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School subsidies for the poor: evaluating the Mexican Progresa poverty program

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Abstract

This paper evaluates how the Progresa program, which provides poor mothers in rural Mexico with education grants, has affected enrollment. Poor children who reside in communities randomly selected to participate in the initial phase of the Progresa are compared to those who reside in other (control) communities. Pre-program comparisons check the randomized design, and double-difference estimators of the program's effect on the treated are calculated by grade and sex. Probit models are also estimated for the probability that a child is enrolled, controlling for additional characteristics of the child, their parents, local schools, and community, and for sample attrition, to evaluate the sensitivity of the program estimates. These estimates of program short-run effects on enrollment are extrapolated to the lifetime schooling and the earnings of adults to approximate the internal rate of return on the public schooling subsidies as they increase expected private wages.

JEL classification: I21; I32; J13; J24
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1. Introduction

This paper analyzes the impact on school enrollment of a school subsidy program in poor rural communities in Mexico called Progresa. The program was randomly allocated among an initial group of 495 localities, which allows for a straightforward evaluation of the short-run effect of the program by comparing mean enrollment rates of those eligible (i.e., poor) for assistance in the treatment and control villages. The implementation of

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the randomized design is appraised by comparing pre-program differences between enrollments in the treatment and control populations. The robustness of the "difference in difference" program effects to the introduction of additional controls and different samples is gauged by estimating a parallel probit model at the individual child level. These short-run program effects are then used to extrapolate long-run cohort program effects on lifetime schooling and earnings, offering a hypothetical assessment of how the public outlays on school subsidies in this program would be recovered in the form of enhanced private earnings of the children of the families who were offered the school subsidies.

The paper follows this order, but it first reviews in Section 2 alternative programs that have been adopted elsewhere to achieve some of the same objectives as the Progresa program, in terms of poverty alleviation and expanded schooling. The rationale for seeking a more efficient and equitable arrangement is evident, but empirical evidence on what policies are effective for the poor in low income countries is fragmentary, and evaluations of randomized program interventions are rare. Section 3 describes the administrative form of the Mexican program, the pattern of pre-program school enrollments, and the randomized design of the social experiment which produced the data analyzed here. Section 4 reviews the conceptual and empirical model of enrollment, which implicitly embodies determinants of the household demand for schooling of children, including the community supply of school services, and the community opportunity cost of the time of school-age children who are not in school. Estimates of the enrollment mean differences between treatment and control groups are reported in Section 5, before and after the initiation of the program's school subsidies. Then the probability of enrollment is estimated at the individual child level in Section 6, using first the same panel data as examined in the difference in difference analysis, and then using a larger unrestricted pooled sample of all cross section observations. Controls are included in these child level probit equations for household endowments, school supply, and community characteristics, in part to assess whether the response of the treated to the program differs across these sub-populations. Section 7 combines these estimates of the short-run program enrollment effects and the wage structure in neighboring communities to construct for cohorts of children what they might expect to receive in enhanced earnings over their lifetimes due the program induced schooling effects. Section 8 and Appendix A conclude with estimates of the short-run program effects on child labor and fertility to broaden the basis for evaluating the Progresa program.

2. Alternative social welfare programs and their evaluation

Poverty alleviation programs have taken a variety of forms, but they typically achieve distributional gains at a cost in terms of efficiency. In most high-income countries, welfare programs provide transfers to people with incomes and assets below a specified level. 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 tend to distort private resource allocations of the beneficiary. In particular, means-tested poverty programs are thought to reduce the time beneficiaries work in the paid labor force, because their earnings are effectively taxed at a higher rate than that of non-beneficiaries, because they also loose program transfers as they increase their earnings.[1]

The previous government of Mexico under Salinas had adopted another common poverty alleviation strategy, supporting the prices for farm outputs, or paying subsidies for farm inputs. Two justifications for these programs are that farm incomes are lower than non-farm incomes, and that farm prices are more volatile than other prices, leading to greater variation in farm incomes than non-farm incomes, before taxes and transfers. But the disadvantage of agricultural price supports is that they are not typically targeted to the poor, but only to farmers who are on average relatively poor; rich farmers benefit as well, typically in proportion to the size of their output. Farm price supports also encourage an inefficient allocation of resources by providing incentives for labor and capital to be allocated to the production of the price-supported commodities, and retard an efficient out-migration from agriculture. As a result, consumers pay a higher price for farm products and states subsidize agricultural exports or tax agricultural imports.

In neither the income supplement nor the output price-support program is there any reason to expect beneficiaries to invest more in the acquisition of skills or the accumulation of capital to boost their future productivity or income. The program diminishes the incentive for beneficiaries to augment their future income and reduce their dependence on such transfers, which often involves their changing sector of employment or accumulation of new types of job experience.[2]

The Progresa program in Mexico was designed to minimize disincentives to work by not conditioning transfers on current income after the initial targeting of the program to the poor, based on geographical and household poverty information. Most programs designed to increase school enrollments among the poor build schools closer to where they live, increase the resources for the schools in terms of raising teacher salaries and training, reducing class size, and augmenting other educational inputs. These “supply” approaches may increase enrollments in some cases, but may not be especially effective in increasing enrollment among the poor, leaving a wide and possibly growing gap between the educational attainment of the children of the poor and rich (e.g., Deolalikar, 1997). The household “demand” approach provides subsidies which can be administratively targeted to the poor within a community, and perhaps thereby able to close the gap between enrollments of the poor and not

[1] 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 have traditionally supported only mothers without a co-resident father.

[2] 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 program, and contributed to the redesign of this program in 1996 to include a lifetime limitation of 5 years of transfers, and to the funding of coordinated child care and job training programs to encourage poor mothers to become self-supporting.

poor, reducing the substantial inequality in schooling and income found in Mexico and in many other parts of Latin America.

3. Administration of Progresa, randomized treatment design, and existing enrollment

3.1. Patterns

Targeting of the poor was first achieved by identifying from administrative and census data the rural communities in Mexico, which were the poorest and least likely to experience economic growth given the governments commitment to liberalize international trade (e.g., NAFTA), and reduce price supports (e.g., end tortilla subsidies). The second level of targeting required the collection of a census in October 1997 of all households in each of 495 of these poor rural communities. Information thus collected at the outset on income, consumption, consumer durables, and assets was used to construct a latent poverty index for the household. Only those persons in households below a certain poverty level were eligible for the assistance provided by Progresa (Behrman and Todd, 1999; Skoufias et al., 1999). About two-thirds of the Censused households were ultimately designated "poor" and thus eligible for Progresa transfers when the program was initiated in their locality.

However, only about two-thirds (314 out of 495) of the localities were randomly selected to receive the program activities during the first 2 years (Summer 1998 to Summer 2000). The remaining 181 non-Progresa localities, which serve here as controls, received the program in the third year, starting in the fall of 2000. The federal government announced in the summer of 1998 in the randomly selected Progresa localities that educational grants would be available to the eligibly poor mothers of a child enrolled in school and confirmed by their teacher to be attending 85% of the school days. These grants were provided for children enrolled in grades 3 through 9, or the last 4 years of elementary school and the next 3 years of junior secondary school. The program grants were promised for only 3 years, since the election in the fall of 2000 would lead to a change in government which might decide to change the program.

The magnitude of the educational grants is reported in Table 1 on a monthly basis in the first school term of the program. The size of the grants increase several fold at the higher grades. A premium for girls was introduced in junior secondary school, because enrollment rates for girls decreased relative to those for boys in the secondary schools in these communities. Every 6 months, the grants were adjusted upward to compensate for inflation as measured by the consumer price index reported by the Bank of Mexico (Cody and Djebbari, 1999).[3] To assess the relative magnitude of these school subsidies, the grant

[3] There are additional supports for eligible families. A transfer payment for school materials was initially set at 120 pesos per year at the primary level and 240 pesos at the junior secondary level, paid to the mother for each school term in which her child is enrolled in the program-subsidized grades. Finally, a "food" transfer of 50 pesos per household is provided the mother, if the members of the household receive program prescribed medical check ups, immunizations, and health education lectures. Pregnant and lactating women, and children under 2 years of age were given nutritional supplement, as were other young children who were not growing at an acceptable rate and were deemed at risk of malnourishment (PROGRESA, 1999).

Table 1
Monthly payments for Progresa program eligible families for children who attend at least 85% 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 2 months.

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

a mother would receive if her daughter were enrolled in the 9th grade would amount to 255 pesos per month, or 44% of the typical male day-laborer's wage in these rural communities, and roughly two-thirds of what a child this age earned if working full time.[4] In sum, the Progresa educational grants should have reduced by 50–75% the private economic costs of attending school for children qualified to enroll in grades 3 through 9.

To evaluate the effect of the program on a child's enrollment, the enrollment is conditioned on the years of schooling completed, k, which qualifies the child to enroll in next grade, $k+1$, for which there may (or may not) be a program school subsidy. Table 2 reports the distribution of children and their enrollment rates in the full sample of 495 rural communities by age and years of schooling completed, as obtained from two household surveys conducted before the Progresa educational grants were announced (i.e., October 1997 and March 1998). This is the benchmark against which the program's impact on enrollments is to be evaluated. Variation in age appears to be less important for explaining enrollments than years of schooling completed, as seen by comparing the marginal tabulations by age on the right of Table 2, with the marginal tabulation on the bottom by years of schooling completed. In contrast with high-income countries, within a single grade the age of students varies widely, and the enrollment rate does not drop markedly

[4] The daily wage for male agricultural labor reported in the 1998 and 1999 Community Surveys averaged 29 pesos for the communities studied here. It is assumed that the person works 20 days a month, for a monthly wage of 580 pesos. Only a few children age 10–16 report a wage in the five Household Survey cycles analyzed in this paper, and it would be unlikely that these respondents are a representative sample of all children, or that their reported wages are a precise indication of what the average child could earn if he or she worked (cf. Table A-2). The educational grants at the younger ages are half of what a young child age 10–13 reports earning, and perhaps 1/2 to 2/3 of what a child 14–16 receives. Progresa administrators suggest that the grants are scaled to compensate the family for the foregone earnings of the child who attends school rather than works, but it seems likely that the grants are somewhat less than the opportunity value of full-time child labor.

