SCHOOL SUBSIDIES FOR THE POOR: EVALUATING A MEXICAN STRATEGY FOR REDUCING POVERTY

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ABSTRACT

This paper assesses how the Progresa Program has affected the school enrollment of Mexican youth in the first 15 months of its operation. Progresa provides poor mothers in poor rural communities with education grants, if their children attend school regularly. Enrollment rates are compared between groups of poor children who reside in communities randomly selected to participate in the initial phase of the Progresa program and those who reside in other comparably poor (control) communities. Pre-program comparisons document how well the randomized design is implemented, and double-differenced estimators are reported over time within this panel of children. Probit models are then estimated for the probability that an individual child is enrolled, which statistically controls for additional characteristics of the child, their parents, local schools, and community, and for samples of different compositions, to evaluate the sensitivity of the estimated program effects to these variations. If the current relationship of the program outlays to enrollments, and that of schooling to increased adult earnings, both persist in the future, the internal rate of return to the Progresa educational grants as an investment is estimated to be about 8 percent, which accrues in addition to the program s efficacy as a poverty reduction program.

Poverty alleviation programs have taken a variety of forms. Social welfare programs provide transfers in cash and kind to people with incomes and assets below a specified level in most high-income countries. These transfer programs may also impose additional conditions and limitations, such as in the United States, where welfare payments are provided primarily to lone mothers with dependent children. These conditional transfer programs are likely to distort the allocation of private economic resources and thereby reduce the efficiency of the economy. In particular, means-tested poverty programs are expected to reduce the time beneficiaries work in the paid labor force, because the earnings from paid work of beneficiaries is taxed at a higher rate than is the earnings of non-beneficiaries. Other distortions in behavior are also attributed to these programs, although the evidence is more controversial. For example, in the United States, those states which provide more generous welfare payments also report on average less frequent marriage and more non-marital childbearing (e.g. Schultz, 1994; Rosenzweig, 1999), which could be attributed to these programs which support only mothers without a resident husband.

In populations engaged in agriculture, poverty alleviation programs have often taken the form of setting minimum prices for farm outputs or paying subsidies for farm inputs. Two frequently heard justifications for these programs are, first, farm incomes are on average lower than non-farm incomes, and second, farm prices are volatile causing farm incomes, before transfers and taxes, to vary more over time than non-farm incomes. But in contrast to means-tested income supports in welfare programs, agricultural price supports are generally not specifically targeted to poor families, but only to a relatively poor occupation or industry. Farm price supports also contribute to an inefficient allocation of resources by encouraging the production of outputs of less economic value than would otherwise occur, including the allocation of more labor and capital to the production of price-supported commodities and a corresponding reduction in the rate of net migration out of agriculture. As a result, consumers usually pay a higher price for farm products, and states subsidize agricultural exports and tax agricultural imports.

In neither the income supplement nor output-price support program is there an expectation that beneficiaries will invest more in the acquisition of skills or the accumulation of capital to boost their future productivity and consumption, and thus diminish their dependence on transfers from the state. Indeed, one consequence of both of these types of poverty programs is to erode the workers incentive to change their sector of employment, or accumulate new types of productive job experience, or generally invest in human capital. The growing appreciation of the cumulative lifetime career costs of these distortions strengthened the dissatisfaction in the United States with its Aid for Families with Dependent Children (AFDC) program, which led to the redesign of this program in 1996 to include a lifetime limitation of not more than five-year of transfers, and to the funding of coordinated child care and job training programs to encourage poor mothers to become self supporting.

The Progresa Program in Mexico, which is examined in this paper, takes a different approach to poverty reduction by subsidizing the investment of poor families in their children s human capital. It provides means-tested transfers to poor rural mothers, whose children are enrolled in school from grades 3 through 9. The program seeks to reduce the current level of poverty in Mexico and to increase the schooling, and thereby the future productivity, of children from poor rural families. This anti-poverty program started paying grants to eligible beneficiaries in September 1998, and by the start of 2000 the program had enlisted two million families in Mexico, or about one-tenth of the entire Mexican population. The government implemented the Program as a phased social experiment, collecting sufficient background and follow-up survey information to facilitate cost-benefit evaluations of the Program. Although the Progresa program may reduce labor supply of these poor families, to the extent that it increases the incomes of poor mothers, it does not impose a further tax on earnings that specifically deters work in the market economy. The distortion in resource allocation caused by the Progresa program is thus minimized to what might be called a pure income effect associated with achieving the program objective of poverty reduction. But it does not in addition distort relative prices or returns from work, marital status, or resource allocation in general by its conditions for eligibility.

One change in resource allocation expected from the Progresa Program is an increase in the school enrollment of poor children. The objective of this paper is to estimate the enrollment impact of the program on children from poor households (i.e. who are administratively designated as eligible for program benefits) who live in poor rural communities that were randomly selected to participate in the first phase of the Progresa Program (e.g. the treatment). The program impact is inferred from comparisons with poor children from randomly selected communities (e.g. the control) which did not benefit from the first expansion of the Progresa Program. Both the treatment and control populations were surveyed twice in the year before the Program was announced, and followed for two years after the Program commenced, providing a total of five survey cycles for this study. There are about 200,000 people in 495 poor rural communities originally included in a background Census in October 1997, of which almost two-thirds of the communities were designated as program treatment areas in September 1998, and in these areas about two-thirds of the households were judged eligible for program benefits or sufficiently poor according to a combination of initial household census and community indicators of consumption, wealth, and income.

The design of the Progresa Program, and the initial patterns of enrollment are reviewed in section 1. A framework for studying enrollment decisions is outlined in section 2. Estimates of the difference between treatment and control groups are explored in section 3, whereas the probability of enrollment for the individual child is estimated in section 4. Section 5 combines information on the estimated Program enrollment effects, the outlays on educational subsidies, and the wage structure to construct the internal rate of return realized within the program, assuming its only goal is to invest efficiently in educational human capital. It is, of course, more than that, for it also seeks to alleviate current problems of low levels of consumption among a major group of Mexico s poorest people.

1. Program Mechanism, Enrollment Pattern, and Evaluation

In a low income rural community in Mexico that is designated as participating in the Progresa Program, mothers living in an administratively determined poor household whose child enrolled in grades 3 through 9 could receive from the central government a check every two months for the amount indicated in Table 1.[1] The size of this educational grant increases fourfold from 3rd to 9th grade, and they are slightly larger for girls than boys at the junior secondary school level, based on the concern that enrollment rates of girls lag about six percentage points behind boys at this level (Cf. Appendix Table A-1). To assess the relative magnitude of these educational grants, it is useful to note that a daughter enrolled in the 9th grade might qualify a family to receive a grant of 255 pesos per month, which amounts to 44 percent of the typical male day-laborer s wage in these agricultural communities.[2]

The likelihood that a child is enrolled in school is related to the child s age and years of schooling completed at the end of the prior school year, as illustrated in Table 2. Beneath the number of children enumerated in the October 1997 census of households and March 1998 survey is the enrollment rate for children in each age-education cell. Comparing in Table 2 the marginal column on the right that represents the age-specific enrollment rates, and the bottom row that represents the grade-completed enrollment rates, it is evident that enrollment rates are more sensitive to grade than age. The primary school enrollment rate among children who had completed

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[1] These amounts have increased every six months to adjust for inflation figures reported by the Bank of Mexico (Coady and Djebbari, 1999). There are additional supports (cash and kind) for school materials of 120 pesos per year at the primary level and 240 pesos per year at the secondary level, provided on a term by term basis (Progresa, 1999).

[2] The daily wage for male agricultural labor reported in the 1998 and 1999 Community Surveys averaged 29 pesos, and the worker is assumed to work 20 days a month to yield a monthly wage of 580 pesos. There is an additional monthly transfer per family of 90 pesos, if the mother s family receives recommended periodic health exams, which can lead to further health care, including nutritional supplements and immunizations for children as well as pregnant and lactating women (Coady and Djebbari, 1999; PROGRESA, 1999).

grades 1 through 5 is about 95 percent, and recovers to 97 percent after a child completes the first year of junior secondary school or 7th grade. In the transition year from elementary to junior secondary school, however, the enrollment rate falls to 58 percent, after completing 6th grade, and drops again to 63 percent in the first year of senior secondary school. This analysis focuses, therefore, on enrollment rates within groups of children stratified by the number of grades they have completed. This partitioning of the sample also facilitates estimation of program effects, for a child to qualify for a Progresa educational grant they must have completed the 2nd to 8th grade and be currently enrolled.

Two samples are analyzed from the base census and follow-up surveys: a balanced panel and a pooled sample. The panel sample includes all children age 5 to 16 observed in the October 1997 household census, who completed the age, schooling, and enrollment questions, for whom the schooling of co-resident parents is reported, and the locality is matched to other community information files. The panel sample is further restricted to include only those children who could be followed and matched in the subsequent pre-program survey round in March 1998, and then in three surveys in October 1998, May 1999, and November 1999, which occurred after the Progresa Program had started to provide education grants.

The second larger pooled sample includes all children age 5 to 18 who are observed at least once and can be linked to sufficient household data to estimate the basic enrollment model.[3] When a specific variable is not reported by the household or

[3] In the fifth round of the survey, information was collected on resident children up to age 18, rather than only to age 16 as in the previous four survey rounds. These observations on older children are retained in the analysis, and additional age dummies are added for age 17 or 18 in the estimates of probit models for enrollment probabilities. Because of the 6 to 16 age limitation in rounds 2 through 4, the sample of children in the highest grade group, 9 or more,

missing for one of the 495 localities, a dummy variable is included to reflect the missing status of this variable, to avoid selecting the estimation sample on the basis of reporting characteristics. The working sample includes 314 localities where Progresa began to operate after the summer of 1998 and 181 non-Progresa (control) localities. These localities span the poorest regions of seven states of Mexico: Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi, Veracruz, and Guerrero.[4] The number of children age 5 to 16 enumerated in the first census is 40,959, and the number for which all five survey observations are matched is 19,716. Appendix Table A-1 reports the mean and standard deviation of the variables considered in the analysis for the panel and pooled samples, divided by males and females, separately for primary and secondary school levels.

Each locality has its own primary school, and its teacher-to-student ratio is examined as an crude indicator of school quality. Only about a quarter of the localities has its own junior secondary school, and thus the distance from the locality to the nearest such school is considered as an indicator of the time costs that a child and family could take into account in determining whether to enroll in junior secondary school.[5] Finally, two variables are included to capture the remoteness of the community

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and probably also the lowest grade group, with no years of schooling completed, are truncated and may therefore be unrepresentative of the entire population, particularly in the panel sample. The differenced estimators should therefore be approached with caution as they relate to the groups of children with zero and 9 or more grades of school completed.

[4] The procedure used in the program to select a locality as a Progresa program areas or not, and the procedures used to designate a household as poor are described and analyzed elsewhere (Skoufias, et al. 1999; Behrman and Todd, 1999).

[5] The characteristics of the secondary school cannot be matched because some students traveled to schools that were not designated as nearest, and thus were not included in the database on schools. Thus, some students traveled longer distances to reach a preferred

from an urban labor market: the road distance to (a) the Cabeceras or the municipal administrative center (sample mean 10 km), and to (b) the nearest of the 39 largest metropolitan areas in Mexico (sample mean 104 km). Workers in urban labor markets tend to receive higher wages, and perhaps a larger wage premium for schooling. Greater distances should translate therefore into poorer local job opportunities and lower opportunity costs of the time of school-age children, but on the other hand, larger distances to urban areas would raise the costs of migration to these markets and probably reduce the information available locally about the higher educational returns in the urban labor force. In Latin America, as elsewhere, better educated youth are more likely to migrate from their rural birthplace to an urban area, once they reach maturity (Schultz, 1988).

The years of schooling completed by the mother and father are also treated as likely determinants of a child s school enrollment probability.[6] Information on family income is not directly included as a control variable, because household monetized income is influenced by the labor force behavior of the mother and other family

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junior secondary school than is attributed to them according to the available government data.

[6] Two dummy variables are also included to indicate if the parental education information is not available because the mother or father is not enumerated in the household. This procedure controls for the effect of lone parents, although I would prefer to deal with this variation in household composition as another jointly determined aspect of the coping strategies of women and their families. Exclusion of children without a father in the household would reduce the size of the child panel sample by about 12 percent and exclusion of those without a mother of the child would have reduced the sample by 5 percent (Table A-1). Thus, elimination of this source of variation by excluding all but intact parental couples could have introduced substantial sample selection bias and potential parameter bias in the subsequent estimation of program effects.

members, including the children themselves.[7] The pre-program realized income would therefore be behaviorally interrelated with the family s school enrollment decisions and the net relationship would not provide an unbiased estimate of the one-way causal effect of income on enrollment. Measures of post-program earned income could be expected to respond further to the program s effect on enrollment itself. Indeed, later the response of child labor to the Progresa experiment is explicitly considered. However, a latent variable index for household economic well-being is constructed from the 1997 regional census, from information on household consumption, assets and income. Because the Progresa Program used this index to determine a binary indicator of whether the household is sufficiently poor to be eligible for program benefits, this eligibility indicator, E, is treated as an exogenous conditioning variable for the enrollment decision in both the subsequent analysis of enrollment differences at the group level, and the enrollment probabilities estimated at the individual child level.

2. A Framework for Analysis of School Enrollment Decisions

The framework helps to connect the subsequent analyses, first at the group level and then at the individual child level, suggesting individual, family, community, school, and program administrative variables which might influence the likelihood that a specific child or a group of children enroll in school.[8] Let the probability of being enrolled in

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[7] Preliminary analyses of family labor supply responses to the Progresa Program suggest small effects. Some reduction is found for child labor, offset by small increases in male adult labor supply, and little change in female adult labor force participation (Gomez de Leon and Parker, 1999, 2000).

[8] Virtually all of the reported variation in school attendance is accounted for by the variation in enrollment that is analyzed here. Elsewhere I describe the role of the same explanatory factors to account for the variation in attendance rates among the children who report being enrolled and answering the attendance question (Schultz, 2000b). See Appendix Table A-1 for the magnitudes of attendance for the responding sample.

school for the i th child at the time of a survey be denoted as S_i. This likelihood of enrollment is affected by family demand for schooling, which may respond to such factors as school quality and access, the opportunity cost of the student s time minus enrollment subsidies provided after the start of Progresa, by household endowments and parent education, and a host of unobserved factors, such as those affecting the local labor market wage returns to schooling, and the family s own preferences for schooling. If the unobserved determinants of enrollment combined with various specification and stochastic errors create a normally distributed disturbance that is unrelated to the observed variables used to explain enrollment behavior, the probit model is a candidate to describe the enrollment decision process, and its parameters can be estimated by maximum likelihood methods. The standard errors of these probit estimates are adjusted for the clustering at the locality level of the explanatory variables representing the program, school and other community characteristics, which is analogous to the White adjustment for heteroscedasticity. [9]

A linear approximation of the estimated enrollment model can be expressed as follows:

i =1,2...,n (1)

[9] The probit models were also estimated assuming that random errors differed in their variances across families and this source of heteroscedasticity was thus shared by siblings, without modifying any of the basic findings discussed here. The Huber (1967) -White adjustment of the estimates for community cluster (n=495) effects increased modestly the standard errors, which are used here to calculate the reported absolute values of the asymptotic t ratios.

where i indexes the child, n represents the total number of children in the cross sectional survey, and the explanatory variables and the interpretation of their linearized effects on enrollments are discussed below, i.e. derivatives of the probit function evaluated at sample means.

