Schooling Impacts of Conditional Cash Transfers on Young Children: Evidence from Mexico

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I. Introduction

Oportunidades (formerly PROGRESA) has been operating since 1997 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 1998–2000). 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.[1] 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

[1] The overall evaluation of the initial years of PROGRESA is summarized in Skoufias and Mc-Clafferty (2001), Parker (2003), Behrman and Skoufias (2005), and Skoufias (2005).

behaviors (e.g., Parker and Skoufias 2000; Schultz 2004; Behrman, Sengupta, and Todd 2005; Todd and Wolpin 2006). Such extrapolation of short-term effects to longer term impacts may be misleading, for example, if there is learning-by-doing by program beneficiaries or by program providers or initial "pioneering" effects that dissipate over time (Behrman and King 2008; King and Behrman 2009).

This article examines the educational impacts of *Oportunidades* on young children in the medium term in 2003, about 5.5 years after households in the original treatment group began receiving benefits. We consider the group of children ages 0–8 in 1997, just prior to the program intervention, or those ages 6–14 in 2003. In particular, we study the program's impacts on the age of starting school, on grade progression, and on the number of completed school grades.

The education of children ages 0–8 in 1997 just prior to the intervention, or those ages 6–14 in the 2003 survey, might have generally benefited some from the income resource effect of transfers to families, particularly if capital markets for human capital investments are imperfect. In addition, the distinctive components of the program affected children in this age range differentially depending on the children's ages in 1997. The oldest children in this age range 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 continue into secondary school—which occurred for most individuals when they were in their early teenage years.[2]

The youngest children in the 0–8-year-old age range in 1997, in contrast, did not directly receive any educational scholarships by 2003, although these children's education may have been affected by their parents' anticipation of receiving future scholarships. Most significantly, however, the youngest group benefited from the nutritional supplements for infants,[3] which may have improved their educational gains from attending school once they became of school age. One of the original rationales for *Oportunidades* was to exploit

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[2] Previous evaluations have in fact demonstrated that the largest effects of the program were precisely at this transition between primary and secondary school (see Parker and Skoufias 2000; Schultz 2004; Behrman et al. 2005; Todd and Wolpin 2006).

[3] See Behrman and Hoddinott (2005) for a summary of this program as well as evidence that the program has positively affected infant and preschool child growth.

interactions among various investments in human resources with the hope that the "whole might be more than the sum of the parts."[4] Although there is a presumption that effects of early nutrition on child education are likely to be important as well as a number of studies that report associations in data that are consistent with such effects (e.g., see Pollitt 1990; Behrman 1996; Victora et al. 2008), there is a relatively small literature for developing countries that has persuasively examined the causal effects.[5] We aim in this essay to provide suggestive evidence of a link, although the package nature of benefits complicates the isolation of the impact of each component of the program.

The information that we use to evaluate the *Oportunidades* medium-term impact on education of children ages 0-8 in 1997 is that provided in the ENCEL (the *Oportunidades* Evaluation Survey) from 2003, linked with earlier data, particularly the 1997 preprogram Survey of Household Socio-Economic Characteristics (ENCASEH97) data. The strategy of analysis includes direct assessment of the impacts using two different approaches:

1. Difference and difference-in-difference treatment effect estimates using the original treatment starting in the 1998 group (T1998) and the original control group with treatment starting in 2000 (T1999) 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.
2. Difference and difference-in-difference matching estimates between those who obtained treatment in 1998 (T1998) and the 2003 matched com-

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[4] 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). 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 effects on all other outcomes (e.g., health, nutrition). The latter effect also is embodied in the impact estimates in this article, as well as in the other available impact evaluation estimates.

[5] There are a few studies that have investigated the impacts of nutrition on schooling of younger children using instrumental variable estimators with price shocks (Alderman et al. 2001), sibling characteristics (Glewwe et al. 2000; Glewwe and King 2001), or weather shocks (Alderman et al. 2006), 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) 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 preschool 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 (Pollitt et al. 1993; Maluccio et al. 2009), as well as on adult wages for males (Hoddinott et al. 2008). The INCAP studies, in fact, are probably the most persuasive evidence of such effects.

parison group that was incorporated into the program in 2004 (C2003), to investigate the impact of exposure to the program for 5.5 years versus no exposure.

We use these methods to provide new estimates on the medium-term increase in education for children ages 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 regarding the impact of scholarships on education of children and regarding interactions over time between components of the program package, particularly nutrition for infants and preschool children and subsequent educational outcomes when those children are of school age.

We organize our study as follows. Section II provides some program background. Section III introduces the basic data and considers the program impact on sample attrition and some aspects of the sample used for the matching estimates. Section IV considers the estimated program impacts using the approaches indicated above. Section V concludes.

II. Program Background

Oportunidades began operating in small rural communities in 1997 and has gradually expanded to urban areas. It now covers 5 million families, about one quarter of all families in Mexico. The program has a number of dimensions that might have affected the educational achievement of children ages 6–14 in 2003, who were ages 0–8 at the time the program was initiated in 1997.[6]

A. Cash Transfers to the Mothers in the Treatment Households

Conditional on the whole family (including preschoolers) regularly visiting health clinics and school-aged children attending school, *Oportunidades* beneficiaries receive bimonthly cash transfers, equivalent on average to about 20% of household consumption (Behrman and Hoddinott 2005). These cash transfers may have several income effects. First, they may have induced increased education for all the children in the treatment group through intrahousehold allocations, although very few of the children who were 0–8 years of age in 1997 were directly eligible for the schooling-related transfers (see Sec. II.E). Second, the increased income is likely to result in greater use of health services,

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[6] Families that were eligible for the program were determined primarily by statistical analysis of preprogram characteristics (see Parker [2003], Behrman and Skoufias [2005], and Skoufias [2005] for details). Eligible households receive all the program components for which they are eligible and for which they satisfy the program coresponsibilities (e.g., they receive the educational grants if and only if children are enrolled in the ages for which these grants are given). In other work, we have studied the impacts of the program on education and work outcomes for older youth (Behrman, Parker, and Todd 2007; Behrman et al. 2008).

which may increase children's health and thereby also impact education (see Strauss and Thomas 1995, 1998). Third, the income transfers may be used for increased nutrient consumption that also may affect children's education. There has been considerable controversy over the extent to which increased income translates into increased nutrient consumption (see Behrman and Deolalikar 1987, 1988; Bouis and Haddad 1992; Subramanian and Deaton 1996; Behrman, Foster, and Rosenzweig 1997), but estimates for the *Oportunidades* rural sample indicate that a 10% increase in income translates into a 3%–4.5% increase in caloric availability, with much of this increase going to foods richer in micronutrients (Hoddinott and Skoufias 2004). Finally, the income transfers, which go to the mother of the household, may have household bargaining effects. Although 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 given to mothers are directed toward child health and nutrition than when given to fathers and, partly for this reason, *Oportunidades* directs resources to mothers (Thomas 1990, 1994; Haddad and Hoddinott 1994; Alderman et al. 1995; Strauss and Thomas 1995; Haddad, Hoddinott, and Alderman 1996; Behrman 1997; Rubalcava, Teruel, and Thomas 2002).[7] For all these reasons related to household income, we expect children in the treatment group to have better health and nutrition, which may translate into better schooling performance even for children who do not receive the schooling scholarship.

B. Participation in the Pláticas

Oportunidades participants are required to attend regular 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 Rivera et al. 2000). 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 health and nutritional insults, these children would seem to be the most likely beneficiaries.

C. 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

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

ages of 4 months and 2 years and to children between 2 and 4 years (up to 59 months) if signs of malnutrition are detected by the clinic personnel.[8] 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 result in high levels of acceptability and intake (see Rosado 1999; Rosado et al. 2000). 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 on their cognitive development and school performance.[9] As noted, children between ages 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 indirectly from the increased household income in kind, akin to the cash transfer discussed in Section II.A.

D. Growth Monitoring

A prerequisite for receiving nutritional supplements is ongoing growth monitoring of preschool children. Conventional wisdom holds that there may be a high payoff to growth monitoring through increasing the probability that parents or other caregivers become aware of nutritional problems before longer run damage occurs, although we are unable to locate rigorous evaluations of growth-monitoring programs in Latin America. The direct beneficiaries, once again, are likely to have been primarily the children under 2 years of age (because they are most vulnerable to nutritional insults) when the program started—and therefore under 8 years of age in 2003.

E. Direct Educational Components of the Program

A final major component of the program that is obviously germane to our interests in this article is the conditionality (beyond the income effect noted

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[8] 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*.

[9] 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 Adato, Coady, and Ruel 2000; Behrman and Hoddinott 2005). Behrman and Hoddinott (2005) estimate that about 64% of infants age 0–12 months and 61% of toddlers 12–24 months received the nutritional supplements. Rivera et al. (2004), based on a subsample of infants in T1998 communities, estimate that 58% of infants regularly consumed the supplement (at least 4 days a week).