Table 2
Distribution of children age 6–16 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)

Age	Years of education completed										
	0	1	2	3	4	5	6	7	8	9	Total
6	2979	758	51	1		2	1	1			3793
	0.927	0.975	0.941	1.000		1.000	1.000	1.000			0.937
7	1252	2434	492	40		1		1			4220
	0.908	0.996	0.988	0.975		1.000		1.000			0.969
8	386	1618	1986	479	32	1		1			4503
	0.837	0.989	0.993	0.990	1.000	1.000		1.000			0.978
9	131	552	1476	1659	331	38	2	1			4190
	0.649	0.984	0.984	0.993	0.991	1.000	1.000	0.000			0.978
10	106	228	657	1568	1602	389	28	1	1	1	4581
	0.519	0.939	0.973	0.984	0.991	0.987	0.857	1.000	1.000	1.000	0.971
11	73	73	295	692	1458	1451	281	19	1		4343
	0.397	0.918	0.963	0.964	0.986	0.986	0.904	1.000	1.000		0.964
12	74	64	168	401	851	1346	1284	230	14		4432
	0.405	0.734	0.869	0.898	0.949	0.969	0.780	0.983	1.000		0.888
13	64	75	101	169	349	723	1463	715	155	17	3831
	0.219	0.773	0.733	0.757	0.891	0.934	0.586	0.969	0.974	0.647	0.776
14	50	54	82	115	183	378	1128	601	567	104	3262
	0.160	0.722	0.354	0.626	0.754	0.836	0.389	0.942	0.975	0.731	0.685
15	18	25	31	45	76	138	556	229	260	221	1599
	0.278	0.940	0.548	0.444	0.553	0.739	0.318	0.934	0.954	0.588	0.610
16	4	1	7	7	2	13	57	15	26	31	163
	0.000	1.000	0.571	0.000	0.000	0.462	0.228	0.800	0.923	0.581	0.479
Total	5137	5882	5346	5176	4884	4480	4800	1814	1024	374	38,917
	0.866	0.978	0.964	0.957	0.959	0.951	0.577	0.956	0.969	0.631	0.899

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

when a child is older than might be expected if he or she had started school at the authorized age of 6 and proceeded thereafter without setback. The primary school enrollment rate among children who had completed grades 1 through 5 is about 96%, and recovers to 97% after a child completes the first year of junior secondary school, or grade 7 (bottom row in Table 2). In the transition year from elementary to junior secondary school, however, the enrollment rate falls to 58%, after completing 6th grade, and drops again to 63% in the first year of senior secondary school. Thus, both the regularity in pre-program enrollment rates and the administrative requirement of the Progresa program suggest that the analysis of enrollment rates should focus on the effects within groups of children stratified by the number of grades they have completed and not mainly by their age. This stratification also facilitates estimation of program effects, for a child to qualify immediately for a Progresa educational grant they must have completed the 2nd to 8th grade and be currently enrolled. However, parents would have been financially encouraged by the program to enroll their children in the first two grades, in order to qualify for entering the third through ninth grades.

The data analyzed in this paper include children age 5–16 in the initial household census, and age 6–16 in the subsequent three rounds of the household survey, and age 6–18 in the final household survey conducted in November 1999.[5] The number of children age 5–16 enumerated in the initial census is 40,959, but of these, only 19,716 can be followed and matched in all five rounds of the surveys and are included in the panel sample. The attrition is undoubtedly partly due to outmigration, but is mainly a reflection of errors in identification codes which occurred for a few enumerators in the second round, and the age limitations on the children reporting in the subsequent surveys, which may make the oldest and youngest groups in the matched panel sample unrepresentative. Table A-1 reports the mean and standard deviation of the variables analyzed for the panel matched sample and the unrestricted pooled sample of all valid child observations, divided by gender, separately for primary and secondary school levels.

4. Modeling school enrollment and the empirical specification of evaluation methods

In a setting such as rural Mexico where the schools serving the surveyed population are public, and free of tuition, the private price of schooling is predominantly the opportunity value of the time a student withdraws from other activities to attend school. Children are engaged in many activities outside of school in addition to paid work and even productive activities based in the home or family business, such as farming. In Becker's (1965) model of household production and consumption, the opportunity cost of an individual's time is the marginal value of her or his output in these alternative valued activities. The price of schooling is then this shadow wage of the child minus any Progresa school subsidy, plus any direct costs of attending school, such as special school clothing or uniforms, books and materials, and transportation costs.

[5] The first two cycles in October 1997 and March 1998 are referred to as pre-program, whereas the data collected in October 1998, May 1999, and November 1999 are referred to as post-program.

The effect of the school subsidy is both to decrease the price of schooling and increase the family's income. To the extent that schooling is a normal consumption good for which demand increases with income, or income relaxes a credit constraint that allows the poor family to invest more in the schooling of their child, both the income effect and the income-compensated price of schooling effect of the school subsidy will increase the household's demand for schooling. I do not try to back out the income-compensated price effect.[6] Although the purpose of the Progresa program is to alleviate poverty while encouraging poor families to invest in the future productive opportunities of their children, the program might be redesigned if it were possible to decompose the income and conditional price effects. Since any conditional transfer involves monitoring costs and welfare losses for some, it might be optimal in some circumstances to provide an unconditional income transfer to increase enrollments (Martinelli and Parker, 2003).

The school subsidy may have additional consequences on household demands and behavior, some of which are empirically assessed in the appendix. Child labor, for example, would decrease in response to the school subsidy if child leisure and schooling were complements in the family's utility function, as illustrated by Ravallion and Wodon (2000). It is possible to add to their model of child time allocation the labor/leisure choices of the parents, and then school subsidy effects would involve additional terms for the income-compensated cross-substitution effects and income effects on the parent's labor supply. The uncompensated effects of the school subsidy on parent labor supply would not necessarily be positive or negative in sign. For example, the child attending school could be associated with the mother engaging in more work substituting for the child's labor, but this tendency might be offset by the income effect of the subsidy on the mother's demand for her own leisure.

Fertility might also be affected by the school subsidy. If child quality, proxied by schooling, and child quantity (fertility) are substitutes in the parent's utility function, the income-compensated school subsidy effect would cause a reduction in fertility (Rosenzweig and Wolpin, 1980). But the income effect on fertility is probably positive and may not be negligible (Schultz, 1997), holding out the possibility that the school subsidy could, on balance, increase fertility. Since Progresa explicitly informed parents that the program subsidies were assured for only 3 years, any expected impact on fertility would presumably be small. But this could become a more serious consideration when the program is viewed by parents as a permanent entitlement.[7]

[6] I am reluctant to interpret the coefficient on family income as an unbiased estimate of the income effect, because measured family income will tend to be endogenous, depending as it does on family labor supply decisions, notably of the child. One approach would be to have used suitable instruments to estimate the effect of family income on school enrollment, such as family nonhuman capital in the form of the value of business assets and land. But even these instruments may reflect saving behavior over time which could be related to preferences toward child schooling and labor supply and thus be an invalid instrument. The more limited objective of this paper is to estimate the income-uncompensated effects of the schooling subsidy or evaluate the total impact of the program arising from both the price and income effects.

[7] Fertility or family composition are excluded from the set of household control variables, because fertility and school enrollment decisions are likely to be simultaneous. Adding controls for household composition would undoubtedly bias the estimation of the school enrollment model and might also distort program evaluation estimates.

Even this simple theoretical framework for family decision making illustrates that the behavioral consequences of the school subsidy are ambiguous in their sign, with the exception of the effect on enrollment, which is expected to be positive. The current family income is conceptually endogenously determined by labor supply decisions of family members, including that of the child in question. But the pre-program October 1997 latent threshold of property, which determines the child's eligibility for program assistance and the school subsidy is unavoidably assumed exogenous for the purposes of program evaluation.

4.1. Empirical specification of enrollment determinants

This household framework for considering the determinants of school enrollment provides some guidance on suitable variables to include in an empirical reduced-form model. First several community characteristics are expected to influence the demand for schooling. Each locality has its own primary school. The ratio of the number of school-age (6–12) children per primary school teacher in the locality as of 1997 is examined as an (inverse) indicator of school quality, which is not itself affected by the local enrollment rate.[8] Unfortunately, no other indicators of school quality are available, nor is there evidence on how class size may have increased due to any effect of the program on enrollments. 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 an indicator of the time costs that a child and family could take into account in determining whether to enroll in junior secondary school.[9] Finally, two variables are included to capture the remoteness of the community from an urban labor market: the road distance to (a) the Cabecera or the municipal administrative center (sample mean 10 km), and (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. Greater distances to urban centers 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 wage structures or returns to schooling 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).[10]

[8] This specification of the student/teacher ratio avoids being itself a function of the enrollment rate, and thus an endogenous explanatory variable, as well as one which could be affected by program treatment if the program raised enrollment. With primary school enrollment rates of about 97%, the margin for primary enrollment to feedback on the student/teacher ratio appears to be moderate. Indeed, final estimates based on replacing actual student/teacher ratio with the preferred school-age child-to-teacher ratio increased the estimated effect of this school supply variable on enrollment rates by less than 10%, without affecting the program effects. This improved specification was suggested by Geeta Kingdon.

