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# CONDITIONAL CASH TRANSFERS AND THEIR IMPACT ON CHILD WORK AND SCHOOLING: EVIDENCE FROM THE *PROGRESA* PROGRAM IN MEXICO

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## ABSTRACT

In this paper we investigate whether a conditional cash transfer program such as the Programa Nacional de Educación, Salud y Alimentación (PROGRESA) can simultaneous combat the problems of low school attendance and child work. PROGRESA is a new program of the Mexican government aimed at alleviating extreme poverty in rural areas. It combats the different causes of poverty by providing cash benefits that are targeted directly to households on the condition of children attending school and visiting health clinics on a regular basis. Some of the questions addressed are as follows: Does the program reduce child labor? Does it increase participation in school activities? Does the latter occur at the expense of children's leisure time? And how do the effects of the program vary by age group and gender?

Our empirical analysis relies on data from a quasi-experimental design used to evaluate the impact of the program involving a sample of communities that receive PROGRESA benefits (treatment) and comparable communities that receive benefits at a later time (control). We estimate the effect of “treatment on the treated” using both double-difference and cross sectional difference estimators. Our estimates show significant increases in the school attendance of boys and girls that are accompanied by significant reductions in the participation of boys and girls in work activities. We also find that the program has a lower impact on the incidence of work for girls relative to boys.

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# 1. INTRODUCTION

Within Latin America, a number of new antipoverty programs have been introduced over the past few years specifically focused toward increasing investment in human capital, as measured in particular by education, but also by health and nutrition. In general, these programs represent a significant departure from previous antipoverty policies within the region, for they are based on the premise that one of the fundamental causes of poverty and of its intergenerational transmission is the *lack* of investment in human capital. A distinguishing characteristic of these programs is the provision of cash transfers on the condition that poor families send their children to school.

One of the first programs of this kind was the *Programa de Educación, Salud y Alimentación* (the Education, Health, and Nutrition Program) (PROGRESA) introduced by the Federal Government of Mexico in 1997, as part of its renewed effort to break the intergenerational transmission of poverty. The program, while providing cash transfers, aims to increase families' investment in human capital as defined by education, health, and nutrition. To achieve this objective, PROGRESA conditions cash transfers on children's enrollment and regular school attendance as well as clinic attendance. These transfers, corresponding, on average, to a 22 percent increase in the income levels of the beneficiary families, are given directly to mothers. The program also includes in-kind health benefits and nutritional supplements for children up to age 5, and pregnant and lactating women. PROGRESA has grown rapidly and now covers 2.6 million families in extreme poverty in rural areas—about 40 percent of all rural families in Mexico.

In this paper, we conduct a detailed analysis of the extent to which PROGRESA has an impact on schooling, work, and time allocation of boys and girls ages 8–17.[1] Some of the questions addressed are as follows:

- Does the program reduce child labor?
- Does it increase participation in school activities and, if so, is this at the expense of children's leisure time?
- How do the effects of the program vary by age group and gender?

Our empirical analysis relies on data from a quasi-experimental design used to evaluate the impact of the program involving a sample of communities that receive PROGRESA benefits (treatment) and comparable communities that receive benefits at a later time (control). Our analysis is conducted in two parts, using a progressively broader definition of work. In the first part, we use data from various survey instruments used in the evaluation of PROGRESA and applied to both treatment and control groups before and after program implementation. In this way, we can estimate the impact of the program using the double-difference estimator, which is commonly acknowledged as the preferred estimator for program evaluation. In the second part, we take advantage of a

[1] This study is one of a number of studies conducted as part of the PROGRESA Evaluation project conducted by the International Food Policy Research Institute (IFPRI) under the direction of Emmanuel Skoufias. The studies that are directly related include Schultz (2000) and Behrman, Sengupta, and Todd (2001a), which focus on the impact of the program on schooling and on continuing in higher school grades. Both studies use only a binary indicator of whether a child is in school and do not consider work at all. Behrman, Sengupta, and Todd (2001b) focus on the impact of the program on child achievement test scores, while Coady (2000) evaluates the cost effectiveness of the schooling impact of the program. A related study by Demombynes (2001) considers work in addition to schooling, and is discussed further below.

module on time use, carried out about a year after program implementation. This module allows us to use a broader definition of work that includes time allocated during the previous day to domestic and farm activities; this also allows us to examine the impact of PROGRESA on leisure.

Empirical studies using data from other countries find that the marginal effect of an unconditional income change is surprisingly small on either school enrollment or on child labor.[2] This suggests that unconditional cash transfer programs that increase household income can have only a limited effect toward increasing either child school enrollment and decreasing child labor simultaneously. However, cash or in-kind transfer programs that are conditioned on school enrollment may be more effective at achieving this dual objective. The conditioning of the cash transfers on schooling reduces the shadow price of schooling, which, in turn, can reinforce the income effect of the cash transfer as long as schooling and work are substitutes for each other. However, an increase in child school attendance does not necessarily imply a reduction in the incidence or even in the intensity of all the kinds of work performed by children. Not all kinds of work may be substituted for with schooling. Moreover, increased school attendance may replace the leisure time rather than work time of children. Ravallion and Wodon (2000), for example, examine the impact of the Food for Education program in Bangladesh that provides rice to eligible families in exchange for sending their children to school. They find that the lower incidence of child labor accounted for 25 percent of the increase in enrollment of boys in school. This implies that most of the increased

[2] See Behrman and Knowles (1999) and Nielsen (1998).

attendance of boys in school took place at the expense of leisure. Whether this is also the case for boys and girls participating in the PROGRESA program in Mexico is one of the main questions addressed in our study.

The paper begins with a description of PROGRESA and a model explaining how PROGRESA may be expected to affect investment in children's human capital and time in work. The third section describes the evaluation methodology followed by a data description. We then begin analysis of our data, providing a brief description of children's labor market activities and time allocation in the poor rural areas where PROGRESA operates. This is followed by our results on the impact of PROGRESA on labor force participation and time allocation. We conclude with interpretations of our results and related policy considerations.

## 2. A DESCRIPTION OF *PROGRESA*

For Mexico, the design of PROGRESA represents a significant change in the provision of social programs. First, in contrast to previous poverty alleviation programs in Mexico, PROGRESA targets the household level in order to ensure that the resources of the program are directed and delivered to households in extreme poverty. General food subsidies are widely acknowledged to have had a high cost to government and a negligible effect on poverty because of the leakage of benefits to nonpoor households. Under PROGRESA, communities are first selected using a marginality index based on

census data. Then, within the selected communities, households are chosen using socioeconomic data collected for all households in the community.[3]

Second, unlike earlier social programs in Mexico, PROGRESA has a multisectoral focus. By design, the program intervenes simultaneously in health, education, and nutrition. The integrated nature of the program reflects a belief that addressing all dimensions of human capital simultaneously has greater social returns than their implementation in isolation. Improved health and nutritional status are not only desirable in themselves, but have an indirect impact through enhancing the effectiveness of education programs since, for example, school attendance and performance are often adversely affected by poor health and nutrition. Poor health is therefore both a cause and consequence of poverty. Also by design, PROGRESA differs in the mechanism of delivering its resources. Recognizing the potential of mothers to effectively and efficiently use resources in a manner that responds to the family's immediate needs, PROGRESA gives benefits exclusively to mothers.

Under the first benefit component, education, PROGRESA provides monetary educational grants for each child less than 18 years of age enrolled in school between the third grade of primary and the third grade of secondary school (see Table 1). The grant amounts, adjusted every six months for inflation, increase as children progress to higher grades, in order to reflect the income children would contribute to their families if they

[3] According to program description documents, there is also a third step in the selection process whereby the list of potential beneficiaries is amended after presenting it to and getting feedback from the community assemblies. The evaluation of the program's targeting revealed that the third step of the selection process was not rigorously applied and thus its importance was minute. For more details on the selection of beneficiary households in the program, see Skoufias, Davis, and de la Vega (2001) and Skoufias, Davis, and Behrman (1999).

were working. Additionally, at the junior high level, the grants are slightly higher for girls than for boys.[4] For example, during the second half of the year 1999, the amounts of the monthly educational grants ranged from 80 pesos (about $US8) in the third grade of primary to 280 pesos ($US28) for boys and 305 pesos ($US30) for girls in the third year of secondary school.

**Table 1—PROGRESA monthly cash transfer schedule (nominal pesos)**

| | January-June 1998 | July-December 1998 | January-June 1999 | July-December 1999 |
|---|---|---|---|---|
| Educational grant per child[a] | | | | |
| Primary | | | | |
| 3rd grade | 65 | 70 | 75 | 80 |
| 4th grade | 75 | 80 | 90 | 95 |
| 5th grade | 95 | 100 | 115 | 125 |
| 6th grade | 130 | 135 | 150 | 165 |
| Secondary | | | | |
| 1st - male | 190 | 200 | 220 | 240 |
| 2nd - male | 200 | 210 | 235 | 250 |
| 3rd - male | 210 | 220 | 245 | 265 |
| 1st - female | 200 | 210 | 235 | 250 |
| 2nd - female | 220 | 235 | 260 | 280 |
| 3rd - female | 240 | 255 | 285 | 305 |
| Grant for school materials per child | | | | |
| Primary - September | - | In-kind | - | 110 |
| Primary - January | 40 | - | 45 | - |
| Secondary - September | - | 170 | - | 205 |
| Grant for consumption of food per household[b] | | | | |
| Cash transfer | 95 | 100 | 115 | 125 |
| Maximum grant per household | 585 | 625 | 695 | 750 |

**Source: Hernandez, Gomez de Leon, and Vasquez (1999).**

[a] Conditioned on child school enrollment and regular attendance.
[b] Conditioned on attending scheduled visits to health centers.

[4] In poor areas of Mexico, girls tend to drop out of school earlier than boys; the grants are intended to help reverse this tendency.

The second component, health, provides basic health care for all members of the family, whose services are provided by the Ministry of Health and by IMSS-Solidaridad, a branch of the Mexican Social Security Institute. The third component, nutrition, includes a fixed monetary transfer (equal to 125 pesos or about $US13 monthly) for improved food consumption, as well as nutritional supplements, which are principally targeted to children between the ages of four months and two years, and pregnant and breastfeeding women. They are also given to children between ages 2 and 5 if any signs of malnutrition are detected.

The objective of designing benefits to provide incentives for increased human capital is revealed through the fact that receipt of the benefits is contingent on fulfillment of certain obligations by the beneficiary families. The monetary educational grants are linked to the school attendance of children so that if a child misses more than 15 percent of school days in a month (for unjustified reasons), the family will not receive the grant that month. Similarly, families must complete a schedule of visits to the health care facilities in order to receive the monetary supports for improved nutrition.

The conditionality of the cash transfers is bound to interact in many and complex ways with the preferences and income constraints faced by beneficiary households. Next, we present a simple economic framework that reflects the most important features of these interactions. The model highlights the fact that the conditions of the program may affect households and children’s time allocation differently, depending on the household’s preferences and its initial location in the feasible set. This is particularly useful when it comes to evaluating empirically the impact of the program.

