed to begin their working career at age 18 and conclude at age 70. All costs and benefits are discounted to time zero.

We assume a starting salary equal to the average obtained by youth aged 18 in the rural ENCEL areas of 2003, which is equal to $US163 monthly. This is a conservative estimate of initial earnings opportunities as probably a number of youth will migrate to urban areas, where salaries tend to be higher. We use the schooling impacts on boys aged 15 to 16 (see Tables 4a and 4b) obtained under the two different matching methods.

Table 10 provides benefit-cost estimates of *Oportunidades*, under the three different assumptions of the rate of return to schooling attainment (6, 8 and 10 percent) and three potential discount rates (3, 5 and 10

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no evidence that school resource costs increased due to the increased enrollment so we do not include a measure of these costs. In an earlier study we also found no evidence of congestion costs or other spillover effects on students in the same schools who were not program beneficiaries, so we do not adjust for spillover costs either (Behrman, Sengupta and Todd 2005).

29 Skoufias and Parker (2001) show that the reduction in work of Oportunidades in the early years of the program was about 75% of the size of the increase in school enrollment.

30 This probably overestimates the size of additional household resources as we assume the increase in spending on child clothing is entirely due to the conditionality effect whereas part of this impact may be an income or bargaining effect.

31 The distortionary cost (often called the "deadweight loss") of raising a dollar of tax revenue in the United States has been estimated to range from $0.17 to $0.56, depending on the type of tax used (e.g., Ballard, Shoven and Whalley 1985, Feldstein 1995). Estimates for some other countries range from $0.18 to $0.85, depending on the tax (van der Gaag and Tan 1997). Harberger (1997) suggests using a shadow price of $1.20-1.25 for all fiscal flows on a project. We use the upper number in his suggested range.

percent). Program benefits are several times higher than program costs under nearly all scenarios, with the exception of a very high discount rate and a low estimated return to education. Overall, then, even if the program were considered as only a human capital investment program, the overall benefits would seem to significantly outweigh the costs.

## 8. Conclusions

This paper has gone beyond the previous short-run studies of the direct impact of *Oportunidades* on time in school by analyzing impacts of *Oportunidades* on rural adolescent youth after 5.5 years of benefits. It examines impacts on schooling attainment as well as impacts on cognitive achievement tests and on working. With respect to schooling, the results are indicative of significant increases. The matching estimates suggest that boys aged 9 to 12 pre-program achieve about 0.9 to 1 grades of additional schooling and girls 9 to 12 pre-program attain about 0.7 to 0.8 additional grades compared with similar children not receiving the program. Estimates using longitudinal difference-in-difference matching and repeated cross-sectional difference-in-difference matching, holding age constant, show similar results. The estimates imply an increase in overall schooling attainment of about 12 to 15 percent for boys and 9 to 10 percent for girls after 5.5 years of program benefits.

With respect to work, the program significantly decreases the rate of working for young boys (aged 9 to 10 pre-program), presumably due to postponement of entering the labor force because of more time spent in school. Older boys show no significant impact on labor force participation, perhaps conflating positive impacts for those finishing their schooling and continued negative impacts for those still in school. For girls, who have low overall rates of labor force participation, there is evidence for significant and positive impacts on work only for older girls (13 to 15 pre-program), although this group demonstrated no significant increase on schooling levels. Finally, for both male and female youth, participation in the program is associated with shifting away from agricultural work, particularly for older youth.

In summary, the medium-term impacts of the program appear to be quite positive with important increases in schooling attainment, and for older youth, some higher rates of working and a shift away from agricultural work to non-agricultural work. Estimates based on earnings functions from adults indicate fairly high benefits-to-costs unless the rate of return to schooling is assumed to be low and the

discount rate high. Future rounds of the evaluation sample will however be necessary to follow the youth as they enter adulthood to confirm to what extent the increases in schooling are associated with a sustained increase in labor income.

We found few significant impacts of the program on achievement tests. As previously discussed, this finding is likely partially a consequence of the lack of pre-program data (which we have for other indicators). Nonetheless, a crucial area for future research remains to ascertain the impact of conditional cash transfer programs on achievement, particularly given the number of similar programs in place or in the planning stages in other countries in Latin America and elsewhere. Similar findings in other contexts might suggest the importance of design changes to the program such as relating grants to performance rather than enrollment, and a greater focus on improving school quality.