[9] The characteristics of the secondary school cannot be matched because some students traveled to schools that were not designated as "nearest", and thus their schools were not included in the database reporting the single nearest matched schools. Some students must have traveled longer distances to reach a preferred junior secondary school than I attribute to them, based on the only available government data.

[10] The community surveys did ask questions about the magnitude of daily wages in agriculture, but only the question for male wages was responded to by most communities, whereas only about a third reported a distinct female adult wage, and only some 5% reported a child's wage.

At the household level, only two variables are included in addition to the program eligibility indicator. The years of schooling completed by the mother and father are treated as likely determinants of a child's school enrollment probability.[11] 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 members, including the children themselves.[12] However, as discussed earlier a latent variable index for household economic well-being is constructed from the 1997 household 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.

In the following analysis of the impact of the Progresa program on school enrollment, it is useful to see the linkage between the two stages in the analysis. The first is based on difference in differences between groups of children in the localities that receive the program treatment and those in the control localities. The second stage of the analysis proceeds at the level of the school-age child and includes the controls suggested by the above empirical specification.[13] Let the probability of being enrolled in school for the ith 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 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

[11] 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% and exclusion of those without a mother of the child would have reduced the sample by 5% (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.

[12] Preliminary analyses of family labor supply responses to the Progresa program suggest small effects. A reduction in 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). Child labor responses to the program are analyzed further in Appendix A.

[13] 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, 2000a). See Table A-1 for the magnitudes of attendance for the responding sample. In sum, neither the program nor the household and community variables account for much of the variation in attendance.

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 (1982) adjustment for heteroscedasticity.[14]

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

$$S_i = \alpha_0 + \alpha_1 P_i + \alpha_2 E_i + \alpha_3 P_i E_i + \sum_{k=1}^{K} \gamma_{ki} C_{ki} + \sum_{j=1}^{J} \beta_j X_{ji} + e_i \quad i = 1, 2 \ldots, n \qquad (1)$$

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, α_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, α_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 α_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 α_3, which should be approximately zero until the program transfer payments are announced, and thereafter it is expected to be positive.[15] 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 α_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, $k = 0, 1, \ldots, 8, 9$, or more, which would qualify the

[14] 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 (1982) 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.

[15] During the first year of the program's operation, some households were added to the poor–eligible group, and thereafter were qualified to receive educational grants. This group represents only a few percent of those who are designated here as non-poor throughout the five survey cycles. This miscategorization of some children would presumably bias down the estimated program effects obtained here, since some of the "controls" are in reality being provided with the program treatment. Information is not available to me when this group changed status and became eligible for Progresa assistance.

child to enroll in the $k+1$ grade. The coefficients on these dummy variables, γ_k, thus adjust for linear differences in enrollment by grade level.[16]

With the passage of time, some variables that explain the probability of enrollment in Eq. (1) may change, such as C, which would change if a child completes one grade of schooling and qualifies 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, 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 (Eq. (2)) that combines all five survey cross sections:

$$S_{it} = \sum_{t=1}^{5} \alpha_{0t} + \alpha_1 P_i + \alpha_2 E_i + \alpha_3 (P_i E_i) + \sum_{k=1}^{K} \gamma_k C_{kit} + \sum_{t=3}^{5} (\alpha_{1t}^{*} P_i + \alpha_{3t}^{*} (P_i E_i)) + \sum_{j=1}^{J} \beta_j X_{jit} + e_{it}. \tag{2}$$

Eq. (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 school levels are estimated separately. The primary sample is defined as all children age 6–16 who report $C_{kt}=1$, for $k=0, 1, 2, \ldots, 5$, indicating that they have not yet completed primary school, and the secondary sample is defined as all children age 6–18 who report $C_{kt}=1$ for $k=6$, 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 Eq. (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 grants. Fig. 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 school enrollment of poor children, which are stratified by grade completed. The first

[16] 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, $k=2, 3, \ldots, 8$, 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, 2000b).

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}

Fig. 1. Schematic comparison of the proportion of children enrolled in school at time period *t*.

hypothesis tested by the "difference estimator" of the program level effects according to Fig. 2 is as follows:

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

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:

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

Even if the program placement were random, statistical correlation between program designated areas and pre-program enrollments might exist fortuitously.[17] 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 difference estimator (DD1) is defined as in Fig. 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

[17] Table D-1 reports the means of the core variables in this analysis of school enrollment rates for the poor children in Progresa and non-Progresa localities. None of the differences between the sample means is statistically significant, suggesting that the randomization of the selection of localities to receive initially the Progresa grants was not systematic with regard to these variables.

I. 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.

Fig. 2. Group differences representing effects of program grants.

initiated. One possible measure of the program's effect on inequality in enrollment is defined in Fig. 2:

$$H_4 \quad D2 < 0 \quad \text{Post-program period average}, \quad t = 3, 4, 5$$

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:

$$H_5 \quad D2 = 0 \quad \text{Pre-program period average}, \quad t = 1, 2.$$

A difference in difference estimator (DD2 defined in Fig. 2) can again remove any time invariant sources of the pre-program 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 difference estimators are preferred to the post-program differences, because they remove persistent sources of regional variation in enrollment that might exist. 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. 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. Finally, interactions between the program effects and characteristics of the family and community can be estimated to test whether treatment effects are heterogeneous. Such heterogeneity is neglected by the standard difference in difference evaluation method.

5. 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 difference 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.[18] If the conventional level of confidence required to accept the hypothesis is 5% or less, the D1 in the post-program surveys is significantly non-zero and positive from the 1st to 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% noted in the pre-program periods in Table 2, to about 69%. 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 difference (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 unweighted average value of D1 and DD1 over the grades 1 through 8 are of 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.[19] The D2 differences are negative from grade 1 to 6 in the post-program period, and statistically significant and negative from grade 4 to 6, implying the program reduces inequality, but the impact is largest after the last 3 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 difference, DD2, is negative from grade 1 to 8, and is statistically significant

[18] A joint χ^2 test is performed for whether the estimated mean enrollment for the treatment minus the control populations in the sex/grade cell is statistically significantly different from zero, as obtained from maximum likelihood estimates of a probit model fit to these contingency tables.

[19] 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.

Table 3

Differences between enrollment rates between Progresa and non-Progresa poor children and over time[a] (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–pre-program difference in differences DD1		
	All	Female	Male	All	Female	Male	All	Female	Male
0	0.009	0.010	0.007	– 0.002	– 0.010	0.006	– 0.011	– 0.021	– 0.001
	(0.351)	(0.433)	(0.615)	(0.854)	(0.564)	(0.742)	(0.482)	(0.353)	(0.969)
1	0.001	– 0.009	0.010	0.022	0.007	0.036	0.020	0.016	0.025
	(0.410)	(0.816)	(0.376)	(0.008)	(0.418)	(0.002)	(0.136)	(0.652)	(0.070)
2	– 0.004	– 0.013	0.006	0.020	0.018	0.021	0.023	0.031	0.015
	(0.276)	(0.386)	(0.506)	(0.009)	(0.796)	(0.001)	(0.226)	(0.693)	(0.030)
3	0.015	0.025	0.005	0.032	0.013	0.049	0.017	– 0.012	0.044
	(0.278)	(0.162)	(0.882)	(0.008)	(0.679)	(0.001)	(0.219)	(0.508)	(0.014)
4	0.008	– 0.016	0.030	0.041	0.038	0.044	0.033	0.055	0.013
	(0.500)	(0.836)	(0.266)	(0.001)	(0.261)	(0.001)	(0.053)	(0.335)	(0.064)
5	0.015	0.005	0.025	0.047	0.055	0.041	0.032	0.050	0.017
	(0.129)	(0.544)	(0.125)	(0.001)	(0.232)	(0.000)	(0.146)	(0.647)	(0.077)
6	0.024	0.048	– 0.019	0.111	0.148	0.065	0.087	0.100	0.085
	(0.345)	(0.433)	(0.002)	(0.002)	(0.001)	(0.317)	(0.004)	(0.070)	(0.005)
7	– 0.012	– 0.005	– 0.015	0.013	0.025	0.003	0.025	0.030	0.018
	(0.894)	(0.854)	(0.958)	(0.147)	(0.533)	(0.006)	(0.378)	(0.583)	(0.062)
8	– 0.030	– 0.051	– 0.016	0.001	0.015	– 0.010	0.031	0.066	0.006
	(0.913)	(0.932)	(0.836)	(0.162)	(0.575)	(0.100)	(0.347)	(0.687)	(0.235)
9 or More	0.103	0.327	– 0.156	0.066	0.111	0.026	– 0.037	– 0.216	0.182
	(0.534)	(0.001)	(0.006)	(0.317)	(0.042)	(0.813)	(0.914)	(0.044)	(0.020)