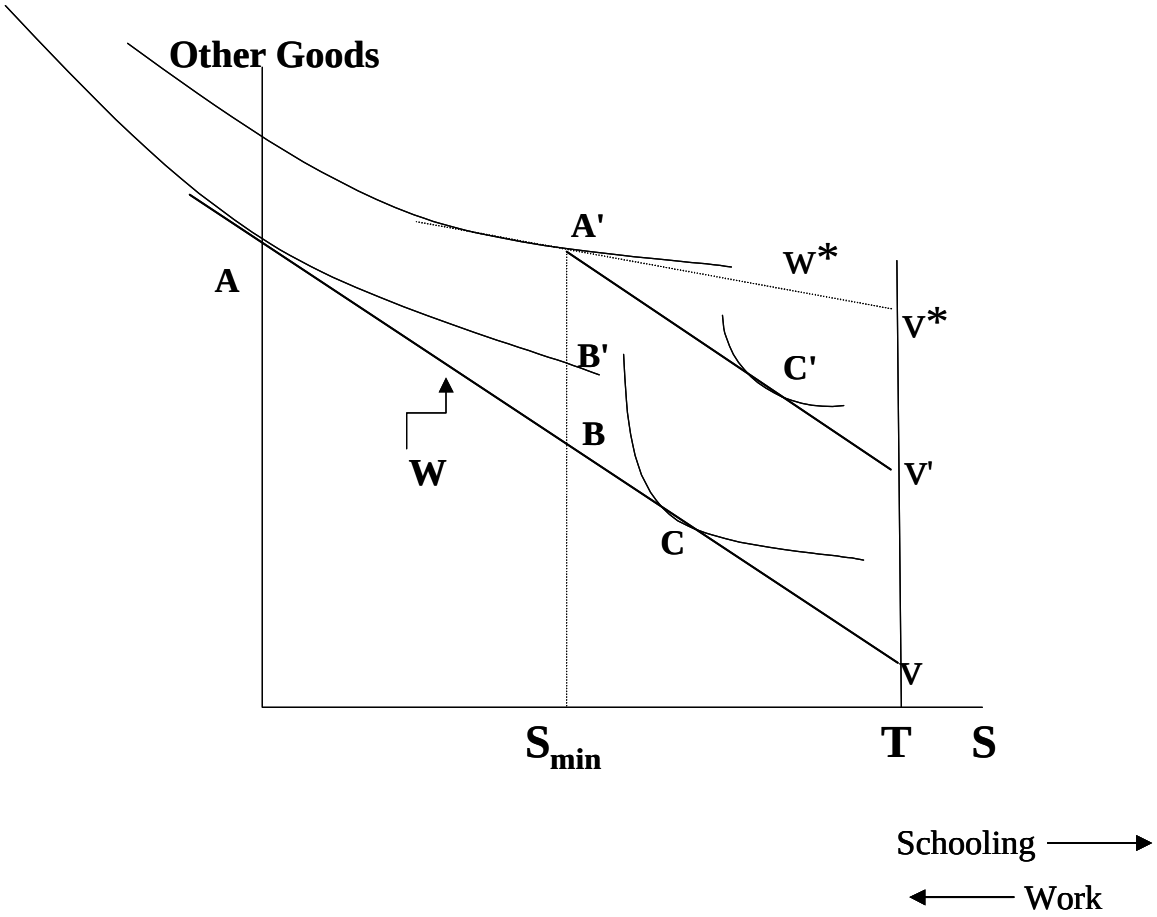
Figure 1 illustrates some of these effects graphically. The vertical axis of the graph depicts the quantity of other goods available for consumption in the household; the horizontal axis measures the time a child devotes to schooling (or in human capital investment). Full or 100 percent attendance rate occurs when the child devotes all non-leisure time in school attendance (including school-related homework) (i.e., $S = T$ where $T$ denotes the amount of time available after excluding leisure time, which for simplicity's sake is assumed to be fixed). The vertical line of height $V$ at the value of $S = T$ denotes the maximum amount of other goods available in the household when a child devotes all time to schooling and not working. When a child divides her time between work and schooling, then the opportunity set of the household is described by the line $TVA$. The negative slope of this line is given by the real market wage $W$ for child labor that describes the trade-off in the market between the consumption of other goods and schooling (or work).[5] By devoting one hour less to schooling and working one extra hour in market work, the household can earn $W$ additional units of other goods.

Let $S_{min}$ denote the 85 percent attendance rate required by the PROGRESA program. Eligibility for the benefits of PROGRESA causes the budget line in the region between points $T$ and $S_{min}$ to shift up without changing its slope and increases the non-labor component of income upward to the point $V'$. To the extent that the household fulfills *all* the requirements of the program, then $V' - V$ equals the maximum amount of benefits that the household can obtain from the program. In consequence, the feasible

[5] It is assumed that the opportunity cost of child schooling is the fixed market wage for child labor. The assumption of a perfectly competitive labor market can be replaced by (or combined) with the assumption that children work at home producing home produced commodities that are perfectly substitutable with market purchased commodities with no additional complications (see Skoufias 1994).

budget constraint of an eligible family is now described by the line *TV′A′BA*, which is discontinuous at the point $S_{min}$.

**Figure 1—The effect of conditional cost transfers on children's school attendance and work**




Of course, differences in family non-earned income and market opportunities may be one important reason as to that some children are enrolled or not enrolled in school. To keep the exposition simple, we assume that the income opportunities of households are identical and consider the case when we have two different types of households

represented by two different indifference curves. The household denoted by the tangency at point *C* represents households with a child that has an attendance rate close to 100 percent ($S > S_{min}$) and works only a very small fraction of her time. The indifference curve that crosses the vertical axis at point *A* represents households with a child who does not attend school at all ($S = 0$) and devotes all of her free time to market work. Although it does not have to be so, for simplicity's sake, point *A* is depicted as a tangency point between the indifference curve of the household and the real wage line *W*.

The discontinuity of the budget constraint of the household, in combination with the assumption of utility maximization, implies that there is a minimum conditional cash transfer that will induce the household to send its child to school. Let $B'$ denote the point of intersection of the indifference curve of household *A* with the vertical line at $S_{min}$. Then the vertical difference $B' - B$ represents the minimum cash transfer that will make household *A* just indifferent between complying with the 85 percent attendance requirement and keeping its child out of school. A conditional cash transfer less than $B' - B$ is insufficient to induce children to attend school.

In Figure 1, it is implicitly assumed that the size of the conditional cash transfer $V' - V$ is greater than the minimum amount $B' - B$ needed to induce household *A* to enroll the child in school and comply with the 85 percent attendance requirement. In consequence, household *A* finds it to its advantage to enroll the child in school. As can be seen, participation in the program is likely to affect households differently, depending on their location on the budget line before the administration of the program. Consider household *C*, for example. Such a household may be considered to represent households

with children of primary school age (attendance rates for primary school children are close to 95 percent) or households with children of secondary school age who were regularly attending school before the administration of the program. Since the conditions are not binding, the program is likely to have only a pure income effect represented in Figure 1 by the parallel upward shift in the portion of the budget constraint between points $T$ and $S_{min}$. For these households the impact of the program may be concentrated at increasing the time they devote to schooling such as spending more time studying rather than enrollment.[6]

For a contrast, consider household *A.* At first sight, it appears that for this household it is impossible to attribute income and substitution effects to the program since the final equilibrium point $A'$ is not a tangency point. Yet, one can still apply the familiar concepts of income and substitution effects using the analytical framework of "linearizing" the budget constraint.[7] Linearizing the budget constraint amounts to transforming point $A'$ into a tangency point by drawing a line tangent to the indifference curve at $A'$ (i.e., finding the shadow wage $W^*$) and finding the corresponding level of non-earned income (or shadow income) $V^*$ that corresponds to the shadow wage $W^*$. It becomes apparent that household *A*'s participation in the program results in both substitution and income effects that tend to reinforce each other. The cash transfer component of the program leads to a pure income effect that increases schooling, while

[6] It should be noted that the program might also have important dynamic effects by increasing the probability that children continue on to higher grades in school. These dynamic effects of PROGRESA are explored by Behrman, Sengupta, and Todd (2001a).

[7] For more details, see Killingsworth (1983).

the condition that the child devote at least 85 percent of his time to school leads to a price effect. Based on standard economic theory, the price effect may be further decomposed into a substitution and income effect. At the final equilibrium point $A'$, the lower shadow wage $W^*$ ($< W$) represents the lower price of schooling as a result of the program, while the total increase in household income as a result of the program may be considered to be the cash transfer $V' - V$ plus the implicit extra income $V^* - V'$ earned as a result of the lower price of schooling.

To summarize, the economic framework presented above implies that participation in the program is likely to affect households differently depending on their constraints and preferences (or location on the budget line) before the administration of the program. For households for which the program constraints are binding, the program results in income and substitution effects that can reinforce its impact. For households for which the constraints of the program are nonbinding, the program is likely to have only income effects. Given the heterogeneity of households' preferences and constraints, the extent to which the program has a significant impact on the human capital and work of children can only be determined through empirical analysis. We now turn to a description of information sources and methods we use to evaluate empirically the impact of PROGRESA on children's human capital investment and work.

## 3. DATA, EMPIRICAL SPECIFICATION, AND RESULTS

The fundamental problem in the evaluation of any social program is the fact that households participating in the program cannot be simultaneously observed in the

alternative state of no treatment. For a proper evaluation of the impact of a program, it is necessary to observe a group of households similar to beneficiary households in every respect possible but do not benefit from the program. In the case of PROGRESA, the solution to this evaluation problem is achieved by random assignment of localities into treatment and control groups. Annual fiscal constraints and logistical complexities associated with the operation of PROGRESA in very small and remote rural communities did not permit the program to cover all of the eligible localities at once. Instead, the program covered localities in phases. PROGRESA's quasi-experimental design takes advantage of the sequential expansion of the program to select a comparable or control group from the set of localities that are eligible for, but not yet covered by, the program.

Specifically, the sample used in the evaluation of PROGRESA consists of repeated observations (panel data) collected for 24,000 households from 506 localities in the seven states of Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi, and Veracruz. Of the 506 localities, 320 localities assigned to the treatment group ($T = 1$) and 186 localities were assigned as controls ($T = 0$). Specifically, the 320 treatment localities were randomly selected using probabilities proportional to size from a universe of 4,546 localities that were covered by phase II of the program in the seven states mentioned above. Using the same method, the 186 control localities were selected from a universe of 1,850 localities in these seven states that were to be covered by PROGRESA in later phases. As originally planned, the localities serving the role of a control group started receiving PROGRESA benefits by December 2000.

The school attendance and work data used in this report come from the Survey of Household Socioeconomic Characteristics (*Encuesta de Características Socioeconómicas de los Hogares* or ENCASEH)) and the Evaluation Survey of PROGRESA ((*Encuesta Evaluación de los Hogares* or ENCEL). The Survey of Household Socioeconomic Characteristics is an economic census and is the data survey used to select which households in the eligible communities will participate in PROGRESA. The Evaluation Survey was designed especially for the purposes of the evaluation and consists of a baseline survey on the 24,077 households of the evaluation sample and follow-up surveys every 6 months.[8] We use a special time-use module carried out one time only as part of the June 1999 ENCEL that also allows us to look at hours spent in school and work, as well as analyze the impact of PROGRESA on participation and time spent in household work.

The quasi-experimental design of PROGRESA's evaluation represents a conscious attempt to ensure that the group that does not receive the treatment (control group) is similar in terms of both observable *and* unobservable characteristics to the group that does receive treatment (the treatment group). A successful randomization of the program can also ensure that all potential sources of bias are evenly distributed among treatment and control groups. This feature allows evaluators to attribute post-program differences between the treatment and control groups to the program.[9] Behrman

[8] The data include quite extensive information on numerous individual, household, and community characteristics, including all sources of income, labor market participation, demographic and socioeconomic information, children's school attendance, health utilization, and community characteristics, among others.

[9] See Heckman, La Londe, and Smith (1999).

and Todd examine the extent to which the selection of PROGRESA's localities may be considered as random in great detail.[10] They conduct a comparison of the means of key variables in two dimensions. First, they construct locality level means of all the household-level variables and then compare these means between the two groups (i.e., control and treatment). Second, they compare the means of the same variables between treatment and control groups using the household and/or individual level data. When these comparisons and tests are performed at the locality level (i.e., comparing locality means of age, education, income, access to health care, etc.), the hypothesis that the means are equal between treatment and control localities is not rejected. In contrast, performing the same comparison using household-level data, they find that the null hypothesis is rejected more frequently than would be expected by chance given standard significance levels. This latter finding was interpreted as being due to the fact that the samples are large, which means that even minor differences could lead to rejection.

In light of these earlier findings about preprogram differences between localities, Table 2a presents the mean of the variables used in our analysis separately for the samples of boys and girls in the treatment and control groups in the November 1997 round of the survey prior to the administration of the program. Clearly, there are some apparent differences in the means of treatment and control samples at the individual level. These differences are validated further by the estimates presented in Table 2b. In this table, the probability of being included in the treatment sample, separately for boys and

[10] Behrman and Todd (1999).

