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

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7.1 Introduction

Governments throughout Latin 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, formerly 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. As of 2006, 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 spring 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]

With the availability of the 2003 follow-up rural evaluation survey ENCEL2003, it is now possible to assess directly some important

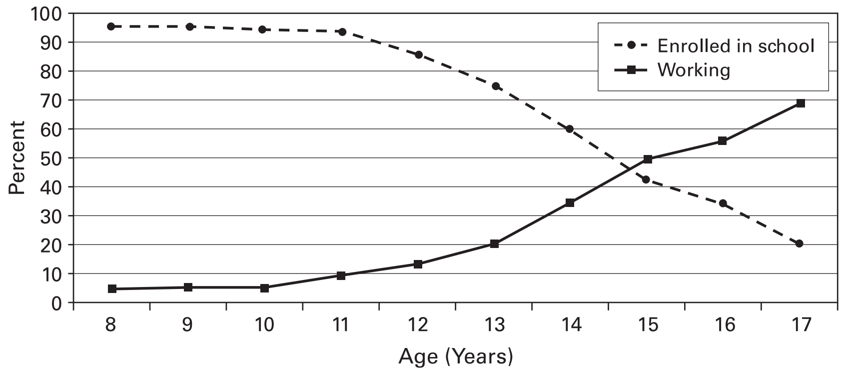

Figure 7.1
School enrollment and labor force participation of boys in *Oportunidades* communities prior to program implementation.
Source: Parker and Skoufias (2000).

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. 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 affected 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. Figures 7.1 and 7.2 illustrate 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-

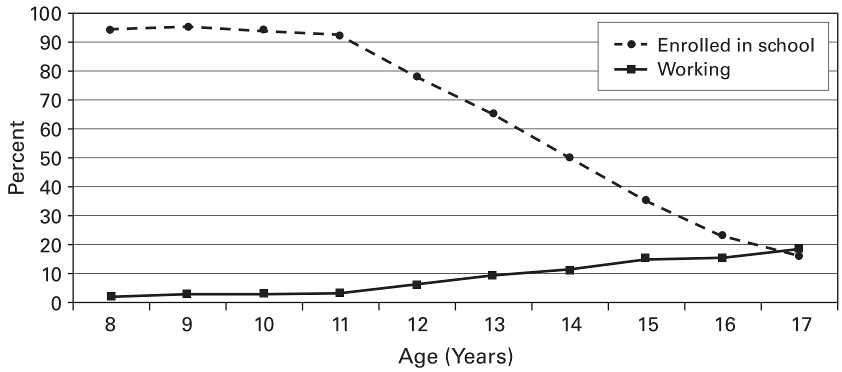

Figure 7.2
School enrollment and labor force participation of girls in *Oportunidades* communities prior to program implementation.
Source: Parker and Skoufias (2000).

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 preprogram Survey of Household Socioeconomic Characteristics (ENCASEH) data. We also link the household-level data to school-level data on characteristics that reflect school quality from the Ministry of Education.

As noted, the treatment and control villages were originally chosen by a randomized experimental design. Over time, however, attrition—mainly due to migration for work, schooling, or marriage—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 7.3) 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. They are missing in cases where the entire household left the village.

Our impact estimates reveal significant positive impacts of differential exposure (1.5 years difference) 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 program 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, perhaps surprisingly, do not reveal any positive and significant impacts on achievement scores. 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 for the part of the life cycle considered in this paper. 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.

Finally, we find that the program has a statistically significant impact on 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.

The paper is organized as follows. Section 7.2 provides a brief description of the features of the Oportunidades program. Section 7.3 describes the basic sample design, the data, and the econometric method used to control for nonrandom attrition/migration. Sections 7.4 and 7.5 present the empirical results and section 7.6 concludes.

Table 7.1
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

7.2 Program Background

Oportunidades began operating in 1997 in small rural communities in Mexico. The program has gradually expanded into urban areas and by 2006 covered about one-quarter of all families in Mexico. Table 7.1 shows the monthly grant levels available for children between the third and twelfth grades in the second semester of 2003. Originally, the program provided grants only for children between the third and ninth grades. In 2001, however, the grants were extended to high school. The grant amounts are slightly higher (by about 13 percent) for girls than boys in secondary (seventh to ninth grade) and high school (tenth to twelfth grade). 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 funds, where eligibility is determined on the basis of a marginality index designed to identify the poorest families within each community.[8] In rural areas, program administrators visited all households in each village and, after collecting some screening 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 in rural areas 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 schooling grants, but excludes primary school scholarships and cash transfers for food. In practice, however, very few (less than 50) households in our sample of interest transitioned to the modified version of the program by 2003. For the analysis of this paper, we concentrate on those households initially eligible for the full program.[9]

7.3 Sample Design, the Data, and Attrition

7.3.1 Sample Design

The 2003 Rural Evaluation Survey continues the original treatment and control experimental design begun in 1997. As noted, the original sample design involved 506 communities, of which 320 were 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 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.[11] Between 1997 and 2000, evaluation surveys with detailed information on many 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 datasets, particularly the preprogram 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 following analysis, 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.

7.3.2 Attrition of Youth in the Original T1998 and T2000 Households

We now consider program attrition in 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 percent and despite significant differences between those reinterviewed and those lost to follow-up for many important characteristics. Fitzgerald, Gottschalk, and Moffitt (1998) summarize:

> By 1989 the Michigan Panel Study on Income Dynamics (PSID) had experienced approximately 50 percent sample loss from cumulative attrition from its initial 1968 membership . . . (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) (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" . . . (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. Similar results are presented for three developing country longitudinal datasets in Alderman et al. (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 it does 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.[12] As noted in the introduction, parents were asked questions about children who left the family, so for many of the outcomes (such as grades of schooling), data are available despite the child having left the household.

Table 7.2 (panel A) summarizes some statistics regarding sample attrition in this period for the original treatment (T1998) and original control (T2000) groups, focusing on all youth eligible for the program under the original program definition (pobre).[13] The numbers in this table are striking. Two-fifths (41 percent) 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 attainment, effective attrition is less than 20 percent because information on outcomes is provided by the parents or other informants. Also, the overall attrition rates do not differ significantly 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 percent) than for boys (36 percent), 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. Total attrition includes individuals who have separated from households that are still in the sample in 2003 (table 7.2, panel B) and individuals from households that are no longer in the sample in 2003 (table 7.2, panel C).

