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Medium-Term Effects of the *Oportunidades* Program Package, including Nutrition, on Education of Rural Children Age 0-8 in 1997

by

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Executive Summary

The Program *Oportunidades* (formerly *PROGRESA*) has been operating in small rural communities since 1997, providing cash grants to families in exchange for regular school attendance of children and youth as well as regular health clinic attendance and nutritional supplements for infants and very young children and for pregnant and lactating women. This paper provides estimates of the medium-term impacts on education for rural children aged 0 to 8 in 1997 just prior to the initial rural intervention, or those aged 6 to 14 in the 2003 Rural Evaluation Survey. The education of the oldest children within this age range would have been expected to have been affected primarily by the scholarship component of the program. The education of the youngest children within this age range, in contrast, would not have been affected directly by the scholarship component of the program by 2003 because they were too young to have advanced by then to the third grade at which the scholarships begin (though there may have been indirect effects through expectations about scholarships in the future). But on the other hand, these children were beneficiaries of the infant nutritional supplements, which may have improved their educational performance when they became of school age.

The results in this paper extend those of previous evaluations based on the Rural Evaluation Surveys between 1998 and 2000 and are complementary to the concurrent evaluation for rural children aged 15-21 in 2003. Our analysis is based on new information provided in the 2003 Rural Evaluation Survey (ENCEL), which is linked with earlier data, in particular the 1997 pre-program Survey of Household Socio-economic Characteristics (ENCASEH) data. The ENCEL2003 provides another round of information on the original evaluation treatment and control households who began receiving *Oportunidades* benefits in 1998 and 2000 respectively. The original program evaluation was characterized by a random experiment with households from 320 communities with less than 2,500 inhabitants being assigned to receive benefits (T1998) and households from 186 being assigned to receive benefits approximately 18 months later (T2000). Given the incorporation of the original control group to receive benefits in 2000, the ENCEL2003 also included a new comparison group (C2003), constructed through the matching of communities to the communities of the original ENCEL evaluation.

The strategy of analysis includes direct assessment of the impacts using two different approaches. *First*, we estimate difference-in-difference treatment effect estimates using the original treatment and control groups for children who were of school age (6-8) in 1997 and treatment-control difference estimates for children who were of pre-school age (0-5) in 1997. These estimates we term as *differential exposure* impacts as they compare individuals in the original treatment group who have received benefits for about 5 and a half years with individuals in the original control who by 2003 had received benefits for about four years. *Second*, we present matching estimates between those who had obtained treatment in 1998 and the 2003 matched comparison group. The 2003 matched comparison group had not yet received program benefits at the time of the survey, thus these matching estimators provide impact estimates of the

program after five and a half years of treatment. We study a number of different education indicators, including age of entry to primary school, failure and progression rates, grades of completed schooling, and whether the individual had entered secondary school.

Both sets of results are suggestive of some limited, but important impacts of *Oportunidades* on behaviors related to schooling for rural children 0-8 in 1997. As expected, the matching results show generally larger impacts of the program than the differential exposure results, reflecting the longer time of program benefits of the treatment group relative to the comparison group. The estimated impacts do, however, vary substantially by age group. It is instructive to review the results by age, given that different components of the program are relevant for different age groups in the T1998 group.

Children aged 0 to 2 in 1997 were exposed directly only to the infant nutritional supplement and check-up components of the program (though they may have been affected indirectly by other aspects of the program, such as income transfers to other household members) The estimates show some positive impacts of the program on these children, both in the differential exposure and the matching estimates. There is some weak evidence they are likely to enter school at a slightly earlier age and some stronger evidence from the matching estimates showing they are more likely to progress on time and have higher years of completed schooling as they begin to enter school. In particular, boys and girls age 1 in 1997 (7 in 2003) are more likely to have completed a year of schooling in 2003 relative to the new C2003 comparison group. This group, however is only recently entering school age, it thus is early for final conclusions on the eventual impacts of the early nutritional intervention to be drawn. This is particularly true of those children age 6 in 2003 (infants in 1997). While they are the children most likely to have benefited from the early nutritional intervention, only age at enrollment and current enrollment could be studied in this paper. The initial evidence of the impact on children aged 1 in 1997 is suggestive of important impacts, however, and consistent with the interpretation of education impacts as a direct result of the early nutrition intervention received.
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Most children aged 3 to 5 in 1997 had no or very limited direct program exposure beyond health check-ups because they were too old for the infant nutritional supplements and too young for the educational grants (though a few who were malnourished may have received nutritional supplements and, again, all of them may have been affected indirectly to transfers to the household for others, such as older siblings). Therefore it perhaps is not surprising that for children aged 3 to 5, there are few positive and significant impacts. Boys aged 5 in 1997 (11 in 2003) show a reduction in the probability of ever failing a grade under both sets of estimations as do boys aged 4 who show an increase in the probability of progressing on time. For other ages and for girls, there are no significant positive effects. There are also some puzzling effects, girls aged 5 show a negative impact on the probability of progressing on time as do boys aged 5 in 1997 on years of schooling in the matching estimates. Note that children aged 3 to 5 in 1997 would likely have benefited less (if at all) from the nutritional intervention

than younger children and in 2003, at age 9 to 11 could have, at most, received grants for a couple of years 2 to 3 years. In this sense, it is logical to expect lower impacts for this group than for the other groups studied here. In particular, for the differential exposure estimates which compare the T1998 group with the T2000 group, there would not even be differences in eligibility for grants between the two groups, given the short difference in program exposure (both groups would have been eligible to begin receiving grants in the third grade around the year 2000 or 2001, at this point both the T1998 and T2000 groups were receiving benefits. Any significant differences in schooling would have to derive from other program components (for instance through higher income of the family overall rather than the grants specific to children in this age group).

Children aged 6 to 8 in 1997 had much greater exposure by 2003 to the educational grants than the younger children considered above. For children aged 6 to 8 in 1997 (12 to 14 in 2003), there are unambiguous positive effects of the program on schooling attainment. These children would by and large have been eligible to receive the grants beginning in 1998 or 1999 and thus have been eligible to receive grants for all or nearly all of the experimental period. The estimates show strong impacts of the program on progressing through school, years of schooling attainment and the proportion entering secondary. In particular, boys aged 6 to 8 show significant increases, between 0.42 (those aged 6 in 1997) to 0.903 (in 2003) additional years of schooling compared with similar youth without benefits. Girls aged 6 to 8 also show important impacts, girls aged 6 in 1997 (12 in 2003) attain 0.73 years of additional schooling. Matching estimates of the impact on the proportion of boys and girls entering secondary shows increases of about one-third for both boys and girls.

The evidence thus far shows strong impacts for children aged 6 to 8 in 1997 on their level of schooling attainment and smaller impacts for children below this age group. As described above, this is likely to reflect the difference in program components available. Impacts for the older children likely derive from the receipt of grants, whereas younger children by and large were not eligible yet or eligible for only a short period of time for the grants by 2003. The results are consistent with the point that, thus far, education grants seem to provide the largest impacts on schooling, compared with the possible impact of other components. A caveat is that the younger groups studied in this paper, e.g. up to age 5 in 1997 (12 in 2003) are still presumably in the early phases of their educational career and thus have had comparatively less time for education impacts to become evident. To assess whether there are these effects as these children become older, as well as effects of infant nutritional interventions on education for the later primary and secondary school ages, it is important to continue to trace these children as they age.

1. Introduction:

Oportunidades (formerly *PROGRESA*) has now been operating for more than six years in small communities in rural areas of Mexico. Its central objective of linking monetary transfers to investments in the human capital of poor children and family members has been adopted in a number of other countries in Latin America and the Caribbean, as well as in other parts of the world. A rigorous external evaluation, with several rounds of panel data in 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 evaluation results demonstrate significant impacts in improving infant and child nutrition, reducing child labor, improving health outcomes, and increasing school enrollment, among other short-term effects.[i] Some of the initial evaluation studies also have generated estimates of longer-run effects, but they have done so conditional on assumptions such as stability in schooling transition matrices or in the structural relations underlying family behaviors (e.g., [5-8]).

With the availability of the 2003 follow-up rural evaluation survey (ENCEL2003), it is now possible to begin to assess directly some important medium-run effects of the program. This report examines the educational impacts of *Oportunidades* on young children in the medium term in 2003, that is, about five and a half years after households in the original treatment group began receiving benefits. We consider in this paper the group of children aged 0 to 8 in 1997 just prior to the program intervention, or those aged 6 to 14 in 2003.[ii] We study in particular the impacts on

- age of starting school,
- enrollment in school in 2003,
- grade failure,
- grade progression on time,
- number of completed school grades,
- enrollment in secondary school.

The education of children aged 0 to 8 in 1997 just prior to the intervention, or those aged 6 to 14 in the 2003 survey, might be expected to have benefited some from the resource effect of transfers to families more or less independently of the children's ages. But there also might be expected to be some important respects in which different components of the program affected children in this age range differentially depending on the children's ages in 1997.

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i The overall evaluation of the initial years of *PROGRESA* is summarized in [1-4].

ii This report complements other reports that are part of the *INSP* 2004 evaluation of *Oportunidades*, particularly the reports on medium-term impacts on education and related outcomes in rural areas for children 9-15 in 1997 or 15 to 21 in 2003 (see [9]) and on short-term impacts on education in urban areas (see [10]), but also reports on other topics such as targeting. It also complements earlier studies on education (see [5-7, 11]).

The oldest children in this age range were of ages at which they had the potential to benefit by 2003 from a number of years of the *Oportunidades* scholarships that started with enrollment in the third grade. For these children it would seem likely that the scholarship program would be the most important component of the *Oportunidades* intervention. These children also were old enough for the most part to face by 2003 a critical juncture in schooling attainment in poor communities in rural Mexico -- whether primary school graduates continued into secondary school, which occurred for most individuals when they were in their early teenage years (see Figure 1.1, which also illustrates the inverse labor market-schooling enrollment relation, particularly for boys). [iii]

The youngest children in the 0-8 year-old age range in 1997, in contrast, were likely to have benefited little or not at all from the scholarships by 2003 (though if their families have forward-looking behavior these children's education may have been affected by the expectation that they soon would start receiving scholarships). But on the other hand, the youngest group benefited from the nutritional supplement for infants,[iv] which may have improved their educational gains from attending school once they became of school age. Therefore considering those children at the younger end of the age group permits exploration of one of the original rationale for *Oportunidades*: to exploit interactions among various investments in human resources in hopes that the "whole might be more than the sum of the parts."[v] However, because of the nature of the intervention and the duration of the initial International Food Policy Research Institute (IFPRI) evaluation of the original rural *Oportunidades* program, that evaluation did not explicitly address the empirical magnitudes of such interactions, though one of the papers in the evaluation was a literature review on such interactions (see [13]). While there is a presumption that such effects of nutrition on child education are likely to be important and a number of studies that report associations in data that are consistent with such effects (e.g., see [14, 15]), there is a relatively small literature for developing countries that has examined the causal effects persuasively.[vi]

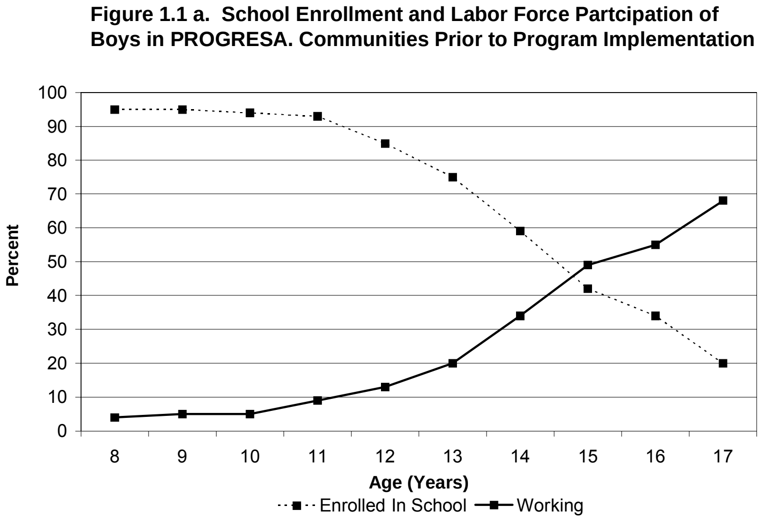
[iii] Previous evaluations have, in fact, demonstrated that the largest effects of the program were precisely at this transition between primary and secondary school (see [5-8]).

iv See [12] for a summary of this program and evidence that it has significant impact on infant and pre-school child growth

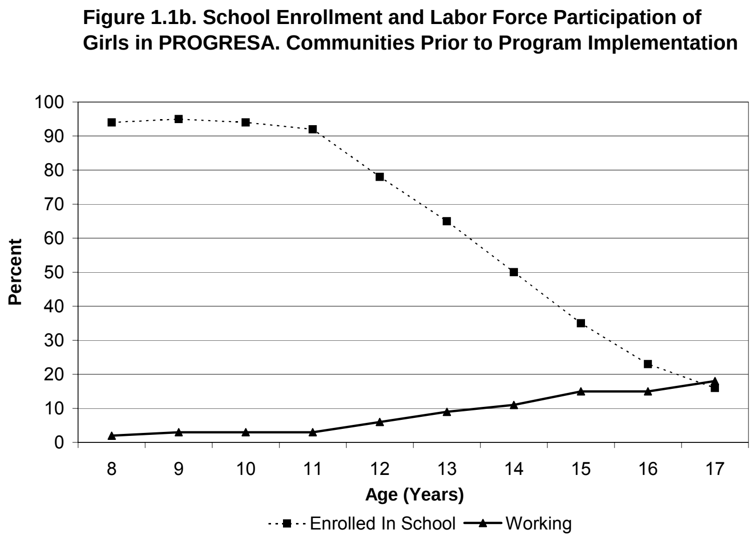
v Positive interactions might exist because there may be complementarities in the sense that having more of one human resource increases the marginal impact of another (e.g., more time in school has greater impact on learning for better nourished and healthier children), which is what is explored in this paper. In addition there may be positive interactions because: (1) there may be economies of scale in terms of program delivery and (2) there may be "spillovers" in the sense that any single component of the program that changes prices that households face or income that they receive (e.g., school scholarships) in general have affects on all other outcomes (e.g., health, nutrition). The latter effect also is embodied in the impact estimates in this paper, as well as in the other available impact evaluation estimates.

vi There are a few studies that have investigated the impacts of nutrition on schooling of younger children using instrumental variable estimators with price shocks [16], sibling characteristics [17, 18], or weather shocks [19] and, subject to the assumptions necessary for the instruments to be valid, have found significant and fairly important effects on age of initial enrollment, grade progression and cognitive test scores. There also have been several examinations of the Institute of Nutrition of Central American and Panama (INCAP)

Figure1.1 School enrollment and labor force participation of children in *PROGRESA* communities prior to program implementation



Source: [6]



Source: [6]

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experimental intervention in four villages in Guatemala that have found significant and fairly substantial effects of the nutritional supplement (*Atole*) on aspects of education at various points in the life cycle, including pre-school cognition, age of starting school, schooling progression rates, schooling attainment, and cognitive test scores in adolescence and in adulthood a quarter of a century after the intervention [20, 21]. The INCAP studies, in fact, are probably the most persuasive evidence of such effects, though there may be a question of how much to generalize from the experience in these four villages in Guatemala.