**Table 2a—Sample means of key variables**

| | Treatment | | Control | |
|---|---|---|---|---|
| | Boys | Girls | Boys | Girls |
| Variable | N=8,986 | N=8,200 | N=5,377 | N=5,282 |
| Attending school? (1=Yes 0=No) | 0.733 | 0.690 | 0.725 | 0.677 |
| Working? (1=Yes 0=No) | 0.236 | 0.088 | 0.216 | 0.064 |
| Age = 8 years (1=Yes 0=No) | 0.121 | 0.115 | 0.115 | 0.114 |
| Age = 9 years (1=Yes 0=No) | 0.106 | 0.111 | 0.100 | 0.104 |
| Age = 10 years (1=Yes 0=No) | 0.117 | 0.115 | 0.119 | 0.116 |
| Age = 11 years (1=Yes 0=No) | 0.105 | 0.116 | 0.109 | 0.113 |
| Age = 12 years (1=Yes 0=No) | 0.108 | 0.108 | 0.116 | 0.101 |
| Age = 13 years (1=Yes 0=No) | 0.098 | 0.102 | 0.100 | 0.106 |
| Age = 14 years (1=Yes 0=No) | 0.102 | 0.093 | 0.098 | 0.098 |
| Age = 15 years (1=Yes 0=No) | 0.097 | 0.091 | 0.097 | 0.092 |
| Age = 16 years (1=Yes 0=No) | 0.078 | 0.076 | 0.077 | 0.078 |
| Age = 17 years (1=Yes 0=No) | 0.068 | 0.073 | 0.070 | 0.078 |
| Missing mother characteristics (1=Yes 0=No) | 0.070 | 0.087 | 0.069 | 0.094 |
| Mother speaks indigenous language (1=Yes 0=No) | 0.351 | 0.345 | 0.347 | 0.322 |
| Mother speaks Spanish (1=Yes 0=No) | 0.279 | 0.279 | 0.260 | 0.236 |
| Mother's age | 36.252 | 35.425 | 36.107 | 35.270 |
| Mother is literate (1=Yes 0=No) | 0.565 | 0.564 | 0.555 | 0.548 |
| Mother completed primary school (1=Yes 0=No) | 0.565 | 0.565 | 0.561 | 0.553 |
| Mother completed secondary school (1=Yes 0=No) | 0.025 | 0.026 | 0.029 | 0.025 |
| Missing father characteristics (1=Yes 0=No) | 0.146 | 0.155 | 0.132 | 0.160 |
| Father speaks indigenous language (1=Yes 0=No) | 0.333 | 0.326 | 0.337 | 0.305 |
| Father speaks Spanish (1=Yes 0=No) | 0.302 | 0.297 | 0.314 | 0.283 |
| Father's age | 37.125 | 36.517 | 37.685 | 36.787 |
| Father is literate (1=Yes 0=No) | 0.614 | 0.619 | 0.630 | 0.617 |
| Father completed primary school (1=Yes 0=No) | 0.582 | 0.581 | 0.589 | 0.588 |
| Father completed secondary school (1=Yes 0=No) | 0.034 | 0.037 | 0.038 | 0.031 |
| Marginality Index | 0.483 | 0.473 | 0.536 | 0.524 |
| Distance of municipality center | 9.226 | 9.377 | 10.115 | 10.147 |
| Distance from secondary school | 2.231 | 2.224 | 2.229 | 2.296 |
| Children between 0 and 2 years of age | 0.438 | 0.448 | 0.419 | 0.447 |
| Children between 3 and 5 years of age | 0.589 | 0.610 | 0.600 | 0.611 |
| Boys between 6 and 7 years of age | 0.237 | 0.239 | 0.240 | 0.236 |
| Girls between 6 and 7 years of age | 0.225 | 0.237 | 0.226 | 0.236 |
| Boys between 8 and 12 years of age | 1.116 | 0.562 | 1.112 | 0.528 |
| Girls between 8 and 12 years of age | 0.530 | 1.103 | 0.527 | 1.083 |
| Boys between 13 and 18 years of age | 1.046 | 0.599 | 1.022 | 0.588 |
| Girls between 13 and 18 years of age | 0.526 | 0.973 | 0.556 | 0.994 |
| Males between 19 and 54 years of age | 1.083 | 1.095 | 1.107 | 1.097 |
| Females between 19 and 54 years of age | 1.161 | 1.149 | 1.184 | 1.160 |
| Males 55 years old or older | 0.180 | 0.172 | 0.182 | 0.189 |
| Females 55 years old or older | 0.151 | 0.138 | 0.164 | 0.150 |

girls, is related to observed individual and household characteristics. In a “pure” randomized design, observed individual or household characteristics should have no

significant role in predicting the assignment of an individual/household into the treatment sample. As the probit estimates in Table 2b reveal, there are some significant preprogram differences between treatment and control samples. For example, boys who attend school or who are working are more likely to be in the treatment sample. Also, boys and girls whose father speaks Spanish are less (more) likely to be in the treatment (control) sample.

These results are in general agreement with the findings of Behrman and Todd at the household level. Given that they cannot reject the equality of means at the locality level, we interpret our findings as providing strong ground for evaluating program impact using an estimator (such as the double-difference estimator discussed in more detail below) that measures program impact taking into account any preexisting differences in child school attendance and work rates.

School attendance is defined according to those who respond that the child attends school. This question is identical over the different rounds of analysis. Our definition of *working* includes all workers who report that they worked the previous week (whether paid or unpaid). There is also a follow-up question to capture individuals who may engage in informal activities but that the respondent may not have initially considered as work. This question asks about participation in (1) selling a product, (2) helping in family business, (3) making products to sell, (4) washing, cooking, or ironing, and (5) working in agriculture activities or caring for animals. Individuals who respond that they engage in any of these activities, we include also as working. It should be emphasized that domestic activities are *not* included in this definition of work.

## Table 2b—The probability of being in the treatment sample and individual/ household characteristics

| | Probit estimates | | | |
|---|---|---|---|---|
| | Boys | | Girls | |
| Variable | Coefficient | z-value | Coefficient | z-value |
| Attending school? (1=Yes 0=No) | 0.041 | 2.05 | 0.013 | 0.63 |
| Working? (1=Yes 0=No) | 0.049 | 2.14 | 0.092 | 2.66 |
| Age = 9 years (1=Yes 0=No) | 0.000 | 0.01 | 0.013 | 0.74 |
| Age = 10 years (1=Yes 0=No) | -0.020 | -1.23 | -0.003 | -0.21 |
| Age = 11 years (1=Yes 0=No) | -0.023 | -1.30 | 0.005 | 0.33 |
| Age = 12 years (1=Yes 0=No) | -0.037 | -2.16 | 0.013 | 0.69 |
| Age = 13 years (1=Yes 0=No) | -0.030 | -1.79 | -0.005 | -0.25 |
| Age = 14 years (1=Yes 0=No) | -0.015 | -0.83 | -0.010 | -0.47 |
| Age = 15 years (1=Yes 0=No) | -0.024 | -1.16 | 0.000 | 0.00 |
| Age = 16 years (1=Yes 0=No) | -0.022 | -0.94 | 0.000 | -0.01 |
| Age = 17 years (1=Yes 0=No) | -0.031 | -1.18 | -0.011 | -0.42 |
| Missing mother characteristics (1=Yes 0=No) | 0.068 | 1.12 | 0.070 | 1.09 |
| Mother speaks indigenous language (1=Yes 0=No) | -0.047 | -0.66 | -0.072 | -0.98 |
| Mother speaks Spanish (1=Yes 0=No) | 0.090 | 1.54 | 0.131 | 2.12 |
| Mother's age | 0.002 | 1.53 | 0.002 | 1.40 |
| Mother is literate (1=Yes 0=No) | 0.045 | 1.47 | 0.006 | 0.20 |
| Mother completed primary school (1=Yes 0=No) | -0.046 | -1.49 | -0.012 | -0.37 |
| Mother completed secondary school (1=Yes 0=No) | -0.079 | -1.51 | -0.026 | -0.50 |
| Missing father characteristics (1=Yes 0=No) | -0.008 | -0.12 | -0.115 | -1.58 |
| Father speaks indigenous language (1=Yes 0=No) | 0.136 | 1.85 | 0.146 | 2.15 |
| Father speaks Spanish (1=Yes 0=No) | -0.146 | -2.03 | -0.142 | -2.07 |
| Father's age | -0.001 | -0.64 | -0.003 | -1.86 |
| Father is literate (1=Yes 0=No) | -0.016 | -0.48 | 0.018 | 0.58 |
| Father completed primary school (1=Yes 0=No) | 0.011 | 0.35 | -0.036 | -1.02 |
| Father completed secondary school (1=Yes 0=No) | -0.023 | -0.47 | -0.010 | -0.20 |
| Marginality Index | -0.041 | -0.99 | -0.041 | -0.98 |
| Distance of municipality center | -0.006 | -1.40 | -0.005 | -1.13 |
| Distance from secondary school | 0.005 | 0.40 | 0.002 | 0.18 |
| Children between 0 and 2 years of age | 0.022 | 1.93 | 0.004 | 0.31 |
| Children between 3 and 5 years of age | -0.002 | -0.20 | -0.003 | -0.24 |
| Boys between 6 and 7 years of age | -0.004 | -0.27 | 0.001 | 0.04 |
| Girls between 6 and 7 years of age | -0.001 | -0.05 | -0.001 | -0.06 |
| Boys between 8 and 12 years of age | 0.004 | 0.42 | 0.019 | 1.81 |
| Girls between 8 and 12 years of age | 0.003 | 0.31 | 0.006 | 0.55 |
| Boys between 13 and 18 years of age | 0.009 | 1.11 | 0.007 | 0.83 |
| Girls between 13 and 18 years of age | -0.013 | -1.51 | -0.003 | -0.30 |
| Males between 19 and 54 years of age | -0.006 | -0.57 | 0.000 | -0.02 |
| Females between 19 and 54 years of age | -0.018 | -1.46 | -0.012 | -0.97 |
| Males 55 years old or older | 0.009 | 0.48 | 0.000 | -0.02 |
| Females 55 years old or older | -0.033 | -1.79 | -0.018 | -0.90 |
| Number of observations | 14,363 | | 13,482 | |
| Wald chi2(40) = | 51.080 | | 38.520 | |
| Prob > chi2 = | 0.113 | | 0.537 | |
| Pseudo R2 = | 0.012 | | 0.012 | |

**Source: Authors' calculations based on the November 1997 household census (ENCASEH).**
Note: All coefficients are expressed as marginal effects dF/dX.

The time-use survey allows us to broaden our definition of work to include these activities.[11] It collects information on 18 activities carried out during the previous day for all individuals age 8 or more.[12] In our analysis, we first construct overall measures of leisure time, defined as the difference between 24 hours and the time spent on all reported activities, that is, residual time. We also examine the composition of time spent. While there may be no effect on overall leisure time, there may be effects on substitution between different types of work. In particular, there is likely to be an increase in time spent on schooling and a reduction in time spent on work activities. We consider three types of work, including market work, farm work, and domestic work where we analyze participation and hours spent in each activity, as well as participation and time spent in school.

Figures 2 and 3 show the school enrollment rate and the labor force participation of boys and girls, respectively, by age, using the sample of all children from households eligible to receive PROGRESA benefits between ages 8 and 17 prior to implementation of the program, (i.e., from the ENCASEH of November 1997). It is noteworthy that for boys (Figure 2), the school enrollment rate is close to 95 percent, while the labor force

[11] In this section we do not estimate the impact of PROGRESA on hours spent in work but rather postpone this for the section on time use. This decision was made due to the fact that over time, the structure and design of the ENCEL questionnaires has changed such that it is difficult to compare hours worked before and after program implementation for workers. The after-program data result in the awkward problem that many individuals who declare they are working in the participation questions (which are identical over time) have no information for hours worked. Furthermore the time-use module allows us to include a broader definition of work, which includes domestic work and other unpaid activities as work.

[12] We exclude from our analysis children who were interviewed on Sunday or Monday, as they presumably would not have attended school the previous day.

**Figure 2—School enrollment and labor force participation of boys in PROGRESA communities prior to program implementation**

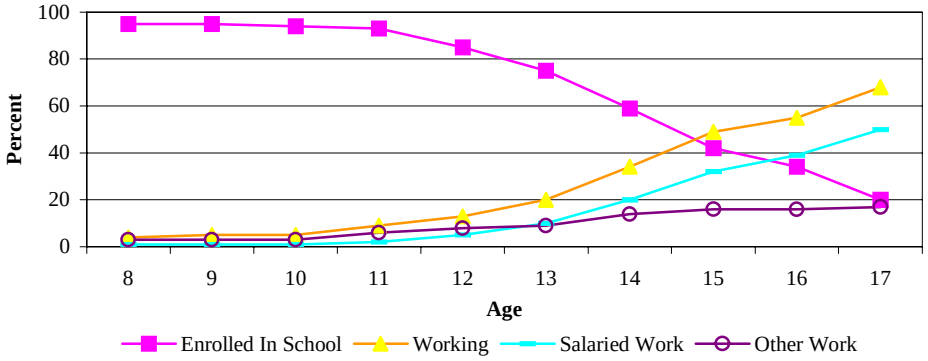
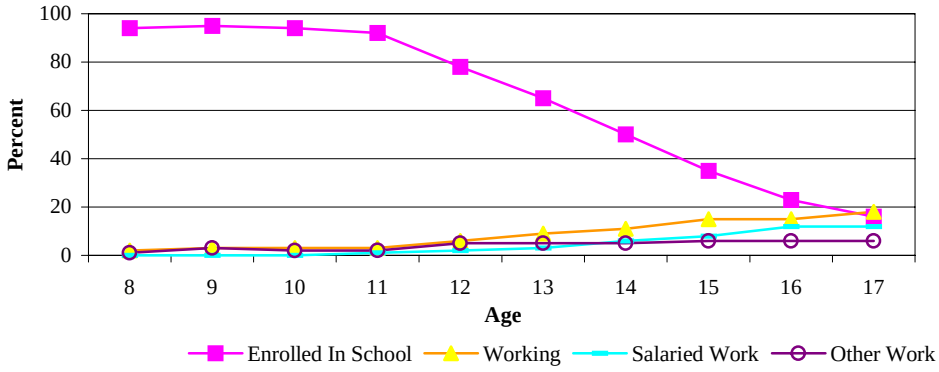



**Figure 3—School enrollment and labor force participation of girls in PROGRESA communities prior to program implementation**




participation is quite low (less than 5 percent) up until the ages of about 10 to 11 when the percentage of boys enrolled in school begins to decline and the percentage participating in the labor market begins to grow substantially. At early ages, participation is generally dominated by unsalaried work that is primarily self-employment and helping

in family businesses. That is, when children begin to work, they are likely to begin working in more nonsalaried rather than salaried work. By the age of 14, the percentage of children in salaried work begins to exceed that of other types of work. By the age of 16, the majority of boys report working and the majority of these workers are in salaried work.