Table 7.2
Proportion attriting by 2003 from original ENCASEH: individuals 9 to 15 in 1997

	Treatment		Control		
	(T1998)		(T2000)		
	N	Mean	N	Mean	P > |Z|
Total proportion attriting (individual or household)					
9 to 15 years	10,102	0.388	6,155	0.392	0.563
Boys	5,269	0.355	3,115	0.368	0.231
Girls	4,831	0.422	3,039	0.417	0.644
Proportion due to individual attrition					
9 to 15 years		0.246		0.267	0.003
Boys		0.213		0.239	0.006
Girls		0.282		0.296	0.181
Proportion due to household attrition (individual not found because household moves)					
9 to 15 years		0.141		0.125	0.003
Boys		0.142		0.129	0.092
Girls		0.140		0.120	0.014

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

About 62 percent of those lost to follow-up are individuals who left households that stayed in the sample.[14] There are some significant differences at the 5 percent 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 versus T2000 attrition differences are not significant, 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 preprogram individual characteristics, parental characteristics, and housing characteristics. We performed this estimation for boys and girls together and separately. Appendix tables 7.12 and 7.13 tabulate the estimates. The first specification (column 1), not surprisingly, replicates the patterns noted with regard to table 7.2. Specification (2) indicates that a number of the preprogram 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 attrition into account in generating program impact estimates.

7.3.3 Method Used to Account for Attrition in Estimation of Program Impacts

To describe the method, we first 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 or 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 postprogram 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.

In the absence of the attrition, we can simply exploit 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:

$$\Delta_1 = E(Y_1|R=1, A=0) - E(Y_0|R=0, A=0)$$
$$= E(Y_1|R=1, A=0) - E(Y_0|R=1, A=0)$$
$$+ \quad E(Y_0|R=1, A=0) - E(Y_0|R=0, A=0).$$

Δ_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:

$$(Y_1, Y_0) \perp\!\!\!\perp A|X, R \tag{7.1}$$

and that

$$0 < \Pr(A = 1|X, R) < 1. \tag{7.2}$$

Condition (7.2) ensures that we do not lose all individuals with characteristics X to attrition.

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

$$Y_0 \perp\!\!\!\perp R|X \tag{7.3}$$

and

$$0 < \Pr(R = 1|X) < 1. \tag{7.4}$$

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 (7.1) gives

$$E(Y_0|R=0, A=0, X) = E(Y_0|R=0, X)$$

$$E(Y_1|R=1, A=0, X) = E(Y_1|R=1, X)$$

and (7.3) gives

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

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

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

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

$$\int E(Y_1|R=1,A=0,X)\frac{f(X|R=1)f(X|R=1,A=0)}{f(X|R=1,A=0)}dX$$

$$-\int E(Y_0|R=0,A=0,X)\frac{f(X|R=1)f(X|R=0,A=0)}{f(X|R=0,A=0)}dX,$$

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 attrition into account is

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

where $\hat{W}_i = \hat{f}(X_i|R=1)/\hat{f}(X_i|R=1,A=0)$ is a weight applied to each nonattriting member of the treatment group and $\hat{W}_j = \hat{f}(X_j|R=0)/\hat{f}(X_j|R=0,A=0)$ is a weight applied to each nonattriting member of the control group. The weights adjust for differences in the distribution of the X characteristics between the treatment and control groups that can arise 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 (7.1) and (7.2) imply

$$Y_0 \perp\!\!\!\perp A \,|\, \Pr(A=1|X,R) \tag{7.3}$$

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

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

where n_1 and n_0 are the number of individuals in the treatment and control groups. The weights are $\hat{W}_i = \hat{f}(P_i|R=1)/\hat{f}(P_i|R=0, A=0)$ and $\hat{W}_j = \hat{f}(P_j|R=0)/\hat{f}(P_j|R=0, A=0)$ where $P_i = \Pr(A_i = 1|X_i, R_i)$, which we estimate by a probit model. Through this procedure, each individual observed postprogram receives a weight equal to the ratio of the density of his/her P_j with respect to the preprogram (and preattrition) distribution (of treatments or controls) divided by the density estimated with respect to the postprogram distribution. Effectively, this procedure reweights the postprogram 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 randomly conditional on X, within each of the groups.[15]

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 earlier, 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. Results from the probit regressions used to derive the weights are provided in appendix tables 7.12 and 7.13.

7.3.4 Woodcock-Johnson Achievement Tests

As part of the ENCEL2003 fieldwork, Woodcock-Johnson achievement tests (WJ) in the areas of reading, math, and written language skills were given to a subsample of adolescents 15 to 21 years of age in 2003. The Woodcock-Johnson is one of the principal test series used to measure achievement in the United States and is very commonly administered. 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.[16]

Three tests were applied. Test 22 of the Woodcock-Johnson tests is Letter-Word Identification (reading), consisting of showing test-takers various pictures, letters, and progressively harder words where the examinee is asked to say what is in the picture, 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,

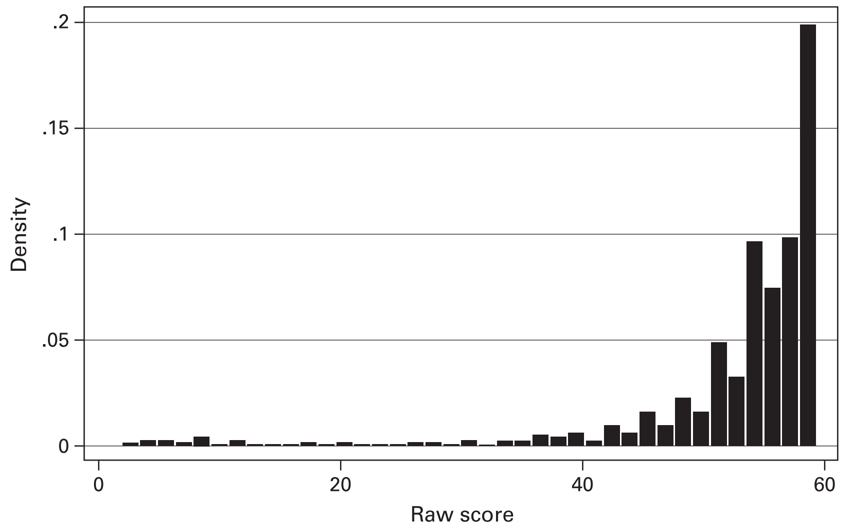

Figure 7.3
Distribution of WJ raw scores: Reading.

Applied Problems, tests the subject's skills in solving practical problems. The test begins with such activities 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 test where the examiner reads aloud letters and words and the examinee must write them down correctly.