**Table 1: Monthly amount of educational grants (pesos) in second semester of 2003**

| Grade | Boys | Girls |
|---|---|---|
| **Primary** | | |
| 3rd year | 105 | 105 |
| 4th year | 120 | 120 |
| 5th year | 155 | 155 |
| 6th year | 210 | 210 |
| | | |
| **Secondary** | | |
| 1st year | 305 | 320 |
| 2nd year | 320 | 355 |
| 3rd year | 335 | 390 |
| | | |
| **Upper Secondary (High School}** | | |
| 1st year | 510 | 585 |
| 2nd year | 545 | 625 |
| 3rd year | 580 | 660 |

**Table 2: Sample size of adolescents 9 to 15 in 1997, 15 to 21 in 2003**

| | Original treatment group: T1998 | Original control group: T2000 | New comparison group:C2003 |
|---|---|---|---|
| Adolescents in both 1997 ENCASEH and ENCEL2003 (or retrospective 1997 information for C2003 group) | 8,984 | 5,591 | 5,011 |
| Program eligible adolescents in both 1997 ENCASEH and ENCEL2003 with 1997 ENCASEH | 6,182 | 3,742 | 4,795 |
| | | | |
| Total # applied WJ tests | 2,918 | 2,605 | 2,143 |
| Program eligible adolescents applied achievement tests with information in 1997 and 2003. | 1,426 | 1,216 | 1,651 |

Note: Program eligible refers to eligibility in 1997, for the C2003 group, the program's 2003 eligibility criteria is reported here. See text for more details.

**Table 3: Logit Model for Probability of Participating in Rural *Oportunidades***
*D=1 Original poor households in treatment group 1998*
*D=0 Households eligible under new model in new comparison group*

| Variable | Coef. | Std. Err. | z | Variable | Coef. | Std. Err. | z |
|---|---|---|---|---|---|---|---|
| Age of Household head | 0.002 | 1.640 | 0.101 | Total HH income squared | 0.000 | 0.000 | 1.320 |
| Age of Spouse | -0.004 | 0.003 | -1.650 | Blender | -0.011 | 0.052 | -0.210 |
| Gender of Household head | 0.644 | 0.090 | 7.170 | Refrigerator | 0.183 | 0.091 | 2.020 |
| Hh head speaks indigenous lang. | 0.274 | 0.066 | 4.160 | Gas stove | 0.576 | 0.065 | 8.910 |
| Spouse speaks indigenous lang. | 0.267 | 0.072 | 3.720 | Gasheater | 0.250 | 0.129 | 1.950 |
| Grades of schooling HH head | -0.058 | 0.008 | -7.320 | Radio | 0.109 | 0.035 | 3.070 |
| Grades of schooling spouse | -0.129 | 0.009 | -15.100 | Television | 0.150 | 0.043 | 3.450 |
| Employed HH head | -0.542 | 0.075 | -7.230 | Video | 0.304 | 0.142 | 2.140 |
| Employed spouse | -0.445 | 0.054 | -8.300 | Washer | 0.603 | 0.154 | 3.920 |
| Children 0 to 5 | -0.393 | 0.023 | -16.900 | Car | -0.098 | 0.177 | -0.550 |
| Children 6 to 21 | -0.265 | 0.024 | -10.950 | Truck | 0.048 | 0.126 | 0.380 |
| Children 13 to 15 | 0.036 | 0.030 | 1.190 | State1 | 1.144 | 0.116 | 9.820 |
| Children 16 to 20 | 0.060 | 0.024 | 2.480 | State2 | 0.630 | 0.080 | 7.870 |
| Women 20 to 39 | 0.086 | 0.058 | 1.500 | State3 | 0.768 | 0.084 | 9.150 |
| Women 40 to 59 | -0.104 | 0.048 | -2.160 | State4 | 0.341 | 0.080 | 4.270 |
| Women 60+ | -0.379 | 0.045 | -8.450 | State5 | 0.382 | 0.080 | 4.790 |
| Men 20 to 39 | 0.021 | 0.038 | 0.560 | State6 | -0.463 | 0.075 | -6.130 |
| Men 40 to 59 | -0.495 | 0.050 | -9.810 | Missing grades of schooling HH head | -2.156 | 0.080 | -26.970 |
| Men 60+ | -0.727 | 0.056 | -13.100 | Missing grades of schooling spouse | -2.352 | 0.116 | -20.240 |
| # Rooms | 0.238 | 0.020 | 12.020 | Missing age HH head | 0.937 | 0.853 | 1.100 |
| Electricity in HH | -0.242 | 0.040 | -6.100 | Missing age spouse | -0.836 | 1.321 | -0.630 |
| Water in HH | -0.367 | 0.039 | -9.430 | Missing indig HH head | 1.974 | 1.387 | 1.420 |
| Dirt floor | -0.583 | 0.048 | -12.050 | Missing working HH head | 1.045 | 2.219 | 0.470 |
| Room material (inferior) | 0.197 | 0.043 | 4.610 | Missing working spouse | 1.087 | 0.409 | 2.660 |
| Wall material (inferior) | 0.135 | 0.046 | 2.940 | Missing water | -0.336 | 0.468 | -0.720 |
| Own animals | 0.198 | 0.040 | 4.960 | Missing electricity | 0.404 | 0.522 | 0.770 |
| Own land | 0.223 | 0.037 | 6.080 | Missing rooms | 0.371 | 0.345 | 1.070 |
| Score | 1.632 | 0.108 | 15.120 | Missing income | -2.203 | 0.762 | -2.890 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Score squared | -0.115 | 0.015 | -7.630 | Missing ownanimals | -0.609 | 0.534 | -1.140 |
| Total HH income | -0.000 | 0.000 | -16.480 | Missing ownland | 0.824 | 0.523 | 1.570 |
| | | | | Constant | -1.658 | 0.242 | -6.870 |
| Number of obs | 13336 | | | | | | |
| LR chi2(60) | 10591 | | | Pseudo R2 | 0.584 | | |
| Prob > chi2 | 0.000 | | | Log likelihood | -3771.644 | | |