For girls (Figure 3), it is also the case that labor force participation is extremely low at early ages. However, in contrast to boys, labor force participation increases very slowly with age. Girls at the age of 17, for example, have a low participation rate in the labor market, close to 17 percent. When girls do work at very young ages, they tend to be involved in nonsalaried activities. Above the age of 12, the chances that girls participate in salaried activities are approximately equal as they are for nonsalaried activities.

To get a better sense of the patterns of time use in the communities where PROGRESA operates, we use the time allocation module. Since this module was applied after the start of the program, we limit our descriptive analysis to the households and individuals in the control communities. Table 3 presents the overall participation and daily hours spent for each of our groups of analysis in each of the 18 activities covered by the time-use survey. Since the reference period is the previous day only, the overall levels of participation are likely to be lower than those based, say, on a two-week recall period. For instance, whereas it is likely that at least one individual of the family goes to the market at some point over the two week period (e.g., that the participation rate using a two-week period of reference would be close to 100 percent) the fact that our reference

period is short will underestimate the percentage of individuals who carry out this activity.

**Table 3—Time use in poor communities prior to program implementation (control group)**

| | Children aged 8 to 17 | | | |
|---|---|---|---|---|
| | Boys | | Girls | |
| Type of activity | Percent participation | Daily hours[a] | Percent participation | Daily hours[a] |
| Working for salary or wage | 8.4% | 7.6 | 2.8% | 7.7 |
| Working in own business | 0.3 | 3.8 | 0.3 | 3.8 |
| Working family land | 8.3 | 5.2 | 2.3 | 4.6 |
| Attending school | 67.5 | 5 | 64.3 | 5 |
| Doing homework after school | 66.5 | 1.1 | 63.7 | 1.1 |
| Community work | 1.5 | 2.5 | 1.4 | 2.4 |
| Voluntary work for neighbors or other relatives | 0.6 | 2.3 | 0.4 | 1.9 |
| Purchasing food or other products for household | 1.1 | 1.6 | 2.7 | 1.1 |
| Sewing, making clothes for household members | 0.3 | 1.4 | 2.9 | 1.2 |
| Taking household members to school, clinic, or work | 0.1 | 1.3 | 0.4 | 0.5 |
| Cleaning house | 0.5 | 1 | 29 | 1.1 |
| Washing and ironing clothes for household members | 0.2 | 1.1 | 20.1 | 1.5 |
| Preparing food | 0.2 | 1.5 | 21.4 | 1.3 |
| Fetching water, firewood, or throwing out trash | 28.6 | 1.1 | 25.5 | 0.9 |
| Taking care of animals | 11.2 | 1.6 | 7.2 | 1.1 |
| Taking care of small children, elderly and sick | 2.5 | 1.7 | 8.1 | 2.3 |
| Making household repairs | 2.1 | 1.8 | 0.8 | 1 |
| Transportation time to work, school, market, etc. | 58.7 | 0.4 | 50.6 | 0.4 |
| Other activities | 23.9 | 1.8 | 21.6 | 1.7 |

**Source: Authors' calculations based on the June 1999 evaluation survey (ENCEL).**

[a] Conditional on participating.

Table 3 shows that about two-thirds of children report attending school the previous day; of those attending, almost all report spending some positive time doing homework, approximately one hour a day, with no overall differences by gender. With respect to work activities, Table 3 shows some general differences by gender in terms of

the type of work children perform. Boys are more likely to be in salaried work than girls, although overall participation rates of both groups are low. Girls, on the other hand, have much higher participation in domestic activities such as cleaning, cooking, sewing, and preparing food, activities where boys have very minimum levels of participation. The only domestic activity where boys have a similar participation level as girls is the category of fetching water, firewood, and/or throwing out trash. Boys, however, do have slightly higher participation levels in working the family land and taking care of animals.

## MEASURING ELIGIBILITY

In our regressions, we focus only on the group of individuals (families) who are eligible for the program. The selection process of eligible households in the communities where PROGRESA operates consisted in the case of the early phases of the program in two steps. Originally, a set of households were selected and incorporated into the program according to the discriminant analysis procedure.[13] In the evaluation sample, the percentage of households selected corresponds to approximately 52 percent of all households in the communities. Nevertheless, to correct perceived errors in leaving out households, in particular, elderly households, a second selection took place in which an additional 25 percent of households in treatment communities were identified as eligible. However, it appears that some of these families experienced substantial delays in their incorporation. As of November, 1999, the date of our last survey used in this report, only

[13] For more details see Skoufias, Davis, and Behrman (1999) and Skoufias, Davis, and de la Vega (2001).

819 of the 3,023 newly eligible households had been incorporated, that is, 2,204 of these households had not yet begun to receive benefits.

In our evaluation, we use the sample of all eligible households, irrespective of whether some eligible households in treatment did in fact receive any benefits. Thus, the program effect estimated is inclusive of the operational efficiency or inefficiency with which the program operated. As such, our impact estimates thus measure the "mean direct effect of the offer to treat." Such estimates are less likely to be affected by selection biases associated with the choice to receive or possible attrition from the program. These estimates provide a lower bound for the impact of the "treatment on the treated" or the households that actually received the treatment.[14]

## IMPACT ON LABOR-FORCE PARTICIPATION AND SCHOOL ENROLLMENT OF CHILDREN

As a first step toward evaluating the impact of the program, Figures 4a-4b and 5a-5b present a straightforward comparison of the (unconditional) mean labor-force participation rate and school participation before and after the start of the program, in treatment and control villages for all boys and girls between 12 and 17 years of age in households selected as eligible for PROGRESA benefits. As can be easily seen, in the survey round before the start of the program (i.e., November 1997), there appear to be some differences in the labor-force participation rates of boys (and girls) between

[14] Parker and Skoufias (2000) provide estimates of the "treatment on the treated effect of the program" and find that the program has only slightly higher impact on those who actually receive treatment. This suggests that the bias due to attrition or selection is not serious.

**Figure 4a—Percent working: All boys 12-17 years old**

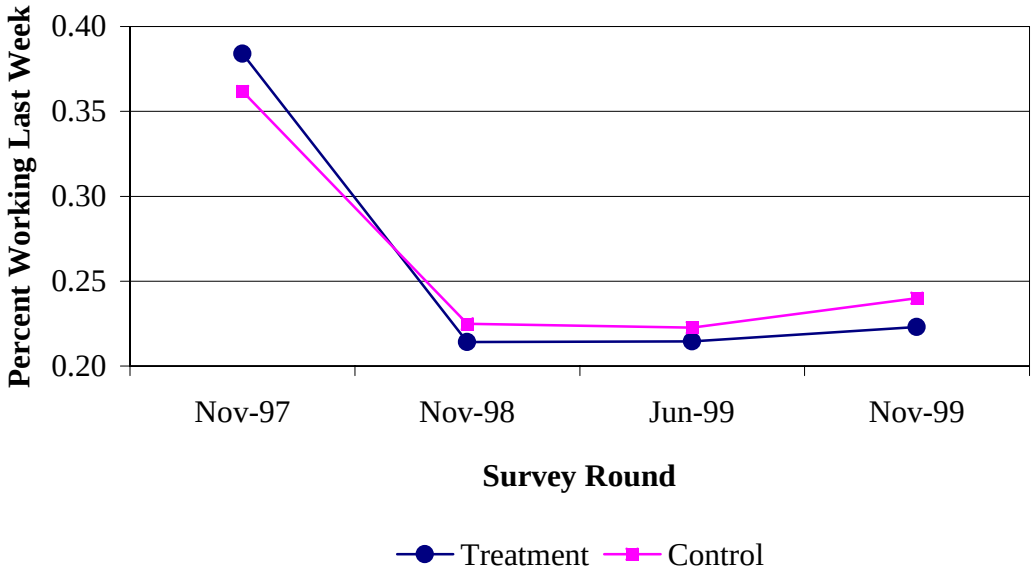
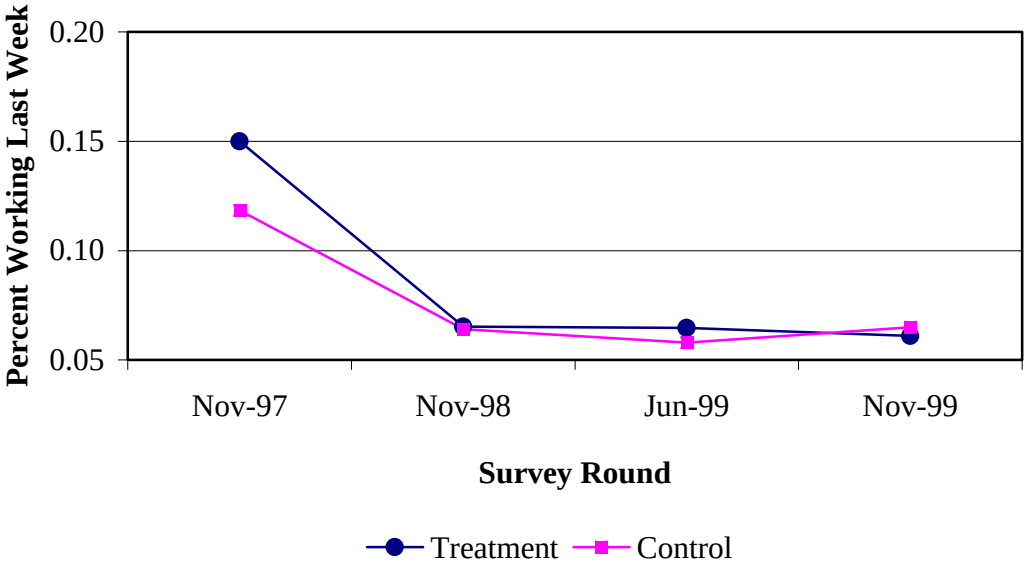



**Figure 4b—Percent working: All girls 12-17 years old**




**Source:** Authors' calculations based on the PROGRESA census in November 1997 (ENCASEH), and the November 1998, June 1999, and November 1999 PROGRESA evaluation (ENCEL) survey data.

treatment and control villages prior to the introduction of PROGRESA. For example, in November 1997, the labor force participation rate was slightly lower in control than in treatment villages. By November 1998 (the first round after the introduction of PROGRESA), the mean labor force participation rate of boys and girls in both treatment and control villages appears to decrease and remain at that lower level for the remaining rounds.

In contrast, Figures 5a and 5b reveal that the mean school attendance rate of both boys and girls were practically identical between treatment and control villages. By November 1998, the first round of the survey after the start of the program, the mean attendance rate of boys in treatment villages is noticeably higher than the mean attendance rate in the villages not yet covered by PROGRESA (see Figure 5a). Although mean attendance rates also show a slight increase in the control villages, the increase in the mean attendance rate in treatment villages is considerably higher. The same pattern can also be observed in the mean school attendance of girls of the same age category (compare Figures 5b and 5a).