First, there may be an effect on enrollments, $_1$, associated with residing in a Progresa locality, P_i =1 (otherwise zero), although the random assignment of the community locations for the Progresa Program is designed to minimize any such difference before the program informed the community of who would benefit from the Program. There may also be an effect, $_2$, of being designated as a child from a poor household, E_i = 1 (otherwise zero), who would be eligible for Progresa benefits when the transfer payments are initiated, if the family resides in a Progresa locality. One common hypothesis is that credit constraints limit the investment of the poor in their children s education, suggesting that $_2$ would be negative. An interaction binary variable defined as the product of the Progresa and poor variables, $P_i E_i$, would then exert an additional effect on enrollment denoted $_3$, which should be approximately zero until the Program transfer payments are announced, and thereafter it is expected to be positive. Having controlled for the two-way interaction effect, the direct effect of the Progresa Program for those who are not eligible for the educational grants, or $_1$, might be small even after the program has started, possibly capturing spillover effects between poor and rich families in Progresa-served communities and errors in program administration. Enrollment rates vary across grades in a school system (Cf. Table 2), and thus a control is needed for the grade level to which the child would be qualified to enroll. The variable C_k is defined as 1 if the child has completed precisely k years of school, k= 0, 1,

..., 8, 9 or more, which would qualify the child to enroll in the k+1 grade, and the coefficients on these dummy variables , $_{k}$, adjust for linear differences in enrollment by grade level.[10]

With the passage of time, some variables that explain the probability of enrollment in equation (1) may change, such as C which would change if a child completes one grade of schooling and is thus qualified to enroll in the next. The net effect of all unobserved variables that change over time is partially captured in the probit model by allowing a shift in the estimated intercept specific to each time period or survey cycle. In other words , $_{0t}$ is allowed to vary in each round of the survey, where t = 1,2,3,4,5. Because Progresa grants only started in September 1998, and the program effects on enrollments represented by the coefficients on P and PE are estimated as an additional set of interaction effects for the post-program periods in October 1998, May 1999 and November 1999 (t = 3, 4 and 5, respectively), and the estimated post-program effects are distinguished by asterisks in the enrollment equation (2) that combines all five survey cross sections:

(2)

[10] A three-way interaction effect between P, E, and C_k for the years when the program offers an educational grant for students in grades, k = 2, 3, ... , 8, was also introduced to demarcate the targeted range of educational subsidies, but they are were not precisely defined by the available data and are not reported. Cf. Schultz 2000c.

Equation (2) is estimated separately for boys and girls, because the probit parameters differ significantly by gender, particularly at the secondary school level. Given the relatively high level of enrollment at the primary level and the sharp decline in enrollment at the transition to the secondary level, the two levels are estimated separately. The primary sample is defined as all children age 6 to 16 who report C_{kt} =1, for k=0,1,2,...6, indicating that they have not yet completed primary school, and the secondary sample is defined as all children age 6 to 18 who report C_{kt} =1 for k=7,8,9 or more. It is assumed that Progresa s effect on enrollment is uniform by school level across grades by gender, when the probit model for equation (2) is estimated at the individual level, but the effect is allowed to vary by grade level in the group-differences.

If the J control variables, X , were uncorrelated in each time period with the program designated localities, P, and the eligibility of the poor, E , the program effect on enrollment could be obtained directly by stratifying the population by E and P and observing the incremental effect of P and PE in the periods after the program started to make educational awards. Figure 1 illustrates the implied four way stratification of the population of children for the purposes of calculating an enrollment rate, $S_{g\,t}$, g = 1,2,3,4. The Program effect in the post-program periods represents the Program s impact on the enrollment of poor children in school, which are stratified by grade completed. The first hypothesis tested by the differenced estimator of the Program level effects according to Figure 2 is as follows:

H_1 D1 = (S_{1t} S_{2t}) > 0 Post-program period average, t = 3,4,5 .

One way to investigate whether the P and E are randomized is to determine if the pre-program differences in enrollment rates between the poor children in Progresa and non-Progresa localities are in fact statistically not different from zero:

$H_2 \quad D1 = (S_{1t} \quad S_{2t}) = 0 \qquad$ Pre-program period average, $t = 1,2$.

Even if the Program placement were random, statistical correlation between program designated areas and pre-program enrollments might exist fortuitously. If the pre-program regional differences between eligible Progresa and control children were due to omitted variables that do not change over time in their impact on enrollment, the baseline pre-program differences in enrollments may be subtracted from that for the same children observed in the panel sample in post-program periods, and thus the difference in differences estimator (DD1) is defined as shown in Figure 2, which is expected to represent the positive impact of the program holding constant for persistent sources of pre-program regional variation:

$H_3 \quad DD1 = D1(\text{post-program}) - D1(\text{pre-program}) > 0$

Program transfers are only available to children of poor households, and this targeting of the program is expected to affect the distribution of enrollment by income levels within the Progresa localities. The enrollment rate difference between non-poor and poor households is expected to be positive before the program, and to decrease relative to that observed in non-Progresa localities after the program is initiated. One possible measure of the Program s effect on inequality in enrollment is defined in Figure 2:

$H_4 \qquad D2 < 0 \qquad$ Post-program period average, $t = 3,4,5$

But before the program started the two types of localities are expected, under random assignment of the programs, to exhibit the same degree of income inequality in enrollments, and this null hypothesis of random program placement is again testable :

$H_5 \qquad D2 = 0 \qquad$ Pre-program period average, $t = 1,2$.

A difference in differences estimator (DD2 defined in Figure 2) can again remove any time invariant sources of the preprogram regional variations in inequality, given the linear approximation postulated here:

H_6 $\quad$ DD2 < 0 .

Even if the randomization of program placement is not challenged, and H_2 and H_5 cannot be rejected, the difference in differences estimators are preferred to the post-program differences, because they remove persistent sources of regional variation in enrollment that might be expected to exist to some degree. It may still be useful to add additional explicit control variables and estimate their marginal effects jointly with those of the program on the enrollment of poor children, because this should increase the statistical power of the model estimated at the level of the individual child to isolate significant effects attributable to the program treatment, if there are any (Manning, et al. 1982). The estimated impact of the controls can also help to evaluate alternative policy options that might contribute to the social objective of increasing enrollment rates, particularly among the poor.

3. Enrollment Differences Between Progresa and Non-Progresa Localities

Table 3 reports the values of D1 for each grade level in the pre-program and post-program periods as well as the difference in differences over time or DD1, first for both sexes combined, and then for girls and boys separately. Beneath the difference in enrollment rates between the Progresa and non-Progresa localities, the statistical probability is reported (in parentheses) that the observed difference could have occurred randomly in a saturated probit model. If the conventional level of confidence required to accept the hypothesis is 5 percent or less, the D1 in the post-program surveys is significantly non-zero and positive after the 1st through 6th grades for both sexes combined. The largest difference in enrollment is for those children who had completed grade 6, and were thus qualified to enroll in junior secondary school; for this group the enrollment rate increases by 11.1 percentage points, from the level of 58 percent noted in the pre-program periods in Table 2, to about 69 percent. Note also that this program impact is disproportionately concentrated among girls, whose enrollment rate increases 14.8 percentage points compared with the boys whose enrollment increases 6.5 percentage points.

The pre-program values of D1 are positive in seven out of ten cases, but in none of these cases is the difference statistically different from zero, suggesting that the randomization of program placement with regard to prior enrollment levels as specified by hypothesis 2 is not rejected. Nonetheless, the difference in differences (DD1) estimate of the program s impact on enrollment rates is reported in the last three columns in Table 3, and they are also all positive from grade 1 to 8, and statistically significant for the groups having completed grade 4 and 6. The average value of D1 and DD1 over the grades 1 through 8 are of roughly similar magnitudes for both sexes combined, 3.6 and 3.4 percentage point increases in enrollment levels, respectively.

Table 4 reports D2 from the pre-program and post-program periods and the DD2 over time to assess whether the Progresa Program reduced inequality within localities between enrollments of non-poor and poor. Since this measure is only one of many that might be devised to represent inequality, it is not a unique measure of program impact as in the case of the level effects.[11] The D2 differences are negative from grade 1 to 6

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[11] For example one might be interested in how schooling gaps between children whose parents are better and worse educated changed with the onset of the Progresa Program, rather than measuring inequality with respect to the single threshold of the latent indicator of poverty defined as a condition of eligibility for Progresa transfers.

in the post-program period and statistically significant and negative after grades 4 through 6, implying the program reduces inequality, but the impact is largest after the last three years of primary school. The pre-program values of D2 are not jointly statistically significantly different from zero, but it is different for grade 6, and in this case it is surprisingly positive. The difference in differences, DD2, is negative from grade 1 to 8, and is statistically significant for grade 6. The average values for DD2 for grades 1 through 8 are larger in negative value than those of D2 post-program, 3.1 percentage points compared with 2.6 , respectively. There is evidence that the program has reduced income related inequalities in enrollment within localities, as would be expected based on the evidence presented earlier that the program induced an increase in the level of enrollments among the poorer households.

4. Response of Enrollment Probabilities to Program and Control Variables

Maximum likelihood estimates of the probit model for enrollment of the individual child are expressed as derivatives of enrollment with respect to the explanatory variables. The two Program associated enrollment effects on the poor are associated with living in a Progresa (P) locality and that of the Progresa-Eligible interaction (PE) as reported in rows 1 and 2 in Table 5, and summed to represent the net effect averaged across the three post-program survey rounds, 3, 4, and 5. This net effect of the program is estimated separately for girls and boys, at the primary and secondary school levels, first for the panel sample which underlies the previously reported group-difference estimators, and also for the larger pooled sample of children. The Probit model additionally controls for the child s age, mother s and father s years of schooling, primary school teacher-student ratio, distance to junior secondary school, and distances from the locality to urban areas (Schultz, 2000c). In brackets beneath the program net impact on the poor s enrollment probability is the statistical probability that this net impact of the program is zero, according to a joint ^{2} test associated with the likelihood ratio. The program s net impact on enrollment is statistically significant at the 1 percent level in 7 out of the 8 possible tests for different sexes, school levels, and samples, and in the eighth sample it satisfies the test at the 5 percent level. Thus, there appears to be a general positive enrollment effect of the program in the post-program surveys for both genders, both samples, and both school levels, with the inclusion of added control variables and the changes in sample composition.

At the primary school level the panel sample estimates imply that the average effect of the program across the three post-program rounds is to increase enrollment rates of girls by .96 percentage points, and boys by .74 percentage points, from the initially high enrollment rate of 94 percent (Table A-1). In the pooled sample which has a lower initial enrollment rate of 90 percent, the program is associated with an increase in enrollment rates for girls of 1.45 and boys of 1.07 percentage points, according to the estimated probit model.

At the secondary school level, the average enrollment effect of the program across the three post-program rounds in the panel sample is an increase of 9.3 percentage points for girls and 5.8 percentage points for boys, from their initial levels of 67 and 73 percent, respectively.[12] In the larger pooled sample the secondary school

[12] Earlier results reported (Schultz, 2000C) suggested that for boys, the program effect on enrollment declined on the later survey rounds, but this appears to have been due to an earlier error in my matching of the grade completed in round 5.

enrollment effects for girls average 7.2 percentage points and for boys 3.5. The selectivity that may be built into the panel sample compared with the more inclusive pooled sample weakens the estimated life cycle effect of the program for boys and girls.

The coefficients on the control variables are only described briefly here; they are reported fully elsewhere (Schultz, 2000c). The estimated effect of one more year of mother s schooling is to increase the probability of primary school enrollment for a daughter by .29 percentage points, and .18 for a son, whereas an added year of schooling of the father is associated with a .17 percentage point higher enrollment probability for a daughter and .26 for a son. Based also on the panel sample, at the junior secondary school level, the impacts are larger, with an additional year of the mother s schooling increasing her daughters probability of being enrolled by 1.5 percentage points and her son by 1.1, while the father s schooling is associated with an increase in his daughter s enrollment of 1.5 percentage points and his son s by 2.0. They are in the anticipated directions of favoring the offspring of the same sex as the parent, but the differences of mother s and father s schooling are never statistically significant at the 5 percent level (Cf. Thomas, 1994).

Distance to secondary school is associated with lower secondary school enrollment, whereas the greater the distance to the Cabecera or to the nearest metropolitan center the higher are enrollment rates, particularly at the secondary school level. Residing in a town that is only 50 kilometers from a metropolitan area, rather than the sample mean of about 100 kilometers, is associated with a secondary school enrollment rate being 5.5 percentage points lower for girls and 6.0 lower for boys. Nearby cities appear to dissuade rural children from enrolling for additional years in school, a challenging regularity for society to take into account as the transportation system improves and small towns become more closely integrated with neighboring cities. The poverty indicator used to target the Progresa transfer payments is associated with a significant reduction in enrollment rates of 0.9 percentage point at the primary level for both boys and girls, and 4.7 percentage points for girls and no significant effect for boys at the secondary level. This difference in the effect of household poverty on secondary school enrollments of boys and girls helps to explain why the Progresa educational grants as a poverty reduction program have increased the secondary school enrollment of girls more than that of boys (Schultz, 1988).

To explore other policies that might encourage schooling, two of the control variables in the probit model for enrollment can be interpreted as capturing the impact of traditional educational policies. First, access to secondary schools could be improved to stimulate greater enrollment. Twelve percent of the sample currently have to travel more than 4 kilometers to a junior secondary school. Building additional schools and staffing them so that these children reside only four kilometers from their junior secondary school is predicted to increase secondary school enrollments by .46 percentage points for girls and by .34 for boys.[13] A second policy constraint incorporated as a control variable in the probit model of enrollment is the teacher-student ratio in the local primary school. Currently about 15 percent of the primary school aged children have a local

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[13] Other studies of education have also estimated the enrollment effect of distance to school has a larger negative impact on enrollment for girls than on boys, particularly at the secondary school level (e.g., Tansel, 1997), an expected pattern if parents are especially reluctant to send teen-age daughters greater distances to school (King and Hill, 1993).

primary school where the average class size is greater than 30. Building enough classrooms and providing the teachers to prevent any school from having an average class size in excess of 30 would, according to the estimated model, raise primary school enrollments by .1 percentage points for both boys and girls. Neither of these traditional education-based supply policy options for increasing enrollment rates appears to be a particularly effective means for raising enrollment rates, and moreover, neither could readily be targeted to the poor as is possible with school subsidies in the Progresa Program.

Table 6 compares the individual child probit-model estimates of the derivatives of the Progresa Program on school enrollment, averaged across the 6 years of primary school and 3 years of junior secondary school (Table 5), and the group-differenced estimates averaged across the grade levels 1 through 9 (Table 3). The probit estimates are based on two alternative samples the panel and the larger pooled samples whereas the group-differenced estimates rely on the panel sample to avoid changes in the composition of groups over time. The probit model adds 10 additional control variables, whereas the group-differenced estimates allow for program effects to differ for every grade, rather than only between primary and secondary school levels as assumed in the probit specification. The estimated program effect on girls enrollments is relatively similar across statistical models, controls, and samples, varying narrowly between 3.4 and 3.7 percentage points. In the case of enrollment probabilities of boys, the three estimates based on the panel sample are also of a similar value, ranging from 2.4 to 2.8 percentage points, but the probit model fit to the larger pooled sample suggests that the Progresa Program had a smaller effect on the enrollment of boys, on the order of 1.9 percentage points on average. However approximated, the Progresa Program has had a significant impact of increasing the school enrollment rates among children in poor rural households, and my preferred estimates are those based on the difference in differences (DD1) which control for persistent unobserved factors that might have influenced the level of enrollments across these localities that were administratively assigned to benefit from the first phase of the Progresa or be followed as controls.