**Table 4a**
**Estimated Impacts of *Oportunidades* on Grades of Education**
**Local linear matching † T1998 vs. C2003**

**Longitudinal difference-in-difference matching**

| Age in 2003 | Impact (Standard error) | Obs | # common support | C2003 grades of schooling, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | 0.69 (0.16) | 1735 | 1713 | 6.92, 10.0% |
| 17-18 | 0.75 (0.18) | 1643 | 1623 | 7.54, 10.0% |
| 19-21 | 0.03 (0.18) | 1846 | 1822 | 7.19, 0.4% |
| **Boys** | | | | |
| 15-16 | 1.00 (0.15) | 2022 | 1994 | 6.85, 14.6% |
| 17-18 | 0.93 (0.21) | 1703 | 1682 | 7.20, 13.0% |
| 19-21 | 0.55 (0.18) | 2022 | 1994 | 7.08, 7.8% |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 4b**
**Estimated Impacts of *Oportunidades* on Grades of Education**
**Local linear matching † T1998 vs. C2003**
**Difference-in-difference matching using children of same age pre-program**

| Age in 2003 | Pre-program difference | Alter-program difference | **Impact**: Difference-in-difference matching | C2003 Grades of schooling, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | -0.50 | 0.32 | 0.82 | 6.92, 11.8% |
| | (0.33) | (0.18) | (0.38) | |
| 17-18 | -0.36 | 0.47 | 0.83 | 7.54, 11.0% |
| | (0.42) | (0.22) | (0.47) | |
| 19-21 | 0.09 | 0.33 | 0.24 | 7.19, 3.3% |
| | (0.30) | (0.22) | (0.37) | |
| **Boys** | | | | |
| 15-16 | -0.21 | 0.62 | 0.83 | 6.85, 12.1% |
| | (0.31) | (0.16) | (0.35) | |
| 17-18 | 0.03 | 0.51 | 0.48 | 7.20, 6.7% |
| | (0.05) | (0.18) | (0.19) | |
| 19-21 | -0.67 | 0.60 | 1.27 | 7.08, 18.0% |
| | (0.35) | (0.22) | (0.00) | |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 5a**
**Estimated Impacts of *Oportunidades* on Probability of Working**
**Local linear matching † T1998 vs. C2003**

**Longitudinal difference-in-difference matching**

| Age in 2003 | Impact (Standard error) | Obs | # common support | C2003 working, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | 0.01 (0.03) | 1889 | 1867 | 0.17, 5.9% |
| 17-18 | -0.01 (0.04) | 1764 | 1743 | 0.28, 3.6% |
| 19-21 | 0.064 (0.038) | 2114 | 2089 | 0.32, 20% |
| **Boys** | | | | |
| 15-16 | -0.14 (0.04) | 2019 | 1994 | 0.49, 28.6% |
| 17-18 | 0.06 (0.04) | 1837 | 1816 | 0.70, 8.6% |
| 19-21 | -0.02 0.04 | 2252 | 2223 | 0.81, 2.5% |

† Difference-in-difference estimator, imposing common support (trimming=2%), 5 neighbors. Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 5b**
**Estimated Impacts of *Oportunidades* on Probability of Working**
**Local linear matching † T1998 vs. C2003**

**Difference-in-difference matching using children of same age pre-program**

| Age in 2003 | Pre-program difference | Alter-program difference | Impact: Difference-in-difference matching | C2003 working, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | 0.01 | 0.01 | 0.00 | 0.17, 0% |
| | (0.03) | (0.03) | (0.04) | |
| 17-18 | 0.04 | 0.002 | -0.04 | 0.28, 14.3% |
| | (0.03) | (0.03) | (0.04) | |
| 19-21 | -0.01 | 0.08 | 0.09 | 0.32, 28.1% |
| | (0.03) | (0.03) | (0.04) | |
| **Boys** | | | | |
| 15-16 | 0.01 | -0.18 | -0.19 | 0.49, 38.8% |
| | (0.05) | (0.03) | (0.06) | |
| 17-18 | 0.00 | -0.007 | -0.007 | 0.70, 1.0% |
| | (0.07) | (0.04) | (0.08) | |
| 19-21 | 0.05 | -0.006 | -0.056 | 0.81, 7.0% |
| | (0.04) | (0.03) | (0.05) | |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 6a**
**Estimated Impacts of *Oportunidades* on Probability of Agricultural Work**
**Local linear matching † T1998 vs. C2003**

