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# Conditional Cash Transfers and Their Impact on Child Work and Schooling: Evidence from the PROGRESA Program in Mexico

Over the past few years, a number of Latin American countries have introduced antipoverty programs specifically focused toward increasing investment in human capital, as measured, in particular, by education, but also by health and nutrition. 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 its intergenerational transmission is the lack of investment in human development. A distinguishing characteristic of the programs is the provision of cash transfers on the condition that poor families take their children out of work and send them to school.

One of the first programs of this kind to be implemented was Mexico's Programa de Educación, Salud y Alimentación (the Education, Health, and Nutrition Program), known by its Spanish acronym, PROGRESA. Introduced in 1997, the program, which provides cash transfers, is aimed at increasing 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 attendance in school, as well as clinic attendance. These transfers correspond, on average, to a 22 percent increase in the income levels of the beneficiary families and are given directly to the mother of the family. The program also includes in-kind health benefits and nutritional supplements for children up to age five

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and for pregnant and lactating women. PROGRESA has grown rapidly. It now covers 2.6 million families in extreme poverty in rural areas, or 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 among boys and girls between eight and seventeen years of age.[1] We address several questions. 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? 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 program's impact. The data cover 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, incorporating a progressively broader definition of work. In the first part we examine 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 are able to estimate the program's impact using the double difference estimator that is commonly acknowledged as a preferred estimator for program evaluation. In the second part we take advantage of a module on time use, carried out about a year after program implementation. This module allows us to consider 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 based on data from other countries find that an unconditional income change has a surprisingly small marginal effect on both school enrollment and child labor.[2] This suggests that unconditional cash

1. This study is only one of a number of studies conducted as part of the PROGRESA evaluation project carried out 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 (2001), who focus on the program's impact on schooling and continued attendance in higher school grades. Both studies use only a binary indicator of whether a child is in school, and they do not consider work at all. Behrman, Sengupta, and Todd (2000) focus on the program's impact on child achievement test scores, while Coady and Parker (2001) evaluate the cost effectiveness of the impact on schooling impact. A related study by Demombynes (2001) considers work in addition to schooling; it is discussed further below.

2. See Behrman and Knowles (1999); Nielsen (1998).

transfer programs that increase household income may have only a limited effect on increasing child school enrollment and decreasing child labor simultaneously. 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 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 are substitutes for schooling. Moreover, increased school attendance may reduce children's leisure time rather than their work time. Ravallion and Wodon, for example, examine the impact of the Food for Education program in Bangladesh, which provides rice to eligible families in exchange for sending their children to school.[3] 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 school attendance of boys 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 is organized as follows. We begin 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 subsequent section describes the evaluation methodology and the data. We then present the 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.

## 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 applies targeting at the

3. Ravallion and Wodon (2000).

household level to ensure that the resources of the program are directed and delivered to households in extreme poverty, that is, the households that can most benefit from the program. General food subsidies are widely believed to have had a high cost on the government budget 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. Households within the selected communities are then chosen using socioeconomic data collected for all households in the community.[4]

Second, unlike earlier social programs in Mexico, PROGRESA contains 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 considering each 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 school attendance and performance are often adversely affected by poor health and nutrition. Poor health is therefore both a cause and a consequence of poverty. PROGRESA also differs in the mechanism for delivering its resources. Benefits are given exclusively to mothers in recognition of their potential to effectively and efficiently use resources in a manner that reflects the immediate needs of the family.

Under the first benefit component, education, PROGRESA provides monetary educational grants for each child less than eighteen years of age who is enrolled in school between the third grade of primary school and the third grade of secondary school (see table 1). The grant amounts are adjusted every six months for inflation, and they increase as the children progress to higher grades, to reflect the income they would contribute to their families if they were working. At the junior high level, the grants are slightly higher for girls than for boys.[5] For example, during the second

4. According to program description documents, the selection process includes a third step 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 this third step was not rigorously applied, and its importance was thus minute. For more details on the selection of beneficiary households in the program, see Skoufias, Davis, and de la Vega (2001); Skoufias, Davis, and Behrman (1999).

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

**TABLE 1. PROGRESA Monthly Cash Transfer Schedule**
Nominal pesos

| Type of grant | Amount of grant | | | |
|---|---|---|---|---|
| | *January to June 1998* | *July to December 1998* | *January to June 1999* | *July to December 1999* |
| Educational grant per child[a] | | | | |
| Primary (all children) | | | | |
| Third grade | 65 | 70 | 75 | 80 |
| Fourth grade | 75 | 80 | 90 | 95 |
| Fifth grade | 95 | 100 | 115 | 125 |
| Sixth grade | 130 | 135 | 150 | 165 |
| Secondary (boys) | | | | |
| First grade | 190 | 200 | 220 | 240 |
| Second grade | 200 | 210 | 235 | 250 |
| Third grade | 210 | 220 | 245 | 265 |
| Secondary (girls) | | | | |
| First grade | 200 | 210 | 235 | 250 |
| Second grade | 220 | 235 | 260 | 280 |
| Third grade | 240 | 255 | 285 | 305 |
| Grant for school materials per child[a] | | | | |
| Primary (September) | — | In–kind | — | 110 |
| Primary (January) | 40 | — | 45 | — |
| Secondary (September) | — | 170 | — | 205 |
| Grant for food consumption per household[b] | | | | |
| Cash transfer | 95 | 100 | 115 | 125 |
| Maximum grant per household | 585 | 625 | 695 | 750 |

Source: Hernández, Gómez de León, and Vásquez (1999).
— Grant not given in this period.
a. Conditioned on child school enrollment and regular attendance.
b. Conditioned on attending scheduled visits to health centers.

half of 1999, the monthly educational grants ranged from 80 pesos (about U.S.$8) in the third grade of primary school to 280 pesos (U.S.$28) for boys and 305 pesos (U.S.$30) for girls in the third year of secondary school.

The second component, health, provides basic health care for all members of the family. 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 monthly, or about U.S.$13) for improved food consumption and nutritional supplements. The supplements are principally targeted to children between the ages of four months and two years and to pregnant

and breastfeeding women. They are also given to children between the ages of two and five if any signs of malnutrition are detected.

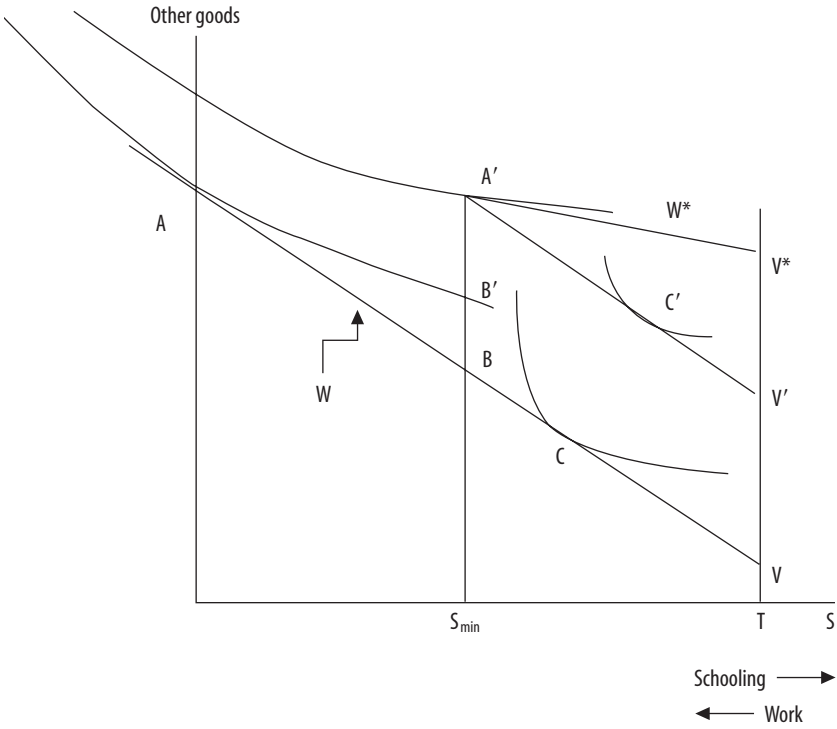
The objective of designing benefits to provide incentives for increasing human capital is revealed through the fact that the receipt of benefits is contingent on fulfillment of certain obligations by the beneficiary families. The monetary educational grants, for example, are linked to the school attendance of children. 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 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 redirect children's time allocation differently depending on the households' preferences and their initial location in the feasible set. This is particularly useful when it comes to evaluating the program's impact empirically.