5. Cost-Benefit Analysis of the Progresa Program in Terms of Human Capital

Increases in enrollment rates attributed to the Progresa Program in Table 3 can be cumulated to approximate the lifetime changes in years of enrollment that an average poor youth could expect to receive, if the Progresa Program were to become permanent. This cohort cumulation in enrollments is simulated in the first column of Table 7, based on the enrollment rates for the pre-program periods. These baseline figures imply that if a poor child had been once enrolled in school, and completed the first grade, he or she could expect in the Progresa localities to complete (on average) 6.80 years of school by the end of junior secondary school (out of a possible 9 years). Relatively few children continue further in school without leaving the region. If the D1 post-program enrollment effects from Table 3 are added to the baseline enrollment rates and cumulated for a cohort of children, this synthetic measure of expected years of post-program enrollment increases to 6.95 years, or a gain over the baseline of .15 years of schooling. But conditions deteriorated in this two year period October 1997 to November 1999 (Handa, et al, 2000), and in localities that did not benefit from the Progresa program, the expected cumulative school enrollment of a child fell in the three post-program rounds from 6.66 to 6.14 years of schooling, as shown in Table 7. The cumulative enrollment of the poor is .81 years greater in the Progresa localities (6.95) than in the non-Progresa (control) localities (6.14) in the post-program rounds of the survey, or based on D1 evaluated post-program. According to the difference in differences (DD1), that corrects for persistent pre-program differences in enrollments, the gain in cumulative enrollment of the poor is .66 years, which is accepted here as the preferred (and minimum) estimate of the program s long term effect on child schooling attainment. This DD1 program gain is slightly larger for girls, .72 years of additional schooling, than for boys, .64 years. From their baseline in schooling before the program started, expected educational enrollment through junior secondary school is expected to increase for girls by 11 percent in the Progresa localities, closing the gender differential in schooling among these poor families. Although the social experimental intervention of the Progresa program appears to have been relatively uncorrelated with initial levels (and inequality) of enrollment rates as tested by H_2 (and H_5), the pre-program differences between program and control localities are nonetheless conservatively taken into account in the difference in differences estimator (DD1).

Estimates of the wage structure for men and women in 39 metropolitan areas of Mexico based on a 1996 Survey (Encuesta Nacional de Empleo Urbano) imply that wages for both men and women are approximately 12 percent higher for each year of secondary school they have completed, and these estimates are robust to whether sample selection bias is corrected (Parker, 1999). Matching the rural communities surveyed here to these return estimates in the nearest metropolitan area, one also finds that this pattern of 12 percent private returns to secondary school is a reasonable approximation for what youth in these Progresa Program areas of Mexico could expect to receive if they had resided in their nearest urban labor market. However, the comparable returns to primary schooling are only about 5 percent per year in these same urban areas, revealing an increasingly common pattern in the world in which private returns to secondary schooling are markedly higher than those to primary schooling (Schultz, 1988).

Unfortunately, there are insufficient wage earners in the rural population surveys to estimate a local wage return to schooling in agriculture. Most workers in these rural areas are self-employed farmers. To estimate the effect of education on a farmer s profits, additional data are required which were not available in the Progresa census and surveys. When farm production functions are estimated in the United States or in low-income countries, the implied internal rate of return to the farmer s schooling is generally of a similar magnitude as the returns to schooling for urban wage earners (Jamison and Lau, 1982; Huffman, 2000).[14] However, a large share of the youth from these rural localities currently migrate to the city to find their adult livelihood. The wage gains realized by an average youth who become better educated in these rural areas are likely, therefore, to be larger than those estimated within urban areas, because better educated rural youth are more likely to migrate to an urban destination (Schultz, 1988).

[14] No variation in wages is assumed by age or correction for the growth in nominal wages in the three years 1996 to 1999, when the last Progresa survey is collected. Implicitly the relative wage structure by educational attainment is assumed constant, whereas in reality, the returns have been increasing over time. As noted earlier, no upward adjustment in the returns is made for the probability of rural-urban migration increasing wage opportunities for the rural youth who attend school more years. The net effect of these simplifications in the expected lifetime earnings calculation probably understates the human capital returns to Progresa grants. Any such estimate clearly embodies many sources of uncertainty given the gaps in my information on migration and rural earnings opportunities of youth in these rural regions of Mexico.

The monthly wage for an average worker in the urban areas in 1996 is 1300 pesos, which is used here as the benchmark for wage opportunities in 1999.

One further piece of information is needed to approximate the internal rate of return to the Progresa educational grants this is the share of potential beneficiaries of the program who are qualified to enroll at each schooling level who enroll and receive the Progresa educational grant (Table 1). The surveys provide a post-program estimate of the proportion of poor-eligible children residing in a Progresa locality who are enrolled given that they had completed the requisite number of years of schooling to qualify. Weighting grades 2 to 8 equally, one obtains an average estimate of 87 percent of the eligibles are enrolled who could attend grades 3 through 9 in the three post-program surveys. The proportion of the qualified poor youth who actually receive Progresa payments in the November 1999 survey is 72.5 percent, averaged over grades 3 to 9.

The five pieces of information needed to calculate the internal rate of return to the Progresa Program are now specified. If the Program educational grants (Table 1) are viewed as the investment expenditures of the Progresa Program, which are only paid to 73 percent of the potential beneficiaries, and the impact of these program subsidies is to increase the educational attainment of the cohort of poor youth by .66 years of schooling (Table 7, DD1), for which the youth earn a 12 percent higher wage per year of schooling over their adult lifetimes (age 18 to 65) based on the 1996 urban average wage, then the Progresa educational payments are earning an internal rate of return of about 8 percent per year, in addition to their role of reducing current poverty.

6. Summary and Conclusions

The level of enrollment rates of comparably poor children in Progresa localities (treatment) are higher than in non-Progresa localities (control) in the three survey rounds collected after September 1998 when the Progresa program began dispensing educational grants to poor mothers whose children were enrolled in school in grades 3 through 9 and attended school at least 85 percent of the time. This difference estimator of Progresa s impact on the enrollments of the poor is reported in Table 3 (D1 > 0, post-program). It is statistically significantly different from zero within each distinguished group of children who had completed grades 1 through 6 in the previous year. These differences are sometimes larger for girls than boys, as seen in Table 3. To confirm the independence of the placement of the Progresa Program from enrollments and the definition of eligibility, the regional differences in enrollment by the poor are also calculated before the program started and shown not to differ significantly from zero (Table 3, D1 = 0, pre-program). Difference in differences over time confirm a slightly smaller program impact on enrollment as evident in the difference in differences (DD1) estimator (Table 6) which is plotted in Figures 3 and 4. The cumulative cohort effect of the program based on the difference in differences estimator is an increment of .66 years on the baseline level of 6.80 years of schooling which is recorded in the pre-program rounds of the surveys, for both sexes combined.

The Progresa program targets its educational subsidies to the very poor, even in the geographically restricted, relatively immobile, poor rural communities. It is also shown that these targeted transfer payments have the effect of reducing the inequality in school enrollments within the Progresa localities compared with that in the non-Progresa localities (Table 4, D2 < 0 post-program), and they are statistically significant after grades 4 through 6. The pre-program inequality differences between the Progresa and non-Progresa localities are not jointly statistically significant. The estimated difference in differences in enrollment inequality over time (Table 4, DD2) is negative and statistically significant after grade 6 , and slightly larger overall than the D2 measured post-program.

One way to assess whether a roughly two-thirds of a year increment in schooling is worth the cost of the Progresa Program is to compare the expected program payments to the resulting expected increase in adult productivity of the students who stand to benefit from a permanently established Progresa Program. If the current urban wage differentials approximate what the Program beneficiaries can expect to earn from their schooling in terms of future percentage increases in their wages, a rough estimate of the internal rate of return to the educational transfers provided by the program is 8 percent per year in real terms (adjusted for inflation). This would appear to be a reasonable rate of return if the program were designed only to foster human capital investments. But it is clearly more than this, since it is channeled to the poor and operates to reduce current poverty and raise current consumption levels. For the majority of the poor rural families whose children would have attended school without the program s educational grants, the Progresa outlays are pure income transfers. But for the roughly one in twenty who are induced to enroll their child in school, they may experience a decrease in their children s supply of labor to work in the labor force or in household production. But as described in the appendix, although there is such a child labor supply response associated with the family being eligible for Progresa educational grants, the magnitude of the response is modest and cannot eliminate the consumption gains of the program to poor families (Cf. Ravillion and Wodon, 2000).

Another possible side effect of the Progresa Program could be on fertility, for the educational grants would appear to subsidize parents for the cost of a child s schooling, which might reduce the private cost of an additional child. Other studies that have sought to estimate the effect of a reduction in the cost of schooling on fertility have found that the income uncompensated cross-price effect is negative and outweighs the associated (positive) income effect of this reduction in the price of schooling, leading to the empirical conclusion that children and child schooling appear to be substitutes (Rosenzweig and Wolpin, 1980, 1982). In the Mexican panel sample analyzed here, there is also no statistical evidence that poor women who had a Progresa-eligible child who had completed grades 2 through 8 were more likely to have a birth in the six months preceding the November 1999 survey than comparable women residing in a non-Progresa locality.[15]

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[15] To evaluate the possible effect of the Progresa Program on fertility, the final survey round collected in November 1999 is analyzed, and the probability of having a birth between this round and the previous one in May 1999 is estimated in a probit specification corrected for cluster design as a function of the woman s age, years of schooling, being designated poor (and eligible for program grants if resident in a Progresa locality), whether resident in a Progresa locality, and the interaction of poor and Progresa. The last two variables (Progresa and Progresa-Poor interaction) are also added only for those women who have a child who is eligible for Progresa educational grants, having completed in the previous school year grades 1 through 8. The coefficients on these last two variables are reported in Table A-3, and their sum is viewed as an estimate of the Progresa Program s effect on fertility. This program effect is estimated for all women age 20 to 49 with the additional control for the woman s age squared, and for the five-year age brackets 20-24, 25-29, etc., with only the linear age control variable. It may be noted that no mothers age 15-19 had children of school age and thus the program is presumed to have had no estimable effect on the fertility of this youngest group of women who could bear children. For all women, the derivative of fertility with respect to the placement of the Progresa program is negative for women age 35-39 and 40-44, and approaches significance at the 10 percent level. For the eligible mothers, there is a statistically significant effect only for the age group 20-24 where the effect is positive, but collinearity prevents estimation of the two interaction variable coefficients jointly. In the available time frame, I would conclude that there are no consistent and statistically significant effects of the program on fertility.

Other traditional poverty reduction programs, such as income-support welfare systems or price-support agricultural programs as were funded by the previous Mexican poverty alleviation programs, offer no empirical evidence or theoretical reason to expect that these programs encourage investments in human capital or promote a more efficient allocation of private or social resource. Indeed both of these common forms of poverty programs – supporting incomes and output prices -- are related to major distortions in the allocation of the family s labor and other resources of the beneficiaries. These types of resource distortion are minimized by the design of Progresa (to leave only pure income effects).

Although it is not always a politically popular feature of a welfare program focused on poor areas, Progresa should help the children of poor Mexican farmers find a better place to work, by encouraging them to invest in schooling, which in turn facilitates the migration of these young people from their origin communities to other parts of the Mexican economy where wages and long-term career opportunities are more attractive. Thus, it is expected that Progresa will encourage the interregional migration that is needed at the macro-economic level to ease the extreme poverty that has persisted for generations in the more remote rural parts of Central and Southern Mexico. Subsidizing schooling among the rural poor may be a development strategy that deserves more attention as a targeted policy in many parts of the world that can both reduce entrenched intergenerational transmission of poverty and promote long-term economic growth.

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Appendix Describing the Effects of Progresa on Related Family Demand Behavior

The Progresa Program by reducing the price of schooling for children in poor families may affect the demand of these families for a variety of related goods and consumer behavior. According to the Slutsky decomposition, the effect of the program can be thought of as having a pure income effect that should raise the demand for all normal goods, and a cross-price effect that should reduce the demand for substitutes and increase demand for complements of the child s schooling. Because of the child s time constraint, it might be expected that a program that caused an increase in school enrollment would be associated with an offsetting decrease in child labor supply to work in the labor market and in the home. Indeed, some advocates of policies to reduce child labor hold out the promise that decreasing child labor would lead to increasing the schooling of the same children and thereby improve the child s future economic opportunities (i.e. that the income uncompensated effect of a ban on child labor would be to increase schooling). This claim, however, not only assumes substitution between child schooling and work (i.e. a positive cross-price effect) but also anticipates that the cross-price effect dominates the income effect that would in this case operate in the opposite direction and reduce the demand for schooling and other normal goods.

Measuring child labor may be more difficult than it would seem. Household surveys in low income countries do not generally find as large a proportion of children working as expected by social observers. The Progresa census and surveys asked the respondent in all five rounds whether a child age 8 to 18 worked. A second question followed up those who reported the child as not working by a further line of inquiry as to whether the child produced something that was sold in the market. A third question was addressed in round 3 to respondents who had answered negatively the two previous questions, asking whether the child was engaged in any housework. Finally, for children working in paid labor they were asked how many hours they worked last week. The full sequence of questions were administered only in round 3 and 5 on which this analysis is based.[16] Summing the children reported as working in the first two questions yields 2.5 percent of the girls prepared for primary school as working, and 7.6 percent of the comparable boys as working (Cf. Sample means in Table A-2, and for ages see Tables 2 and A-2). Of the secondary school qualified girls, 8.3 percent work, whereas 28.4 percent of the boys work. The number of hours worked for pay per week is .09 and .36 for primary school girls and boys, whereas the average hours rises to .48 and 1.76 for the secondary school girls and boys, respectively. Except for teen-age males the labor force participation rates appear relatively low.

However, adding to the participation rate those children who contribute to housework, the primary school girls participation rate rises to 12.5 percent and the participation of secondary school girls increases to 33.2 percent, roughly equivalent to that of boys. Clearly, housework is a relatively more common activity for girls than boys, and conversely for boys in market labor force participation.

[16] In round 3 the three questions were 59, 60, and 62, whereas in round 5 they were respectively number 50, 51, and 53. In rounds 1 and 2 the third question on housework was omitted and the hours worked in paid activity was omitted from round 4.

The estimates of the probit model are reported in Table A-2 to explain the probability that a child participates in (A) the labor force or housework and (B) in only the labor force , whereas a regression is fit in (C) to hours worked for pay last week. In comparison with poor children in non-Progresa localities (controls), the Progresa Program s effect is the sum of the Progresa resident coefficient plus the coefficient on the interaction of the Progresa and Poor dummies that makes the family eligible for the educational grants if the child is regularly enrolled. This program effect expressed as a derivative suggests that only the secondary school boys respond to Progresa by significantly ($p < .05$) reducing their labor force participation (B) by 3.6 percent. When the housework is also included in participation (A) the primary males exhibit a significant response of reducing participation by 1.6 percent, the secondary females reduce their participation by 4.1 percent, whereas the secondary males reduce their participation by 3.4 percent, though the significance of this last estimate does not satisfy the conventional 5 percent threshold. The child s hours of paid work also tend to be reduced in the Progresa-Eligible families, but the amounts are small: .04 hours per week for primary females and .11 hours for primary males, by .21 hours for secondary males, whereas the response is not statistically significant for secondary females.