**Longitudinal difference-in-difference matching**

| Age in 2003 | Impact (Standard error) | Obs | # common support | C2003 agric work, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | -0.004 (0.02) | 1890 | 1868 | 0.03, 13.3% |
| 17-18 | -0.02 (0.02) | 1764 | 1744 | 0.06, 33.3% |
| 19-21 | 0.01 (0.02) | 2114 | 2089 | 0.05, 20.0% |
| **Boys** | | | | |
| 15-16 | -0.09 (0.04) | 2020 | 1996 | 0.35, 25.7% |
| 17-18 | -0.02 (0.04) | 1837 | 1815 | 0.43, 4.7% |
| 19-21 | -0.094 0.047 | 2252 | 2223 | 0.46, 20.4% |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 6b**
**Estimated Impacts of *Oportunidades* on Probability of Agricultural Work**
**Local linear matching † T1998 vs. C2003**

**Difference-in-difference matching using children of same age pre-program**

| Age in 2003 | Pre-program difference | After-program difference | **Impact**: Difference-in-difference matching | C2003 agric work, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | -0.02 | 0.00 | 0.02 | 0.03, 66.7% |
| | (0.02) | (0.01) | (0.02) | |
| 17-18 | 0.03 | 0.00 | -0.03 | 0.06, 50.0% |
| | (0.02) | (0.02) | (0.03) | |
| 19-21 | -0.04 | 0.01 | 0.05 | 0.05,100.0% |
| | (0.02) | (0.01) | (0.02) | |
| **Boys** | | | | |
| 15-16 | 0.01 | -0.11 | -0.12 | 0.35, 34.3% |
| | (0.05 | (0.03) | (0.06) | |
| 17-18 | 0.03 | -0.05 | -0.08 | 0.43, 18.7% |
| | (0.06) | (0.04) | (0.06) | |
| 19-21 | 0.03 | -0.04 | -0.07 | 0.46, 15.2% |
| | (0.06) | (0.03) | (0.07) | |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 7**
**Robustness: Estimated Impacts of *Oportunidades* on Grades of Education**
**Local linear matching † T2000 vs. C2003**

**Longitudinal difference-in-difference matching**

| Age in 2003 | Impact (Standard error) | Obs | # common support | C2003 Grades of schooling, % impact |
|---|---|---|---|---|
| **Girls** | | | | |
| 15-16 | 0.57 (0.17) | 1324 | 1310 | 6.92, 8.2% |
| 17-18 | 0.55 (0.18) | 1236 | 1224 | 7.54, 7.3% |
| 19-21 | -0.43 (0.18) | 1412 | 1396 | 7.19, -6.0% |
| **Boys** | | | | |
| 15-16 | 0.88 (0.16) | 1350 | 1336 | 6.85, 12.8% |
| 17-18 | 0.70 (0.21) | 1330 | 1316 | 7.20, 9.7% |
| 19-21 | 0.40 (0.14) | 1229 | 1445 | 7.08, 5.6% |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 8**
**Sub-sample of youth taking achievement tests: Impacts on Grades of Education.**
**Local linear matching † T1998 vs. C2003**

| | Difference-in-difference estimator of schooling | | After program difference estimator | | Obs |
|---|---|---|---|---|---|
| Age in 2003 | Education03-Education97 | C2003 Grades of schooling, % impact | Education03 | C2003 Grades of schooling, % impact | |
| **Girls** | | | | | |
| 15-16 | 0.60 (0.24) | 7.18, 8.4% | 0.30 (0.25) | 7.18, 4.2% | 571 |
| 17-18 | 0.98 (0.25) | 7.52, 13.0% | 0.60 (0.30) | 7.52, 8.0% | 517 |
| 19-21 | 1.00 (0.30) | 7.14, 14.0% | 0.86 (0.50) | 7.14, 12.0% | 306 |
| **Boys** | | | | | |
| 15-16 | 1.18 (0.25) | 7.4, 16.0% | 0.40 (0.27) | 7.4, 5.4% | 568 |
| 17-18 | 0.80 (0.32) | 7.9, 10.1% | 0.21 (0.30) | 7.9, 2.7% | 440 |
| 19-21 | 0.05 (0.57) | 7.5, 0.1% | -0.27 (0.81) | 7.5, -0.4% | 264 |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 9**
**Estimated Impacts of *Oportunidades* on Math and Writing Achievement tests**
**Local linear matching † T1998 vs. C2003**

