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**Long-Term Impacts of the *Oportunidades* Conditional Cash Transfer Program on Rural Youth in Mexico**

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# Long-Term Impacts of the *Oportunidades* Conditional Cash Transfer Program on Rural Youth in Mexico*

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## Abstract

This paper studies the long-term effects of participation in the Mexican *Oportunidades* program on a variety of outcomes and behaviors of rural youth in Mexico. It analyzes data from a social experiment, which randomly phased-in the program in rural Mexican villages. In 1997, 320 villages (the treatment group) were randomly selected for early incorporation into the program and 186 villages (the control group) were designated as a control group to be incorporated eighteen months later. This paper examines whether differential exposure to the program significantly impacted educational attainment, labor market outcomes, marriage, migration and cognitive achievement of youth. The results show positive impacts of longer exposure on grades of schooling attained, but no effects on achievement tests. With respect to work, we find an overall reduction in work for male youth.

## 1 Introduction

Governments throughout Latin America and South America have adopted conditional cash transfer programs aimed at alleviating short-term poverty and reducing the intergenerational transmission of poverty by providing incentives for private investment in schooling and health.[1] The *Oportunidades* program, formally called PROGRESA, has operated in rural areas of Mexico since 1997, giving cash grants to poor families in exchange for their children's regular attendance at school and for visits to health clinics. Currently, five million families participate in the program, which represents about one-fourth of all families in Mexico.

For evaluation purposes, the *Oportunidades* program was initially implemented as a randomized social experiment, with 320 rural villages assigned to the treatment group and 186 assigned to the control group. Eligible households in treatment villages began receiving benefits in the spring of 1998. The program was withheld from households in the control villages for 18 months, after which they were also incorporated.[2] A rigorous external evaluation, with several rounds of panel data and an experimental design, as well as other approaches to analysis such as regression discontinuity design and structural modeling, was implemented at the beginning of the program (covering the 1998-2000 period). The early evaluation results demonstrated significant impacts in reducing child labor, improving health outcomes, and increasing school enrollment, among other short-term effects.[3] Some of the initial evaluation studies also generated estimates of longer-run effects, under assumptions such as stability in schooling transition matrices or in the structural relations underlying family behaviors.[4]

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[1] Such programs exist in Brazil, Chile, Colombia, Guatemala, and Nicaragua.

[2] According to program administrators, control households were not informed ahead of time about the plans for their incorporation.

[3] The overall evaluation of the initial years of PROGRESA is summarized in Behrman and Skoufias (2004), Skoufias (2001), Skoufias and McClafferty (2001), Parker (2003)

[4] See, e.g., Schultz (2002), Behrman, Sengupta and Todd (2005), and Todd and Wolpin (2004).

With the availability of the 2003 follow-up rural evaluation survey (ENCEL2003), it is now possible to assess directly some important longer-run effects of the program. Moreover, in 2003 achievement tests were applied, making it possible for the first time to evaluate whether the program significantly influenced the cognitive achievement of participating children/youth. This paper examines the impacts of *Oportunidades* on a variety of behaviors and outcomes of rural youth in 2003, more than five years after households in the original treatment group began receiving benefits. Specifically, we examine whether differential exposure to the program as experienced by the treatment and control households significantly impacted educational attainment, labor market outcomes, marriage, fertility, migration and cognitive achievement. We also explore how schooling impacts vary with the type of school available, as captured by select school quality characteristics.

Our analysis sample consists of youth who were aged 9 to 15 in 1997 just prior to the program intervention (aged 15 to 21 in 2003). We focus on this group as they encompass those who, prior to the intervention, were at or close to the transition between primary and secondary school–a critical juncture in schooling attainment in poor communities in rural Mexico. Figure 1.1 illustrates how schooling attendance and labor market participation vary with age. Part of the reason for the sharp drop-off in school attendance during the transition to secondary school is that many villages do not have a secondary school in close proximity, so attending often requires incurring additional traveling costs. Because of the importance of the primary-secondary transition, early teens with four to six grades of completed schooling in treatment households in 1997 faced considerably different incentives for continuing in school than if they were in the control households. By the time the control villages were incorporated in late 1999, these individuals were likely to be beyond the critical decision period regarding secondary school enrollment.[5]

Our analysis is based on information provided in the 2003 Rural Evaluation Survey (ENCEL2003), which provides a follow-up round of information on the original experimental treatment and control samples. We link the follow-up data to the baseline data, in particular the 1997 pre-program Survey of Household Socio-economic Characteristics (ENCASEH) data. We also link the household level data to school level data on characteristics that reflect school quality.

As noted, the treatment and control villages were originally chosen by a randomized experimental design. Over time, however, attrition, mainly due to migration, led to some observable differences between the groups. The empirical strategy adopted in this paper is to assess program impacts using a difference-in-difference approach combined with a density reweighting method (described in section 3 below) to take into account attrition occurring between the baseline and follow-up surveys. The problem of attrition is mitigated somewhat by the fact that the follow-up survey asks parents questions about any children who migrated away from the household. Thus, data are available for many outcomes of interest even if children migrated. Entire households that left the experimental villages were not followed, but migrants within villages were followed.

Our impact estimates reveal significant positive impacts of long-term (5.5 years) exposure to the program on school grades completed. On average, youth in the treatment group have about 0.2 more years of schooling than youth in the control group, both for boys and girls. Larger effects on the order of 0.5 years are observed for the subset of youth who were near the transition between primary and secondary school at the time the program was introduced. Our estimates also suggest that boys with longer exposure progressed significantly faster through school. When we compare children who attended schools of differing quality, we generally find larger schooling impacts for children attending better quality schools.

A final area of education impacts are those related to Woodcock Johnson achievement tests, which were carried out in reading, mathematics and written language skills for adolescents 15 to 21 in 2003. Our impact results do not reveal any significant differences between the treatment and control groups. We explore some possible explanations for the lack of impacts on test scores.

The theoretical long-term effect of *Oportunidades* on working behavior is ambiguous. On the one hand, the program might reduce work if it leads children to spend more time in school. On the other hand, if participating in the program facilitates grade progression, then youth may complete their targeted schooling levels earlier and begin working at earlier ages. Our results show overall negative effects of *Oportunidades* on employment for boys and insignificant effects for girls. Boys in the treatment group are also less likely to participate in agricultural work than boys in the control group.

Finally, we find that the program has a statisti-

[5] Previous evaluations demonstrated that the largest effects of the program were precisely at this transition between primary and secondary school (see Behrman, Sengupta and, Todd P. (2005), Schultz (2004), and Todd and Wolpin (2004)).

cally significant impact on marriage and migration rates. Male youth aged 9 to 15 in 1997 (15 to 21 in 2003) are about 6 percent less likely to migrate out of their household relative to the control group, while the effects are also negative for girls but not statistically significant. With respect to marriage, both girls and boys in the original treatment group have a lower probability of being married by 2003.

The paper is organized as follows. Section 2 provides a brief description of the features of the *Oportunidades* program. Section 3 describes the basic sample design, the data, and the econometric method used to control for nonrandom attrition/migration. Section 4 presents the empirical results and Section 6 concludes.

# 2 Program Background

*Oportunidades* (previously called PROGRESA) began operating in 1997 in small rural communities in Mexico. The program has gradually expanded into urban areas and today covers about one quarter of all families in Mexico. Table 2.1 shows the monthly grant levels available for children between the third grade and the twelfth grade in the second semester of 2003. Originally, the program provided grants only for children between the third and ninth grade. In 2001, however, the grants were extended to high school. The grant amounts are slightly higher (by about 13%) for girls than boys in secondary and high school. This gender disparity is meant to provide an additional incentive for sending girls to school, because girls traditionally have lower enrollment rates at the secondary and high school levels. The program also provides grants for school supplies and a fixed transfer linked to regular health clinic attendance.[6]

Regular school attendance is required to continue receiving the monthly grant payments as is attendance at a health talk once a month for high school students. Program rules allow students to fail each grade once. If students repeat a particular grade more than once, then education benefits are discontinued permanently.[7]

Within villages, only families that satisfy eligibility criteria receive the *Oportunidades* program, where eligibility is determined on the basis of a marginality index designed to identify the poorest families within each community.[8] Program administrators visited all households in each village and, after collecting some screener information on them, informed them of their eligibility status. Because of the method of incorporation and because program benefits are generous relative to most families' incomes, almost all families deemed eligible decide to participate in the program. However, not all families are induced by the transfers to send all their children to school; they are allowed to receive partial benefits if they send only a subset of their eligible children to school. According to program rules, households are subject to program recertification every three years, a process by which households receive a visit and their household characteristics are again evaluated to see if they continue to be eligible. 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 educational grants, but excludes primary school scholarships and cash transfers for food. In practice, however, very few households in our sample of interest transitioned to the modified version of the program. For the analysis of this paper, we concentrate on those households initially eligible for the full program who did not transition to any other form of the program.[9]

# 3 Sample design, the data, and attrition

## 3.1 Sample design

The 2003 Rural Evaluation Survey continues the original treatment and control experimental design begun in 1997. The original sample design involved selecting 506 communities with 320 randomly assigned to receive benefits immediately and the other 186 to receive benefits later.[10] The eligible households in the original treatment localities (T1998) began receiving

[6] All monetary grants are given to the mother of the family, with the exception that scholarships for upper-secondary school, with the approval of the parents, can be given directly to the youth.

[7] Note this allows a student theoretically to receive two years of grants for the same grade for each grade in which the student enrolls.

[8] Program eligibility is based in part on discriminant analysis applied to the October 1997 household survey data. The discriminant analysis uses information on household composition, crowding indices, household assets (such as whether the house has a dirt floor or whether the family owns a car), and other factors.

[9] A small number of originally eligible households never received program benefits, mostly because they migrated away from their community before being informed they were eligible for the program. These households are not included in our analysis.

[10] Due to budget restrictions, the program was phased-in over time. The evaluation sample included localities phased-in in 1998 for the original treatment group (T1998) and localities phased-in in 2000 for the original control group (T2000).

program benefits in the spring of 1998 whereas the eligible households in the original control group (T2000) began receiving benefits at the end of 1999. Between 1997 and 2000, evaluation surveys with detailed information on many evaluation indicators including education, health, income and expenditures were applied to households in both groups every six months.

In the year 2003, a new follow-up round of the rural evaluation survey (ENCEL2003) was carried out. The sample includes eligible and ineligible households in the original treatment (T1998) and original control (or delayed treatment, T2000) groups. We link the T1998 and T2000 data from 2003 to earlier data sets, particularly the pre-program 1997 ENCASEH data, to have longitudinal data on individual children who were 9 to 15 years of age in 1997 and 15 to 21 in 2003. As in the previous ENCEL surveys, the ENCEL2003 contains data on a myriad of program outcomes, including schooling, labor and expenditures. Additionally, the ENCEL2003 contains new modules, including Woodcock-Johnson achievement tests applied to adolescents and a school level questionnaire applied to directors and teachers at schools where *Oportunidades* beneficiaries attended.

To undertake the analysis below, a number of decisions had to be made regarding the accuracy of some of the raw data and how to construct the variables of interest. Appendix A provides details on these matters.

### 3.2 Attrition of youth in the original T1998 and T2000 households.

We now turn to consideration of program attrition of the original evaluation ENCEL sample. Some researchers have questioned whether the gains from collecting longitudinal data are worth the costs (e.g., Ashenfelter, Deaton, and Solon (1986)), because of concerns about selective attrition. Many analysts share the intuition that attrition is likely to be selective on characteristics such as schooling and thus that high attrition is likely to bias estimates made from longitudinal data.

Most of the previous work on attrition in large longitudinal samples is for developed economies, for example, the studies published in a special issue of *The Journal of Human Resources (JHR)* (Spring 1998) on "Attrition in Longitudinal Surveys." The surprising conclusion of many of the studies is that that biases in estimated socioeconomic relations due to attrition are small despite attrition rates sometimes as high as 50% and despite significant differences between those re-interviewed and those lost to follow-up for many important characteristics. For example, Fitzgerald, Gottschalk, and Moffitt (1998) summarize:

> By 1989 the Michigan Panel Study on Income Dynamics (PSID) had experienced approximately 50% sample loss from cumulative attrition from its initial 1968 membership… (p. 251)
>
> We find that while the PSID has been highly selective on many important variables of interest, including those ordinarily regarded as outcome variables, attrition bias nevertheless remains quite small in magnitude. … (most attrition is random)... (p. 252)
>
> Although a sample loss as high as [experienced] must necessarily reduce precision of estimation, there is no necessary relationship between the size of the sample loss from attrition and the existence or magnitude of attrition bias. Even a large amount of attrition causes no bias if it is 'random' … (p. 256)

The other studies in this special issue of the JHR further confirm these findings for the PSID or reach similar conclusions for other important panel data such as the Survey of Income and Program Participation (SIPP), the National Longitudinal Surveys of Labor Market Experience (NLS), and the Labor Supply Panel Survey in the Netherlands (see Falaris and Peters (1998), Lillard and Panis (1998), Van den Berg and Lindeboom (1998), Zabel (1998), Ziliak and Kniesner (1998). Similar results are presented for three developing country longitudinal data sets in Alderman, Behrman, Kohler, Maluccio, and Watkins (2001).

While such results suggest that attrition is not always a major source of bias, it is nonetheless important to examine whether attrition is selective in any particular study. In the present case, sample attrition can cause problems for our analysis if it changes the composition of the treatment sample differently than the composition of the control sample. In our study, the attritors consist of individuals who were in the sample in 1997 but not in the 2003 follow-up sample.[11] As noted in the introduction, parents were asked questions about children who left the family, so for many of the outcomes (such as years of education), data are available despite the child having left the household.

[11] For other purposes it may be of interest to consider the details of sample attrition across the rounds of the panel data collected because it may be relevant when an individual attrited from the sample.

Table 3.1 (panel A) summarizes some statistics regarding sample attrition in this period for the original treatment (T1998) and original control (T2000) groups, focusing first on all youth in the community and then on those eligible for the program under the original program definition (pobre) and the modified program definition (pobreden).[12] The numbers in this table are striking. Two-fifths (41%) of the individuals aged 9 to 15 in 1997 were not in the sample six years later, which certainly is a large enough proportion to raise concerns. For most of our variables of interest, though, including years of schooling and occupation, actual attrition is less than 20 percent, because information on outcomes is provided by the parents or other informants. In fact, there are not large or statistically significant differences in overall attrition between the T1998 and T2000 samples (see t-tests in last column of the table). The proportion lost to follow-up is a little higher for girls (42%) than for boys (36%), though for neither is there a statistically significant difference between T1998 and T2000 for total attrition. On an aggregate level, sample attrition does not appear to be significantly associated with receipt of treatment.

Consideration of more disaggregated patterns, however, reveals some systematic attrition patterns related to treatment status. Overall attrition of individuals aged 9 to 15 in 1997 includes: (i) individuals who have separated from households that are still in the sample in 2003 (Table 3.1, Panel B) and (ii) individuals from households that are no longer in the sample in 2003 (Table 3.1, Panel C). About 62% of those lost to follow-up are individuals who left households that stayed in the sample.[13] There are some significant differences at the 5% level if individual and household attrition are considered separately; there is higher individual attrition among the T2000 group (for boys) and higher household attrition among the T1998 group (for girls). So, while the aggregate T1998 vs. T2000 attrition differences are not significant even at the 20% level, disaggregated patterns indicate some differences.

To better understand the determinants of attrition, we estimated the probability of being lost to follow-up for individuals 9 to 15 years old in 1997 from the T1998 and T2000 groups – again, for total attritors, individual attritors and household attritors. For each of these three dependent variables, we estimated two specifications: (1) whether in T1998 group and (2) whether in T1998 group plus interactions between being in the T1998 group and pre-program individual characteristics, parental characteristics and housing characteristics. We performed this estimation for boys and girls together and separately. Appendix B tabulates the estimates. The first specification (column 1), not surprisingly, replicates the patterns noted with regard to Table 3.1. Specification (2) indicates that a number of the pre-program individual, parental and housing characteristics interact significantly with treatment (i.e., being in the T1998 group) in predicting attrition.

Thus, the timing of treatment appears to be significantly negatively associated with individual migration and significantly positively associated with household migration – and there are a number of significant interactions with individual, parental and housing characteristics (differing in many cases for boys versus girls). Therefore biases could result if we do not correct for attrition in our estimation of program impact. We next describe how we take into account attrition in generating program impact estimates.

### 3.3 Method used to account for attrition in estimation of program impacts

To describe the method, we first have to introduce some notation. Following the standard notation in the evaluation literature, let $Y_1$ denote the potential outcome of an individual if in the treatment (T1998) group and $Y_0$ the potential outcome if in the control group, which received treatment later (T2000). (In our application, treatment corresponds to receiving the longer exposure to the program.) Let $R = 1$ denote that the individual is a member of the experimental treatment group and $R = 0$ that he/she is a member of the control group. We restrict attention to eligible households and, for simplicity, do not introduce additional notation to denote conditioning on eligibility for the program.