[a] For definition of D1 and DD1, see Figs. 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 Fig. 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	0.010	0.009	0.011	0.049	0.010	0.094	0.039	0.001	0.083
	(0.609)	(0.752)	(0.691)	(0.063)	(0.784)	(0.014)	(0.229)	(0.978)	(0.073)
1	− 0.002	0.010	− 0.013	− 0.032	− 0.034	− 0.030	− 0.030	− 0.044	− 0.017
	(0.904)	(0.703)	(0.601)	(0.083)	(0.205)	(0.225)	(0.259)	(0.248)	(0.640)
2	− 0.009	− 0.012	− 0.006	− 0.027	− 0.011	− 0.040	− 0.018	0.002	− 0.033
	(0.64)	(0.646)	(0.816)	(0.099)	(0.659)	(0.069)	(0.476)	(0.962)	(0.327)
3	− 0.009	− 0.032	0.012	− 0.027	− 0.016	− 0.037	− 0.018	0.015	− 0.049
	(0.637)	(0.243)	(0.649)	(0.083)	(0.464)	(0.091)	(0.461)	(0.661)	(0.156)
4	0.002	0.026	− 0.022	− 0.043	− 0.044	− 0.038	− 0.045	− 0.070	− 0.017
	(0.936)	(0.327)	(0.408)	(0.007)	(0.053)	(0.087)	(0.070)	(0.046)	(0.624)
5	− 0.020	− 0.003	− 0.037	− 0.047	− 0.047	− 0.049	− 0.027	− 0.044	− 0.012
	(0.293)	(0.909)	(0.165)	(0.003)	(0.042)	(0.025)	(0.279)	(0.220)	(0.720)
6	0.042	− 0.009	0.124	− 0.035	− 0.119	0.061	− 0.077	− 0.110	− 0.064
	(0.023)	(0.736)	(0.000)	(0.006)	(0.000)	(0.001)	(0.001)	(0.000)	(0.048)
7	0.014	0.010	0.015	0.002	0.026	− 0.021	− 0.012	0.016	− 0.036
	(0.627)	(0.814)	(0.710)	(0.910)	(0.369)	(0.441)	(0.738)	(0.755)	(0.457)
8	0.023	0.024	0.029	0.002	− 0.026	0.025	− 0.021	− 0.050	− 0.004
	(0.545)	(0.665)	(0.577)	(0.936)	(0.428)	(0.406)	(0.629)	(0.438)	(0.948)
9 or More	− 0.022	− 0.284	0.266	0.014	0.094	0.110	0.036	0.190	− 0.156
	(0.726)	(0.002)	(0.003)	(0.551)	(0.006)	(0.000)	(0.593)	(0.049)	(0.096)

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

[b] The differences are tested for statistical significance by fitting the enrollment rate contingency table as illustrated in Fig. 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.

for grade 6. The unweighted 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.

6. 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-age child-to-teacher ratio, distance to junior secondary school, and distances from the locality to urban areas (Schultz, 2000b). 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 χ^2 test associated with the likelihood ratio. The program's net impact on enrollment is statistically significant at the 0.5% level in seven out of the eight possible tests for different sexes, school levels, and samples, and in the eighth sample, it satisfies the test at the 2% level. Thus, there is 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 across variations 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 0.92 percentage points, and boys by 0.80 percentage points, from the initially high enrollment rate of 94% (see Table A-1). In the pooled sample which has a lower initial enrollment rate of 90%, the program is associated with an increase in enrollment rates for girls of 1.27 and boys of 1.18 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 9.2 percentage points for girls and 6.2 percentage points for boys, from their initial levels of 67% and 73%, respectively.[20] In the larger pooled sample, the secondary school enrollment effects for girls average 7.1 percentage points and for boys 5.2. The selectivity that may be built into the panel sample compared with the more inclusive pooled sample reduces slightly the estimated life cycle effect of the program for boys and girls as summarized in Table 6.

[20] Earlier results reported (Schultz, 2000b) 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.

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 matched sample				Pooled sample			
	Primary		Secondary		Primary		Secondary	
	Female	Male	Female	Male	Female	Male	Female	Male
Progresa locality a*$_1$ (t ratio)	0.0005	− 0.0088	− 0.0232	0.0048	0.0028	− 0.0061	− 0.0148	0.0166
	(0.12)	(1.62)	(0.81)	(0.18)	(0.42)	(1.00)	(0.62)	(0.79)
Poor eligible household	0.0087	0.0168	0.1155	0.0572	0.0099	0.0179	0.0861	0.0353
in Progresa locality a*$_3$ (t ratio)	(2.17)	(3.97)	(4.32)	(2.51)	(1.58)	(3.32)	(3.88)	(1.80)
Net Progresa impact	0.0092	0.0080	0.0923	0.0620	0.0127	0.0118	0.0713	0.0519
[significance non-zero	[0.0030]	[0.0015]	[0.0003]	[0.0027]	[0.0026]	[0.0029]	[0.0020]	[0.0102]
based on joint χ^2 test]								
Sample size	33,795	36,390	13,872	14,523	55,396	59,344	24,761	26,696
Pseudo R^2	0.3728	0.3712	0.3116	0.2979	0.4340	0.4179	0.3336	0.3231

In addition to the explanatory variables P, E, PE and the post-program interactions with P and PE, which define the program effect (see Eq. (2)), the primary school enrollment model includes dummies for ages 6, 7, 9, . . ., 16, and 17 or 18, dummies for grades completed 1 – 5 (0 omitted category), PE interacted with grades completed, survey rounds, mother's and father's years of schooling, dummies for parents not in residence in the household, child 6 – 12/primary school teacher ratio in locality, distance to secondary school, dummies to indicate that either of the school variables are missing in the government data base, and distance to Cabecera and to nearest metropolitan area. The secondary school enrollment model includes dummies only for ages 12 – 16, and 17 or 18, grades completed 6 – 8 (9 or more omitted category), PE interacted with grade completed 6, 7, or 8, and otherwise the same as for the primary school enrollment specification in terms of survey round, household, and community control variables.

The coefficients on the control variables are reported elsewhere (Appendix Tables B-1–B-8) and are only described briefly here. The estimated effect of one more year of mother's schooling in the panel sample is to increase the probability of primary school enrollment for a daughter by 0.26 percentage points, and 0.14 for a son, whereas an added year of schooling of the father is associated with a 0.16 percentage point higher enrollment probability for a daughter and 0.23 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 daughter's probability of being enrolled by 1.3 percentage points and her son by 0.87, while the father's schooling is associated with an increase in his daughter's enrollment of 1.4 percentage points and his son's by 1.9. 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% 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 km from a metropolitan area, rather than the sample mean of about 100 km, is associated with a secondary school enrollment rate being 5.5 percentage points lower for girls and 5.9 lower for boys. Nearby cities appear to dissuade rural children from enrolling for additional years in school, a regularity to take into account as the transportation system improves and small towns become more closely linked with neighboring cities.

The poverty indicator, E, used to target the Progresa transfer payments at the household level is associated with a significant reduction in enrollment rates of 0.9 percentage point at the primary level for both boys and girls, and with a 4.4 percentage points reduction at the secondary level for girls, whereas this effect of coming from a poor household has an insignificant and small effect on secondary enrollment for boys. This gender difference in the effect of household poverty on secondary school enrollments of boys and girls may help to explain why the Progresa educational grants as they reduce poverty have also increased the secondary school enrollment of girls more substantially than that of boys (Schultz, 1988).

To explore other "supply" oriented educational policies that might encourage schooling, two of the control variables in the probit model for enrollment can be further interpreted. First, access to secondary schools could be improved to increase enrollment. Twelve percent of the sample currently have to travel more than 4 km to a junior secondary school. Building additional schools and staffing them so that these children reside only 4 km from their junior secondary school is predicted to increase secondary school enrollments by 0.40 percentage points for girls and by 0.29 for boys.[21] A second policy constraint incorporated as a control variable in the probit model of enrollment is the

[21] 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 teenage daughters greater distances to school (King and Hill, 1993).

school-age child-to-teacher ratio in the local primary schools. Currently about 15% of the primary school-age children have a local primary school where the potential average class size is greater than 30. Building enough classrooms and providing the teachers to prevent any school from having more than 30 school-age children per local primary school teacher would, according to the estimated model, raise primary school enrollments by 0.1 percentage points for both boys and girls. These teacher and school supply effects are estimated to be slightly larger for the pooled sample of children than the panel. Neither of these traditional education "supply" policy options for increasing enrollment rates appears to be an effective means for raising enrollment rates, and moreover, neither could readily be targeted to the poor as is possible with the existing school subsidies.

To assess whether program effects are as assumed homogeneous for different groups, interaction variables with P and PE in the post-program cycles are included in the basic probit model. None of those examined were statistically significant: mother's education, father's education, mother speaks a Indian dialect, or the distance to the Cabecera or metropolitan area. Finally, a measure of permanent income in the form of household total consumption per capita is added to the enrollment model, despite it possible endogeneity, and interacted with P and PE. It was also not statistically significant when interacted with the program, though its direct effect was to increase enrollment, just as the indicator of being poor, E, decreased enrollment.

Table 6 provides a very rough comparison of the overall magnitudes of the two estimates of program impact on enrollments. The individual child probit-model estimates of the derivatives of the Progresa program on school enrollment are simply averaged across the 6 years of primary school and 3 years of junior secondary school (Table 5), and the group-differenced estimates are averaged across the grade levels 1 through 9 (Table 3). The probit estimates are based on two alternative samples—the matched 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 four estimates range from 2.5 to 2.8 percentage points, suggesting that the Progresa program had a smaller effect on the enrollment of boys than on girls. However approximated, the Progresa program has had a significant short-run impact increasing school enrollment rates among children in poor rural households in the first 2 years of operation.