**Difference matching**

| Age in 2003 | Math | | Writing | | Obs |
|---|---|---|---|---|---|
| | Impact (Standard error) | C2003 math score, % impact | Impact (Standard error) | C2003 writing score, % impact | |
| **Girls** | | | | | |
| 15-16 | -1.44 (0.62) | 31.6, -4.6% | -0.43 (0.83) | 35.4, -1.2% | 600 |
| 17-18 | -0.43 (0.78) | 31.9, -1.3% | 0.73 (0.93) | 35.6, 2.1% | 550 |
| 19-21 | 1.92 (1.42) | 32.0, 6.0% | 2.41 (1.74) | 34.9, 6.9% | 334 |
| **Boys** | | | | | |
| 15-16 | -0.88 (0.80) | 32.9, -2.7% | 0.15 (0.93) | 35.4, 0.4% | 584 |
| 17-18 | -0.08 (0.85) | 32.8, -0.2% | 1.27 (1.21) | 35.8, 3.5% | 446 |
| 19-21 | -1.02 (1.54) | 33.3, -3.1% | -0.12 (2.16) | 34.4, -0.3% | 275 |

† Difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications. Bandwidth=0.8.

**Table 10: Costs and Benefits of the Oportunidades Progam in US dollars***

| Impact=1.0 grades of schooling | | | | | | Return to education | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Initial Earnings | | | 6% | | 8% | | 10% | |
| Discount rate | W/o program | With program | Costs | Benefits | B/C Ratio | Benefits | B/C Ratio | Benefits | B/C Ratio |
| 3% | 1855 | 1966 | 500 | 1801 | 3.60 | 2679 | 5.36 | 3557 | 7.11 |
| 5% | 1855 | 2003 | 390 | 664 | 1.70 | 1082 | 2.77 | 1499 | 3.84 |
| 10% | 1855 | 2040 | 215 | 27 | 0.13 | 233 | 1.08 | 438 | 2.04 |

| Impact=0.83 grades of schooling | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Initial Earnings | | | 6% | | 8% | | 10% | |
| Discount rate | W/o program | With program | Costs | Benefits | B/C Ratio | Benefits | B/C Ratio | Benefits | B/C Ratio |
| 3% | 1855 | 1966 | 500 | 1502 | 3.00 | 2231 | 4.46 | 2959 | 5.92 |
| 5% | 1855 | 2003 | 390 | 556 | 1.43 | 903 | 2.32 | 1245 | 3.19 |
| 10% | 1855 | 2040 | 215 | 28 | 0.13 | 198 | 0.92 | 363 | 1.69 |

*Assuming youth take part in the program for 6 years, beginning at age 10 and finishing at age 16.
Impact: impact on boys aged 15-16 in 2003, see Table 4a and 4b.
**A return to experience is included=$0.413E-0.00048E^2$
Costs include distortionary costs of raising funds, administrative program costs and private costs associated with participation, including HH expenditures on transportation and school supplies.
Benefits are net of opportunity cost, e.g. delayed entry to labor market.
Conversion factor: 11 Mexican pesos per 1US$

**Figure 1a School Enrollment and Labor Force Participation of Boys in *Oportunidades* Communities Prior to Program Implementation**