The basic information that we use in this report to evaluate the *Oportunidades* medium-term impact on education of children aged 0-8 in 1997 is that provided in the ENCEL2003, linked with earlier data, particularly the 1997 pre-program Survey of Household Socio-economic Characteristics (ENCASEH97) data. The strategy of analysis includes direct assessment of the impacts using two different approaches:[vii]

- Difference and difference-in-difference treatment effect estimates using the original treatment starting in 1998 group (T1998) and the original control group with treatment starting in 2000 (T2000) for children in different age ranges in 1997 to investigate the impact of the *differential program exposure* of about 1.5 years for these two groups.

- Matching estimates between those who obtained treatment in 1998 (T1998) and the 2003 matched comparison group that was incorporated into the program in 2004 (C2003) to investigate the impact of *exposure to the program for five and a half years versus no exposure*.

We use these methods to provide new estimates on the medium-term increase in education for children age 0-8 in 1997 due to the program. These estimates enable us to explore some of the fundamental assumptions of the design of the program about the impact of scholarships on education of children and about interactions over time between components of the program package, particularly nutrition for infants and pre-school children and subsequent educational outcomes when those children are of school age.

We organize our study as follows. Section 2 provides some program background. Section 3 introduces the basic data, considers the program impact on sample attrition, which is a necessary prelude to examining the impact on child education because of loss to the longitudinal sample of attritors, and considers some aspects of the sample used for the matching estimates. Section 4 considers the estimated programs impacts using, in turn, each of the approaches indicated above. Section 5 concludes.

2. Program Background

Oportunidades began operating in small rural communities in 1997 and has gradually expanded to urban areas, now covering a total of 5 million families, about one quarter of all families in Mexico. The program has a number of dimensions that might have

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vii A third possibility would be to use regression discontinuity analysis to compare those treated (but from families not too far below the eligibility cutoff) with those ineligible (but not too far above the eligibility cutoff) within original treatment and control groups. While under the original definition of program eligibility, this approach was appropriate (see [22]), changes in the eligibility criteria and incorporation process mean that many of the households who would have formed part of those ineligible originally begin to receive benefits prior to 2000 so this approach is not very promising for the present paper.

affected the educational achievement by 2003 of children then aged 6-14, but who were 0-8 in 1997 immediately before the program was initiated:viii

(1) Cash transfers to the mothers in the treatment households: Conditional on the whole family (including pre-schoolers) regularly visiting health clinics and school-aged children attending school, *Oportunidades* beneficiaries receive bi-monthly cash transfers, equivalent on average to about 20% of household consumption [12]. These cash transfers may have induced increased education for all the children in the treatment group through intrahousehold allocations even though very few of the children in our sample who were 0-8 years of age in 1997 were immediately eligible themselves for their mothers to receive the transfers conditional on our sample children attending the third or higher grade of schooling (see point 5 below). That increased income tends to result in more use of health services is broadly accepted (see [23, 24]). There has been considerable controversy over the extent to which increased income translates into increased nutrient consumption (see [25-29]); but estimates for the *Oportunidades* rural sample indicate that a 10% increase in income translates into a 3 to 4.5% increase in caloric availability, with much of this increase going to foods richer in micro nutrients [30]. While there is not much direct evidence on the intrahousehold distribution of nutrients in the *Oportunidades* population, studies on other poor populations have concluded that larger shares of resources that go to mothers are directed toward child health and nutrition than of resources directed to fathers and in part for this reason *Oportunidades* directs resources to mothers [23, 31-37].[ix] Thus, a priori, we expect all the children in the treatment group to have better health and nutrition because of the cash transfers to mothers alone (ignoring the conditionality), and evidence noted above suggests that in such poor populations this translates into better schooling performance.

(2) Participation in the *pláticas*: *Oportunidades* participants are required to attend regularly meetings (*pláticas)* at which, *inter alia*, health and nutritional issues and practices are discussed. These sessions are conducted by physicians and nurses trained in these specific topics (see [38]). If these meetings improve knowledge and practices related to child nutrition and health, they may improve child nutrition and health, again with indirect effects on improving educational performance. While the beneficiaries might be children (and adults) of any age, given that infants and very young children are most at risk of nutritional insults, these children would seem to be the most likely beneficiaries.

(3) Nutritional supplement or "*papilla*": The nutritional component of *Oportunidades* includes the provision of nutritional supplements to pregnant and lactating women and to children between the ages of four months and two years and to children between two

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viii Families that were eligible for the program were determined primarily by statistical analysis of pre-program characteristics (see [1-3] for details). Eligible households receive all the program components for which they are eligible and for which they satisfy the program co-responsibilities (e.g., they receive the educational grants if and only if children are enrolled in the ages for which these grants are given).

ix The scholarships for upper-secondary schooling that were introduced in 2001 can be received by youth instead of the mothers.

and four years (up to 59 months) if any signs of malnutrition are detected by the clinic personnel.[x] Mothers visit the clinic at least once a month to pick up six packets of supplements per eligible child per month with each pack containing five doses, enough for one dose per day. The supplements constitute 20% of calorie requirements and 100% of all necessary micronutrients and have presentational and flavor characteristics that resulted in high levels of acceptability and intake (see [39, 40]). Therefore children in the treatment sample who were less than 24 months of age at the start of treatment (and 6-8 in 2003) should have been the primary beneficiaries of these supplements, with hypothesized benefits in terms of their cognitive development and school performance.[xi] Children between the age of 2 and 4 were also given the supplements if they showed evidence of malnutrition. In addition, children in the treatment sample who were not direct beneficiaries, but who had younger siblings who were direct beneficiaries, may have benefited from an impact of increased household income in kind that is akin to the cash transfer discussed in (1) above.

(4) Growth monitoring: A prerequisite for receiving nutritional supplements is ongoing growth monitoring of preschool children. Conventional wisdom holds that there is a high payoff to such growth monitoring because it increases substantially the probability that parents or other caregivers become aware of nutritional problems before longer-run damage occurs. The direct beneficiaries, once again, are likely to have been primarily the children under two years of age (because they are most vulnerable to nutritional insults) when the program started – and therefore under eight years of age in 2003.

(5) Direct educational components of the program: A final major component of the program that is obviously germane to our interests in this paper is the conditionality (beyond the income effect noted in point 1) of the cash transfers to mothers for children attending school (or scholarships). Regular school attendance (at least 85% of the time) is required to continue receiving the bi-monthly grant payments. With respect to successfully completing the school year, program rules allow students to fail each grade once, but students are not allowed to repeat a grade twice (at that point educational benefits are discontinued permanently for the child). Note this allows a student theoretically to receive two years of grants for the same grade for each grade. Table 2.1 shows the monthly grant levels available for children between the third grade and the twelfth grade in the second semester of 2003 (in addition there are grants for school supplies). Until 2001, the program provided grants only for children between the third and ninth grade. The secondary and high-school grants provide higher amounts (by about 13%) for girls than boys. The extent to which children in our treatment sample benefited between 1997 and 2003 from the direct inducements for schooling provided

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x These supplements also may be given to children in households not currently receiving *Oportunidades* benefits (including children residing in control localities) if any signs of malnutrition are detected, which has the potential to bias downward the estimated impact of *Oportunidades.*

xi Assessments of initial operational aspects of *Oportunidades* indicated difficulties in making these supplements available in sufficient quantities; both local health institutions and *Oportunidades* field staff raised concerns regarding their physical availability (see [12, 41]).

by these conditional transfers depends on how much schooling they had had prior to the program initiation. In the group that received treatment in 1998, many of the children who were 7 or 8 in 1997 were ready to enter the third grade at the time of program initiation and thus had potentially five and a half years of direct benefits by 2003. For younger children in 1997, there generally was a lag before they were ready to enter third grade and thus they had potentially fewer than five and a half years of benefits by 2003. In addition to the direct benefits, of course, even for younger children the initiation of the program may have created increased incentives for schooling if there is forward-looking behavior regarding future benefits. Also, as noted in point (1) above, there may be income effects for children who live in households with older siblings who were beneficiaries.[xii]

Table 2.1. Monthly amount of educational grant (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

xii In 2003, a new component was introduced to the direct educational support part of the program, called *Jóvenes con Oportunidades*. This new component consists in depositing a certain amount of points (equal to pesos) for each year onward from ninth grade until finishing high school in an account under the youth's (not the mother's) name. When the youth finishes high school, the youth can choose between waiting two years and being able to have the account balances (plus interest) to use as he/she wishes or having immediate access to the funds if they are used to participate in one of the following four initiatives: 1) attend college; 2) purchase a health insurance; 3) get a loan to start a business; 4) apply for public housing. This new scheme is likely to provide further incentives to invest in schooling, but we think it is unlikely to be a major contributing factor to the estimated impacts here given that it was only just being introduced as the 2003 ENCEL survey was being carried out. Moreover it was introduced at the same time for the original treatment and control groups, so it should not have created differential incentives between these two groups (though presumably it may have created differential incentives between these two groups and the new 2003 match control that did not receive this benefit).

Finally, we mention the question of continued program eligibility. All households after three years are subject to recertification, a process by which households receive a visit and their household characteristics are evaluated to see if they continue to be eligible for the program. Those found to no longer be eligible for benefits are transitioned to a modified version of the program (Esquema de Apoyos Diferenciados –EDA–), which continues to include secondary and high school educational grants, but excludes primary school scholarships and cash transfers for food. In practice, however, very few households in our sample of interest have been transitioned to the modified version of the program. For the analysis of this paper, therefore, we concentrate on those households initially eligible for the program and do not exclude households who may have been transitioned to the EDA.[xiii]

3. Basic Data, Sample Attrition in T1998 and T2000, and Some Aspects of C2003

3.1 Sample design

The 2003 Rural Evaluation Survey continues the original treatment and control experimental design begun in 1997. The original sample design involved selecting 506 communities with 320 randomly assigned to receive benefits immediately and the other 186 to receive benefits later.[xiv] Only those households that were determined to be poor through statistical procedures with modifications due to community feedback were eligible to receive benefits (see [1-2]). Sections 3.2 and 4 present analysis between eligible households in the original treatment (T1998) and the original control (T2000) groups. Sections 3.3 and 4 also present analysis between the original eligible treatment (T1998) group and households matched in terms of their characteristics in the 2003 comparison group (C2003). The eligible households in the original treatment localities (T1998) began receiving treatment benefits in the spring of 1998 whereas the eligible households in the original control group (T2000) began receiving benefits at the end of 1999. Between 1997 and 2000, evaluation surveys with detailed information on many evaluation indicators including education, health, income and expenditures were applied to households in both groups every six months.