Although the responses in child labor supply are in the direction expected, they are smaller in magnitude than the enrollment increases. Others have found the same pattern (de Gomez and Parker, 1999, 2000). According to Tables 5 and A-2 the Program induced increase in secondary school enrollment of 9.3 percentage points for girls is associated with a 4.1 percent decline in girls working in the labor force or in housework, but by only .4 percent when housework is excluded. Secondary school males increase their total labor force participation by 3.6 percentage points while they increase their enrollment rate by 5.8 percentage points (Table 5). If the reduction in hours of work by the secondary school males occurs entirely among the 6.9 percent who enrolled due to the Progresa Program grants, it is possible that these boys reduced their paid work by 3.0 hours per week (i.e. 3.0 = .21/.0694). These findings from a randomized social experiment imply, as the non-experimental cross sectional estimates of Ravillion and Wodon (2000) suggest, that the enrollment effect of secondary school fellowships (or Progresa grants) reduced by only a small fraction the time Bangladesh (Mexican) children worked. Duraisamy (2000) also finds that a large fraction of Indian teenage children are neither in the labor force nor enrolled in school, suggesting that increasing enrollments need not depress proportionately child work or reduce substantially family earnings. These estimates suggest that most of the educational grants transferred to the poor families by Progresa increase the family s resources and are only marginally offset by a reduction in child labor.

Table A-3 reports a similar analysis of fertility behavior between the fourth and fifth surveys. There is no evidence in this six month rate of births to suggest that the uncompensated schooling price reduction introduced by Progresa had any significant effect on fertility. This finding is also consistent with other studies that have sought to estimate the uncompensated cross-price effect from schooling to fertility or the effect of random variation in fertility on schooling as instrumented by the occurrence of twins (Rosenzweig and Wolpin, 1980, 1982, Schultz, 1997).

Table 1
Monthly Payments for Progresa Program Eligible Families for Children who attend at least 85 Percent of Days[a]

Educational Levels of Students Eligible for Payments		July - December 1998[b]
Primary School - both sexes		
3rd Year		70
4th Year		80
5th Year		105
6th Year		135
Secondary School		
1st Year	Males	200
	Females	210
2nd Year	Males	210
	Females	235
3rd Year	Males	225
	Females	255

Source: Progresa Staff

[a] Excluding those days for which medical or parent excuses were obtained, accumulated over the last two months.

[b] Corresponds to school year first-term, September to December, 1998.

SCHOOL SUBSIDIES FOR THE POOR: EVALUATING A MEXICAN STRATEGY FOR REDUCING POVERTY

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ABSTRACT

This paper assesses how the Progresa Program has affected the school enrollment of Mexican youth in the first 15 months of its operation. Progresa provides poor mothers in poor rural communities with education grants, if their children attend school regularly. Enrollment rates are compared between groups of poor children who reside in communities randomly selected to participate in the initial phase of the Progresa program and those who reside in other comparably poor (control) communities. Pre-program comparisons document how well the randomized design is implemented, and double-differenced estimators are reported over time within this panel of children. Probit models are then estimated for the probability that an individual child is enrolled, which statistically controls for additional characteristics of the child, their parents, local schools, and community, and for samples of different compositions, to evaluate the sensitivity of the estimated program effects to these variations. If the current relationship of the program outlays to enrollments, and that of schooling to increased adult earnings, both persist in the future, the internal rate of return to the Progresa educational grants as an investment is estimated to be about 8 percent, which accrues in addition to the program's efficacy as a poverty reduction program.

Poverty alleviation programs have taken a variety of forms. Social welfare programs provide transfers in cash and kind to people with incomes and assets below a specified level in most high-income countries. These transfer programs may also impose additional conditions and limitations, such as in the United States, where “welfare” payments are provided primarily to lone mothers with dependent children. These conditional transfer programs are likely to distort the allocation of private economic resources and thereby reduce the efficiency of the economy. In particular, means-tested poverty programs are expected to reduce the time beneficiaries work in the paid labor force, because the earnings from paid work of beneficiaries is taxed at a higher rate than is the earnings of non-beneficiaries. Other distortions in behavior are also attributed to these programs, although the evidence is more controversial. For example, in the United States, those states which provide more generous welfare payments also report on average less frequent marriage and more non-marital childbearing (e.g. Schultz, 1994; Rosenzweig, 1999), which could be attributed to these programs which support only mothers without a resident husband.

In populations engaged in agriculture, poverty alleviation programs have often taken the form of setting minimum prices for farm outputs or paying subsidies for farm inputs. Two frequently heard justifications for these programs are, first, farm incomes are on average lower than non-farm incomes, and second, farm prices are volatile causing farm incomes, before transfers and taxes, to vary more over time than non-farm incomes. But in contrast to means-tested income supports in welfare programs, agricultural price supports are generally not specifically targeted to poor families, but only to a relatively poor occupation or industry. Farm price supports also contribute to an inefficient allocation of resources by encouraging the production of outputs of less economic value than would otherwise occur, including the allocation of more labor and capital to the production of price-supported commodities and a corresponding reduction in the rate of net migration out of agriculture. As a result, consumers usually pay a higher price for farm products, and states subsidize agricultural exports and tax agricultural imports.

In neither the income supplement nor output-price support program is there an expectation that beneficiaries will invest more in the acquisition of skills or the accumulation of capital to boost their future productivity and consumption, and thus diminish their dependence on transfers from the state. Indeed, one consequence of both of these types of poverty programs is to erode the workers' incentive to change their sector of employment, or accumulate new types of productive job experience, or generally invest in human capital. The growing appreciation of the cumulative lifetime career costs of these distortions strengthened the dissatisfaction in the United States with its Aid for Families with Dependent Children (AFDC) program, which led to the redesign of this program in 1996 to include a lifetime limitation of not more than five-year of transfers, and to the funding of coordinated child care and job training programs to encourage poor mothers to become self supporting.

The Progresa Program in Mexico, which is examined in this paper, takes a different approach to poverty reduction by subsidizing the investment of poor families in their children's human capital. It provides means-tested transfers to poor rural mothers, whose children are enrolled in school from grades 3 through 9. The program seeks to reduce the current level of poverty in Mexico and to increase the schooling, and thereby the future productivity, of children from poor rural families. This anti-poverty program started paying grants to eligible beneficiaries in September 1998, and by the start of 2000 the program had enlisted two million families in Mexico, or about one-tenth of the entire Mexican population. The government implemented the Program as a phased social experiment, collecting sufficient background and follow-up survey information to facilitate cost-benefit evaluations of the Program. Although the Progresa program may reduce labor supply of these poor families, to the extent that it increases the incomes of poor mothers, it does not impose a further tax on earnings that specifically deters work in the market economy. The distortion in resource allocation caused by the Progresa program is thus minimized to what might be called a pure "income effect" associated with achieving the program objective of poverty reduction. But it does not in addition distort relative prices or returns from work, marital status, or resource allocation in general by its conditions for eligibility.

One change in resource allocation expected from the Progresa Program is an increase in the school enrollment of poor children. The objective of this paper is to estimate the enrollment impact of the program on children from poor households (i.e. who are administratively designated as eligible for program benefits) who live in poor rural communities that were randomly selected to participate in the first phase of the Progresa Program (e.g. the treatment). The program impact is inferred from comparisons with poor children from randomly selected communities (e.g. the control) which did not benefit from the first expansion of the Progresa Program. Both the "treatment" and "control" populations were surveyed twice in the year before the Program was announced, and followed for two years after the Program commenced, providing a total of five survey cycles for this study. There are about 200,000 people in 495 poor rural communities originally included in a background Census in October 1997, of which almost two-thirds of the communities were designated as program “treatment” areas in September 1998, and in these areas about two-thirds of the households were judged eligible for program benefits or sufficiently “poor” according to a combination of initial household census and community indicators of consumption, wealth, and income.

The design of the Progresa Program, and the initial patterns of enrollment are reviewed in section 1. A framework for studying enrollment decisions is outlined in section 2. Estimates of the difference between treatment and control groups are explored in section 3, whereas the probability of enrollment for the individual child is estimated in section 4. Section 5 combines information on the estimated Program enrollment effects, the outlays on educational subsidies, and the wage structure to construct the internal rate of return realized within the program, assuming its only goal is to invest efficiently in educational human capital. It is, of course, more than that, for it also seeks to alleviate current problems of low levels of consumption among a major group of Mexico’s poorest people.

1. Program Mechanism, Enrollment Pattern, and Evaluation

In a low income rural community in Mexico that is designated as participating in the Progresa Program, mothers living in an administratively determined "poor" household whose child enrolled in grades 3 through 9 could receive from the central government a check every two months for the amount indicated in Table 1.[1] The size of this educational grant increases fourfold from 3rd to 9th grade, and they are slightly larger for girls than boys at the junior secondary school level, based on the concern that enrollment rates of girls lag about six percentage points behind boys at this level (Cf. Appendix Table A-1). To assess the relative magnitude of these educational grants, it is useful to note that a daughter enrolled in the 9th grade might qualify a family to receive a grant of 255 pesos per month, which amounts to 44 percent of the typical male day-laborer's wage in these agricultural communities.[2]

The likelihood that a child is enrolled in school is related to the child's age and years of schooling completed at the end of the prior school year, as illustrated in Table 2. Beneath the number of children enumerated in the October 1997 census of households and March 1998 survey is the enrollment rate for children in each age-education cell.

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[1] These amounts have increased every six months to adjust for inflation figures reported by the Bank of Mexico (Coady and Djebbari, 1999). There are additional supports (cash and kind) for school materials of 120 pesos per year at the primary level and 240 pesos per year at the secondary level, provided on a term by term basis (Progresa, 1999).

[2] The daily wage for male agricultural labor reported in the 1998 and 1999 Community Surveys averaged 29 pesos, and the worker is assumed to work 20 days a month to yield a monthly wage of 580 pesos. There is an additional monthly transfer per family of 90 pesos, if the mother's family receives recommended periodic health exams, which can lead to further health care, including nutritional supplements and immunizations for children as well as pregnant and lactating women (Coady and Djebbari, 1999; PROGRESA, 1999).

Comparing in Table 2 the marginal column on the right that represents the age-specific enrollment rates, and the bottom row that represents the grade-completed enrollment rates, it is evident that enrollment rates are more sensitive to grade than age. The primary school enrollment rate among children who had completed grades 1 through 5 is about 95 percent, and recovers to 97 percent after a child completes the first year of junior secondary school or 7th grade. In the transition year from elementary to junior secondary school, however, the enrollment rate falls to 58 percent, after completing 6th grade, and drops again to 63 percent in the first year of senior secondary school. This analysis focuses, therefore, on enrollment rates within groups of children stratified by the number of grades they have completed. This partitioning of the sample also facilitates estimation of program effects, for a child to qualify for a Progresa educational grant they must have completed the 2nd to 8th grade and be currently enrolled.

Two samples are analyzed from the base census and follow-up surveys: a balanced "panel" and a "pooled" sample. The panel sample includes all children age 5 to 16 observed in the October 1997 household census, who completed the age, schooling, and enrollment questions, for whom the schooling of co-resident parents is reported, and the locality is matched to other community information files. The panel sample is further restricted to include only those children who could be followed and matched in the subsequent pre-program survey round in March 1998, and then in three surveys in October 1998, May 1999, and November 1999, which occurred after the Progresa Program had started to provide education grants.

The second larger pooled sample includes all children age 5 to 18 who are observed at least once and can be linked to sufficient household data to estimate the basic enrollment model.[3] When a specific variable is not reported by the household or missing for one of the 495 localities, a dummy variable is included to reflect the missing status of this variable, to avoid selecting the estimation sample on the basis of reporting characteristics. The working sample includes 314 localities where Progresa began to operate after the summer of 1998 and 181 non-Progresa (control) localities. These localities span the poorest regions of seven states of Mexico: Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi, Veracruz, and Guerrero.[4] The number of children age 5 to 16 enumerated in the first census is 40,959, and the number for which all five survey observations are matched is 19,716. Appendix Table A-1 reports the mean and standard deviation of the variables considered in the analysis for the panel and pooled samples, divided by males and females, separately for primary and secondary school levels.

Each locality has its own primary school, and its teacher-to-student ratio is examined as an crude indicator of school quality. Only about a quarter of the localities has its own junior secondary school, and thus the distance from the locality to the nearest such school is considered as an indicator of the time costs that a child and family could take into

[3] In the fifth round of the survey, information was collected on resident children up to age 18, rather than only to age 16 as in the previous four survey rounds. These observations on older children are retained in the analysis, and additional age dummies are added for age 17 or 18 in the estimates of probit models for enrollment probabilities. Because of the 6 to 16 age limitation in rounds 2 through 4, the sample of children in the highest grade group, 9 or more, and probably also the lowest grade group, with no years of schooling completed, are truncated and may therefore be unrepresentative of the entire population, particularly in the panel sample. The differenced estimators should therefore be approached with caution as they relate to the groups of children with zero and 9 or more grades of school completed.

[4] The procedure used in the program to select a locality as a Progresa program areas or not, and the procedures used to designate a household as poor are described and analyzed elsewhere (Skoufias, et al. 1999; Behrman and Todd, 1999).

account in determining whether to enroll in junior secondary school.[5] Finally, two variables are included to capture the remoteness of the community from an urban labor market: the road distance to (a) the Cabeceras or the municipal administrative center (sample mean 10 km), and to (b) the nearest of the 39 largest metropolitan areas in Mexico (sample mean 104 km). Workers in urban labor markets tend to receive higher wages, and perhaps a larger wage premium for schooling. Greater distances should translate therefore into poorer local job opportunities and lower opportunity costs of the time of school-age children, but on the other hand, larger distances to urban areas would raise the costs of migration to these markets and probably reduce the information available locally about the higher educational returns in the urban labor force. In Latin America, as elsewhere, better educated youth are more likely to migrate from their rural birthplace to an urban area, once they reach maturity (Schultz, 1988).

The years of schooling completed by the mother and father are also treated as likely determinants of a child's school enrollment probability.[6] Information on family income is not directly included as a control variable, because household monetized income is influenced

[5] The characteristics of the secondary school cannot be matched because some students traveled to schools that were not designated as nearest, and thus were not included in the database on schools. Thus, some students traveled longer distances to reach a preferred junior secondary school than is attributed to them according to the available government data.

[6] Two dummy variables are also included to indicate if the parental education information is not available because the mother or father is not enumerated in the household. This procedure controls for the effect of lone parents, although I would prefer to deal with this variation in household composition as another jointly determined aspect of the coping strategies of women and their families. Exclusion of children without a father in the household would reduce the size of the child panel sample by about 12 percent and exclusion of those without a mother of the child would have reduced the sample by 5 percent (Table A-1). Thus, elimination of this source of variation by excluding all but intact parental couples could have introduced substantial sample selection bias and potential parameter bias in the subsequent estimation of program effects.

by the labor force behavior of the mother and other family members, including the children themselves.[7] The pre-program realized income would therefore be behaviorally interrelated with the family's school enrollment decisions and the net relationship would not provide an unbiased estimate of the one-way causal effect of income on enrollment. Measures of post-program earned income could be expected to respond further to the program's effect on enrollment itself. Indeed, later the response of child labor to the Progresa experiment is explicitly considered. However, a latent variable index for household economic well-being is constructed from the 1997 regional census, from information on household consumption, assets and income. Because the Progresa Program used this index to determine a binary indicator of whether the household is sufficiently "poor" to be eligible for program benefits, this eligibility indicator, E, is treated as an exogenous conditioning variable for the enrollment decision in both the subsequent analysis of enrollment differences at the group level, and the enrollment probabilities estimated at the individual child level.