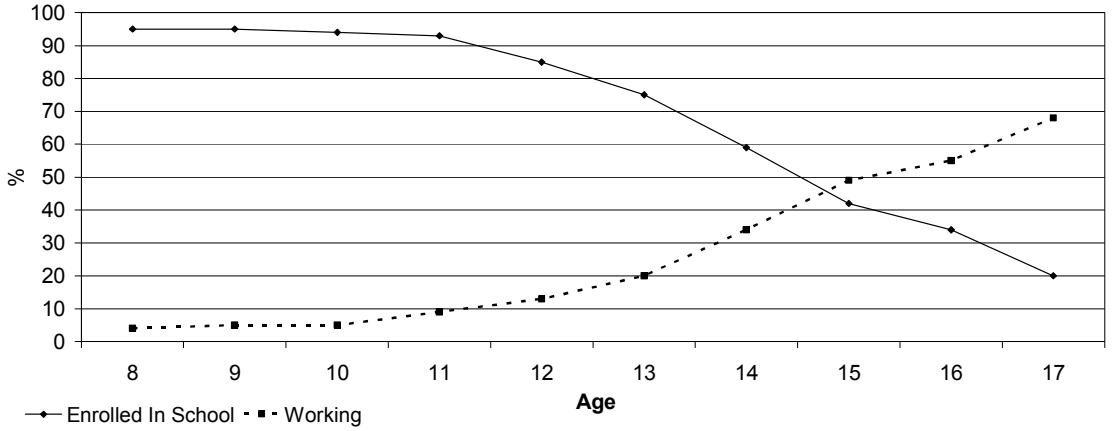
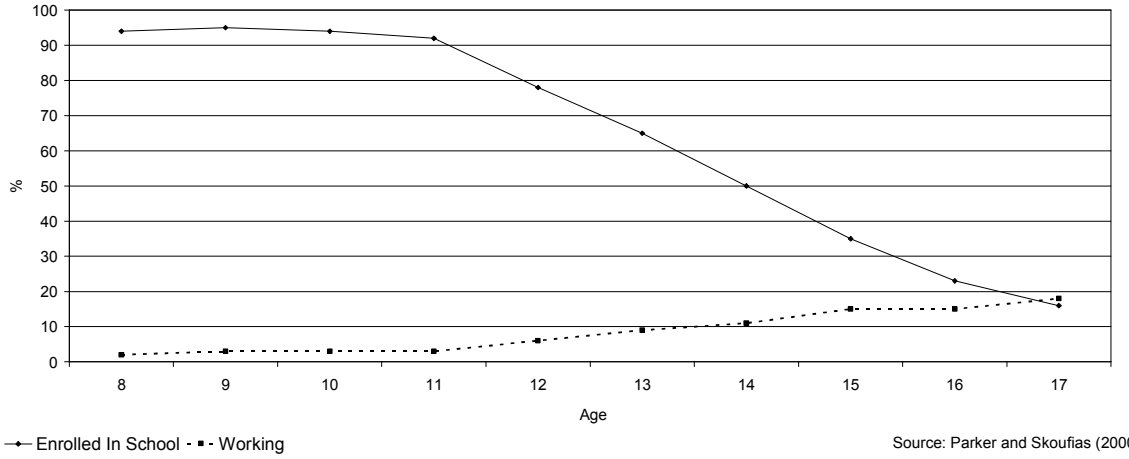



**Figure 1b School Enrollment and Labor Force Participation of Girls in *Oportunidades* Communities Prior to Program Implementation**




Source: Parker and Skoufias (2000)

Figure 2a. Distribution of Propensity Score: Treatment98 T1998

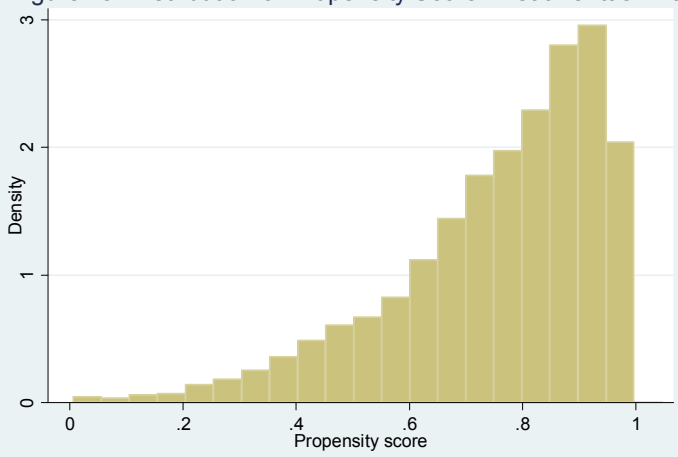
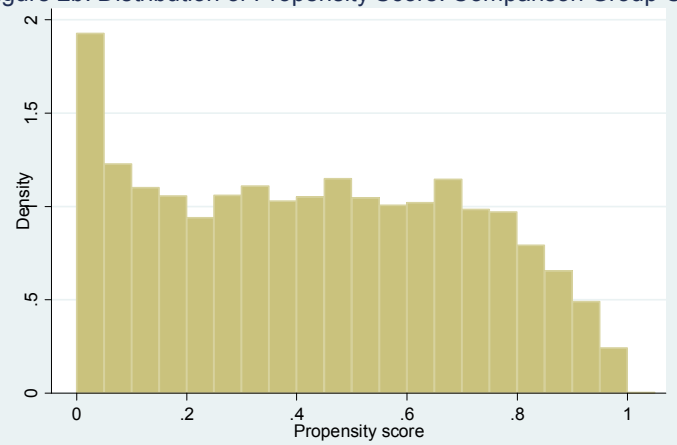