The preceding analysis in combination with our earlier findings from Table 2b imply that to obtain a credible estimate of the impact of the program, it is essential to take into consideration the preexisting differences in the school attendance and work patterns of individuals in treatment and control localities. For this purpose, we adopt a regression-based approach and specify the empirical equation for participation in work (school) as:

**Figure 5a—Percent attending school: All boys 12-17 years old**

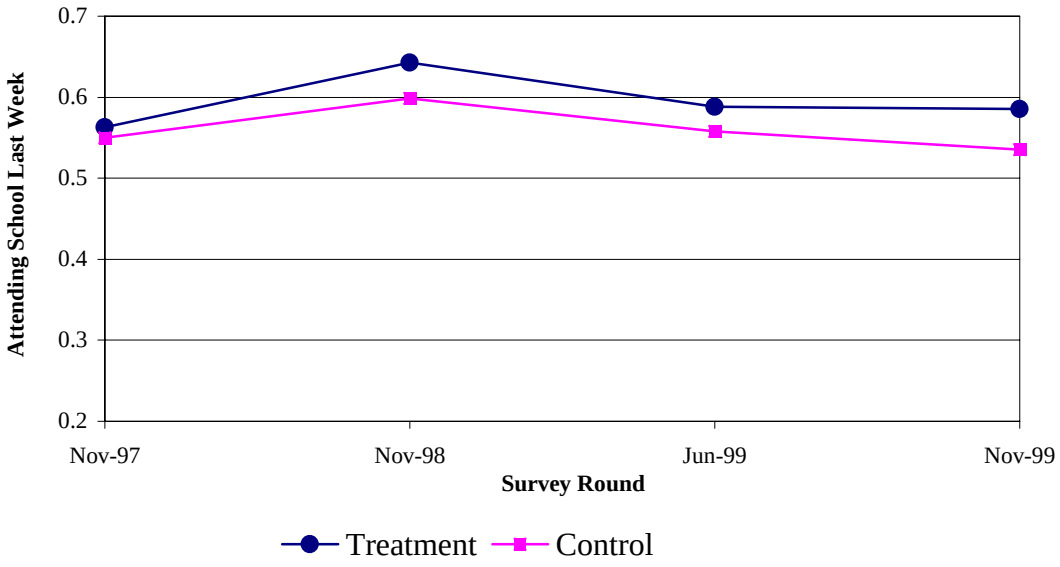
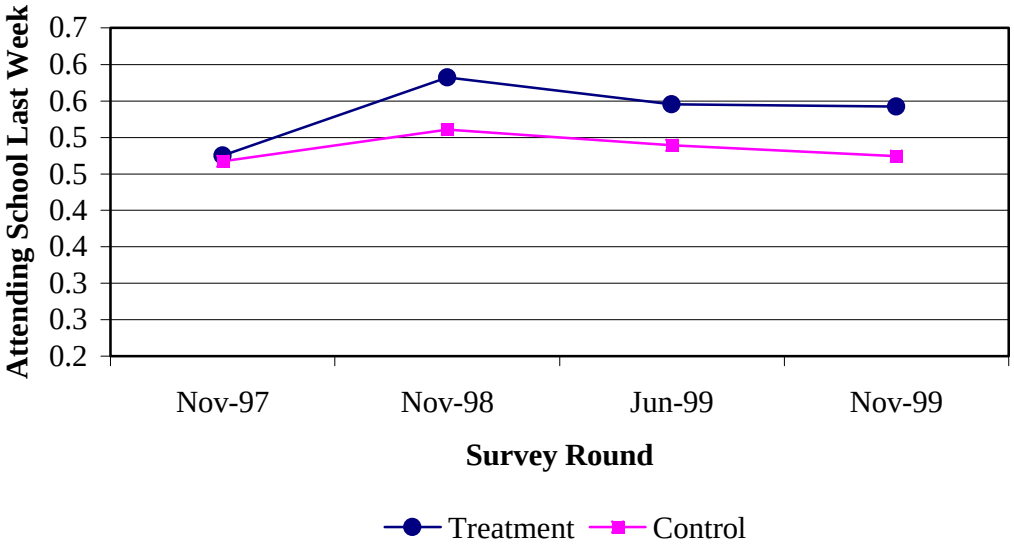



**Figure 5b—Percent attending school: All girls 12-17 years old**




**Source:** Authors' calculations based on the PROGRESA census in November 1997 (ENCASEH), and the November 1998, June 1999, and November 1999 PROGRESA evaluation (ENCEL) survey data.

$$Y(i,t) = \alpha + \beta_T T(i) + \beta_{R2}(R2) + \beta_{R3}(R3) + \beta_{R4}(R4) + \beta_{TR2}(T(i) * R2)$$

$$+ \beta_{TR3}(T(i) * R3) + \beta_{TR4}(T(i) * R4) + \sum_{j=1}^{J} \theta_j X_j(i,t) + \eta(i,t), \qquad (1)$$

where

$Y(i,t)$ = the work (school) outcome indicator for individual $i$ in period $t$,

$\alpha$, $\beta$, $\theta$ = fixed parameters to be estimated,

$T(i)$ = binary variables taking the value of 1 if the household belongs in a treatment community and 0 otherwise (i.e., for control communities),

$R2$, $R3$, $R4$ = binary variables equal to 1 for the second, or third, or fourth rounds of the survey, respectively, *after* the initiation of the program and equal to 0 otherwise,

$\boldsymbol{X}$ = a vector of household and village characteristics, and

$\eta$ = an error term summarizing the influence of random disturbances.

The vector $\boldsymbol{X}$ of control variables consists of parental characteristics, such as the education level of the mother and father of the child, the age of the mother and father, whether parents speak an indigenous language, and whether they also speak Spanish.[15] We also include a number of variables measuring the demographic composition of the

[15] Missing variable dummies are also included in the regressions for the cases in which data are not available (e.g., the father no longer lives in the household).

household. These variables include the number of children ages 0–2 and 3–5, boys and girls ages 6–7, 8–12, and 13–18, men and women ages 19–54, and men and women over age 55. As control variables at the community level, we include an index variable constructed by the PROGRESA administration as a means of summarizing the infrastructure and the level of development of the locality (otherwise known as the marginality index) and a variable measuring distance to the *cabecera municipal*, which is an indicator of distance to the governing center of the municipality (and likely the largest locality of the municipality). This may be taken to be an indicator of the availability of local labor markets. It may, nevertheless, have different impacts on both school and work. Closer available labor markets may make (paid) work more attractive and reduce schooling or, in fact, it may make school more attractive by providing more information about the expected returns to schooling.[16] Finally, we also include a variable measuring distance to the closest secondary school. This provides an indicator of the cost of attending school and thus is likely to affect the relative time spent in both school and work.

The different intercept $\alpha$ terms capture the point that participation in work (school) may vary (for reasons unrelated to PROGRESA) over each round of the analysis. The coefficient $\beta_T$ allows the conditional mean of participation in work or school to differ between eligible households in treatment and control localities before the initiation of the program. A test of the significance of $\beta_T$ also serves the role of a test of

[16] We do not attempt to construct at the individual level predicted wages for children, given the large number of children who do not work for an income.

the randomness in selection of localities. For if there were a truly random selection of localities into control and treatment, then the conditional mean of the outcome indicator should be identical across treatment and control households/individuals.

The coefficients $\beta_{TR}$ associated with the interaction of the treatment dummy $T(i)$ with the dummy variables indicating the round of the survey yield the 2*DIF* estimate of the impact of the program in each round. In this manner, we can examine whether impacts are constant, decreasing, or increasing over time, as well as whether there are seasonal effects. In relation to the discussion in Section 2, the coefficients $\beta_{TR}$ also provide an estimate of the impact of the various income and substitution effects within households induced by participation in the program.[17]

For a better understanding of how the 2*DIF* estimator measures program impact, consider equation (1) above for the simple case where there are only two survey rounds: one round after the start of the program, denoted by $R2 = 1$, and one round before the start of the program, denoted by $R2 = 0$. Then the conditional mean values of the outcome indicator for treatment and control groups before and after the start of the program are as follows:[18]

[17] Given that the variables used to evaluate the impact of the program on schooling and child labor are binary, we adopt a reduced-form approach instead of attempting to decompose the impact of the program into Hicks/Slutsky substitution and income effects. These effects are meaningful and best estimated empirically when data are available of hours of schooling and work (Heckman 1978). For an analysis that decomposes the impact of PROGRESA into income and substitution effects, ignoring the binary nature of the dependent variables, see Demombynes (2001).

[18] Expressions in equations (2a)-(2d) rely on the assumption, $[E(\eta \mid T, R2, \boldsymbol{X})] = 0$, for $T = 1,0$, $R2 = 0,1$ and all $\boldsymbol{X}$.

$$[E(Y \mid T = 1, R2 = 1, \mathbf{X})] = \alpha + \beta_T + \beta_R + \beta_{TR} + \sum_j \theta_j X_j , \quad (2a)$$

$$[E(Y \mid T = 1, R2 = 0, \mathbf{X})] = \alpha + \beta_T + \sum_j \theta_j X_j , \quad (2b)$$

$$[E(Y \mid T = 0, R2 = 1, \mathbf{X})] = \alpha + \beta_R + \sum_j \theta_j X_j , \quad (2c)$$

$$[E(Y \mid T = 0, R2 = 0, \mathbf{X})] = \alpha + \sum_j \theta_j X_j . \quad (2d)$$

The 2*DIF* estimator provides an estimate of the impact of the program that is net of any preprogram differences between treatment and control households and/or any time trends or aggregate effects in changes of the values of the outcome indicator. Specifically,

$$\begin{aligned} \beta_{TR} &= 2DIF = (2a - 2b) - (2c - 2d) = (2a - 2c) - (2b - 2d) \\ &= [E(Y \mid T = 1, R2 = 1, \boldsymbol{X}) - E(Y \mid T = 0, R2 = 1, \boldsymbol{X})] \\ &\quad - [E(Y \mid T = 1, R2 = 0, \boldsymbol{X}) - E(Y \mid T = 0, R2 = 0, \boldsymbol{X})]. \end{aligned} \quad (3)$$

The clustering of the households within villages implies that the household-specific error terms $\eta(i,t)$ are likely to be correlated within each village (as well as across time). Failure to account for such a correlation may lead to a considerable bias in the estimated standard error of the program impact.[19] For this reason, the regression models

[19] For a clear discussion of these issues, see Murray (1998).

account for the clustered nature of the sample and report robust standard error estimates for the impact of the program.[20]

The estimates are obtained by estimating equation (1) using probit. In the analysis of school enrollment, $Y(i,t)$ equals 1 if child $i$ is attending school in the week prior to the interview in round $t$ and is 0 otherwise. In the analysis of child work, $Y(i,t)$ equals 1 if child $i$ is working in the week prior to the interview in round $t$ and is 0 otherwise. Each of the probit equations for child work and schooling were estimated independently of each other by imposing the restriction that disturbance terms in each of the equations are uncorrelated.[21] Given the large number of regressions, we only report the results of the impact of PROGRESA. The complete results with the control variables are available on request.

Table 4 presents the results of the impact of PROGRESA on the probability of working of children. We use the sample of all eligible households in treatment and control villages from the November 1997 round as our baseline round and three post-program rounds of the Evaluation Survey (ENCEL), including the November 1998 round, the June 1999 round, and the November 1999 round.[22] In recognition of the heterogeneity in the potential impact of the program, and given that children of secondary

[20] Robust standard error estimates were obtained using the "robust" option in STATA v6.0.

[21] We have also estimated the model using a bivariate probit model that allows for correlated disturbances and confirmed that the main results do not change.

[22] We use the ENCASEH rather than the ENCEL-Mar98 survey as our baseline of labor market participation, as the March 1998 survey did not include information on labor force participation. Fortunately, the labor market participation questions in the ENCASEH and the remaining evaluation surveys are identical.