2. A Framework for Analysis of School Enrollment Decisions

The framework helps to connect the subsequent analyses, first at the group level and then at the individual child level, suggesting individual, family, community, school, and program administrative variables which might influence the likelihood that a specific child or a group of children enroll in school.[8] Let the probability of being enrolled in school for the i

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[7] Preliminary analyses of family labor supply responses to the Progresa Program suggest small effects. Some reduction is found for child labor, offset by small increases in male adult labor supply, and little change in female adult labor force participation (Gomez de Leon and Parker, 1999, 2000).

[8] Virtually all of the reported variation in school attendance is accounted for by the variation in enrollment that is analyzed here. Elsewhere I describe the role of the same explanatory factors to account for the variation in attendance rates among the children who report being enrolled and answering the attendance question (Schultz, 2000b). See Appendix Table A-1 for the magnitudes of attendance for the responding sample.

th child at the time of a survey be denoted as S_i. This likelihood of enrollment is affected by family demand for schooling, which may respond to such factors as school quality and access, the opportunity cost of the student's time minus enrollment subsidies provided after the start of Progresa, by household endowments and parent education, and a host of unobserved factors, such as those affecting the local labor market wage returns to schooling, and the family's own preferences for schooling. If the unobserved determinants of enrollment combined with various specification and stochastic errors create a normally distributed disturbance that is unrelated to the observed variables used to explain enrollment behavior, the probit model is a candidate to describe the enrollment decision process, and its parameters can be estimated by maximum likelihood methods. The standard errors of these probit estimates are adjusted for the clustering at the locality level of the explanatory variables representing the program, school and other community characteristics, which is analogous to the White adjustment for heteroscedasticity. [9]

A linear approximation of the estimated enrollment model can be expressed as follows:

$$S_i = a_0 + a_1 P_i + a_2 E_i + a_3 P_i E_i + \sum_{k=1}^{K} g_{ki} \mathrm{C}_{ki} + \sum_{k=1}^{K} b_j \mathrm{X}_{ji} + \mathrm{e}_i , \qquad i = 1,2 \ldots, n \quad (1)$$

where i indexes the child, n represents the total number of children in the cross sectional

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[9] The probit models were also estimated assuming that random errors differed in their variances across families and this source of heteroscedasticity was thus shared by siblings, without modifying any of the basic findings discussed here. The Huber (1967) -White adjustment of the estimates for community cluster (n=495) effects increased modestly the standard errors, which are used here to calculate the reported absolute values of the asymptotic t ratios.

survey, and the explanatory variables and the interpretation of their linearized effects on enrollments are discussed below, i.e. derivatives of the probit function evaluated at sample means.

First, there may be an effect on enrollments, a_1, associated with residing in a Progresa locality, P_i =1 (otherwise zero), although the random assignment of the community locations for the Progresa Program is designed to minimize any such difference before the program informed the community of who would benefit from the Program. There may also be an effect, a_2, of being designated as a child from a poor household, E_i = 1 (otherwise zero), who would be eligible for Progresa benefits when the transfer payments are initiated, if the family resides in a Progresa locality. One common hypothesis is that credit constraints limit the investment of the poor in their children's education, suggesting that a_2 would be negative. An interaction binary variable defined as the product of the Progresa and poor variables, $P_i E_i$, would then exert an additional effect on enrollment denoted a_3, which should be approximately zero until the Program transfer payments are announced, and thereafter it is expected to be positive. Having controlled for the two-way interaction effect, the direct effect of the Progresa Program for those who are not eligible for the educational grants, or a_1, might be small even after the program has started, possibly capturing "spillover effects" between poor and rich families in Progresa-served communities and errors in program administration. Enrollment rates vary across grades in a school system (Cf. Table 2), and thus a control is needed for the grade level to which the child would be qualified to enroll. The variable C_k is defined as 1 if the child has completed precisely k years of school, k= 0, 1, ..., 8, 9 or more, which would qualify the child to enroll in the k+1 grade, and the coefficients on these dummy variables , $?_k$, adjust for linear differences in enrollment by grade level.[10]

With the passage of time, some variables that explain the probability of enrollment in equation (1) may change, such as C which would change if a child completes one grade of schooling and is thus qualified to enroll in the next. The net effect of all unobserved variables that change over time is partially captured in the probit model by allowing a shift in the estimated intercept specific to each time period or survey cycle. In other words , a $_{0t}$ is allowed to vary in each round of the survey, where t = 1,2,3,4,5. Because Progresa grants only started in September 1998, and the program effects on enrollments represented by the coefficients on P and PE are estimated as an additional set of interaction effects for the post-program periods in October 1998, May 1999 and November 1999 (t = 3, 4 and 5, respectively), and the estimated post-program effects are distinguished by asterisks in the enrollment equation (2) that combines all five survey cross sections:

$$S_{it} = \sum_{t=1}^{5} a_{ot} + a_1 P_i + a_2 E_i + a_3 (P_i E_i) + \sum_{k=1}^{K} g_k C_{kit}$$

$$+ \sum_{t=3}^{4} (a^*_{1t} P_i + a^*_{3t} (P_i E_i)) + \sum_{j=1}^{J} b_j X_{jit} + e_{it} \qquad (2)$$

Equation (2) is estimated separately for boys and girls, because the probit parameters

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[10] A three-way interaction effect between P, E, and C_k for the years when the program offers an educational grant for students in grades, k = 2, 3, ... , 8, was also introduced to demarcate the targeted range of educational subsidies, but they are were not precisely defined by the available data and are not reported. Cf. Schultz 2000c.

differ significantly by gender, particularly at the secondary school level. Given the relatively high level of enrollment at the primary level and the sharp decline in enrollment at the transition to the secondary level, the two levels are estimated separately. The primary sample is defined as all children age 6 to 16 who report $C_{kt} = 1$, for k=0,1,2,...6, indicating that they have not yet completed primary school, and the secondary sample is defined as all children age 6 to 18 who report $C_{kt} = 1$ for k=7,8,9 or more. It is assumed that Progresa's effect on enrollment is uniform by school level across grades by gender, when the probit model for equation (2) is estimated at the individual level, but the effect is allowed to vary by grade level in the group-differences.

If the J control variables, X , were uncorrelated in each time period with the program designated localities, P, and the eligibility of the poor, E , the program effect on enrollment could be obtained directly by stratifying the population by E and P and observing the incremental effect of P and PE in the periods after the program started to make educational awards. Figure 1 illustrates the implied four way stratification of the population of children for the purposes of calculating an enrollment rate, S_{gt} , g = 1,2,3,4. The Program effect in the post-program periods represents the Program's impact on the enrollment of poor children in school, which are stratified by grade completed. The first hypothesis tested by the "differenced estimator" of the Program level effects according to Figure 2 is as follows:

H_1 $\quad D1 = (S_{1t} - S_{2t}) > 0$ $\quad$ Post-program period average, t = 3,4,5 .

One way to investigate whether the P and E are randomized is to determine if the pre-program differences in enrollment rates between the poor children in Progresa and non-Progresa localities are in fact statistically not different from zero:

H_2 $D1 = (S_{1t} - S_{2t}) = 0$ Pre-program period average, $t = 1,2$.

Even if the Program placement were random, statistical correlation between program designated areas and pre-program enrollments might exist fortuitously. If the pre-program regional differences between eligible Progresa and control children were due to omitted variables that do not change over time in their impact on enrollment, the baseline pre-program differences in enrollments may be subtracted from that for the same children observed in the panel sample in post-program periods, and thus the difference in differences estimator (DD1) is defined as shown in Figure 2, which is expected to represent the positive impact of the program holding constant for persistent sources of pre-program regional variation:

H_3 DD1 = D1(post-program) - D1(pre-program) > 0

Program transfers are only available to children of poor households, and this targeting of the program is expected to affect the distribution of enrollment by income levels within the Progresa localities. The enrollment rate difference between non-poor and poor households is expected to be positive before the program, and to decrease relative to that observed in non-Progresa localities after the program is initiated. One possible measure of the Program's effect on inequality in enrollment is defined in Figure 2:

H_4 $D2 < 0$ Post-program period average, $t = 3,4,5$

But before the program started the two types of localities are expected, under random assignment of the programs, to exhibit the same degree of income inequality in enrollments, and this null hypothesis of random program placement is again testable :

H_5 $D2 = 0$ Pre-program period average, $t = 1,2$.

A difference in differences estimator (DD2 defined in Figure 2) can again remove any time invariant sources of the preprogram regional variations in inequality, given the linear approximation postulated here:

H_6 DD2 < 0 .

Even if the randomization of program placement is not challenged, and H_2 and H_5 cannot be rejected, the difference in differences estimators are preferred to the post-program differences, because they remove persistent sources of regional variation in enrollment that might be expected to exist to some degree. It may still be useful to add additional explicit control variables and estimate their marginal effects jointly with those of the program on the enrollment of poor children, because this should increase the statistical power of the model estimated at the level of the individual child to isolate significant effects attributable to the program treatment, if there are any (Manning, et al. 1982). The estimated impact of the controls can also help to evaluate alternative policy options that might contribute to the social objective of increasing enrollment rates, particularly among the poor.

3. Enrollment Differences Between Progresa and Non-Progresa Localities

Table 3 reports the values of D1 for each grade level in the pre-program and post-program periods as well as the difference in differences over time or DD1, first for both sexes combined, and then for girls and boys separately. Beneath the difference in enrollment rates between the Progresa and non-Progresa localities, the statistical probability is reported (in parentheses) that the observed difference could have occurred randomly in a saturated probit model. If the conventional level of confidence required to accept the hypothesis is 5 percent or less, the D1 in the post-program surveys is significantly non-zero and positive after the 1st through 6th grades for both sexes combined.

The largest difference in enrollment is for those children who had completed grade 6, and were thus qualified to enroll in junior secondary school; for this group the enrollment rate increases by 11.1 percentage points, from the level of 58 percent noted in the pre-program periods in Table 2, to about 69 percent. Note also that this program impact is disproportionately concentrated among girls, whose enrollment rate increases 14.8 percentage points compared with the boys whose enrollment increases 6.5 percentage points.

The pre-program values of D1 are positive in seven out of ten cases, but in none of these cases is the difference statistically different from zero, suggesting that the randomization of program placement with regard to prior enrollment levels as specified by hypothesis 2 is not rejected. Nonetheless, the difference in differences (DD1) estimate of the program's impact on enrollment rates is reported in the last three columns in Table 3, and they are also all positive from grade 1 to 8, and statistically significant for the groups having completed grade 4 and 6. The average value of D1 and DD1 over the grades 1 through 8 are of roughly similar magnitudes for both sexes combined, 3.6 and 3.4 percentage point increases in enrollment levels, respectively.

Table 4 reports D2 from the pre-program and post-program periods and the DD2 over time to assess whether the Progresa Program reduced inequality within localities between enrollments of non-poor and poor. Since this measure is only one of many that might be devised to represent inequality, it is not a unique measure of program impact as in the case of the level effects.[11] The D2 differences are negative from grade 1 to 6 in the

[11] For example one might be interested in how schooling gaps between children whose parents are better and worse educated changed with the onset of the Progresa Program, rather than measuring inequality

post-program period and statistically significant and negative after grades 4 through 6, implying the program reduces inequality, but the impact is largest after the last three years of primary school. The pre-program values of D2 are not jointly statistically significantly different from zero, but it is different for grade 6, and in this case it is surprisingly positive. The difference in differences, DD2, is negative from grade 1 to 8, and is statistically significant for grade 6. The average values for DD2 for grades 1 through 8 are larger in negative value than those of D2 post-program, !3.1 percentage points compared with !2.6 , respectively. There is evidence that the program has reduced income related inequalities in enrollment within localities, as would be expected based on the evidence presented earlier that the program induced an increase in the level of enrollments among the poorer households.

4. Response of Enrollment Probabilities to Program and Control Variables

Maximum likelihood estimates of the probit model for enrollment of the individual child are expressed as derivatives of enrollment with respect to the explanatory variables. The two Program associated enrollment effects on the poor are associated with living in a Progresa (P) locality and that of the Progresa-Eligible interaction (PE) as reported in rows 1 and 2 in Table 5, and summed to represent the net effect averaged across the three post-program survey rounds, 3, 4, and 5. This net effect of the program is estimated separately for girls and boys, at the primary and secondary school levels, first for the panel sample which underlies the previously reported group-difference estimators, and also for the larger pooled sample of children. The Probit model additionally controls for the child's age,

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with respect to the single threshold of the latent indicator of poverty defined as a condition of eligibility for Progresa transfers.

mother's and father's years of schooling, primary school teacher-student ratio, distance to junior secondary school, and distances from the locality to urban areas (Schultz, 2000c). In brackets beneath the program net impact on the poor's enrollment probability is the statistical probability that this net impact of the program is zero, according to a joint $?^2$ test associated with the likelihood ratio. The program's net impact on enrollment is statistically significant at the 1 percent level in 7 out of the 8 possible tests for different sexes, school levels, and samples, and in the eighth sample it satisfies the test at the 5 percent level. Thus, there appears to be a general positive enrollment effect of the program in the post-program surveys for both genders, both samples, and both school levels, with the inclusion of added control variables and the changes in sample composition.

At the primary school level the panel sample estimates imply that the average effect of the program across the three post-program rounds is to increase enrollment rates of girls by .96 percentage points, and boys by .74 percentage points, from the initially high enrollment rate of 94 percent (Table A-1). In the pooled sample which has a lower initial enrollment rate of 90 percent, the program is associated with an increase in enrollment rates for girls of 1.45 and boys of 1.07 percentage points, according to the estimated probit model.

At the secondary school level, the average enrollment effect of the program across the three post-program rounds in the panel sample is an increase of 9.3 percentage points for girls and 5.8 percentage points for boys, from their initial levels of 67 and 73 percent, respectively.[12] In the larger pooled sample the secondary school enrollment effects for girls average 7.2 percentage points and for boys 3.5. The selectivity that may be built into the panel sample compared with the more inclusive pooled sample weakens the estimated life cycle effect of the program for boys and girls.

The coefficients on the control variables are only described briefly here; they are reported fully elsewhere (Schultz, 2000c). The estimated effect of one more year of mother's schooling is to increase the probability of primary school enrollment for a daughter by .29 percentage points, and .18 for a son, whereas an added year of schooling of the father is associated with a .17 percentage point higher enrollment probability for a daughter and .26 for a son. Based also on the panel sample, at the junior secondary school level, the impacts are larger, with an additional year of the mother's schooling increasing her daughters probability of being enrolled by 1.5 percentage points and her son by 1.1, while the father's schooling is associated with an increase in his daughter's enrollment of 1.5 percentage points and his son's by 2.0. They are in the anticipated directions of favoring the offspring of the same sex as the parent, but the differences of mother's and father's schooling are never statistically significant at the 5 percent level (Cf. Thomas, 1994).