7. How to analyze the public costs and private benefits of the Progresa program

Progresa's short-run effects on enrollments, as estimated by grade and reported in Table 3, can be demographically extrapolated to forecast long-run effects on final schooling attainment for a cohort of children, and assigned a monetary value by relying on the private wage returns to schooling prevailing in urban areas surrounding the communities

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 matched sample differences	
	Panel sample	Pooled sample	Post-program D1	DD1
Girls	3.70	3.22	3.43	3.50
Boys	2.60	2.50	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.

assisted by Progresa. This type of exercise depends on the stability of the short-run program effects over time, which might instead snowball or peter out, and the assumption that rural youth migrate to the city or receive comparable private wage returns on their increased education. Better information on migration rates and earnings trajectories of youth benefitting from the program could increase greatly our confidence in the results of such a simulation.

The program-attributed changes in enrollment rates in 1998 and 1999 are assumed to persist into the future, implying that a cohort would accumulate the additional years of schooling that are shown in Table 7. The first column of Table 7 is based on the enrollment rates for the pre-program periods. These baseline figures imply that if a poor child is 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 and disappearing from my sample.

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 cumulative cohort measure of expected years of post-program enrollment increases to 6.95 years (column 3), or a gain over the baseline of 0.15 years of schooling. But economic conditions in agriculture

Table 7
Cumulative expected enrollment years for birth cohort of poor children who enroll and complete grade 1

Grade completed	Pre-program rounds 1 and 2		Post-program rounds 3, 4, and 5		Difference in differences	
	Progresa	Non-Progresa	Progresa	Non-Progresa	D1	DD1
1	0.977	0.975	0.975	0.953	0.022	0.020
2	0.936	0.938	0.939	0.899	0.040	0.042
3	0.896	0.884	0.904	0.837	0.067	0.041
4	0.856	0.838	0.866	0.768	0.098	0.080
5	0.816	0.786	0.825	0.695	0.130	0.100
6	0.464	0.428	0.511	0.352	0.159	0.121
7	0.436	0.407	0.484	0.330	0.154	0.125
8	0.414	0.399	0.450	0.306	0.144	0.129
Expected total years enrolled for both sexes	6.80	6.66	6.95	6.14	0.81	0.66
Years enrolled females	6.66	6.62	6.95	6.19	0.76	0.72
Years enrolled males	6.93	6.72	6.96	6.11	0.85	0.64

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 (columns 3 and 4). The cumulative enrollment of the poor is 0.81 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, as implied by the D1 post-program evaluation method (column 5). According to the difference in difference (DD1), that corrects for persistent pre-program differences in enrollments, the gain in cumulative enrollment of the poor is 0.66 years (column 6), which is considered here as the preferred (and conservative) estimate of the program's long term effect on child schooling attainment. This DD1 program gain is larger for girls, 0.72 years of additional schooling, than for boys, 0.64 years. From their baseline in schooling before the program started, expected educational enrollment through junior secondary school increases for girls by 11% in the Progresa localities, closing the gender differential in schooling among these poor families.

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% higher for each year completed of secondary school, and these estimates are not substantially affected when corrected for possible sample selection bias (Parker, 1999). Matching the rural Progresa and control communities surveyed here to city-specific return estimates in the nearest metropolitan area, one also finds neighboring private returns to secondary school are about 12%. However, the returns to primary schooling are considerably lower in the same urban areas, revealing an increasingly common pattern in Mexico and elsewhere in which private returns to secondary schooling are higher than those to primary schooling (Schultz, 1988; Bouillon et al., 1999).

Unfortunately, there are insufficient wage earners in the rural population surveys to estimate a local wage return to schooling, and sample selection problems would probably be severe because most workers are self-employed farmers, and the better educated youth migrate to the city. To estimate the effect of education on a farmer's profits, additional data are required which were not collected in the Progresa census and surveys.[22] I have assumed that the rural youth after finishing their education by age 16 migrate to the urban area at age 18, and then work until their retirement at age 65. The rural migrant is assumed to receive a wage 20% less than average for their education and age, as approximated in the 1997 urban labor force survey. An internal rate of return is then calculated which equalizes the present discounted value of this increment in lifetime earnings of the youth to the present discounted cost of the program education grants offered to the cohort. One final adjustment in the calculation is needed, for not every poor child in the cohort collects a

[22] When farm production functions are estimated in the United States and in low-income countries, the implied internal rate of return to the farmers schooling is substantial, and of a similar magnitude to that observed in urban labor force surveys (e.g., Jamison and Lau, 1982; Huffman, 2001). Rural–urban migrants stand to benefit more from their education (Schultz, 1988), first because they incur lower opportunity cost of attending school in rural areas than do those born in urban areas, and second because the probability of migration from rural to urban areas increases with the schooling of the individual, and the return to rural–urban migration should therefore be treated as in part a return on their schooling.

educational grant from the program, because some are not enrolled and others who are enrolled do not apply or collect their grant. As of November 1999 the internal estimates of the program were that 73% of the children in poor families of the age when they would be likely to enroll in grades 3 through 9 were actually receiving an educational grant. Incorporating this final factor which reduces the cost of the grants by a quarter, the discount rate that equalizes the present value of the program grants and the earnings increment is 8% per year.

Five working assumptions are needed to approximate the internal rate of return to the Progresa program. The program educational grants (Table 1) are viewed as the investment expenditures of the Progresa program, of which only 73% are paid to potential beneficiaries, and the impact of these program subsidies is to increase the educational attainment of a cohort of poor youth by 0.66 years of schooling (Table 7, DD1), for which the youth earn a 12% higher wage per year of schooling over their adult working lifetimes (age 18–65) based on the 1996 urban wage structure. In addition to the program's important role of raising consumption levels in poor rural households by 20–25%, the Progresa public educational outlays appear to be earnings an internal rate of return on private wages of about 8% per year.

The inelasticity of demand for schooling still poses a puzzle. The school subsidy offered by the program appears to have reduced the private costs of attending school by more than half, but it only increased the educational attainment of the hypothetical cohort by 10% (0.66/6.80 from Table 7). A demand elasticity of less than − 0.2 in absolute value terms appears small and suggests there are limitations on what can be expected in public programs seeking to raise schooling levels among the poor.

Yet, the program's effect reducing inequality in enrollment between poor and non-poor households was shown in Table 4. The program's relative impact on inequality in schooling can also be quantified by contrasting it to the capacity of parents to transmit their own educational advantages to their children. Given the highly significant effects of the education of the mother and father on the child's schooling, one can simulate with the probit estimates what would be the difference in educational attainment for a child with parents who are both two standard deviations above the sample average, compared with a child with parents who were two standard deviations below the average (cf. Table A-1). The educational gap in schooling expected between these two children would be on the order of 0.7 years, or about the same as the cumulative impact of Progresa.[23] From this perspective, the program subsidies have made a marked difference in how poverty is replicated through the intergenerational transmission of schooling differentials.

[23] A final dimension along which to measure the Progresa impact would compare what the program has achieved compared with the long term trends in educational attainment in Mexico. At the national level, Mexico has advanced its schooling levels for youth by roughly a year per decade. It seems unlikely that the same rate of progress has been achieved in these rural poor communities, but I do not know of any estimates. In any case, the contribution of Progresa has been to move this pace of national progress forward 6 or 7 years in a segment of the population that starts out significantly behind. All of these measures of achievement are difficult to evaluate. Improving measures of educational progress which are targeted to the poor should receive more attention with the importance of schooling for personal welfare and economic growth.

8. 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 offering educational grants to poor mothers whose children were enrolled in school in grades 3 through 9. 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 often larger for girls than boys. The randomization of assignment of localities to the first wave of the Progresa program is tested by calculating the differences in enrollment by the poor in the treated and control localities before the program started. These pre-program differences were not significant, suggesting that the implementation of the random assignment was performed successfully (Table 3, D1 = 0, pre-program). Difference in difference estimates over time confirm a slightly smaller program impact on enrollment, as shown by the DD1 estimates (Table 6) which are plotted in Figs. 3 and 4 for girls and boys, respectively. The cumulative cohort effect on schooling attainment are extrapolated, and the difference in difference estimator implies that the program has caused an increment of 0.66 years on the baseline level of 6.80 years of schooling.

The Progresa program targets geographically and economically (at the household level) the poor, located in relatively immobile, rural villages of Central and Southern Mexico. Evidence is presented that this targeted transfer payment has the effect of reducing the economic inequality in school enrollments within the Progresa localities compared with that in the non-Progresa localities (Table 4, D2 < 0 post-program), and these impacts on

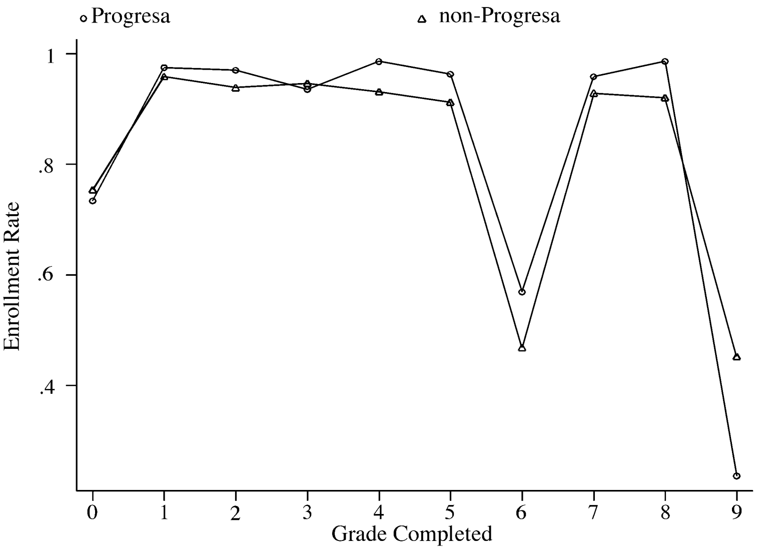

Fig. 3. Girls' enrollments in Progresa and non-Progresa localities over time.