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, whereas the horizontal axis measures the time a child devotes to schooling (or human capital investment). Full or 100 percent attendance occurs when the child devotes all nonleisure time to school attendance, including school-related homework (that is, $S = T$, where $T$ denotes the amount of time available after excluding leisure time, which for simplicity is assumed to be fixed). The vertical line of height $V$ at $S = T$ denotes the maximum amount of other goods available in the household when a child devotes all her time to schooling and not to working. When a child divides her time between work and school, 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, which describes the trade-off in the market between the consumption of other goods and schooling (or work).[6] By devoting one hour less to schooling

6. The opportunity cost of child schooling is assumed to be 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 homemade

**FIGURE 1. The Effect of Conditional Cost Transfers on Children's School Attendance and Work**




A: Initially not attending.
C: Initially attending full time.
T: Maximum amount of time available excluding leisure.
$S_{min}$: Program's required school attendance.

and working one extra hour for wages, 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 PROGRESA benefits causes the budget line in the region between points $T$ and $S_{min}$ to shift up without changing its slope and increases the nonlabor component of income upward to the point $V'$. To the extent that the household fulfills all the requirements

commodities that are perfectly substitutable with market-purchased commodities, with no additional complications (see Skoufias, 1994).

of the program, then $V' - V$ equals the maximum amount of benefits that the household can obtain from the program. The feasible budget constraint of an eligible family is now described by the line $TV'A'BA$, which is discontinuous at the point $S_{min}$.

Of course, differences in families' nonearned income and market opportunities may be one important reason why 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 in which we have two different types of households represented by distinct 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 that does not attend school at all ($S = 0$) and devotes all of her free time to market work. For simplicity, point $A$ is depicted as a tangency point between the indifference curve of the household and the real wage line, $W$, although this does not have to be the case.

The discontinuity of the household's budget constraint, 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 $S_{min}$. 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 their child out of school. A conditional cash transfer less than $B' - B$ is insufficient to induce children to attend school.

Figure 1 implicitly assumes 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. Household $A$ therefore 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 program's impact may be concentrated on increasing the time they devote to schooling, such as spending more time studying, rather than on raising enrollment.[7]

For a contrast consider household $A$. At first sight, it appears that income and substitution effects cannot be attributed to the program since the household's final equilibrium point $A'$ is not a tangency point. One can still apply the familiar concepts of income and substitution effects, however, using the analytical framework of linearizing the budget constraint.[8] Linearizing the budget constraint amounts to transforming point $A'$ into a tangency point by drawing a line tangent to the indifference curve at $A'$ (that is, finding the shadow wage, $W^*$) and finding the corresponding level of nonearned income (or shadow income), $V^*$, that corresponds to the shadow wage, $W^*$. 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 the condition that the child devote at least 85 percent of its time in 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 that results from the program, while the total increase in household income stemming from 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.

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. In households for which the program constraints are binding, the program results in income and substitution effects that can reinforce its impact. The program is likely to have only income effects, however, in households for which the constraints of the program are nonbinding. Given the heterogeneity of households' preferences and constraints, the extent to which the program has a significant impact on the

7. The program may also have important dynamic effects by increasing the probability that children continue on to higher grades in school. These dynamic effects are explored by Behrman, Sengupta, and Todd (2001).

8. For more details, see Killingsworth (1983).

human capital and work of children can only be determined through empirical analysis.

## Data, Empirical Specification, and Results

We now turn to a description of the information sources and methods we use to empirically evaluate PROGRESA's impact on children's human capital investment and work. 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. The proper evaluation of a program's impact thus requires observing a group of households that are similar to beneficiary households in every respect possible except that they 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 the logistical complexities associated with the operation of PROGRESA in very small, remote rural communities did not permit the program to cover all of the eligible localities at once. Instead, the program covered localities in phases. The sequential expansion of the program makes it possible to select a comparable or control group from the set of localities that are eligible for the program but are not yet covered by the program.

Specifically, the sample used to evaluate PROGRESA consists of repeated observations (panel data) collected for 24,000 households from 506 localities in the seven states of Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz. Of the 506 localities, 320 were assigned to the treatment group ($T = 1$) and 186 were assigned to the control group ($T = 0$). The 320 treatment localities were randomly selected using probabilities proportional to size from a universe of 4,546 localities that were covered by the second phase 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—also in the seven states—that were to be covered by PROGRESA in later phases. 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 PROGRESA Evaluation Survey (Encuesta de Evaluación de los Hogares, or ENCEL). The Survey of Household Socioeconomic Characteristics is an economic census that is 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; it consists of a baseline survey of the 24,077 households of the evaluation sample and follow-up surveys every six months.[9] We also consider a special time-use module carried out once only as part of the June 1999 ENCEL. This module allows us to look at hours spent in school and work, as well as to 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 (the control group) is similar to the group that does receive treatment (the treatment group) in terms of both observable and unobservable characteristics. 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 postprogram differences between the treatment and control groups to the program.[10] Behrman and Todd examine in great detail the extent to which the selection of PROGRESA's localities may be considered random.[11] They compare 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 control group and the treatment group). Second, they compare the means of the same variables for the two groups using the household- and individual-level data. When these comparisons and tests are performed at the locality level, the hypothesis that the means are equal between treatment and control localities is not rejected. However, when the same comparisons are performed using household-level data, it is found that the null hypothesis is rejected more frequently than would be expected by chance given standard significance levels. They interpret this

9. 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, child's school attendance, health care use, and community characteristics.

10. See Heckman, LaLonde, and Smith (1999).

11. Behrman and Todd (1999).

latter finding as stemming from the fact that the samples are large, which means that even minor differences could lead to rejection.

In light of these earlier findings, it is necessary to consider whether there are preprogram differences among localities in the context of our topic. Table 2 presents the means of the variables used in our analysis 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. The means of the treatment and control samples exhibit clear differences at the individual level. These differences are validated by the estimates presented in table 3, which relates the probability of being included in the treatment sample to observed individual and household characteristics. In a pure randomized design, observed individual or household characteristics should have no significant role for predicting whether an individual or household is assigned to the treatment sample. As the probit estimates in table 3 reveal, however, the treatment and control samples demonstrate significant preprogram differences. For example, boys who attend school or who are working are more likely to be in the treatment sample than in the control sample. Also, boys (girls) whose father speaks Spanish are less (more) likely to be in the treatment (control) sample than children whose father speaks an indigenous language only.

These results are in general agreement with the findings of Behrman and Todd at the household level. Given that Behrman and Todd 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 over 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 selling a product; helping in family business; making products to sell; washing, cooking or ironing; and working in agriculture activities or caring for animals. Individuals who respond that they engage in any of these activities are considered as working. Domestic activities are not included in this definition of work.

**TABLE 2. Sample Means of Key Variables**

| | *Treatment group* | | *Control group* | |
|---|---|---|---|---|
| *Variable* | *Boys* | *Girls* | *Boys* | *Girls* |
| 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 is 8 years (1 = yes, 0 = no) | 0.121 | 0.115 | 0.115 | 0.114 |
| Age is 9 years (1 = yes, 0 = no) | 0.106 | 0.111 | 0.100 | 0.104 |
| Age is 10 years (1 = yes, 0 = no) | 0.117 | 0.115 | 0.119 | 0.116 |
| Age is 11 years (1 = yes, 0 = no) | 0.105 | 0.116 | 0.109 | 0.113 |
| Age is 12 years (1 = yes, 0 = no) | 0.108 | 0.108 | 0.116 | 0.101 |
| Age is 13 years (1 = yes, 0 = no) | 0.098 | 0.102 | 0.100 | 0.106 |
| Age is 14 years (1 = yes, 0 = no) | 0.102 | 0.093 | 0.098 | 0.098 |
| Age is 15 years (1 = yes, 0 = no) | 0.097 | 0.091 | 0.097 | 0.092 |
| Age is 16 years (1 = yes, 0 = no) | 0.078 | 0.076 | 0.077 | 0.078 |
| Age is 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 from 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 |
| No. observations | 8,986 | 8,200 | 5,377 | 5,282 |

Source: Authors' calculations, based on data from the November 1997 ENCASEH household census.