Distance to secondary school is associated with lower secondary school enrollment, whereas the greater the distance to the Cabecera or to the nearest metropolitan center the higher are enrollment rates, particularly at the secondary school level. Residing in a town that is only 50 kilometers from a metropolitan area, rather than the sample mean

[12] Earlier results reported (Schultz, 2000C) suggested that for boys, the program effect on enrollment declined on the later survey rounds, but this appears to have been due to an earlier error in my matching of the grade completed in round 5.

of about 100 kilometers, is associated with a secondary school enrollment rate being 5.5 percentage points lower for girls and 6.0 lower for boys. Nearby cities appear to dissuade rural children from enrolling for additional years in school, a challenging regularity for society to take into account as the transportation system improves and small towns become more closely integrated with neighboring cities. The poverty indicator used to target the Progresa transfer payments is associated with a significant reduction in enrollment rates of 0.9 percentage point at the primary level for both boys and girls, and 4.7 percentage points for girls and no significant effect for boys at the secondary level. This difference in the effect of household poverty on secondary school enrollments of boys and girls helps to explain why the Progresa educational grants as a poverty reduction program have increased the secondary school enrollment of girls more than that of boys (Schultz, 1988).

To explore other policies that might encourage schooling, two of the control variables in the probit model for enrollment can be interpreted as capturing the impact of traditional educational policies. First, access to secondary schools could be improved to stimulate greater enrollment. Twelve percent of the sample currently have to travel more than 4 kilometers to a junior secondary school. Building additional schools and staffing them so that these children reside only four kilometers from their junior secondary school is predicted to increase secondary school enrollments by .46 percentage points for girls and by .34 for boys.[13] A second policy constraint incorporated as a control variable in the

[13] Other studies of education have also estimated the enrollment effect of "distance to school" has a larger negative impact on enrollment for girls than on boys, particularly at the secondary school level (e.g., Tansel, 1997), an expected pattern if parents are especially reluctant to send teen-age daughters greater distances to school (King and Hill, 1993).

probit model of enrollment is the teacher-student ratio in the local primary school. Currently about 15 percent of the primary school aged children have a local primary school where the average class size is greater than 30. Building enough classrooms and providing the teachers to prevent any school from having an average class size in excess of 30 would, according to the estimated model, raise primary school enrollments by .1 percentage points for both boys and girls. Neither of these traditional education-based “supply” policy options for increasing enrollment rates appears to be a particularly effective means for raising enrollment rates, and moreover, neither could readily be targeted to the poor as is possible with school subsidies in the Progresa Program.

Table 6 compares the individual child probit-model estimates of the derivatives of the Progresa Program on school enrollment, averaged across the 6 years of primary school and 3 years of junior secondary school (Table 5), and the group-differenced estimates averaged across the grade levels 1 through 9 (Table 3). The probit estimates are based on two alternative samples – the panel and the larger pooled samples – whereas the group-differenced estimates rely on the panel sample to avoid changes in the composition of groups over time. The probit model adds 10 additional control variables, whereas the group-differenced estimates allow for program effects to differ for every grade, rather than only between primary and secondary school levels as assumed in the probit specification. The estimated program effect on girls’ enrollments is relatively similar across statistical models, controls, and samples, varying narrowly between 3.4 and 3.7 percentage points. In the case of enrollment probabilities of boys, the three estimates based on the panel sample are also of a similar value, ranging from 2.4 to 2.8 percentage points, but the probit model fit to the larger pooled sample suggests that the Progresa Program had a smaller effect on the enrollment of boys, on the order of 1.9 percentage points on average. However approximated, the Progresa Program has had a significant impact of increasing the school enrollment rates among children in poor rural households, and my preferred estimates are those based on the difference in differences (DD1) which control for persistent unobserved factors that might have influenced the level of enrollments across these localities that were administratively assigned to benefit from the first phase of the Progresa or be followed as controls.

5. Cost-Benefit Analysis of the Progresa Program in Terms of Human Capital

Increases in enrollment rates attributed to the Progresa Program in Table 3 can be cumulated to approximate the lifetime changes in years of enrollment that an average poor youth could expect to receive, if the Progresa Program were to become permanent. This cohort cumulation in enrollments is simulated in the first column of Table 7, based on the enrollment rates for the pre-program periods. These baseline figures imply that if a poor child had been once enrolled in school, and completed the first grade, he or she could expect in the Progresa localities to complete (on average) 6.80 years of school by the end of junior secondary school (out of a possible 9 years). Relatively few children continue further in school without leaving the region. If the D1 post-program enrollment effects from Table 3 are added to the baseline enrollment rates and cumulated for a cohort of children, this synthetic measure of expected years of post-program enrollment increases to 6.95 years, or a gain over the baseline of .15 years of schooling. But conditions deteriorated in this two year period October 1997 to November 1999 (Handa, et al, 2000), and in localities that did not benefit from the Progresa program, the expected cumulative school enrollment of a child fell in the three post-program rounds from 6.66 to 6.14 years of schooling, as shown in Table 7. The cumulative enrollment of the poor is .81 years greater in the Progresa localities (6.95) than in the non-Progresa (control) localities (6.14) in the post-program rounds of the survey, or based on D1 evaluated post-program. According to the difference in differences (DD1), that corrects for persistent pre-program differences in enrollments, the gain in cumulative enrollment of the poor is .66 years, which is accepted here as the preferred (and minimum) estimate of the program's long term effect on child schooling attainment. This DD1 program gain is slightly larger for girls, .72 years of additional schooling, than for boys, .64 years. From their baseline in schooling before the program started, expected educational enrollment through junior secondary school is expected to increase for girls by 11 percent in the Progresa localities, closing the gender differential in schooling among these poor families. Although the social experimental intervention of the Progresa program appears to have been relatively uncorrelated with initial levels (and inequality) of enrollment rates as tested by H_2 (and H_5), the pre-program differences between program and control localities are nonetheless conservatively taken into account in the difference in differences estimator (DD1).

Estimates of the wage structure for men and women in 39 metropolitan areas of Mexico based on a 1996 Survey (Encuesta Nacional de Empleo Urbano) imply that wages for both men and women are approximately 12 percent higher for each year of secondary school they have completed, and these estimates are robust to whether sample selection bias is corrected (Parker, 1999). Matching the rural communities surveyed here to these return estimates in the nearest metropolitan area, one also finds that this pattern of 12 percent private returns to secondary school is a reasonable approximation for what youth in these Progresa Program areas of Mexico could expect to receive if they had resided in their nearest urban labor market. However, the comparable returns to primary schooling are only about 5 percent per year in these same urban areas, revealing an increasingly common pattern in the world in which private returns to secondary schooling are markedly higher than those to primary schooling (Schultz, 1988).

Unfortunately, there are insufficient wage earners in the rural population surveys to estimate a local wage return to schooling in agriculture. Most workers in these rural areas are self-employed farmers. To estimate the effect of education on a farmer's profits, additional data are required which were not available in the Progresa census and surveys. When farm production functions are estimated in the United States or in low-income countries, the implied internal rate of return to the farmer's schooling is generally of a similar magnitude as the returns to schooling for urban wage earners (Jamison and Lau, 1982; Huffman, 2000).[14] However, a large share of the youth from these rural localities currently migrate to the city to find their adult livelihood. The wage gains realized by an average youth who become better educated in these rural areas are likely, therefore, to be larger

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[14] No variation in wages is assumed by age or correction for the growth in nominal wages in the three years 1996 to 1999, when the last Progresa survey is collected. Implicitly the relative wage structure by educational attainment is assumed constant, whereas in reality, the returns have been increasing over time. As noted earlier, no upward adjustment in the returns is made for the probability of rural-urban migration increasing wage opportunities for the rural youth who attend school more years. The net effect of these simplifications in the expected lifetime earnings calculation probably understates the human capital returns to Progresa grants. Any such estimate clearly embodies many sources of uncertainty given the gaps in my information on migration and rural earnings opportunities of youth in these rural regions of Mexico.

than those estimated within urban areas, because better educated rural youth are more likely to migrate to an urban destination (Schultz, 1988). The monthly wage for an average worker in the urban areas in 1996 is 1300 pesos, which is used here as the benchmark for wage opportunities in 1999.

One further piece of information is needed to approximate the internal rate of return to the Progresa educational grants – this is the share of potential beneficiaries of the program who are qualified to enroll at each schooling level who enroll and receive the Progresa educational grant (Table 1). The surveys provide a post-program estimate of the proportion of poor-eligible children residing in a Progresa locality who are enrolled given that they had completed the requisite number of years of schooling to qualify. Weighting grades 2 to 8 equally, one obtains an average estimate of 87 percent of the eligibles are enrolled who could attend grades 3 through 9 in the three post-program surveys. The proportion of the qualified poor youth who actually receive Progresa payments in the November 1999 survey is 72.5 percent, averaged over grades 3 to 9.

The five pieces of information needed to calculate the internal rate of return to the Progresa Program are now specified. If the Program educational grants (Table 1) are viewed as the investment expenditures of the Progresa Program, which are only paid to 73 percent of the potential beneficiaries, and the impact of these program subsidies is to increase the educational attainment of the cohort of poor youth by .66 years of schooling (Table 7, DD1), for which the youth earn a 12 percent higher wage per year of schooling over their adult lifetimes (age 18 to 65) based on the 1996 urban average wage, then the Progresa educational payments are earning an internal rate of return of about 8 percent per year, in addition to their role of reducing current poverty.

6. Summary and Conclusions

The level of enrollment rates of comparably poor children in Progresa localities (treatment) are higher than in non-Progresa localities (control) in the three survey rounds collected after September 1998 when the Progresa program began dispensing educational grants to poor mothers whose children were enrolled in school in grades 3 through 9 and attended school at least 85 percent of the time. This difference estimator of Progresa's impact on the enrollments of the poor is reported in Table 3 (D1 > 0, post-program). It is statistically significantly different from zero within each distinguished group of children who had completed grades 1 through 6 in the previous year. These differences are sometimes larger for girls than boys, as seen in Table 3. To confirm the independence of the placement of the Progresa Program from enrollments and the definition of eligibility, the regional differences in enrollment by the poor are also calculated before the program started and shown not to differ significantly from zero (Table 3, D1 = 0, pre-program). Difference in differences over time confirm a slightly smaller program impact on enrollment as evident in the difference in differences (DD1) estimator (Table 6) which is plotted in Figures 3 and 4. The cumulative cohort effect of the program based on the difference in differences estimator is an increment of .66 years on the baseline level of 6.80 years of schooling which is recorded in the pre-program rounds of the surveys, for both sexes combined.

The Progresa program targets its educational subsidies to the very poor, even in the geographically restricted, relatively immobile, poor rural communities. It is also shown that these targeted transfer payments have the effect of reducing the inequality in school enrollments within the Progresa localities compared with that in the non-Progresa localities (Table 4, D2 < 0 post-program), and they are statistically significant after grades 4 through 6. The pre-program inequality differences between the Progresa and non-Progresa localities are not jointly statistically significant. The estimated difference in differences in enrollment inequality over time (Table 4, DD2) is negative and statistically significant after grade 6 , and slightly larger overall than the D2 measured post-program.

One way to assess whether a roughly two-thirds of a year increment in schooling is worth the cost of the Progresa Program is to compare the expected program payments to the resulting expected increase in adult productivity of the students who stand to benefit from a permanently established Progresa Program. If the current urban wage differentials approximate what the Program beneficiaries can expect to earn from their schooling in terms of future percentage increases in their wages, a rough estimate of the internal rate of return to the educational transfers provided by the program is 8 percent per year in real terms (adjusted for inflation). This would appear to be a reasonable rate of return if the program were designed only to foster human capital investments. But it is clearly more than this, since it is channeled to the poor and operates to reduce current poverty and raise current consumption levels. For the majority of the poor rural families whose children would have attended school without the program's educational grants, the Progresa outlays are pure income transfers. But for the roughly one in twenty who are induced to enroll their child in school, they may experience a decrease in their children's supply of labor to work in the labor force or in household production. But as described in the appendix, although there is such a child labor supply response associated with the family being eligible for Progresa educational grants, the magnitude of the response is modest and cannot eliminate the consumption gains of the program to poor families (Cf. Ravillion and Wodon, 2000).

Another possible side effect of the Progresa Program could be on fertility, for the educational grants would appear to subsidize parents for the cost of a child's schooling, which might reduce the private cost of an additional child. Other studies that have sought to estimate the effect of a reduction in the cost of schooling on fertility have found that the income uncompensated cross-price effect is negative and outweighs the associated (positive) income effect of this reduction in the price of schooling, leading to the empirical conclusion that children and child schooling appear to be substitutes (Rosenzweig and Wolpin, 1980, 1982). In the Mexican panel sample analyzed here, there is also no statistical evidence that poor women who had a Progresa-eligible child who had completed grades 2 through 8 were more likely to have a birth in the six months preceding the November 1999 survey than comparable women residing in a non-Progresa locality.[15]

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[15] To evaluate the possible effect of the Progresa Program on fertility, the final survey round collected in November 1999 is analyzed, and the probability of having a birth between this round and the previous one in May 1999 is estimated in a probit specification corrected for cluster design as a function of the woman's age, years of schooling, being designated poor (and eligible for program grants if resident in a Progresa locality), whether resident in a Progresa locality, and the interaction of poor and Progresa. The last two variables (Progresa and Progresa-Poor interaction) are also added only for those women who have a child who is eligible for Progresa educational grants, having completed in the previous school year grades 1 through 8. The coefficients on these last two variables are reported in Table A-3, and their sum is viewed as an estimate of the Progresa Program's effect on fertility. This program effect is estimated for all women age 20 to 49 with the additional control for the woman's age squared, and for the five-year age brackets 20-24, 25-29, etc., with only the linear age control variable. It may be noted that no mothers age 15-19 had children of school age and thus the program is presumed to have had no estimable effect on the fertility of this youngest group of women who could bear children. For all women, the derivative of fertility with respect to the placement of the Progresa program is negative for women age 35-39 and 40-44, and approaches significance at the 10 percent level. For the eligible mothers, there is a statistically significant effect only for the age group 20-24 where the effect is positive, but collinearity prevents estimation of the two interaction variable coefficients jointly. In the available time frame, I would conclude that there are no consistent and statistically significant effects of the program on fertility.

Other traditional poverty reduction programs, such as income-support welfare systems or price-support agricultural programs as were funded by the previous Mexican poverty alleviation programs, offer no empirical evidence or theoretical reason to expect that these programs encourage investments in human capital or promote a more efficient allocation of private or social resource. Indeed both of these common forms of poverty programs -- supporting incomes and output prices -- are related to major distortions in the allocation of the family's labor and other resources of the beneficiaries. These types of resource distortion are minimized by the design of Progresa (to leave only "pure income effects").

Although it is not always a politically popular feature of a welfare program focused on poor areas, Progresa should help the children of poor Mexican farmers find a better place to work, by encouraging them to invest in schooling, which in turn facilitates the migration of these young people from their origin communities to other parts of the Mexican economy where wages and long-term career opportunities are more attractive. Thus, it is expected that Progresa will encourage the interregional migration that is needed at the macro-economic level to ease the extreme poverty that has persisted for generations in the more remote rural parts of Central and Southern Mexico. Subsidizing schooling among the rural poor may be a development strategy that deserves more attention as a targeted policy in many parts of the world that can both reduce entrenched intergenerational transmission of poverty and promote long-term economic growth.

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Appendix Describing the Effects of Progresa on Related Family Demand Behavior

The Progresa Program by reducing the price of schooling for children in poor families may affect the demand of these families for a variety of related goods and consumer behavior. According to the Slutsky decomposition, the effect of the program can be thought of as having a pure income effect that should raise the demand for all “normal” goods, and a cross-price effect that should reduce the demand for substitutes and increase demand for complements of the child’s schooling. Because of the child’s time constraint, it might be expected that a program that caused an increase in school enrollment would be associated with an offsetting decrease in child labor supply to work in the labor market and in the home. Indeed, some advocates of policies to reduce child labor hold out the promise that decreasing child labor would lead to increasing the schooling of the same children and thereby improve the child’s future economic opportunities (i.e. that the income uncompensated effect of a ban on child labor would be to increase schooling). This claim, however, not only assumes substitution between child schooling and work (i.e. a positive cross-price effect) but also anticipates that the cross-price effect dominates the income effect that would in this case operate in the opposite direction and reduce the demand for schooling and other normal goods.