Let $A = 1$ if an individual is present in the before sample (1997) but is not present in the post-program follow-up sample (2003). $X$ denotes characteristics of the individual (such as gender, age, parental education) whose distribution is assumed to be unaffected by whether treatment is received (such as age, gender, or education level of parents).

In the absence of the attrition, we can simply ex-

[12] We use the former (original) definition for all of our analysis below, but include in this table some information regarding the latter definition to illustrate that the two definitions lead to similar conclusions regarding whether sample attrition was related to program exposure.

[13] That aggregate attrition rates for girls exceed those for boys as noted above is entirely because more girls were individual attritors than boys (28% versus 21% among the T1998 group, 30% versus 24% among the T2000 group).

ploit the randomized treatment assignment and estimate the *average impact of treatment on the treated (TT)* by the difference in means:

$$\Delta_{TT} = E(Y_1|R=1) - E(Y_0|R=0).$$

This is an unbiased estimator of the treatment impact, because $E(Y_0|R=0) = E(Y_0|R=1)$ by virtue of the randomization.

Now suppose that some fraction of individuals attrit from the experimental samples. Consider what is estimated by the difference in means taken over individuals who did not attrit:

$$\begin{aligned}\Delta_1 &= E(Y_1|R=1, A=0) - E(Y_0|R=0, A=0) \\ &= E(Y_1|R=1, A=0) - E(Y_0|R=1, A=0) + \\ \{E(Y_0|R &= 1, A=0) - E(Y_0|R=0, A=0)\}\end{aligned}$$

$\Delta_1$ is potentially a biased estimator of the average impact of the program for nonattritors. Because of attrition, there is no longer any guarantee that the last term equals zero.

One possible approach to addressing the attrition problem is to assume that attrition is random within $R$ strata conditional on some set of observables $X$ :

$$(Y_1, Y_0) \perp\!\!\!\perp A \,|X, R \qquad \text{(M-1)}$$

and that

$$0 < \Pr(A=1|X, R) < 1. \qquad \text{(M-2)}$$

Condition (M-2) ensures that we do not lose all individuals with characteristics $X$ to attrition.

In addition, we note that the experimental assignment of $R$ implies

$$Y_0 \perp\!\!\!\perp R \,|X \qquad \text{(R-1)}$$

and

$$0 < \Pr(R=1|X) < 1. \qquad \text{(R-2)}$$

Under these assumptions,

$$\Delta_X = E(Y_1|R=1, A=0, X) - E(Y_0|R=0, A=0, X)$$

provides an unbiased estimate of the program effect for the subgroup of individuals with characteristics $X$ who did not attrit. To see why, note that (M-1) gives

$$\begin{aligned}E(Y_0|R &= 0, A=0, X) = E(Y_0|R=0, X) \\ E(Y_1|R &= 1, A=0, X) = E(Y_1|R=1, X)\end{aligned}$$

and (R-1) gives

$$E(Y_0|R=0, X) = E(Y_0|R=1, X).$$

Thus, $\Delta_X = E(Y_1|R=1, X) - E(Y_0|R=1, X)$, which is the average impact of treatment on the treated for individuals with characteristics $X$.

The overall average effect of treatment on the treated is given by

$$\begin{aligned}\Delta &= \\ \int \{E(Y_1|R &= 1, X) - E(Y_0|R=0, X)\} \cdot f(X|R=1)dX.\end{aligned}$$

To motivate the estimator we use, write the above expression as

$$\begin{aligned}&\int E(Y_1|R=1, A=0, X)\tfrac{f(X|R=1)}{f(X|R=1, A=0)} f(X|R= \\ &\qquad 1, A=0)dX \\ &-\int E(Y_0|R=0, A=0, X)\tfrac{f(X|R=1)}{f(X|R=0, A=0)} f(X|R= \\ &\qquad 0, A=0)dX,\end{aligned}$$

where $f(X|R=1) = f(X|R=0)$ because of the initial random assignment.

An estimator for the average impact of treatment on the treated that takes into account attrition is

$$\hat{\Delta}_{TT} = \frac{1}{n_1}\Sigma_{i=1}^{n_1} Y_{1i}\hat{W}_i - \frac{1}{n_0}\Sigma_{j=1}^{n_0} Y_{0j}\hat{W}_j,$$

where $\hat{W}_i = \frac{\hat{f}(X_i|R=1)}{\hat{f}(X_i|R=1, A=0)}$ is a weight applied to each member of the treatment group and $\hat{W}_j = \frac{\hat{f}(X_j|R=0)}{\hat{f}(X_j|R=0, A=0)}$ is a weight applied to each member of the control group. The weights adjusts for differences in the distribution of the $X$ characteristics arising over time because of attrition.

When $X$ is of high dimension, it can be difficult to implement this weighting procedure, as calculating the weights requires potentially high dimensional nonparametric density estimates. For this reason, we make use of the dimension reduction theorem of Rosenbaum and Rubin (1983). Their theorem shows that conditions (M-1) and (M-2) imply

$$Y_0 \perp\!\!\!\perp A \,|\ \Pr(A=1|X, R) \qquad \text{(M-1)}$$

where $\Pr(A=1|X, R)$ is the probability of attriting (the so-called propensity score), which can be estimated by a parametric model such as a logit or probit model. Thus, we can implement the reweighting estimator using as the weights the ratio of the univariate densities of the propensity score:

$$\hat{\Delta}_{TT_2} = \frac{1}{n_1}\Sigma_{i=1}^{n_1} Y_{1i}\hat{W}_i - \frac{1}{n_0}\Sigma_{j=1}^{n_0} Y_{0j}\hat{W}_j,$$

where $n_1$ and $n_0$ are the number of individuals in the treatment and control groups. The weights are $\hat{W}_i = \frac{\hat{f}(P_i|R=1)}{\hat{f}(P_i|R=0, A=0)}$ and $\hat{W}_j = \frac{\hat{f}(P_j|R=0)}{\hat{f}(P_j|R=0, A=0)}$ where $P_i = \Pr(A_i = 1|X_i, R_i)$, which we estimate by a probit model. Through this procedure, each individual observed post-program receives a weight equal to the ratio of the density of his/her $P_j$ with respect to the post-program distribution (of treatments or controls) divided by the density estimated with respect to the preprogram (and pre-attrition) distribution. Effectively, this procedure reweights the post-program observations to have the same distribution of $X$ as they did prior to the attrition. The key assumption that justifies application of this procedure is that attrition is random conditional on $X$, within each of the groups.[14]

The estimator can be implemented by a weighted regression of outcomes on a constant term and on a treatment group indicator. The estimated coefficient associated with the treatment indicator is $\hat{\Delta}_{TT}$.In estimating program impacts, we use the reweighted regression method as described above, except that we apply the analysis to differences in outcomes rather than cross-sectional outcomes to take into account any preprogram differences between the groups.

## 3.4 Woodcock Johnson achievement tests.

As part of the ENCEL2003 fieldwork, achievement tests in the areas of reading, math and written language skills from the Woodcock-Johnson Tests (WJ) were applied to a sub-sample of adolescents 15 to 21 years of age in 2003. The Woodcock Johnson is one of the principal tests used to measure achievement in the United States and is very commonly applied. The tests have been validated between the ages of 2 and 90. A Spanish version is also available and has been adapted to Latin American contexts.[15]

Three tests were applied. Test 22 of the Woodcock Johnson tests is Letter-word Identification (reading), consisting of showing those taking the test various pictures, letters and progressively harder words where the examinee is asked to say what is in the picture, and then to state letters, and then words. In the case of words, the examinee must pronounce the word correctly for it to be classified as a correct answer. Test 25, Applied Problems tests the subject's skills in solving practical problems. The test begins with such aspects as counting the number of balls on a page and progresses to mathematical problems such as calculating fractions. Test 26 Dictation is a basic writings skills tests, where the examiner reads aloud letters and words and the examinee must write down the letter/word correctly.

Figures 3a through 3c show density histograms of each of the three tests, where the sum of the area of the bars equals one. Noteworthy is the graph of reading scores, which shows that most of the test scores are bunched at the right hand tail of the distribution, implying that a majority of those taking the tests scored at or near the maximum raw score permitted. This is problematic for the analysis as there is less variation in the scores than might be desired and therefore it is potentially less likely that impacts of the program could be observed.[16] The other two achievement tests in mathematics and writing show much greater dispersion in their scores, suggesting more possibilities for changes in scores as a result of the program.

Why might we expect to observe an impact of the program on achievement tests? Firstly, if children attain a higher level of schooling as a result of *Oportunidades*, then this higher level of schooling should lead to higher achievement scores. This assumes, however, that the WJ tests are in fact influenced by grades of completed schooling in the environment under study, e.g. rural areas in Mexico. To first verify that schooling levels are associated with higher scores on achievement tests, we carried out a simple regression analysis of the test scores on schooling and on other individual, parental and household level control variables. We model schooling in terms of total grades of schooling (e.g. assuming a linear relationship) as well as a more flexible specification that includes indicator variables for each grade of schooling. The dependent variable is the raw score reported on each test. The estimates are shown in Appendix Table C1.

For all three tests, grades of schooling has a highly significant relationship with achievement test scores.

[14] This assumption would allow, for example, attrition decisions to be based on the average treatment effect experienced by one's group (which depends on $X$). It does not allow attrition decisions to be based on one's own idiosyncratic gain from treatment.

[15] In developing the Spanish version, the Woodcock Johnson team gathered calibrating and equating data from over 2,000 monolingual or nearly monolingual Spanish-speaking subjects from six countries (Mexico, Puerto Rico, Costa Rica, Spain, Argentina, and Peru) as well as in five US states (Arizona, California, Florida, New York, and Texas).

[16] The fact that almost all scores in reading are near the maximum might initially suggest that the test was too easy. However, when we compare the average scores on this reading test with the average achievement according to the program scoring of individuals in the United States, the comparison implies, improbably, that the average ENCEL examinee has an equivalent reading skill as the average college graduate in the United States. A possible explanation relates to the design of the test where individuals must pronounce the words correctly for a question to be scored correctly. Unlike English, in Spanish, pronunciation rules are very clear, thus it is very possible that one could pronounce a word correctly even if one had never seen the word before. In this sense, the test would seem likely to generate much higher scores in Spanish than in English.

In particular, an additional year of schooling increases the raw scores, defined as the number of questions answered correctly (from a maximum of 58 questions), of the WJ reading test by 1.3, the math test by 1.05 and the written language test by 1.4. In the case of reading, the relationship between schooling and the test scores looks fairly linear, in the case of math and writing, however, most of the positive effect of schooling derives from secondary and high school years of education, with the primary years having few significant effects relative to the achievement test scores of those with no formal schooling.

We now turn to a description of the sample that took the tests and the achievement tests results. Table 3.2 shows that the tests were applied to a total of 7,666 individuals between the ages of 15 and 21 in 2003. We are particularly interested in the sample of those youth originally eligible for the program in the 1997 survey. Table 3.2 shows the number of youth with test scores in our age groups who can be matched back to their 1997 characteristics. About 1,426 students in the original T1998 sample can be matched back versus 1,216 in the T2000 sample. While the total sample size is reasonably large, disaggregating the analysis by age and gender does lead to some small sample size cells. Table 3.2 further shows that youth in the T2000 group were more likely to be applied the achievement tests than the T1998 youth.

One limitation for the current analysis is that these tests were only carried out only in 2003, so no baseline information on test scores is available. To take into account different probabilities of being in the sample (e.g. taking the tests) between the T1998 and T2000 groups, we use the cross-sectional reweighting estimator described above with the weights reflecting the probability of being in the test-taking sample. (See Appendix Table C.2 for the model used to predict the probability of being in the sample, on which the construction of the weights is based).

# 4 Program Impact Estimates

In this section, we present impact estimates based on the weighted difference-in-difference estimator described in section 3. We present impacts by age, gender, and baseline schooling level, because, as noted in section one, impacts likely vary depending on where children were in their schooling career when the program began. In particular, we hypothesize that there may be substantial effects of treatment for those children who in 1997 were at the critical age for making marginal schooling decisions, that is, in the 11-13 age range at which decisions are made regarding enrolling in lower secondary school. In this section, we estimate the effects of differential program exposure on education, work, marriage and migration. We also explore whether the schooling impacts vary by school characteristics (type of school available and teacher-pupil ratio).

We carry out a difference-in-difference regression analysis, where the program impact is captured through an indicator variable measuring whether the individual resided in a T1998 versus T2000 locality interacted with an indicator for post-program year (2003). We carried out both simple regressions only controlling for the impact variables, as well as specifications with additional control variables, which may reduce the standard errors of the estimated program effects. The control variables include parental age, education, indigenous status, and household characteristics.[17]

For all of the tables in this section, the first column gives the value for the relevant variable for the T2000 group (which is also of interest as an estimate of what would have happened without the additional exposure to the program that the T1998 group had), the second and third columns gives the estimated differential treatment impact, e.g. the increase or decrease observed in the indicator studied, and the standard error for the T1998 group in comparison to the T2000 group. The fourth column gives the percentage changes for the T1998 group as compared with the T2000 group.

## 4.1 Education

*Impacts on School enrollment in 2003*: Prior to the program in 1997, the enrollment rates for T1998 and T2000 groups aged 9-15 years were not significantly different at the 5% level. As shown in Table 4.1, school enrollment rates in 1997 were 0.82 for T1998 boys and 0.81 for T2000 boys, 0.77 for T1998 girls and 0.76 for T2000 girls. Evaluations of short-run program impacts found that the program increased school enrollment for children age 9-15. The program also facilitated grade progression, increased school reentry rates and reduced drop-out and repetition rates. By 2003, the youth in our sample are 15-21 years old. Even if the program increased schooling grades completed as was its intent, it also may have reduced the probability that children age 15-21 were still in school in 2003 if they tended to finish their schooling "earlier". Furthermore, the new high school grants went into effect in 2001, but depending on their year of schooling prior to 1998, this may have been af-

[17] The notes to the table give the full set of control variables.

ter many of those in T1998 had finished secondary schooling and/or made their enrollment decisions for secondary school.

ln 2003 the enrollment rates for the T2000 group were 0.24 for boys and 0.26 for girls.[18] The difference-in-difference estimates in the second column of Table 4.2 indicate on average no significant differential program exposure on enrollment in 2003 for either boys or girls. However, we do find significant impacts when we disaggregate by age and baseline schooling levels. Enrollment rates are significantly higher for the T2000 children in the younger end of the age range and who had less schooling in 1997. The enrollment rates in 2003 were 0.48 for both boys and girls who were 9-10 in 1997 (15-16 in 2003), 0.24 for girls and 0.21 for boys 11-12 in 1997 (17-18 in 2003), and 0.10 for girls and 0.08 for boys 13-15 in 1997 (19-21 in 2003). The enrollment rates in 2003 decline monotonically with higher grades completed in 1997 – for girls from 0.37 for up to three grades to 0.10 for six grades (with a slight increase to 0.15 for seven plus grades) and for boys from 0.37 for up to three grades to 0.11 for six plus grades.

Given these patterns, one might expect a higher probability of differential program exposure impact among children who were relatively young and/or had relatively limited schooling in 1997 – because a higher proportion of these children would seem to be at the margin of enrolling in school. The difference-in-difference estimates by the age groups indicate, however, only one significant effect – a negative one for girls who were 9 to 10 in 1997 that implies a 8.3% decrease in 2003 enrollment rates (also see Figure 4.1). The difference-in-difference estimates by the schooling grades completed by 1997 indicate only one significant effect – a decrease for T1998 versus T2000 for boys who had six grades of schooling grades completed in 1997 that implies a 49.1% decrease in 2003 enrollment rates. The results, while generally insignificant, suggest that children from later-treated households were more likely to still be in school in 2003, perhaps because the T1998 youth progressed faster through school (see below).

*Impacts on Grade Progression:* We next examine how early exposure to the program affected grade progression. We measure progression by the proportion of students reported to have completed at least five additional school grades between 1997 and 2003, suggesting a progression rate that avoided dropout and failure.[19] The results shown in Table 4.3 indicate significant positive program impacts on the proportion of boys progressing regularly through school, implying an average 7.4% increase for boys of all ages considered. Those boys aged 11 and 12 in 1997, and close to the critical juncture for entering secondary school, show significant 14.1% increases in the proportion of those who progress on time. Boys who had four and five grades of schooling attainment in 1997 show significant increases of 8.4% and 28.8%. For girls, while the coefficients are also generally positive, they are insignificant. Girls typically have faster progression rates than boys even in the absence of the program intervention. Earlier evaluation results found that the program had a greater short-term impact on boys in terms of improving continuation rates.(See Behrman, Sengupta and Todd, 2005).