In the year 2003, a new follow-up round of the rural evaluation survey (ENCEL2003) was carried out. The sample includes eligible and ineligible households in the original treatment (T1998) and original control (or delayed treatment, T2000) groups and a new sample of households from comparison communities that were selected through

xiii A small number of originally eligible households never received program benefits, mostly deriving from their migrating from their community before being informed they were eligible for the program. These households are not included in our analysis.

xiv Due to budget restrictions, the program was phased-in over time. The original 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). The 2003 sample also included a new control group for 2003 (C2003) that was phased-in in 2004.

matching on observed community characteristics from communities that had not had the program by 2003 (C2003) to be otherwise similar to the program communities.[xv] We link the T1998 and T2000 data from 2003 to earlier data sets, particularly the pre-program 1997 ENCASEH data, to have longitudinal data on individual children who were 0 to 8 years of age in 1997 and 6 to 14 in 2003. For the C2003 data we use recall data to characterize their status in 1997 (described further in Section 4.2). As in the previous ENCEL surveys, the ENCEL2003 contains data on a myriad of program outcomes, including several indicators of education.xvi

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

3.2 Attrition of children in the original T1998 and T2000 households.

Some researchers (see [44]) have questioned whether the gains from collecting longitudinal data are worth the costs. One problem in particular that has concerned analysts is that sample attrition may lead to selective samples and make the interpretation of estimates problematic. 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* (Spring 1998) on "Attrition in Longitudinal Surveys" (for related statistical literature on missing values and survey non-response see, for instance, [45, 46]). The striking result of the studies presented in this special *JHR* issue is that the biases in estimated socioeconomic relations due to attrition are small despite attrition rates as high as 50% and significant differences between those re-interviewed and those lost to follow-up for many important characteristics. For example, [47] summarize:

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xv For details concerning the matching on a limited set of observed community characteristics that was used to select this sample, see [42]. Note that this matching is to establish a good sample for comparison, in the absence of an ongoing experimental design. This should not be confused with the matching estimates between children in the T1998 and C2003 samples that are discussed in Section 4 below that use a much more extensive set of observed characteristics for the matching.

xvi 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 attend and youth in the matched comparison sample (C2003) are likely to attend. These test scores are analyzed for rural youth aged 15 to 21 in 2003 (9 to 15 in 1997) in [9] and are analyzed for children aged 24-72 months in 2003 in another 2004 technical evaluation document [43], but they are not available for analyses of the age group covered in the present paper.

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

The other studies in this special issue of the *JHR* further confirm these findings for the PSID or reach similar conclusions for other important panel data such as the Survey of Income and Program Participation (SIPP), the National Longitudinal Surveys of Labor Market Experience (NLS), and the Labor Supply Panel Survey in the Netherlands [48-52]. Similar results are presented for three developing country longitudinal data sets in [53].

While such results suggest that attrition may not be as much of a problem as often has been claimed, they also suggest that it is important to examine whether attrition is a problem in any particular study. In the present case, sample attrition can cause problems for our analysis if the sample attrition is not independent of the program effects because in such a case it changes the composition of the treatment sample differently than the composition of the control sample. For the estimation of the impact of differential program exposure, we are concerned with sample attrition in the sense of individuals who were in the sample in 1997 but not in the sample in 2003. [xvii]

Table 3.1 (panel A) summarizes some statistics regarding sample attrition in this period for the original treatment (T1998) and original control (T2000) groups, focusing first on all youth in the community and then on those eligible for the program under the original program definition (*pobre*) and the modified program definition (*pobreden*).[xviii] The numbers in this table are striking. Basically 23% of the individuals aged 0 to 8 in 1997 were not in the sample six years later, which certainly is a large enough proportion to suggest the need to be concerned with attrition (though not as large as the 41% in the older group of rural youth aged 9 to 15 in 1997 that is considered in [9] because for the younger age range of interest in the present paper relatively few individuals have been lost to the sample because they have left their parental households to migrate away for work, schooling, or marriage). However there are not large or statistically significant

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[xvii] 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.

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

differences at the 10% level in overall attrition between the T1998 and T2000 samples, The proportion lost to follow-up is about the same for girls (21% in T1998, 19% in T2000) as for boys (20% in both T1998 and T2000); only for girls is there a statistically significant difference between T1998 and T2000 for overall attrition at the 10% level. So on an overall aggregate level it appears that sample attrition is not significantly associated with treatment at least for boys, though attrition is higher at the 10% significance level for the T1998 than the T2000 group for girls.

The consideration of more disaggregation reveals some further systematic patterns in attrition related to treatment. Overall attrition of individuals aged 0 to 8 in 1997 includes: (i) individuals who have separated from households that are still in the sample in 2003 and (ii) individuals who are in households that are no longer in the sample in 2003. About 18% of those lost to follow-up are individuals in households that left the sample. There are some significant differences if individual and household attrition are considered separately: at the 10% level, higher individual attrition among the T2000 group for boys and higher household attrition among the T1998 group for girls. So, while the overall treatment-control differences are not significant for boys, greater disaggregation suggests some differences.

Table 3.1. Proportion attriting by 2003 of original ENCASEH sample: individuals 0 to 8 in 1997

	Treatment (T1998)		Control (T2000)		P>|Z|
	N	Mean	N	Mean	
A. Total proportion attriting (individual or household)					
0 to 8 years (all)	19,493	0.232	12,055	0.229	0.564
0 to 8 years (poor using original definition)	14,610	0.205	8,783	0.199	0.236
0 to 8 years (poor using pobreden)	17,356	0.212	10,602	0.205	0.131
By gender					
Boys 0 to 8 years (poor using original definition)	7,434	0.203	4,393	0.203	0.993
Girls 0 to 8 years (poor using original definition)	7,168	0.208	4,378	0.194	0.068
B. Proportion due to individual attrition					
0 to 8 years (all)		0.052		0.050	0.504
0 to 8 years (poor using original definition)		0.041		0.045	0.238
0 to 8 years (poor using pobreden)		0.047		0.048	0.659
By gender					
Boys 0 to 8 years (poor using original definition)		0.038		0.045	0.096
Girls 0 to 8 years (poor using original definition)		0.045		0.044	0.961
C. Proportion due to household attrition (individual not found because household -HH- moves)					
0 to 8 years (all)		0.181		0.180	0.802
0 to 8 years (poor using original definition)		0.164		0.154	0.051
0 to 8 years (poor using pobreden)		0.166		0.157	0.055

By gender			
Boys 0 to 8 years (poor using original definition)	0.165	0.158	0.375
Girls 0 to 8 years (poor using original definition)	0.163	0.149	0.047

Notes: The last column gives the significance level for mean differences between T1998 and T2000 based on t-tests.

Therefore we have undertaken analysis of the probability of being lost to follow-up for individuals 0 to 8 years old in 1997 in eligible households from the T1998 and T2000 groups – again, for total attritors, individual attritors and household attritors. For each of these three dependent variables, we have estimated two specifications: (1) only whether in T1998 group and (2) whether in T1998 group plus interactions between being in the T1998 group and pre-program individual characteristics, parental characteristics and housing characteristics. We have undertaken such estimates for boys and girls together and separately. Appendix B gives these estimates. Specification (1), not surprisingly, replicates the patterns noted with regard to Table 3.1. Specification (2) indicates that a number of the pre-program individual, parental and housing characteristics interact significantly with treatment (i.e., being in the T1998 group) to affect attrition.

Thus, though in the aggregate there is not evidence of significant impacts of the differential timing of treatment on attrition for boys, the timing of treatment appears to be significantly positively (i.e., more attrition for T2000 than for T1998) associated with individual attrition for boys and significantly positively associated with household attrition for girls (i.e., less attrition for T2000 than for T1998) – and there are a number of significant interactions with individual, parental and housing characteristics (differing in many cases for boys versus girls). Therefore biases may result if we do not correct for attrition in our estimates in the next section – so we do correct by re-weighting observations to counter the effects of differential attrition.

3.3 Characteristics of the C2003 sample used for the matching estimates with the T1998 group

The outside comparison group C2003 that we use for the matching estimates in Section 4 consists of households living in rural areas in which *Oportunidades* was not yet available in 2003. These localities were selected using propensity score matching (based on locality level aggregate data) to match the characteristics of the localities where the program was available according to aggregate locality measures of characteristics, as is described in detail in [42]. The respondents in the households in the C2003 sample were not only asked about their characteristics and behaviors in 2003, but also were asked recall information about their pre-program characteristics and behaviors in 1997 to be used in the matching estimates (see Section 4.1 below).

Such recall information, of course, generally is subject to greater measurement error than information about current characteristics and behaviors. It would be desirable, if possible, to compare the T1998 and C2003 samples with regard to information particularly on education for the individuals of interest for this study, children age 0-8 in 1997. However because most of these children were of pre-school age in 1997, such possibilities are limited.

Ideally, matching would allow us to balance our samples prior to the program, that is, ideally after matching, no pre-program differences would be observable between the treated and matched comparison group. Nevertheless, Appendix D shows estimates of "pre-program" differences for the two outcome indicators for those age groups for which we are able to carry them out (grades of total schooling and whether ever failing a grade). As in [9], children in the matched comparison group show higher levels of education than those in T1998. This implies that despite the efforts to establish a good comparison sample in C2003 though matching at the level of community characteristics, the C2003 group is from a context in which pre-program schooling was higher than for the T1998 group. Difference-in-difference matching might control for such differences in these contexts, e.g. these pre-program differences likely reflect unobserved characteristics associated with education levels and program impacts, which can be differenced out when pre-program information is available. But we are able to use difference-in-difference estimates for schooling outcomes only for the oldest children in the sample relevant for the present paper because only they were of school age prior to the program in 1997. xix

Given that for younger age groups we are unable to control for pre-program education differences, we propose an alternative, which is, to use as pre-program differences, the mean education values for the group of children aged 6 to 14 in 1997 (e.g. where the after-program-initiation differences reflect children aged 6 to 14 in 2003). This allows us to carry out difference in difference estimators, on the assumption that differences in the educational outcomes of children 6 to 14 in 1997 are representative of pre-program differences or predicted pre-program differences of children 6 to 14 in 2003. This methodology is only possible for those indicators on which there was information collected in 1997 for the new comparison group (in the current context, grades of schooling and ever failing a grade). These results are included in Appendix E.

4. Program Impact Estimates on Education of Children 0-8 in 1997

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xix For children for whom we are able to construct difference in difference matching estimates by following them over the panel, one might inquire as to potential attrition of the sample. Our paper in [9]) showed that for those aged 9 to 15 in 1997, much fewer youth left the household in the new comparison group than in the original treatment group. There, we argued that while possible the trends partially reflected program impacts, the estimated impacts were too large to be plausible and likely reflected under-reporting in the new comparison group of who was actually in the household in 1997. In the current context, there are again some differences in attrition (or underreporting), but to a much lesser extent (e.g. less than 5% attrition for both groups in the relevant ages).

We now turn to the program impact estimates of the two types noted in the introduction:

- Difference and difference-in-difference treatment effect estimates using the original treatment and control groups for children in different age ranges in 1997 to investigate the impact of the *differential program exposure* of about 1.5 years for these two groups.

- Matching estimates between those who obtained treatment in 1998 and the 2003 matched comparison group to investigate the impact of *exposure to the program for five and a half years versus no exposure*.

These are both medium-term impact estimates in the sense that the "treated" group in each case has received the program for a substantial length of time (about five and a half years), but involves two different comparisons. The first method gives the effects in 2003 of different exposure to the program between 1998 (T1998) and 2000 (T2000). The second method gives the medium-term effects in 2003 of program exposure starting in 1998 (T1998) versus no program exposure to date (C2003).

We first describe in somewhat greater detail these two types of estimates. We then consider in turn the estimated impact through using these two methods of the *Oportunidades* program on a series of educational outcomes for children age 0-8 in 1997 and 6-14 in 2003 living in the small communities in which the rural program was first introduced. The program is hypothesized, as noted in Section 2, to increase the amount of education through improving the pre-program child development, including nutritional and related development, and through reducing the costs of schooling school-aged children (at least once they enroll in third grade) to families, which may operate through affecting a variety of behaviors – including school enrollment, failure of grades in school, dropping out of school, and grades of school attainment. In Section 4.2, we examine a number of such possibilities in turn.

Section 4.1 Two approaches to estimating program impacts.

<u>Comparison of T1998 and T2000 using difference and difference-in-difference estimates</u>: The first set of estimates that we present are difference and difference-in-difference treatment effect estimates that use the original treatment and control groups for children in different age ranges in 1997. The basic idea here is to exploit the experimental design of assignment to treatment in 1998 (T1998) versus assignment to treatment in 2000 (T2000) to evaluate whether the *differential exposure* to treatment between the two groups had impacts on education as of 2003. The difference estimates are obtained basically by subtracting the T2000 value of a variable of interest in 2003 from the T1998 value in 2003. The difference-in-difference estimates are obtained basically by subtracting the difference between the 2003 and 1997 values for a T2000 variable of interest from the difference between the 2003 and 1997 values for the same T1998 variable. Our prior, as noted in Sections 1 and 2, is that the extent of the

differences in impacts may depend critically on child ages in 1997, prior to the initiation of the program for the T1998 group. In particular, we hypothesize that there may be substantial effects of treatment for those children who in 1997 were (a) exposed to the nutritional supplements and growth monitoring for infants (were 0-2 in 1997 and therefore 6-8 in 2003) or (b) were 6-8 in 1997 and therefore 12-14 in 2003 and had attained the critical age for making the marginal schooling regarding enrolling in lower secondary school. We also hypothesize that there may be other important differences in effects by age because, for example, those T1998 children who entered the third (or higher) grade in 1998 or 1999 received two years of direct scholarship treatment that the same schooling (and, approximately, birth) cohort in T2000 did not receive. On the other hand, those T1998 children who entered the third grade in or after 2000 (children approximately aged 3 to 5 in 1997) received the same direct scholarships for school attendance as the T2000 children in the same schooling cohort (though the former may have received other benefits, such as the cash transfers discussed in Section 2 for two years longer). That is, we hypothesize that children aged 3 to 5 are likely to show lower effects of the program, given there are no likely differences in terms of nutrition interventions or education grants for this age group.