**TABLE 3. Individual and Household Characteristics and the Probability of Being in the Treatment Sample**[a]

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 is 9 years (1 = yes, 0 = no) | 0.000 | 0.01 | 0.013 | 0.74 |
| Age is 10 years (1 = yes, 0 = no) | −0.020 | −1.23 | −0.003 | −0.21 |
| Age is 11 years (1 = yes, 0 = no) | −0.023 | −1.30 | 0.005 | 0.33 |
| Age is 12 years (1 = yes, 0 = no) | −0.037 | −2.16 | 0.013 | 0.69 |
| Age is 13 years (1 = yes, 0 = no) | −0.030 | −1.79 | −0.005 | −0.25 |
| Age is 14 years (1 = yes, 0 = no) | −0.015 | −0.83 | −0.010 | −0.47 |
| Age is 15 years (1 = yes, 0 = no) | −0.024 | −1.16 | 0.000 | 0.00 |
| Age is 16 years (1 = yes, 0 = no) | −0.022 | −0.94 | 0.000 | −0.01 |
| Age is 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 from 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 |

**TABLE 3. Continued**
Probit estimates

| | *Boys* | *Girls* |
|---|---|---|
| *Variable* | *Coefficient* | *Coefficient* |
| *Summary statistic* | | |
| Wald chi-squared (40) | 51.080 | 38.520 |
| Probability > chi-squared | 0.113 | 0.537 |
| Pseudo $R^2$ | 0.012 | 0.012 |
| No. observations | 14,363 | 13,482 |

Source: Authors' calculations, based on data from the November 1997 ENCASEH household census.
a. All coefficients are expressed as marginal effects *dF*/*dX*.

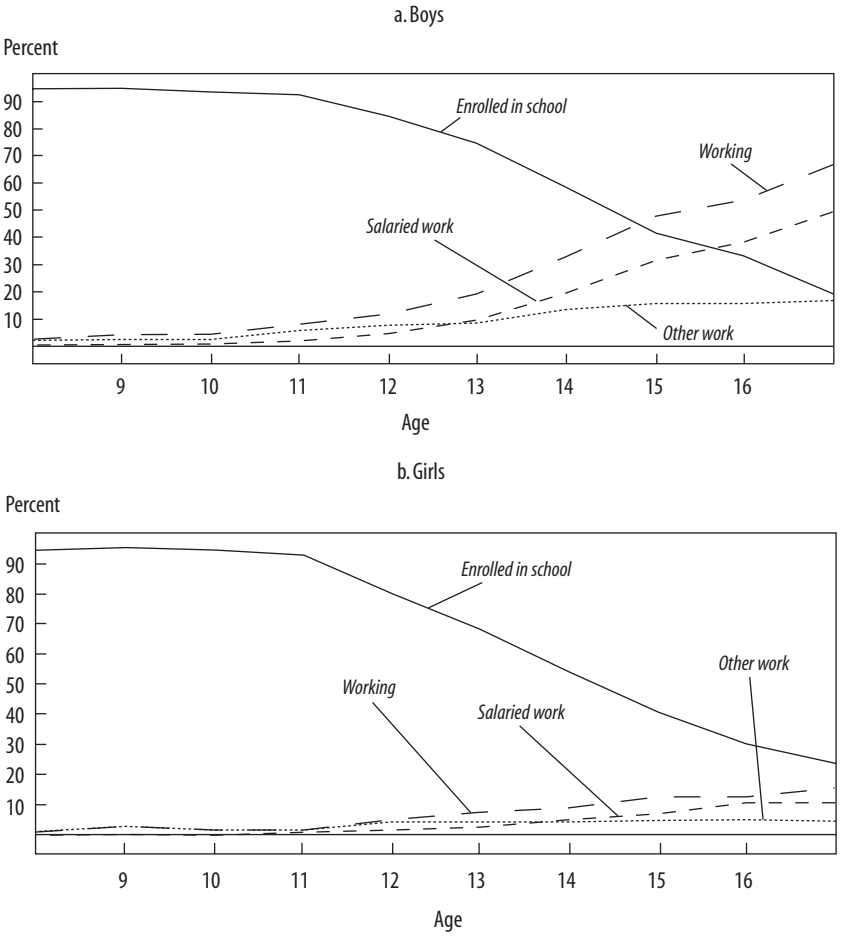
The time-use survey allows us to broaden our definition of work to include domestic work and other unpaid activities.[12] The survey provides information on eighteen activities carried out during the previous day for all individuals aged eight or more.[13] In our analysis, we first construct overall measures of leisure time, defined as the difference between twenty-four hours and the time spent on all reported activities (that is, leisure is a residual). We also examine the composition of time spent. While the program may have no effect on overall leisure time, it may cause substitution between different types of activities. In particular, time spent on schooling is likely to increase and time spent on work activities is likely to decrease. We consider three different types of activities, namely, market work, farm work, and domestic work, analyzing participation and hours spent in each activity, as well as participation and time spent in school.

Figure 2 shows the school enrollment rates and the labor force participation of boys and girls by age, using the sample of all children from households eligible to receive PROGRESA benefits between the ages of eight and seventeen prior to the program's implementation. For boys, the school enrollment rate is close to 95 percent and the labor force participa-

12. We do not estimate PROGRESA's impact on work hours in this section, but rather postpone this discussion for the section on time use. The structure and design of the ENCEL questionnaires have changed over time, such that it is difficult to compare hours worked before and after program implementation. The postprogram data raise the awkward problem that no information on hours worked is available for many individuals who declare they are working in the participation questions (which are identical over time). Furthermore, the time-use module allows us to include a broader definition of work, which includes domestic work and other unpaid activities.

13. 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 in PROGRESA Communities prior to Program Implementation**




Source: Authors' calculations, based on data from the November 1997 ENCASEH census.

tion is quite low (less than 5 percent) up until the age of about ten or eleven, 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, primarily self-employment and helping in family businesses. In other words, when children begin to work, they are likely to participate in

nonsalaried work rather than salaried work. Only at the age of fourteen does the percentage of children in salaried work begin to exceed that of other types of work. By the age of sixteen, the majority of boys report working, and the majority of these workers are in salaried work.

For girls, labor force participation is also extremely low at early ages. In contrast to boys, however, labor force participation increases very slowly with age. At the age of seventeen, for example, girls have a low participation rate in the labor market, at close to 17 percent. When girls do work at very young ages, they tend to be involved in nonsalaried activities. Over the age of twelve, the chance that girls participate in salaried activities is approximately equal to the chance that they work in nonsalaried activities.

The time-allocation module gives a better sense of the patterns of time use in the communities where PROGRESA operates. Since this module was applied after the start of the program, we limit our descriptive analysis to households and individuals in the control communities. Table 4 presents the overall participation and daily hours spent in each of the eighteen activities covered by the time-use survey. Since the reference period is the previous day, 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 in the family goes to the market at some point over the two-week period (that is, 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.

The table 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 4 shows some general differences by gender in terms of the type of work children perform. Boys are more likely to engage in salaried work than girls, although the overall participation rates of both groups are low. Girls have a high participation in domestic activities such as cleaning, cooking, sewing, and preparing food, while boys have very minimum levels of participation in these areas. The only domestic activity for which boys have a similar participation level as girls is the category of fetching water and firewood or throwing out trash. Boys, however, have slightly higher participation levels in working the family land and taking care of animals.

**TABLE 4. Time Use of Children Aged Eight to Seventeen in Poor Communities prior to Program Implementation (Control Group)**

| | *Boys* | | *Girls* | |
|---|---|---|---|---|
| *Type of activity* | *Participation (percent)* | *Daily hours*[a] | *Participation (percent)* | *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 or 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 data from the June 1999 ENCEL evaluation survey.
a. Conditional on participating.

## *Measuring Eligibility*

Our regressions focus only on the group of individuals (families) who are eligible for the program. The process of selecting eligible households in the communities where PROGRESA operates involved two steps in the early phases of the program. First, a set of households were selected and incorporated into the program according to a discriminant analysis procedure.[14] In the evaluation sample, the percentage of households selected corresponds to approximately 52 percent of all households in the communities. A second selection was undertaken to correct perceived errors in leaving out households, particularly elderly households. This round identified an additional 25 percent of households in treatment communities as eligible to receive benefits, although some of these families experienced

14. For more details see Skoufias, Davis, and Behrman (1999); Skoufias, Davis, and de la Vega (2001)

substantial delays in their incorporation. As of November 1999 (the date of the last survey used in this report), only 819 of the 3,023 newly eligible households had been incorporated, while the remaining 2,204 had not yet begun to receive benefits from PROGRESA.