*Impacts on Educational Attainment:* In 1997, for both boys and girls in the 9 to 15 age range, there was no significant difference at baseline between schooling grades completed for the T1998 versus T2000 groups (See Table 4.1). By 2003, the estimates shown in Table 4.4 indicate that, for both boys and girls, there were significant differences of about a fifth of a grade on average (0.18 for boys and 0.20 for girls). Thus, greater exposure to the program for the T1998 group increased on average their schooling grades completed by about 2.4% for boys to 2.7% for girls beyond the schooling grades completed of the T2000 group by 2003.

Disaggregation by age group in 1997 and schooling grades completed in 1997, in addition to sex, is informative. For girls, the estimated impacts increase with age in 1997, and are significant for those aged 11-12 (implying a 2.3% increase) and for those 13-15 (implying a 4.3% increase). For boys, the estimated impacts peak for the middle age group in 1997, and are significant for all three age groups, implying a 2.7% increase for those in the 9-10 age group in 1997, a 3.1% increase for those 11-12 and a 1.8% increase for those 13-15. (Also see Figure 4.3 overall.) For both girls and boys, there are significant positive impacts for almost all of those who had less than seven grades of schooling completed in 1997 (with the single exception of boys who had only up to three grades of school-

[18] We give the rates for T2000 in the first column in the table because this group had less treatment than the T1998 group. The second column gives the difference between the rates for T1998 and T2000.

[19] It might appear that one could estimate the impact of the program on failure or dropout by looking, for instance, at the number of years failed or whether an individual has ever faileda grade. However, to fail a grade, an individual must be enrolled in school. If Oportunidades affects enrollment, as previously evaluations found it does, then the program might appear to increase failure rates for students who were induced by the program to be enrolled or to be enrolled in higher grades than they would have otherwise. We find that our indicator of progressing on time avoids these interpretation problems.

ing completed in 1997). In both cases the largest effects are observed for those who had five grades of schooling completed in 1997 (effects of 6.8% for girls, 4.4% for boys). Thus there are some small differences in the patterns for girls versus boys, but for both there were significant positive effects of greater program exposure on 2003 educational attainment levels. The effects are most pronouced for those who were entering the last year of primary school at the time the program was introduced.

*Impacts on Achievement Test Scores:* Tables 4.5 through 4.7 present the principal results on the impact of *Oportunidades* on achievement tests.[20] Overall, the results indicate no effects of greater program exposure on test scores. For all three achievement tests, the results generally show insignificant results, independent of age or baseline schooling levels. In fact, for math and written language skills, for some age groups, there are some unexpected negative and significant effects of the program on achievement scores.

Here, we explore some possible explanations for the finding of no impacts on achievement test scores. First, the tests were only applied in 2003, making it impossible to control for any preprogram differences between the groups. The results for the other outcome variables, for which preprogram data are available, indicate that preprogram differences are not significant. Nevertheless, we have no way of verifying whether any preprogram difference existed in achievement test scores. Second, the tests were applied to only a sub-sample of youth age 15 to 21 in 2003. The smaller sample size makes it more difficult to detect modest size impacts. It is also possible that this subsample to which the tests were applied experienced lower program impacts than the full sample. To examine this conjecture, we estimated the impacts of *Oportunidades* on grades of schooling completed for the sub-sample of youth taking the achievement tests. The results, reported in Appendix Table C.3, are similar to reported earlier for boys (Table 4.4), with on average boys from T1998 taking the tests showing about 0.21 additional grades of schooling than boys from T2000 taking the achievement tests. For girls, however, the results show overall no significant differences in grades of schooling between T1998 and T2000 for the sub-sample of those taking the tests. Thus, for the subsample of girls taking the tests, the impacts on grades completed and on test scores both tended to be insignificant. For boys, the test score results are surprising, because we find insignificant effects on test scores despite a significant impact on years of schooling.

Of course, there are other explanations for the lack of impacts on test scores that do not relate to data limitations. Low school quality might result in students achieving higher grades of schooling without improving their performance on achievement tests. Moreover, the higher enrollments induced by *Oportunidades* may have actually lowered school quality, both through congestion and through adding marginal students who would otherwise not have been attending school. Such an analysis is beyond the scope of the present study, given the data available. However, the test score results raise important questions for future investigation.

## 4.2 Work

The theoretical effect of *Oportunidades* on the probability of working is ambiguous. Suppose children have three alternative uses of their time: leisure, work, and school. The program subsidizes school-going, we would expect children to substitute away from time spent in leisure and work and towards time spent in school. However, as they accumulate schooling, they would be expected to receive higher wage offers. Assuming diminishing marginal returns to schooling, at some point, the marginal benefit of schooling (higher future wages) will no longer exceed the marginal cost (foregone wages and leisure time). These considerations would lead us to expect that over the short-run, the program would decrease working, but over the longer-run, the program might increase working. We next consider how the program affects three different measures related to work: the probability of working, the probability of participating in the agricultural sector, and the impact on monthly labor income.

*Impacts on Employment Levels and Employment in Agriculture:* Prior to the program in 1997, 0.18 of the T1998 boys and (significantly less at the 5% level) 0.16 of the T2000 boys were working; also in 1997, 0.08 of the T1998 girls and (significantly less at the 1% level) 0.05 of the T2000 girls were employed (Table 4.1). Because of life-cycle work patterns, in 2003 the proportions employed were much higher – for example, for the T2000 boys 0.65 and for the T2000 girls 0.26 (Table 4.8). The gender differentials in reported work are substantial.

The difference-in-difference estimate of the impact of the differential exposure to the program on working in 2003 shows that greater exposure significantly decreases the proportion working by 4.1% for boys, with no significant effects for girls (Table 4.8). When we disaggregate by age and baseline schooling levels,

[20] The tests were applied in the home, so taking the test does not depend on whether the child is enrolled in school.

for boys there are significant estimated declines in the proportions working in 2003 of -5.5% for those in the 13-15 age group in 1997 (19-21 in 2003) and of -15.9% for those who had seven plus grades of schooling completed in 1997.

Schooling is often claimed to have higher returns in non-agricultural than in agricultural work. We therefore examine whether *Oportunidades* induced any change in the unconditional probability of participating in agricultural work. Prior to the program in 1997, 0.16 of the T1998 boys and (significantly less at the 1% level) 0.14 of the T2000 boys were working in agriculture; also in 1997, 0.04 of the T1998 girls and (significantly less at the 1% level) 0.03 of the T2000 girls were employed in agriculture (Table 4.1) Because of life-cycle work patterns and the fact that a very high percentage of labor in the communities of interest works in agriculture, in 2003 the proportions employed in agriculture were much higher for the T2000 boys (0.42) and similar for the T2000 girls (0.05) (see Table 4.9).

The difference-in-difference estimates indicate a significant estimated decline (-22.4%) in the proportion of boys working in agriculture in 2003, but only for those who had 7 or more grades of school at baseline (Table 4.9).[21] None of the estimates is significant for girls, perhaps due to the relatively low participation rates in agricultural work for girls noted above.

*Impacts on Average Monthly Labor Income:* A priori, the program was expected to increase productivity through more schooling (which was increased, as noted above), which would likely increase wages and labor income for individuals who had completed their schooling. We therefore examine the effects of longer program exposure on average monthly labor income. We do not condition our analysis on working (an endogenous variable that is also affected by treatment), so the impacts we estimate on wages may reflect changes in the proportions of individuals working as well as changes in the earnings of working individuals.

Prior to the program in 1997 there were no significant differences in average monthly labor income for either boys or girls between T1998 and T2000 youth (Table 4.1). The difference-in-difference estimates in Table 4.10 indicate a significantly positive impact on average monthly wages for girls on average of 25.2%, but no significant effect for boys. This has an interesting interpretation with respect to the previous results on work. Given that the differential program exposure significantly reduced the probability of working for boys (see Table 4.8), it is not surprising that average labor income for boys falls as a result of the differential program exposure. For girls, however, where there was no significant increase in employment, so that the significant impact on wages suggests that the differential program exposure increased overall earnings for girls who work. This is consistent with the increased schooling that girls in T1998 received, although the results could also reflect increases in days or hours worked. The disaggregated estimates indicate that the average impact for girls is due to the impacts for girls who were relatively young when the program began (a significant increase of 36.4% for those 9 and 10 years old in 1997) and the least schooled (a significant 60.3% increase for those with up to three grades of schooling completed in 1997). For boys, in contrast, there are no significant estimated impacts for the three age groups considered for 1997, but surprisingly significantly negative estimated effects for the least and most schooled groups in 1997 of -11.2% and -30.7%, respectively.

## 4.3 Marriage

Marriage is a major life-cycle transition that could be affected by the program, perhaps through interactions with decisions about education, work and migration. For this analysis individuals are defined to be married if they report they are legally married or are living together (co-habitating). The literature suggests that increased schooling is likely to lead to lower marriage rates for youth in the age range being studied, which is likely to give them greater choices before they settle down in marital relations.

At baseline, in 1997, very small proportions of the children age 9 to 15 were married (<0.02 for girls, <0.01 for boys – see Table 4.1), though with significantly higher (at the 10% level) proportions for the T2000 girls than for the T1998 girls. In 2003 26% of T2000 girls were married and 10% of T2000 boys (Table 4.11).

The difference-in-difference estimates in Table 4.11 indicate that the proportion of girls married was not significantly affected by the program, at the 10% significance level. The estimated overall impact on boys age 9 to 15 in 1997 also is not significant at the 10% level. Disaggregating by age group and baseline schooling grades, however, there are some significant negative impact estimates, for boys with little (four) or relatively a lot (seven plus) grades of schooling in 1997. These estimates imply a decline of -12.9% in the proportion married by 2003 in the former case and a decline of -25.4% in the proportion married by

[21] Simple difference-in-difference estimates that do not include any additional control variables indicate that the program statistically significantly decreases the proportion working for both boys and girls.

2003 in the latter case. Thus, some of the youth with earlier exposure to the program appear to be delaying marriage.

### 4.4 Migration

Standard models of migration posit that migration occurs when it increases the expected welfare of the decision maker. In the context of rural Mexico, such an increase may be expected for a number of reasons, including better prospects for human capital investment, better prospects in labor markets and better prospects in marriage markets. In general, migration may involve movement of individuals or family units. The early literature focused on the individual incentives for migration in which case the decision-maker is the potential or actual migrant (e.g., Sjaastad (1962), Todaro (1969)). More recent literature has considered family strategies in which, for example, one child may be sent to another area in part to diversify earnings risk. Here, the decision-making unit is not the actual or potential migrant alone but the family unit of that individual (e.g., Todaro (1969), Falaris and Peters (1998)). The expected welfare gains from migration, of course, are likely to depend on individual and family characteristics. The gains moving from small poor rural communities, such as those in the *Oportunidades* evaluation sample, are likely to be greater for more-schooled individuals if the returns to more schooling are higher in urban areas than in the program communities, as is generally thought to be the case.

How would being in an eligible household in a treatment area be expected to affect migration? From the household perspective it would seem that the dominant effect would be to reduce household migration, because of the higher income due to the program operating in the origin community. However, the program could also increase household migration, if the income provided under the program alleviates liquidity constraints that precluded desired migration. For individual youth, as long as they were in school in grades covered by the program, the program also would seem to reduce their (or their families') incentives to migrate. However, once they completed school, if that schooling is greater than they would have had without the program and if returns to schooling are greater in labor and marriage markets (or in studying further) in more-urban areas, the dominant effect would seem to be to increase migration.

The proportion of individual youth who had migrated from their parental households between 1997 and 2003 is large – about a third of boys (0.32 of the T2000 boys, see Table 4.12) and almost four tenths of girls (0.39 of the T2000 girls). The difference-in-difference estimates in Table 4.12 imply that the proportion of boys who migrated was reduced due to the differential program exposure by a significant -6.2%.[22] The disaggregated estimates indicate that this drop was due primarily to significant declines for the oldest (-10.0% for boys age 13 to 15 in 1997) and most-schooled (-13.5% for boys with seven plus grades completed in 1997) boys. The proportion of girls on average who migrated was not changed significantly by the differential program exposure, but there was a significant drop of -12.3% in the proportion of the girls with up to three grades of completed school in 1997 that migrated by 2003.

The gender difference in the impact of the differential program exposure on migration is striking – with younger and less-schooled girls affected but older and more-schooled boys affected. The gender difference may reflect a greater tendency for girls to migrate for marriage and for boys to migrate for work.

## 5 How impacts vary with quality of schooling

In addition to time in school, school characteristics (or "school quality") are widely thought to affect educational outcomes.[23] That raises the question of whether the impact of differential exposure to the *Oportunidades* treatment might depend on school characteristics. A hypothesis behind why they might vary is that parents may be more responsive to the *Oportunidades* grants in sending their children to school if they perceive that the quality is high and thus the benefits of sending children (apart from receiving the grants) is high. Furthermore, if returns to schooling are higher when schooling quality is higher, then *Oportunidades* students who study in higher quality schools may show higher increases in earnings in the medium to long term when they enter the labor market, relative to other students with similar education but who attend lower quality schools. In

[22] In estimating impacts on migration, we do not need to weight for attrition, because we observe whether the person/family migrated for the entire sample.

[23] There is an extensive literature on the relationship between educational outcomes and school quality. See, e.g, Alderman, Behrman, Ross and Sabot (1996), Alderman, Orazem and Paterno (2001), Behrman and Birdsall (1983), Behrman, Birdsall and Kaplan (1996), Behrman, Khan, Ross and Sabot (1997), Behrman, Rosenzweig and Taubman (1996), Behrman, Ross and Sabot (2004), Betts (1995, 1996), Card and Krueger (1992), Grogger (1996), Heckman, Layne-Farrar and Todd (1996), Lloyd, Mensch, and Clark (2000).

this sense, school quality may affect educaitonal outcomes as well as working and income impacts. Here, we focus on how educational attainment impacts vary with school quality, because the work and income impacts are likely to only be observed over a longer time horizon.

To investigate these questions, we consider selected school characteristics that are potential measures of school quality. As part of the ENCEL2003 survey, detailed questionnaire were undertaken on school quality, applied to the school director, as well as two teachers at the school, randomly selected. The data are of a high quality and are useful for evaluating the level of quality available at schools where *Oportunidades* students attend as well as comparing this quality with quality available at a national level. (See Appendix A for more details on data construction).

We focus on measures of school quality at the secondary level, considering this to be the most relevant school level given the age group studied and the relatively low number of youth in our sample with more than a secondary school level education. We choose two variables on which to focus, the type of secondary school available to the youth and the student teacher ratio in that school/schools of interest. Of course there are many potential measures of school quality, here we only analyze two that we consider to be important in the Mexican environment. Future more in depth studies of school quality should consider other variables such as school infraestucture and teacher qualifications.

Our estimation strategy is to replicate the estimates of the impact of *Oportunidades* on grades of schooling completed, dividing the sample into two groups, those youth who only have access to a telesecundaria school versus students who have access to at least one of another type (general or technical school) and dividing the sample between those with a high student teacher ratio (prior to the program) and those with a low student teacher ratio. Telesecundaria schools differ from other secondary schools in Mexico as they rely on videos by satellite shown during class time in different subjects, followed by time spent doing exercises. There is only one teacher for subjects. They are thought to be a cost-effective way to bring secondary schooling to rural areas and are the most common type of schools in rural areas of Mexico. General secondary schools have more school infrastructure and each subject is taught by a specialized instructor. Technical secondary schools also have a specialized instructor who teaches each subject, teaching focuses on technological education, with generally some relation to the particular economic activities of the relevant region. For our analysis, we focus only on impact estimates of grades of schooling attainment.

Tables 4.13 and 4.14 provide the differential exposure estimates by school characteristics. Table 4.13 shows that the impacts appear to be higher when students have access to a general or technical secondary school. The differential exposure results indicate that which represents the impact on grades of schooling, for both boys and girls, is overall more than twice the size for students who have access to a general or technical school versus those who only have access to a telesecundaria school.

Table 4.14 shows impacts by student teacher ratios at available secondary schools prior to the program. Here we simply divide available schools according to those above and below the average student teacher ratio in the sample, "high" student teacher ratios are those with more than 20 students per teacher, "low" student teacher ratios are those with less than 20 students per teacher. Generally, lower student/teacher ratios are perceived at the international level to represent higher quality, presumably because students in small classes receive more attention. The results are suggestive that students having access to schools with lower student/teacher ratios tend to show higher program impacts although program impacts are significant for both groups. For instance, for all boys aged 9 to 15 in 1997, program impacts when they have access to secondary schools with low student/teacher ratios are 0.25 versus 0.17 in schools with high student/teacher ratios. The corresponding results for girls aged 9 to 15 in 1997 are 0.18 in schools with high student/teacher ratios versus 0.12 in schools with low student teacher ratios.