Figures 7.3 through 7.5 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 program impacts could be observed. 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

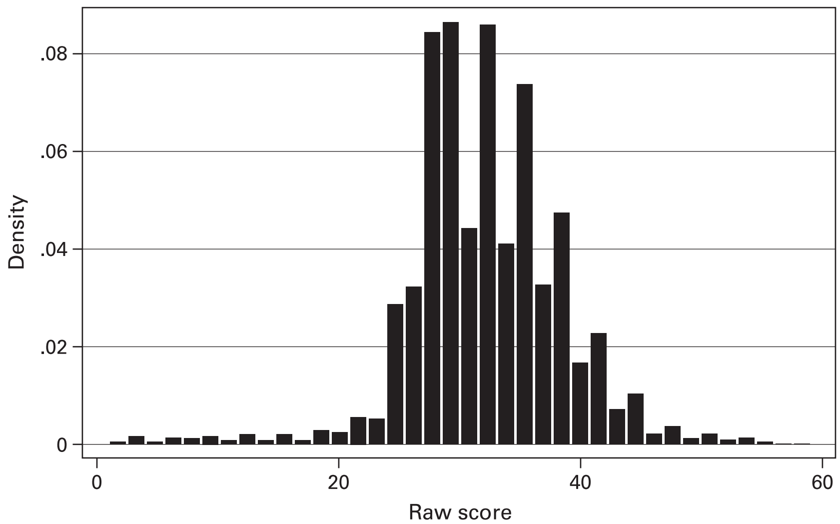

Figure 7.4
Distribution of WJ raw scores: Mathematics.

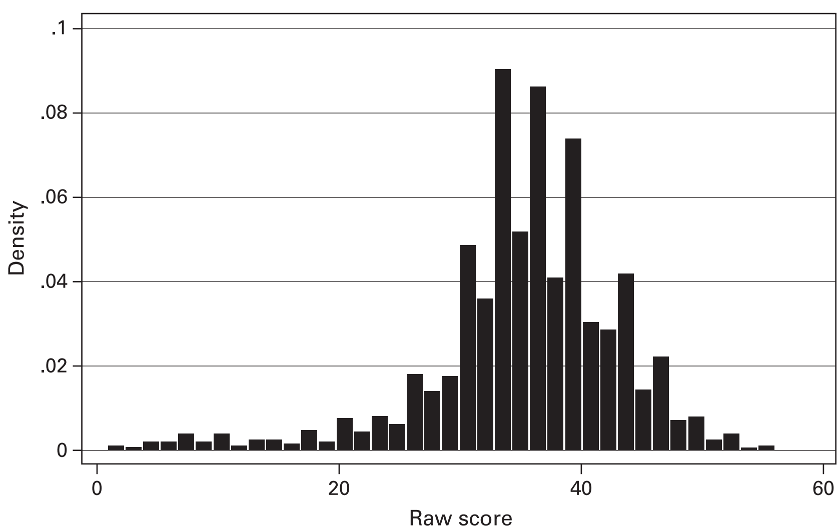

Figure 7.5
Distribution of WJ raw scores: Written language.

lead to higher achievement scores. This assumes, however, that the tests are in fact influenced by grades of completed schooling in the environment under study; namely, 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 7.14.

For all three tests, grades of schooling have a highly significant relationship with achievement test scores. 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), on 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 who took the tests and the achievement tests results. While the tests were applied to a total of 7,666 individuals between the ages of 15 and 21 in 2003, we use here those youth originally eligible for the program in the 1997 survey. About 1,426 students in the original T1998 sample and 1,216 students in the T2000 sample can be linked back to the baseline 1997 data records. While the total sample size is reasonably large, disaggregating the analysis by age and gender does lead to some small sample size cells.

One limitation for the current analysis is that these tests were only carried out in 2003, so no baseline information on test scores is available. To take into account different probabilities of being in the sample (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 7.15 for the model used to predict the probability of being in the sample, on which the construction of the weights is based).

7.4 Program Impact Estimates

In this section, we present impact estimates based on the weighted difference-in-difference estimator described in section 7.3. We present impacts by age, gender, and baseline schooling level because, as noted in section 7.1, 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 or T2000 locality, interacted with an indicator for postprogram 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 tables 7.3–7.7, 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 give the estimated differential treatment impact (i.e., 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.

7.4.1 Education

7.4.1.1 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 percent level. As shown

Table 7.3
Differences in preprogram means in 1997 between T1998 and T2000

	Mean values in 1997		P > |Z|, |T| Preprogram 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
Marriage[3]			
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. Proportion currently married or cohabiting. Sample includes all program-eligible individuals ages 9 to 15 in 1997 who are also interviewed in 2003.

in table 7.3, school enrollment rates in 1997 were 0.82 for T1998 boys, 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 re-entry 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 when younger. Furthermore, the new high school grants went into effect in 2001, but depending on their years of schooling prior to 1998, this may have been after many of those in T1998 had finished secondary schooling and/or made their enrollment decisions for high school.

In 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 7.4 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 base-

Table 7.4
Impact of differential exposure to *Oportunidades* on proportions enrolled in school: Difference-in-difference estimates: Adolescents 9 to 15 in 1997, T1998 versus T2000.

	Proportion enrolled in 2003 of T2000 group	Impact		
		Coefficient	Std. error	% change relative to T2000 group
Girls				
All girls 9 to 15 in 1997	0.26	−0.017	[0.014]	−6.6%
By age in 1997				
9–10	0.48	−0.040	[0.024]*	−8.3%
11–12	0.24	−0.003	[0.025]	−1.2%
13–15	0.10	−0.007	[0.023]	−7.3%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	0.48	−0.001	[0.024]	−0.2%
11–12	0.21	−0.029	[0.023]	−13.9%
13–15	0.08	−0.015	[0.022]	−18.2%
By completed 1997 schooling grades				
<=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 weighted *difference-in-difference* regression estimates. Weights described in section 7.3. 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.

line schooling levels. Enrollment rates are significantly higher for the T2000 children in the younger end of the age range 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 greater impact of differential program exposure for 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 percent decrease in 2003 enrollment rates (also see figure 7.6). 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 completed in 1997, which implies a 49.1 percent decrease in 2003 enrollment rates. The results, while generally insignificant, sug-

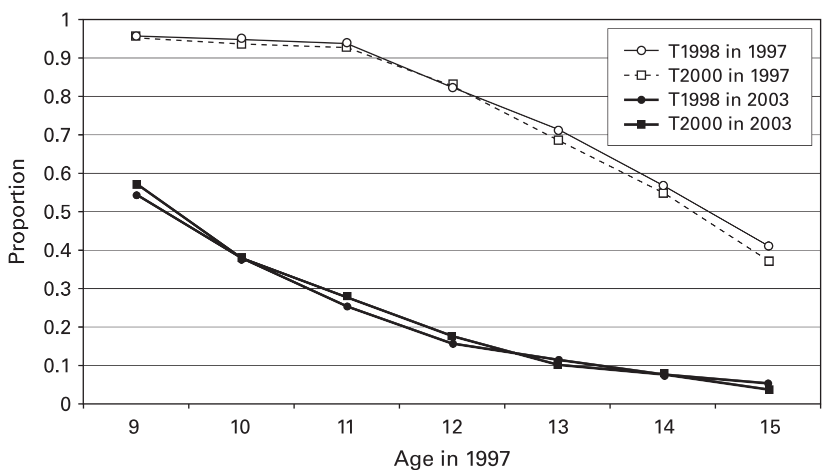

Figure 7.6
Proportion attending school in 1997–2003 by age in 1997.
Source: ENCASEH97. ENCEL03.

gest 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 next section).