We carry out regression analysis where the program impact is captured through a dummy variable measuring whether the individual resided in a T1998 versus T2000 locality (interacted with the year for the cases where difference in difference estimators, e.g. before and after are carried out.) We carried out both simple regressions only controlling for the impact variables, as well as additional specifications where additional control variables were added to the regression, which may reduce the standard errors of the estimated program effects. These control variables include parental age, education, indigenous status, and household characteristics (number of rooms, electricity, type of floor and water/sewage system).

We present in the rows for these estimates in Tables 4.2, 4.4, 4.5, 4.7. 4.9 and 4.11, by gender, separate estimates by the relevant ages in 1997 (with the implied ages in 2003 indicated as well). The first column gives the value for the relevant variable for the T2000 group (which is of interest as an estimate of what would have happened without the additional exposure to the program that the T1998 group had), the second and third columns gives the estimated differential treatment impact 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. The table notes indicate additional controls from 1997 that were used because the samples were not perfectly balanced in all cases between the T1998 and T2000 samples in 1997. Generally, as noted in the tables, difference-in-difference estimates are possible only for children in the 6-8 age range in 1997 (because only for such children are there observations related to schooling in 1997: the other children were too young). For younger children the tables give difference estimates for 2003 (though still with controls for pre-program characteristics in 1997).

<u>Comparison of T1998 and C2003 using matching estimates</u>: Our second set of estimates evaluate impacts in 2003 of exposure to the program since 1998 versus no

exposure as of 2003 using matching methods. For each individual in the treatment group (T1998), these methods select a comparable individual from the outside comparison group. The outside comparison group C2003 that we use consists of households living in rural areas in which *Oportunidades* was not yet available in 2003. These localities were selected using propensity score matching (based on locality level aggregate data) to match the characteristics of the localities were the program was available according to aggregate locality measures of characteristics (but see Section 3.3 for some qualifications).

There are a number of alternative methods for selecting the comparable individual in the control group. We have explored alternatives and found that the basic results are robust to these alternatives. Therefore we use for the estimates that are reported in this paper the "nearest neighbor" method because it is the simplest and is available in at least one widely-used software (e.g., STATA). An extensive discussion of this and alternative matching methods and how they can be implemented within the particular context of the project is provided in [42]; Appendix C to this paper provides a briefer discussion.

Because we use for the comparison group data households living in localities where the program is not yet available, these households are unlikely to be affected by the existence of the program. However, the data are drawn from different geographic areas from the treatment group and therefore the controls may experience different local area effects (labor market conditions, quality of schooling, quality of health clinics, prices) that may be relevant determinants of the outcomes of interest. Difference-in-difference matching methods provide a way of taking into account unobserved fixed locality characteristics. These methods compare the change in outcomes in the treatment group (post-program minus baseline) to the change in outcomes in the control group. However, as noted above, such methods are possible only for the children already of school age in 1997. For children who were of pre-school age in 1997, we cannot use difference-in-difference matching estimates to control for fixed locality characteristics, but only difference matching estimates. This makes the estimates for this age range somewhat problematic because the C2003 group comes from communities that are different than the T1998 group, which is why the latter but not the former had received the program (again, see Section 3.3). Therefore, as a check on our results, we also carry out results using the mean educational indicators of children aged 6 to 14 in 1997 as a proxy for pre-program differences for grades of schooling and the proportion ever failing a grade (Appendix E).

To implement the matching estimates, we need to estimate the propensity for treatment based on observed pre-program characteristics for the T1998 that also are available, based on recall, in the C2003 group. These variables include demographic characteristics of the households in 1997, schooling of household head and spouse in 1997, whether the household head and spouse spoke indigenous language in 1997, whether the household head and spouse are employed, a number of household characteristics and consumer and production durables in 1997, the puntaje score income in 1997 (that was generated from statistical analysis to classify whether households were eligible for the program) and state of residence in 1997. Table 4.1 gives the estimates, which have fairly good predictive power (pseudo R squared = [0.58]). The estimated relation then is used to generate propensity scores for households in the C2003 sample. Figures 4.1 a and b gives the distribution of propensity scores in the original treatment group (T1998) and the distribution of propensity scores in the C2003 comparison group. Although a number of households in C2003 have characteristics that make them poor matches for households in the T1998 (i.e., they have very low propensity scores), there is adequate support in the sense of a number of households in C2003 that have propensity scores similar to those in T1998.

Table 4.1. Probit Model for Probability of Participating in Rural *Oportunidades*
D=1 Original poor households in T1998
D=0 Poor households not yet in program in C2003

Variable	Coef.	Std. Err.	z	Variable	Coef.	Std. Err.	z
Age of Household head	0.002	1.640	0.101	Total HH income squared	0.000	0.000	1.320
Age of Spouse	-0.004	0.003	-1.650	Blender	-0.011	0.052	-0.210
Gender of Household head	0.644	0.090	7.170	Refrigerator	0.183	0.091	2.020
Hh head speaks indigenous lang.	0.274	0.066	4.160	Gas stove	0.576	0.065	8.910
Spouse speaks indigenous lang.	0.267	0.072	3.720	Gasheater	0.250	0.129	1.950
Grades schooling HH head	-0.058	0.008	-7.320	Radio	0.109	0.035	3.070
Grades schooling spouse	-0.129	0.009	-15.100	Television	0.150	0.043	3.450
Employed HH head	-0.542	0.075	-7.230	Video	0.304	0.142	2.140
Employed spouse	-0.445	0.054	-8.300	Washer	0.603	0.154	3.920
Children 0 to 5	-0.393	0.023	-16.900	Car	-0.098	0.177	-0.550
Children 6 to 21	-0.265	0.024	-10.950	Truck	0.048	0.126	0.380
Children 13 to 15	0.036	0.030	1.190	State1	1.144	0.116	9.820
Children 16 to 20	0.060	0.024	2.480	State2	0.630	0.080	7.870
Women 20 to 39	0.086	0.058	1.500	State3	0.768	0.084	9.150
Women 40 to 59	-0.104	0.048	-2.160	State4	0.341	0.080	4.270
Women 60+	-0.379	0.045	-8.450	State5	0.382	0.080	4.790
Men 20 to 39	0.021	0.038	0.560	State6	-0.463	0.075	-6.130
Men 40 to 59	-0.495	0.050	-9.810	Missing grades schooling HH head	-2.156	0.080	-26.970
Men 60+	-0.727	0.056	-13.100	Missing grades schooling spouse	-2.352	0.116	-20.240
# Rooms	0.238	0.020	12.020	Missing age HH head	0.937	0.853	1.100
Electricity in HH	-	0.040	-6.100	Missing age spouse	-0.836	1.321	-0.630

	0.242						
Water in HH	-0.367	0.039	-9.430	Missing indig HH head	1.974	1.387	1.420
Dirt floor	-0.583	0.048	-12.050	Missing working HH head	1.045	2.219	0.470
Room material (inferior)	0.197	0.043	4.610	Missing working spouse	1.087	0.409	2.660
Wall material (inferior)	0.135	0.046	2.940	Missing water	-0.336	0.468	-0.720
Own animals	0.198	0.040	4.960	Missing electricity	0.404	0.522	0.770
Own land	0.223	0.037	6.080	Missing rooms	0.371	0.345	1.070
Score	1.632	0.108	15.120	Missing income	-2.203	0.762	-2.890
Score squared	-0.115	0.015	-7.630	Missing ownanimals	-0.609	0.534	-1.140
Total HH income	0.000	0.000	-16.480	Missing ownland	0.824	0.523	1.570
				Constant	-1.658	0.242	-6.870
Number of obs	13336						
LR chi2(60)	10591			Pseudo R2	0.584		
Prob > chi2	0.000			Log likelihood	-3771.644		

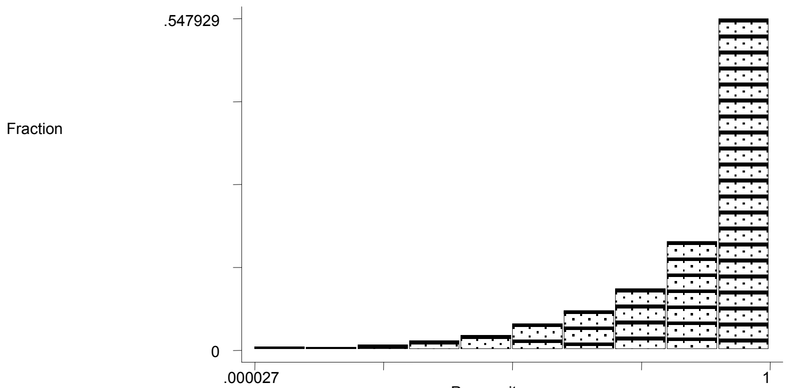

Figure 4.1.a Distribution of Propensity Score for Original Treatment98

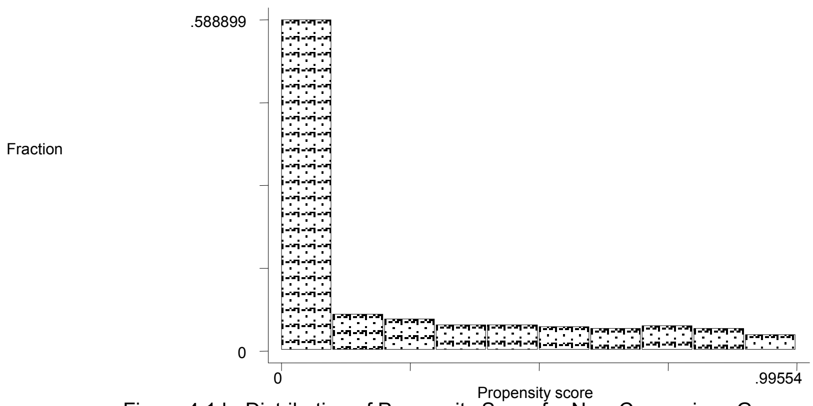

Figure 4.1.b. Distribution of Propensity Score for New Comparison Group

The tables for the matching estimates between T1998 and C2003 below present by gender and age the C2003 value of the variable of interest, the estimate for the program impact on this variable, the standard error and significance level of the estimates, the number of observations on which the estimates are based, and the percentage change implied by the impact estimate relative to the value with no program exposure for the C2003 group. For the indicators for which it is possible (e.g. grades of schooling and proportion ever failing), we report difference in difference matching estimates both from using pre-program data from the same group of children and from those using children aged 6 to 14 in 1997. This second methodology provides an alternative estimator, useful for the ages (less than 6) for which we have no pre-program information as these children were too young to have been in school. For our other indicators, e.g. progressing through school and age of entry to primary school, there is no obvious manner or data with which to construct pre-program equivalents, although progressing through school has some of the quality of difference-in-difference estimates (i.e., differences in experiences over a time interval).

4.2 Estimated impacts on educational indicators for children age 0-8 in 1997

<u>Age at starting school (Tables 4.2, 4.3)</u>: If infants and very young children have better nutrition and health, they tend to develop physically and cognitively more rapidly, and therefore may be sufficiently mature to enter school when younger. Several studies have found significant effects of pre-school nutrition on age at starting school in countries as varied as Ghana, Pakistan, the Philippines and Zimbabwe (see [16-19, 54]). If starting schooling earlier permits completing a given level of schooling when younger and expanding the number of post-schooling years in which to reap any productivity gains from schooling, the impact over the life-cycle can be considerable (see [54]).

For Mexico the potential gains from lowering the age of starting school are perhaps less than in many countries because the majority of children are in school at age six, the legal starting age (Figure 4.2). But it still is of interest to explore whether there is some evidence of a significant program impact on age of starting school. We are able to explore this question for children aged 1 and 2 in 1997 because for children of other ages we do not have information on the age at which they started school (see Appendix A). The differential exposure estimates in Table 4.2 (though not the matching estimates in Table 4.3) indicate a significant (at the 5% level) reduction in the age of starting school of -0.09 years or 1.5% for girls (but not for boys).These results are thus consistent with some potential effects, but not very large ones, in reducing the age when children enter primary school.

Table 4.2. Impact of Differential Exposure of Oportunidades on Age at starting School
Difference Estimates:
T1998 versus T2000.

		Impact		
Age in	**Average age starting**	**Coefficient**	**Std. error**	**% change relative**

1997	2003	School T2000			to T2000 group
Girls					
1	7	6.08	-0.049	[0.047]	-0.8%
2	8	6.17	-0.092	[0.046]**	-1.5%
Boys					
1	7	6.12	0.031	[0.045]	0.5%
2	8	6.25	-0.035	[0.047]	-0.6%

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

Table 4.3. Medium-Term Impacts of Oportunidades on Age at Starting Primary School
Matching estimates: T1998 versus C2003 §

Age in 1997	2003	Average age Starting school C2003	Impact Estimate	Standard Error +	Number of Observations §§	% change relative to C2003 group
Girls						
1	7	6.11	0.003	0.196	779	0.0%
2	8	6.27	-0.168	0.127	816	-2.7%
Boys						
1	7	6.26	-0.014	0.084	771	-0.2%
2	8	6.22	0.125	0.093	835	2.0%

Note: Estimates based on difference matching estimates for children 1 to 2 in 1997.
§ Nearest neighbor (5) matching. Estimator imposes common support.
§§ Treatment and control observations.
+ Standard errors based on bootstrap with 500 replications.