To summarize, our results suggest that *Oportunidades* impacts do differ with the quality of schooling available, at least as captured by the two quality indicators considered here. As there are many other aspects of school quality, our paper provides only an initial glimpse into these areas. The potential relationship of the impact of Oportundades to school quality should be an important topic for future work.

# 6 Conclusions

This paper presented an assessment of the impacts of *Oportunidades* on rural adolescent youth after five and a half years of benefits. The results on the effects of differential exposure to the program indicate that children with a year and a half more of benefits achieve about 0.2 grades of additional schooling. This paper also analyzed the impact of *Oportunidades* on achievement tests in the areas of reading, mathematics and written language. Achievement tests are considered to be among the most objective measures of the extent children are learning more in school as a result of their additional schooling, and are likely to be highly correlated with the returns to schooling when entering the labor force. Our impact analysis did not find statistically significant impacts of the program on achievement test scores. Such a finding might suggest the need for design changes in the program, such as linking grants to performance rather than enrollment, to provide more encouragement for learning. There clearly a need for a more in depth look at the quality of the schools the children are attending to determine whether the program might usefully be supplemented by supply-side interventions aimed at school quality. As discussed in the text, there are some other possible explanations for the finding of no impact on achievement test scores, despite documented effects on educational attainment. A limitation for the test score analysis, but not for the education analysis, was the lack of baseline data. An additional limitation was the much smaller sample size (compared with the overall sample) to which the achievement tests were applied, which might explain why significant effects were not found.

With respect to work, our analysis revealed some significant impacts, principally on boys who tend to have much higher labor force participation rates than do girls in the rural communities under study. Boys with longer exposure have a reduced probability of working but for girls there is no significant impact. Boys also have a reduced probability of working in agriculture. As discussed earlier, the theoretical effect of the program on work is ambiguous. On the one hand children in school are likely to show a reduced participation in work. Once they have finished school, however, the increase in their schooling should result in higher employment and wages. At least for boys in the age group studied here, the apparent dominant effect thus far is for schooling to substitute for work, perhaps not surprising for the age group analyzed in this paper. For girls, there is no significant effect on work; however, labor market participation remains low for females in these rural communities and previous evaluations also did not find reductions in work for girls, with the exception of time spent in domestic housework (see Parker and Skoufias, 2000).

It clearly is of great interest to study the impact of *Oportunidades* on work trajectories and income of youth after they have finished their schooling. Nevertheless, in the current context this topic still seems to be premature. Many of the youth in our sample continue to be in school and even those who have finished their schooling are likely to be only about to begin to enter the workforce. Furthermore, the children we study in this paper could only have received a maximum of five and a half years of the education grants, even though the program provides education grants for 10 grades (third grade through twelfth grade). Additional rounds of data and evaluation will likely be necessary to evaluate the effect of the Program on the future employment and income of its current beneficiaries.

Another important area for future research is the relationship of the program to migration and whether youth who increase their education level will be more likely to migrate out of the community and possibly see higher returns and greater benefits from this increased schooling. This would seem to be a critical area of research for the longer-term impacts of the program, and likely necessitates return visits to the communities of interest and possibility following the migrants themselves.

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## 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). An additional concern arises over whether age inconsistencies over time as well as in other indicators might reflect errors in id numbers resulting in individuals "matching" incorrectly.

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.

We also deleted from the sample individuals who reported impossible changes in the schooling grades completed over time. That is, we eliminated individuals reporting negative changes in schooling or those reporting they had completed 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.

*Progressing through school* is defined as 1 if the difference between schooling grades completed in 1997 and schooling grades completed in 2003 is at least five, otherwise it is defined as zero. The college degree is assumed to start after preparatory school so it is counted initially as 15 grades of school plus the number of years of college reported, up to a maximum of 5 years. For those reporting masters or doctoral degrees, it was considered that they already had 17 years of education and the upper bound for these degrees was set at three years.

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 the variable 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.

*Monthly labor income* is constructed by using survey information on payments and the periods for these payments for employment. We deal with outliers by eliminating the top 1% of monthly labor income. Individuals with no income are coded as having 0 pesos of monthly labor income.

Individuals are defined to be *married* if they report they are legally married or are living together (cohabitating).

*Attrition and migration:* Here we describe the definitions and differences between attritors and migraters, given some peculiarities of the survey design. In general, most attrition is due to migration, either of an individual within a particular household or because of an entire household leaving the sample. Other potential reasons for attrition are refusal to answer (only relevant at the household level as there is only one informant per household) or death. With regard to household-level attrition, of the 24,077 households in the original ENCASEH 1997 sample, 3,989 households do not have a completed socio-economic survey in 2003, an attrition rate of about 16%. We have some information for the reason a household was not interviewed for a majority of, but not all households. Only a low percentage of households refused to answer the survey, most household level attrition appears to be due to migration.

Turning to individual attrition, in accordance with the survey definition, we define attritors to be individuals who have been out of the household for a least one year as well as those who have passed away. Thus, nearly all individuals in our sample who attrit are migrators given that in this age group mortality rates are very low. In the survey, individuals who have left the household less than a year prior to the survey are considered as residents (e.g. non-migrants) and the survey is conducted as if they were residents. Only individuals who have left the household more than a year previously are considered as migrants by the survey. All survey information is captured for all individuals except migrants and those who have passed away (e.g. attritors), some very limited information is captured for attritors. For our analysis on migration, we depart a bit from the survey by considering any individual reported to have left the household (including those having left less than a year prior) to be migrants.

*Linking school characteristics to individuals.*

In this sub-section, we describe how we construct construct and link school characteristics data to students in our sample. The data collected present some challenges for the analysis, because they contain data on characteristics of schools in 2003 and there is no equivalent baseline data prior to the program. Taking school characteristics in 2003 to be exogenous to schooling attainment is probably not correct given that characteristics such as student teacher ratio, as well as other indicators of school investment are undoubtedly affected by the Program. In the current context, that is, analyzing whether program impacts on schooling vary by available school quality, the most appropriate measures are pre-program levels, either with data carried out prior to the program or from data on characteristics in 2003 that would be unlikely to change over time.

We use pre-program administrative data from the Secretary of Public Education (SEP) on schools in 1997 and we also use data from the director's survey carried out as part of the ENCEL 2003 survey. We focus on the type of secondary school available to youth and the student teacher ratio using pre-program information from SEP. Type of school in a given school is unlikely to change over time, thus using 2003 information on type of school should be exogenous to program impacts, unless in response to the program Oportunidades, a number of new schools were built post-program. For this reason, we also use the type of school available as defined by pre-program data from the SEP for the differential exposure results, obtaining very similar results.

Using the school information from the ENCEL2003 data, we construct our definition of access to a secondary school using information on the actual school attended by individuals in 2003. Given this information is missing for a number of individuals actually enrolled as well as nearly all individuals not enrolled, we carry out the following procedure. For each community, we construct a list of all secondary schools attended by youth within the community. For all individuals in each community, we assume the available supply of schools reflects that list of schools attended. We then merge this list to the actual schools who were interviewed and this determines the supply of schools at the community level. In this sense, we construct indicators of the potential supply of schools at the community level.[24] Using this method, we are successful at merging characteristics of available schools for 79.2% of individuals in T1998 and 78.9% in T2000.[25]

With respect to the SEP data, we use administrative information from 1997 to construct our relevant indicators. For each community, we assign the relevant secondary school to be either the secondary school inside the community, or when the community has no school, the secondary school (or schools) which is the closest to the community. Carrying out this procedure resulted in matching approximately 85% of students to a potential supply of schools. In practice, using both sets of data results in similar estimates, those presented in the paper are derived from the 1997 SEP estimations.

## Appendix B. Analysis of Attrition

This appendix provides the estimates and greater details that underlie the discussion of attrition in Section 3.2. Table B.1 gives probit estimates for the probability of being lost to follow-up overall (hereafter "overall") 9 to 15 years old in 1997 in eligible households from the T1998 and T2000 groups – again, for all attritors, individual attritors and household attritors. For each of these three dependent variables, there are estimates for two specifications: (1) Only whether in T1998 group and (2) whether in T1998 group plus interactions between whether treated and pre-program individual characteristics, parental characteristics and housing characteristics. Tables B.2 and B.3 present similar estimates, but separately for boys and girls.

[24] Note, however, that these community level indicators of the supply of schools might be correlated with other community level variables affecting schooling, for instance local labor markets. E.g. if having access to only telesecundaria schools is correlated with few potential labor market options, then the impacts estimated here may confound both school quality with labor market options. In this sense, the results presented in this section should not be considered definitive with respect to school quality but simply suggestive of potential differences.

[25] We are able to match however only 58.5% of those in C2003. This lesser success in the C2003 group reflects the sample design for the school questionnaires. This was because sample design for schools attended by the T1998 and T2000 groups was able to take into account more precise information on where Oportunidades beneficiaries attending school, whereas in the C2003 by definition, this was not possible. We consider this quite problematic for considering school quality effects in the matching analysis. First it reduces our sample sizes as we do not have school characteristics for an important minority of the sample, furthermore the lower success rate of capturing schools attended by the C2003 group implies the sample of those with school characteristics may vary in important unobserved ways and likely to be correlated with these impact estimates. For this reason, in this analysis we only report impact estimates by school characteristics using the differential exposure analysis.

The first specification (column (1)), not surprisingly, replicates the patterns noted with regard to Table 3.1. The second specification (column (2)) indicates that a number of the pre-program individual, parental and housing characteristics interact significantly with T1998 to affect attrition:

Among the pre-program individual characteristics:

- Age in 1997 significantly negatively interacts with T1998 for household attrition for girls.
- Speaking an indigenous language significantly negatively interacts with T1998 for household attrition overall and for boys and girls considered separately.
- Own-schooling significantly positively interacts with T1998 for household attrition overall and for girls

Among the pre-program parental characteristics:

- Father's schooling grade attainment significantly positively interacts with T1998 overall and for boys for individual attrition and significantly negatively interacts with T1998 overall and for boys for household attrition.
- Father's age significantly positively interacts with T1998 for total and individual attrition for girls.
- Father speaking an indigenous language significantly positively interacts with T1998 for total attrition overall and for girls and for household attrition overall and for girls and boys separately.
- Father being bilingual significantly negatively interacts with T1998 for total attrition overall and for girls and for individual attrition for girls.
- Mother's age significantly negatively interacts with T1998 for total attrition and individual attrition for girls.
- Mother being bilingual significantly positively interacts with T1998 for total attrition overall and for girls.

Among the pre-program housing characteristics:

- Number of rooms in the house significantly positively interacts with T1998 for total attrition overall and for individual attrition overall.
- Whether the house had electricity significantly negatively interacts with T1998 for household attrition overall.
- Whether the house had indoor water significantly positively interacts with T1998 for individual attrition overall and for boys and for total attrition for boys.

Thus, though in the aggregate there is not evidence of significant impacts of the timing of treatment on attrition, the timing of treatment appears to be significantly negatively associated with individual migration and significantly positively associated with household migration – and there are a number of significant interactions with individual, parental and housing characteristics (differing in many cases for boys versus girls). Therefore biases may result if we do not correct for attrition in our estimates – so we do correct by re-weighting observations to counter the effects of differential attrition. For some of our outcome variables, we do have information for individual migrants, as this was provided by the household informant. For these variables, we then only reweight to take into account household level attrition.

**Figure 1.1b. School Enrollment and Labor Force Participation of Girls in PROGRESA. Communities Prior to Program Implementation**

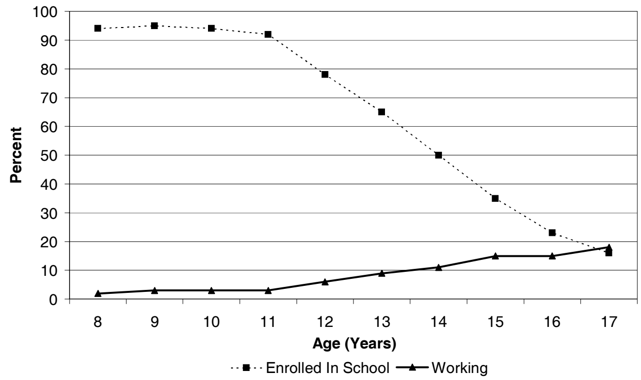



**Figure 1.1 a. School Enrollment and Labor Force Partcipation of Boys in PROGRESA. Communities Prior to Program Implementation**

**Table 2.1. 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 3.1. Proportion attriting by 2003 from original ENCASEH: individuals 9 to 15 in 1997**

| | Treatment (T1998) | | Control (T2000) | | P>\|Z\| |
|---|---|---|---|---|---|
| | **N** | **Mean** | **N** | **Mean** | |
| **A. Total proportion attriting (individual or household)** | | | | | |
| 9 to 15 years (all) | 15,126 | 0.406 | 9460 | 0.409 | 0.589 |
| 9 to 15 years (poor using original definition) | 10,102 | 0.388 | 6,155 | 0.392 | 0.563 |
| 9 to 15 years (poor using pobreden) | 12,773 | 0.397 | 7,912 | 0.396 | 0.859 |
| *By gender* | | | | | |
| Boys 9 to 15 years (poor using original definition) | 5,269 | 0.355 | 3,115 | 0.368 | 0.231 |
| Girls 9 to 15 years (poor using original definition) | 4,831 | 0.422 | 3,039 | 0.417 | 0.644 |
| **B. Proportion due to individual attrition** | | | | | |
| 9 to 15 years (all) | | 0.247 | | 0.260 | 0.022 |
| 9 to 15 years (poor using original definition) | | 0.246 | | 0.267 | 0.003 |
| 9 to 15 years (poor using pobreden) | | 0.254 | | 0.269 | 0.016 |
| *By gender* | | | | | |
| Boys 9 to 15 years (poor using original definition) | | 0.213 | | 0.239 | 0.006 |
| Girls 9 to 15 years (poor using original definition) | | 0.282 | | 0.296 | 0.181 |
| **C. Proportion due to household attrition** | | | | | |
| (individual not found because household moves) | | | | | |
| 9 to 15 years (all) | | 0.159 | | 0.149 | 0.044 |
| 9 to 15 years (poor using original definition) | | 0.141 | | 0.125 | 0.003 |
| 9 to 15 years (poor using pobreden) | | 0.143 | | 0.127 | 0.001 |
| *By gender* | | | | | |
| Boys 9 to 15 years (poor using original definition) | | 0.142 | | 0.129 | 0.092 |
| Girls 9 to 15 years (poor using original definition) | | 0.140 | | 0.120 | 0.014 |

Notes: 1) The last column gives the significance level for mean differences between T1998 and T2000 based on t-tests. 2) Number of cases for boys and girls does not sum to total cases given a few missing observations on gender.