Measuring child labor may be more difficult than it would seem. Household surveys in low income countries do not generally find as large a proportion of children working as expected by social observers. The Progresa census and surveys asked the respondent in all five rounds whether a child age 8 to 18 worked. A second question followed up those who reported the child as not working by a further line of inquiry as to whether the child produced something that was sold in the market. A third question was addressed in round 3 to respondents who had answered negatively the two previous questions, asking whether the child was engaged in any housework. Finally, for children working in paid labor they were asked how many hours they worked last week. The full sequence of questions were administered only in round 3 and 5 on which this analysis is based.[16] Summing the children reported as working in the first two questions yields 2.5 percent of the girls prepared for primary school as working, and 7.6 percent of the comparable boys as working (Cf. Sample means in Table A-2, and for ages see Tables 2 and A-2). Of the secondary school qualified girls, 8.3 percent work, whereas 28.4 percent of the boys work. The number of hours worked for pay per week is .09 and .36 for primary school girls and boys, whereas the average hours rises to .48 and 1.76 for the secondary school girls and boys, respectively. Except for teen-age males the labor force participation rates appear relatively low.

However, adding to the participation rate those children who contribute to housework, the primary school girls participation rate rises to 12.5 percent and the participation of secondary school girls increases to 33.2 percent, roughly equivalent to that of boys. Clearly, housework is a relatively more common activity for girls than boys, and conversely for boys in market labor force participation.

The estimates of the probit model are reported in Table A-2 to explain the probability that a child participates in (A) the labor force or housework and (B) in only the labor force , whereas a regression is fit in (C) to hours worked for pay last week. In

[16] In round 3 the three questions were 59, 60, and 62, whereas in round 5 they were respectively number 50, 51, and 53. In rounds 1 and 2 the third question on housework was omitted and the hours worked in paid activity was omitted from round 4.

comparison with poor children in non-Progresa localities (controls), the Progresa Program's effect is the sum of the Progresa resident coefficient plus the coefficient on the interaction of the Progresa and Poor dummies that makes the family eligible for the educational grants if the child is regularly enrolled. This program effect expressed as a derivative suggests that only the secondary school boys respond to Progresa by significantly ($p < .05$) reducing their labor force participation (B) by 3.6 percent. When the housework is also included in participation (A) the primary males exhibit a significant response of reducing participation by 1.6 percent, the secondary females reduce their participation by 4.1 percent, whereas the secondary males reduce their participation by 3.4 percent, though the significance of this last estimate does not satisfy the conventional 5 percent threshold. The child's hours of paid work also tend to be reduced in the Progresa-Eligible families, but the amounts are small: .04 hours per week for primary females and .11 hours for primary males, by .21 hours for secondary males, whereas the response is not statistically significant for secondary females.

Although the responses in child labor supply are in the direction expected, they are smaller in magnitude than the enrollment increases. Others have found the same pattern (de Gomez and Parker, 1999, 2000). According to Tables 5 and A-2 the Program induced increase in secondary school enrollment of 9.3 percentage points for girls is associated with a 4.1 percent decline in girls working in the labor force or in housework, but by only .4 percent when housework is excluded. Secondary school males increase their total labor force participation by 3.6 percentage points while they increase their enrollment rate by 5.8 percentage points (Table 5). If the reduction in hours of work by the secondary school males occurs entirely among the 6.9 percent who enrolled due to the Progresa Program grants, it is possible that these boys reduced their paid work by 3.0 hours per week (i.e. 3.0 = .21/.0694). These findings from a randomized social experiment imply, as the non-experimental cross sectional estimates of Ravillion and Wodon (2000) suggest, that the enrollment effect of secondary school fellowships (or Progresa grants) reduced by only a small fraction the time Bangladesh (Mexican) children worked. Duraisamy (2000) also finds that a large fraction of Indian teenage children are neither in the labor force nor enrolled in school, suggesting that increasing enrollments need not depress proportionately child work or reduce substantially family earnings. These estimates suggest that most of the educational grants transferred to the poor families by Progresa increase the family's resources and are only marginally offset by a reduction in child labor.

Table A-3 reports a similar analysis of fertility behavior between the fourth and fifth surveys. There is no evidence in this six month rate of births to suggest that the uncompensated schooling price reduction introduced by Progresa had any significant effect on fertility. This finding is also consistent with other studies that have sought to estimate the uncompensated cross-price effect from schooling to fertility or the effect of random variation in fertility on schooling as instrumented by the occurrence of twins (Rosenzweig and Wolpin, 1980, 1982, Schultz, 1997).

Table 1
Monthly Payments for Progresa Program Eligible Families for Children who attend at least 85 Percent of Days[a]

Educational Levels of Students Eligible for Payments			July - December 1998[b]
Primary School - both sexes			
	3rd Year		70
	4th Year		80
	5th Year		105
	6th Year		135
Secondary School			
	1st Year	Males	200
		Females	210
	2nd Year	Males	210
		Females	235
	3rd Year	Males	225
		Females	255

Source: Progresa Staff

a Excluding those days for which medical or parent excuses were obtained, accumulated over the last two months.

b Corresponds to school year first-term, September to December, 1998.

Table 2
Distribution of Children Age 6 to 16 in October 1997 and March 1998 in Panel Sample, by Age and Years of Schooling Completed in Previous Year
(beneath the number of children in each cell is the proportion of that cell enrolled)

	Years of Education Completed										
Age	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**Total**
6	2979 0.927	758 0.975	51 0.941	1 1.000		2 1.000	1 1.000	1 1.000			3793 0.937
7	1252 0.908	2434 0.996	492 0.988	40 0.975		1 1.000		1 1.000			4220 0.969
8	386 0.837	1618 0.989	1986 0.993	479 0.990	32 1.000	1 1.000		1 1.000			4503 0.978
9	131 0.649	552 0.984	1476 0.984	1659 0.993	331 0.991	38 1.000	2 1.000	1 0.000			4190 0.978
10	106 0.519	228 0.939	657 0.973	1568 0.984	1602 0.991	389 0.987	28 0.857	1 1.000	1 1.000	1 1.000	4581 0.971
11	73 0.397	73 0.918	295 0.963	692 0.964	1458 0.986	1451 0.986	281 0.904	19 1.000	1 1.000		4343 0.964
12	74 0.405	64 0.734	168 0.869	401 0.898	851 0.949	1346 0.969	1284 0.780	230 0.983	14 1.000		4432 0.888
13	64 0.219	75 0.773	101 0.733	169 0.757	349 0.891	723 0.934	1463 0.586	715 0.969	155 0.974	17 0.647	3831 0.776
14	50 0.160	54 0.722	82 0.354	115 0.626	183 0.754	378 0.836	1128 0.389	601 0.942	567 0.975	104 0.731	3262 0.685
15	18 0.278	25 0.940	31 0.548	45 0.444	76 0.553	138 0.739	556 0.318	229 0.934	260 0.954	221 0.588	1599 0.610
16	4 0.000	1 1.000	7 0.571	7 0.000	2 0.000	13 0.462	57 0.228	15 0.800	26 0.923	31 0.581	163 0.479
Total	5137 0.866	5882 0.978	5346 0.964	5176 0.957	4884 0.959	4480 0.951	4800 0.577	1814 0.956	1024 0.969	374 0.631	38917 0.899

Source: Estimated by the author based on the two pre-program rounds of the survey for only children who are matched in all five rounds or the Panel Sample.

Table 3
Differences Between Enrollment Rates Between Progresa and Non-Progresa Poor Children and Over Time.
(Significance Levels in Parentheses Beneath Differences)[b]

Year of Schooling Completed in Previous Year	Pre-Program Difference of Poor Progresa - Non-Progresa D1			Post-Program Difference of Poor Progresa - Non-Progresa D1			Post-Preprogram Difference in Differences DD1		
	All	**Female**	**Male**	**All**	**Female**	**Male**	**All**	**Female**	**Male**
0	.009 (.351)	.010 (.433)	.007 (.615)	-.002 (.854)	-.010 (.564)	.006 (.742)	-.011 (.482)	-.021 (.353)	-.001 (.969)
1	.001 (.410)	-.009 (.816)	.010 (.376)	.022 (.008)	.007 (.418)	.036 (.002)	.020 (.136)	.016 (.652)	.025 (.070)
2	-.004 (.276)	-.013 (.386)	.006 (.506)	.020 (.009)	.018 (.796)	.021 (.001)	.023 (.226)	.031 (.693)	.015 (.030)
3	.015 (.278)	.025 (.162)	.005 (.882)	.032 (.008)	.013 (.679)	.049 (.001)	.017 (.219)	-.012 (.508)	.044 (.014)
4	.008 (.500	-.016 (.836)	.030 (.266)	.041 (.001)	.038 (.261)	.044 (.001)	.033 (.053)	.055 (.335)	.013 (.064)
5	.015 (.129)	.005 (.544)	.025 (.125)	.047 (.001)	.055 (.232)	.041 (.000)	.032 (.146)	.050 (.647)	.017 (.077)
6	.024 (.345)	.048 (.433)	-.019 (.002)	.111 (.002)	.148 (.001)	.065 (.317)	.087 (.004)	.100 (.070)	.085 (.005)
7	-.012 (.894)	-.005 (.854)	-.015 (.958)	.013 (.147)	.025 (.533)	.003 (.006)	.025 (.378)	.030 (.583)	.018 (.062)
8	-.030 (.913)	-.051 (.932)	-.016 (.836)	.001 (.162)	.015 (.575)	-.010 (.100)	.031 (.347)	.066 (.687)	.006 (.235)
9 or More	.103 (.534)	.327 (.001)	-.156 (.006)	.066 (.317)	.111 (.042)	.026 (.813)	-.037 (.914)	-.216 (.044)	.182 (.020)

Notes:

[a] For definition of D1 and DD1, see Figures 1 and 2 and text

[b] The differences are tested for being different from zero by fitting a linear regression model with discrete additive variables to fit the contingency table for enrollment rates illustrated in Figure 1, and then the coefficients are tested jointly with an F statistic for whether differences are zero.

Table 4
Difference Between Enrollment Inequality Between Progresa and Non Progresa Localities[a]
(Significance levels in parentheses beneath differences)[b]

Years of Schooling Completed in Previous Year	Pre-Program Differences D2			Post-Program Differences D2			Difference in Differences DD2		
	All	Female	Male	All	Female	Male	All	Female	Male
0	.010 (.609)	.009 (.752)	.011 (.691)	.049 (.063)	.010 (.784)	.094 (.014)	.039 (.229)	.001 (.978)	.083 (.073)
1	-.002 (.904)	.010 (.703)	-.013 (.601)	-.032 (.083)	-.034 (.205)	-.030 (.225)	-.030 (.259)	-.044 (.248)	-.017 (.640)
2	-.009 (.64)	-.012 (.646)	-.006 (.816)	-.027 (.099)	-.011 (.659)	-.040 (.069)	-.018 (.476)	.002 (.962)	-.033 (.327)
3	-.009 (.637)	-.032 (.243)	.012 (.649)	-.027 (.083)	-.016 (.464)	-.037 (.091)	-.018 (.461)	.015 (.661)	-.049 (.156)
4	.002 (.936)	.026 (.327)	-.022 (.408)	-.043 (.007)	-.044 (.053)	-.038 (.087)	-.045 (.070)	-.070 (.046)	-.017 (.624)
5	-.020 (.293)	-.003 (.909)	-.037 (.165)	-.047 (.003)	-.047 (.042)	-.049 (.025)	-.027 (.279)	-.044 (.220)	-.012 (.720)
6	.042 (.023)	-.009 (.736)	.124 (.000)	-.035 (.006)	-.119 (.000)	.061 (.001)	-.077 (.001)	-.110 (.000)	-.064 (.048)
7	.014 (.627)	.010 (.814)	.015 (.710)	.002 (.910)	.026 (.369)	-.021 (.441)	-.012 (.738)	.016 (.755)	-.036 (.457)
8	.023 (.545)	.024 (.665)	.029 (.577)	.002 (.936)	-.026 (.428)	.025 (.406)	-.021 (.629)	-.050 (.438)	-.004 (.948)
9 or more	-.022 (.726)	-.284 (.002)	.266 (.003	.014 (.551)	.094 (.006)	.110 (.000)	.036 (.593)	.190 (.049)	-.156 (.096)

Notes:

[a] For definition of D2 and DD2 see figures 1 and 2 and text.

[b] The differences are tested for statistical significance by fitting the enrollment rate contingency table as illustrated in Figure 1 by a linear regression with discrete additive variables, and then coefficients are jointly tested for the differences being non zero with the F test.

Table 5

Probit Estimates of the Effects of Progresa on the Enrollment Probability of the Poor in the Post-Program Periods

Estimated Derivatives at Sample Means	**Panel Sample**				**Pooled Sample**			
	Primary		**Secondary**		**Primary**		**Secondary**	
	Female	**Male**	**Female**	**Male**	**Female**	**Male**	**Female**	**Male**
Progresa Locality a^*_1 (t ratio)	-.0001 (.03)	-.0101 (1.83)	-.0259 (.91)	-.0012 (.06)	-.0024 (.34)	-.0100 (1.58)	-.0274 (1.16)	.0059 (.29)
Poor Eligible Household in Progresa Locality a^*_2 (t ratio)	.0097 (2.36)	.0175 (4.06)	.1186 (4.5)	.0694 (2.69)	.0169 (2.64)	.0207 (3.71)	.0993 (4.58)	.0404 (2.13)
Net Progresa Impact [Significance non-zero based on Joint $?^2$ test]	.0096 [.0027]	.0074 [.0038]	.0927 [.0002]	.0582 [.0050]	.0145 [.0003]	.0107 [.0069]	.0719 [.0011]	.0345 [.0193]
Sample Size	33,795	36,390	13,872	14,523	55,396	59,344	25,761	26,696
Pseudo R^2	.3587	.3512	.2890	.2715	.3659	.3635	.2994	.2847

Table 6

Probit and Differenced Estimates of the Average Program Effect on Enrollment Over Grades 1 Through 9 (In Percentage Point Changes)

Sample By Sex	Individual Child Probit Derivatives		Group Panel Sample Differences	
	Panel Sample	Pooled Sample	Post-Program D1	DD1
Girls	3.74	3.37	3.43	3.50
Boys	2.42	1.88	2.83	2.47

Source: Tables 3 and 5. For example, DD1 is summed for grades in Table 3 and divided by 9; Probit derivatives for primary school multiplied by 6 plus secondary school multiplied by 3, divided by 9.