TABLE 1

MONTHLY AMOUNT OF *OPORTUNIDADES* 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

Note. The exchange rate was about P 10 per US$1.

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 bimonthly 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). This allows a student theoretically to receive 2 years of grants for the same grade, for each grade. Table 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 grades. 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 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 years old in 1997 were ready to enter the third grade at the time of program initiation and thus had potentially 5.5 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 5.5 years of direct scholarship 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 Section II.A above, there may be income and intrahousehold allocation effects for children who live in households with older siblings who were beneficiaries.

TABLE 2
BENEFITS RECEIVED IN PROGRESA/*OPORTUNIDADES* PROGRAM BETWEEN 1998 AND 2003
(BY TREATMENT GROUP)

Treatment Group	Age in 1997	Nutritional Supplements	Fixed Cash Transfer[a] (Years)	Educ. Grants[b] for Child	Educ. Grants[b] for Older Children (Years)
T1998	0–2	Yes	5.5	No	5.5
T1999	0–2	No	4	No	4
C2003	0–2	No	0	No	0
T1998	3–5	No	5.5	1–2 years	5.5
T1999	3–5	No	4	1–2 years	4
C2003	3–5	No	0	No	0
T1998	6–8	No	5.5	3–4 years	5.5
T1999	6–8	No	4	3–4 years	4
C2003	6–8	No	0	No	0

[a] Linked to regular clinic attendance, including growth monitoring and monthly health talks.
[b] Linked to regular school attendance.

Table 2 summarizes program benefits that each age group and treatment group would be expected to receive and is useful for illustrating the degree to which we can isolate the effects of the nutritional component of the program.[10] Children ages 0–2 years preprogram in T1998 receive nutritional supplements whereas T1999 children do not. However, the families of children ages 0–2 years in T1998 also receive the fixed monetary transfer for a longer period of time than T1999 families. Comparing these two groups then will provide impact estimates of the entire health/nutritional component, including the fixed monetary transfer; however, it will not provide the impact of the nutritional supplements separately. We can to some extent isolate the impact of the nutritional supplements by comparing the impact of the 0–2-year-olds to the program impact of the 3–5-year-olds. The main difference between T1998 3–5-year-olds and T1999 3–5-year-olds is the time of receipt of the fixed nutrition grant.[11] Thus, comparing impacts between these two age groups can largely isolate the particular impact of the nutritional supplements over time. We use this approach to study the effect of the nutritional supplements on age of entrance into primary school and on grades of schooling attainment. Given that children ages 0–2 years preprogram are only beginning their school

[10] See Martinelli and Parker (2008) for a theoretical analysis of the different impacts of PRO-GRESA/*Oportunidades* and an empirical exercise isolating the conditionality effects on household expenditures.

[11] Whereas children ages 0–2 years automatically received the supplement, as noted above, children between the ages of 3 and 5 years were eligible to receive supplements if they were diagnosed with malnutrition, so that comparing the impacts of 0–2-year-olds with those of 3–5-year-olds may underestimate the impacts of the supplements.

participation, this strategy will be more fruitful with later rounds of data than were available for this study.

III. Evaluation Design and Data

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.[12] Only those households that were identified to be poor through discriminant analysis (with some modifications due to community feedback) were deemed eligible for the program (see Behrman and Skoufias 2005; Skoufias 2005). The eligible households in the original treatment localities (T1998) began receiving benefits in the spring of 1998 whereas the eligible households in the original control group (T1999) began receiving benefits about 18 months later, 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 6 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, T1999) groups and a new sample of households from comparison communities that were selected through matching on observed community characteristics from a larger set of communities that had not had the program by 2003 (C2003).[13] The C2003 sample respondents answered questions concerning their characteristics and behaviors in 2003 as well as retrospective questions about their characteristics and behaviors in 1997, used in part to determine whether they would have been eligible for the program at that time. We link the T1998 and T1999 data from 2003 to the preprogram 1997

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[12] Due to budget restrictions, the program was phased-in over time.

[13] Todd (2004) describes in detail the matching used to select the communities forming the new comparison group. Using preprogram census community data, the matching was carried out, including the following variables: housing characteristics, including the proportion of households with now running water and without electricity; average number of rooms and proportion with dirt floor; household assets, including the proportion of households with telephone, refrigerator, washing machine, car, and gas stove; proportion of children working; proportion of young children in the household; number of persons with disabilities; age and gender; proportion of female-headed households; average household education in the community; and size of locality and distance to education and health facilities. 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 Sec. IV that use a much more extensive set of observed characteristics for the matching.

ENCASEH data, to have longitudinal data on individual children who were 0–8 years of age in 1997 and 6–14 years of age in 2003. As in the previous ENCEL surveys, the ENCEL2003 contains data on a myriad of program outcomes, including several indicators of educational performance.

The comparison of T1998 and T1999 (what we term differential exposure estimates) continues to be based on the original randomized experimental design of the program. Given that T1999 households also began to receive benefits late in the year 1999, comparing the two groups provides information on the effect of differential lengths of participation in the program, in this case 5.5 years versus almost 4 years. The differential exposure estimates are likely to be most informative for those impact indicators that would be expected to accumulate with additional time in the program, such as child height and grades of completed schooling

Estimates based on an experimental design are generally more convincing than those based on nonexperimental designs because they are less susceptible to selectivity bias. However, social experiments are usually short-lived, lasting only a year or two, and therefore do not usually speak to the long-term effects of exposure to a program. For this reason, studies of long-term effects often rely on nonexperimental estimates. In this essay, we exploit the experimental data to study the effects of 5.5 years of exposure versus 4 years, and also use nonexperimental data and a matching methodology to study the effects of 5.5 years of exposure versus no exposure.

The difference-in-difference estimates we report rely on the assumption that in the absence of the program, trends in the outcome indicators would be similar in the treatment and control communities. To get an idea of potential differential trends in the communities over time, we examine preprogram census information on community characteristics between 1995 and 2000. Table 3 provides changes over time between 1995 and 2000 for the three groups of communities as well as t-tests comparing the two treatment groups with the C2003 group. Given that the T1998 communities began receiving benefits in 1998 (before the 2000 census), the most appropriate comparison is T1999 versus C2003 communities. For the available educational variables, table 3 shows that those measuring schooling attainment show no significant differences between changes over time in the T1999 and C2003 groups. There are, however, significant differences in population changes, with the C2003 communities showing a larger population growth over time than the treatment communities and a significantly smaller change in the proportion of households with electricity overtime in C2003. Overall, however, given our focus on educational impacts, the lack of differences in educational trends is encouraging.

TABLE 3

TRENDS IN POPULATION AND HOUSING CHARACTERISTICS PREPROGRAM (BY TREATMENT GROUP)

	C2003		T1998		T1999		Difference-in-Differences			
	Change	SD	Change	SD	Change	SD	(T1998−C2003)	t-Statistic	(T1999−C2003)	t-Statistic
Population between 6 and 14	3.6%	17.9	−.6%	17.7	−1.2%	17.8	−4.24	−2.20	−4.84	−2.29
Population over 15	10.4%	37.1	1.2%	31.0	.7%	29.6	−9.26	−2.40	−9.72	−2.39
Population between 5 and 14 able to read	5%	.19	4%	.13	2%	.13	−.01	−.65	−.03	−1.37
Population over 15 able to read	2%	.12	3%	.07	3%	.07	.02	1.29	.01	.76
Population speaks indigenous language	0%	.07	0%	.06	−1%	.09	−.00	−.30	−.01	−1.07
Population speaks indigenous language and not Spanish	−13%	.24	−17%	.29	−16%	.29	−.04	−1.28	−.03	−.87
Inhabitants per house	−.2	.81	−.5	.56	−.4	.62	−.29	−3.60	−.22	−2.57
Houses with electricity	7%	.21	12%	.33	12%	.29	.05	2.00	.05	1.84
Observations	116		320		186					

Source. 2000 census and 1995 *conteo* surveys.

Most previous evaluation studies of the *Oportunidades* program are based on the experimental design, but one previous study by Diaz and Handa (2006) provides some evidence that matching estimators can replicate the experimental estimates. Diaz and Handa (2006) attempt to replicate the short-run estimates from *Oportunidades*'s initial experimental design on expenditure shares, school enrollment, and child work using matching estimators. They use the Survey of Income and Expenditures in Mexico (ENIGH), a nationally representative cross-sectional survey, to construct a comparison group that was matched to beneficiary households in the ENCEL treatment group. For the indicators where similar survey questions are available (school enrollment and child work), Diaz and Handa are able to closely replicate the experimental design estimates using cross-sectional matching estimators, with insignificant differences between the experimental design estimates and those based on their matching analysis. For those based on expenditures, where the survey instruments vary substantially, the estimates are significantly different. This is suggestive evidence that matching methods can replicate estimates based on randomized designs in the context of schooling and *Oportunidades*. It is particularly notable that Diaz and Handa were able to replicate results using only cross-sectional matching, presumably subject to greater bias than the difference-in-difference matching estimators that we primarily use here.