School enrollment in 2003 (Table 4.4, Figure 4.2): Figure 4.2 shows trends in enrollment for children of our sample in 2003 and comparable trends for children of the same age group, six years earlier in 1997. It is noteworthy, that whereas there are few differences in enrollment between the T1998 and T2000 groups in both years, the children in 2003 show much higher levels of enrollment by age than those children of the same age group in 1997. This reflects important increases in enrollment by cohort over time, some of this cohort increase may be due to the program but this cannot be isolated from other factors affecting enrollment.

Turning to program impact estimates, the T2000 group had fairly high enrollment rates in 2003: 0.95 or higher for children then aged 6-8 in 2003 (0-2 in 1997), but somewhat lower for children then aged 12-14 in 2003 (0.88 for girls aged 12, 0.82 for girls aged 13, and 0.73 for girls aged 14 – and a little higher for boys). Perhaps because of the relatively high enrollment rates for the T2000 group, there is no evidence of a significantly positive impact on enrollment due to the greater exposure of the T1998 coefficient. Indeed, the only significant difference (at the 10% level) suggests a puzzling -3.9% reduction in enrollment rates in 2003 for girls aged 6 in that year.xx

Figure 4.2. Proportion Attending School in 1997 and 2003 by age in 1997

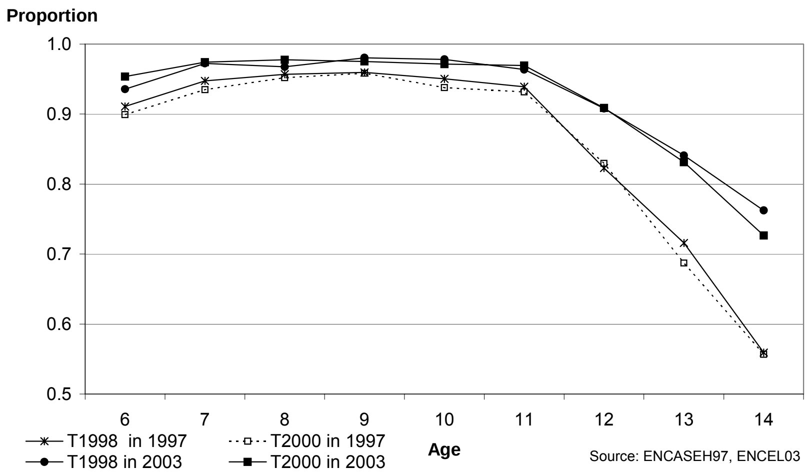

Table 4.4. Impact of Differential Exposure of Oportunidades on School Enrollment in 2003
T1998 versus T2000.

Age in 1997	Age in 2003	Proportion enrolled of T2000 group	Impact: Coefficient	Impact: Std. error	Impact: % change relative to T2000 group
Girls					
0	6	0.950	-0.037	[0.021]*	-3.9%
1	7	0.972	-0.015	[0.013]	-1.5%
2	8	0.970	0.002	[0.011]	0.2%

xx Unfortunately, school enrollment in 1997 was not captured as part of the retrospective information applied to the C2003 comparison group. This we find quite problematic for carrying out matching impact estimators, given results in Appendix D showing the comparison group to overall have higher levels of schooling in 1997. We do not think it is appropriate to carry out difference estimates assuming no pre-program differences between the T1998 and C2003 groups in enrollment, thus we do not report matching estimates of program impact on current enrollment.

3	9	0.979	-0.002	[0.010]	-0.2%
4	10	0.965	0.014	[0.010]	1.5%
5	11	0.972	0.020	[0.031]	2.1%
6	12	0.881	-0.004	[0.028]	-0.5%
7	13	0.817	0.023	[0.029]	2.8%
8	14	0.726	-0.001	[0.032]	-0.1%
Boys					
0	6	0.956	-0.003	[0.017]	-0.3%
1	7	0.964	0.001	[0.014]	0.1%
2	8	0.983	-0.017	[0.011]	-1.7%
3	9	0.978	0.002	[0.009]	0.2%
4	10	0.982	-0.006	[0.009]	-0.6%
5	11	0.978	-0.04	[0.029]	-4.1%
6	12	0.915	0.001	[0.026]	0.1%
7	13	0.865	-0.019	[0.026]	-2.2%
8	14	0.756	0.002	[0.029]	0.3%

Note: Estimates based on difference in difference regression estimates for children 5 to 8 in 1997, children 0 to 2 in 1997 use only 2003 data. 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 10% level.

Whether ever failed a grade in school as of 2003 (Tables 4.5, 4.6, Figure 4.3):

Here, we concentrate on those of primary school age only (e.g. 1 to 5 in 1997 or 6 to 11 in 2003). Failing a grade is conditional on enrolling in school, thus the observed impact of *Oportunidades* on failure reflect changes in who enrolls in school as well as the direct effects on reducing failure for children whose enrollment was not affected by the program. However, at the primary level it is likely that program impacts on enrollment are quite low so that the compositional effects are likely small. For children who by 2003 were of secondary school age, however, this is no longer the case, for this reason we exclude children of this age group (e.g. 6 to 8 in 1997, 12 to 14 in 2003) from this analysis.

The proportions who had ever failed a grade in the C2003 group increase substantially with age (with the exception of girls age 8 in 1997), and are higher for boys than for girls. A majority of the coefficients for greater exposure to *Oportunidades* in Tables 4.6 and 4.7 are negative, suggesting that greater exposure tends to reduce grade failure, But the estimates are fairly imprecise in the sense that the standard errors are large relative to the impact point estimates and only for boys age 5 in 1997 (11 in 2003) is significantly non-zero (for both sets of estimates, implying a 21% decline using the differential exposure estimates and a 46% decline in the proportion who had ever failed using the matching estimators). Thus the evidence is fairly limited, but suggests, if anything a beneficial program effect of reducing failure rates.

Figure 4.3. Proportion Ever Failing in 1997 and 2003 by age in 1997

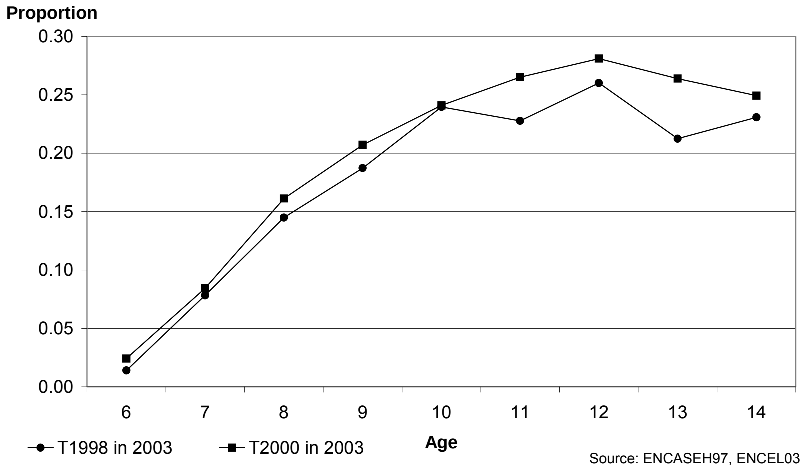

Table 4.5. Impact of Differential Exposure of Oportunidades on Ever Failing
Difference Estimates:
T1998 versus T2000.

		Impact		
Age in	Prop. Ever failing	Coefficient	Std. error	% change relative

1997	2003	of T2000 group			to T2000 group
Girls					
1	7	0.064	0.015	[0.012]	23.3%
2	8	0.116	-0.020	[0.023]	-17.3%
3	9	0.153	-0.009	[0.024]	-5.9%
4	10	0.214	-0.030	[0.026]	-14.0%
5	11	0.196	-0.006	[0.026]	-3.1%
Boys					
1	7	0.086	-0.022	[0.026]	-29.0%
2	8	0.161	-0.022	[0.028]	-16.2%
3	9	0.244	-0.028	[0.028]	-11.5%
4	10	0.272	0.007	[0.029]	2.6%
5	11	0.325	-0.068	[0.030]**	-20.9%

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

Table 4.6. Medium-Term Impacts of Oportunidades on the Probability on Ever Failing a Grade
Matching estimates: T1998 versus C2003 §

Age in 1997	2003	Proportion C2003 Ever Fail 2003	Impact Est	Std. Error +	Number Obs §§	% change relative to C2003 group
Girls						
1	7	0.057	0.013	[0.035]	766	22.0%
2	8	0.146	-0.006	[0.051]	889	-4.4%
3	9	0.134	0.037	[0.040]	850	27.9%
4	10	0.211	-0.043	[0.060]	964	-20.6%
5	11	0.205	0.044	[0.042]	902	21.4%
Boys						
1	7	0.101	0.011	[0.047]	802	10.9%
2	8	0.168	-0.048	[0.066]	919	-28.8%
3	9	0.226	0.063	[0.045]	800	27.8%
4	10	0.309	-0.004	[0.070]	1030	-1.2%
5	11	0.305	-0.141	[0.072]*	949	-46.2%

Note: Estimates based on difference in difference matching estimates for children 5 to 8 in 1997, other children use only 2003 data, e.g. assume had not yet failed a grade in 1997.
§ Nearest neighbor (5) matching. Estimator imposes common support.
§§ Treatment and control observations.

+ Standard errors based on bootstrap with 500 replications.
* Estimates significant at the 10% level.

Progressing on time through school grades as of 2003 (Tables 4.7, 4.8, Figure 4.4): Children who progress through school on time are defined to be those who progress one grade each year starting at age seven. (We exclude six year olds from this analysis as few have completed a year of schooling). The proportions who progress on time by this definition are considerably less than 1.0. For example for the C2003 group, the proportions range from 0.33 to 0.63 for boys and from 0.37 to 0.67 for girls (see Table 4.8). The differential exposure to *Oportunidades* had significant (at the 5% level) and fairly substantial impact on increasing the proportion of both girls and boys aged 7 in 1997 and 13 in 2003 who had progressed on time through six grades since 1997 – by 17.3% for girls and 18.2% for boys (Table 4.7) as well as boys aged 4 in 1997 (11 in 2003) by a significant 3.1%. The matching comparisons between T1998 and C2003, with the differential program exposure effectively of five and a half years, suggest more extensive effects (presumably due to the greater difference in exposure), with 13 of the 16 coefficient estimates positive and six of the positive coefficient estimates significantly non-zero at the 10% level (versus one of the negative estimates). The significant positive estimates, moreover, suggest fairly large effects: for boys, increases of 26% to 86% and for girls, increases of 44% to 46%. (Table 4.8).

Figure 4.4. Progressing on time in 2003 by age in 1997

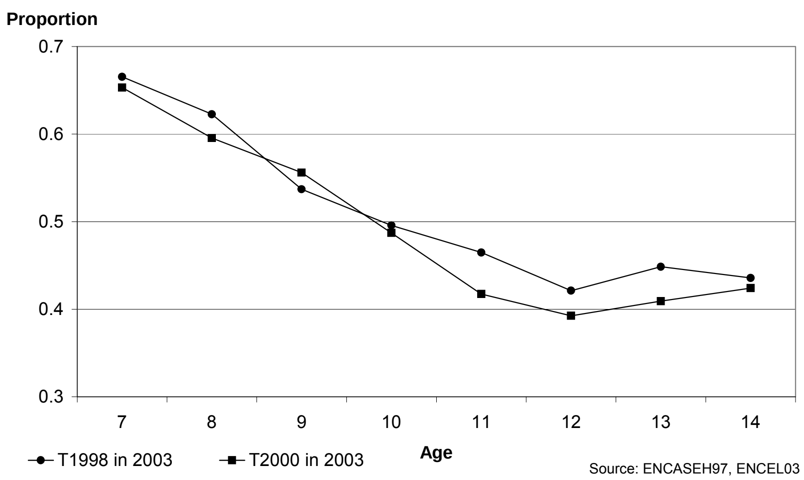

Table 4.7. Impact of Differential Exposure of Oportunidades on Progressing on Time
Difference Estimates
T1998 versus T2000.

			Impact		
Age in 1997	2003	Proportion progressing on time of T2000 group	Coefficient estimate	Standard error	% change relative to T2000 group
Girls					

1	7	0.793	0.014	[0.029]	1.8%
2	8	0.665	0.037	[0.033]	5.6%
3	9	0.589	-0.035	[0.032]	-5.9%
4	10	0.539	0.005	[0.031]	0.9%
5	11	0.508	0.006	[0.032]	-1.2%
6	12	0.406	0.007	[0.031]	1.7%
7	13	0.480	0.083	[0.033]**	17.3%
8	14	0.444	-0.027	[0.033]	-6.1%
Boys					
1	7	0.729	-0.001	[0.032]	-0.1%
2	8	0.607	0.024	[0.033]	4.0%
3	9	0.517	-0.010	[0.032]	-1.9%
4	10	0.456	0.014	[0.031]	3.1%
5	11	0.383	0.064	[0.032]**	16.7%
6	12	0.367	-0.014	[0.032]	-3.8%
7	13	0.441	0.080	[0.031]**	18.2%
8	14	0.403	-0.014	[0.032]	-3.5%

Note: Progressing on time = proportion completing 6 additional grades of schooling between 1997-2003 for those 6 to 8 in 1997, =proportion completing one year in 2003 for 1 year olds and 2 grades for 2 year olds.
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 5% level.