Figure 3a. Distribution of WJ raw scores: Reading

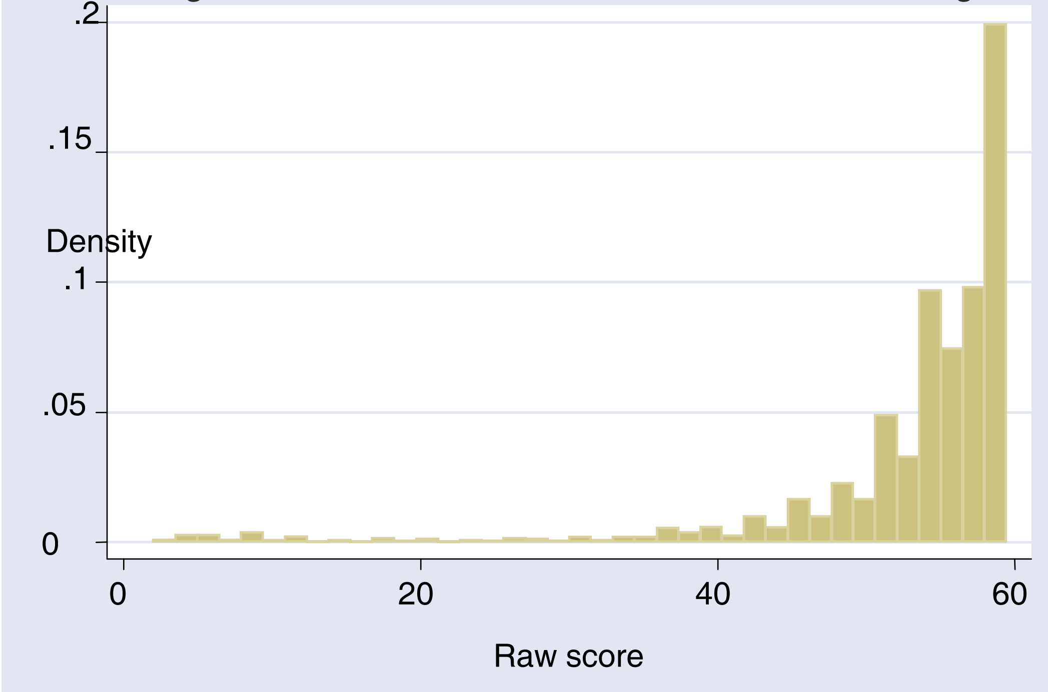
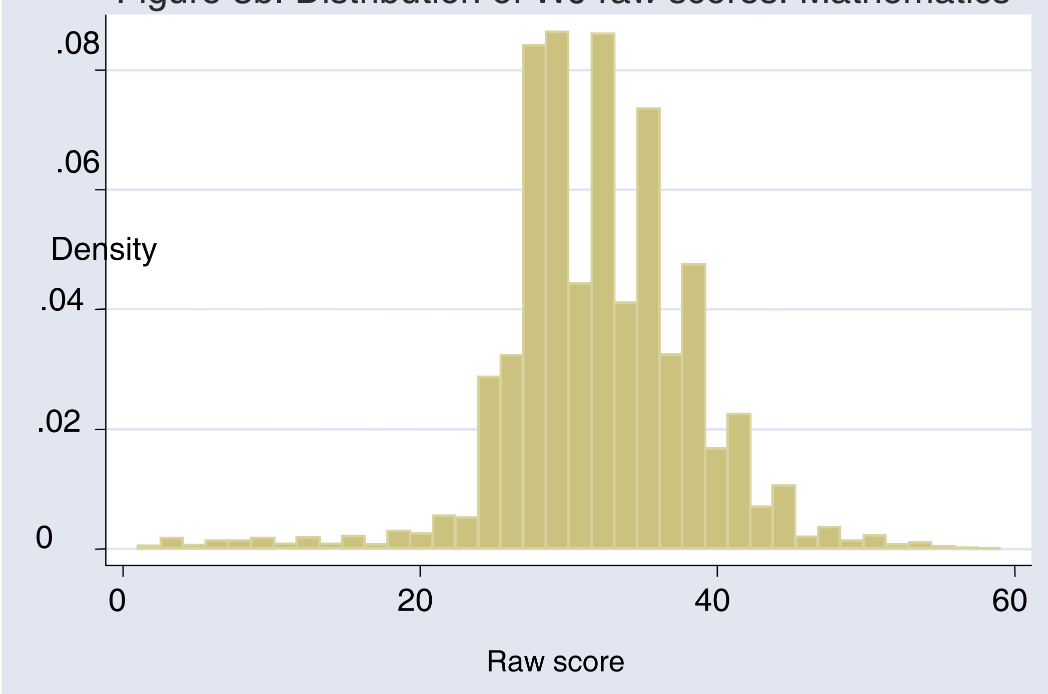



Figure 3b. Distribution of WJ raw scores: Mathematics



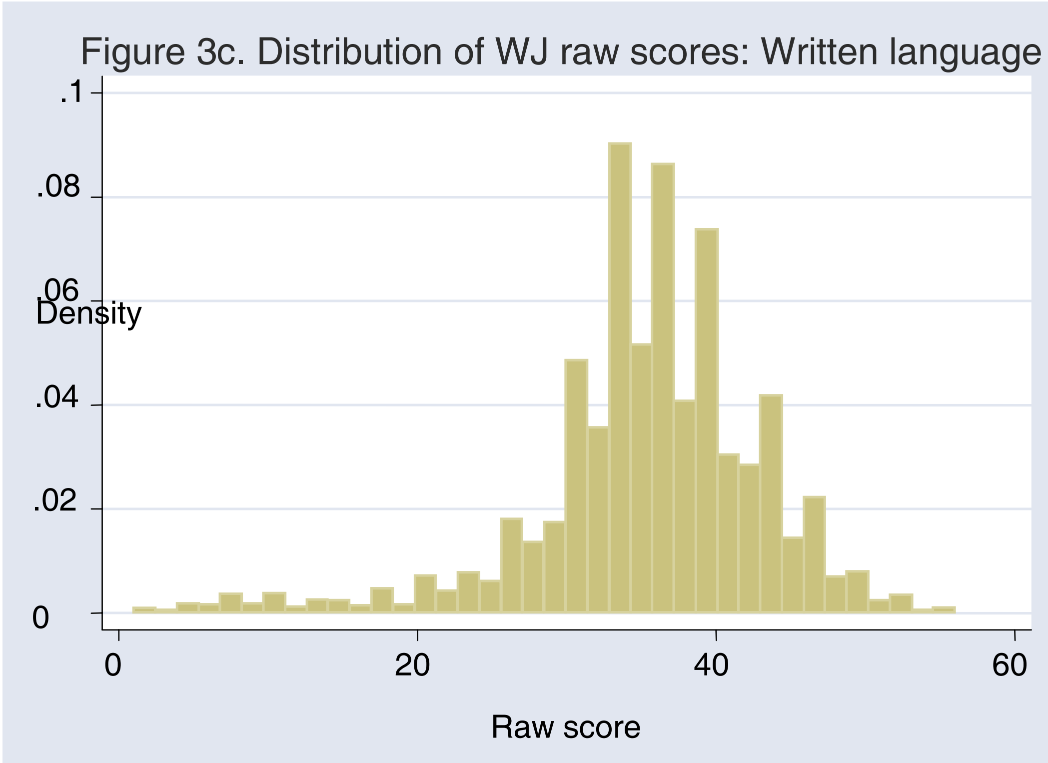


Figure 3c. Distribution of WJ raw scores: Written language
Density
0
.02
.04
.06
.08
.1
0
20
40
60
Raw score

**Table 3.2: Sample size of adolescents applied the Woodcock Johnson tests: ENCEL2003**

| Adolescents 15 to 21 in 2003 | Original treatment group: T1998 | Original control group: T2000 |
|---|---|---|
| *Total # Applied WJ tests* | 2,918 | 2,605 |
| | | |
| # applied WJ tests and matching with 1997 ENCASEH | 2,170 | 1,878 |
| Total adolescents matching with 1997 ENCASEH | 8,984 | 5,591 |
| % (# applied WJ/total adolescents) | 24.1% | 33.6% |
| | | |
| # Eligible applied WJ tests and matching with 1997 ENCASEH | 1,426 | 1,216 |
| Total Eligible adolescents matching with 1997 ENCASEH | 6,182 | 3,742 |
| % (Eligible applied WJ/total eligible adolescents) | 23.1% | 32.4% |

**Table 4.1. Differences in Pre-Program Means in 1997 between T1998 and T2000 for Indicators Considered in Section 4 (when pre-program measures exist).**

| | Mean values in 1997 | | P>\|Z\|, \|T\| Pre-program difference between T1998 and T2000 |
|---|---|---|---|
| | **T1998** | **T2000** | |
| **School enrollment**[1] | | | |
| Boys 9 to 15 in 1997 | 0.821 | 0.807 | 0.182 |
| Girls 9 to 15 in 1997 | 0.773 | 0.757 | 0.085 |
| **Grades of schooling completed** | | | |
| Boys 9 to 15 in 1997 | 4.514 | 4.513 | 0.967 |
| Girls 9 to 15 in 1997 | 4.580 | 4.610 | 0.568 |
| **Employment**[2] | | | |
| Boys 9 to 15 in 1997 | 0.179 | 0.164 | 0.040 |
| Girls 9 to 15 in 1997 | 0.078 | 0.054 | 0.000 |
| **Proportion working in agriculture sector** | | | |
| Boys 9 to 15 in 1997 | 0.160 | 0.137 | 0.002 |
| Girls 9 to 15 in 1997 | 0.044 | 0.027 | 0.000 |
| **Average monthly labor income**[3] | | | |
| Boys 9 to 15 in 1997 | 33.341 | 46.213 | 0.260 |
| Girls 9 to 15 in 1997 | 12.609 | 11.561 | 0.832 |
| **Marriage**[4] | | | |
| Boys 9 to 15 in 1997 | 0.002 | 0.002 | 0.868 |
| Girls 9 to 15 in 1997 | 0.007 | 0.014 | 0.077 |

Notes: 1. Proportion currently enrolled; 2. Proportion currently working; 3. Pesos; 4. Proportion currently married or co-habitating; 5. Proportion of individuals leaving household.
Sample includes all program-eligible individuals aged 9 to 15 in 1997 who are also interviewed in 2003. t-tests are used to test for 1997 (pre-program) differences in the means between T1998 and T2000 (column 3 gives the levels at which the mean differences in 1997 are significant). There are no entrees in this table for progressing on time (Section 4.1) and migration (Section 4.4) because both variables refer to changes between 1997 and 2003, not to states in 1997 and 2003.

**Figure 4.1. Proportion Attending School in 1997 and 2003 by age in 1997**

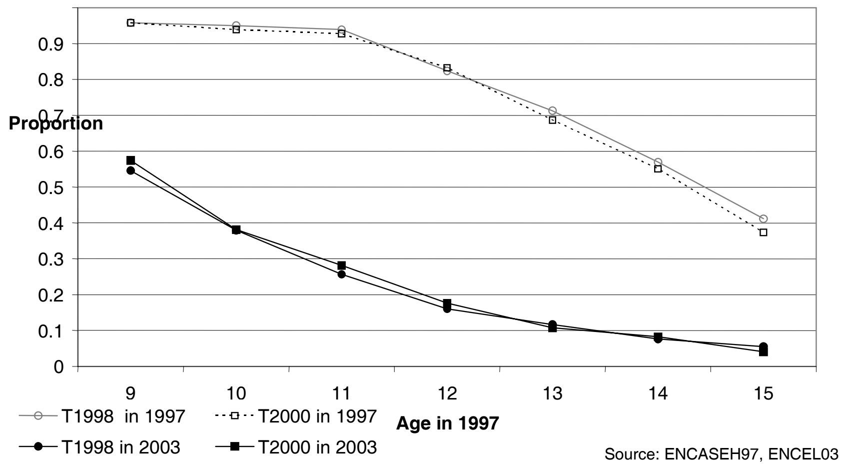
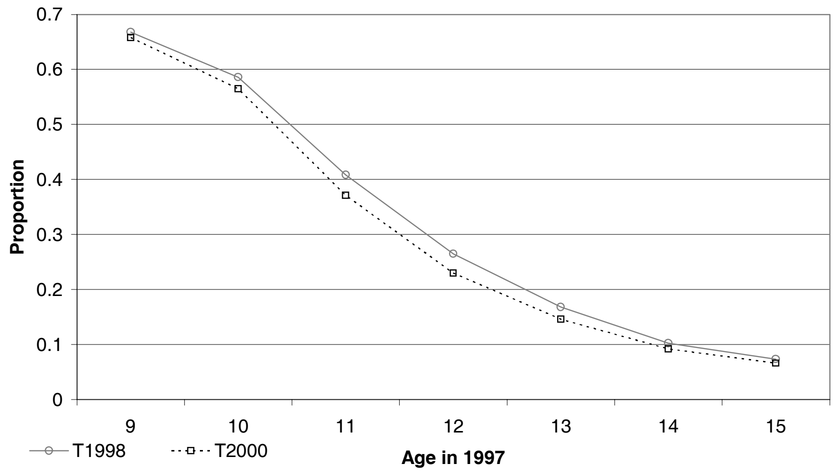


**Table 4.2. Impact of Differential Exposure to *Oportunidades* on Proportions Enrolled in School:**
*Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T998 versus T2000.

| | | Impact | | |
|---|---|---|---|---|
| | **Proportion enrolled in 2003 of T2000 group** | **Coefficient** | **Std. error** | **% change relative to T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.26 | -0.017 | [0.014] | -6.6% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.48 | -0.040 | [0.024]* | -8.3% |
| 11 to 12 | 0.24 | -0.003 | [0.025] | -1.2% |
| 13 to 15 | 0.10 | -0.007 | [0.023] | -7.3% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.37 | -0.007 | [0.024] | -1.9% |
| 4 | 0.33 | -0.018 | [0.032] | -5.5% |
| 5 | 0.24 | -0.048 | [0.031] | -20.4% |
| 6 | 0.10 | -0.040 | [0.029] | -40.0% |
| 7 + | 0.15 | -0.052 | [0.038] | -35.2% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.24 | -0.012 | [0.014] | -5.0% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.48 | -0.001 | [0.024] | -0.2% |
| 11 to 12 | 0.21 | -0.029 | [0.023] | -13.9% |
| 13 to 15 | 0.08 | -0.015 | [0.022] | -18.2% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.37 | -0.030 | [0.023] | -8.1% |
| 4 | 0.25 | -0.015 | [0.029] | -6.1% |
| 5 | 0.19 | 0.000 | [0.031] | 0.0% |
| 6 | 0.11 | -0.054 | [0.032]* | -49.1% |
| 7 + | 0.11 | 0.032 | [0.035] | 28.5% |

Notes: Estimates based on *difference-in-difference* regression estimates. Controls for parental age, education, indigenous status , housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level.

**Figure 4.2. Progressing on time in 2003 by age in 1997**



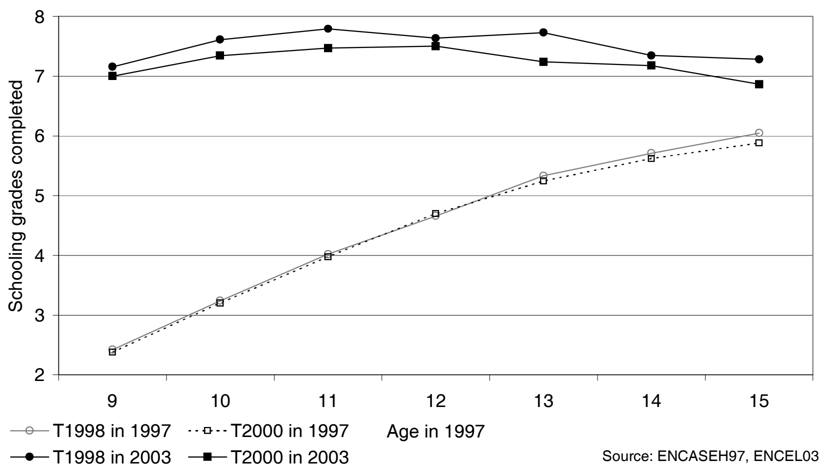

Source: ENCASEH97, ENCEL03

**Table 4.3. Impact of Differential Exposure to *Oportunidades* on Progressing through School on Time (Defined as Whether Completed Five or More Grades between 1997 and 2003)**
*Difference-in-difference Estimates: Boys Adolescents 9 to 15 in 1997*
T1998 versus T2000

| | Proportion progressing on Time in 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | Coefficient | Std. error | % change relative to T2000 group |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.308 | 0.008 | [0.009] | 2.6% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.606 | -0.008 | [0.018] | -1.3% |
| 11 to 12 | 0.303 | 0.020 | [0.018] | 6.6% |
| 13 to 15 | 0.090 | 0.012 | [0.010] | 13.4% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.491 | 0.013 | [0.017] | 2.6% |
| 4 | 0.521 | 0.020 | [0.024] | 3.8% |
| 5 | 0.191 | 0.010 | [0.020] | 5.2% |
| 6 | 0.093 | 0.012 | [0.013] | 12.9% |
| 7 + | 0.121 | -0.007 | [0.017] | -5.8% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.312 | 0.023 | [0.008]*** | 7.4% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.619 | 0.020 | [0.018] | 3.2% |
| 11 to 12 | 0.298 | 0.042 | [0.017]** | 14.1% |
| 13 to 15 | 0.099 | 0.012 | [0.010] | 12.1% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.493 | 0.010 | [0.017] | 2.0% |
| 4 | 0.522 | 0.044 | [0.023]* | 8.4% |
| 5 | 0.156 | 0.045 | [0.019]** | 28.8% |
| 6 | 0.103 | 0.018 | [0.014] | 17.4% |
| 7 + | 0.103 | 0.009 | [0.016] | 8.8% |

Notes: Estimates based on difference in whether progressed at least five grades between 1997 and 2003 regression estimates (which effectively are difference-in-difference estimates for whether grades completed changed by at least five during the time period between 1997 and 2003). Controls for parental age, education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Figure 4.3. Grades of schooling completed in 1997 and 2003 by age in 1997**