Table 7

Cumulative Expected Enrollment Years for Birth Cohort of Poor Children who Enroll and Complete Grade 1

Grade Completed	Preprogram Rounds 1 and 2		Post-Program Rounds 3, 4, and 5		Difference in Differences	
	Progresa	Non-Progresa	Progresa	Non-Progresa	DI	DDI
1	.977	.975	.975	.953	.022	.020
2	.936	.938	.939	.899	.040	.042
3	.896	.884	.904	.837	.067	.041
4	.856	.838	.866	.768	.098	.080
5	.816	.786	.825	.695	.130	.100
6	.464	.428	.511	.352	.159	.121
7	.436	.407	.484	.330	.154	.125
8	.414	.399	.450	.306	.144	.129
Expected Total Years Enrolled for Both Sexes	6.80	6.66	6.95	6.14	.81	.66
Years Enrolled Females	6.66	6.62	6.95	6.19	.76	.72
Years Enrolled Males	6.93	6.72	6.96	6.11	.85	.64

Table A - 1
Means and Standard Deviations of all variables Examined in Enrollment Models for Panel and Pooled Samples, by Primary and Secondary School and by Sex[a]

Variable Name	Sample 1 - Panel				Sample 2 - Pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
Sample Size	33795	36390	13872	14523	55396	59344	25761	26696
Enrollment	.942	.937	.674	.730	.896	.898	.578	.635
Attendance[d]	.972	.971	.981	.980	.970	.968	.982	.978
Progresa Locality	.605	.613	.600	.625	.612	.618	.606	.629
Eligible (Poor)	.733	.735	.603	.622	.726	.731	.587	.592
Progresa × Eligible	.454	.462	.369	.408	.448	.456	.362	.383
Completed Schooling								
0	.127	.120			.183	.172		
1	.169	.173			.175	.185		
2	.181	.187			.167	.170		
3	.188	.186			.171	.172		
4	.173	.171			.155	.155		
5	.161	.163			.148	.149		
6			.557	.504			.551	.491
7			.200	.220			.166	.185
8			.139	.160			.135	.157
9 or more			.104	.116			.148	.167

Age of Child:								
6	.068	.063	.000	.000	.090	.083	.000	.000
7	.115	.110	.000	.000	.124	.120	.000	.000
8	.152	.151	.000	.000	.138	.135	.000	.000
9	.155	.148	.000	.000	.133	.129	.000	.000
10	.165	.157	.002	.001	.142	.140	.001	.001
11	.142	.135	.029	.031	.123	.120	.022	.023
12	.098	.106	.162	.142	.089	.096	.121	.107
13	.047	.057	.249	.225	.047	.054	.192	.172
14	.027	.035	.246	.254	.032	.037	.207	.211
15	.014	.020	.189	.203	.024	.028	.211	.215
16	.007	.009	.104	.123	.020	.022	.187	.204
17-18	.001	.002	.019	.020	.006	.007	.057	.064
Mother's Schooling[e]	2.85 (2.65)	2.79 (2.64)	2.71 (2.48)	2.62 (2.50)	2.71 (2.70)	2.68 (2.68)	2.50 (2.47)	2.47 (2.48)
Father's Schooling[e]	2.93 (2.77)	2.88 (2.73)	2.75 (2.59)	2.78 (2.70)	2.80 (2.81)	2.76 (2.75)	2.58 (2.58)	2.60 (2.64)
Mother Not Present	.047	.049	.047	.048	.062	.062	.068	.061
Father Not Present	.103	.108	.108	.114	.127	.125	.132	.130
School Characteristics:								
Primary School Student/Teacher Ratio[a]	17.4 (14.1)	17.4 (13.9)	16.6 (13.4)	16.7 (13.6)	17.1 (14.3)	17.1 (14.2)	16.5 (13.5)	16.7 (13.5)
No Information on Primary School	.293	.290	.301	.295	.301	.298	.300	.295

Distance to Secondary School (km)[e]	2.10 (1.90)	2.13 (1.87)	2.03 (1.86)	2.05 (1.86)	2.16 (1.93)	2.15 (1.92)	2.07 (1.89)	2.08 (1.87)
No Distance to Secondary School	.022	.016	.009	.008	.029	.024	.011	.010
Community Characteristics:								
Distance to Cabeceras (km)	9.61 (6.17)	9.51 (5.96)	9.75 (6.32)	9.42 (5.74)	9.63 (6.05)	9.59 (5.96)	9.79 (6.30)	9.54 (5.90)
Distance to Nearest Metro Area (km)[f]	104. (42.5)	105. (43.1)	104. (42.0)	105. (41.7)	103. (42.6)	104. (42.7)	104. (41.6)	105. (41.3)
Community Daily Agricultural Wage:								
For Men[e]	29.2 (10.4)	29.2 (10.4)	31.2 (10.8)	29.9 (10.6)	29.0 (10.7)	29.0 (10.9)	30.3 (11.0)	29.7 (10.9)
For Women[e]	11.5 (14.3)	11.3 (14.4)	11.6 (15.2)	11.5 (14.6)	11.8 (14.4)	11.4 (14.3)	11.6 (14.9)	11.6 (14.7)
No Wage for Men	.021	.022	.017	.026	.029	.031	.026	.032
No Wage for Women	.562	.570	.583	.575	.549	.565	.576	.568

[a] The standard deviations of continuous variables are reported in parentheses beneath their means. In the case of binary dummy variable (= 1 or 0), the standard deviation is a function of the mean (*SD* = $\sqrt{mean(1-mean)}$).

[b] Primary sample includes all children age 6 to 16 who have completed from 0 to 5 years of school and are thus qualified to enroll in primary school grades 1 to 6.

[C] Secondary sample includes all children age 6 to 16 who have completed from 6 to 9 or more years of schooling and are thus qualified to enroll in secondary school.

[d] Attendance rate based on those who are enrolled and respond to the attendance question. Thus, for primary female panel sample 70.8 percent of all girls report an attendance rate of 97.2 percent. But of those 94.2 percent who are reported to be enrolled, 21.4 percent do not answer the attendance question.

[e] Variable mean and standard deviation based on entire sample where non-reporters are set to zero and the subsequent dummy is included in the regression. Thus in the case of primary student-teacher ratio, the mean for reporting schools is 24.6 (17.43/(1.0-.292)).

[f] Distance measured from locations in Hidalgo (State) and the nearest of four cities (Queretaro, Puebla, Tampico, or Mexico City), in Michoacan (State) from Morelia (Capital), in Puebla from Puebla, in Queretaro from Queretaro, in San Luis Potosi from San Luis Potosi, in Veracruz and Veracruz, and in Guerrero from Acapulco (largest city in State).

Table A-2

Probit and Regression Estimates of the Effect of Progresa on the Probability that Poor Children Work in the Labor Force or in Housework if Eligible for Education Grants: Rounds 3 and 5.

Explanatory Variables	Primary		Secondary	
	Female	Male	Female	Male
A. Child Works or Helps in Housework				
Resident in Progresa	.0045 (.38)	-.0009 (.07)	-.0057 (.31)	-.0157 (.79)
Eligible and Progresa	-.0190 (1.56)	-.0168 (1.41)	-.0355 (1.66)	-.0178 (.76)
Total Program Effect [Significance]	-.0145 [.11]	-.0159 [.048]	-.0412 [.012]	-.0335 [.056]
Sample Size	17,912	19,396	10,702	11,270
Sample Mean of Market Work or Housekeeping Questions	.125	.112	.332	.324
B. Child Works				
Total Program Effect [Significance]	.0019 [.58]	-.0086 [.11]	-.0042 [.60]	-.0362 [.031]
Sample Size	17,912	19,363	10,702	11,270
Sample Mean of Market Work Question	.0248	.0764	.0828	.2840

C. Hours Child Works for Pay				
Total Program Effect [Significance]	.0424 [.031]	-.1140 [.0023]	-.0617 [.35]	-.2083 [.058]
Sample Size	17,654	18,757	10,429	10,362
Sample Mean Hours	.0866	.356	.480	1.76

Table A-3

Probit Estimates of Derivatives Between Program Eligibility and Probability of Birth in Six Months Prior to November 1999 by Women's Age[a]

Age of Woman in November 1999	20 - 49		20 - 24	25 - 29	30 - 34	35 - 39	40 - 44[b]	45 - 49[b]
Mothers with child who had completed 2 to 8 years of education interacted with:								
Progresa Locality	-.0024 (.27)		c	-.0246 (1.16)	-.0221 (1.20)	.0173 (.79)	-.0226 (.32)	.0012 (.40)
Progresa × Poor	.0057 (.57)		.0459 (1.49)	.0085 (.31)	.0484 (1.69)	.0031 (.10)	-.0015 (.16)	-.0027 (.94)
Total Effect of Progresa Program [Significance]	.0033 [.62]		.0459 [.14]	-.0161 [.18]	-.0263 [.23]	.0204 [.33]	-.0041 [.42]	-.0015 [.33]
Mean of Birth Rate	.0411		.0615	.0655	.0468	.0335	.0138	.0027
Sample Size	17,434		3,661	3,327	2,972	2,803	2,457	2,214

[a] Probit maximum likelihood estimates with cluster occurrence weighting for heteroscedasticity (Huber, 1967). Other controls made age, years of mother's education, and poor, with a quadratic term or age for the sample for all age groups covered. No women 15-19 had children of relevant school age.

[b] Collinearity restricted specification to include only Progresa and Progresa-Poor interaction for mothers of children in Progresa-eligible group.

[c] Collinearity between Progresa and Progresa-poor interaction led to near singularity. Removal of Progresa with eligible beneficiaries converged. Linear probability model led to more stable results with all interactions and similar derivatives.

Figure 1

Schematic Comparison of the Proportion of Children Enrolled in School at time period t

Program Selection of Locality	Economic Endowments of Households	
	Poor Households Eligible for Progresa grants	Not Poor Households and Ineligible for grants
Progresa Localities	S_{1t}	S_{3t}
Non-Progresa (Control) Localities	S_{2t}	S_{4t}

Figure 2

Group Differences Representing Effects of Program Grants

1. Program-Control Differences in Outcomes among Comparable-Eligible (Poor) Groups

$$D1_t = S_{1,t} - S_{2,t}$$

Assumes Program placement is orthogonal to all other factors affecting or correlated with outcomes variables.

II. Double-Differenced Estimator of Change in Outcomes between Program-Control Eligible Groups over time:

$$DD1_t = (S_{1,t} - S_{2,t}) - (S_{1,t-1} - S_{2,t-1})$$

III. Non-eligible-Eligible Differences between Program and Control regions measure Program effect on reducing equality in access to schooling, or a measure of targeting effectiveness:

$$D2_t = (S_{3,t} - S_{1,t}) - (S_{4,t} - S_{2,t})$$

IV. Double-Differenced Estimator of Change in Inequality in Outcome over time:

$$DD2_t = (S_{3,t} - S_{1,t}) - (S_{4,t} - S_{2,t}) - [(S_{3,t-1} - S_{1,t-1}) - (S_{4,t-1} - S_{2,t-1})]$$

Assumes all factors affecting economic group differences in Program and Control regions do not change over time.

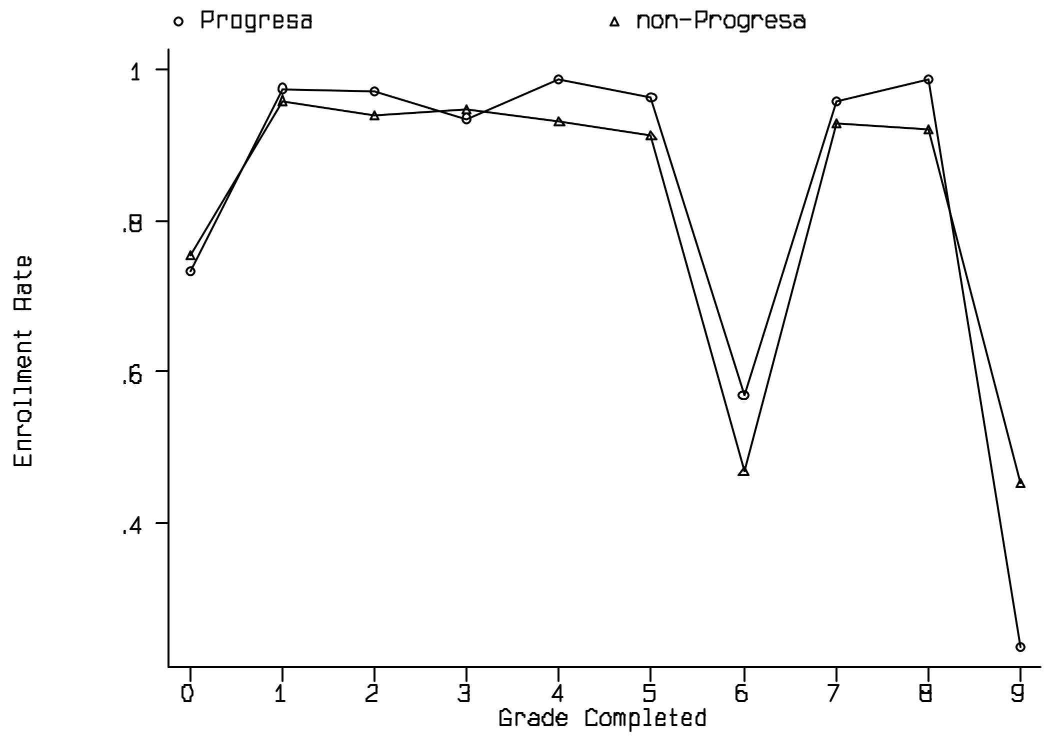

Figure 3: Girls’ Enrollments in Progresa and Non-Progresa Localities Over Time

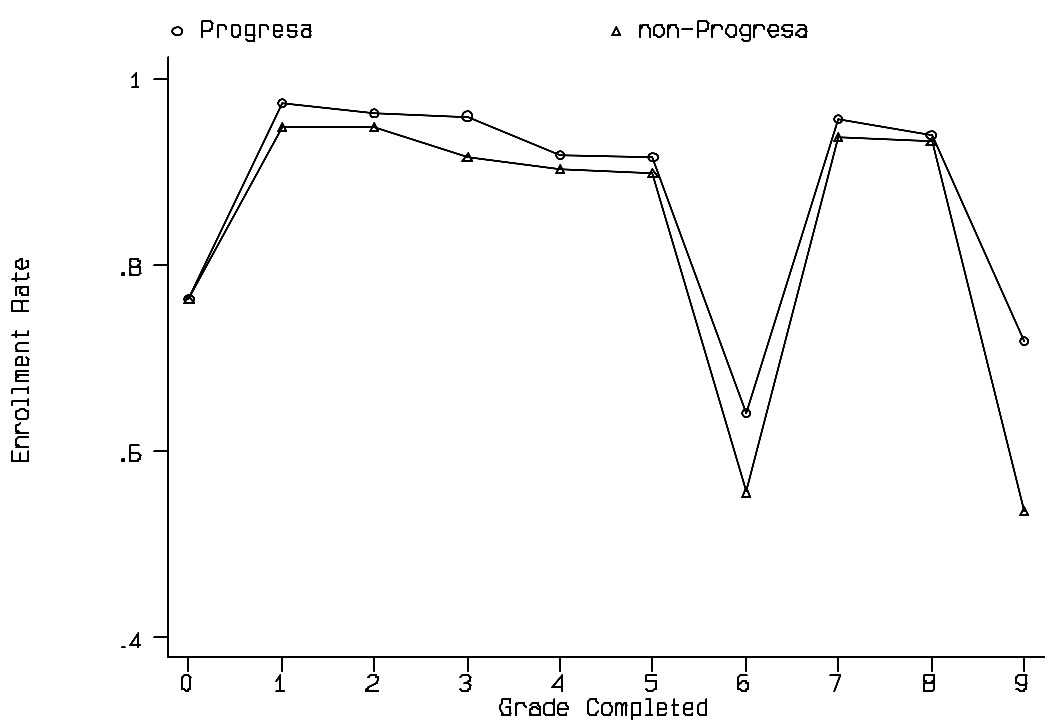

Figure 4: Boys’ Enrollment in Progresa and Non-Progresa Localities Over Time