Table 4.8. Medium-Term Impacts of Oportunidades on Proportion Progressing through School on Time
Matching estimates: T1998 versus C2003 §

Age in 1997	Age in 2003	C2003: Proportion Progressing on Time	Impact Estimates	Standard Error §	Number of Observations §§	% change relative to C2003 group
Girls						
1	7	0.587	0.259	[0.081]**	983	44.1%
2	8	0.674	0.020	[0.065]	927	3.0%
3	9	0.616	-0.002	[0.071]	889	-0.3%
4	10	0.551	0.024	[0.071]	993	4.4%
5	11	0.545	-0.132	[0.079]*	933	-24.3%
6	12	0.538	0.096	[0.071]	943	17.8%
7	13	0.390	0.056	[0.071]	981	14.4%
8	14	0.366	0.168	[0.086]*	836	45.9%
Boys						
1	7	0.560	0.157	[0.090]*	980	28.0%
2	8	0.629	-0.064	[0.075]	971	-10.2%
3	9	0.527	0.138	[0.069]*	837	26.1%

4	10	0.478	0.008	[0.087]	1070	1.6%
5	11	0.492	0.017	[0.072]	973	3.4%
6	12	0.565	0.009	[0.065]	912	1.6%
7	13	0.379	0.192	[0.072]**	942	50.7%
8	14	0.330	0.285	[0.068]**	1,051	86.4%

Note: Progressing on time = proportion completing 6 additional grades of schooling between 1997-2003 for those 6 to 8 in 1997, =proportion completing one year in 2003 for 1 year olds in 1997, 2 grades for 2 year olds etc.
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 5% level.
* indicates significance for a t-test at 10% level.

Grades of schooling completed (Tables 4.9, 4.10, Figure 4.5): Figure 4.5 shows grades of schooling attained in 2003 for the T1998 and T2000 group and also includes grades of schooling for the same age group in 1997 (e.g. 6 to 14). There are few differences between T1998 and T2000 in both years, again what is noteworthy is what might be termed a cohort effect, overall children aged 6 to 14 in 2003 are achieving much higher rates of schooling than those children aged 6 to 14 in 1997. This may reflect partly program impacts or other factors which tend to increase schooling (e.g. growth in GDP). Again, however, since the T2000 was incorporated into the program only a year and a half after the T1998 group, these cohort effects largely difference out in the estimates.

In the C2003 group with no program exposure, girls have higher on average grades completed than do boys for every age considered (Table 4.10) most likely because higher proportions of girls progress through the grades on time (Table 4.9).[xxi] The only significant effects of greater program exposure for T1998 than for T2000 are that boys aged 11 and 13 in 2003 (5 and 7 in 1997) who were exposed in T1998 rather than in T2000 had an increase in schooling attainment of 3.9% and 3.7% respectively. The matching estimates between the T1998 and C2003 groups with the former having five and a half years of exposure to the program and the latter no exposure indicate fairly important effects, particularly for boys. For seven of the eight included ages for boys, there are significantly positive (at the 10% level) impact estimates that imply increases of from 9% to 37%.[xxii] For girls, five of the eight coefficient estimates are positive and both of the significant (at the 10% level) estimates are positive; these indicate increases of 58% for those age 1 in 1997 (and 7 in 2003) and 15% for those age 6 in 1997 (14 in 2003).[xxiii]

xxi In the original evaluation sample prior to the program in 1997, girls also had higher average schooling attainment than boys even though they had lower enrollment rates because a higher proportion of girls progressed through the grades on time [5].
xxii One of the two exceptions is (surprisingly) significantly negative (age 2 in 1997 and 8 in 2003) and the other is insignificant (4 in 1997; 10 in 2003).
xxiii In alternative estimates for this indicator of educational success, rather than comparing the values in 2003 with those in 1997 for the identical children, we compared with the 1997 average values for all children. These estimates indicate somewhat larger effects and significant positive

Figure 4.5. Grades of Schooling Completed in 1997 and 2003 by age in 1997

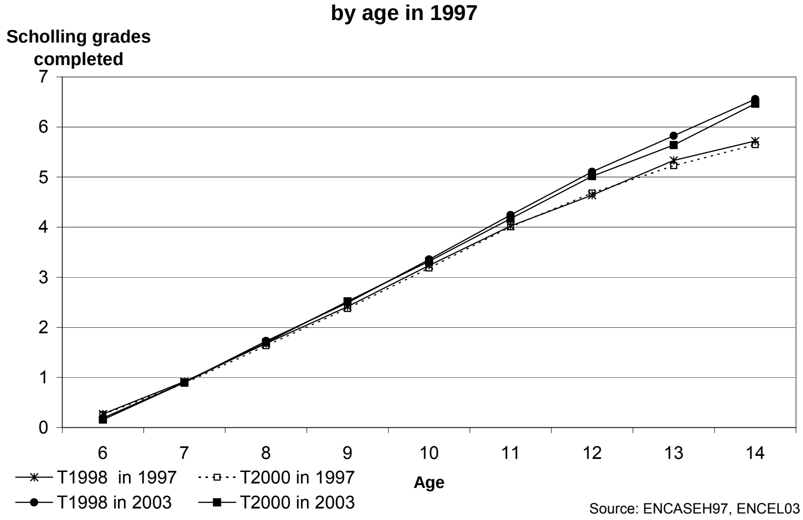

Table 4.9. Impact of Differential Exposure of Oportunidades on Completed Grades of Schooling by 2003:
T998 versus T2000.

Age in 1997	Age in 2003	Completed Grades of Schooling in 2003 of T2000 group	Impact: Coefficient Estimate	Impact: Standard Error	Impact: % change relative to T2000 group
Girls					
1	7	0.882	0.024	[0.041]	2.70%
2	8	1.635	0.051	[0.047]	3.10%
3	9	2.519	-0.033	[0.054]	-1.30%
4	10	3.354	0.010	[0.064]	0.30%
5	11	4.202	0.009	[0.079]	0.20%
6	12	5.051	0.034	[0.084]	0.70%
7	13	5.556	0.110	[0.101]	2.00%
8	14	6.493	-0.097	[0.102]	-1.50%
Boys					
By age in 1997					
1	7	0.819	0.002	[0.042]	0.20%
2	8	1.608	0.023	[0.048]	1.40%
3	9	2.410	-0.034	[0.059]	-1.40%
4	10	3.208	-0.013	[0.067]	-0.40%
5	11	3.908	0.153	[0.079]*	3.90%
6	12	4.882	0.003	[0.094]	0.10%

effects for boys and girls of almost all ages (see Appendix E). These alternative estimates do not follow the same individuals over time, as do the estimates in the text. Therefore the estimates in the text are preferred and are emphasized in this report.

7	13	5.385	0.200	[0.096]**	3.70%
8	14	6.223	-0.008	[0.096]	-0.10%

Notes: Estimates based on difference in difference regression estimates for children 6 to 8 in 1997, children 0 to 2 in 1997 use only 2003 data. 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 5% level.
* indicates significance for a t-test at 10% level.

Table 4.10 Medium-Term Impacts of Oportunidades on Completed Grades of Schooling
Matching estimates: T1998 versus C2003 §

Age in 1997	2003	C2003: Completed grades of schooling 2003	Impact estimates	Standard Error +	Number of Observations §§	% change relative to C2003 group
Girls						
1	7	0.669	0.390	[0.091]**	956	58.21%
2	8	1.726	0.068	[0.092]	917	3.93%
3	9	2.554	-0.059	[0.110]	887	-2.30%
4	10	3.362	-0.041	[0.118]	988	-1.23%
5	11	4.283	-0.261	[0.219]	527	-6.09%
6	12	5.021	0.730	[0.309]**	773	14.54%
7	13	5.839	0.129	[0.256]	908	2.21%
8	14	6.567	0.208	[0.265]	828	3.17%
Boys						
1	7	0.623	0.231	[0.092]**	960	37.11%
2	8	1.677	-0.209	[0.114]*	966	-12.49%
3	9	2.389	0.276	[0.144]*	836	11.54%
4	10	3.178	-0.042	[0.168]	1066	-1.33%
5	11	4.054	-0.363	[0.214]*	646	8.95%
6	12	4.849	0.424	[0.270]*	791	8.74%
7	13	5.773	0.476	[0.173]**	860	8.25%
8	14	6.253	0.903	[0.230]**	978	14.44%

Note: Estimates based on difference in difference matching estimates for children 5 to 8 in 1997, other children use only 2003 data, e.g. assume 0 grades of schooling in 1997.
§ Nearest neighbor (5) matching. Estimators impose common support.
§§ Treatment and control observations.
+ Standard errors based on bootstrap with 500 replications.
** Estimates significant at the 5% level.
* Estimates significant at the 10% level.

Whether had entered secondary school by 2003 (Tables 4.11. 4.12):
Finally, we examine for those children aged 6 to 8 in 1997 (12 to 14 in 2003), program impacts on the proportion who had entered secondary school by 2003. As discussed

earlier, a number of previous evaluations of the initial program years noted that the largest education impact occurred at the transition to secondary school, e.g. by increasing the proportion of children who, having finished primary school, went on to secondary school. We concentrate here only on children aged 6 to 8 in 1997 (12 to 14 in 2003), e.g. the group which could most likely have enrolled in secondary school by 2003. Of course those children aged 12 in 2003 might have not yet enrolled, in this sense we are partially capturing the effect of age at enrollment to secondary school. For children 7 or 8 in 1997 (13 or 14 in 2003), it seems likely they would have already entered secondary school if they were planning to enter secondary school.

Table 4.11 shows differential exposure estimates for the probability of entering secondary school. Some positive effects are evident, in particular for both girls and boys aged 7 in 1997 (13 in 2003), implying increases of 20.7% for girls and 11.1% for boys on the probability of enrolling in secondary school. Table 4.12 provides matching estimates, and shows much larger impacts, generally positive and significant for two ages. The estimates imply increases in the proportion entering secondary school of 41.5% and 32.9% for boys aged 12 and 14 respectively in 2003 (6 and 8 in 1997). For girls, the estimates are similar, although slightly lower in magnitude. Program impacts imply an increase in the proportion enrolling in secondary school of 32.5% and 25.7% for girls aged 12 and 13 in 2003 (6 and 7 in 1997) respectively.

Table 4.11: Impact of Differential Exposure of Oportunidades on Proportion Enrolling in Secondary School by 2003
Difference estimates: Children 6 to 8 in 1997.
T1998 versus T2000.

			Impact		
Age in 1997	**2003**	**Proportion enrolling in secondary school: T2000**	**Coefficient**	**Std. error**	**% change relative to T2000 group**
Girls					
6	12	0.411	0.051	[0.034]	12.41%
7	13	0.541	0.112	[0.031]***	20.72%
8	14	0.675	-0.023	[0.030]	-3.41%
Boys					
6	12	0.431	-0.025	[0.035]	-5.80%
7	13	0.523	0.058	[0.031]*	11.09%
8	14	0.644	-0.004	[0.030]	-0.62%

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).
*** Estimates significant at the 1% level.
* Estimates significant at the 10% level.

Table 4.12. Medium Term Impacts of Oportunidades on Proportion Enrolling in Secondary

School

Matching estimates: Treatment98 vs. New comparison group never receiving benefits §

Age in 1997	2003	Proportion enrolling Sec. School C2003	Impact Est	Std. Error +	Number Obs §§	% change relative to C2003 group
Girls						
6	12	0.397	0.129	0.070*	823	32.5%
7	13	0.534	0.137	0.074*	1013	25.7%
8	14	0.604	0.103	0.086	879	17.0%
Boys						
6	12	0.376	0.156	0.058**	817	41.5%
7	13	0.584	-0.013	0.070	900	-2.2%
8	14	0.594	0.195	0.068**	1003	32.9%

Note: Estimates based on difference matching estimates.
§ Nearest neighbor (5) matching. Estimator imposes common support.
§§ Treatment and control observations.
+ Standard errors based on bootstrap with 500 replications.
** indicates significance for a t-test at 5% level.
* indicates significance for a t-test at 10% level

5. Summary and Conclusions

Previous evaluations of the educational impacts of *Oportunidades* have focused on the short-run impacts for children at least of school age (and possibly of sufficient age to be eligible for the scholarship program that commences in the third grade of primary level). In this paper we focus instead on the medium-term impacts on children who were for the most part too young to be eligible for the scholarship program, indeed, for the most part too young to be in school, at the time of the initiation of the rural program: children 0-8 in 1997 or 6-14 in 2003. These children all may have been affected by the increased income of beneficiaries of the program and by forward-looking expectations regarding future scholarship support form the program. But only the oldest group of children in the age range considered were eligible immediately or soon after the initiation of the program for direct scholarship support, and the youngest group of children in this age range were still not eligible for scholarship support by 2003. On the other hand the youngest children in this age range were eligible for the nutrient supplements for infants and children under 24 months of age, which other studies suggests may have important benefits for their education when they become of school age. This is an important example of alleged synergies, or interactions, among various components of human capital that are one of the major motivations for the rationale and advocacy of integrated human capital investment programs such as *Oportunidades*, but also many other programs both before and after the initiation of *Oportunidades*.

This report has focused exclusively on education impacts and potential critics might ask whether the design of the program (e.g. linking grants to enrollment and attendance) would necessarily imply positive impacts of the program, and in this sense not be too interesting or surprising. This argument however, ignores the point that even if one expects the impacts to be positive, e.g. to increase schooling, this hypothesis gives no guidance on the potential size of the impacts. Full participation is not mandatory, e.g. many families may not send all of their children to school. For some children, the family may feel the grant does not compensate the opportunity cost of their time. In fact, the magnitude of the program impacts make clear that many children/youth are not in fact enrolled in school when they are still eligible to receive Oportunidades grants (see Graph 4.2 which demonstrates many children at age 14 in the T1998 group are no longer enrolled in school). It is clearly of interest for the program to know the size of program impacts.