IV. Methodology

As described in the introduction, we evaluate the impacts of participation in the *Oportunidades* program using two different approaches: (i) difference and difference-in-difference comparisons between the original randomized treatment (T1998) and control (T1999) groups, which have by now both been incorporated into the program but which have 1.5 years difference in exposure duration; and (ii) difference and difference-in-difference comparisons between the original treatment group (T1998) and a new matched comparison group (C2003) that had not been exposed to the program. In both contexts, we use only the sample of program-eligible individuals.[14]

Usually, difference-in-difference estimators are implemented using before and after information on the same individuals. In the current context, however,

[14] All eligible individuals in treatment communities are invited to participate in the program. *Oportunidades* has had very high take-up rates in rural areas; in the T1998 communities, approximately 97% of families take up the program. With such high take-up rates, the intent-to-treat estimators are nearly identical to treatment-on-the-treated estimates. 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 additional details on data and variable construction.

many of the children in our sample were not in school preprogram, and it is more meaningful to implement the difference-in-difference estimators using different children pre- and postprogram initiation but holding the age constant over time. We use this approach both for the differential exposure estimates and for the matching estimates. For the oldest group of children (e.g., those ages 6–8 preprogram), for whom longitudinal difference-in-difference estimators are feasible, we compare estimates using the age-constant strategy to those obtained using longitudinal estimators.

A. Differential Exposure Estimates: Comparison of Original Randomized Treatment (T1998) and Control (T1999) Groups, Accounting for Attrition

For a comparison of T1998 and T1999 using difference and difference-in-difference estimates, we present first difference and difference-in-difference treatment effect estimates based on the original randomized treatment and control groups for children of different ages. These estimates exploit the fact that the treatment group (T1998) received the program 1.5 years earlier than the control group (T1999) to evaluate the effects of differential exposure between the two groups on education outcomes, measured in 2003. As noted in Sections I and II, our prior is that the magnitude of the 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 (ages 0–2 years in 1997 and therefore 6–8 years in 2003), or (*b*) were ages 6–8 years in 1997 and therefore 12–14 years in 2003 and had attained the critical age for making the schooling decision 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 2 years of direct scholarship treatment that the same schooling (and, approximately, birth) cohort in T1999 did not receive. However, those T1998 children who entered the third grade in or after 2000 (children approximately ages 3–5 years in 1997) received the same direct scholarships for school attendance as the T1999 children in the same schooling cohort (although the former may have received other benefits, such as those due to the cash transfers discussed in Sec. II, for 2 years longer). That is, we hypothesize that children ages 3–5 are likely to show lower effects of the program, given there are no major differences in terms of nutritional interventions or educational grants for this age group.

For each of the outcome measures and for both preprogram and postprogram-initiation data, we estimate a linear regression of the outcome variable on an

indicator of whether the individual resided in an original treatment or control locality. The additional covariates include parental age, education, indigenous status, and household characteristics (number of rooms, electricity, type of floor, and water/sewage system).[15] The difference-in-difference estimates are constructed by taking the difference of program impacts "before" and after program initiation.

As discussed above, we do not have preprogram educational information for those ages 0–5 years preprogram (6–11 years after) because children below age 5 were not in school. While the experimental design should ensure no significant preprogram differences between the T1998 and T1999 groups, we nevertheless allow for the possibility of preprogram differences between T1998 and T1999 using the group of children ages 6–14 years in 1997. This allows us to carry out difference-in-difference estimators, on the assumption that differences in the educational outcomes of children 6–14 years in 1997 are representative of preprogram differences of children 6–14 years in 2003. We will use a similar age-constant differencing strategy below in the matching analysis.

One concern in evaluating longer term impacts of the 1.5 years difference in program exposure is that of sample attrition of the original evaluation ENCEL sample. Sample attrition can be particularly problematic if it is correlated with program participation. For the purpose of this study, we are concerned with sample attrition of individuals who were in the baseline sample in 1997 but not in the 2003 follow-up sample.[16] Table 4 (sec. A) summarizes some statistics regarding sample attrition in this period for the original treatment (T1998) and original control (T1999) groups, focusing first on all youth in the community and then on those eligible for the program; 23% of the individuals ages 0–8 years in 1997 were not in the sample 6 years later, which certainly is a large enough proportion to raise concerns about attrition. As seen in table 4, most attrition is due to household rather than individual attrition. There are, however, not large or statistically significant differences at the 10% level in overall attrition between the T1998 and T1999 samples. The proportion lost to follow-up is about the same for girls (21% in T1998; 19% in T1999) as for boys (20% in both T1998 and T1999); only for girls is there a statistically significant difference between T1998 and T1999 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

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[15] We adjust standard errors for potential correlations between individuals within communities.

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

TABLE 4
PROPORTION OF ORIGINAL ENCASEH SAMPLE ATTRITING BY 2003
(INDIVIDUALS OF AGES 0–8 YEARS IN 1997)

	Treatment (T1998)		Control (T1999)		
	N	Mean	N	Mean	$P > |Z|$
A. Total proportion attriting (individual or household):					
0–8 years (all)	19,493	.232	12,055	.229	.564
0–8 years (eligible)	14,610	.205	8,783	.199	.236
By gender:					
Boys 0–8 years (eligible)	7,434	.203	4,393	.203	.993
Girls 0–8 years (eligible)	7,168	.208	4,378	.194	.068
B. Proportion due to individual attrition:					
0–8 years (all)		.052		.050	.504
0–8 years (eligible)		.041		.045	.238
By gender:					
Boys 0–8 years (eligible)		.038		.045	.096
Girls 0–8 years (eligible)		.045		.044	.961
C. Proportion due to household attrition (individual not found because HH moves):					
0–8 years (all)		.181		.180	.802
0–8 years (eligible)		.164		.154	.051
By gender:					
Boys 0–8 years (eligible)		.165		.158	.375
Girls 0–8 years (eligible)		.163		.149	.047

Source. Authors' calculations with 1997 ENCASEH and 2003 ENCEL data.
Note. The last column gives the significance level for mean differences between T1998 and T1999, based on t-tests. HH = household.

boys, although attrition is higher at the 10% significance level for the T1998 than the T1999 group for girls.

Overall attrition of individuals ages 0–8 years 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. Of the 23% of the sample attriting, a majority, 18%, are individuals in households that left the sample. There are some significant although overall small differences if individual and household attritions are considered separately: at the 10% level, higher individual attrition among the T1999 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 indicates some differences.

Table B1 estimates the probability of being lost to follow-up for individuals 0–8 years old in 1997 in eligible households from the T1998 and T1999 groups—again, for total attritors, individual attritors, and household attritors—and shows that a number of the preprogram individual, parental, and housing characteristics interact significantly with treatment (that is, being in the T1998 group) to affect attrition. To account for possible attrition biases,

we employ a weighting method that is equivalent to a matching-on-observables approach. That is, we carry out difference-in-difference regression estimates for which both the treatment and control group observations are weighted to adjust for differences in the distribution of the X characteristics arising over time because of attrition. Using the dimension-reduction theorem of Rosenbaum and Rubin (1983), we implement the reweighting estimator using, as the weights, the ratio of the univariate densities of the propensity score. Through this procedure, each individual observed postprogram-initiation receives a weight equal to the ratio of the density of his or her probability of attriting with respect to the postprogram-initiation distribution divided by the density estimated with respect to the preprogram (and preattrition) distribution. Effectively, this procedure reweights the postprogram-initiation observations to have the same distribution of X as they did prior to the attrition. The key assumption that justifies application of this procedure is that attrition is random conditional on X, within each of the groups.[17] Details and derivations are provided in Behrman, Parker, and Todd (2008).