Figure 2b. Distribution of Propensity Score: Comparison Group C2003



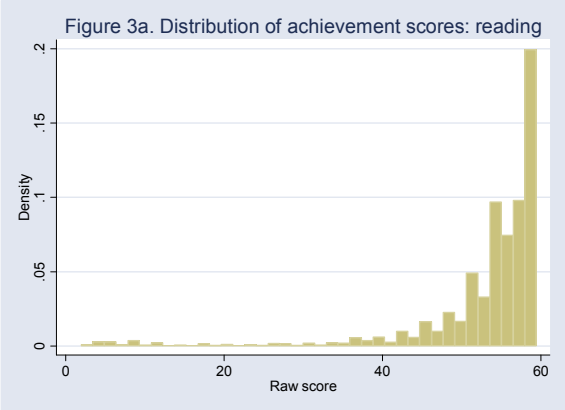


Figure 3a. Distribution of achievement scores: reading
Density
0
.05
.1
.15
.2
0
20
40
60
Raw score

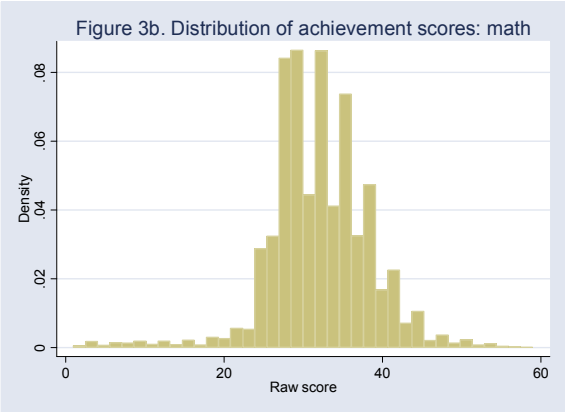


Figure 3b. Distribution of achievement scores: math
Density
0
.02
.04
.06
.08
0
20
40
60
Raw score

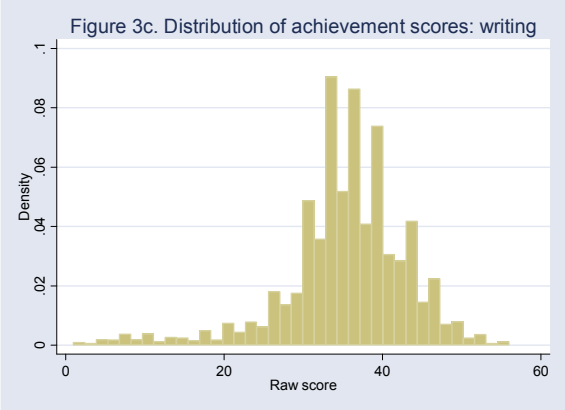


Figure 3c. Distribution of achievement scores: writing
Density
0
.02
.04
.06
.08
.1
0
20
40
60
Raw score

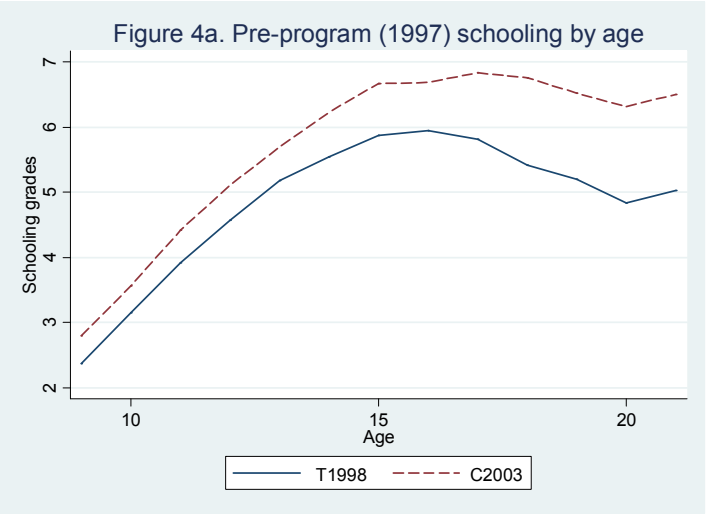


Figure 4a. Pre-program (1997) schooling by age
Schooling grades
2
3
4
5
6
7
10
15
20
Age
T1998
C2003

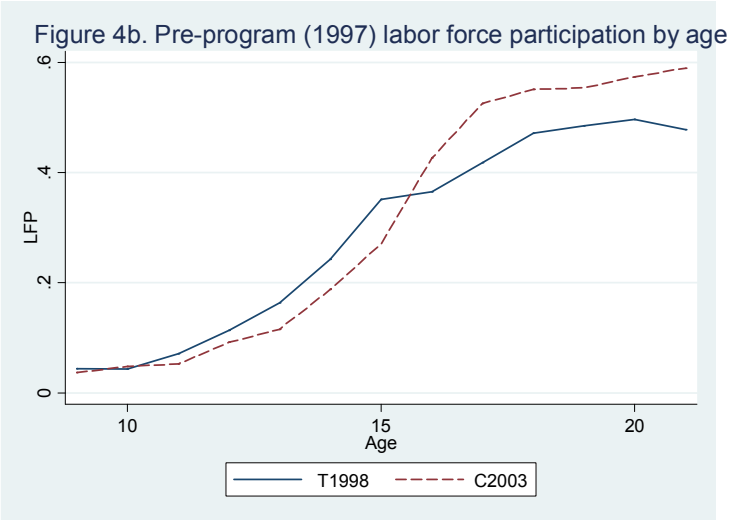


Figure 4b. Pre-program (1997) labor force participation by age
LFP
0
.2
.4
.6
10
15
20
Age
T1998
C2003