Our evaluation of the program's impact is based on the sample of all eligible households, irrespective of whether they did, in fact, receive any benefits. The estimated effect of PROGRESA thus reflects the program's operational efficiency or inefficiency. Our estimates of the program's impact thus measure the mean direct effect of the offer to treat. These estimates are less likely to be affected by biases such as selection biases associated with the choice to receive benefits or possible attrition from the program. They provide a lower bound for the impact of the treatment on the treated, or the households that actually received the treatment.[15]

### *Impact on the Labor Force Participation and School Enrollment of Children*

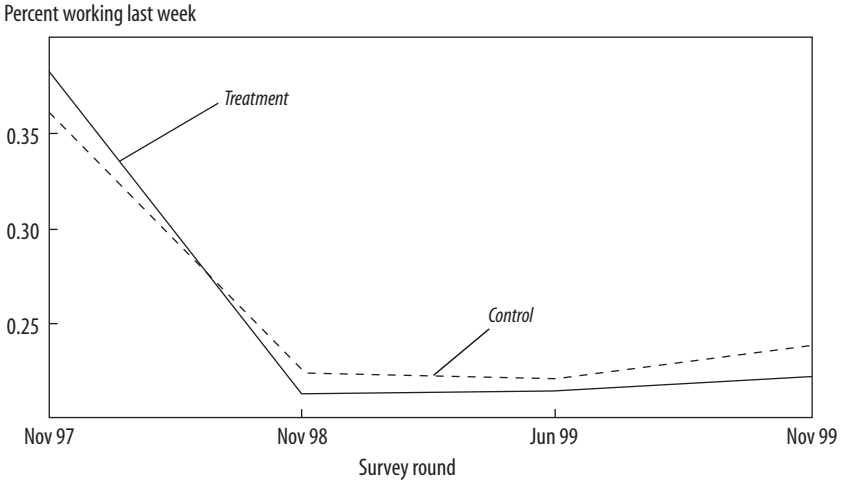
Figures 3 and 4 present a straightforward comparison of the (unconditional) mean labor force participation rate and school participation before and after the start of the program, in treatment and control villages for all boys and girls between twelve and seventeen years of age in households selected as eligible for PROGRESA benefits. As evident in figure 3, treatment and control villages exhibit differences in the labor force participation rates of boys and girls prior to the introduction of PROGRESA. For example, in November 1997 the labor force participation rate was slightly lower in control villages than in treatment villages. By November 1998 (the first survey after the introduction of PROGRESA), the mean labor force participation rates of boys and girls in both treatment and control villages decrease and remain at that lower level for the remaining rounds.

In contrast, figure 4 reveals that the mean school attendance rate of both boys and girls were practically identical among treatment and control villages. By November 1998, the mean attendance rate of both boys and girls in treatment villages is noticeably higher than the mean attendance rate in the villages not yet covered by PROGRESA. 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.

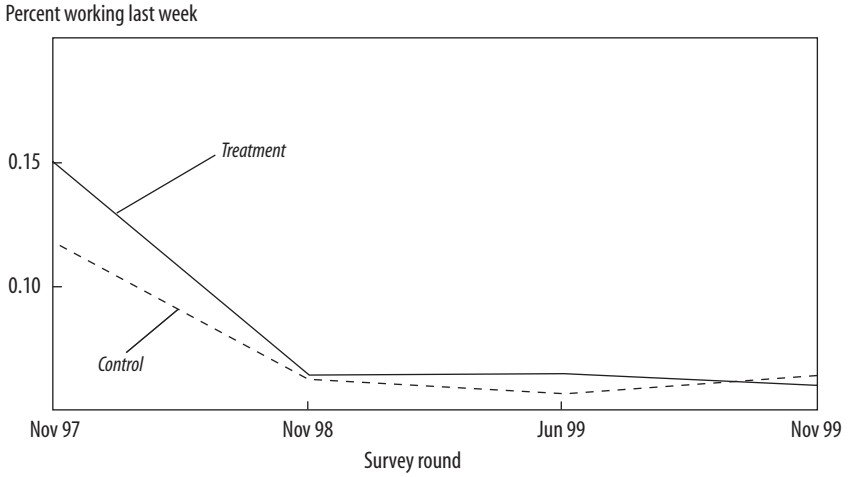
15. Parker and Skoufias (2000) provide estimates of the effect of the treatment on the treated and find that the program has only a slightly higher impact on those who actually receive treatment. This suggests that the bias due to attrition or selection is not serious.

**FIGURE 3. Mean Labor Force Participation Rate**

a. Boys aged twelve to seventeen

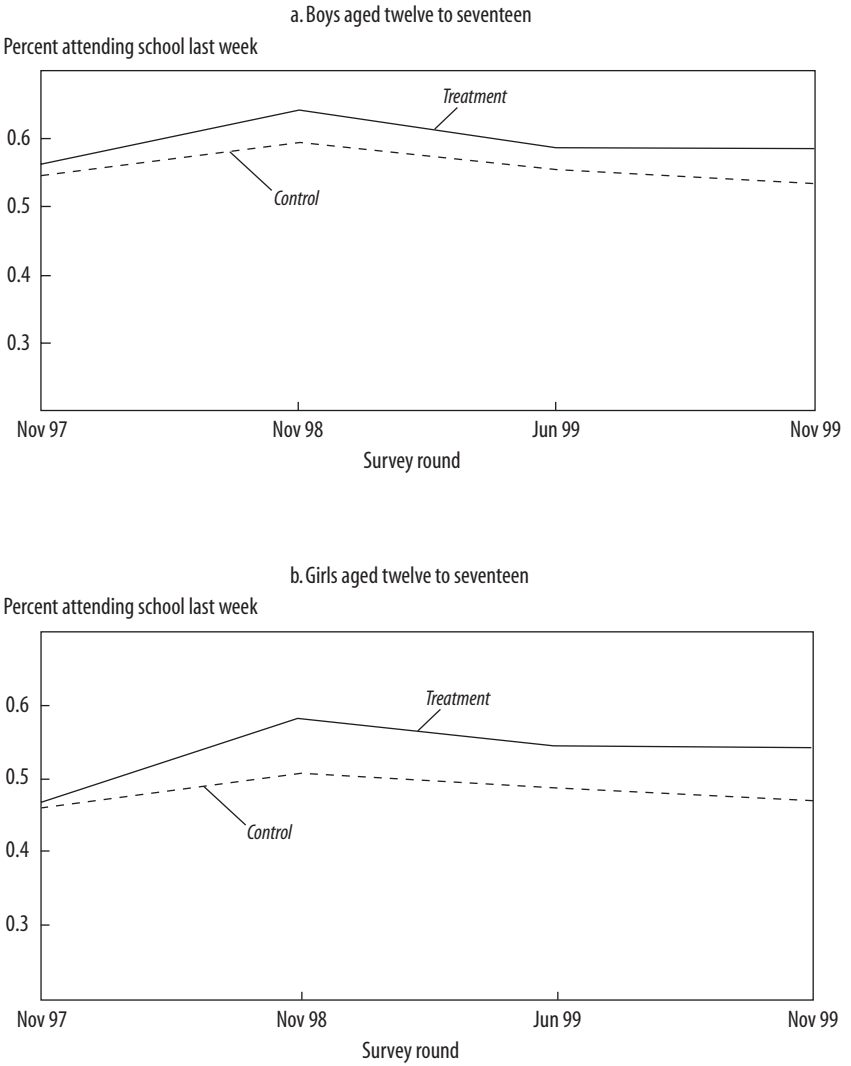



b. Girls aged twelve to seventeen




Source: Authors' calculations, based on data from the November 1997 ENCASEH census and the November 1998, June 1999, and November 1999 ENCEL evaluation surveys.

**FIGURE 4. Mean School Attendance Rate**




Source: Authors' calculations, based on data from the November 1997 ENCASEH census and the November 1998, June 1999, and November 1999 ENCEL evaluation surveys.

The preceding analysis, in combination with our earlier findings in table 4, implies that a credible estimate of the program's impact must 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

$$
\begin{aligned}
Y(i,t) = {} & \sum_{r=1}^{4} \alpha_{0i} + \beta_T T(i) + \beta_{TR2}[T(i) * R2] + \beta_{TR3}[T(i) * R3] \\
& + \beta_{TR4}[T(i) * R4] + \sum_{j=1}^{J} \theta_j X_j(i,t) + \eta(i,t),
\end{aligned} \tag{1}
$$

where $Y(i, t)$ is the work (school) outcome indicator for individual $i$ in period $t$; $\alpha$, $\beta$, and $\theta$ are fixed parameters to be estimated; $T(i)$ is a binary variable taking the value of 1 if the household belongs in a treatment community and 0 otherwise (that is, for control communities); $R2$, $R3$, and $R4$ are binary variables equal to 1 for the second, third, and fourth rounds of the survey, respectively, after the initiation of the program and equal to 0 otherwise; $\mathbf{X}$ is a vector of household and village characteristics; and $\eta$ is an error term summarizing the influence of random disturbances.