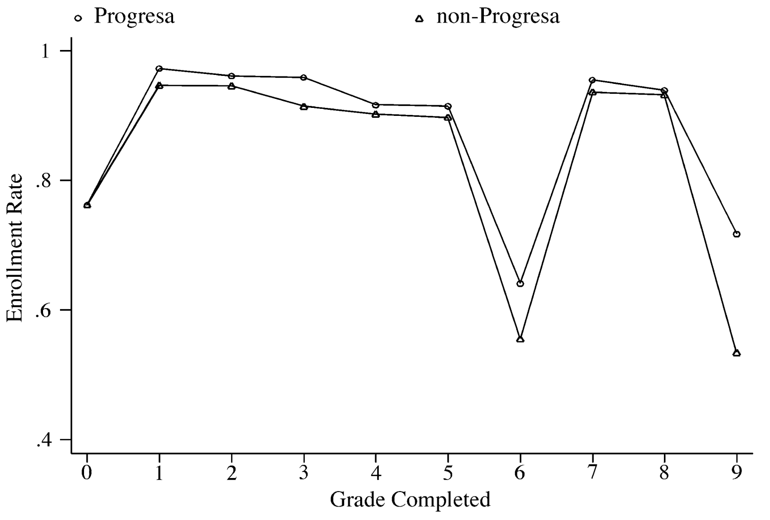

Fig. 4. Boys' enrollment in Progresa and non-Progresa localities over time.

enrollment inequality reach statistical significance from grades 4 through 6. The pre-program inequality differences between the Progresa and non-Progresa localities are not jointly statistically significant and consequently appear to be assigned randomly. The estimated difference in difference in enrollment inequality over time (Table 4, DD2) is negative and statistically significant after grade 6, and of about the same magnitude as 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 sustained Progresa program. If the current neighboring urban wage differentials approximate what the program beneficiaries can expect to earn from their schooling in terms of future percentage increases in their wages, an estimate of the internal rate of return to the educational grants provided by the program is 8% per year in real terms (adjusted for inflation). This would appear to be a moderate rate of return if the program were designed only to foster human capital investments. But it is clearly more than this, since it is concentrated on the poor and has the objective of reducing current poverty by raising current consumption levels for this group. For the majority of the poor rural families whose children would have attended school without the program's educational grants, the Progresa outlays are simple income transfers or a rent that does not change their behavior. But for the one in ten who are induced by the program subsidy to enroll their child in school, they may therefore experience a decrease in their children's work in the market labor force or in home production. But as described in Appendix A, although there is a significant reduction in child work associated with the family being eligible for Progresa educational grants, the magnitude of the response appears to be modest and cannot offset more than a fifth of the total consumption gains associated with the program grants (cf. Ravallion 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 would reduce the private cost of an additional child of the same schooling level. 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. The empirical literature has concluded that the number of children and child schooling appear to be substitutes for families in low-income countries (Rosenzweig and Wolpin, 1980, 1982; Schultz, 1997). In the Mexican panel sample analyzed here, I could find 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 last survey in November 1999 than comparable women residing in a non-Progresa locality.[24] Nor was it evident that fertility of young women age 15–19 was affected by the school subsidy, for whom the opportunity cost of having a child would have been increased by the program (cf. Appendix A).

No theoretical reason or empirical evidence is know for why other traditional poverty reduction programs, such as income-support welfare systems or price-support agricultural programs, would encourage investments in human capital or promote a more efficient allocation of private or social resources. Indeed, both of these common forms of poverty alleviation programs are linked 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 initial design of Progresa. But if the program becomes a permanent entitlement for the more than two million rural poor families it currently serves, it may become politically necessary to monitor periodically the income of beneficiaries and make the program means-tested. Such a change in administration opens the door to the traditional distortions on labor supply behavior that have plagued poverty programs in other settings.

Although it is not always a politically popular feature of a welfare program geographically focused on poor areas, an advantage of Progresa is that it should help the children of poor Mexican farmers find a better place to work, by encouraging them to

[24] 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 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-4, 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 20–49 with the additional control for the woman's age squared, and for the 5-year age brackets 20–24, 25–29, etc., with only the linear age control variable. 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% 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 short window of time for which a program effect on fertility could be anticipated, I would conclude that there are no consistent and statistically significant effects of the program on fertility.

invest in well-rewarded schooling, which, in turn, facilitates their migration away from their origin communities to other parts of the Mexican economy where wages and long-term career prospects are better. Thus, it should be 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 (Bouillon et al., 1999). Subsidizing schooling among the rural poor may thus be a development strategy that deserves more widespread consideration as a geographically and economically targeted policy which can both reduce entrenched intergenerational transmission of poverty and promote long-term economic growth.

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Appendix A

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	33,795	36,390	13,872	14,523	55,396	59,344	25,761	26,696
Enrollment	0.942	0.937	0.674	0.730	0.896	0.898	0.578	0.635
Attendance[d]	0.972	0.971	0.981	0.980	0.970	0.968	0.982	0.978
Progresa Locality	0.605	0.613	0.600	0.625	0.612	0.618	0.606	0.629
Eligible (poor)	0.733	0.735	0.603	0.622	0.726	0.731	0.587	0.592
Progresa × Eligible	0.454	0.462	0.369	0.408	0.448	0.456	0.362	0.383

Table A-1 (*continued*)

Variable name	Sample 1—panel				Sample 2—pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
Completed schooling								
0	0.127	0.120			0.183	0.172		
1	0.169	0.173			0.175	0.185		
2	0.181	0.187			0.167	0.170		
3	0.188	0.186			0.171	0.172		
4	0.173	0.171			0.155	0.155		
5	0.161	0.163			0.148	0.149		
6			0.557	0.504			0.551	0.491
7			0.200	0.220			0.166	0.185
8			0.139	0.160			0.135	0.157
9 or More			0.104	0.116			0.148	0.167
Age of child								
6	0.068	0.063	0.000	0.000	0.090	0.083	0.000	0.000
7	0.115	0.110	0.000	0.000	0.124	0.120	0.000	0.000
8	0.152	0.151	0.000	0.000	0.138	0.135	0.000	0.000
9	0.155	0.148	0.000	0.000	0.133	0.129	0.000	0.000
10	0.165	0.157	0.002	0.001	0.142	0.140	0.001	0.001
11	0.142	0.135	0.029	0.031	0.123	0.120	0.022	0.023
12	0.098	0.106	0.162	0.142	0.089	0.096	0.121	0.107
13	0.047	0.057	0.249	0.225	0.047	0.054	0.192	0.172
14	0.027	0.035	0.246	0.254	0.032	0.037	0.207	0.211
15	0.014	0.020	0.189	0.203	0.024	0.028	0.211	0.215
16	0.007	0.009	0.104	0.123	0.020	0.022	0.187	0.204
17–18	0.001	0.002	0.019	0.020	0.006	0.007	0.057	0.064
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	0.047	0.049	0.047	0.048	0.062	0.062	0.068	0.061
Father not present	0.103	0.108	0.108	0.114	0.127	0.125	0.132	0.130
School characteristics								
Primary school student/teacher ratio[a]	17.1 (14.8)	17.0 (14.0)	15.8 (13.6)	16.0 (13.9)	17.0 (15.2)	17.0 (15.2)	16.5 (13.8)	16.0 (14.0)
No information on primary school	0.312	0.307	0.312	0.313	0.325	0.321	0.316	0.313
School characteristics								
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	0.022	0.016	0.009	0.008	0.029	0.024	0.011	0.010

(continued on next page)

Table A-1 (*continued*)

Variable name	Sample 1—panel				Sample 2—pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
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	0.021	0.022	0.017	0.026	0.029	0.031	0.026	0.032
No wage for women	0.562	0.570	0.583	0.575	0.549	0.565	0.576	0.568

[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 $\left(\text{S.D.} = \sqrt{\text{mean}(1 - \text{mean})}\right)$.

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

[c] Secondary sample includes all children age 6–16 who have completed 6–9 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% of all girls report an attendance rate of 97.2%. But of those 94.2% who are reported to be enrolled, 21.4% 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 – 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).

A.1. Evidence of the cross-effects of Progresa on child work and fertility

By reducing the price of schooling for children in poor families, the Progresa program may affect the demand of these families for a variety of related goods and behaviors. According to the Slutsky decomposition of the resulting household demands, derived from a simple static model, the effect of the school subsidy 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. Some advocates of policies to reduce child labor anticipate that decreasing child labor would increase the schooling of children and thereby improve the child's future economic opportunities (i.e., the income uncompensated effect of a ban on child labor which reduced child wage opportunities would increase child enrollment in school). This assumes child schooling and work are substitutes (i.e., a positive income-compensated cross-price effect) and that this cross-price effect dominates the income effect which would reduce the demand for schooling and other normal goods.