Source: ENCASEH97, ENCEL03

**Table 4.4. Impact of Differential Exposure to *Oportunidades* on Schooling Grades Completed**
*Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | Schooling grades completed by 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | Coefficient | Std. error | % change relative to T2000 group |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 7.52 | 0.201 | [0.047]*** | 2.7% |
| **By age group in 1997** | | | | |
| 9 to 10 | 7.43 | 0.075 | [0.076] | 1.0% |
| 11 to 12 | 7.75 | 0.181 | [0.091]** | 2.3% |
| 13 to 15 | 7.44 | 0.320 | [0.077]*** | 4.3% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 6.03 | 0.057 | [0.083] | 0.9% |
| 4 | 7.76 | 0.180 | [0.106]* | 2.3% |
| 5 | 7.75 | 0.529 | [0.113]*** | 6.8% |
| 6 | 7.37 | 0.304 | [0.097]*** | 4.1% |
| 7 + | 9.68 | 0.117 | [0.121] | 1.2% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 7.54 | 0.180 | [0.045]*** | 2.4% |
| **By age group in 1997** | | | | |
| 9 to 10 | 7.38 | 0.197 | [0.075]*** | 2.7% |
| 11 to 12 | 7.68 | 0.241 | [0.088]*** | 3.1% |
| 13 to 15 | 7.56 | 0.139 | [0.074]* | 1.8% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 5.97 | 0.137 | [0.074]* | 2.3% |
| 4 | 7.63 | 0.196 | [0.102]* | 2.6% |
| 5 | 7.89 | 0.347 | [0.111]*** | 4.4% |
| 6 | 7.67 | 0.204 | [0.103]** | 2.7% |
| 7 + | 9.62 | 0.047 | [0.111] | 0.5% |

Note: Estimates based on *difference-in-difference* regression estimates. Controls for parental age, education, indigenous status , housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Table 4.5. Impact of Differential Exposure of *Oportunidades* on Woodcock Johnson: Reading skills**

*Difference Estimates: Adolescents 9 to 15 in 1997*

T1998 versus T2000.

| | Raw score WJ test 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | Coefficient | Std. error | % change relative to T2000 group |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 53.56 | -0.11 | [0.483] | -0.2% |
| **By age group** | | | | |
| 9 to 10 | 53.74 | -1.244 | [0.882] | -2.3% |
| 11 to 12 | 54.04 | 0.016 | [0.745] | 0.0% |
| 13 to 15 | 53.58 | 0.191 | [0.898] | 0.4% |
| **By grades of schooling 1997** | | | | |
| <=3 | 51.50 | -1.339 | [1.160] | -2.6% |
| 4 | 54.55 | 0.259 | [0.771] | 0.5% |
| 5 | 54.83 | 0.651 | [0.666] | 1.2% |
| 6 | 54.70 | 0.379 | [0.735] | 0.7% |
| 7 + | 55.11 | 1.11 | [1.086] | 2.0% |
| | | | | |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 53.64 | 0.199 | [0.496] | 0.4% |
| **By age group** | | | | |
| 9 to 10 | 53.60 | 0.112 | [0.780] | 0.2% |
| 11 to 12 | 54.36 | -0.544 | [0.680] | -1.0% |
| 13 to 15 | 53.35 | 0.491 | [1.134] | 0.9% |
| **By grades of schooling 1997** | | | | |
| <=3 | 51.94 | 0.85 | [0.994] | 1.6% |
| 4 | 54.82 | -1.365 | [0.927] | -2.5% |
| 5 | 54.94 | -1.164 | [1.151] | -2.1% |
| 6 | 54.66 | 0.055 | [0.648] | 0.1% |
| 7 + | 54.68 | 1.118 | [1.462] | 2.0% |

Note: Estimates based on difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).

* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Table 4.6. Impact of Differential Exposure of *Oportunidades* on Woodcock Johnson: Mathematics skills.**
*Difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | | Impact | | |
|---|---|---|---|---|
| | **Raw score WJ test 2003 of T2000 group** | **Coefficient** | **Std. error** | **% change relative to T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 32.01 | -0.225 | [0.326] | -0.7% |
| **By age group** | | | | |
| 9 to 10 | 32.27 | -0.725 | [0.573] | -2.2% |
| 11 to 12 | 32.04 | -0.186 | [0.575] | -0.6% |
| 13 to 15 | 31.85 | 0.078 | [0.638] | 0.2% |
| **By grades of schooling 1997** | | | | |
| <=3 | 30.67 | -1.042 | [0.619]* | -3.4% |
| 4 | 32.25 | -0.106 | [0.632] | -0.3% |
| 5 | 32.84 | -0.311 | [0.713] | -0.9% |
| 6 | 33.02 | 0.022 | [0.807] | 0.1% |
| 7 + | 33.38 | 2.037 | [1.100]* | 6.1% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 33.27 | -0.574 | [0.366] | -1.7% |
| **By age group** | | | | |
| 9 to 10 | 32.79 | -0.228 | [0.545] | -0.7% |
| 11 to 12 | 34.20 | -1.145 | [0.635]* | -3.3% |
| 13 to 15 | 33.31 | -0.744 | [0.765] | -2.2% |
| **By grades of schooling 1997** | | | | |
| <=3 | 31.87 | -0.829 | [0.595] | -2.6% |
| 4 | 33.42 | -0.548 | [0.661] | -1.6% |
| 5 | 34.27 | -1.219 | [1.192] | -3.6% |
| 6 | 34.62 | -0.258 | [0.662] | -0.7% |
| 7 + | 35.42 | 1.334 | [1.476] | 3.8% |

Note: Estimates based on difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Table 4.7. Impact of Differential Exposure of *Oportunidades* on Woodcock Johnson: Written Language skills**

*Difference Estimates: Adolescents 9 to 15 in 1997*

T1998 versus T2000.

| | | Impact | | |
|---|---|---|---|---|
| | **Raw score WJ test 2003 of T2000 group** | **Coefficient** | **Std. error** | **% change relative to T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 36.12 | -0.301 | [0.410] | -0.8% |
| **By age group** | | | | |
| 9 to 10 | 36.93 | -1.361 | [0.725]* | -3.7% |
| 11 to 12 | 35.83 | 0.186 | [0.678] | 0.5% |
| 13 to 15 | 36.22 | -0.501 | [0.801] | -1.4% |
| **By grades of schooling 1997** | | | | |
| <=3 | 34.48 | -1.541 | [0.848]* | -4.5% |
| 4 | 36.98 | -0.294 | [0.795] | -0.8% |
| 5 | 36.61 | 0.242 | [0.832] | 0.7% |
| 6 | 36.91 | 0.722 | [0.871] | 2.0% |
| 7 + | 38.10 | 1.607 | [1.398] | 4.2% |
| | | | | |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 36.19 | -0.011 | [0.393] | 0.0% |
| **By age group** | | | | |
| 9 to 10 | 36.32 | -0.454 | [0.617] | -1.3% |
| 11 to 12 | 36.49 | 0.177 | [0.641] | 0.5% |
| 13 to 15 | 36.18 | -0.549 | [0.832] | -1.5% |
| **By grades of schooling 1997** | | | | |
| <=3 | 34.55 | -0.644 | [0.737] | -1.9% |
| 4 | 36.72 | -0.636 | [0.733] | -1.7% |
| 5 | 37.44 | 0.032 | [1.094] | 0.1% |
| 6 | 36.62 | 1.052 | [0.715] | 2.9% |
| 7 + | 39.53 | 0.72 | [1.377] | 1.8% |

Note: Estimates based on difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).

* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Figure 4.4. Proportion Working in 1997 and 2003 by age in 1997**

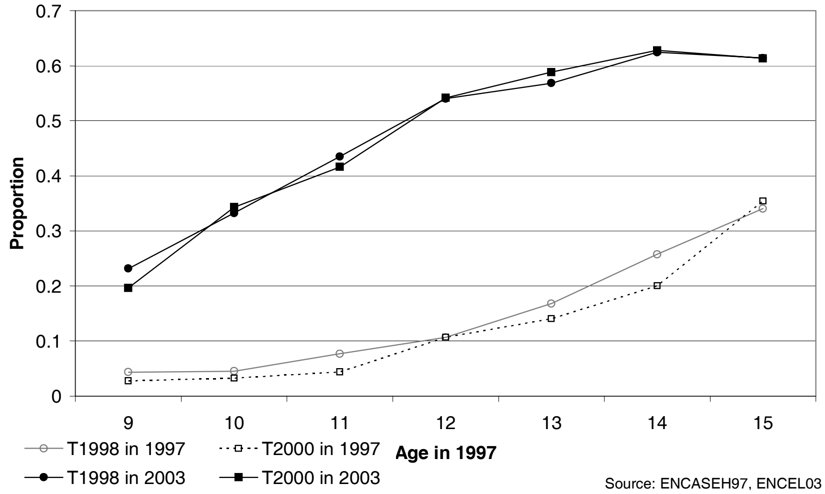
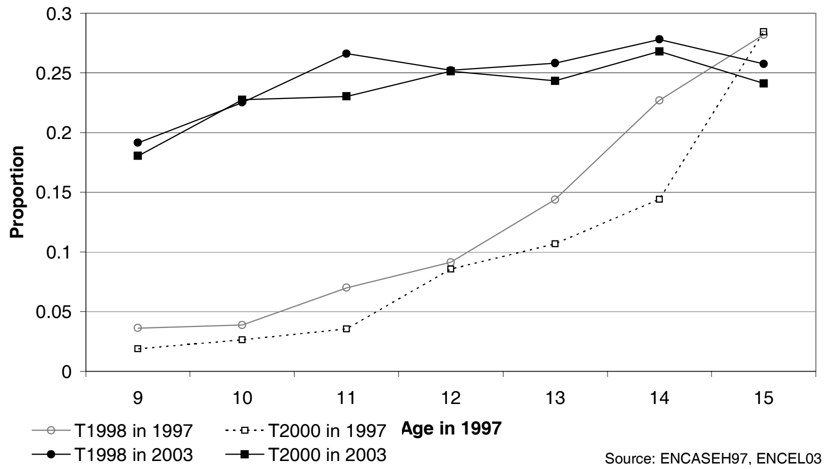


**Table 4.8. Impact of Differential Exposure to *Oportunidades* on Probability of Working:**
*Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | Proportion working in 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | **Coefficient** | **Std. error** | **% change relative to T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.26 | -0.013 | [0.013] | -5.0% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.14 | -0.008 | [0.019] | -5.6% |
| 11 to 12 | 0.34 | -0.010 | [0.024] | -2.9% |
| 13 to 15 | 0.40 | -0.020 | [0.025] | -5.0% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.18 | -0.010 | [0.020] | -5.5% |
| 4 | 0.24 | -0.016 | [0.031] | -6.8% |
| 5 | 0.25 | -0.032 | [0.033] | -12.7% |
| 6 | 0.34 | -0.006 | [0.034] | -1.8% |
| 7+ | 0.35 | 0.005 | [0.044] | 1.4% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.65 | -0.027 | [0.015]* | -4.1% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.40 | -0.015 | [0.024] | -3.8% |
| 11 to 12 | 0.67 | -0.007 | [0.026] | -1.0% |
| 13 to 15 | 0.83 | -0.046 | [0.025]* | -5.5% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.53 | -0.013 | [0.023] | -2.5% |
| 4 | 0.61 | 0.010 | [0.034] | 1.6% |
| 5 | 0.70 | -0.041 | [0.037] | -5.9% |
| 6 | 0.79 | 0.011 | [0.036] | 1.4% |
| 7 + | 0.85 | -0.136 | [0.041]*** | -15.9% |

Notes: Estimates based on difference-in-difference regression estimates. Controls for parental age, education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Figure 4.5. Proportion Working in Agriculture Sector in 1997 and 2003 by age**



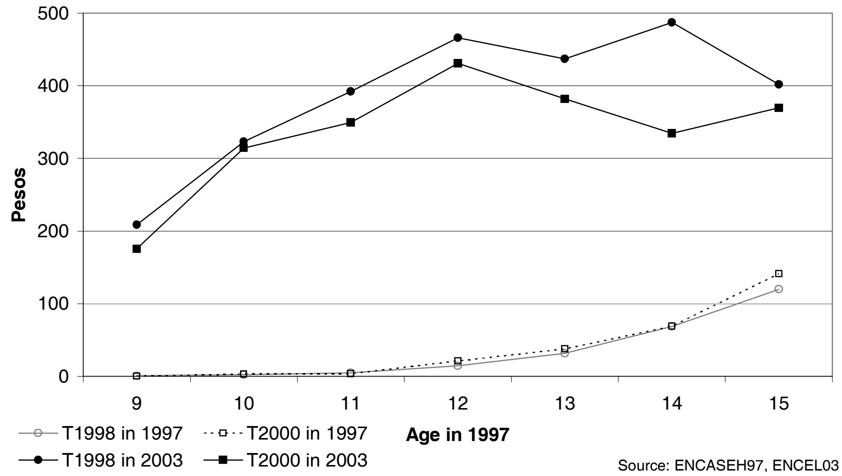

Source: ENCASEH97, ENCEL03

**Table 4.9. Impact of Differential Exposure to *Oportunidades* on Probability of Working in Agricultural Sector.** *Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | Proportion of agricultural workers in 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | **Coefficient** | **Std. error** | **% change relative to T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.049 | -0.008 | [0.008] | -16.4% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.059 | -0.016 | [0.013] | -27.0% |
| 11 to 12 | 0.039 | 0.008 | [0.014] | 20.5% |
| 13 to 15 | 0.047 | -0.014 | [0.015] | -29.6% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.074 | -0.025 | [0.015] | -33.9% |
| 4 | 0.048 | -0.007 | [0.018] | -14.6% |
| 5 | 0.021 | 0.014 | [0.018] | 66.3% |
| 6 | 0.032 | 0.002 | [0.020] | 6.2% |
| 7 + | 0.042 | -0.014 | [0.021] | -33.2% |
| | | | | |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.422 | -0.022 | [0.015] | -5.2% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.356 | -0.025 | [0.024] | -7.0% |
| 11 to 12 | 0.436 | 0.007 | [0.027] | 1.6% |
| 13 to 15 | 0.462 | -0.034 | [0.028] | -7.4% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.432 | -0.015 | [0.024] | -3.5% |
| 4 | 0.411 | 0.005 | [0.035] | 1.2% |
| 5 | 0.440 | -0.03 | [0.039] | -6.8% |
| 6 | 0.429 | 0.024 | [0.039] | 5.6% |
| 7 + | 0.376 | -0.084 | [0.046]* | -22.4% |

Notes: Estimates based on difference-in-difference regression estimates. Controls for parental age, education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Figure 4.6. Monthly Labor Income in 1997 and 2003 by age in 1997**



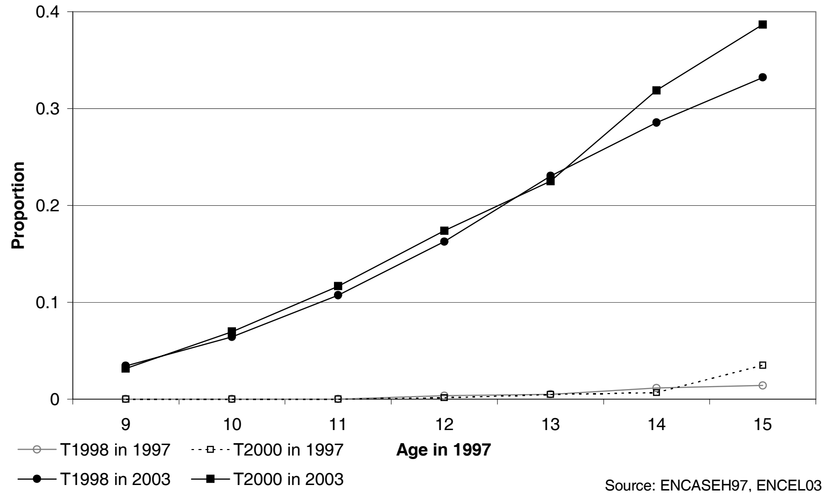

Source: ENCASEH97, ENCEL03

**Table 4.10. Impact of Differential Exposure to *Oportunidades* on Wages in Pesos per Month**
*Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | Monthly labor income of T2000 group | Impact | | |
|---|---|---|---|---|
| | | **Coefficient** | **Std. error** | **% change relative To T2000 group** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 154.36 | 38.951 | [15.458]** | 25.2% |
| **By age group in 1997** | | | | |
| 9 to 10 | 120.75 | 43.925 | [18.253]** | 36.4% |
| 11 to 12 | 160.37 | 20.019 | [30.857] | 12.5% |
| 13 to 15 | 179.03 | 50.375 | [36.295] | 28.1% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 100.81 | 60.774 | [21.230]*** | 60.3% |
| 4 | 148.87 | 24.7 | [32.136] | 16.6% |
| 5 | 164.95 | 31.639 | [42.019] | 19.2% |
| 6 | 215.15 | 10.396 | [46.827] | 4.8% |
| 7 + | 199.58 | 36.519 | [69.032] | 18.3% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 519.71 | -26.636 | [20.093] | -5.1% |
| **By age group in 1997** | | | | |
| 9 to 10 | 384.91 | -31.062 | [26.323] | -8.1% |
| 11 to 12 | 619.51 | -19.287 | [37.501] | -3.1% |
| 13 to 15 | 550.39 | -22.664 | [45.087] | -4.1% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 407.72 | -45.469 | [26.486]* | -11.2% |
| 4 | 570.54 | -25.701 | [46.436] | -4.5% |
| 5 | 610.92 | 38.267 | [58.640] | 6.3% |
| 6 | 604.71 | 32.605 | [63.351] | 5.4% |
| 7 + | 541.39 | -166.253 | [83.984]** | -30.7% |