The vector $\mathbf{X}$ of control variables consists of parental characteristics, including the education level of the child's mother and father, the age of the mother and father, whether the parents speak an indigenous language, and whether they also speak Spanish.[16] We also include a number of variables measuring the demographic composition of the household. These variables include the number of children aged zero to two and aged three to five, boys and girls aged six to seven, eight to twelve, and thirteen to eighteen, men and women aged nineteen to fifty-four, and men and women aged fifty-five and older. 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 the distance from the locality to the *cabecera municipal*, or the governing center of the municipality (and likely the largest locality of the municipality). This is used as an indicator of the availability of local labor markets. It may, however, have different effects on both school and

16. Missing variable dummies are also included in the regressions for cases in which data are not available (for instance, because the father no longer lives in the household).

work. Closer available labor markets may make paid work more attractive and reduce schooling, or it may make school more attractive by providing more information about the expected returns to schooling.[17] Finally, we also include a variable measuring the distance to the closest secondary school from the locality. This provides an indicator of the cost of attending school; it 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 over each round of the analysis for reasons unrelated to PROGRESA. 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 as a test of the randomness in selection of localities. A truly random selection of control and treatment localities would generate a conditional mean of the outcome indicator that was identical across treatment and control households and individuals.

The coefficients $\beta_{TR}$, which are associated with the interaction of the treatment dummy $T(i)$ with the dummy variables indicating the round of the survey, yield the 2DIF estimate of the program's impact in each round. This allows us to examine whether the impact is constant, decreasing, or increasing over time, as well as whether there are seasonal effects. The coefficients $\beta_{TR}$ also provide an estimate of the impact of the various income and substitution effects induced by households' participation in the program.[18]

For a better understanding of how the 2DIF estimator measures program impact, consider equation 1 for the simple case in which 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 $R2 = 0$. The conditional mean values of the outcome indicator for treatment and control groups before and after the start of the program are then[19]

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

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

19. Expressions 2a through 2d rely on the assumption that $E(\eta \mid T,R2,\mathbf{X}) = 0$, for $T = 1, 0$, $R2 = 0, 1$, and all $\mathbf{X}$.

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

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

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

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

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

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

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's impact.[20] The regression models therefore account for the clustered nature of the sample and report robust standard error estimates for the program's impact.[21]

The estimates are obtained by estimating equation 1 using a probit model. In the analysis of school enrollment, $Y(i, t)$ equals 1 if child $i$ attended school in the week prior to the interview in round $t$ and equals 0 otherwise. In the analysis of child work, $Y(i, t)$ equals 1 if child $i$ worked in the week prior to the interview in round $t$ and equals 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.[22] 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.

20. For a clear discussion of these issues, see Murray (1998).

21. Robust standard error estimates were obtained using the robust option in STATA v6.0.

22. We also estimated a bivariate probit model that allows for correlated disturbances; the main results do not change.

Table 5 presents the results of PROGRESA's impact on the work participation rate of children. We use the sample of all eligible households in treatment and control villages, taking the November 1997 ENCASEH survey as our baseline and considering three postprogram rounds of the Evaluation Survey (ENCEL), namely, November 1998, June 1999, and November 1999.[23] In recognition of the heterogeneity in the potential impact of the program, and given that children of secondary 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 the ages of eight and eleven (primary school ages) and children between the ages of twelve and seventeen (secondary school ages). We also examine separately the age groups of children aged twelve to thirteen, fourteen to fifteen, and sixteen to seventeen in order to highlight any difference in the program's impact across these age groups. Previous research shows that the highest impact on attendance is at the secondary level of schooling.[24]

The results are presented showing the initial level of participation in work activities (that is, prior to the program's implementation) and the impact estimates for each round of the ENCEL carried out after 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.[25]

The results in table 5 show that PROGRESA has had a clear negative impact on children's work. Beginning with the group of boys aged eight to eleven, the program had a consistently negative impact on work in the first round of the ENCEL. In November of 1998, for instance, the probability of working for boys aged eight to eleven fell by 1.3 percentage points, from an overall preprogram participation rate of 6.2 percent. PROGRESA thus accounts for a reduction of approximately 21 percent (0.013/0.062) in

23. We use the November 1997 ENCASEH census rather than the March 1998 ENCEL survey as our baseline of labor market participation because 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.

24. See Schultz (2000); Coady and Parker (2001).

25. The estimates reported were obtained using the dprobit command in STATA v7.0. They can easily be converted into percentage changes or elasticities by dividing the marginal effect by the preprogram level, both of which are reported in tables 5 and 6.

**TABLE 5. Impact of PROGRESA on the Probability of Children's Working**[a]
Difference-in-difference estimates

| | | *Impact* | | | | | |
|---|---|---|---|---|---|---|---|
| | | *November 1998* | | *June 1999* | | *November 1999* | |
| *Age group* | *Preprogram level* | *Coefficient* | t *statistic* | *Coefficient* | t *statistic* | *Coefficient* | t *statistic* |
| Boys | | | | | | | |
| 8 to 11 | 0.0620 | −0.013 | −2.0 | −0.009 | −1.4 | −0.011 | −1.3 |
| 12 to 17 | 0.3775 | −0.032 | −1.6 | −0.033 | −1.6 | −0.047 | −2.1 |
| 12 to 13 | 0.1715 | −0.016 | −1.0 | −0.025 | −1.6 | −0.038 | −2.2 |
| 14 to 15 | 0.4058 | −0.045 | −1.7 | −0.041 | −1.5 | −0.042 | −1.4 |
| 16 to 17 | 0.6299 | −0.028 | −0.8 | −0.016 | −0.4 | −0.052 | −1.3 |
| Girls | | | | | | | |
| 8 to 11 | 0.0353 | −0.005 | 0.8 | −0.003 | −0.6 | −0.000 | −0.5 |
| 12 to 17 | 0.1317 | −0.018 | −1.7 | −0.011 | −1.0 | −0.023 | −1.8 |
| 12 to 13 | 0.0870 | −0.015 | −1.6 | −0.011 | −1.1 | −0.007 | −0.7 |
| 14 to 15 | 0.1495 | −0.032 | −2.3 | −0.023 | −1.5 | −0.038 | −2.4 |
| 16 to 17 | 0.1727 | 0.007 | 0.3 | 0.017 | 0.7 | −0.020 | −0.8 |

Source: Authors' calculations, based on data from the November 1997 ENCASEH census and the November 1998, June 1999, and November 1999 ENCEL evaluation surveys.

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

the probability of this group's working. The program seems to have had a lower negative impact on the labor force participation rate of boys aged twelve to seventeen: 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 their working.

For boys aged twelve to thirteen in the November 1999 round, PROGRESA reduced the probability of their working by 22 percent relative to their probability prior to the program (0.038/0.1715). For boys aged fourteen to fifteen, the estimates show that the probability of working fell by 11 percent in the first postprogram round, with insignificant changes in later rounds. The group of boys aged sixteen to seventeen saw no significant reduction in the probability of working.

The program does not appear to have any impact for girls of primary school age, whose working rate is about half that of boys. Girls between twelve and seventeen years of age, however, experienced significant reductions associated with PROGRESA, despite their overall lower labor force participation level prior to the program. The average participation rate of girls aged twelve to seventeen prior to the program was 13.17 per-

centage points; PROGRESA reduced this participation by 1.8 percentage points in November 1998 and 2.3 percentage points in November 1999. These effects correspond approximately to a 14 percent and 17 percent reduction in the probability of working.

As with boys, the larger effects on girls aged twelve to seventeen are principally concentrated on girls aged fourteen to fifteen. For girls aged twelve to thirteen, the effects are significant only in the first postprogram round, when participation fell by about 17 percent from the preprogram level. For girls aged fourteen to fifteen, the effects are consistently large and significant over time, with reductions in the probability of work ranging from about 20 to 25 percent, depending on the round. As with boys, the program's effect on work is not significant for girls aged sixteen to seventeen. PROGRESA does not appear to be very successful at reducing the work of boys and girls in this age group.