7.4.1.2 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 (see figure 7.7).[19] The results shown in table 7.5 indicate significant positive program impacts on the proportion of boys progressing regularly through school, implying an average 7.4 percent 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 percent 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 percent and 28.8 percent. 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 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).

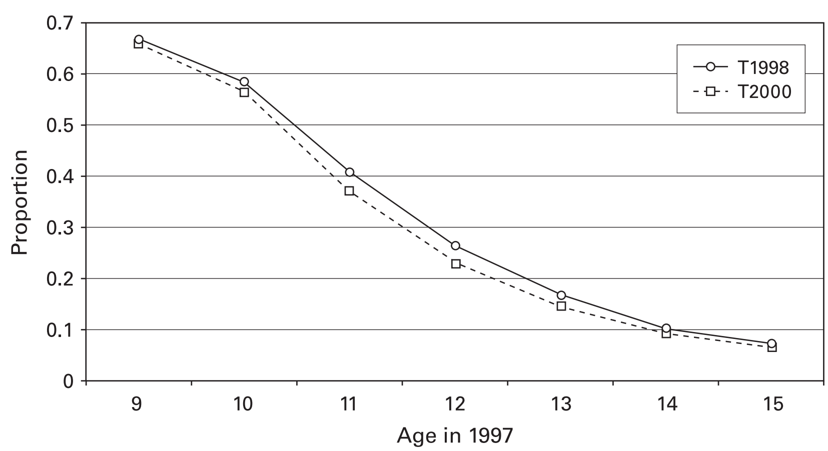

Figure 7.7
Progressing on time in 2003 by age in 1997.
Source: ENCASEH97. ENCEL03.

Table 7.5
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 in 1997				
9–10	0.606	−0.008	[0.018]	−1.3%
11–12	0.303	0.020	[0.018]	6.6%
13–15	0.090	0.012	[0.010]	13.4%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	0.619	0.020	[0.018]	3.2%
11–12	0.298	0.042	[0.017]**	14.1%
13–15	0.099	0.012	[0.010]	12.1%
By completed 1997 schooling grades				
<=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 weighted difference in whether progressed at least five grades between 1997 and 2003 regression estimates. Weights described in section 7.3. 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 7.6
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 in 1997				
9–10	7.43	0.075	[0.076]	1.0%
11–12	7.75	0.181	[0.091]**	2.3%
13–15	7.44	0.320	[0.077]***	4.3%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	7.38	0.197	[0.075]***	2.7%
11–12	7.68	0.241	[0.088]***	3.1%
13–15	7.56	0.139	[0.074]*	1.8%
By completed 1997 schooling grades				
<=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%

Notes: Estimates based on weighted *difference-in-difference* regression estimates. Weights described in section 7.3. 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.

7.4.1.3 Impacts on Schooling 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 7.3). By 2003, the estimates shown in table 7.6 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 the schooling grades completed by about 2.4 percent for boys and 2.7 percent for girls, beyond the schooling grades completed by the T2000 group by 2003.

Disaggregation by age and gender groups in 1997 and schooling grades completed in 1997 is informative. For girls, the estimated impacts increase with age in 1997, and are significant for those aged 11–12 (implying a 2.3 percent increase) and for those 13–15 (implying a 4.3 percent 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 percent increase for those in the 9–10 age group in 1997, a 3.1 percent increase for those 11–12, and a 1.8 percent increase for those 13–15. (Also see figure 7.8 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 schooling completed in 1997). In both cases the largest effects are observed for those

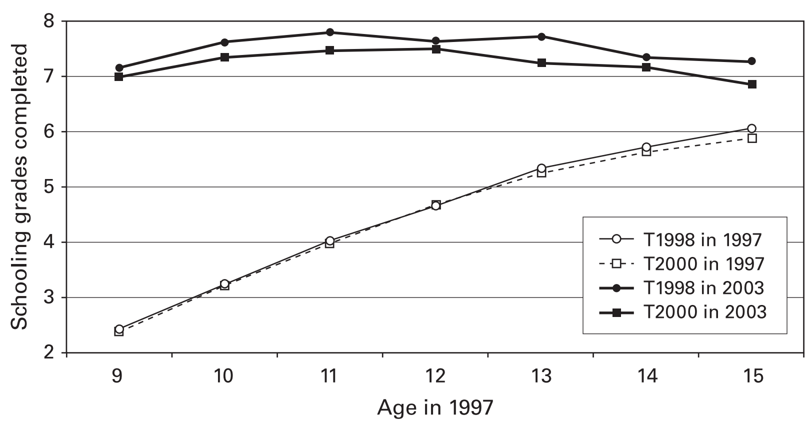

Figure 7.8
Grades of schooling completed in 1997 and 2003 by age in 1997.
Source: ENCASEH97. ENCEL03.

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

7.4.1.4 Impacts on Achievement Test Scores Table 7.7 presents 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 two of the 24 regression results reported in table 7.7, (girls aged 9 to 10 in writing and boys aged 11 to 12 in the case of math), there are unexpected negative and significant effects of the program on achievement scores, although both are barely significant at the 10 percent level.

Here we explore some possible explanations for the finding of no positive 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 given to only a subsample 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 whom the tests were given 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 subsample of youth taking the achievement tests. The results, reported in appendix table 7.16, are similar to those reported earlier for boys (table 7.6), 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 subsample of those taking the tests. Thus, for the subsample of girls taking the tests, the impacts on grades completed and on test

Table 7.7
Impact of differential exposure of *Oportunidades* on Woodcock Johnson: Difference estimates: Adolescents 9 to 15 in 1997, T1998 versus T2000.

		Impact		
	2003 WJ raw score of T2000 group	Coefficient	Std. error	% change relative to T2000 group
Reading skills				
Girls				
All girls 9 to 15 in 1997	53.56	−0.110	[0.483]	−0.2%
By age				
9–10	53.74	−1.244	[0.882]	−2.3%
11–12	54.04	0.016	[0.745]	0.0%
13–15	53.58	0.191	[0.898]	0.4%
Boys				
All boys 9 to 15 in 1997	53.64	0.199	[0.496]	0.4%
By age				
9–10	53.60	0.112	[0.780]	0.2%
11–12	54.36	−0.544	[0.680]	−1.0%
13–15	53.35	0.491	[1.134]	0.9%
Mathematics skills				
Girls				
All girls 9 to 15 in 1997	32.01	−0.225	[0.326]	−0.7%
By age				
9–10	32.27	−0.725	[0.573]	−2.2%
11–12	32.04	−0.186	[0.575]	−0.6%
13–15	31.85	0.078	[0.638]	0.2%
Boys				
All boys 9 to 15 in 1997	33.27	−0.574	[0.366]	−1.7%
By age				
9–10	32.79	−0.228	[0.545]	−0.7%
11–12	34.20	−1.145	[0.635]*	−3.3%
13–15	33.31	−0.744	[0.765]	−2.2%
Written language				
Girls				
All girls 9 to 15 in 1997	36.12	−0.301	[0.410]	−0.8%
By age				
9–10	36.93	−1.361	[0.725]*	−3.7%
11–12	35.83	0.186	[0.678]	0.5%
13–15	36.22	−0.501	[0.801]	−1.4%

Table 7.7
(continued)

	2003 WJ raw score of T2000 group	Impact		
		Coefficient	Std. error	% change relative to T2000 group
Boys				
All boys 9 to 15 in 1997	36.19	−0.011	[0.393]	0.0%
By age				
9–10	36.32	−0.454	[0.617]	−1.3%
11–12	36.49	0.177	[0.641]	0.5%
13–15	36.18	−0.549	[0.832]	−1.5%

Notes: Estimates based on weighted difference regression estimates. Weights described in section 7.3. 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.

scores both tended to be insignificant. For boys, the test score results continue to be surprising.