The interrelationship between the school subsidy and child labor, and hence the child's contribution to family income, is therefore germane to an evaluation of the consumption benefits of Progresa. At one extreme, suppose that the school subsidy leads to a reduction in the value of child labor to the family which wholly offsets the subsidy. Then the poor family benefitting from the Progresa program which decided to increase the enrollment of their children in school would experience no increase in current consumption, but could only hope to benefit from the human capital of their children in the future. It is not possible here to precisely value the child labor adjustment attributed to the program, but two approaches for estimating the child labor supply response are investigated.

First, the differences in means of child labor supply between the treatment and control poor populations can be estimated. The post-program treatment control difference in the mean child labor supply outcome variables can be attributed to the treatment offer in the localities where the Progresa educational grants were provided, compared to where they were not, assuming the treatment allocation was random. The second approach estimates a labor supply equation for the child work outcomes, including as possible determinants the control variables used to account for enrollment in the paper, and also conditioning on the child's dichotomous current enrollment decision. Because both the child enrollment and work decisions are likely to be affected by the same unobserved variables, such as the opportunity value of child labor in the community or the preferences of the parents, the enrollment will be correlated with the error in the child labor supply equations. Enrollment should then be treated as endogenous and possibly measured with error in the child labor supply equation. The Progresa program treatment and eligibility are used as instruments to predict endogenous enrollment, and that predicted variation in enrollment due to the random allocation of the program across localities, identifies the instrumental variable (IV) estimate of the program impact on child work behavior.

Unfortunately, the sequence of questions on child work are not identical over the five rounds of the survey, and to include household work in the broadest measure of child work, I restrict this analysis to survey rounds 3 and 5. Thus, these two post-program surveys are used to estimate the mean difference post-program. But estimates of the difference in difference between the pre-program and post-program periods is not calculated, because household work was not measured in the pre-program period.

Child labor is peculiarly difficult to measure empirically. Household surveys in low-income countries often find a smaller proportion of children working than social observers expect to find. The Progresa census (October 1997) and subsequent surveys asked the respondent first whether a child age 8–16 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. The sum of these two responses is designated as "market work". A third question was added in rounds 3 and 5 (October 1998 and November 1999) to respondents who had answered "no" to the two previous questions. They were then asked whether the child was engaged in any housework. This permits the broadest definition of "market or household work". Finally, for each child in the paid labor force, the respondent was asked a fourth question: How many "hours per day" did the child "work for pay". The usable sample to analyze hours includes those not working for pay (i.e., zero hours) and those answering positive hours. This sample excludes a small fraction of children reported to be working for pay, but with a missing value to the hours question.

Table A-2 shows the proportion and number of children working in the paid labor force and reporting a wage in the initial October 1997 Census, tabulated by the child's gender and by age from 8 to 16, and their mean reported wage. About twice as many males as females participate in the paid labor force, and male wages tend to be higher than female wages among the youngest children, and wages are roughly equal between boys and girls age 12 or more. These average reported wages should not be interpreted as an unbiased or precisely defined indicator of what the average child could obtain as a wage if they decided to work, due to sample selection bias, but it may be noted that reported earnings of children appear to exceed the Progresa educational grants (cf. Table 1).

The sample means for the three dichotomous measures of child work, and the hours worked for pay, are reported in the bottom row of Table A-3 for each subsample: female and male children in the primary and secondary school panel samples, as previously analyzed. Column 2 shows that 2.4% of the girls in the primary school sample work in the market, and 7.5% of the boys work in the market. Of the girls qualified for secondary school, 7.7% work in the market, while 26.1% of the boys work. Primary girls and boys work in paid employment on average for 0.08 and 0.35 h/day, whereas secondary girls and boys work 0.44 and 1.59 h/day, respectively. Dividing these entire sample average hours by the participation rate in paid work, one sees that the small fraction of primary school prepared girls and boys who do work for pay tend to work full time, or 8.17 (i.e., 0.0833/ 0.0102 = 0.0817) and 7.99 hours a day, respectively. Similarly, for secondary school children who work in paid employment their average hours of work is 8.21 and 7.99 for girls and boys, respectively. Although children who work tend to work full time, there is still substantial variation in hours worked among those in the paid labor force, and some secondary school children enrolled in school are also working, particularly among the boys. When the participation of children in housework is included in column 1 of Table A-

Table A-2
All children in October 1997 household census of all 500 Progresa evaluation villages

Age	Proportion (samples size) in paid labor force		Average monthly wage pesos (20 days)	
	Female	Male	Female	Male
8	0.003 (1751)	0.006 (1888)	178	353
9	0.004 (1686)	0.007 (1699)	99	350
10	0.008 (1802)	0.014 (1920)	184	373
11	0.007 (1782)	0.021 (1745)	607	346
12	0.022 (1710)	0.053 (1898)	387	420
13	0.040 (1674)	0.098 (1737)	467	413
14	0.066 (1612)	0.187 (1721)	538	482
15	0.115 (1604)	0.305 (1706)	584	593
16	0.151 (1518)	0.438 (1564)	637	599

3, the primary school girl's participation rises to 12.0%, and that of secondary school girls rises to 31.2%, roughly equivalent to that of boys. As expected, housework is a more common activity among girls than boys, and market work is conversely a more common activity among boys than girls.

A.2. Differences in means associated with Progresa

The measured difference between the child work variables in the treatment and control communities is presented as a reduced form estimate of the program's impact. It is derived from estimating a probit or linear OLS or Tobit model for each work outcome which includes as explanatory variables those used in the previous study of enrollment: a dummy for the fifth (versus the third) survey round, dummies for age and years of education completed, eligible for the Progresa education grants (i.e., E or poor), residing in a Progresa locality (P), and the interaction between the last two variables (PE). The sum of the coefficients on the P and PE variables, reported in the first row of estimates in Table A-3, is the difference in means of the work variables attributable to the Progresa program, and beneath the difference in means is the probability that this would occur randomly based on a joint statistical test that the sum of the two coefficients is equal to zero in the probit, OLS, or Tobit model, respectively.

All of the differences in child work between treatment and control populations are negative, as expected, and they are statistically significant at least at the 10% level for the probability of paid work (column 3, Table A-3) for primary school females and males and for secondary school males, for household and market work (column 1, Table A-3) for secondary school females, for paid work for secondary school males, for the OLS hours (column 4) for primary school boys, and for the Tobit hours (column 5) for primary school females and males and secondary school males. These difference estimates suggest that secondary females work 4.1 percentage points less in household and market work and secondary school males work 2.6 percentage points less in market work, 2.0 percentage points less in paid work, and 0.16 h less per day (according to the Tobit specification) in Progresa localities than in the control localities. Primary school children engage in less market and paid work, and their hours in paid work declines by 0.03 and 0.07 h/day, for females and males, respectively. Other investigators of the Progresa survey data have also found small reductions in child work associated with the program treatment (Gomez de Leon and Parker, 1999, 2000). These unrestricted reduced form estimates are in the anticipated direction, and are of reasonable magnitude for at least the secondary school children.

A.3. Estimates of child labor supply conditional on school enrollment

The second approach for evaluating the effect of the Progresa program on child work estimates the determinants of child labor supply variables, but also includes as a possible determinant of child labor supply the child's contemporaneous school enrollment. In the second row of estimates in Table A-3, the enrollment variable is treated as exogenous and measured without error. The third row of estimates is based on instrumental variable

Table A-3
Estimates of Progresa program effects on child work from surveys collected in October 1998 and November 1999

	Work market and household	Work market	Paid work participation	Hours of paid work	
				Linear	Tobit[a] (expected value)
Estimation method	Probit (1)	Probit (2)	Probit (3)	OLS (4)	Tobit (5)
Primary school females					
Sample size	16,384	16,384	16,156	16,156	16,156
Reduced form program effect mean difference of program (*p* value)	– 0.0040 (0.69)	– 0.0039 (0.25)	– 0.0007* (0.069)	– 0.0276 (0.13)	– 0.0313* (0.067)
Conditional effect of enrollment assumed exogenous (*p* value)	– 0.194** (0.000)	– 0.0367** (0.000)	– 0.0085** (0.000)	– 0.427** (0.000)	– 0.154** (0.000)
Assumed endogenous program instruments (*p* value)	– 0.148** (0.000)	– 0.0258* (0.061)	– 0.0040** (0.008)	– 0.658** (0.000)	– 0.218** (0.000)
Mean of dependent variable (standard deviation)	0.120 (0.325)	0.0240 (0.153)	0.0102 (0.100)	0.0833 (0.861)	0.0833 (0.861)
Primary school males					
Sample size	17,844	17,844	17,271	17,271	17,271
Reduced form program effect mean difference of program (*p* value)	– 0.0132 (0.17)	– 0.0053 (0.33)	– 0.0031* (0.063)	– 0.0778* (0.061)	– 0.0671** (0.049)
Conditional effect of enrollment assumed exogenous (*p* value)	– 0.289** (0.000)	– 0.249** (0.000)	– 0.133** (0.000)	– 2.30** (0.000)	– 0.669** (0.000)
Assumed endogenous program instruments (*p* value)	– 0.188** (0.000)	– 0.120** (0.000)	– 0.0179** (0.017)	– 2.22** (0.000)	– 0.237 (0.23)
Mean of dependent variable (standard deviation)	0.109 (0.312)	0.0750 (0.263)	0.0442 (0.206)	0.353 (1.66)	0.353 (0.166)