B. Matching Estimates
To estimate impacts of longer run exposure to the program against the benchmark of no program, we compare the original treatment group (T1998) with the comparison group (C2003) that was drawn from rural areas that had not yet been incorporated into the program as of 2003. Because the C2003 group was not selected at random, we use matching methods to take into account differences in observed characteristics between the T1998 and C2003 samples. For each individual in the treatment group (T1998), these methods select an individual from the outside comparison group (C2003) that is comparable in terms of observed characteristics.[18]

The households living in localities where the program was not yet available at the time of the collection of the C2003 data were unlikely to have been affected in any way by the existence of the program. However, because they live in different geographic areas from the treatment group sample, they may experience different local area effects (labor market conditions, quality of schooling, quality of health clinics, prices) that may also be relevant determinants

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

[18] The localities that were included in the sampling frame for C2003 were initially selected by matching on locality-level characteristics. This first matching procedure should not be confused with the matching that we perform in this study, which uses both locality-level and individual-level characteristics in selecting the matches.

of outcomes of interest (schooling, work, achievement scores). The difference-in-difference matching methods that we implement control for fixed locality characteristics. These methods compare the change in outcomes in the treatment group (postprogram-initiation minus baseline) to the change in outcomes in the control group. The C2003 sample was not incorporated into the main sample until 2003. The respondents were asked retrospective questions with regard to demographics, poverty, schooling, and work in the baseline year 1997, which provides the preprogram information needed for the difference-in-difference matching estimators.

Matching estimators were first developed and are widely used in the statistics literature (e.g., Rosenbaum and Rubin 1983). They have more recently been applied and extended in the economics literature (e.g., Heckman, Ichimura, and Todd 1997). To describe the estimators that we use, we need to introduce some notation. Let Y_1 denote the outcome for persons who receive the treatment and Y_0 the outcome without treatment. Let $\Delta = Y_1 - Y_0$. The evaluation problem arises because Y_1 and Y_0 can never be observed simultaneously for the same individual and thus Δ, the estimator of interest, can never be estimated directly. Let $D = 1$ if persons participate in the program and $D = 0$ if not; X denotes other characteristics used as conditioning variables, and $P(X) = \Pr(D = 1|X)$, denotes the conditional probability of participating in the program, conditional on X. The matching literature refers to $P(X)$ as the propensity score.

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

$$\text{TT} = E(\Delta|X, D = 1) = E(Y_1 - Y_0|X, D = 1)$$

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

The cross-sectional matching estimator assumes

$$E[Y_{0t}|P(X), D = 1] = E[Y_{0t}|P(X), D = 0] \tag{1}$$

$$0 < \Pr(D = 1|X < 1) \tag{2}$$

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

$$\Delta_{D=1} = \left(\frac{1}{n_1}\right)\sum_i Y_{1i}[P(X_i)] - E[Y_{0i}|P(X_i), D = 0], \tag{3}$$

where the sum is over n_1, the number of treated individuals with X values

that satisfy condition (2).[19] The expression $E[Y_{0i}|P(X_i), D = 0]$ represents the matched outcome for each treated individual, which can be estimated nonparametrically by nearest-neighbor, kernel, or local-linear regression.

The difference-in-difference matching estimator requires longitudinal or repeated cross-sectional 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

$$E[Y_{0t} - Y_{0t'}|P(X), D = 1] = E[Y_{0t} - Y_{0t'}|P(X), D = 0] \qquad (4)$$

$$0 < \Pr(D = 1|X) < 1), \qquad (5)$$

where t is a postprogram-initiation time period, and t' a preprogram time period. The difference-in-difference matching estimator, based on longitudinal data, is

$$\Delta_{D=1} = \left(\frac{1}{n_{1t}}\right)\sum_i \{Y_{1i}[P(X_i)] - Y_{0it'}[P(X_i)]\}$$

$$- \{E[Y_{0it}|P(X_i), D = 0] - E[Y_{0it'}|P(X_i), D = 0]\}, \qquad (6)$$

where n_{1t} is the number of treated observations, and the summation is over the participants.[20] The first term in curly braces is the change in outcomes for the program participant, and the second term is the matching estimate of the change in outcomes in the no-treatment state based on the control group data.

The propensity score matching estimators are estimated in two stages. In the first stage, $P(X)$ is estimated using a logit model and a set X consisting of preprogram (1997) household and locality level characteristics.[21] In the second stage, the matched outcomes are constructed; that is, $E[Y_{0t}|P(X), D = 0]$ for the cross-sectional estimator and, in addition, $E[Y_{0t'}|P(X), D = 0]$ for the difference-in-difference estimator. We restrict analysis to those eligible for the program under the original eligibility criteria. The model estimates the determinants of participating, where participants are those eligible individuals

[19] Condition (2) ensures that matches can be found for the treated individuals. See Heckman et al. (1997) for a discussion of the relevance of common support restrictions for matching estimators.
[20] Note that at the baseline time period, we observe Y_{0it}' (no treatment outcomes) for the $D = 1$ and $D = 0$ groups.
[21] The distribution of X should be unaffected by the receipt of treatment. Using preprogram characteristics ensures that this requirement is satisfied because, in 1997, none of the respondents had any knowledge of the program.

living in treatment (T1998) communities and those who do not participate are those eligible individuals living in comparison (C2003) communities. As mentioned earlier, take-up rates of the program in rural areas are nearly universal.

The variables used for the matching include demographic characteristics of the households in 1997, grades of schooling completed of household head and spouse in 1997, whether the household head and spouse spoke an indigenous language in 1997, whether the household head and spouse were employed in 1997, a number of household characteristics and consumer and production durables in 1997, the *Oportunidades puntaje* score for program eligibility, and state of residence in 1997. We use only preprogram information, to avoid conditioning on any information that arguably could have been affected by the program. Nevertheless, preprogram information for the C2003 group relies on retrospective information, which for some variables, such as income, may be subject to recall biases. Unfortunately, only the C2003 group received the retrospective questionnaire; if such a questionnaire had been administered to the other groups, we would have been able to judge the reporting bias by comparing retrospective information with information reported in 1997. However, we carry out several different sets of estimates, varying the variables used to calculate the propensity scores. First, we exclude variables from the propensity scores likely to be particularly subject to recall biases, including household income and the eligibility index, which uses income as one of the criteria. We then experimented with a number of different matching specifications, including some using only 2003 characteristics, which are unlikely to have changed since preprogram (e.g., household age, parental schooling attainment). The results are not fundamentally changed; in appendix C (figs. C1–C4), we present graphs showing the distributions of the propensity scores for different specifications.[22]

Table 5 gives the estimated logit model, for which the variables are jointly significant at the 0.1% level (according to a χ^2 test) and which has fairly good

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[22] To check the specification of our propensity score model, we implemented several variants of balancing tests. We first carried out balancing tests summarized in Smith and Todd (2005). In particular, we carry out a test based on Rosenbaum and Rubin (1983) that calculates standardized differences for each covariate between the treatment and matched comparison group. Less than 5% of the covariates have standardized differences above the value of 20%, above which Rosenbaum and Rubin (1983) consider a "large bias." Second, we implemented a procedure suggested by Dehejia and Wahba (2002) in which we stratify treatment and control observations into stratum based on the estimated propensity score (in our case, quintiles). We then tested for significant differences between the covariates of the propensity score within each stratum. The vast majority of the covariates (all but two) showed no significant differences by quintile, and dividing by smaller groups (deciles) showed no significant differences in any of the covariates.

TABLE 5
LOGIT MODEL FOR PROBABILITY OF PROGRAM PARTICIPATION

Variable	Coefficient	SD	Variable	Coefficient	SD
Age of household head	.001	.003	Constant	−4.553	.322
Age of spouse	−.012	.003	Blender	−.082	.069
Gender of household head	1.185	.114	Refrigerator	.390	.116
HH head speaks indigenous lang.	.356	.086	Gas stove	.905	.090
Spouse speaks indigenous lang.	.503	.093	Gas heater	.275	.160
Grades of schooling (HH head)	.019	.011	Radio	.276	.047
Grades of schooling (spouse)	−.062	.011	Television	.212	.058
Employed HH head	−.748	.086	Video	.493	.179
Employed spouse	−.741	.068	Washer	.692	.192
Children 0–5	−.671	.031	Car	−.435	.238
Children 6–12	−.410	.032	Truck	.26	.163
Children 13–15	.179	.039	Guerrero state	.816	.121
Children 16–20	.153	.031	Michoacan state	1.06	.106
Women 20–39	.226	.070	Puebla state	.830	.108
Women 40–59	−.254	.060	Queretaro state	.568	.106
Women 60+	−.564	.058	Hidalgo state	.371	.104
Men 20–39	−.061	.045	Morelos state	−.685	.100
Men 40–59	−.865	.065	Missing grades of schooling (HH head)	−.802	.190
Men 60+	−1.224	.075	Missing grades of schooling (spouse)	−2.834	.241
No. rooms	.383	.032	Missing age (HH head)	.367	.791
Electricity in HH	−.397	.052	Missing age (spouse)	−2.884	1.134
Water in HH	−.651	.050	Missing indig. HH head	3.626	1.271
Dirt floor	−.901	.064	Missing working HH head	.408	1.082
Roof material (inferior)	.322	.055	Missing working spouse	2.184	.536
Wall material (inferior)	.185	.061	Missing water	−.153	.712
Own animals	.293	.051	Missing electricity	−1.532	.712
Own land	.487	.047	Missing rooms	−.356	.363
Score	2.63	.141	Missing own animals	−1.270	.650
Score squared	−.177	.020	Missing own land	2.61	.930
Number of obs.	13,357				
LR χ^2(58)	4,799		Pseudo R^2	.264	
Prob. > χ^2	.000		Log likelihood	6,679	

Source. Authors' calculations from ENCASEH97 and ENCEL2003 data.