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.[21] 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.

7.4.2 Work

The theoretical effect of Oportunidades on the probability of working is ambiguous for the ages considered in this study. 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 toward 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

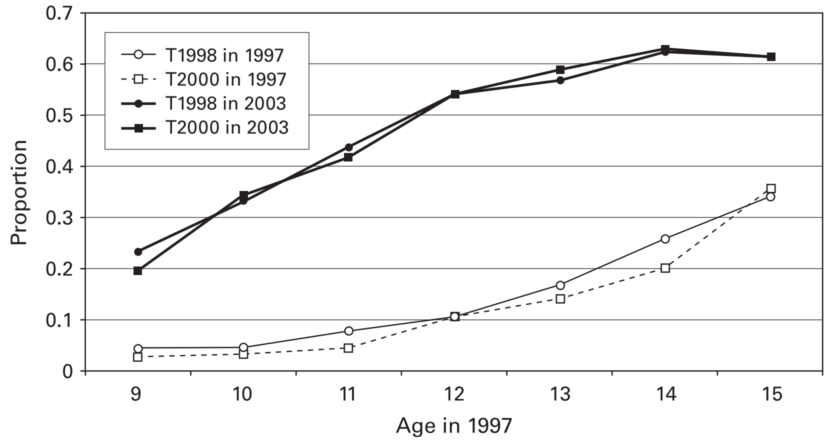

Figure 7.9
Proportion working in 1997 and 2003 by age in 1997.
Source: ENCASEH97. ENCEL03.

schooling (higher future wages) will no longer exceed the marginal cost (foregone wages and leisure time). These considerations lead us to expect that over the short run, the program decreases working, but over the longer run, the program might increase working. We next consider how the program affects the probability of working.

Prior to the program in 1997, 0.18 of the T1998 boys and (significantly less at the 5 percent level) 0.16 of the T2000 boys were working; also in 1997, 0.08 of the T1998 girls and (significantly less at the 1 percent level) 0.05 of the T2000 girls were employed (table 7.3 and figure 7.9). 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 7.8). The gender differentials in reported work are substantial.

The difference-in-difference estimate of the impact of differential exposure to the program on working in 2003 shows that greater exposure significantly decreases the proportion working, by 4.1 percent for boys, with no significant effects for girls (table 7.8). When we disaggregate by age and baseline schooling levels, for boys there are significant estimated declines in the proportions working in 2003, −5.5 percent for those in the 13–15 age group in 1997 (19–21 in 2003) and −15.9 percent for those who had seven-plus grades of schooling completed in 1997.

Table 7.8
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 in 1997				
9–10	0.14	−0.008	[0.019]	−5.6%
11–12	0.34	−0.010	[0.024]	−2.9%
13–15	0.40	−0.020	[0.025]	−5.0%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	0.40	−0.015	[0.024]	−3.8%
11–12	0.67	−0.007	[0.026]	−1.0%
13–15	0.83	−0.046	[0.025]*	−5.5%
By completed 1997 schooling grades				
<=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 weighted *difference-in-difference* regression estimates. Weights described in section 7.3. 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.

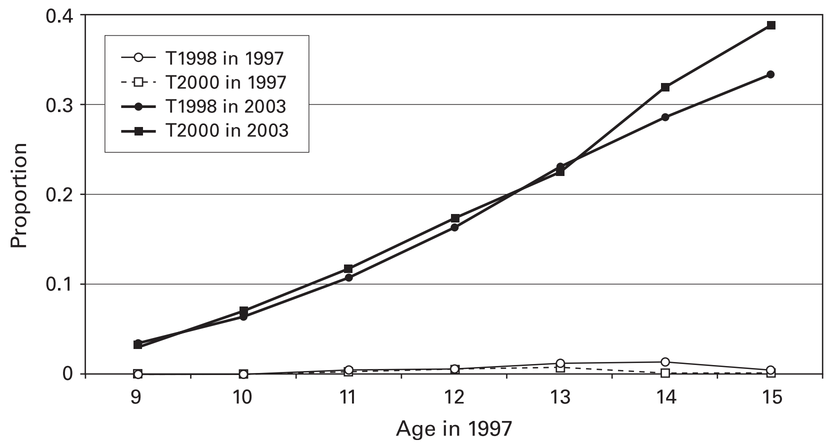

Figure 7.10
Proportion married in 1997 and 2003 by age.
Source: ENCASEH97. ENCEL03.

7.4.3 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 as married if they report they are legally married or are living together (cohabitating). 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 7.3 and figure 7.10), though with significantly higher (at the 10 percent level) proportions for the T2000 girls than for the T1998 girls. In 2003 26 percent of T2000 girls and 10 percent of T2000 boys were married (table 7.3).

The difference-in-difference estimates in table 7.9 indicate that the proportion of girls married was not significantly affected by the program, at the 10 percent significance level. The estimated overall impact on boys age 9 to 15 in 1997 also is not significant at the 10 percent level. Disaggregating by age group and baseline schooling, however, there are some significant negative impact estimates, for boys with little (four) or relatively many (seven-plus) grades of schooling in 1997. These estimates imply a decline of −12.9 percent in the proportion

Table 7.9
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 in 1997				
9–10	0.09	−0.007	[0.013]	−7.7%
11–12	0.22	−0.008	[0.020]	−3.6%
13–15	0.43	−0.019	[0.022]	−4.4%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	0.01	0.008	[0.006]	60.0%
11–12	0.07	−0.014	[0.011]	−20.3%
13–15	0.19	−0.013	[0.016]	−6.8%
By completed 1997 schooling grades				
<=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 weighted *difference-in-difference* regression estimates. Weights described in section 7.3. 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.

married by 2003 in the former case and a decline of −25.4 percent in the proportion married by 2003 in the latter case. Thus, some of the youth with earlier exposure to the program appear to be delaying marriage.