This paper contributes to the available empirical evidence primarily by exploring the medium-term impact of the program package, including the nutrition components, for infants and young children on subsequent school performance. The evidence thus far is consistent with some important initial impacts of the early nutrition intervention on early school outcomes. In particular, those aged 1 in 1997 (7 in 2003) show important increases in schooling and the probability of progressing on time as they begin their schooling careers. Infants in 1997 (age 6 in 2003) are those who likely would benefit most from the early nutrition intervention, however given they were only age 6 in 2003 limited the analysis that could be carried out, although that carried out found no significant impacts on age at entry to school or current enrollment.

The age group 3 to 5 in 1997 most likely did not benefit from the early nutritional intervention and also by 2003 would have only recently begun to be eligible themselves to receive *Oportunidades* grants. In this sense, of those analyzed in this paper, they are likely the group we would expect to have the lowest impacts of the program, although positive impacts might derive through the general increase in family income received the program. In fact, there are few consistent patterns of positive effects on this age group, both using the differential exposure and the matching estimates.

In comparison, those aged 6 to 8 in 1997 (12 to 14 in 2003) show large and positive increases in a number of schooling indicators, including years of completed schooling, the proportion progressing on time as well as the proportion entering secondary school. This group was eligible to receive grants for a majority of the program's duration and it is likely this and the conditionality aspect of the grants that explain the much larger impacts of the program for this age group, relative to those aged 3 to 5 in 1997.

In summary, the results are overall consistent with general program objectives of increasing education. The group aged 6 to 8 show important and plausible increases in schooling levels associated with the program in general, and the grants in particular. While limited by the fact that infants in 1997 are only beginning to enter school by 2003, the results are also consistent with potentially important positive effects of the nutrition intervention on later schooling indicators.

While our results are on the whole positive, it is clear that continued evaluation is needed. Most of the children studied here are still only of primary school age in 2003 and thus could have only have received a very partial cycle of the *Oportunidades* grants. Providing estimates of the eventual total effect of the program on their schooling necessitates their continued follow-up. This would seem particularly urgent and relevant for the youngest age group, e.g. those aged 0 to 2 in 1997, further evaluation will allow the analysis of whether the apparent initial synergies between health and education observed here will be observed over time.

Nevertheless, based on these results, one might ask whether they are indicative of a program that after nearly 6 years of operation is functioning well, or whether changes might be warranted, for instance in the structure of the grants. Due to the design of the evaluation, this is a difficult question to answer, e.g. because there is no variation in the program, we can only really evaluate the impact of *Oportunidades*, not alternative programs or variations which could have greater or smaller impacts (although see [56] where a limited cost benefit analysis of alternative programs (namely building schools) was carried out). For the next phase of the evaluation, the program might consider incorporating cost-benefit analysis, in order to provide some guidance on whether alternative programs could have similar impacts at less cost, or greater impacts at the same cost.

But our sense is that the education impacts found in this paper, particularly for older children, are important. Our knowledge of evaluations of education programs in other contexts leads us to speculate that many common alternative programs, for instance increasing funding directly to schools, are unlikely to have larger impacts per peso of resources used. It is of course possible and even probable that some fine tuning of the grant amounts might improve the impact per peso; for example, given high enrollment at the primary level, previous evaluations have suggested reassigning primary grants to secondary school or higher levels.

As in our companion paper on adolescents [9], we believe the final word on whether the program has been successful at reducing poverty in the next generation, is whether the additional education that children are apparently receiving will lead to a higher income when these children enter the labor force. Unfortunately, for the group studied in this paper, the evaluation will likely still need to wait a number of years in order to have evidence on this issue. One indicator however which could be measured earlier and is likely to correlate with earnings in the future derives from whether children are learning more as a result of their greater years of schooling. As was done for adolescents, then it might make sense in future rounds of the ENCEL to apply achievement tests to the age group considered in this report and in this way, have a better indicator of learning. This is particularly important given that the present study has no way of analyzing the extent to which teachers may be “rubber stamping” children. E.g. if teachers feel more pressure to pass children with unsatisfactory levels of achievement in order to assure they do not lose the education grant, then program impacts might show higher years of schooling, but this would presumably not be true or would be less true of achievement tests. In this sense, as well, achievement tests would likely be the best indicator of education impacts.

Appendix A. Construction of Variables

<u>Sample construction:</u>

The analysis uses children aged 0 to 8 in 1997 or those 6 to 14 in 2003. In practice, there are inconsistencies in the ages reported, e.g. not all youth reported to be age 0 to 8 in 1997 are within the range of 6 to 14 in 2003 (or even slightly outside the range). An additional concern arises over whether age inconsistencies over time as well as in other indicators might reflect errors in id numbers resulting in individuals "matching" incorrectly.

To correct some errors and insure that we are correctly matching individuals over the six-year period, we deleted from the sample any individual who was more than two years off in 2003 with respect to what would be his or her "correct" age according to that reported in 1997. Additionally we eliminated individuals who reported changing gender between the periods.

We also deleted from the sample individuals who reported impossible changes in the schooling grade attainment over time. That is, we eliminated individuals reporting negative changes in schooling or those reporting they had completed more than 8 grades of schooling over the six-year period.

<u>Definition of outcome indicators:</u>

Grades of completed schooling is constructed for both 1997 and 2003 using information on the level and grade. Years in preschool or kindergarten were not counted. Primary school education was allowed to have a maximum of six grades, secondary school was allowed a maximum of three additional grades, and high school a further additional three grades. Further, we constrained the number of grades of completed schooling a child could have in the following way: children six year old could have at most 1 year of completed schooling, children seven years old could have at most 2 years of completed schooling etc. For schooling in 2003, we carried out the following corrections: For cases which were inconsistent with this rule (e.g. suggesting a child had more schooling than should be possible given his/her age), we used the grade they were currently attending in school and/or the grade they had attended in the previous year to correct the information on years of completed schooling. For the cases in which inconsistencies remained or where they were no longer attending school, we defined years of schooling to be missing for these individuals and they were dropped from the sample. For schooling in 1997, a similar procedure was carried out, however, here there was no retrospective information on previous school enrollment nor current grade enrolled so that possible errors were defined to be missing.

Progressing through school on time is defined as 1 if the difference between schooling grade attainment in 1997 and schooling grade attainment in 2003 is at least six for those of school age (at least 6) in 1997, otherwise it is defined as zero. For those who were below school age in 1997 (less than 6), this variable is defined as 1 if the difference between schooling grade attainment in 1997 and schooling grade attainment in 2003 is at least equal to the number of years they were 6 or older between 1997 and 2003, otherwise it is defined as zero.

Ever failing a grade both in 1997 and 2003 is defined using the retrospective information in 2003 on grades failed and when they were failed to compare the year the grade was failed and the age of the individual at that time. Note this definition is based on self-reporting.

Age at entry is unfortunately not directly asked in the 2003 or 1997 data used here. We construct an indicator of age at entry based on the retrospective information available in the 2003 data on enrollment from the school years 2000 and onwards. This is carried out only for children aged 7 to 8 in 2003 (1 to 2 in 1997) as only for this group are we reasonably confident we have sufficient information to identify the year of school entry.

The proportion of those enrolled in secondary is defined only for those 6 to 8 in 1997 (12 to 14 in 2003) and is defined by using both current grade enrolled (>=7) as well as previous grades of schooling attained (>=7).

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

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

Appendix B. Analysis of Attrition in T1998 and T2000

This appendix provides the estimates and greater details that underlie the discussion of attrition in Section 3.2. Table B.1 gives probit estimates for the probability of being lost to follow-up for individuals 0 to 8 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 in T1998 group and pre-program individual characteristics, parental characteristics and housing characteristics. Tables B.2 and B.3 present similar estimates, but separately for boys and girls.

Table B.1. Probability of attriting between 1997 and 2003 as a function of characteristics in 1997:
Children 0 to 8 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	0.022	0.003	0.039	-0.01	-0.02
	[0.005]	[0.036]	[0.003]	[0.020]*	[0.005]*	[0.032]
Interactions						
T1998*age		0.006		0.002		0.004
		[0.004]		[0.002]		[0.004]
T1998*gender		0.015		0.009		0.006
		[0.011]		[0.005]		[0.010]
T1998*indigenous		0.038		0.004		0.032
		[0.023]*		[0.010]		[0.021]
T1998*schooling		-0.019		-0.005		-0.014
		[0.011]*		[0.005]		[0.010]
T1998*enrolled		-0.026		-0.003		-0.021
		[0.018]		[0.008]		[0.016]
T1998*father schooling		0.004		0.001		0.003
		[0.003]		[0.001]		[0.002]
T1998*father age		-0.001		0.001		-0.001
		[0.001]		[0.000]		[0.001]
T1998*father indigenous		-0.05		0.019		-0.068
		[0.038]		[0.028]		[0.029]**
T1998*father bilingual		0.108		0.021		0.081
		[0.046]**		[0.027]		[0.041]**
T1998*mother schooling		0.003		-0.003		0.005
		[0.002]		[0.001]**		[0.002]**
T1998*mother age		-0.003		-0.002		-0.001
		[0.001]***		[0.000]***		[0.001]
T1998*mother indigenous		0.046		-0.027		0.1
		[0.034]		[0.007]***		[0.036]***
T1998*mother bilingual		-0.087		0.012		-0.088
		[0.018]***		[0.013]		[0.014]***
T1998*rooms		0		-0.001		0.001

		[0.001]		[0.000]		[0.001]
T1998*electricity		0.025		0.002		0.02
		[0.013]**		[0.006]		[0.011]*
T1998*water		0.027		-0.001		0.027
		[0.014]**		[0.006]		[0.013]**
T1998*dirt floor		0.031		-0.007		0.04
		[0.014]**		[0.005]		[0.013]***
Observations	23393	22756	23393	22756	23393	22756

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

Table B.2. Probability of attriting between 1997 and 2003 as a function of characteristics in 1997:
Boys 0 to 8 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	0.031	0.006	0.012	-0.006	0.007
	[0.008]	[0.051]	[0.004]*	[0.023]	[0.007]	[0.046]
Interactions						
T1998*age		0.01		0.002		0.008
		[0.006]*		[0.002]		[0.005]
T1998*indigenous		0.038		-0.001		0.039
		[0.033]		[0.011]		[0.031]
T1998*schooling		-0.03		-0.004		-0.025
		[0.016]*		[0.006]		[0.015]*
T1998*enrolled		-0.056		-0.008		-0.044
		[0.024]**		[0.010]		[0.021]**
T1998*father schooling		0.006		0		0.005
		[0.003]		[0.001]		[0.003]*
T1998*father age		-0.001		0.001		-0.001
		[0.001]		[0.000]		[0.001]
T1998*father indigenous		-0.013		0.027		-0.039
		[0.062]		[0.042]		[0.051]
T1998*father bilingual		0.069		0.013		0.048
		[0.066]		[0.033]		[0.059]
T1998*mother education		0.002		-0.002		0.004
		[0.003]		[0.001]		[0.003]
T1998*mother age		-0.003		-0.001		-0.002
		[0.001]**		[0.001]**		[0.001]
T1998*mother indigenous		0.033		-0.019		0.072
		[0.047]		[0.011]*		[0.048]
T1998*mother bilingual		-0.084		0.015		-0.09
		[0.026]***		[0.019]		[0.020]***
T1998*rooms		0		0		0
		[0.002]		[0.000]		[0.002]
T1998*electricity		0.039		0.008		0.025

		[0.018]**		[0.008]		[0.016]
T1998*water		0.025		0.002		0.022
		[0.019]		[0.008]		[0.018]
T1998*dirt floor		0.031		-0.003		0.035
		[0.019]		[0.007]		[0.018]**
Observations	11827	11500	11827	11500	11827	11500

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

Table B.3. Probability of attriting between 1997 and 2003 as a function of characteristics in 1997:
Girls 0 to 8 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	-0.014	0.035	0	0.08	-0.014	-0.036
	[0.008]*	[0.051]	[0.004]	[0.036]**	[0.007]**	[0.044]
Interactions						
T1998*age		0.003		0.002		0
		[0.006]		[0.003]		[0.005]
T1998*indigenous		0.035		0.009		0.022
		[0.032]		[0.015]		[0.029]
T1998*schooling		-0.008		-0.006		-0.002
		[0.015]		[0.007]		[0.014]
T1998*enrolled		0.008		0.005		0.004
		[0.028]		[0.014]		[0.025]
T1998*father schooling		0.002		0.001		0.001
		[0.004]		[0.002]		[0.003]
T1998*father age		-0.001		0.001		-0.001
		[0.001]		[0.001]		[0.001]
T1998*father indigenous		-0.083		0.012		-0.091
		[0.046]*		[0.036]		[0.035]***
T1998*father bilingual		0.143		0.029		0.112
		[0.063]**		[0.040]		[0.058]*
T1998*mother education		0.003		-0.003		0.006
		[0.003]		[0.002]*		[0.003]**
T1998*mother age		-0.003		-0.002		0
		[0.001]**		[0.001]***		[0.001]
T1998*mother indigenous		0.061		-0.032		0.13
		[0.049]		[0.009]***		[0.053]**
T1998*mother bilingual		-0.09		0.006		-0.086
		[0.024]***		[0.016]		[0.020]***
T1998*rooms		-0.001		-0.001		0.002
		[0.002]		[0.001]		[0.002]
T1998*electricity		0.012		-0.005		0.015
		[0.017]		[0.007]		[0.016]
T1998*water		0.029		-0.004		0.033

		[0.019]		[0.008]		[0.018]*
T1998*dirt floor		0.031		-0.01		0.046
		[0.019]		[0.008]		[0.018]**
Observations	11546	11256	11546	11256	11546	11256

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

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

Among the pre-program individual characteristics:

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

Among the pre-program parental characteristics:

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

xxiv We do not here attempt to give interpretations to why these effects are significant in terms, for example, of price and income effects because attrition may be due to a number of behaviors and our interest here is primarily in describing attrition in order to be able to correct for it in estimates in the next sections.

significantly negatively interacts with T1998 for individual attrition for boys and girls combined and for boys and girls considered separately.
- Mother being bilingual significantly negatively interacts with T1998 for total and household attrition for boys and girls combined and for boys and girls considered separately.