Table 6, which is based on the identical sample of children, reveals that the negative impact on participating in work activities is accompanied by a positive and significant impact on the probability of attending school. PROGRESA increased the attendance rate of boys of primary school age by 1.3 percentage points in November 1998 and by 1.8 percentage points in November 1999. This amounts to a 1.4 and 1.9 percent increase in the fraction of boys in this age group attending school. No significant impact is found on the attendance rate of girls in the same age group. As already mentioned, these low impact estimates are not surprising considering that the attendance rates of both boys and girls in this age group are already high, at close to 94 percent. The conditions of the program are not binding for households with children in this age group, such that 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 was 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 effect for boys between twelve and seventeen years of age is significant in every postprogram round, with a 7.6 percent, 5.6 percent, and 10.2 percent increase in the attendance rate of boys in November 1998, June 1999,

**TABLE 6. The Impact of PROGRESA on the Probability of Children's Being Enrolled in School[a]**

Difference-in-difference estimates

| Age group | Preprogram level | Impact: November 1998 Coefficient | November 1998 t statistic | June 1999 Coefficient | June 1999 t statistic | November 1999 Coefficient | November 1999 t statistic |
|---|---|---|---|---|---|---|---|
| Boys | | | | | | | |
| 8 to 11 | 0.9363 | 0.013 | 1.8 | 0.011 | 1.6 | 0.018 | 2.7 |
| 12 to 17 | 0.5678 | 0.043 | 2.4 | 0.032 | 1.8 | 0.058 | 2.8 |
| 12 to 13 | 0.8128 | 0.025 | 1.5 | 0.023 | 1.3 | 0.033 | 1.8 |
| 14 to 15 | 0.5263 | 0.063 | 2.3 | 0.053 | 2.1 | 0.050 | 1.7 |
| 16 to 17 | 0.2780 | 0.026 | 0.9 | 0.009 | 0.3 | 0.054 | 1.9 |
| Girls | | | | | | | |
| 8 to 11 | 0.9402 | 0.003 | 0.1 | 0.006 | .01 | –0.003 | 0.3 |
| 12 to 17 | 0.4807 | 0.078 | 4.3 | 0.075 | 3.8 | 0.095 | 4.3 |
| 12 to 13 | 0.7184 | 0.058 | 3.1 | 0.067 | 3.2 | 0.075 | 3.7 |
| 14 to 15 | 0.4312 | 0.092 | 3.4 | 0.101 | 3.4 | 0.109 | 3.7 |
| 16 to 17 | 0.2070 | 0.031 | 1.3 | –0.002 | –0.1 | 0.018 | 0.7 |

Source: Authors' calculations, based on data from the November 1997 ENCASEH census and the November 1998, June 1999, and November 1999 ENCEL evaluation surveys.

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

and November 1999, respectively. The effects of the program are even higher for girls.[26] In November 1998 the attendance rate increased by 16 percent relative to the preprogram level, and in November 1999 it increased by 19.8 percent.

The displacement of the incidence of child work is generally smaller, however, 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 resulted in a decrease of 3.2 percentage points in the work activity participation rate of boys aged twelve to seventeen (see table 5) and an increase of 4.3 percentage points in the incidence of school enrollment (see table 6).

One interpretation of these results is that the increased rate of school attendance of boys in this age group is obtained mainly by boys with-

26. This is similar to results obtained by Schultz (2000).

drawing from labor force activities rather than combining school with work. In similar programs, such as the 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, which implies that the program cuts children's leisure time.[27] The lower incidence of child work resulting from 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 school enrollment of boys aged twelve to seventeen 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 owing to the PROGRESA program accounts for 23 percent of the increase in the school attendance rate of girls aged twelve to seventeen 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 among girls is probably occurring through the combination of domestic work with school. Whether this is indeed the case can only be addressed through closer investigation of the time-use survey (see below).

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

27. See Ravallion and Wodon (2000).

### *Impact on Leisure and Time Use*

Given that the time-use module was carried out only once (approximately one year after the program's implementation), we cannot employ the same double-difference estimator used above. This limits us to using the cross-sectional difference estimator, which compares postprogram differences in the means of treatment and control groups. For 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 ordinary least squares (OLS).

Since we only have one round of data on time use, PROGRESA's impact is measured by a simple dummy variable indicating whether the family lives in a treatment community or a control community. Specifically, the coefficient $\gamma$ provides an estimate of the cross-sectional difference in the conditional mean leisure between children in treatment and control communities, that is,

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

Similarly, participation in activities is analyzed using a probit model of the form

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

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 preexisting differences in the work rates of boys and girls in the treatment and control samples suggest that the estimates of the program's impact 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, our estimates of equation 1 offer some valuable information on preprogram differences in

child school enrollment and work rates. For the two different activities and the five different age groups analyzed in tables 5 and 6 (ten cases in total), 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 (that is, $\beta_T = 0$ in equation 1) in 20 percent of the cases for boys. For girls we reject the same hypothesis for 40 percent of the total cases 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 of the program's impact.

The analysis of the program's impact 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. We account for the censoring at zero on the basis of Heckman's two-stage method for correcting for selection bias.[28] Thus to find the program's impact on the hours spent on each activity, we estimate an equation of the form

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

where $H^A(i)$ and $\lambda(i)$ represents 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 the sale of products. Farm work is defined as working on land (including, but not limited to, family land) and caring for animals. Finally, domestic work encompasses realizing purchases for the family; making clothes for family members; taking a family member to school, work, the health center, or the hospital; cleaning the house; washing and ironing clothes; cooking; fetching water or firewood or disposing of trash; and caring for small children, elderly family members, or sick individuals. Leisure is defined as the total hours in a day minus time spent in all work activities and in nonwork activities such as transportation.

The reference period for the time-use questions only covers time spent in the activity during the previous day. This is not an ideal timeframe, since the day in question may not have been typical of the child's normal activities. Additionally, many of the listed activities are done infrequently (that is, not daily), such that the survey is likely to underestimate

28. See Heckman (1979).

participation in certain activities. The survey was designed in this way as it was thought the short timeframe would reduce recall bias, given the large number of activities included in the questionnaire.[29]

The format implies that the impact on these variables must be interpreted with caution. In the case of schooling, children who are in fact enrolled in school may not have attended on the previous day. Our school participation measure thus effectively captures both enrollment and attendance. Fortunately, the main ENCEL survey provides more direct information on enrollment, so 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 school enrollment than does the more direct measure of asking the parent whether the child is attending school. The overall bias is about 15 percent, that is, of children reporting they were enrolled in school, about 15 percent reported 0 hours spent at school the previous day in the time-use survey.[30]

Our basic control variables are identical to those included above in the analysis of labor force participation. To identify the Heckman models, we use distance to school and to local labor markets as identifying variables for children. We hypothesize that these factors will affect the probability

29. Analyses of time use generally suffer from the defect that individuals may engage in more than one activity simultaneously, such as cooking and caring for children. The survey tries to address this point through a series of questions about the activities in which individuals spent the most time and the 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 simultaneously. Furthermore, many of the activities that respondents reported doing simultaneously are difficult to interpret. For instance, in almost a third of the cases in which individuals report doing two activities at the same time, one of the activities is transportation while the other is in most cases either school attendance or paid work activity. It is not plausible that the two activities were done at the same time; rather, they are related activities that were done at different times. Consequently, we ignore the issue of activities that may have been done at the same time, although this may cause us to slightly overestimate leisure and time spent on each activity. In particular, the amount of time spent on domestic work may be overestimated. This should not bias the results with regard to the program's impact on time spent, unless PROGRESA makes beneficiaries more or less likely to do more than one activity at a 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.

30. This would seem to suggest a rather high rate of absenteeism, which is largely explained by the fact that the school year is almost over and absenteeism is high at the end of the school year. The reasons most commonly given for a child's missing days of school are illness, work, and the teacher not showing up at school.

of participation in school or work, but not the amount of time spent in each activity.[31]

The results presented above 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, in that there may be a greater need for child labor during the summer months (that is, June through September). Alternatively, it may reflect the fact that many interviews were likely carried out at, or close to, the end of the school year, when children have fewer conflicts with the time they dedicate to work. In other words, during the summer months when school is not in session, the program's incentive to reduce children's work effort may be largely eliminated. It is perhaps unfortunate that the only time-use module was carried out in this same period, since the results reported here may underestimate the impact as a result. Some children may already have been out of school when the survey was carried out, and the program is thus much less likely to have affected their time allocation. While school did not officially get out until the middle of July, it is possible that schools in rural areas end early or that rates of attendance decrease as the end of the school year approaches. To ensure that we exclude interviews conducted when school was no longer in session, we discount all interviews that were carried out after July 4. For interviews carried out after that date, the proportion of children who report attending school the previous day decreases considerably.[32]

31. Through its benefits, PROGRESA is likely to increase school enrollment, but those students who reenroll in school (that is, who were not enrolled prior to the program) are not necessarily representative of those students who were attending before receiving program benefits. They may be lower-ability students who are less likely or less able to spend time doing homework, so they may actually lower the average time that children dedicate to schooling relative to the control group. It might then appear (falsely) as if PROGRESA had either reduced on the amount of time spent on schooling or had a lower increase than expected. One way to correct for this issue is to identify which children were in school prior to the program. While the time-use survey was only carried out once after the program was implemented, we do have other variables on school enrollment from a survey carried out prior to program implementation, which we can link to the time-use sample. When we repeated the analysis after eliminating children from the sample who were not previously in school but reenrolled after beginning to receive PROGRESA benefits, we obtained similar results, which are not reported here.