7.4.4 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, incentives to migrate may include 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). The 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 from moving away from small poor rural communities, such as those in the Oportunidades evaluation sample, are likely to be greater for individuals with more schooling if the returns to 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 program's 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 effect would 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 7.10 and figure 7.11) and almost four-tenths of girls (0.39 of the T2000 girls). The difference-in-difference estimates in table 7.10 imply that the proportion of boys who migrated was reduced due to the differential program exposure by a significant −6.2 percent.[22] The disaggregated estimates indicate that this drop was due primarily to significant declines for the oldest (−10.0 percent for boys age 13 to 15 in 1997) and most-schooled (−13.5 percent 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 percent 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.

7.5 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 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, are high. Furthermore, if returns to schooling are higher when school 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 this sense, school quality may affect educational outcomes as well as working and income impacts.

To investigate these questions, we consider selected school characteristics that are potential measures of school quality. As part of the ENCEL2003 survey, detailed questionnaires were undertaken on

Table 7.10
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, T1998 versus T2000

	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 in 1997				
9–10	0.21	−0.035	[0.018]*	−16.7%
11–12	0.36	0.021	[0.023]	5.8%
13–15	0.57	−0.014	[0.022]	−2.5%
By completed 1997 schooling grades				
<=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 in 1997				
9–10	0.12	0.014	[0.015]	12.1%
11–12	0.30	−0.025	[0.021]	−8.4%
13–15	0.51	−0.051	[0.021]**	−10.0%
By completed 1997 schooling grades				
<=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.

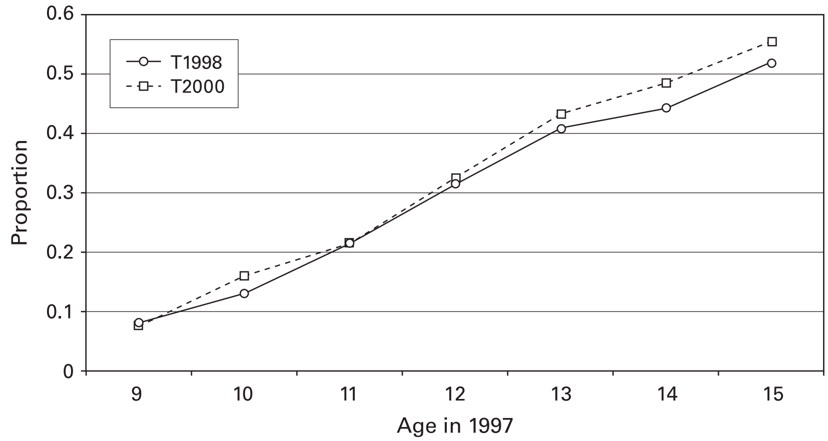

Figure 7.11
Proportion of individuals leaving household in 2003 by age in 1997.
Source: ENCASEH97. ENCEL03.

school quality and given to school directors, as well as to two teachers at each 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 the quality available at a national level. We also use administrative information from the Ministry of Education, which provides preprogram indicators of school quality. (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 understand to be important in the Mexican environment.

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 satellite videos shown during class time in different subjects, followed by time spent doing exercises. There is only one teacher for all 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 specialized instructors who teach 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.

Table 7.11 provides the differential exposure estimates by school characteristics, showing that the impacts appear to be higher when students have access to a general or technical secondary school. The differential exposure results indicate that 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 7.11 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 or equal to 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. The results suggest 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 low student-teacher ratios versus 0.12 in schools with high 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

Table 7.11
Impact of differential exposure of *Oportunidades* on grades of schooling completed, by school quality characteristics: 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				
9–10	0.019	[0.075]	0.069	[0.231]
11–12	0.186	[0.091]**	0.349	[0.261]
13–15	0.176	[0.073]**	0.566	[0.209]***
Boys				
All boys 9 to 15 in 1997	0.162	[0.044]***	0.250	[0.122]**
By age				
9–10	0.169	[0.075]**	−0.092	[0.184]
11–12	0.186	[0.085]**	1.022	[0.255]***
13–15	0.145	[0.072]**	0.178	[0.200]
	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				
9–10	0.013	[0.102]	0.029	[0.085]
11–12	0.217	[0.121]*	0.184	[0.100]*
13–15	0.290	[0.098]***	0.130	[0.084]
Boys				
All boys 9 to 15 in 1997	0.250	[0.122]**	0.172	[0.050]***
By age				
9–10	−0.092	[0.184]	0.189	[0.084]**
11–12	1.022	[0.255]***	0.087	[0.095]
13–15	0.178	[0.200]	0.228	[0.082]***

Notes: Estimates based on weighted *difference-in-difference* regression estimates. Weights described in section 7.3. 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.

into these areas. The potential relationship of the impact of Oportunidades to school quality should be an important topic for future work.

7.6 Conclusions

This chapter presented an assessment of the impacts of Oportunidades on rural adolescent youth after 5.5 years of benefits, comparing youth receiving the program 5.5 years versus 4 years. The results on the effects of differential exposure to the program indicate that children with a year and a half more of benefits achieve on average about 0.2 grades of additional schooling.[24] This chapter 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 to which children are learning more 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 positive 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.[25] There is 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 schooling. A limitation for the test score analysis, but not for other indicators, 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.

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. As discussed earlier, the theoretical effect of the program on work is ambiguous for the age group considered in this study. 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 chapter. 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).

Another important area for future research is the relationship of the program to migration and whether youth who increase their schooling 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 possibly following the migrants themselves.

Appendix A: Construction of Variables

Sample Construction

The analysis uses youth aged 9 to 15 in 1997 (15 to 21 in 2003). In practice, there are inconsistencies in the ages reported; for example, not all youth reported to be age 9 to 15 in 1997 are within the range of 15 to 21 in 2003 (or even slightly outside the range). To correct some errors and insure that we are correctly matching individuals over the six-year period, we deleted from the sample any individual who was more than two years off in 2003 with respect to what would be his or her "correct" age according to that reported in 1997. Additionally, we eliminated individuals for whom gender is reported to be different between the two time periods. We also deleted from the sample individuals who reported impossible changes in the grades completed over time. That is, we eliminated individuals reporting negative changes in schooling or those reporting they had completed more than eight grades of schooling over the six-year period, about 6 percent of the cases.

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 an additional three grades. We allowed up to five years for college education and an additional one to five for graduate work.

Progressing through school on time 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.

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.

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 socioeconomic survey in 2003. We have some information for the reason a household was not interviewed for a majority of, but not all, households. Only a low percentage, less than 5 percent, of households refused to answer the survey; most household-level attrition appears to be due to migration.

For those households who remain, 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. In the survey, individuals who have left the household less than a year prior to the survey are still considered to be residents and the survey is conducted as if they were residents. For our analysis on migration, we define 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

Preprogram administrative data is available from the Secretary of Public Education on schools in 1997 for some school quality indicators. To match available school quality to each individual in the sample, we as-sign the relevant supply 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 percent of students to a potential supply of schools.