## Table 4—The impact of PROGRESA on the probability of working: Boys and girls

| | Difference in difference estimates | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Boys | | | | | | | Girls | | | | | |
| | | Impact | | | | | | | Impact | | | | |
| | **Pre-program level** | November-98 | | June-99 | | November-99 | | **Pre-program level** | November-98 | | June-99 | | November-99 | |
| Age group | | Coefficient | T-statistic | Coefficient | T-statistic | Coefficient | T-statistic | | Coefficient | T-statistic | Coefficient | T-statistic | Coefficient | T-statistic |
| 8 to 11 | **0.0620** | -0.013 | -2.0 | -0.009 | -1.4 | -0.011 | -1.3 | **0.0353** | -0.005 | 0.8 | -0.003 | -0.6 | -0.000 | -0.5 |
| 12 to 17 | **0.3775** | -0.032 | -1.6 | -0.033 | -1.6 | -0.047 | -2.1 | **0.1317** | -0.018 | -1.7 | -0.011 | -1.0 | -0.023 | -1.8 |
| 12 to 13 | **0.1715** | -0.016 | -1.0 | -0.025 | -1.6 | -0.038 | -2.2 | **0.0870** | -0.015 | -1.6 | -0.011 | -1.1 | -0.007 | -0.7 |
| 14 to 15 | **0.4058** | -0.045 | -1.7 | -0.041 | -1.5 | -0.042 | -1.4 | **0.1495** | -0.032 | -2.3 | -0.023 | -1.5 | -0.038 | -2.4 |
| 16 to 17 | **0.6299** | -0.028 | -0.8 | -0.016 | -0.4 | -0.052 | -1.3 | **0.1727** | 0.007 | 0.3 | 0.017 | 0.7 | -0.020 | -0.8 |

**Source: Authors' calculations based on the PROGRESA November 1997 census (ENCASEH) and the November 1998, June 1999, and November 1999 PROGRESA evaluation surveys (ENCEL).**

Notes:

1. The coefficients reported are the marginal effects of the PROGRESA program on the probability of working.
2. T-values calculated are based on robust standard errors that account for clustering of individuals within villages.
3. See text for a detailed description of the other control variables used in the regression.

school age are more likely to be working or out of school or involved in both activities at the same time, we split the sample into two groups: children between ages 8 and 11 (primary school ages) and children between ages 12 and 17 (secondary school ages). We also examine the age groups of children 12 to 13, 14 to 15, and 16 to 17 separately, thus allowing the impact of the program to be different across these age groups. Previous research has shown that the highest attendance impacts of PROGRESA are at the secondary level.[23]

The results are presented showing the initial level of participation in work activities (that is, prior to program implementation) and the impact estimates for each round of the ENCEL carried out after program implementation. The impact from each round should be interpreted as the percentage point difference from the preprogram level (not from the previous round). In other words, the estimates reported represent the marginal effects of being in a household eligible for PROGRESA benefits on the probability of being in the labor force.[24]

The results based on Table 4 show clear negative impacts of PROGRESA on children's work. Beginning with the group of boys ages 8 to 11, the results show a consistently negative impact on work in the first round of the ENCEL. For instance, in November of 1998, the results show a reduction in 1.3 percentage points of the probability of working for boys ages 8 to 11, whose overall participation rates prior to the

[23] See Schultz (2000) and Coady (2000).

[24] The estimates reported were obtained using the "dprobit" command in STATA v6.0. They can be easily converted into percentage changes or elasticities by dividing the marginal effect by the preprogram level, both reported in Tables 4 and 5.

program were 6.2 percent. Thus, PROGRESA accounts for a reduction of approximately 21 percent (0.013/0.062) in the probability of working for this group. In contrast for boys between 12 and 17 years of age, the program seems to have a lower negative impact on labor force participation. For older boys, PROGRESA accounts for a reduction of 12.4 percent (November 1999) to 8.5 percent (November 1998 and June 1999 rounds) in the probability of working.

For boys ages 12 to 13 in the November 1999 round, PROGRESA reduces the probability of working by 22 percent relative to their probability of working prior to the program (0.038/0.1715). For boys ages 14 to 15, the estimates show a significant reduction of about 11 percent in the probability of working only in the first-after program round, which is insignificant in later rounds. For boys ages 16 to 17, there is no significant reduction in the probability of working.

For girls of primary school age, who have about half of the working rate of boys, the program does not appear to have any impact. However, for girls between 12 and 17 years of age, in spite of their overall lower labor force participation level prior to the program, there are some significant reductions associated with PROGRESA. The average participation rate of girls ages 12–17 prior to the program was 13.17 percentage points, and PROGRESA reduced this participation by 1.8 percentage points in November 1998 and 2.3 percentage points in November 1999. These effects correspond to approximately a 14 percent and 17 percent reduction in the probability of working.

As with boys, the analysis shows larger effects on girls ages 12–17, principally concentrated on girls ages 14–15. For girls ages 12–13, the effects are significant only in

the first-after program round, and show a reduction in participation equivalent to a reduction from the preprogram level of about 17 percent. For girls ages 14–15, the effects are consistently large and significant over time, showing a reduction in the probability of work ranging from (depending on the round) 20 to 25 percent. As with boys, the effects of PROGRESA on work are not significant for girls ages 16–17. PROGRESA does not appear to have much success at reducing the work of boys and girls in this age group.

Based on the identical sample of children, Table 5 reveals that the negative impacts on participating in work activities are accompanied by negative and significant impacts on the probability of attending school. PROGRESA increases the attendance rate of children of primary school age 1.3 percentage points in November 1998 and 1.8 percentage points in November 1999. These impacts amount to a 1.4 and 1.9 percent increase in the fraction of boys of primary school age attending school, respectively. No significant impacts are found on the attendance rate of girls in the same age group. As mentioned in Section 2, these low impact estimates are not surprising, considering that the attendance rates of both boys and girls in this age group are already high—close to 94 percent. For households with children in this age group, the conditions of the program are not binding, and as a result the program is likely to have only an income effect. Interestingly, in November 1998, the marginal effect of the program on the school attendance of boys is 1.3 percentage points, which is identical to the negative marginal effect of the program on their participation rate in work activities. This suggests that in the first round after PROGRESA, the increased school attendance rate of younger boys is

## Table 5—The impact of PROGRESA on the probability of attending school: Boys and girls

| | Difference in Difference Estimates | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Boys | | | | | | Girls | | | | | |
| | | Impact | | | | | | Impact | | | | | |
| | | November-98 | | June-99 | | November-99 | | November-98 | | June-99 | | November-99 | |
| Age group | **Pre-program level** | Coefficient | T-statistic | Coefficient | T-statistic | Coefficient | T-statistic | **Pre-program level** | Coefficient | T-statistic | Coefficient | T-statistic | Coefficient | T-statistic |
| 8 to 11 | **0.9363** | 0.013 | 1.8 | 0.011 | 1.6 | 0.018 | 2.7 | **0.9402** | 0.003 | 0.1 | 0.006 | .01 | -0.003 | 0.3 |
| 12 to 17 | **0.5678** | 0.043 | 2.4 | 0.032 | 1.8 | 0.058 | 2.8 | **0.4807** | 0.078 | 4.3 | 0.075 | 3.8 | 0.095 | 4.3 |
| 12 to 13 | **0.8128** | 0.025 | 1.5 | 0.023 | 1.3 | 0.033 | 1.8 | **0.7184** | 0.058 | 3.1 | 0.067 | 3.2 | 0.075 | 3.7 |
| 14 to 15 | **0.5263** | 0.063 | 2.3 | 0.053 | 2.1 | 0.050 | 1.7 | **0.4312** | 0.092 | 3.4 | 0.101 | 3.4 | 0.109 | 3.7 |
| 16 to 17 | **0.2780** | 0.026 | 0.9 | 0.009 | 0.3 | 0.054 | 1.9 | **0.2070** | 0.031 | 1.3 | -0.002 | -0.1 | 0.018 | 0.7 |

**Source: Authors' calculations based on the PROGRESA November 1997 census (ENCASEH) and the November 1998, June 1999, and November 1999 PROGRESA evaluation surveys (ENCEL).**

Notes:

1. The coefficients reported are the marginal effects of the PROGRESA program on the probability of attending school.
2. T-values calculated are based on robust standard errors that account for clustering of individuals within villages.
3. See text for a detailed description of the other control variables used in the regression.

obtained exclusively by boys withdrawing from work activities instead of combining school with work.

The analysis reveals that PROGRESA has a larger positive effect on the attendance rates of boys and girls of secondary school age. The marginal effects for boys between 12 and 17 years of age are all significant in every round after the start of the program and amount to a 7.6 percent, 5.6 percent, and 10.2 percent increase in the attendance rate of boys in November 1998, June 1999 and November 1999 rounds, respectively. For girls the effects of the program are even higher.[25] In November 1998, the attendance rate increases by 16 percent, while by November 1999, the attendance rate increases by 19.8 percent relative to the preprogram level.

In general, however, the displacement of the incidence of child work is smaller than the gain in schooling for both boys and girls. When significant, the estimated marginal effects of PROGRESA on the probability of school enrollment of boys turn out to be only slightly higher (in absolute value) than the marginal effects of the program on the probability that boys participate in work activities. For example, in November 1998, PROGRESA results show a decrease of the work activity participation rate of boys between 12 and 17 years of age by 3.2 percentage points (see Table 4) and an increase in the incidence of school enrollment by 4.3 percentage points (see Table 5).

One interpretation of these results is that the increased rate of school attendance of secondary school age boys is obtained mainly by boys withdrawing from labor force activities rather than combining school with work. In other similar programs, such as the

[25] This is similar to the result obtained by Schultz (2000).

Food for Education program in Bangladesh, the lower incidence of child labor was found to account for 25 percent of the increase in the fraction of boys attending school, implying that the program cuts children's leisure time.[26] The lower incidence of child work due to the PROGRESA program appears to account for a considerably higher percentage of the increase in school enrollment, ranging from 74 percent of the increase in the enrollment of boys ages 12–17 in school in November 1998 (the first school year after the start of the program) to 81 percent in November 1999 (the second year of the program).

In contrast to boys, the estimated marginal effects of PROGRESA on the fraction of girls attending school are considerably higher (in absolute value) than the marginal effects of the program on the probability that girls participate in work activities. The lower incidence of work among girls due to the PROGRESA program accounts for 23 percent of the increase in the attendance rate of girls 12 to 17 years of age in school in November 1998 (the first school year after the start of the program) to 24 percent in November 1999 (the second year of the program). Also, the estimated effect of PROGRESA on schooling is much larger for girls than for boys. Given that the participation of girls in work activities as defined is already quite low, these results suggest that most of the increased school attendance of girls is most likely occurring by girls combining domestic work with school. Whether this is indeed the case can only be addressed by closer investigation of the time-use survey in the later half of this report.

[26] See Ravallion and Wodon (2000).

In summary, the results show important negative effects on the probability of children participating in work, both for boys and girls. In fact, in proportional terms, the reduction in the probability of working is similar for boys and girls, although, given the higher preprogram participation rate of boys in work, the absolute reductions for boys are of course larger. The results also show generally large increases in school enrollment, particularly for girls. Whereas for boys, the increases in school enrollment are similar to the reductions in work, the increases in school enrollment for girls are much larger than their reduction in work, suggesting either that girls reduce their leisure time, or that other types of work are reduced.

## IMPACT ON LEISURE AND TIME USE

Given that the time-use module use was carried out only once (approximately one year after program implementation), we cannot employ the same double-difference estimator. This limits us to using the cross-sectional difference estimator that compares the post program differences in the means between treatment and control groups. Using the sample of eligible households, the leisure time of individual *i* denoted by *L*(*i*) is specified as

$$L(i) = \alpha_0 + \gamma T_i + \sum_{j=1}^{J} \theta_j X_j(i) + \eta(i), \tag{4}$$

where $T(i)$ represents a binary variable equal to 1 if individual *i* lives in a treatment community and 0 otherwise, and $X_j(i)$ represents the vector of *J* control variables for individual *i* (described above).

Equation (4) is estimated using OLS.

Note that since we only have one round of data for time use, the impact of PROGRESA is measured by a simply dummy variable indicating whether the family lives in a treatment community or a control community. Specifically the coefficient γ provides an estimate of the cross-sectional difference in the conditional mean leisure between children in treatment and control communities, i.e.,

$$\gamma = [E(L \mid T = 1, \boldsymbol{X}) - E(L \mid T = 0, \boldsymbol{X})]. \quad (5)$$

Along the same line, participation in activities is analyzed using a probit model of the form:

$$P^A(i) = \alpha_0^* + \gamma^* T(i) + \sum_{j=1}^{J} \theta_j^* X_j(i) + \eta(i) \quad (6)$$

where $P^A(i)$ is a binary variable taking the value 1 if individual *i* participates in activity *A* and 0 otherwise, and the rest of the variables are as specified above.

The earlier findings about pre-existing differences in the work rates of boys and girls between the treatment and control samples suggest that the estimates of the impact of the program on time use and leisure may be biased. Although it is not possible to determine whether preprogram hours devoted to specific activities and leisure were different, we can at least use our estimates of equation (1) to obtain some valuable information preprogram differences in child school enrollment or work rates. For the two different activities and the five different age groups (10 cases in total) analyzed in Tables 4 and 5, we are able to reject the hypothesis that there are no significant differences in the mean values of these variables among individuals in treatment and control localities prior to program implementation (i.e., $\beta_T = 0$ in equation [1]) in 20 percent of the cases for boys. In contrast, for girls, we rejected the same hypothesis for 40 percent of the cases in total and for 80 percent of the cases concerning work only. This suggests that, at least for boys, even post-program comparisons between treatment and controls are likely to be unbiased estimates about the impact of the program.