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

Table 7 presents the results on PROGRESA's impact on total leisure time for boys and girls. For boys, PROGRESA does not appear to have significant effects on leisure time: the analysis produces consistently insignificant effects for boys in all age groups. For girls, PROGRESA has a negative and significant effect on leisure time. The size of the impact for the overall group of girls aged eight to seventeen is relatively small, however, corresponding to about 0.2 hours per day, or 1.4 hours per week. The negative effect is largely concentrated on girls aged twelve to thirteen, who show larger reductions in leisure time of about 0.4 hours per day, or about 2.8 hours per week. Given the program's large impact on raising the school enrollment of girls, these effects suggest that girls may increase schooling by more than they reduce work, even when a broad definition for work is used. We examine this hypothesis in more detail below.

Table 8 presents the results for PROGRESA's impact on participation and hours dedicated to school and work, including the impact on overall work and the impact on the three subcategories of work (market work, farm work, and domestic work). Here, we split the sample by age groups identical to those above, namely, twelve to thirteen, fourteen to fifteen, and sixteen to seventeen. Nevertheless, the impact estimates by age group begin to give rise to sample size problems. Given the necessary data-cleaning exercise of eliminating interviews in which the reference period was Saturday or Sunday and interviews carried out after July 4, we have less than 1,000 total cases. This results in only 100 cases of positive work hours for some of our work categories. We therefore focus on our results for the larger groups of children aged eight to seventeen and twelve to seventeen, rather than for the disaggregated age groups.

Table 8 shows that PROGRESA caused a significant increase in the school participation of boys. The size of the impact is approximately 4 percentage points for the group of boys aged twelve to seventeen, which corresponds to an increase in school participation of about 8 percent. This impact appears to be largely concentrated on boys aged twelve to thirteen, which is broadly consistent with previous studies of PROGRESA's impact on schooling.[33] With respect to hours spent in school, the only significant impact is an increase in time dedicated to school of almost one hour daily among boys aged sixteen to seventeen.

33. See Schultz (2000); Behrman, Sengupta, and Todd (2001).

**TABLE 7. The Impact of PROGRESA on Children's Daily Hours of Leisure**
Cross-sectional difference estimates

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

Source: Authors' calculations, based on data from the June 1999 ENCEL evaluation survey.

Turning now to work, we first consider the total work participation 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 aged eight to seventeen. Concentrating on the group of boys aged twelve to seventeen 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 evidence on the possible substitution that may exist between work and school for boys in these communities.[34] Overall hours dedicated to work are not affected, however. This suggests that the program's impact is 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 to reduce the number of hours worked by children who attend school and work.

With regard to PROGRESA's impact on the type of work for boys, the results show a negative impact on participation in market work for the group of boys aged eight to seventeen, with larger reductions in the group

34. In the case of Bangladesh, Ravallion and Wodon (2000) find much lower proportional reductions in work relative to increases in schooling. This may reflect the different nature of the benefits provided, or it may be related to the fact that we use a broad definition of work, whereas they consider only market work activities.

**TABLE 8. Impact of PROGRESA on Children's Time Use**
Cross-sectional difference estimates

| Activity and age group | Participation | | | Daily hours | | |
|---|---|---|---|---|---|---|
| | Preprogram level | Impact | | Preprogram level | Impact | |
| | | Coefficient | t statistic | | Coefficient | t statistic |
| *Boys* | | | | | | |
| School | | | | | | |
| 8 to 17 | 0.68 | 0.022 | 1.9 | 6.07 | 0.073 | 1.5 |
| 12 to 17 | 0.57 | 0.042 | 2.5 | 6.30 | 0.038 | 0.5 |
| 12 to 13 | 0.76 | 0.041 | 1.9 | 6.16 | −0.157 | −1.6 |
| 14 to 15 | 0.58 | 0.034 | 1.2 | 6.36 | 0.084 | 0.6 |
| 16 to 17 | 0.31 | 0.034 | 1.2 | 6.40 | 0.489 | 2.3 |
| All work | | | | | | |
| 8 to 17 | 0.47 | −0.023 | −1.9 | 3.82 | −0.148 | −1.3 |
| 12 to 17 | 0.55 | −0.035 | −2.2 | 4.70 | −0.260 | −1.7 |
| 12 to 13 | 0.44 | −0.014 | −0.5 | 2.97 | −0.667 | −3.1 |
| 14 to 15 | 0.58 | −0.046 | −1.7 | 4.50 | 0.025 | 0.1 |
| 16 to 17 | 0.69 | −0.044 | −1.5 | 6.36 | −0.245 | −0.9 |
| Market work | | | | | | |
| 8 to 17 | 0.09 | −0.006 | −1.8 | 7.47 | −0.169 | −1.0 |
| 12 to 17 | 0.15 | −0.021 | −2.3 | 7.60 | −0.168 | −1.0 |
| 12 to 13 | 0.05 | −0.020 | −3.1 | 6.49 | 2.039 | 0.8 |
| 14 to 15 | 0.13 | −0.012 | −0.7 | 7.74 | −0.274 | −0.8 |
| 16 to 17 | 0.30 | −0.024 | −0.9 | 7.76 | −0.118 | −0.6 |
| Farm work | | | | | | |
| 8 to 17 | 0.34 | −0.020 | −1.7 | 2.87 | −0.016 | −0.3 |
| 12 to 17 | 0.37 | −0.024 | −1.6 | 1.65 | −0.034 | −0.4 |
| 12 to 13 | 0.31 | 0.022 | 0.9 | 1.48 | −0.090 | −0.7 |
| 14 to 15 | 0.42 | −0.044 | −1.6 | 1.54 | 0.257 | 0.9 |
| 16 to 17 | 0.40 | −0.063 | −2.1 | 1.99 | −0.443 | −1.5 |
| Domestic work | | | | | | |
| 8 to 17 | 0.18 | −0.006 | −0.7 | 2.01 | −0.119 | −0.7 |
| 12 to 17 | 0.21 | −0.015 | −1.2 | 4.11 | −0.163 | −0.7 |
| 12 to 13 | 0.18 | −0.014 | −0.8 | 3.07 | −0.242 | −0.7 |
| 14 to 15 | 0.21 | −0.007 | −0.3 | 4.26 | −0.339 | −0.8 |
| 16 to 17 | 0.26 | −0.016 | −0.63 | 4.73 | −0.179 | −0.4 |
| *Girls* | | | | | | |
| School | | | | | | |
| 8 to 17 | 0.64 | 0.040 | 3.4 | 6.03 | 0.121 | 2.5 |
| 12 to 17 | 0.51 | 0.065 | 3.5 | 6.30 | 0.111 | 1.5 |
| 12 to 13 | 0.71 | 0.066 | 3.0 | 6.11 | 0.138 | 1.4 |
| 14 to 15 | 0.52 | 0.079 | 2.7 | 6.55 | −0.004 | 0.0 |
| 16 to 17 | 0.23 | 0.040 | 1.5 | 6.38 | 0.186 | 0.4 |
| All work | | | | | | |
| 8 to 17 | 0.52 | −0.032 | −2.5 | 3.42 | −0.112 | −1.1 |
| 12 to 17 | 0.63 | −0.032 | −2.0 | 4.00 | −0.202 | −1.5 |

**TABLE 8. Continued**
Cross-sectional difference estimates

| Activity and age group | Participation | | | Daily hours | | |
|---|---|---|---|---|---|---|
| | Preprogram level | Impact | | Preprogram level | Impact | |
| | | Coefficient | t statistic | | Coefficient | t statistic |
| 12 to 13 | 0.53 | –0.015 | –0.6 | 2.83 | –0.274 | –1.4 |
| 14 to 15 | 0.65 | –0.043 | –1.6 | 3.90 | –0.281 | –1.3 |
| 16 to 17 | 0.76 | –0.045 | –1.7 | 5.19 | –0.044 | –0.2 |
| Market work[a] | | | | | | |
| 8 to 17 | 0.02 | 0.000 | –0.1 | 7.47 | –0.436 | –1.2 |
| 12 to 17 | 0.05 | 0.000 | 0.0 | 7.58 | –0.912 | –2.4 |
| 12 to 13 | 0.01 | 0.003 | 1.2 | 6.25 | n.a. | n.a. |
| 14 to 15 | 0.04 | –0.015 | –1.8 | 8.55 | n.a. | n.a. |
| 16 to 17 | 0.12 | 0.013 | 0.7 | 7.78 | n.a. | n.a. |
| Farm work | | | | | | |
| 8 to 17 | 0.48 | –0.040 | –3.2 | 2.87 | –0.076 | –0.8 |
| 12 to 17 | 0.58 | –0.043 | –2.6 | 3.31 | –0.161 | –1.3 |
| 12 to 13 | 0.51 | –0.023 | –0.9 | 2.45 | –0.249 | –1.5 |
| 14 to 15 | 0.61 | –0.045 | –1.6 | 3.33 | –0.203 | –0.6 |
| 16 to 17 | 0.69 | –0.071 | –2.4 | 4.26 | 0.001 | 0.0 |
| Domestic work | | | | | | |
| 8 to 17 | 0.09 | 0.000 | –0.1 | 2.00 | 0.287 | 1.4 |
| 12 to 17 | 0.10 | –0.004 | –0.5 | 2.11 | 0.541 | 1.9 |
| 12 to 13 | 0.10 | –0.005 | –0.4 | 2.34 | 0.006 | 0.0 |
| 14 to 15 | 0.10 | 0.003 | 0.2 | 1.24 | 1.322 | 3.0 |
| 16 to 17 | 0.10 | –0.010 | –0.6 | 2.06 | 0.736 | 1.4 |