Appendix B: Analysis of Attrition

This appendix provides the estimates and a more detailed discussion of the attrition analysis of section 7.3. Tables 7.12 and 7.13 give probit estimates for the probability of being lost to follow-up overall for those 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 preprogram individual characteristics, parental characteristics, and housing characteristics.

The first specification (column 1), not surprisingly, replicates the patterns noted with regard to table 7.2. The second specification (column 2) indicates that a number of the preprogram individual, parental, and housing characteristics interact significantly with T1998 to affect attrition.

Among the preprogram 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 preprogram 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.

Table 7.12
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
	[0.011]	[0.130]	[0.010]***	[0.110]	[0.008]*	[0.088]
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

Note: Levels of all interacted variables are included but not reported. 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 sample.
[b] Household attrition refers to individuals attriting because entire HH attrits.

Table 7.13
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		Individual attrition[a]		Household attrition[b]	
	(1)	(2)	(1)	(2)	(1)	(2)
T1998 = 1; T2000 = 0	0.006 [0.011]	−0.016 [0.139] [0.057]	−0.013 [0.011]	−0.142 [0.131] [0.054]	0.019 [0.008]**	0.095 [0.078] [0.031]
Interactions						
T1998*age		−0.012 [0.008]		0 [0.007]		−0.009 [0.005]*
T1998*indigenous		−0.019 [0.050]		0.075 [0.047]		−0.071 [0.024]***
T1998*schooling		0.014 [0.009]		0.004 [0.008]		0.011 [0.006]**
T1998*enrolled		0.047 [0.071]		0.051 [0.063]		−0.019 [0.044]
T1998*father education		0.002 [0.006]		0.008 [0.006]		−0.005 [0.004]
T1998*father age		0.005 [0.002]**		0.004 [0.002]**		0.001 [0.001]
T1998*father indigenous		0.227 [0.092]**		0.092 [0.089]		0.149 [0.088]*
T1998*father bilingual		−0.173 [0.073]**		−0.11 [0.061]*		−0.045 [0.047]
T1998*mother education		−0.01 [0.006]		−0.005 [0.006]		−0.005 [0.004]
T1998*mother age		−0.004 [0.002]*		−0.004 [0.002]*		0 [0.001]
T1998*mother indigenous		−0.099 [0.069]		−0.067 [0.059]		−0.03 [0.042]
T1998 *mother bilingual		0.119 [0.053]**		0.07 [0.050]		0.054 [0.041]
T1998*rooms		0.014 [0.011]		0.006 [0.008]		0.006 [0.009]
T1998*electricity		−0.002 [0.027]		0.013 [0.024]		−0.016 [0.017]
T1998*water		−0.025 [0.027]		−0.012 [0.024]		−0.011 [0.017]
T1998*dirt floor		−0.002 [0.028]		0.014 [0.026]		−0.011 [0.018]
Observations	7870	7806	7870	7806	7870	7806

Note: Levels of all interacted variables are included but not reported. 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 sample.
[b] Household attrition refers to individuals attriting because entire HH attrits.

- 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 preprogram 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 no evidence that the timing of treatment significantly impacts attrition, the timing of treatment does appear to be significantly negatively associated with individual attrition and significantly positively associated with household attrition—and there are a number of significant interactions with individual, parental, and housing characteristics (differing in many cases for boys versus girls). For this reason, the impact results reported in the paper use weighting to take differences in observables arising from attrition into account.

As noted in section 7.3.4, table 7.14 explores the relationship between test scores and schooling levels by estimating a linear regression of test scores on schooling and on other individual, parental, and household-level control variables. Grades of schooling are highly statistically significant determinants of test scores. (See the discussion in section 7.3.4.)

As discussed in section 7.3.4, we control for differences between the control and treatment groups in the subsample that takes the test using a reweighting method, where the weights reflect the probabilities of

Table 7.14
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
Grades of schooling	1.311 [0.063]***		1.05 [0.047]***		1.416 [0.052]***	
1 grade of schooling		−5.451 [2.269]**		0.139 [1.729]		1.748 [2.013]
2 grades		3.316 [1.636]**		2.683 [1.242]**		1.034 [1.402]
3 grades		3.477 [1.313]***		1.714 [0.987]*		1.700 [1.098]
4 grades		4.302 [1.386]***		0.579 [1.045]		1.667 [1.170]
5 grades		8.186 [1.328]***		2.422 [1.002]**		3.183 [1.122]***
6 grades		12.325 [0.963]***		4.419 [0.723]***		6.023 [0.807]***
7 grades		11.697 [1.193]***		4.432 [0.897]***		7.700 [0.998]***
8 grades		13.287 [1.071]***		5.522 [0.806]***		9.579 [0.897]***
9 grades		14.493 [0.954]***		7.284 [0.716]***		10.816 [0.799]***
10 grades		15.255 [1.098]***		10.463 [0.826]***		13.265 [0.919]***
11 grades		15.520 [1.110]***		11.578 [0.836]***		13.833 [0.930]***
12 or more grades		13.802 [1.089]***		9.339 [0.818]***		12.400 [0.911]***
Age	0.022 [0.031]	0.045 [0.031]	0.036 [0.023]	0.029 [0.024]	0.013 [0.026]	0.019 [0.026]
Gender	−0.548 [0.302]*	−0.443 [0.301]	0.829 [0.228]***	0.852 [0.228]***	−0.391 [0.249]	−0.416 [0.252]*
Indigenous	−3.648 [1.048]***	−3.789 [1.048]***	−3.622 [0.792]***	−3.927 [0.795]***	−4.674 [0.876]***	−4.849 [0.888]***
Indigenous, speaks						
Spanish	4.251 [0.922]***	3.962 [0.921]***	2.632 [0.700]***	2.827 [0.701]***	3.599 [0.773]***	3.580 [0.783]***
Father education	−0.001 [0.078]	0.061 [0.078]	0.138 [0.059]**	0.125 [0.059]**	0.105 [0.065]	0.114 [0.065]*
Father age	0.049 [0.025]**	0.037 [0.024]	0.016 [0.019]	0.002 [0.018]	0.041 [0.020]**	0.029 [0.020]
Father indigenous	−2.381 [1.264]*	−1.198 [1.260]	−0.783 [0.968]	−0.257 [0.969]	−1.646 [1.057]	−1.068 [1.069]

Table 7.14
(continued)