Note. $D = 1$ program-eligible households in treatment group 1998. $D = 0$ program-eligible households in new comparison 2003 communities. The sample used for matching and the impact analysis are restricted to program-eligible households in both the treatment and new comparison communities. Variables used to predict participation are all preprogram data (1997). HH = household. Excluded state is Veracruz.
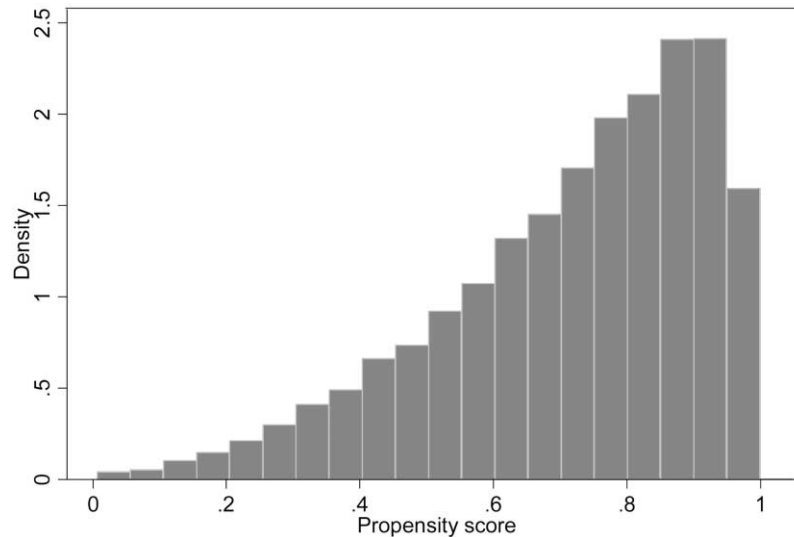
458

Figure 1. Distribution of propensity score: Treatment98
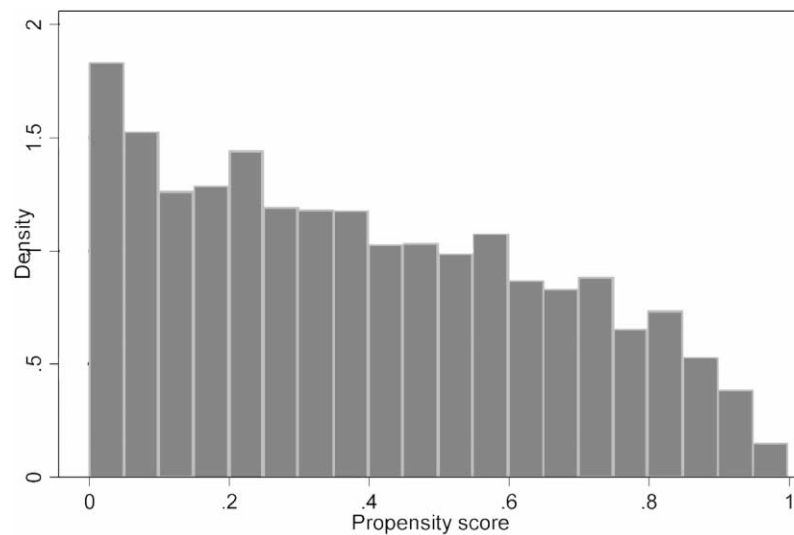
Figure 2. Distribution of propensity score: new comparison group

predictive power (pseudo $R^2 = 0.26$). The estimated relation is then used to generate propensity scores, which are estimated at a household level.

Figures 1 and 2 give the distribution of propensity scores in the original treatment group (T1998) and the distribution of propensity scores in the C2003 comparison group. The distributions between the two groups are clearly different in a way that is consistent with poorer localities receiving benefits

earlier, as intended. Nevertheless, there is adequate support in the sense that a number of households in C2003 have propensity scores similar to those in T1998 throughout the distribution. In implementing the matching estimators, we match on propensity scores and, in addition, match on the child's age and sex. That is, 8-year-old boys in the T1998 group are matched only with 8-year–old boys in the C2003 comparison group living in households with similar propensity scores.

There are many variants of matching methods available in the literature. We have explored a number of alternatives, including nearest-neighbor, kernel, and local-linear matching, with a range of bandwidths and found that the empirical results are quite robust to these alternatives.[23] We report estimates here based on the nearest-neighbor method as well as on local-linear regression. Abadie and Imbens (2006) demonstrate that bootstrap standard errors in nearest-neighbor matching are inconsistent. For the case of nearest-neighbor estimates, they propose a variance estimator (Abadie and Imbens 2002) that we use in this essay to calculate standard errors for nearest-neighbor matching. The critique of Abadie and Imbens does not apply for local-linear matching, for which we generate standard errors using bootstrapping.

As in the previous section, we implement age-constant difference-in-difference estimators, using children ages 6–14 years in the "before" period (1997) and the same age group in the "after" period (2003). For the oldest group (those ages 6–8 years in 1997), for whom we observe educational outcomes at baseline, we can compare the age-constant difference-in-difference estimates to longitudinal difference-in-difference matching estimates.

With respect to attrition, we only observe postprogram-initiation outcomes for individuals who chose to remain in the communities and did not attrit. The difference-in-difference matching estimators are justified under the assumption that attrition is determined by observables that are controlled in the matching, or that attrition is determined in part by time-invariant unobservables that are, on average, eliminated through the differencing procedure. These assumptions, however, are not easily tested or verified. An additional exercise we carried out is to use the sample of youth ages 6–14 years preprogram who did not attrit by 2003 to estimate preprogram differences between T1998 and C2003. The estimates obtained using this more restrictive preprogram group are very similar to those reported here.[24]

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[23] Some evidence suggests advantages of local-linear estimation over simple kernel regression estimation. These advantages include a faster rate of convergence near boundary points and greater robustness to different data design densities. See Fan (1992a, 1992b).

[24] We are grateful to an anonymous referee for this suggestion.

TABLE 6
ESTIMATED IMPACTS OF PROGRAM ON AGE OF ENTRY TO PRIMARY SCHOOL
(EXPERIMENTAL SAMPLE T1998 VS. T1999)

Age in 2003	Impact	SD	T1999 Progression, Impact
Girls:			
7–8	−.052	.031*	6.11, .9%
9–11	.028	.085	6.30, .4%
7–11	−.002	.070	6.23, .0%
Boys:			
7–8	.027	.085	6.10, .4%
9–11	−.000	.010	6.38, 0%
7–11	.008	.037	6.27, .1%

* Significant at 10%.

TABLE 7
ESTIMATED IMPACTS OF PROGRAM ON AGE OF ENTRY TO PRIMARY SCHOOL
(NEAREST-NEIGHBOR DIFFERENCE MATCHING T1998 VS. C2003)

Age in 2003	Impact	SD	T1999 Progression, Impact
Girls:			
7–8	−.080	.045*	6.11, −1.3%
9–11	−.033	.047	6.30, −.5%
7–11	−.050	.034	6.23, −.8%
Boys:			
7–8	.019	.052	6.10, .3%
9–11	−.025	.048	6.38, −.4%
7–11	−.009	.036	6.27, −.1%

* Significant at 10%.

V. Program Impact Estimates on Education of Children Ages 0–8 Years in 1997

We begin with program impacts on the age at starting school (tables 6 and 7). If infants and very young children have better nutrition and health, they tend to develop more rapidly, both physically and cognitively, and therefore may be sufficiently mature to enter school when younger. Studies have found significant effects of preschool nutrition on age at starting school in countries as varied as Ghana, Pakistan, the Philippines, and Zimbabwe (see Glewwe and Jacoby 1995; Glewwe, Jacoby, and King 2000; Alderman et al. 2001; Glewwe and King 2001; Alderman, Hoddinott, and Kinsey 2006). If starting school earlier permits completing a given level of schooling when younger and expanding the number of postschooling years in which to reap any productivity gains from schooling, the impact over the life cycle can be considerable (see Glewwe and Jacoby 1995).