Source: Authors' calculations, based on data from the June 1999 ENCEL evaluation survey.
a. Impact on market hours for girls by age are omitted because of small number of cases.

of boys aged twelve to seventeen. Consistent with the results on schooling participation, the largest reductions in participation in market work appear to be concentrated among boys aged twelve to thirteen, who show PROGRESA-related reductions in market work of approximately 40 percent from initial levels. Nevertheless, PROGRESA has no impact on hours dedicated to market work for boys in any age group.

The results show a reduction in participation in domestic work for boys, particularly for boys aged fourteen and over. With respect to farm work, all the coefficients are negative, but none are significant at conventional levels, implying that there is no evidence that PROGRESA reduces boys' participation in farm work.

In the case of girls, the estimates on school participation are much larger than for boys, which is consistent with the results shown above

and with previous studies.[35] For the group of girls aged eight to seventeen, the average impact of PROGRESA on girls' school participation is almost twice the program's impact for boys. For girls aged twelve to seventeen, PROGRESA increased participation by 7 percentage points, or about 14 percent from an average preprogram level of 51 percent.

With regard to work participation, our measure of total work shows significant reductions stemming from PROGRESA. If the analysis is decomposed by type of work, the results show little impact on reducing market work for girls. The only exception is in the group of girls aged fourteen to fifteen, for whom work participation is significantly reduced, although there is no impact on hours. The largest reductions in work for girls correspond to reductions in domestic work, particularly for girls aged fourteen and over, who show reductions in participation of about 10 percent. While all the estimated coefficients are negative, PROGRESA has no significant effect in reducing the time spent by girls in domestic work. Again, PROGRESA appears to be successful at increasing school participation and reducing participation in work, but with little impact on reducing the hours of children who continue to work.

In summary, the results show that the program's largest impact is on the time use of children above the age of twelve. These age groups correspond to enrollment in secondary (junior high) school. Our results are consistent with previous studies: we find that PROGRESA has a much larger impact on the school participation of girls than boys, producing nearly double the effect. These increases in schooling are associated with reductions in work. Boys show a reduction in both market and domestic work, whereas girls demonstrate significant reductions in domestic work. For boys, the reductions in work participation are approximately equivalent to the increases in school participation, providing evidence that work and school are, to some extent, competing activities. For girls, however, while the reductions in work participation also tend to be significant, the impact tends to be smaller than the increases in school participation. This suggests that the work activities of girls may be more compatible with school, that is, they tend to be activities that can be done within the span of a few hours daily. PROGRESA's impact on the overall time use of girls thus appears to be to reduce their leisure time slightly.

35. See Schultz (2000).

## Concluding Remarks and Policy Considerations

PROGRESA now extends over the large majority of all rural communities in Mexico and benefits about 40 percent of all rural families. This study has analyzed the program's impact on the work and schooling of children. Overall, we find an important impact on children's participation in work activities and school attendance. Double-difference estimates of the program's impact before and after its implementation show significant increases in the school attendance of boys and girls, accompanied by significant reductions in their participation in work activities. We also find that the displacement of the incidence of work is generally 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 through their combining school with domestic work, which is left out of our initial measure of work.

A more inclusive measure of work is obtained from the time-use module, which collected information on the hours devoted to a variety of activities during the previous day. Children, in particular boys and girls of secondary school age, are much more likely to attend school and to spend more time on school activities as a result of PROGRESA. In terms of work, boys of secondary school age show strong reductions in participation in both market work and domestic work. Girls of all ages show reductions in participation and hours spent in domestic work.

The reduction in domestic work for girls as a result of PROGRESA is noteworthy. This is one of the first studies to show that subsidizing school enrollment can reduce the time girls spend in domestic work. PROGRESA is thus associated with both increasing enrollment and reducing domestic work. This suggests that domestic work competes with time spent on school, although many girls combine the two activities. 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 relation between school and work and the extent to which work is a deterrent to school, our findings confirm that children's work is an important deterrent to school for both boys and girls, although less so for girls than boys. When work is defined broadly to

include market, domestic, and farm work, the reductions in work for boys are, to a large degree, comparable with the increases in schooling. In contrast, the reductions in girls' work implied by the coefficients are significantly less than the increases in schooling. This is a likely consequence of the trends shown earlier: while many girls participate in domestic work, they work a low number of hours that permits them to combine school and work. This is confirmed by the fact that PROGRESA decreases the overall leisure time of girls slightly, which is consistent with work reductions that are lower than the increases in school.

Our findings thus suggest that a conditional cash transfer program like PROGRESA can be successful at increasing school attendance and decreasing child labor simultaneously. While encouraging, these findings may raise more questions than they answer. For example, could 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 work less and attend 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. Could similar or even better effects on school attendance and child labor be achieved through alternative means, such as building new schools or improving the quality of educational services?

Most of these questions, together with an analysis of the program's impact on health, nutrition, and consumption, are addressed in detail in numerous studies conducted as part of a project to evaluate PROGRESA.[36] Given the large number of studies involved and the diversity of topics, we focus our discussion on the findings that are directly relevant to children's work and schooling. In a separate study, we also examine the program's impact on the time allocation of the adult members of beneficiary households.[37] 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 that children allocate to school activities do not

36. 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.

37. See Parker and Skoufias (2000).

have any significant effects on the work incentives of adult household members.

Assuming that the program's effects could be sustained over the period in which a child is of school age, we estimate that the program increases educational attainment by about 10 percent overall. If current urban wages approximate what PROGRESA's beneficiaries can expect to earn from their schooling in terms of future percentage increases in their wages, the internal rate of return to PROGRESA's educational benefits, taking into account the cost of the grants, is roughly 8 percent per year.[38] Children, when they reach adulthood, will have permanently higher earnings of 8 percent as a result of the increased years of schooling.

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.[39] Given the complexity of the program, this level of program costs appears to be quite small. It is definitely low compared to the numbers for roughly comparable programs.[40] With regard to alternative programs that might 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 four kilometers from their junior secondary school, the impact on secondary school enrollments would be less than one-tenth the size of that achieved through PROGRESA. Increased access to schooling thus appears to be a less effective means of raising school enrollments than PROGRESA's targeted educational grants to poor families.[41]

The analysis presented here, along with the majority of the results of the evaluation of the program's impact in other areas, shows a large degree of support for the idea that schooling and work are incompatible and that work can be reduced through subsidizing schooling. A well-targeted and efficiently administered conditional cash grant program that lowers the price of schooling, as PROGRESA does, can be successful at inducing families to withdraw their children from work and send them to school instead. Given the program's positive effects on nutrition and

38. See Schultz (2000).

39. See Coady (2000).

40. These programs are the milk subsidy (LICONSA) and the tortilla subsidy (FIDELIST) programs mentioned in Coady (2000).

41. Coady and Parker (2001).

health, the findings of the evaluation provide solid support for the notion that it is possible to combine short-run reduction in rural poverty with improvements in the human capital of both younger and older rural family members.[42]

The opportunity to conduct a rigorous evaluation of PROGRESA has created a higher set of standards for the design and conduct of social policy in Mexico and in Latin America as a whole. Policymakers now have a better sense of what types of programs can effectively alleviate poverty in the short and long terms. However, the list of questions and concerns about program options and design cannot help but grow longer. Could unconditional cash transfers with no strings attached have the same impact on the human capital investment of poor rural families? Is the simultaneous intervention in the areas of education, health, and nutrition preferable to intervening in each of these sectors separately? Is there a minimum cash transfer that could achieve the same program impact, and if so, how could one determine this amount? Early involvement in the design and evaluation of programs implemented in other Latin American countries, such as Argentina, Colombia, Honduras, Jamaica, and Nicaragua, may shed some light on these critical questions.

42. For the impact on current poverty and a summary of all the results on PROGRESA's impact on health and child nutrition, see Skoufias and McClafferty (2001).