Notes: Estimates based on difference-in-difference regression estimates. Controls for parental age, Education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Figure 4.7. Proportion Married in 1997 and 2003 by age in 1997**



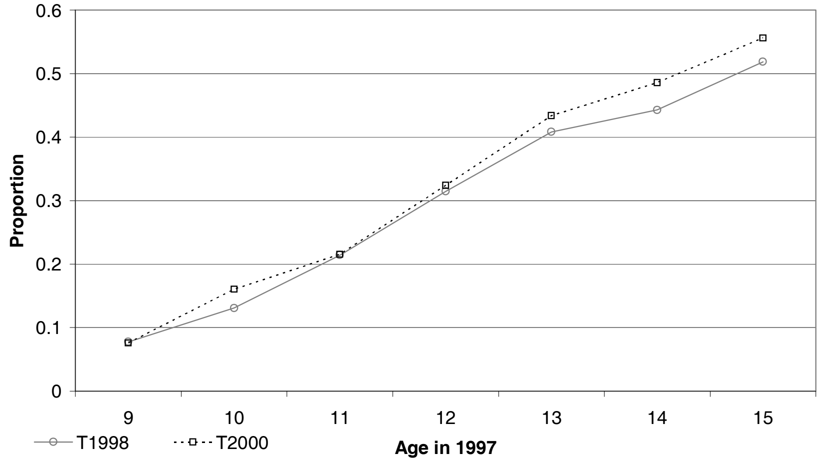

Source: ENCASEH97, ENCEL03

**Table 4.11. Impact of Differential Exposure to *Oportunidades* on Whether Married:**
*Difference-in-difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | Proportion married by 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | Coefficient | Std. error | % change relative to T2000 group |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.26 | -0.010 | [0.011] | -3.9% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.09 | -0.007 | [0.013] | -7.7% |
| 11 to 12 | 0.22 | -0.008 | [0.020] | -3.6% |
| 13 to 15 | 0.43 | -0.019 | [0.022] | -4.4% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.17 | -0.005 | [0.015] | -3.0% |
| 4 | 0.19 | -0.006 | [0.024] | -3.2% |
| 5 | 0.30 | -0.039 | [0.028] | -13.1% |
| 6 | 0.39 | -0.020 | [0.028] | -5.1% |
| 7 + | 0.33 | 0.046 | [0.039] | 13.9% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.10 | -0.006 | [0.007] | -6.2% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.01 | 0.008 | [0.006] | 60.0% |
| 11 to 12 | 0.07 | -0.014 | [0.011] | -20.3% |
| 13 to 15 | 0.19 | -0.013 | [0.016] | -6.8% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.05 | 0.012 | [0.009] | 25.8% |
| 4 | 0.19 | -0.024 | [0.014]* | -12.9% |
| 5 | 0.30 | -0.027 | [0.020] | -9.0% |
| 6 | 0.40 | 0.006 | [0.023] | 1.5% |
| 7 + | 0.21 | -0.053 | [0.028]* | -25.4% |

Notes: Estimates based on difference-in-difference regression estimates. Controls for parental age, education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

Figure 4.8. Proportion of Individuals Leaving Household in 2003 by age in 1997




Source: ENCASEH97, ENCEL03

**Table 4.12. Impact of Differential Exposure of *Oportunidades* on Whether Migrated Since 1997 (leaving HH of origin).** *Difference-in-difference Estimates: adolescents 9 to 15 in 1997* Treatment1998 versus Treatment2000.

| | Proportion migrating by 2003 of T2000 group | Impact | | |
|---|---|---|---|---|
| | | Coefficient | Std. error | % change relative to T2000 group |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.39 | -0.009 | [0.012] | -2.3% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.21 | -0.035 | [0.018]* | -16.7% |
| 11 to 12 | 0.36 | 0.021 | [0.023] | 5.8% |
| 13 to 15 | 0.57 | -0.014 | [0.022] | -2.5% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.28 | -0.034 | [0.019]* | -12.3% |
| 4 | 0.31 | 0.022 | [0.030] | 7.1% |
| 5 | 0.41 | 0.002 | [0.032] | 0.5% |
| 6 | 0.54 | -0.014 | [0.029] | -2.6% |
| 7 + | 0.55 | 0.031 | [0.040] | 5.6% |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.32 | -0.020 | [0.011]* | -6.2% |
| **By age group in 1997** | | | | |
| 9 to 10 | 0.12 | 0.014 | [0.015] | 12.1% |
| 11 to 12 | 0.30 | -0.025 | [0.021] | -8.4% |
| 13 to 15 | 0.51 | -0.051 | [0.021]** | -10.0% |
| **By grades of schooling completed by 1997** | | | | |
| <=3 | 0.18 | 0.008 | [0.016] | 4.6% |
| 4 | 0.28 | -0.037 | [0.026] | -13.0% |
| 5 | 0.36 | -0.033 | [0.031] | -9.2% |
| 6 | 0.46 | -0.030 | [0.030] | -6.5% |
| 7 + | 0.58 | -0.079 | [0.038]** | -13.5% |

Notes: Estimates based on difference-in-difference regression estimates. Controls for parental age, education, indigenous status, housing characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Table 4.13. Impact of Differential Exposure of *Oportunidades* on Grades of Schooling Completed: By type of secondary school available**

*Difference Estimates: Adolescents 9 to 15 in 1997*

T1998 versus T2000.

| | *Access only to telesecondary schools* | | *Access to general or technical schools* | |
|---|---|---|---|---|
| | **Coefficient** | **Std. error** | **Coefficient** | **Std. error** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.131 | [0.046]*** | 0.353 | [0.132]*** |
| **By age group** | | | | |
| 9 to 10 | 0.019 | [0.075] | 0.069 | [0.231] |
| 11 to 12 | 0.186 | [0.091]** | 0.349 | [0.261] |
| 13 to 15 | 0.176 | [0.073]** | 0.566 | [0.209]*** |
| **By grades of schooling 1997** | | | | |
| <=3 | 0.103 | [0.088] | 0.214 | [0.263] |
| 4 | -0.048 | [0.107] | 0.187 | [0.313] |
| 5 | 0.379 | [0.111]*** | 0.441 | [0.343] |
| 6 | 0.178 | [0.093]* | 0.635 | [0.251]** |
| 7 + | 0.08 | [0.105] | 0.238 | [0.319] |
| | | | | |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.162 | [0.044]*** | 0.25 | [0.122]** |
| **By age group** | | | | |
| 9 to 10 | 0.169 | [0.075]** | -0.092 | [0.184] |
| 11 to 12 | 0.186 | [0.085]** | 1.022 | [0.255]*** |
| 13 to 15 | 0.145 | [0.072]** | 0.178 | [0.200] |
| **By grades of schooling 1997** | | | | |
| <=3 | 0.118 | [0.085] | -0.067 | [0.225] |
| 4 | 0.247 | [0.102]** | 0.107 | [0.284] |
| 5 | 0.231 | [0.107]** | 0.186 | [0.288] |
| 6 | 0.242 | [0.099]** | 0.587 | [0.274]** |
| 7 + | 0.123 | [0.094] | 0.255 | [0.311] |

Note: Estimates based on difference in difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).

* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

**Table 4.14. Impact of Differential Exposure of *Oportunidades* on Grades of Schooling Completed: By student/teacher ratio in available secondary school pre-program.**
*Difference Estimates: Adolescents 9 to 15 in 1997*
T1998 versus T2000.

| | *Student/ teacher ratio <20 prior to program* | | *Student/ teacher ratio >=20 prior to program* | |
|---|---|---|---|---|
| | **Coefficient** | **Std. error** | **Coefficient** | **Std. error** |
| **Girls** | | | | |
| All girls 9 to 15 in 1997 | 0.183 | [0.061]*** | 0.120 | [0.052]** |
| **By age group** | | | | |
| 9 to 10 | 0.013 | [0.102] | 0.029 | [0.085] |
| 11 to 12 | 0.217 | [0.121]* | 0.184 | [0.100]* |
| 13 to 15 | 0.290 | [0.098]*** | 0.130 | [0.084] |
| **By grades of schooling 1997** | | | | |
| <=3 | 0.035 | [0.121] | 0.037 | [0.095] |
| 4 | 0.016 | [0.140] | 0.078 | [0.119] |
| 5 | 0.435 | [0.154]*** | 0.500 | [0.125]*** |
| 6 | 0.376 | [0.121]*** | 0.170 | [0.106] |
| 7 + | 0.142 | [0.133] | -0.055 | [0.134] |
| **Boys** | | | | |
| All boys 9 to 15 in 1997 | 0.250 | [0.122]** | 0.172 | [0.050]*** |
| **By age group** | | | | |
| 9 to 10 | -0.092 | [0.184] | 0.189 | [0.084]** |
| 11 to 12 | 1.022 | [0.255]*** | 0.087 | [0.095] |
| 13 to 15 | 0.178 | [0.200] | 0.228 | [0.082]*** |
| **By grades of schooling 1997** | | | | |
| <=3 | -0.067 | [0.225] | 0.185 | [0.092]** |
| 4 | 0.107 | [0.284] | 0.121 | [0.116] |
| 5 | 0.186 | [0.288] | 0.300 | [0.122]** |
| 6 | 0.587 | [0.274]** | 0.132 | [0.115] |
| 7 + | 0.255 | [0.311] | 0.327 | [0.114]*** |

Note: Estimates based on difference in difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).
* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.

*Youth 9 to 15 eligible for benefits in 1997*

| | All attritors | | Individual attrition[a] | | Household attrition[b] | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| T1998=1; T2000=0 | -0.004 | 0.001 | -0.021 | -0.073 | 0.016 | 0.046 |
| | [0.008] | [0.095] | [0.007]*** | [0.087] | [0.005]*** | [0.058] |
| *Interactions* | | | | | | |
| T1998*age | | -0.009 | | -0.002 | | -0.004 |
| | | [0.006] | | [0.005] | | [0.004] |
| T1998*gender | | -0.026 | | -0.014 | | -0.013 |
| | | [0.016] | | [0.014] | | [0.011] |
| T1998*indigenous | | -0.044 | | 0.039 | | -0.065 |
| | | [0.034] | | [0.031] | | [0.018]*** |
| T1998*schooling | | 0.005 | | -0.002 | | 0.008 |
| | | [0.006] | | [0.005] | | [0.004]* |
| T1998*enrolled | | 0.058 | | 0.044 | | 0.002 |
| | | [0.048] | | [0.042] | | [0.030] |
| T1998*father education | | 0.003 | | 0.011 | | -0.008 |
| | | [0.004] | | [0.004]*** | | [0.003]*** |
| T1998*father age | | 0.002 | | 0.001 | | 0.001 |
| | | [0.001] | | [0.001] | | [0.001] |
| T1998*father indigenous. | | 0.172 | | 0.018 | | 0.166 |
| | | [0.065]*** | | [0.055] | | [0.061]*** |
| T1998*father bilingual | | -0.112 | | -0.05 | | -0.049 |
| | | [0.052]** | | [0.044] | | [0.032] |
| T1998* mother education | | -0.004 | | -0.003 | | -0.001 |
| | | [0.004] | | [0.004] | | [0.003] |
| T1998*mother age | | -0.001 | | -0.002 | | 0 |
| | | [0.001] | | [0.001] | | [0.001] |
| T1998* mother indigenous | | -0.055 | | -0.014 | | -0.035 |
| | | [0.047] | | [0.041] | | [0.029] |
| T1998 *mother bilingual | | 0.067 | | 0.024 | | 0.043 |
| | | [0.037]* | | [0.032] | | [0.028] |
| T1998*rooms | | 0.014 | | 0.009 | | 0.003 |
| | | [0.007]** | | [0.005]* | | [0.006] |
| T1998*electricity | | -0.016 | | 0.007 | | -0.022 |
| | | [0.018] | | [0.016] | | [0.012]* |
| T1998*water | | 0.011 | | 0.028 | | -0.015 |
| | | [0.019] | | [0.017]* | | [0.012] |
| T1998*dirt floor | | -0.016 | | 0.004 | | -0.017 |
| | | [0.019] | | [0.017] | | [0.013] |
| Observations | 16257 | 16117 | 16257 | 16117 | 16257 | 16117 |

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
[a]Individual attrition refers to individuals who attrit but original HH stays in 2003 sample
[b]Household attrition refers to individuals attriting because entire HH attrits from 2003 sample.

**Table B.2. Probability of attriting between 1997 and 2003 as a function of characteristics in 1997:** *Boys 9 to 15 in 1997 eligible for benefits in 1997*

| | All attritors | | Individual attrition[a] | | Household attrition[b] | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| T1998=1; T2000=0 | -0.013 | 0.016 | -0.026 | 0.007 | 0.013 | -0.016 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | - | - | - | - | - | - |
| *Interactions* | | | | | | |
| T1998*age | | -0.008 | | -0.005 | | 0 |
| | | [0.008] | | [0.006] | | [0.005] |
| T1998*indigenous | | -0.057 | | 0.011 | | -0.055 |
| | | [0.046] | | [0.039] | | [0.027]** |
| T1998*schooling | | -0.002 | | -0.006 | | 0.004 |
| | | [0.008] | | [0.006] | | [0.005] |
| T1998*enrolled | | 0.059 | | 0.032 | | 0.018 |
| | | [0.067] | | [0.056] | | [0.042] |
| T1998*father education | | 0.003 | | 0.013 | | -0.01 |
| | | [0.006] | | [0.005]*** | | [0.004]** |
| T1998*father age | | 0 | | -0.001 | | 0 |
| | | [0.002] | | [0.002] | | [0.001] |
| T1998*father indigenous | | 0.121 | | -0.043 | | 0.181 |
| | | [0.089] | | [0.064] | | [0.085]** |
| T1998*father bilingual | | -0.06 | | 0.007 | | -0.055 |
| | | [0.073] | | [0.064] | | [0.043] |
| T1998* mother education | | 0 | | -0.001 | | 0.002 |
| | | [0.006] | | [0.005] | | [0.004] |
| T1998*mother age | | 0 | | 0 | | 0.001 |
| | | [0.002] | | [0.002] | | [0.001] |
| T1998* mother indigenous | | -0.023 | | 0.032 | | -0.044 |
| | | [0.065] | | [0.057] | | [0.039] |
| T1998 *mother bilingual | | 0.02 | | -0.016 | | 0.036 |
| | | [0.049] | | [0.039] | | [0.038] |
| T1998*rooms | | 0.012 | | 0.009 | | 0.001 |
| | | [0.009] | | [0.007] | | [0.008] |
| T1998*electricity | | -0.029 | | 0.002 | | -0.027 |
| | | [0.025] | | [0.021] | | [0.017] |
| T1998*water | | 0.049 | | 0.071 | | -0.018 |
| | | [0.027]* | | [0.024]*** | | [0.016] |
| T1998*dirt floor | | -0.028 | | -0.003 | | -0.024 |
| | | [0.027] | | [0.022] | | [0.017] |
| Observations | 8384 | 8311 | 8384 | 8311 | 8384 | 8311 |

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
[a]Individual attrition refers to individuals who attrit but original HH stays in 2003 sample
[b]Household attrition refers to individuals attriting because entire HH attrits from 2003 sample.