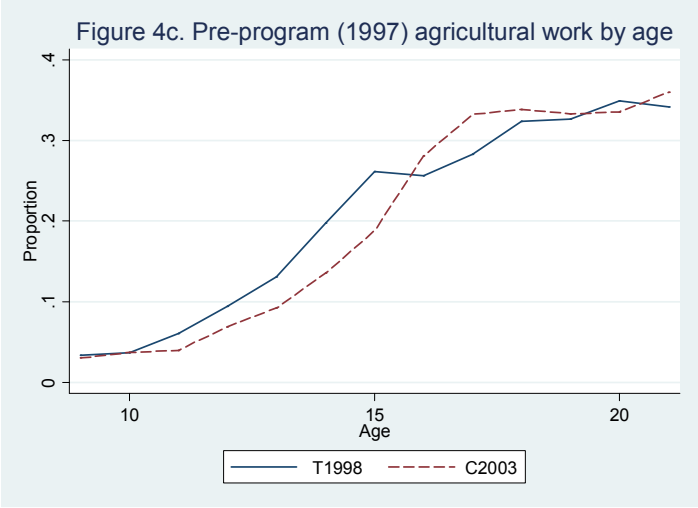


Figure 4c. Pre-program (1997) agricultural work by age
Proportion
0
.1
.2
.3
.4
10
15
20
Age
T1998
C2003

**Appendix Table 1**
**Estimated Impacts of *Oportunidades* on Schooling under Different Matching Estimators**
**Local linear matching and kernel matching† T1998 vs. C2003**
**Longitudinal difference-in-difference matching**

| Age in 2003 | Local linear matching, bandwidth=0.8 Main text estimates | Local linear matching, bandwidth=0.4 | Epanechnikov Kernel matching bandwidth=0.06 |
|---|---|---|---|
| **Girls** | | | |
| 15-16 | 0.69 | 0.66 | 0.66 |
| | (0.16) | (0.16) | (0.16) |
| 17-18 | 0.75 | 0.73 | 0.0072 |
| | (0.18) | (0.18) | (0.19) |
| 19-21 | 0.03 | 0.04 | 0.02 |
| | (0.18) | (0.20) | (0.19) |
| **Boys** | | | |
| 15-16 | 1.00 | 0.95 | 0.97 |
| | (0.15) | (0.14) | (0.13) |
| 17-18 | 0.93 | 0.98 | 0.97 |
| | (0.21) | (0.17) | (0.15) |
| 19-21 | 0.55 | 0.62 | 0.59 |
| | (0.18) | (0.16) | (0.16) |

† Difference-in-difference estimator, imposing common support (trimming=2%). Bootstrapped standard errors (parentheses) with 500 replications.

## Appendix A. Construction of Variables

Sample construction:

The analysis uses youth aged 9 to 15 in 1997 or those 15 to 21 in 2003. In practice, there are inconsistencies in the ages reported, e.g. not all youth reported to be age 9 to 15 in 1997 are within the range of 15 to 21 in 2003 (or even slightly outside the range). To correct some errors and insure that we are correctly matching individuals over the six-year period, we deleted from the sample any individual who was more than two years off in 2003 with respect to what would be his or her "correct" age according to that reported in 1997. Additionally we eliminated individuals who reported changing gender between the periods and those reporting impossible changes in the schooling grades completed over time, e.g. negative changes or increases in more than 8 grades of schooling over the six-year period.

Definition of outcome indicators:

*Grades of completed schooling* is constructed for both 1997 and 2003 using information on the level and grade. Years in preschool and kindergarten were not counted. Primary school education was allowed to have a maximum of six grades, secondary school was allowed a maximum of three additional grades, and high school a further additional three grades. College education could achieve an additional five grades and graduate work an additional five grades.

An individual is considered *employed* in either year if he/she reports having worked the week before or having a job the week before even if they did not actually work because of illness or vacation.

*Agricultural workers* are defined in both 1997 and 2003 according to reported occupational position. Unfortunately, in the 2003 survey, it is impossible to distinguish those who work on their family's land from those working in some other family-owned business. Thus, all individuals reporting to be "jornaleros" or "peons" as well as any who report working in a family business are classified as agricultural workers. Our measure of agricultural work may thus include some individuals not actually performing agricultural work although our prior is that these are likely to be relatively few.

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