The analysis of the impact of the program on the daily hours spent on activities is somewhat more complicated by the censoring of hours at zero for children not participating in different work activities. To account for the censoring at zero, we use Heckman's two-stage method for correcting for selection bias.[27] Thus, in order to find the impact of the program on the hours spent on each activity, we estimate an equation of the form:

[27] See Heckman (1979).

$$H^{A}(i) = \alpha + \gamma T(i) + \sum_{j=1}^{J} \theta_j X_j(i) + \delta\lambda(i) + \eta(i), \quad (7)$$

where $H^A(i)$ and $\lambda(i)$ represent the inverse Mills' ratio calculated from the first-stage probit equation for participation in activity.

Market work consists of all salaried work as well as work corresponding to a business or selling products. Farmwork is defined as working on land (including but not limited to family land) as well as caring for animals. Finally, domestic work consists of (1) realizing purchases for the family, (2) making clothes for family members, (3) taking a family member to school, work, health center or hospital, (4) cleaning the house, (5) washing and ironing clothes, (6) cooking, (7) fetching water, firewood, or disposing of trash, and (8) caring for small children or elderly or sick individuals. Leisure is defined as total hours in a day (e.g., 24) minus time spent in all work activities as well as in other nonwork areas such as transportation.

Note that the reference period for the time-use questions refers only to time spent in the activity during the previous day. This is not particularly ideal, since for some children, the survey may refer to a day that was not "typical" of normal activities. Additionally, many activities may be activities that are undertaken infrequently (i.e., not daily) such that the survey is likely to underestimate participation in certain activities.

The survey was carried out in this way as it was thought it would reduce recall bias, given the large number of activities included in the questionnaire.[28]

Nevertheless, the format implies that the impacts on these variables must be interpreted with caution. In particular, in the case of schooling, children may in fact be enrolled in school, but not attending the previous day. That is, our participation measure in schooling effectively captures both enrollment and attendance. We are fortunate, nevertheless, to have more direct information on enrollment from the main ENCEL survey so that we are able to evaluate the extent to which our school participation variable underestimates enrollment. As expected, comparing the percentage of children who report spending some time in school the previous day results in a lower estimate of children enrolled in school versus the more direct measure of enrollment, e.g., is your child attending school. The bias is overall about 15 percent, which is of children reporting

[28] Analyses of time use generally suffer from the defect that individuals may engage in more than one activity simultaneously, for instance, cooking and caring for children at the same time. The survey actually tries to get at this point through a series of questions that ask individuals about the activities where they spent most time and which activities they carried out at the same time. While well-intentioned, the questions are very difficult to analyze, particularly as there is no way to judge how much time was spent doing two activities at the same time. Furthermore, many of the reported activities done simultaneously are difficult to interpret. For instance, for almost a third of cases where individuals report doing two activities at the same time, one of the activities is transportation, whereas the other activity is in most cases either school attendance or paid work activities. It does not seem plausible that both were done at the same time, but rather that they are related activities done at different times. For this reason, we ignore the issue of activities that may be done at the same time so that our estimates of leisure and time spent in each activity may run the risk of being slightly overestimated. In particular, time spent in domestic work may be overestimated. It should not bias the impact results on time spent unless it is the case that PROGRESA makes beneficiaries more (or less) likely to do more than one activity at the same time. To the extent that it is possible to check this point with the available data, it does not appear that this is the case.

they were enrolled in school, about 15 percent reported 0 hours spent at school the previous day in the time-use survey.[29]

Our basic control variables are identical to those included above in the labor force participation analysis. To identify the Heckman models, we use distance to school and to local labor markets as identifying variables for children, which we hypothesize will affect the probability of activity participation in school or work, but not the amount of time spent in each activity.[30]

Before presenting the empirical results, it is worth recalling that the results previously presented on the labor force participation of children showed that the lowest impacts generally occurred in the June 1999 ENCEL. This may reflect seasonality in the work of children, e.g., there may be a greater need for child work during the summer months (June through September). Alternatively, it may reflect that many interviews were likely carried out at, or close to, the end of the school year and so, children may have fewer conflicts with the time they dedicate to work. That is, during the summer months when school is not in session, the incentive of PROGRESA to reduce children's

[29] This would seem to suggest a high rate of absenteeism, which is largely explained by the point that the school year is almost over and absenteeism is higher at the end of the school year. The reasons most commonly given for a child missing days of school are illness, work, and teacher absenteeism.

[30] Note that through its benefits, PROGRESA is likely to increase school enrollment; nevertheless, those students who re-enroll in school (those not enrolled prior to the program) are not necessarily representative of those students who were attending before receiving program benefits. For instance, they may be lower ability students who are less likely (or able) to spend time doing homework, so that they may actually lower the average time that children dedicate to schooling, as compared with the control group. It might then appear (falsely) as if PROGRESA had reduced (or had a lower increase than expected) on the amount of time spent on schooling. One way to correct for this issue is if one knows which children were in school prior to the program. While our time-use survey was only carried out once after the program, we do have other variables on school enrollment carried out from a survey prior to program implementation, which we can link to our time-use sample. Therefore, we repeat the analysis, eliminating children from the sample who were not previously in school but re-enrolled after beginning to receive PROGRESA benefits. The results were similar, and are not reported here.

work effort may be to a large degree eliminated. It is perhaps unfortunate that this same period was when the only time-use module was carried out. It seems likely that the results reported here may to some extent underestimate the impacts that might be obtained if data had been collected during other months.

Depending on the time at which the survey was carried out, some children may have already been out of school, and thus their time allocation is much less likely to have been affected by the program. While school did not officially get out until the middle of July, we consider it possible that schools in rural areas may end early, or that rates of attendance may decrease as the end of the school year approaches. To make sure we are excluding interviews when school is no longer in session, we exclude all interviews that were carried out after July 4. For interviews carried out after July 4, the proportion of children who report attending school the previous day decreases considerably.[31]

Table 6 presents the results on the impact of PROGRESA on total leisure time for boys and girls. For boys, PROGRESA does not appear to have significant effects on leisure time of boys. The results of the impact of PROGRESA show consistently insignificant effects for boys at all age groups. Nevertheless, for girls, PROGRESA has a negative and significant effect on leisure time. The size of the impact for the overall group of girls ages 8–17 is, however, relatively small, corresponding to about 0.2 hours per day (or 1.4 hours per week). Nevertheless, this negative effect is largely concentrated on girls ages 12–13, who show larger reductions in leisure time, corresponding to about

[31] The possibility that school may end earlier in these rural isolated communities (or that absenteeism may be higher) than according to the national schedule set by the Secretary of Education is, of course, worrying. More analysis is necessary to understand the reasons why children appear to have a lower attendance toward the end of the school year.

0.4 hours per day, or about 2.8 hours per week. These effects suggest, given the large impact of PROGRESA on increasing school enrollment of girls, that girls may increase schooling by more than they reduce work, even when a broad definition for work is used. We will look at this hypothesis in more detail below.

**Table 6—The impact of PROGRESA on leisure: Boys and girls**

| | Boys | | | Girls | | |
|---|---|---|---|---|---|---|
| | Pre-program | Impact | | Pre-program | Impact | |
| Age group | daily hours | Coefficient | T-statistic | daily hours | Coefficient | T-statistic |
| 8 to 17 | 17.37 | -0.018 | -0.2 | 17.74 | -0.196 | -2.4 |
| 12 to 13 | 17.38 | -0.113 | -0.7 | 17.55 | -0.317 | -1.9 |
| 14 to 15 | 16.82 | 0.020 | 0.1 | 17.37 | -0.211 | -1.0 |
| 16 to 17 | 16.80 | 0.204 | 0.8 | 18.00 | 0.010 | 0.0 |

**Source: Authors' calculations based on the June 1999 (ENCEL) PROGRESA evaluation survey data.**

Table 7 presents the results for the impact of PROGRESA on participation and hours dedicated to school and work, including impacts on overall work and impacts by our three categories of work: market work, farmwork, and domestic work. Here, we split the sample by age groups identical to those above (e.g., 12–13, 14–15, and 16–17). Nevertheless, note that these impact estimates by age group begin to give rise to sample size problems. Given the necessary data cleaning exercise that took place (e.g., eliminating interviews where the reference period was Saturday or Sunday as well as those interviews carried out after early July) for these age groups, we have fewer than 1,000 cases overall, which can correspond to, for some of our work categories, to only

## Table 7—The impact of PROGRESA on time use: Work and school of boys and girls

| | Boys | | | | | | Girls | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Participation | | | Daily hours | | | Participation | | | Daily hours | | |
| | **Pre-program** | Impact | | **Pre-program** | Impact | | **Pre-program** | Impact | | **Pre-program** | Impact | |
| Age group | **Level** | Coefficient | T-statistic | **Level** | Coefficient | T-statistic | **Level** | Coefficient | T-statistic | **Level** | Coefficient | T-statistic |
| School | | | | | | | | | | | | |
| 8 to 17 | **0.68** | 0.022 | 1.9 | **6.07** | 0.073 | 1.5 | **0.64** | 0.040 | 3.4 | **6.03** | 0.121 | 2.5 |
| 12 to 17 | **0.57** | 0.042 | 2.5 | **6.30** | 0.038 | 0.5 | **0.51** | 0.065 | 3.5 | **6.30** | 0.111 | 1.5 |
| 12 to 13 | **0.76** | 0.041 | 1.9 | **6.16** | -0.157 | -1.6 | **0.71** | 0.066 | 3.0 | **6.11** | 0.138 | 1.4 |
| 14 to 15 | **0.58** | 0.034 | 1.2 | **6.36** | 0.084 | 0.6 | **0.52** | 0.079 | 2.7 | **6.55** | -0.004 | 0.0 |
| 16 to 17 | **0.31** | 0.034 | 1.2 | **6.40** | 0.489 | 2.3 | **0.23** | 0.040 | 1.5 | **6.38** | 0.186 | 0.4 |
| All work (market+domestic+farm) | | | | | | | | | | | | |
| 8 to 17 | **0.47** | -0.023 | -1.9 | **3.82** | -0.148 | -1.3 | **0.52** | -0.032 | -2.5 | **3.42** | -0.112 | -1.1 |
| 12 to 17 | **0.55** | -0.035 | -2.2 | **4.70** | -0.260 | -1.7 | **0.63** | -0.032 | -2.0 | **4.00** | -0.202 | -1.5 |
| 12 to 13 | **0.44** | -0.014 | -0.5 | **2.97** | -0.667 | -3.1 | **0.53** | -0.015 | -0.6 | **2.83** | -0.274 | -1.4 |
| 14 to 15 | **0.58** | -0.046 | -1.7 | **4.50** | 0.025 | 0.1 | **0.65** | -0.043 | -1.6 | **3.90** | -0.281 | -1.3 |
| 16 to 17 | **0.69** | -0.044 | -1.5 | **6.36** | -0.245 | -0.9 | **0.76** | -0.045 | -1.7 | **5.19** | -0.044 | -0.2 |
| Market | | | | | | | | | | | | |
| 8 to 17 | **0.09** | -0.006 | -1.8 | **7.47** | -0.169 | -1.0 | **0.02** | 0.000 | -0.1 | **7.47** | -0.436 | -1.2 |
| 12 to 17 | **0.15** | -0.021 | -2.3 | **7.60** | -0.168 | -1.0 | **0.05** | 0.000 | 0.0 | **7.58** | -0.912 | -2.4 |
| 12 to 13 | **0.05** | -0.020 | -3.1 | **6.49** | 2.039 | 0.8 | **0.01** | 0.003 | 1.2 | **6.25** | | |
| 14 to 15 | **0.13** | -0.012 | -0.7 | **7.74** | -0.274 | -0.8 | **0.04** | -0.015 | -1.8 | **8.55** | | |
| 16 to 17 | **0.30** | -0.024 | -0.9 | **7.76** | -0.118 | -0.6 | **0.12** | 0.013 | 0.7 | **7.78** | | |
| Domestic | | | | | | | | | | | | |
| 8 to 17 | **0.34** | -0.020 | -1.7 | **2.87** | -0.016 | -0.3 | **0.48** | -0.040 | -3.2 | **2.87** | -0.076 | -0.8 |
| 12 to 17 | **0.37** | -0.024 | -1.6 | **1.65** | -0.034 | -0.4 | **0.58** | -0.043 | -2.6 | **3.31** | -0.161 | -1.3 |
| 12 to 13 | **0.31** | 0.022 | 0.9 | **1.48** | -0.090 | -0.7 | **0.51** | -0.023 | -0.9 | **2.45** | -0.249 | -1.5 |
| 14 to 15 | **0.42** | -0.044 | -1.6 | **1.54** | 0.257 | 0.9 | **0.61** | -0.045 | -1.6 | **3.33** | -0.203 | -0.6 |
| 16 to 17 | **0.40** | -0.063 | -2.1 | **1.99** | -0.443 | -1.5 | **0.69** | -0.071 | -2.4 | **4.26** | 0.001 | 0.0 |
| Farm | | | | | | | | | | | | |
| 8 to 17 | **0.18** | -0.006 | -0.7 | **2.01** | -0.119 | -0.7 | **0.09** | 0.000 | -0.1 | **2.00** | 0.287 | 1.4 |
| 12 to 17 | **0.21** | -0.015 | -1.2 | **4.11** | -0.163 | -0.7 | **0.10** | -0.004 | -0.5 | **2.11** | 0.541 | 1.9 |
| 12 to 13 | **0.18** | -0.014 | -0.8 | **3.07** | -0.242 | -0.7 | **0.10** | -0.005 | -0.4 | **2.34** | 0.006 | 0.0 |
| 14 to 15 | **0.21** | -0.007 | -0.3 | **4.26** | -0.339 | -0.8 | **0.10** | 0.003 | 0.2 | **1.24** | 1.322 | 3.0 |
| 16 to 17 | **0.26** | -0.016 | -0.63 | **4.73** | -0.179 | -0.4 | **0.10** | -0.010 | -0.6 | **2.06** | 0.736 | 1.4 |

**Source: Authors' calculations based on the June 1999 (ENCEL) PROGRESA evaluation survey data.**

Notes: Impact on market hours for girls are omitted due to small number of cases.