For Mexico, the potential gains from lowering the age of starting school are perhaps less than in many countries because the vast majority of children

are in school at age 6, the legal starting age. But it still is of interest to explore whether there is some evidence of a significant program impact on age of starting school, assuming that children starting school earlier reflects that they are developmentally more prepared. We explore this question for children ages 1–5 years in 1997 or 6–11 years in 2003.[25] Both sets of estimates (tables 6 and 7) indicate a significant impact of the program on reducing the age at starting school for girls. The matching estimates, whose impacts reflect the effect of 5.5 years of benefits, show slightly larger reductions on the age at entry to primary (-0.08 years) versus those based on the differential exposure of 1.5 year estimates (-0.05). These results are thus consistent with some potential effects on reducing age at entry, although the overall magnitude is relatively low. For boys, there are no significant impacts of the program on age at entry for both sets of estimators.

It is useful to compare the impacts on age of entry to primary school for those children ages 7–8 years in 2003 with impacts for those children ages 9–11 years for the estimates of 1.5 years of differential exposure. As discussed above, children ages 7–8 years in 2003 would have been eligible to receive nutritional supplements, whereas children ages 9–11 years would largely have been excluded. Thus, comparing differential exposure impact estimates for these two groups of children can provide an approximation of the impact of the nutritional supplement. It is notable that there are no significant impacts of differential program exposure on age at starting school for girls ages 9–11 years in 2003. A triple-difference estimator provides an impact estimate of the nutritional supplements on the age of entry for girls to primary school of 0.05, although given the large standard errors for the estimated impacts for 9–11-year-olds in 2003, this difference is not statistically significant.

We now turn to program impacts on progressing on time through school grades (tables 8 and 9). Children who progress through school on time are defined to be those who progress one grade each year, starting at age 7 years. Progressing on time thus reflects both potential enrollment and continuation/passing rates.[26] Here we focus on the age groups of those 3–8 years in 1997 or 9–14 years after the program was started. The proportions that progress on time by this definition are considerably less than 1.0. For example, for the

[25] We exclude children age 6 years in 2003 from this analysis, as many would be censored (e.g., because they have not yet entered school).

[26] The impacts on failure rates are an alternative indicator of progression; nevertheless, the enrollment effect of the program may change the composition of students (for instance, marginal students may reenroll in the program), making more difficult interpretations of impacts. The impact on failure rates of those who would have enrolled even in the absence of the program is not easily isolated.

TABLE 8
ESTIMATED IMPACTS ON PROGRESSING ON TIME:
DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES
(T1998 VS. T1999)

Age in 2003	Preprogram Difference	After-Program-Initiation Difference	Impact: Difference-in-Difference Regression	C2000 Progression, Impact
Girls:				
9–11	−.002	.001	.003	.84, .3%
	(.017)	(.027)	(.024)	
12–14	.017	.026	.023	.66, 3.5%
	(.003)	(.020)	(.028)	
9–14	.01	.015	.014	.75, 1.9%
	(.013)	(.013)	(.018)	
Boys:				
9–11	.025	.012	−.013	.77, −1.6%
	(.019)	(.019)	(.027)	
12–14	.009	.041	.032	.60, 5.3%
	(.020)	(.022)	(.030)	
9–14	.015	.027	.012	.68, 1.8%
	(.014)	(.014)	(.020)	

TABLE 9
ESTIMATED IMPACTS ON PROGRESSING ON TIME:
DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES
(T1998 VS. T1999)

Age in 2003	Preprogram Difference	After-Program-Initiation Difference	Impact: Difference-in-Difference Matching	C2003 Progression, Impact
Girls:				
9–11	−.081	−.015	.066	.84, 8.0%
	(.02)	(.02)	(.03)	
12–14	−.094	.055	.149	.66, 22.9%
	(.003)	(.020)	(.04)	
9–14	−.087	.020	.107	.75, 14.5%
	(.017)	(.016)	(.02)	
Boys:				
9–11	−.131	.02	.151	.77, 19.6%
	(.019)	(.02)	(.03)	
12–14	−.087	.08	.167	.60, 27.3%
	(.028)	(.02)	(.040)	
9–14	−.011	.049	.160	.68, 23.0%
	(.017)	(.016)	(.020)	

Note. Difference-in-difference estimator, imposing common support (trimming = 2%), three neighbors. Standard errors (parentheses) based on Abadie and Imbens (2002).

TABLE 10
ESTIMATED IMPACTS ON GRADES OF SCHOOLING:
DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES
(T1998 VS. T1999)

Age in 2003	Preprogram Difference	After-Program-Initiation Difference	Impact: Difference-in-Difference Regression	T1999 Grades of Schooling, Impact
Girls:				
6–8	.008	.22	.014	1.00, 1.4%
	(.031)	(.032)	(.05)	
9–11	−.015	−.016	−.001	3.36, −.0%
	(.052)	(.046)	(.06)	
12–14	−.053	.06	.113	5.69, 2.0%
	(.078)	(.074)	(.09)	
6–14	−.016	.023	.049	3.57, 1.4%
	(.034)	(.033)	(.08)	
Boys:				
6–8	.027	.036	.009	.95, .9%
	(.031)	(.032)	(.05)	
9–11	.00	.043	.043	3.19, 1.3%
	(.053)	(.048)	(.06)	
12–14	.046	.138	.092	5.49, 1.7%
	(.079)	(.074)	(.09)	
6–14	.027	.078	.051	3.42, 1.5%
	(.034)	(.033)	(.06)	

C2003 group, the proportions for youth ages 12–14 years in 2003 are 0.65 for girls and 0.61 for boys (see table 9). The 1.5 year differential exposure to *Oportunidades* impacts, while all positive as would be expected, shows no statistically significant impacts at conventional significance levels.

The matching comparisons between T1998 and C2003 suggest important effects (presumably due to the greater time receiving program benefits) with significant and substantial impacts for both boys and girls. The impacts for the older groups (12–14 years in 2003) suggest increases in progression rates of about 11% overall for girls and 16% overall for boys. Higher impacts for the older group presumably reflect the (positive) program effect on the transition decision between primary and secondary school that under normal grade progression occurs around the age of 12 or 13 years (table 9).

We now turn to program impact estimates on grades of schooling completed (tables 10 and 11). The differential exposure results show some significant impacts of greater exposure to the program for both boys and girls, with a point estimate of an increase of 0.05 grades completed, mainly concentrated on the older group as would be expected. Nevertheless, taking into account the preprogram differences, the difference-in-difference estimates are not statistically significant (table 10).

TABLE 11
ESTIMATED IMPACTS ON GRADES OF SCHOOLING:
NEAREST-NEIGHBOR MATCHING (T1998 VS. C2003)

Age in 2003	Preprogram Difference	After-Program-Initiation Difference	Impact: Difference-in-Difference Matching	C2003 Grades of Schooling, Impact
Girls:				
6–8	−.18	−.223	−.04	1.42, -2.8%
	(.022)	(.09)	(.09)	
9–11	−.31	−.038	.27	3.33, 8.2%
	(.052)	(.06)	(.08)	
12–14	−.35	.013	.49	5.64, 8.6%
	(.111)	(.093)	(.14)	
6–14	−.28	−.039	.24	3.44, 6.9%
	(.038)	(.049)	(.06)	
Boys:				
6–8	−.17	−.16	.007	1.10, .7%
	(.021)	(.075)	(.08)	
9–11	−.41	−.027	.40	3.18, 12.3%
	(.051)	(.064)	(.08)	
12–14	−.169	.352	.52	5.49, 9.5%
	(.104)	(.089)	(.14)	
6–14	−.25	.067	.32	3.32, 9.5%
	(.045)	(.044)	(.07)	

Note. Difference-in-difference matching using children of the same age preprogram. Difference-in-difference estimator, imposing common support (trimming = 2%), three neighbors. Standard errors (parentheses) are based on Abadie and Imbens (2002).

Table 11 shows matching estimates based on difference-in-difference matching using, as the preprogram group, children of the same age preprogram. First, it is noteworthy that there are significant differences in schooling preprogram between the two groups, indicating that the C2003 group has higher years of completed schooling in the absence of the program. Using simple difference matching of postprogram-initiation schooling would thus likely underestimate the potential impacts of the program.

The difference-in-difference matching estimates that compare the T1998 groups receiving 5.5 years of benefits to the C2003 group that never received benefits show strong and significant increases in completed years of schooling, increasing in absolute terms with age. Whereas there are no significant effects for children ages 6–8 who had not yet begun to receive the education grants, children ages 12–14 years in 2003 accumulate about 0.5 grades for both boys and girls. These increases represent notable increases in the level of completed schooling for the age groups, on average representing an increase of 8%–9% of grades of current completed schooling.