**Table B.3. Probability of attriting between 1997 and 2003 as a function of characteristics in 1997:** *Girls 9 to 15 in 1997 eligible for benefits in 1997*

| | All attritors (1) | All attritors (2) | Individual attrition[a] (1) | Individual attrition[a] (2) | Household attrition[b] (1) | Household attrition[b] (2) |
|---|---|---|---|---|---|---|
| T1998=1; T2000=0 | 0.006 | -0.016 | -0.013 | -0.142 | 0.019 | 0.095 |
| | [0.011] | [0.139] | [0.011] | [0.131] | [0.008]** | [0.078] |
| | | [0.057] | | [0.054] | | [0.031] |
| *Interactions* | | | | | | |
| T1998*age | | -0.012 | | 0 | | -0.009 |
| | | [0.008] | | [0.007] | | [0.005]* |
| T1998*indigenous | | -0.019 | | 0.075 | | -0.071 |
| | | [0.050] | | [0.047] | | [0.024]*** |
| T1998*schooling | | 0.014 | | 0.004 | | 0.011 |
| | | [0.009] | | [0.008] | | [0.006]** |
| T1998*enrolled | | 0.047 | | 0.051 | | -0.019 |
| | | [0.071] | | [0.063] | | [0.044] |
| T1998*father education | | 0.002 | | 0.008 | | -0.005 |
| | | [0.006] | | [0.006] | | [0.004] |
| T1998*father age | | 0.005 | | 0.004 | | 0.001 |
| | | [0.002]** | | [0.002]** | | [0.001] |
| T1998*father indigenous | | 0.227 | | 0.092 | | 0.149 |
| | | [0.092]** | | [0.089] | | [0.088]* |
| T1998*father bilingual | | -0.173 | | -0.11 | | -0.045 |
| | | [0.073]** | | [0.061]* | | [0.047] |
| T1998* mother education | | -0.01 | | -0.005 | | -0.005 |
| | | [0.006] | | [0.006] | | [0.004] |
| T1998*mother age | | -0.004 | | -0.004 | | 0 |
| | | [0.002]* | | [0.002]* | | [0.001] |
| T1998* mother indigenous | | -0.099 | | -0.067 | | -0.03 |
| | | [0.069] | | [0.059] | | [0.042] |
| T1998 *mother bilingual | | 0.119 | | 0.07 | | 0.054 |
| | | [0.053]** | | [0.050] | | [0.041] |
| T1998*rooms | | 0.014 | | 0.006 | | 0.006 |
| | | [0.011] | | [0.008] | | [0.009] |
| T1998*electricity | | -0.002 | | 0.013 | | -0.016 |
| | | [0.027] | | [0.024] | | [0.017] |
| T1998*water | | -0.025 | | -0.012 | | -0.011 |
| | | [0.027] | | [0.024] | | [0.017] |
| T1998*dirt floor | | -0.002 | | 0.014 | | -0.011 |
| | | [0.028] | | [0.026] | | [0.018] |
| | 7870 | 7806 | 7870 | 7806 | 7870 | 7806 |

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

[a]Individual attrition refers to individuals who attrit but original HH stays in 2003 sample

[b]Household attrition refers to individuals attriting because entire HH attrits from the 2003 sample.

**Table C.1. Differential exposure to Oportunidades:** Households receiving benefits in 1998 versus 2000

| | Pre-program: 97 | | After program: 03 | | P>\|Z\|, \|T\| | | P>\|Z\|, \|T\| |
|---|---|---|---|---|---|---|---|
| | T98 | T00 | T98 | T00 | Pre-program difference among T98 vs. T00 | Difference in Difference | |
| | Mean values | | Mean values | | | | |
| **School enrollment[1]** | | | | | | | |
| Boys 9 to 15 in 1997 | 0.821 | 0.807 | 0.248 | 0.240 | 0.182 | -0.007 | 0.838 |
| Girls 9 to 15 in 1997 | 0.773 | 0.757 | 0.257 | 0.260 | 0.085 | -0.018 | 0.129 |
| **Grades of schooling completed** | | | | | | | |
| Boys 9 to 15 in 1997 | 4.514 | 4.513 | 7.740 | 7.520 | 0.967 | 0.219 | 0.000 |
| Girls 9 to 15 in 1997 | 4.580 | 4.610 | 7.680 | 7.510 | 0.568 | 0.200 | 0.000 |
| **Progressing on time** | | | | | | | |
| Boys 9 to 15 in 1997 | | | 0.340 | 0.312 | | 0.028 | 0.004 |
| Girls 9 to 15 in 1997 | | | 0.321 | 0.308 | | 0.014 | 0.165 |
| Achievement test scores | | | | | | | |
| Boys 9 to 15 in 1997 | | | | | | | |
| Girls 9 to 15 in 1997 | | | | | | | |
| **Employment[2]** | | | | | | | |
| Boys 9 to 15 in 1997 | 0.179 | 0.164 | 0.631 | 0.654 | 0.040 | -0.039 | 0.001 |
| Girls 9 to 15 in 1997 | 0.078 | 0.054 | 0.270 | 0.259 | 0.000 | -0.013 | 0.226 |
| **Proportion working in agric. Sector** | | | | | | | |
| Boys 9 to 15 in 1997 | 0.160 | 0.137 | 0.441 | 0.458 | 0.002 | -0.040 | 0.011 |
| Girls 9 to 15 in 1997 | 0.044 | 0.027 | 0.070 | 0.064 | 0.000 | -0.010 | 0.061 |
| **Average monthly labor income[3]** | | | | | | | |
| Boys 9 to 15 in 1997 | 33.341 | 46.213 | 805.815 | 798.483 | 0.260 | 20.203 | 0.421 |
| Girls 9 to 15 in 1997 | 12.609 | 11.561 | 330.703 | 286.561 | 0.832 | 43.094 | 0.079 |
| **Marriage[4]** | | | | | | | |
| Boys 9 to 15 in 1997 | 0.002 | 0.002 | 0.087 | 0.096 | 0.868 | -0.009 | 0.161 |
| Girls 9 to 15 in 1997 | 0.007 | 0.014 | 0.230 | 0.252 | 0.077 | -0.015 | 0.060 |
| **Migration[5]** | | | | | | | |
| Boys 9 to 15 in 1997 | | | 0.313 | 0.339 | | -0.027 | 0.008 |
| Girls 9 to 15 in 1997 | | | 0.377 | 0.387 | | -0.010 | 0.361 |

Notes: 1. Proportion currently enrolled; 2. Proportion currently working; 3. Pesos; 4. Proportion currently married or co-habitating; 5. Proportion of individuals leaving household.
Sample includes all program-eligible individuals aged 9 to 15 in 1997 who are also interviewed in 2003. All coefficients are weighted to correct for differential attrition (see Section 3 and Appendix B).
t-tests are used to test for 1997 (pre-program) differences in the means between T1998 and T2000 (column 5 gives the levels at which the mean differences in 1997 are significant) and for differences in the changes in the means between 1997 and 2003 for T1998 versus T2000 (column 7gives the levels at which the difference-in-differences in column 6 are significant.) There are no entrees in columns 1, 2 and 5 for progressing on time and migration because both variables refer to changes between 1997 and 2003, not to states in 1997. The differences in each of these variables in 2003, thus, still are difference-in-difference estimates.

## Appendix C: Woodcock Johnson Achievement Tests

**Appendix Table C.1: The relationship between Woodcock Johnson achievement tests and grades of schooling**

*Program eligible youth aged 15 to 21 in 2003*

Dependent variable: raw score

| | Reading | Reading | Math | Math | Writing | Writing |
|---|---|---|---|---|---|---|
| Total grades of schooling | 1.311 | | 1.05 | | 1.416 | |
| | [0.063]*** | | [0.047]*** | | [0.052]*** | |
| 1 grade of schooling | | -5.451 | | 0.139 | | 1.748 |
| | | [2.269]** | | [1.729] | | [2.013] |
| 2 grades | | 3.316 | | 2.683 | | 1.034 |
| | | [1.636]** | | [1.242]** | | [1.402] |
| 3 grades | | 3.477 | | 1.714 | | 1.7 |
| | | [1.313]*** | | [0.987]* | | [1.098] |
| 4 grades | | 4.302 | | 0.579 | | 1.667 |
| | | [1.386]*** | | [1.045] | | [1.170] |
| 5 grades | | 8.186 | | 2.422 | | 3.183 |
| | | [1.328]*** | | [1.002]** | | [1.122]*** |
| 6 grades | | 12.325 | | 4.419 | | 6.023 |
| | | [0.963]*** | | [0.723]*** | | [0.807]*** |
| 7 grades | | 11.697 | | 4.432 | | 7.7 |
| | | [1.193]*** | | [0.897]*** | | [0.998]*** |
| 8 grades | | 13.287 | | 5.522 | | 9.579 |
| | | [1.071]*** | | [0.806]*** | | [0.897]*** |
| 9 grades | | 14.493 | | 7.284 | | 10.816 |
| | | [0.954]*** | | [0.716]*** | | [0.799]*** |
| 10 grades | | 15.255 | | 10.463 | | 13.265 |
| | | [1.098]*** | | [0.826]*** | | [0.919]*** |
| 11 grades | | 15.52 | | 11.578 | | 13.833 |
| | | [1.110]*** | | [0.836]*** | | [0.930]*** |
| 12 or more grades | | 13.802 | | 9.339 | | 12.4 |
| | | [1.089]*** | | [0.818]*** | | [0.911]*** |
| Age | 0.022 | 0.045 | 0.036 | 0.029 | 0.013 | 0.019 |
| | [0.031] | [0.031] | [0.023] | [0.024] | [0.026] | [0.026] |
| Gender | -0.548 | -0.443 | 0.829 | 0.852 | -0.391 | -0.416 |
| | [0.302]* | [0.301] | [0.228]*** | [0.228]*** | [0.249] | [0.252]* |
| Indigenous | -3.648 | -3.789 | -3.622 | -3.927 | -4.674 | -4.849 |
| | [1.048]*** | [1.048]*** | [0.792]*** | [0.795]*** | [0.876]*** | [0.888]*** |
| Indigenous, speaks Spanish | 4.251 | 3.962 | 2.632 | 2.827 | 3.599 | 3.58 |
| | [0.922]*** | [0.921]*** | [0.700]*** | [0.701]*** | [0.773]*** | [0.783]*** |
| Father education | -0.001 | 0.061 | 0.138 | 0.125 | 0.105 | 0.114 |
| | [0.078] | [0.078] | [0.059]** | [0.059]** | [0.065] | [0.065]* |
| Father age | 0.049 | 0.037 | 0.016 | 0.002 | 0.041 | 0.029 |
| | [0.025]** | [0.024] | [0.019] | [0.018] | [0.020]** | [0.020] |
| Father indigenous | -2.381 | -1.198 | -0.783 | -0.257 | -1.646 | -1.068 |
| | [1.264]* | [1.260] | [0.968] | [0.969] | [1.057] | [1.069] |
| Father spanish | 1.977 | 1.613 | 0.599 | 0.35 | 1.898 | 1.715 |
| | [1.158]* | [1.155] | [0.890] | [0.892] | [0.971]* | [0.983]* |
| Mother education | 0.04 | 0.162 | 0.193 | 0.205 | 0.223 | 0.246 |
| | [0.079] | [0.079]** | [0.060]*** | [0.059]*** | [0.065]*** | [0.066]*** |
| Mother age | -0.053 | -0.024 | 0.003 | 0.017 | -0.011 | 0.009 |
| | [0.027]* | [0.027] | [0.021] | [0.020] | [0.023] | [0.022] |
| Mother indigenous | -0.567 | -0.438 | 0.711 | 0.628 | 0.404 | 0.281 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | - - | - - | - - | - - | - - | - - |
| Mother spanish | 0.124 | -0.042 | -0.481 | -0.601 | 0.113 | 0.106 |

**Appendix Table C.1: Continues**

| | | | | | | |
|---|---|---|---|---|---|---|
| | [0.794] | [0.792] | [0.598] | [0.598] | [0.655] | [0.663] |
| Rooms | -0.036 | -0.032 | -0.039 | -0.036 | 0.002 | 0.001 |
| | [0.053] | [0.052] | [0.039] | [0.040] | [0.043] | [0.044] |
| Electricity | 0.582 | 0.768 | 0.555 | 0.558 | 0.944 | 0.919 |
| | [0.373] | [0.372]** | [0.282]** | [0.282]** | [0.308]*** | [0.311]*** |
| Water | -0.7 | -0.731 | 0.245 | 0.34 | 0.146 | 0.249 |
| | [0.328]** | [0.326]** | [0.247] | [0.246] | [0.270] | [0.272] |
| Dirt floor | -0.081 | -0.115 | -0.185 | -0.216 | 0.113 | 0.021 |
| | [0.333] | [0.330] | [0.250] | [0.250] | [0.274] | [0.276] |
| R-squared | 0.2 | 0.22 | 0.25 | 0.26 | 0.32 | 0.31 |

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

**Appendix Table C.2. Probability of being in youth sample taking Woodcock Johnson achievement tests in ENCEL2003 as a function of characteristics in 1997.**

*All youth aged 9 to 15 in 1997*

| | All youth | | Girls | | Boys | |
|---|---|---|---|---|---|---|
| T1998=1, T2000=0 | -0.055 | | -0.055 | | -0.054 | |
| | [0.006]*** | | [0.009]*** | | [0.008]*** | |
| T1998*age | | 0.003 | | 0.003 | | 0.006 |
| | | [0.002] | | [0.003] | | [0.003]* |
| T1998*gender | | 0.003 | | | | |
| | | [0.011] | | | | |
| T1998*indigenous | | 0.012 | | -0.016 | | 0.031 |
| | | [0.026] | | [0.036] | | [0.036] |
| T1998*schooling | | -0.007 | | -0.01 | | -0.005 |
| | | [0.003]** | | [0.005]** | | [0.004] |
| T1998*enrolled | | -0.014 | | -0.001 | | -0.02 |
| | | [0.016] | | [0.023] | | [0.023] |
| T1998*father education | | -0.006 | | -0.002 | | -0.008 |
| | | [0.003]** | | [0.004] | | [0.004]** |
| T1998*father age | | 0 | | -0.001 | | 0 |
| | | [0.001] | | [0.001] | | [0.001] |
| T1998 father indigenous | | -0.032 | | -0.09 | | 0.022 |
| | | [0.046] | | [0.059] | | [0.070] |
| T1998*father bilingual | | 0.043 | | 0.036 | | -0.02 |
| | | [0.050] | | [0.054] | | [0.057] |
| T1998*mother education | | 0.003 | | 0.005 | | 0.002 |
| | | [0.003] | | [0.004] | | [0.004] |
| T1998*mother age | | 0 | | 0.001 | | -0.002 |
| | | [0.001] | | [0.002] | | [0.001] |
| T1998*mother indigenous | | 0.038 | | 0.106 | | -0.015 |
| | | [0.039] | | [0.066] | | [0.046] |
| T1998* mother bilingual | | -0.032 | | -0.085 | | 0.022 |
| | | [0.023] | | [0.029]*** | | [0.037] |
| T1998* rooms | | 0.001 | | 0.002 | | 0.002 |
| | | [0.002] | | [0.003] | | [0.003] |
| T1998*electricity | | 0.007 | | 0.022 | | -0.004 |
| | | [0.013] | | [0.020] | | [0.018] |
| T1998* water | | 0.018 | | 0.012 | | 0.022 |
| | | [0.014] | | [0.020] | | [0.019] |
| T1998*dirt floor | | 0.015 | | 0.029 | | 0.003 |
| | | [0.013] | | [0.020] | | [0.018] |
| Observations | 16435 | 16179 | 7968 | 7852 | 8463 | 8327 |

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%
For each dependent variable, we have estimated two specifications: (1) only whether in T1998 group and (2) whether in T1998 group plus interactions between being in the T1998 group and pre-program individual characteristics, parental characteristics and housing characteristics.

**Appendix Table C.3. Impact of Differential Exposure of *Oportunidades* on Grades of Schooling**

*Adolescents 9 to 15 in 1997 taking the Woodcock Johnson achievement tests*

T1998 versus T2000.

| | Impact | |
|---|---|---|
| | **Coefficient** | **Std. error** |
| **Girls** | | |
| All girls 9 to 15 in 1997 | 0.067 | [0.121] |
| **By age group** | | |
| 9 to 10 | -0.064 | [0.155] |
| 11 to 12 | -0.118 | [0.191] |
| 13 to 15 | 0.298 | [0.205] |
| **By grades of schooling 1997** | | |
| <=3 | -0.076 | [0.200] |
| 4 | -0.052 | [0.214] |
| 5 | 0.206 | [0.269] |
| 6 | 0.37 | [0.253] |
| 7 + | -0.141 | [0.298] |
| | | |
| **Boys** | | |
| All boys 9 to 15 in 1997 | 0.212 | [0.113]* |
| **By age group** | | |
| 9 to 10 | 0.322 | [0.137]** |
| 11 to 12 | 0.141 | [0.189] |
| 13 to 15 | 0.205 | [0.230] |
| **By grades of schooling 1997** | | |
| <=3 | 0.392 | [0.173]** |
| 4 | 0.303 | [0.222] |
| 5 | -0.119 | [0.252] |
| 6 | 0.107 | [0.274] |
| 7 + | 0.493 | [0.294]* |

Note: Estimates based on difference regression estimates. Controls for parental age, education, indigenous status, household characteristics (number of rooms, electricity, type of floor and water/sewage system).

* indicates significance for a t-test at the 10% level. ** at the 5% level *** at the 1% level.