100 cases of positive work hours. For this reason, we put more emphasis on our results for the overall groups of children ages 8–17 and those 12–17, rather than those further disaggregated by age group.

Beginning with the work and school activities of boys, Table 7 shows that for the group of boys ages 8–17, PROGRESA has a significant increase in participation in school. The size of the impact corresponds to, for the group of boys ages 12–17, approximately 4 percentage points, which is an increase of about 8 percent in participation in school. This impact appears to be largely concentrated on boys ages 12-13, which is broadly consistent with previous studies of the impact of PROGRESA on schooling.[32] With respect to hours spent in school, the only significant impact is an increase in time dedicated to school for boys ages 16–17 of almost one hour daily.

Turning now to work, we first consider overall participation in work of boys using the broad definition of work that includes market work, domestic work, and farm activities. The results show that overall participation in work is significantly reduced for the group of boys ages 8–17, and concentrating on the group of boys ages 12–17 shows larger absolute and proportional reductions of 4 percentage points from a preprogram level of 55 percent. It is interesting to note that these reductions in work are practically identical to the increase in schooling participation described above. This again provides some evidence on the possible substitution that may exist between work and school in

[32] See Schultz (2000) and Behrman, Sengupta, and Todd (2001a).

these communities for boys.[33] It is also important to note that overall hours dedicated to work are not affected. This suggests that the impacts of PROGRESA are primarily to increase school enrollment in terms of the number of children in school and to reduce the number of children who are working, but not necessarily, for instance, to reduce the hours worked of children who attend school and work.

Looking at the impact of PROGRESA on type of work for boys, the results show negative impacts on participation in market work for the group of boys ages 8–17, and larger reductions on the group ages 12–17. Consistent with the results on schooling participation, the largest reductions in participation in market work appear to be concentrated on boys ages 12–13, who show reductions in market work due to PROGRESA of approximately 40 percent from initial levels. Nevertheless, there are no impacts of PROGRESA on hours worked of boys in market work for any age group.

With respect to other types of work, the results show a reduction in participation in domestic work for boys, particularly for boys ages 14 and over. With respect to farmwork, whereas all the coefficients are negative, none are significant at conventional levels, implying there is no evidence that participation in farmwork for boys is reduced with PROGRESA.

We now turn to the estimates of PROGRESA on school and work of girls (Table 7). Beginning with schooling, the estimates on participation in school are much larger

[33] It is interesting to note that in the case of Bangladesh, Ravallion and Wodon (2000) find much lower proportional reductions in work compared with school. This may reflect the different nature of the benefits provided, or it may be related to the point that here we use a broad definition of work, whereas their definition uses only market work activities.

than boys, consistent with the results shown above and with previous studies.[34] In fact, for the group of girls ages 8–17, the average impact of PROGRESA on girl's participation in schooling is almost twice the impact as for boys. For girls ages 12–17, from an average level prior to the program of 51 percent, the impact of PROGRESA is to increase participation by 7 percentage points, a percentage increase of about 14 percent.

Turning to participation in work, our measure of overall work, which again includes participation in household work, farm activities, and market activities, shows significant reductions as a result of PROGRESA. Decomposing the analysis by type of work, the results show few impacts of PROGRESA on reducing market work for girls, with the exception of the group of girls ages 14–15, where participation in work is significantly reduced, although there is no impact on hours. The largest reductions in work for girls, nevertheless, correspond to the reductions in domestic work, particularly for girls ages 14 and over, which show reductions in participation in domestic work of about 10 percent. While all the estimated coefficients are negative, there are no significant effects of PROGRESA at reducing time spent in domestic work of girls. Again, the conclusion that appears to be emerging is that PROGRESA is successful at increasing school participation and reducing participation in work; nevertheless, there is little impact on reducing the hours of children who continue to work.

In summary, the results show the largest impacts of the program on the time use of children above the age of 12. These age groups correspond with enrollment in

[34] See Schultz (2000).

secondary (junior high) school and are consistent with the previous point that PROGRESA has the largest impacts on children at this level. Consistent with previous studies, we have found much larger impacts of PROGRESA on school participation for girls than boys, impacts that are nearly double the size of those on boys.

What is also of interest, however, is that these increases in schooling are associated with reductions in work: for boys, there are reductions in both market work and domestic work, whereas for girls, there are significant reductions in domestic work. For boys, the reductions in participation in work are approximately equivalent to the increases in participation in school, providing evidence that work and school can be viewed to some extent as competing activities. For girls, however, whereas there are significant reductions as well in participation in work, these impacts tend to be smaller than the increases in school participation. This suggests, consistent with the descriptive analysis above, that the work activities of girls may be more "compatible" with school; that is, they tend to be activities that can be done in the span of a few hours daily. Thus the impact of PROGRESA on the overall time use of girls appears to be to reduce slightly their leisure time.

## 4. CONCLUDING REMARKS AND POLICY CONSIDERATIONS

PROGRESA now extends over the large majority of all rural communities in Mexico and includes about 40 percent of all rural families as beneficiaries. In this study, we have analyzed the impact of the program on work and schooling of children. Overall,

we have found important impacts on children's participation in work activities and school attendance. Double-difference estimates of the impact of the program before and after the implementation of PROGRESA show significant increases in the school attendance of boys and girls that are accompanied by significant reductions in the participation of boys and girls in work activities. We also find that, in general, the displacement of the incidence of work is smaller than the gain in schooling, particularly for girls. Given that the participation of girls in work activities is already quite low, these results suggest that most of the increased school attendance of girls is most likely occurring by girls combining school with domestic work, which is left out of our measure of work.

A more inclusive measure of work is obtained from the time use module that collected information on the hours devoted during the previous day on a variety of activities. We find that children and, in particular, boys and girls of secondary school age are much more likely to attend school and to spend more time on school activities. In terms of work, boys of secondary school age also show strong reductions in participation in both market work and domestic work. Girls, on the other hand, show reductions in participation and/or hours spent in domestic work at all ages.

The reduction in domestic work for girls as a result of PROGRESA is also noteworthy, and we believe this is one of the first studies to show that subsidizing school enrollment can reduce the time spent in domestic work. Thus PROGRESA is associated with both increasing enrollment and reducing domestic work. This suggests that domestic work competes with time spent on school, although many girls nevertheless combine both domestic work and school. Market work, we have shown, is a much more important

deterrent to school attendance for boys than for girls, in accordance with the higher level of participation of boys relative to girls.

With respect to the general relationship between school and work and the extent to which work appears to be a deterrent to school, our findings confirm that children's work is an important deterrent to school for both boys and girls, but less for girls relative to boys. Using the broader definition of work that includes market, domestic and farm work, the reductions in work for boys are, to a large degree, comparable with the increases in schooling. In contrast, for girls, the reductions in work implied by the coefficients are significantly less than the increases in schooling. This is a likely consequence of the trends shown earlier, that while many girls participate in domestic work, many work a (low) number of hours, which permits them to continue combining both school and work. This is also confirmed by the fact the overall leisure time of girls shows small decreases with PROGRESA, consistent with the lower reductions in work than increases in school.

In sum, our findings suggest that a conditional cash transfer program like PROGRESA is successful at increasing school attendance as well as decreasing child labor simultaneously. While very encouraging, these findings seem to raise many more questions than they answer. For example, is it not possible that the cash transfers (conditioned or not) have a negative effect on the work incentives of adults? From a social welfare perspective, one may still question whether poor rural families really benefit in the long run by having their children working less and attending school more. From a broader policy perspective, one may also ask whether a conditional cash transfer

program like PROGRESA is a cost-effective way of increasing school attendance or decreasing child labor. Is it not possible that similar or even better effects on school attendance and child labor can be achieved through alternative means, such as building new schools or improving the quality of educational services?

Most of these questions, along with an analysis of the impact of the program on health, nutrition, and consumption, are addressed in detail by numerous studies conducted as part of a project evaluating the impact of PROGRESA.[35] Given the large number of studies involved and the diversity of topics, we will focus our discussion to the findings that are directly relevant to schooling and children's work. In a separate study, we have also examined the impact of the program on the time allocation of the adult members of beneficiary households.[36] We find that the program does not have any measurable impact on either the market participation rate of adults or on the hours they devote to work activities. This implies that the cash transfers accompanying the changes in the time allocation of children to school activities do not have any significant effects on the work incentives of adult household members.

Assuming the program effects could be sustained over the period in which a child is of school age, it is estimated that the program results in an overall increase in educational attainment of about 10 percent. Furthermore, if current urban wages approximate what PROGRESA's beneficiaries can expect to earn from their schooling in

[35] For a summary and synthesis of all the findings of the IFPRI-PROGRESA evaluation project, see Skoufias and McClafferty (2001) and Skoufias (2001). All of the reports generated as part of the evaluation project are accessible through the Internet at www.ifpri.org/themes/progresa.htm.

[36] See Parker and Skoufias (2000).

terms of future percentage increases in their wages, the internal rate of return, taking into account the costs of the grants, to PROGRESA's educational benefits is roughly 8 percent per year.[37] Children, when they reach adulthood, will have permanently higher earnings of 8 percent as a result of the increased years of schooling.

Also, a detailed cost analysis of the program provides strong evidence that the program is generally administered in a cost-effective manner. For example, for every 100 pesos allocated to the program, 8.9 pesos are "absorbed" by administration costs.[38] Given the complexity of the program, this level of program costs appears to be quite small. It is definitely relatively low compared to the numbers for roughly comparable programs.[39] Regarding alternative programs that may be able to achieve similar or better results, the evaluation research shows that if additional schools were to be built and staffed so that all children reside only 4 kilometers from their junior secondary school, the impacts on secondary school enrollments would be less than one-tenth the size of those of PROGRESA. Thus, in comparison to the impact of PROGRESA's targeted educational grants to poor families, the effect of increased access to schooling appears to be a relatively less effective means of increasing school enrollments.

To conclude, the analysis here, along with the majority of the results of the evaluation of the impact of the program in other areas, shows a large degree of support for the idea that schooling and work are incompatible and that work can be reduced

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[37] See Schultz (2000).

[38] See Coady (2000).

[39] These programs are the LICONSA and TORTIVALES programs mentioned in Coady (2000).

through subsidizing schooling. A well-targeted and administered conditional cash grant program like PROGRESA that lowers the price of schooling is successful at inducing families to withdraw their children from work and enrolling them in school instead. Taking into consideration the positive effects of the program on nutrition and health, the findings of the evaluation provide solid support to the notion that it is possible to combine short-run reduction in rural poverty with improvements in the human capital of younger and older rural family members.[40]

Undoubtedly, the opportunity to conduct a rigorous evaluation of the program like PROGRESA has created a higher set of standards for the design and conduct of social policy in Mexico and in Latin America in general. As policymakers now have a better sense of what types of programs can be effective toward alleviating poverty in the short run and in the long run, the list of questions and concerns about program options and program design cannot help but grow bigger. Is it possible for unconditional cash transfers to have the same impact on human capital investments of poor rural families? Is the simultaneous intervention in the areas of education, health, and nutrition preferable to intervening in each sector separately? Is there a minimum cash transfer that could achieve the same impact, and if so, how could one determine it? Hopefully, early involvement in the design and evaluation of programs implemented in other Latin American countries, such as Honduras, Nicaragua, Colombia, Jamaica, and Argentina, can shed some light on these critical questions.

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[40] For the impact on current poverty and a summary of all the results on the impact of PROGRESA on health and child nutrition, see Skoufias (2001).

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