To judge the plausibility of these results, a useful comparison can be made between the T1999 and the C2003 groups. This compares beneficiaries re-

TABLE 12
ESTIMATED IMPACTS ON GRADE OF SCHOOLING:
NEAREST-NEIGHBOR MATCHING (T1999 VS. C2003)

Age in 2003	Preprogram Difference	After-Program-Initiation Difference	Impact: Difference-in-Difference Matching	C2003 Grades of Schooling, Impact
Girls:				
6–8	−.19	−.033	.15	1.42, 10.2%
	(.022)	(.122)	(.13)	
9–11	−.32	−.071	.25	3.33, 7.5%
	(.056)	(.070)	(.09)	
12–14	−.31	.09	.40	5.64, 7.1%
	(.107)	(.10)	(.16)	
6–14	−.27	−.10	.17	3.44, 5.0%
	(.038)	(.06)	(.07)	
Boys:				
6–8	−.20	−.17	.038	1.10, 2.7%
	(.022)	(.103)	(.11)	
9–11	−.42	−.024	.40	3.18, 11.9%
	(.056)	(.079)	(.10)	
12–14	−.21	.07	.28	5.49, 4.9%
	(.107)	(.104)	(.16)	
6–14	−.28	−.035	.25	3.32, 7.7%
	(.04)	(.076)	(.09)	

Note. Difference-in-difference matching using children of same age preprogram. Difference-in-difference estimator, imposing common support (trimming = 2%), three neighbors. Standard errors (parentheses) are based on Abadie and Imbens (2002).

ceiving about 4 years of program benefits to those never receiving benefits and, for cumulative indicators such as years of schooling, would be expected to yield smaller impacts than those found comparing the T1998 to the C2003 group. In particular, we would expect impacts that are about 25%–30% smaller. The results of this exercise on grades of completed schooling are presented in table 12. The T1999-C2003 comparison does show smaller impacts on years of schooling, as expected, for both and girls and, in particular, about one-third less than for the T1998-C2003 comparison. For instance, the overall results show increases in schooling for boys of 0.32 grades for the T1998-C2003 comparison versus 0.25 grades for the T1999-C2003 comparison.

Finally, table 13 presents a couple of alternative estimations. First, recall that we have used children of the same age group preprogram to estimate before-program differences. In the first two rows of table 13, we now carry out results using longitudinal matching (e.g., using the same children between 1997 and 2003) and compare these results with estimates based on age-constant difference-in-difference matching. These comparisons are only possible for the oldest age group of our study (e.g., those ages 12–14 years in 2003) for whom

TABLE 13

COMPARING LONGITUDINAL DIFFERENCE-IN-DIFFERENCE MATCHING WITH DIFFERENCE-IN-DIFFERENCE
MATCHING USING CHILDREN OF SAME AGE PREPROGRAM

Age in 2003	Years of Education		Progressing on Time	
	Longitudinal Difference-in-Difference Matching	Same Age Difference-in-Difference Matching	Longitudinal Difference-in-Difference Matching	Same Age Difference-in-Difference Matching
Nearest neighbor:				
Girls:				
12–14	.20	.52	.091	.15
	(.13)	(.14)	(.029)	(.05)
Boys:				
12–14	.54	.50	.082	.17
	(.12)	(.14)	(.028)	(.04)
Local-linear bandwidth = .01:				
Girls:				
12–14	.23	.60	.074	.14
	(.20)	(.19)	(.04)	(.047)
Boys:				
12–14	.49	.63	.105	.17
	(.16)	(.20)	(.03)	(.054)
Local-linear bandwidth = .2:				
Girls:				
12–14	.29	.63	.064	.140
	(.20)	(.18)	(.037)	(.05)
Boys:				
12–14	.54	.50	.094	.165
	(.18)	(.18)	(.04)	(.05)
Local-linear bandwidth = .4:				
Girls:				
12–14	.30	.62	.062	.142
	(.18)	(.20)	(.036)	(.050)
Boys:				
12–14	.58	.50	.086	.160
	(.17)	(.19)	(.032)	(.05)

Note. Nearest-neighbor matching and local-linear matching (T1998 vs. C2003). Difference-in-difference estimator, imposing common support (trimming = 2%), five neighbors. Errors (parentheses) computed using bootstrapping with 200 repetitions. Longitudinal difference-in-difference matching using the same children over time to estimate impacts. Same age difference-in-difference matching uses as preprogram measures those based on children who, preprogram, were the same age as children postprogram initiation.

we observe preprogram education indicators. Table 13 presents the results for progressing on time and grades of completed schooling for this age group. In general, the longitudinal difference-in-difference matching estimates show smaller program effect estimates. This largely reflects that the before-group differences are lower between the 1998 and C2003 groups when longitudinal matching is done, given that children ages 6–8 years have just begun to enter

school and little time has passed for potential differences to have manifested themselves between the T1998 and C2003 groups.

Up until now, we have reported estimates based on nearest-neighbor matching, using standard errors developed by Abadie and Imbens (2002, 2006). In table 13, we use a local-linear matching estimator and provide impact estimates for years of schooling and progressing on time for a wide range of different bandwidths (0.01, 0.2, and 0.4), and we estimate standard errors using bootstrapping with 200 repetitions.[27] The results are quite similar to those based on nearest-neighbor matching. We also estimate bootstrap errors by taking into account potential correlations between individuals in each community; these bootstrap errors are slightly larger than those estimated without clusters but overall do not substantially change the significance levels of the estimates and are not reported here.

VI. 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 article, 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 years old in 1997 or 6–14 years old in 2003. 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. However, 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 suggest may have important benefits for their education when they became of school age. This is an important example of alleged synergies, or interactions, among various components of human capital that are key to the advocacy of integrated human capital investment programs such as *Oportunidades* and many other programs with similar multifaceted human capital enrichment aims.

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[27] The issue of choosing the optimal bandwidth in the context of matching estimators (as opposed to regression estimators) has just begun to receive some attention in the literature (see Galdo, Smith, and Black 2007), and there is little evidence on the best approach. We do not attempt to estimate the optimal bandwidth here for the kernel or local-linear estimators we carry out but rather prefer to provide a range of estimations in which the bandwidth is varied.

This essay contributes to the available empirical evidence primarily by exploring the medium-term impact of the program package, including the nutritional components, for infants and young children on subsequent school performance. The evidence thus far is consistent with some initial impacts of the early nutritional intervention on early school outcomes. In particular, those ages 1–2 years in 1997 (7 years in 2003) show a reduction in the age at entering school, and we have provided suggestive evidence that this partially reflects the effect of the nutritional supplements. The age group 3–5 years 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* scholarship grants. Nevertheless, the matching estimates show important increases in grades of completed schooling and progressing on time, consistent with families anticipating the receipt of grants for these children or possible income effects through program receipt. As expected, those ages 6–8 years in 1997 (12–14 years in 2003) show the largest increases in schooling indicators. 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 larger impacts of the program for this age group.

In summary, the matching estimates show a consistent and important improvement in educational indicators even for younger children who are not yet eligible or are only beginning to be eligible for the educational grants at the time the program initiated. Although limited by the fact that infants in 1997 are only beginning to enter school by 2003, the impact estimates are also consistent with positive effects of the nutritional intervention on later schooling indicators. The group ages 6–8 years preprogram show important and plausible increases in schooling levels, most likely attributable to the scholarship grants. The differential exposure estimates, based on only a year and a half of difference in program receipt, show much smaller impacts and generally insignificant impacts of the program.

The results are encouraging, but it is clear that continued evaluation is needed. Most of the children studied here were still of primary school age in 2003 and thus received the *Oportunidades* scholarship grants for only a few years. 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, for example, infants at the start of the program in 1997. Further evaluation will permit the analysis of whether the apparent initial synergies between health and education observed here continue to be observed over time.

Appendix A
Construction of Variables
Sample Construction
The analysis uses children ages 0–8 in 1997 and therefore 6–14 in 2003. In practice, there are inconsistencies in the ages reported; for example, not all youth reported to be age 0–8 in 1997 are within the range of 6–14 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 identification numbers, resulting in individuals "matching" incorrectly.

To correct some errors and ensure that we are correctly matching individuals over the 6-year period, we deleted from the sample any individual who was more than 2 years off in 2003 with respect to what would be his or her "correct" age according to that reported in 1997. In addition, 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 eight grades of schooling over the 6-year period.

Definition of Outcome Indicators
Grades of completed schooling is constructed for both 1997 and 2003 using information on the level and grade. Years in preschool or kindergarten were not counted. Primary school education was allowed to have a maximum of six grades, lower secondary school was allowed a maximum of three additional grades, and upper secondary (i.e., high school) an additional three grades. Further, we constrained the number of grades of completed schooling a child could have in the following way: children 6 years old could have at most 1 year of completed schooling, children 7 years old could have at most 2 years of completed schooling, and so forth. For schooling in 2003, we carried out the following corrections: for cases that were inconsistent with this rule (e.g., suggesting a child had more schooling than should be possible given his or 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 grades of schooling to be missing for these individuals, and they were dropped from the sample.