	Reading	Reading	Math	Math	Writing	Writing
Father Spanish	1.977 [1.158]*	1.613 [1.155]	0.599 [0.890]	0.350 [0.892]	1.898 [0.971]*	1.715 [0.983]*
Mother education	0.04 [0.079]	0.162 [0.079]**	0.193 [0.060]***	0.205 [0.059]***	0.223 [0.065]***	0.246 [0.066]***
Mother age	−0.053 [0.027]*	−0.024 [0.027]	0.003 [0.021]	0.017 [0.020]	−0.011 [0.023]	0.009 [0.022]
Mother indigenous	−0.567 [1.020]	−0.438 [1.017]	0.711 [0.767]	0.628 [0.768]	0.404 [0.841]	0.281 [0.851]
Mother Spanish	0.124 [0.794]	−0.042 [0.792]	−0.481 [0.598]	−0.601 [0.598]	0.113 [0.655]	0.106 [0.663]
Rooms	−0.036 [0.053]	−0.032 [0.052]	−0.039 [0.039]	−0.036 [0.040]	0.002 [0.043]	0.001 [0.044]
Electricity	0.582 [0.373]	0.768 [0.372]**	0.555 [0.282]**	0.558 [0.282]**	0.944 [0.308]***	0.919 [0.311]***
Water	−0.700 [0.328]**	−0.731 [0.326]**	0.245 [0.247]	0.340 [0.246]	0.146 [0.270]	0.249 [0.272]
Dirt floor	−0.081 [0.333]	−0.115 [0.330]	−0.185 [0.250]	−0.216 [0.250]	0.113 [0.274]	0.021 [0.276]
R-squared	0.20	0.22	0.25	0.26	0.32	0.31

Standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.

being in the test-taking sample. Table 7.15 presents estimates of the probability of being included in the test-taking sample, based on a probit model, where the dependent variable is an indicator of whether in the test-taking sample and the independent variables are characteristics observed in 1997. The specification includes the characteristics both in levels and interacted with the indicator for being in the T1998 sample (only the coefficients on the interacted terms are shown). Not many of the characteristics predict being in the test-taking sample. However, father's education, schooling, and whether the mother is bilingual have statistically significant coefficients. The reweighting estimator applied in section 7.3.4 uses the estimated probability of taking the test (the fitted probit values) to take into account the potential nonrandomness in who takes the test.

Table 7.16 shows the impact of differential exposure to Oportunidades on grades of schooling for the subsample that takes the Woodcock Johnson achievement tests. Boys with longer exposure achieve about 0.21 additional grades of schooling. However, there are no significant impacts for girls.

Table 7.15
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	−0.010	−0.005
	[0.003]**	[0.005]**	[0.004]
T1998*enrolled	−0.014	−0.001	−0.020
	[0.016]	[0.023]	[0.023]
T1998*father education	−0.006	−0.002	−0.008
	[0.003]**	[0.004]	[0.004]**
T1998*father age	0.000	−0.001	0.000
	[0.001]	[0.001]	[0.001]
T1998*father indigenous	−0.032	−0.090	0.022
	[0.046]	[0.059]	[0.070]
T1998*father bilingual	0.043	0.036	−0.020
	[0.050]	[0.054]	[0.057]
T1998*mother education	0.003	0.005	0.002
	[0.003]	[0.004]	[0.004]
T1998*mother age	0.000	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

Note: Levels of all interacted variables are included but not reported.
Standard errors in brackets.
*significant at 10%; **significant at 5%; ***significant at 1%.

Table 7.16
Impact of differential exposure of *Oportunidades* on grades of schooling: Sample taking the Woodcock Johnson achievement tests: Adolescents 9 to 15 in 1997, T1998 versus T2000

	Impact	
	Coefficient	Std. error
Girls		
All girls 9 to 15 in 1997	0.067	[0.121]
By age group		
9–10	−0.064	[0.155]
11–12	−0.118	[0.191]
13–15	0.298	[0.205]
Boys		
All boys 9 to 15 in 1997	0.212	[0.113]*
By age group		
9–10	0.322	[0.137]**
11–12	0.141	[0.189]
13–15	0.205	[0.230]

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.

Acknowledgments

The authors thank Bernardo Hernández and Iliana Yaschine for useful comments.

Notes

1. Such programs exist in Brazil, Chile, Colombia, Guatemala, Honduras, Nicaragua, and Uruguay.

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 (2006), Skoufias (2004), Skoufias and McClafferty (2001), and Parker (2003).

4. See, for example, Schultz (2004), Behrman, Sengupta, and Todd (2005), and Todd and Wolpin (2006).

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 (2005), Schultz (2004), and Todd and Wolpin (2006).

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 mother, can be given directly to the youth.

7. Note this allows a student theoretically to receive two years of grants 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).

11. See Behrman and Todd (1999) for more details and an analysis of the quality of the randomization. In general, Behrman and Todd conclude that there are few significant preprogram differences between the treatment and control group at the locality level, which is the level at which the randomization was done. However, at the individual level, there are more significant preprogram differences than would be expected by chance. As a result, most previous studies adjust for preexisting differences in baseline characteristics of treatments and controls, using control variables and/or difference-in-difference methods.

12. 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.

13. We use the former (original) definition for all of our analysis below, but similar results on sample attrition and program exposure are obtained if alternative definitions of eligibility are used.

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

15. 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. If, for instance, unobserved skills affect the probability of migration and these skills also are a determinant of program impacts, then the assumption that attrition is random conditional on observables would be violated. The sign and magnitude of the bias to the estimates would depend, in this example, on the relationship between unobserved skills and migration as well as that between unobserved skills and program impacts.

16. 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 U.S. states (Arizona, California, Florida, New York, and Texas).

17. The notes to tables 7.3–7.7 give the full set of control variables.

18. We give the rates for T2000 in the first column in table 7.[redacted] 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 failed a grade. However, to fail a grade, an individual must be enrolled in school. If Oportunidades affects enrollment, as previous 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 use the indicator of whether an individual progressed on time to avoid these interpretational problems.

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

21. Behrman, Sengupta, and Todd (2005), however, report that they find no evidence of congestion effects in their comparisons of ineligible children in T1998 versus T2000 villages.

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 et al. (1996); Alderman, Orazem, and Paterno (2001); Behrman and Birdsall (1983); Behrman, Birdsall, and Kaplan (1996); Behrman et al. (1997); Behrman, Rosenzweig, and Taubman (1996); Behrman, Ross, and Sabot (2008); Betts (1995, 1996); Card and Krueger (1992a,b); Grogger (1996); Heckman, Layne-Farrar, and Todd (1996); Lloyd, Clark, and Mensch (2000).

24. Note that our results are consistent with previous studies focusing on short-run impacts of the program (e.g. Behrman, Sengupta, and Todd 2005 and Schultz 2004). These studies estimated short-run impacts comparing the treatment group receiving 18 months of benefits versus the control group. Based on the short-run impacts, both studies simulated a long-run increase of about 0.6 years of schooling. Our corresponding direct estimate here is 0.2 years, based on two groups receiving benefits, but with one group receiving an additional year and a half of program benefits. Given that the program of grants lasts seven years, our estimates would appear to be fairly consistent with those based on the short-run studies.

25. Some recent studies have suggested that merit scholarships at schools are effective at improving test scores, although increasing school quality through inputs such as textbooks has generally had few impacts on learning. See Glewwe and Kremer (2006) for a review of these and other schooling interventions.

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