Among the pre-program housing characteristics:

- Whether the house had electricity significantly positively interacts with T1998 for total and household attrition for boys and girls combined and for total attrition for boys.
- Whether the house had indoor water significantly positively interacts with T1998 for total and household attrition for boys and girls combined and for household attrition for girls.
- Whether the house had dirt floors significantly positively interacts with T1998 for total and household attrition for boys and girls combined and for household attrition for boys and for girls considered separately.

Thus, treatment in 1998 as opposed to in 2000 appears to be significantly negatively positively associated with individual attrition for boys and girls combined and for girls and boys considered separately and significantly negatively associated with total and household attrition for boys – and there are a number of significant interactions with individual, parental and housing characteristics (differing in many cases for boys versus girls). Therefore biases may result if we do not correct for attrition in our estimates – so we do correct by re-weighting observations to counter the effects of differential attrition. This re-weighting gives higher weights to observations of types that in terms of observed characteristics are more likely to attrit in order to preclude under-representation of observations of types for which attrition is higher. Such a procedure, of course, corrects only for attrition related to observed characteristics in the data, not to unobserved characteristics.

Appendix C: Technical Appendix on Matching Estimator Used in the Analysis

This appendix describes the matching methods used in this report to estimate program impacts.

First, we need to introduce some notation.

Let Y_0 denote the outcome for persons who receive the treatment
Let Y_1 denote the outcome without treatment
Let D=1 if persons receive treatment, D=0 if not.
Let X denote other characteristics used as conditioning variables.
Let $P(X)=Pr(D=1|X)$ denote the conditional probability of participating in the program.

Households are offered the Oportunidades program if they (a) live in program areas, (b) are eligible for the program according to the program eligibility criteria (marginality index). Households receive the program if they satisfy (a) and (b) and, in addition, elect to participate in the program.

Alternative matching estimators differ in terms of the assumptions needed to justify their application and in terms of the methods used to match individuals/households. They can be broadly classified into two types of estimators:

cross-sectional (CS) matching estimators compare outcomes for program participants and nonparticipants, where the outcomes are measured at some post-program time period.

difference-in-difference (DID) matching estimators compare the change in outcomes for treatments to the change in outcomes for comparison group members, where the change is measured relative to some preprogram benchmark (baseline) time period.

The advantage of using a difference-in-difference estimator instead of a cross-sectional estimator is that it allows for time-invariant unobservable differences between the outcomes of participants and nonparticipants that might arise, for example, from regional differences. A major advantage of having baseline or preprogram data is that they allow a difference-in-difference strategy to be used.

The specific matching estimators discussed here are:

(a) nearest neighbor cross-sectional matching estimator
(b) nearest neighbor difference-in-difference (DID) matching estimator

A. Identifying assumptions of different estimators

A key parameter of interest in evaluations is the mean impact of treatment on the treated (TT) (where treatment is defined as participating in the program), which gives the average impact of the program for people participating in it. TT can be defined conditional on some characteristics X (TT(X)). For example, in this report we present treatment impacts conditional on age and gender.

$$TT(X)=E(Y_1-Y_0|X,D=1)$$

An averaged parameter may be defined over some support of X, S_X

$$TT = \int E(Y_1-Y_0|X,D=1) f(X|D=1) dX$$

where $f(X|D=1)$ is the density of X.

Cross-sectional Matching
In this report, we use propensity score matching estimators. These estimators were developed in
[14]. This cross-sectional matching estimator assumes:
(CS.1) $E(Y_{0t}|P(X),D=1)=E(Y_{0t}|P(X),D=0)$
(CS.2) $0<Pr(D=1|X)<1$
at some post program time period t. Under these conditions, TT can be estimated by

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

where the sum is over n_1, the number of treated individuals with X values that satisfy CS.2. It is also assumed that the distribution of X does not vary with treatment (for example, the age and gender of the child do not vary with treatment). $E(Y_{0i}\mid P(X_i),D=0)$ represents the matched outcome for each treated individual. The matched outcome can be estimated by a nonparametrically by nearest neighbor, kernel or local linear regression. We use the nearest neighbor method in this report.

B. Difference-in-difference (DID) Matching Estimator

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

(DID.1) $E(Y_{0t} - Y_{0t'}\,|P(X),D=1)=E(Y_{0t}- Y_{0t'}\,|P(X),D=0)$
(DID.2) $0<Pr(D=1|X)<1$
where t is a post-program time period and t' a pre-program time period. The DID matching estimator is given by

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

Where n_{1t} and $n_{1t'}$ are the number of treated observations in the two time periods. Note that at the baseline time period we observe $Y_{0it'}$ (no treatment outcomes) for the D=1 and D=0 groups.

The matching estimators are implemented as follows.

A. Step One - Estimate a model for program participation.

The conditional probability of participating in the program (also called the propensity score) plays an important role in implementing both matching and other kinds of evaluation estimators. As shown by [55], when the probability of participating in the program can be estimated by a parametric procedure (such as logit or probit), which reduces the matching problem to matching on a one-dimensional random variable. That is, the problem is reduced to estimating $E(Y_{0it}|D=0,P(X))$ -- instead of a k dimensional problem -- that of estimating $E(Y_{0it}|D=0,X)$.

Estimating the propensity score P(X) requires first choosing a set X of conditioning variables. It is important to restrict the choice of X variables to variables that are not influenced by the presence of the program. For this reason, X variables are usually chosen to be baseline characteristics of persons or households that are measured prior to the start of the program. As described in section 5 of this report, we estimate the conditional probabilities of program participation by probit regression. The set of matching variables used is presented in Section 5.

B. Step Two - Construct the matched outcomes

Constructing matched outcomes requires estimating $E(Y_{0t}|P(X),D=0)$ for the cross-sectional matching estimator and $E(Y_{0t}|P(X),D=0)$ and $E(Y_{0t'}|P(X),D=0)$ for the difference-in-difference estimator. There are several nonparametric estimators that could be used to estimate these conditional means. The simplest of these methods, which is the one we use in this report, is the nearest neighbor estimator. The impact results presented in this report are based on the nearest 5 neighbor estimator, which is implemented as follows.

(a) form $|P(X_i)-P(X_j)|$ for treatment observation i and for all comparison group observations j.

(b) sort the j observations in terms of $|P(X_i)-P(X_j)|$ from lowest to highest.

(c) Let A_X index the set of 5 D=0 observations with the lowest values of $|P(X_i)-P(X_j)|$. These are the so-called nearest neighbors.

(d) Construct the matched outcome as a simple average over the outcomes for the nearest neighbors.

$$E(Y_{0i}|P(X_i),D_i=0)=(1/5)\sum_j Y_{0j}$$

One concern in implementing nearest neighbor matching estimators is that the matches may be far away in the sense that of $|P(X_i)-P(X_j)|$ may be too large . This would represent a failure of the common support requirement. (Conditions CS.2 and DID.2 above). [xxv]

One way of determining which observations lie in the region of overlapping support is simply to plot the histogram of the $P(X_i)$ values for both the treatment and comparison groups and then visually identify any ranges of $P(X_i)$ where there are no close matches. Another common way of addressing this problem is to employ "caliper matching" where matches are only selected if $|P(X_i)-P(X_j)|<\varepsilon$, for some prespecified ε value, called a "caliper." Another more rigorous way of determining the overlapping support region is to calculate directly the density $f(P(X_i)|D=0)$ at each of the $P(X_i)$ values observed for $D_i=1$ observations, using a nonparametric kernel density estimator:

$$f(P(X_i)|D=0)=\sum_i K(((P(X_i)-P(X_k))/(h_n))),$$

where K is a kernel function, h_n the bandwidth parameter, and the sum is over all nontreated observations for whom $D_j=0$.

[xxv] We term the support of P(X) to be the values for which both $f_x(P(X)|D=1)>0$ and $f_x(P(X)|D=0)>0$ (the region of overlapping support). Implementing matching estimators requires determining for each $P(X_i)$ value, whether it lies in the overlapping support region. The mean program impact can only be obtained for treatment group persons in the overlap region.

After the estimates of the density at each point are obtained, rank the density estimates and find the 1% quantile of the positive density estimates. All values of $P(X_i)$ for which the estimated density exceeds this threshhold are considered to be in the overlapping support region. Values below the threshold are outside the region and are excluded in estimation.

Appendix D: Comparison of T1998 and C2003 differences in education outcomes in 1997.

Table D1. Differences on Grades of Education in 1997 (pre-program) between matched treatment and comparison groups.

Age in 1997	Coefficient	Std. Error	Number Obs §§
Boys			
6	-0.502	0.081***	718
7	-0.631	0.073***	833
8	-0.076	0.055	1057
Girls			
6	-0.620	0.062***	610
7	-0.275	0.077***	906
8	-0.412	0.131***	798

§§ Treatment and control observations.
*** Estimates significant at the 1% level.

Table D2. Differences on Proportion Ever Failing in 1997 (pre-program) between matched treatment and comparison groups.

Age in 1997	Coefficient	Std. Error	Number Obs §§
Boys			
7	-0.054	0.048	884
8	-0.076	0.055	1057
Girls			
7	-0.040	0.049	799
8	0.023	0.053	744

§§ Treatment and control observations.

Appendix E. Alternative Difference-in-Difference Matching Estimates for T1998 versus C2003

Table E.1 Medium Term Impacts of Oportunidades on Grades of Schooling Completed
Matching estimates: T1998 versus C2003 §

Age in 1997	Age in 2003	C2003: Completed Grades of Schooling 2003	Impact Estimates	Standard Error +	Number of Observations §§	% change relative to C2003 group
Boys						
0	6	0.129	0.583	[0.047]***	636	452.11%
1	7	0.623	0.555	[0.084]***	865	89.09%
2	8	1.677	0.403	[0.113]***	1023	24.06%
3	9	2.389	0.767	[0.162]***	1040	32.12%
4	10	3.178	0.486	[0.165]***	1106	15.31%
5	11	4.054	0.625	[0.151]***	1039	15.42%
6	12	4.849	0.570	[0.188]***	1049	11.76%
7	13	5.773	0.270	[0.190]	977	4.68%
8	14	6.253	1.145	[0.219]***	999	18.31%
Girls						
0	6	0.148	0.541	[0.056]***	695	365.36%
1	7	0.669	0.600	[0.094]***	851	89.70%
2	8	1.726	0.618	[0.097]***	945	35.80%
3	9	2.554	0.570	[0.108]***	935	22.32%
4	10	3.362	0.443	[0.112]***	1052	13.17%
5	11	4.283	0.352	[0.183]*	980	8.21%
6	12	5.021	0.907	[0.242]***	984	18.07%
7	13	5.839	0.542	[0.304]*	985	9.28%
8	14	6.567	0.553	[0.241]**	921	8.42%

Note: Estimates based on difference in difference matching estimates but 1997 values are the means for the same ages in that year as in 2003 (i.e., each 14 year old in 2003 is compared with the mean for 14 year olds in 1997, separately for boys and girls).
§ Nearest neighbor (5) matching. Estimators impose common support.
§§ Treatment and control observations.
+ Standard errors based on bootstrap with 500 replications.
* Estimates significant at the 10% level. ** Estimates significant at the 5% level. **** Estimates significant at the 1% level.

Table E.2 Medium Term Impacts of Oportunidades on the Probability on Ever Failing a Grade
Matching estimates: Treatment98 vs. New comparison group never receiving benefits §

Age in	Proportion C2003 Ever Fail 2003	Impact Est	Std. Error +	Number Obs §§	% change relative to C2003 group

1997					
Girls					
1	0.057	0.033	[0.037]	767	57.9%
2	0.146	0.009	[0.054]	887	6.16%
3	0.134	0.084	[0.041]	934	62.7%
4	0.211	-0.043	[0.060]	977	-20.4%
5	0.205	0.049	[0.044]	1033	23.9%
Boys					
1	0.101	0.019	[0.048]	821	18.8%
2	0.168	-0.048	[0.066]	971	-28.6%
3	0.226	0.044	[0.074]	985	19.5%
4	0.309	-0.010	[0.062]	985	-3.2%
5	0.305	-0.047	[0.067]*	889	-15.4%

Note: Estimates based on difference in difference matching estimates but 1997 values are the means for the same ages in that year as in 2003 (i.e., each 14 year old in 2003 is compared with the mean for 14 year olds in 1997, separately for boys and girls).
§ Nearest neighbor (5) matching. Estimator imposes common support.
* Estimates significant at the 10% level. ** Estimates significant at the 5% level. **** Estimates significant at the 1% level.

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