Progressing through school on time is defined as 1 if the difference between age and schooling grade attainment is less than or equal to 7, for those children at least 8 years old. Note that the legal age for entering school is age 6 before

the beginning of the school year so that many children may actually be close to 7 when starting school but be considered to be on time.

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 school enrollment and grade repetition/failure.

Appendix B

TABLE B1
PROBABILITY OF ATTRITING BETWEEN 1997 AND 2003 AS A FUNCTION OF CHARACTERISTICS IN 1997: CHILDREN 0–8 IN 1997 ELIGIBLE FOR BENEFITS IN 1997

	All Attritors		Individual Attrition[a]		Household Attrition[b]	
	(1)	(2)	(1)	(2)	(1)	(2)
T1998 = 1; T1999 = 0	−.006	.022	.003	.039	−.01	−.02
	[.005]	[.036]	[.003]	[.020]*	[.005]*	[.032]
Interactions:						
T1998*age		.006		.002		.004
		[.004]		[.002]		[.004]
T1998*gender		.015		.009		.006
		[.011]		[.005]		[.010]
T1998*indigenous		.038		.004		.032
		[.023]*		[.010]		[.021]
T1998*schooling		−.019		−.005		−.014
		[.011]*		[.005]		[.010]
T1998*enrolled		−.026		−.003		−.021
		[.018]		[.008]		[.016]
T1998*father schooling		.004		.001		.003
		[.003]		[.001]		[.002]
T1998*father age		−.001		.001		−.001
		[.001]		[.000]		[.001]
T1998*father indigenous		−.05		.019		−.068
		[.038]		[.028]		[.029]**
T1998*father bilingual		.108		.021		.081
		[.046]**		[.027]		[.041]**
T1998*mother schooling		.003		−.003		.005
		[.002]		[.001]**		[.002]**
T1998*mother age		−.003		−.002		−.001
		[.001]***		[.000]***		[.001]
T1998*mother indigenous		.046		−.027		.1
		[.034]		[.007]***		[.036]***
T1998*mother bilingual		−.087		.012		−.088
		[.018]***		[.013]		[.014]***
T1998*rooms		0		−.001		.001
		[.001]		[.000]		[.001]
T1998*electricity		.025		.002		.02
		[.013]**		[.006]		[.011]*

TABLE B1 (*Continued*)

	All Attritors		Individual Attrition[a]		Household Attrition[b]	
	(1)	(2)	(1)	(2)	(1)	(2)
T1998*water		.027		−.001		.027
		[.014]**		[.006]		[.013]**
T1998*dirt floor		.031		−.007		.04
		[.014]**		[.005]		[.013]***
Observations	23,393	22,756	23,393	22,756	23,393	22,756

Note. Standard errors are in brackets.
[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.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

Appendix C
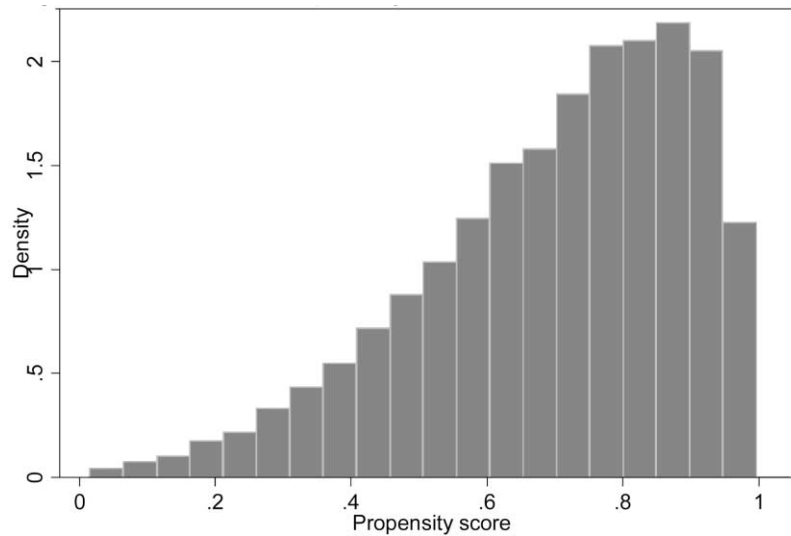
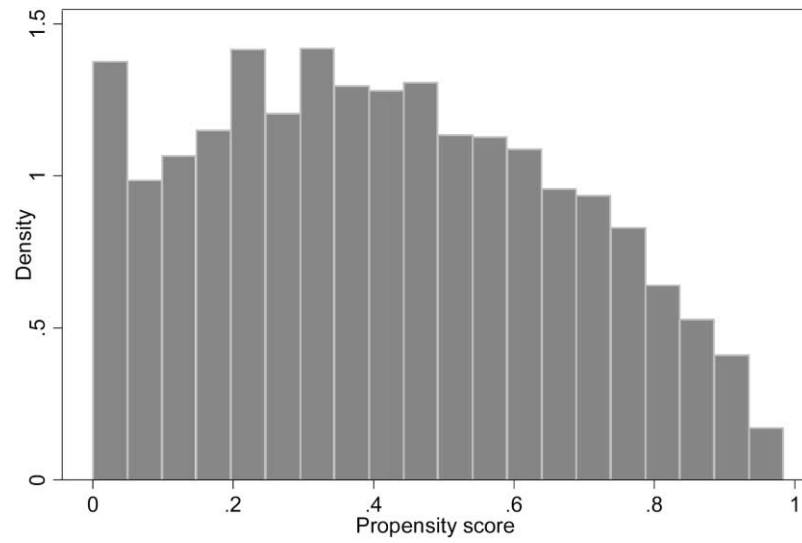**Figure C1.** T1998 propensity score without HH income and scores

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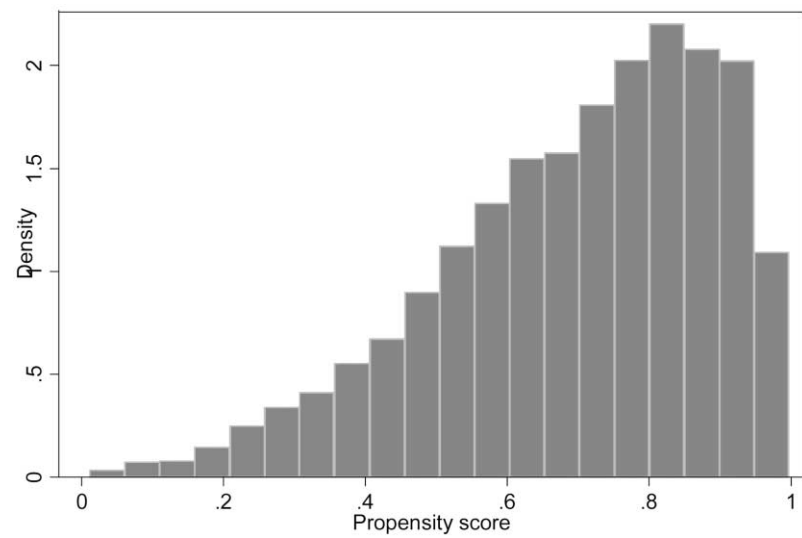**Figure C2.** C2003 propensity score withtout HH income and scores

Figure C3. T1998 propensity score without HH income, scores, and durables
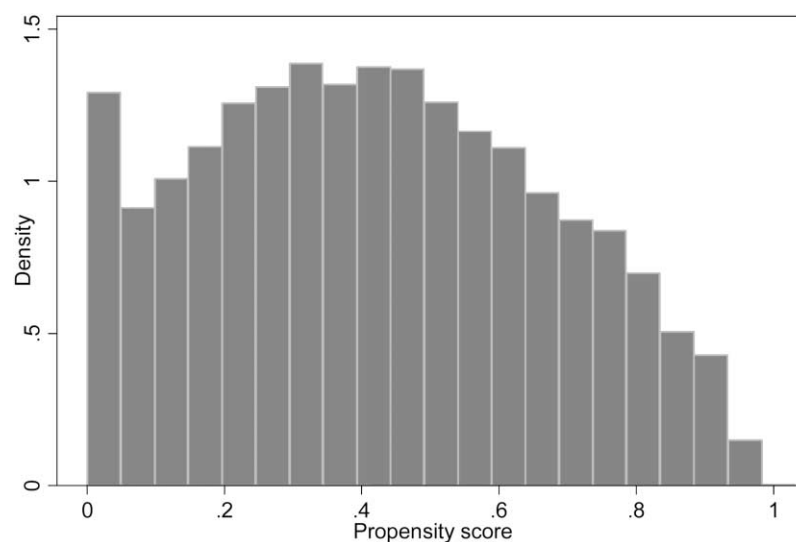
473

Figure C4. C2003 propensity score without HH income, scores, and durables

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