Secondary school females					
Sample size	12,230	12,230	11,927	11,927	11,927
Reduced form program effect mean difference of program (p value)	− 0.0406** (0.024)	− 0.0044 (0.55)	− 0.0048 (0.31)	− 0.0571 (0.35)	− 0.0623 (0.27)
Conditional effect of enrollment assumed exogenous (p value)	− 0.365** (0.000)	− 0.0772** (0.000)	− 0.0571** (0.000)	− 0.657** (0.000)	− 0.760** (0.000)
Assumed endogenous program instruments (p value)	− 0.463** (0.000)	− 0.128** (0.002)	− 0.0527* (0.067)	− 1.44** (0.000)	− 0.627 (0.14)
Mean of dependent variable (standard deviation)	0.313 (0.464)	0.0767 (0.266)	0.0531 (0.224)	0.435 (1.88)	0.435 (1.88)
Secondary school males					
Sample size	12,822	12,822	11,848	11,848	11,848
Reduced form mean difference of program (p value)	− 0.0222 (0.19)	− 0.0256* (0.094)	− 0.0200* (0.081)	− 0.143 (0.15)	− 0.161* (0.080)
Conditional effect of enrollment assumed exogenous (p value)	− 0.428** (0.000)	− 0.407** (0.000)	− 0.337** (0.000)	− 3.18** (0.000)	− 2.83** (0.000)
Assumed endogenous program instruments (p value)	− 0.389** (0.000)	− 0.280** (0.000)	− 0.101 (0.22)	− 5.42** (0.000)	− 1.00 (0.15)
Mean of dependent variable (standard deviation)	0.302 (0.459)	0.261 (0.439)	0.199 (0.400)	1.59 (3.25)	1.59 (3.25)

[a] The derivative of the expected value function implied by the Tobit model is evaluated at the sample means to provide an analogous estimate to the linear OLS specification for the hours labor supply.

* Ten percent significance level.

** Five percent significance level.

methods in which enrollment is treated as endogenous. This preferred set of estimates corrects for any bias introduced by the heterogeneity of families, e.g., preferences affecting both of the coordinated allocations of a child's time between work and school, and for classical errors in the measurement of the dichotomous enrollment variable. All of the demographic, schooling, family, and community control variables included in the probit models of enrollment as reported in the paper are also included here as potential determinants of child work, except for the program and program $\times$ eligible interaction variables, i.e., P and $P \times E$. The critical assumption justifying this estimation strategy is that allocation of poor children between the treatment and control localities is random and hence orthogonal to unobserved factors and heterogeneity that might influence child labor and the measurement error in enrollment. Hausman specification tests are then consulted to assess whether the estimated effects of enrollment differ significantly between the second and third rows in which enrollment is treated as exogenous and endogenous, respectively.

Assuming enrollment is exogenous and measured without error, enrollment is significantly related to all of the child work outcomes in row 2, and these estimates of the partial derivatives of work with respect to enrollment might be combined with estimates of the program effects on enrollment in Table 5 of the paper to evaluate the two-stage effect of the program on child work. The IV estimates in the third row are based on more realistic assumptions. In all 20 gender/school samples and measures of child labor, the IV estimates are negative, and in 13 out of the 20 estimates, they are significant at the 1% level, with an additional 3 cases significant at the 10% level. The Hausman specification tests do not reject exogeneity of enrollment in the primary school work probits, but do reject exogeneity for the secondary school females and males in the market and market plus household work probit models, and occasionally for paid work and hours models. In all cases the instruments jointly explain a significant share of the unexplained variation in enrollment, but probably because the program explains a proportionately small increment in enrollment at the primary level, the Hausman tests fail to reject the exogeneity of primary enrollment.

For primary school females, the IV estimates in Table A-3 imply school enrollment reduces work in the household or market by 14.8 percentage points, by 2.6 percentage points for market work alone, 0.4 percentage points in paid market work, 0.66 fewer hours per day, according to the OLS linear specification, and 0.22 fewer hours, according the Tobit nonlinear specification (in terms of the derivative in the expected value locus evaluated at sample means). Based on my estimate that Progresa increased primary school girl enrollment rates by 0.92 percentage points (Table 5), the IV estimate of the program's effect on household and market work is a reduction of 0.14 percentage points ($0.148 \times 0.0092 = 0.0014$), which might be contrasted to the sample mean of 12 percentage points, or a reduction in this broadest measure of child labor supply of 1.2 percentage points for girls. The parallel calculation leads to similar magnitudes for IV estimates of the program's effect on the labor supply of primary school boys. Their household and market work is about 0.15 percentage points lower due to the program (0.188×0.0080), compared with their sample mean of 10.9%, which represents a 1.4% reduction.

For secondary school females, the IV estimates imply that enrollment in school is associated with a reduction in their probability of working in the market or household by 46 percentage points, in market work by 13 percentage points, in paid work by 5.3 percentage points, and reduce hours worked by 1.44 and 0.61 h/day, depending on whether the linear OLS or nonlinear Tobit specification is consulted. With the program's effect on secondary school female enrollment being 0.092 (Table 5), the labor supply effects of the program are estimated to be 4.3 percentage points in household and market work, 1.18 in market, and 0.49 in paid labor, and a reduction in hours per day paid work of 0.13 and 0.06, respectively. Secondary school males evidence 39% lower household and market participation if they are enrolled in school, 28% lower participation in the market, 10.1% less in paid work, and hours reduction per day by 5.42–0.93, according to the linear and nonlinear hours equation. Because Progresa appears to have increased enrollment rates for secondary school males by 0.062 (Table 5), the program can be attributed an impact of reducing household and market work by 2.4 percentage points for secondary school males, market work by 1.7, paid work by 0.62 percentage points, and paid work by 0.33 and 0.06 h/day, depending on which hours specification is used.

A.4. Effects of Progresa on fertility

Table A-4 reports a reduced form estimate of the program's potential effect on fertility behavior of parents as measured between the fourth and fifth rounds of the surveys, at which time parents could have modified their conception rate in response to the Progresa transfers and affected their birth rate. There is no evidence from variation in this 6-month rate of births to suggest that the income-uncompensated reduction in the price of schooling offered by Progresa had any significant effect on fertility, as noted in the conclusion of the

Table A-4
Derivatives implied by probit estimates of the probability of birth in 6 months prior to November 1999 with respect to program eligibility, 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]
Progresa locality[c]	−0.0024	[d]	−0.0246	−0.0221	0.0173	−0.0226	0.0012
	(0.27)		(1.16)	(1.20)	(0.79)	(0.32)	(0.40)
Progresa × Poor[c]	0.0057	0.0459	0.0085	0.0484	0.0031	−0.0015	−0.0027
	(0.57)	(1.49)	(0.31)	(1.69)	(0.10)	(0.16)	(0.94)
Total effect of Progresa	0.0033	0.0459	−0.0161	−0.0263	0.0204	−0.0041	−0.0015
program[c] [significance]	[0.62]	[0.14]	[0.18]	[0.23]	[0.33]	[0.42]	[0.33]
Mean of birth rate	0.0411	0.0615	0.0655	0.0468	0.0335	0.0138	0.0027
Sample size	17,434	3661	3327	2972	2803	2457	2214

[a] Probit maximum likelihood estimates with cluster occurrence weighting for heteroscedasticity (Huber, 1967). Other controls include age, years of mother's education, poor, and a quadratic term for age for the sample for all age groups combined. 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] The program eligibility variables are also included in linear form. The coefficients reported here are for these variables interacted with a dummy equal to one if the woman has a child age 6–16 who has completed 2–8 years of schooling and is thus eligible for an educational grant.

[d] 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.

paper. This finding is not inconsistent with other studies that have estimated small positive (uncompensated) cross-price effect from schooling to fertility, or the cross-price effect in the other direction, from fertility to schooling using the approximately random variation in fertility associated with the occurrence of twins (Rosenzweig and Wolpin, 1980, 1982, Schultz, 1997).

To assess the possible effect of Progresa on the fertility of girls who could themselves be influenced by the educational grants, I first define a youth fertility sample as all girls prepared to enroll in secondary school who are age 14–18 who could respond to the child education questionnaire in the fifth round of the Progresa evaluation survey in November 1999. This initial sample includes 4698 girls. A probit model is estimated for the likelihood that these girls had a birth in the previous intersurvey interval (in the last 6 months), as a function of their living in a poor household (E), and age dummies for 14, 15, 16, or 17 and 18. When the variables for residing in a Progresa locality (P) and in a poor household who is eligible for a Progresa educational grant (PE) are also included as determinants of fertility in a reduced-form probit model, the joint derivative effect of Progresa treatment for the poor is to reduce the likelihood of a birth by -0.0012 or 1.2 per thousand, compared with the average birth rate is 5.3 per thousand per 6 months in this sample. But this estimate of the program effect is not significantly different from zero (p>0.59). When the actual current enrollment of the child is included instead of the P and PE variables, based on the strong working assumptions that enrollment is exogenous and not subject to measurement error, the estimated derivative of a birth with respect to enrollment is -0.0063 ($z=3.41$), which is significantly negative. When enrollment is instrumented by P and PE, correcting for its likely endogeneity and classical measurement error, the derivative of the birth probability with respect to Progresa-induced change in enrollment increases to -0.0078 ($z=2.29$), and remains significantly different from zero.

Since there are a few girls age 14 to 18 who are only prepared to attend primary school, but have births in this six month interval, and they could qualify for a Progresa educational grants to enroll in primary school, they be added to the initial secondary school prepared sample. This expanded sample defined on age includes 5625 girls for whom the overall birth rate is slightly higher, 6.0 per thousand. Again, the reduced-form differenced specification suggests the girls in poor households in Progresa localities report slightly lower birth rates, -0.0010, but the difference is not significant (p>0.61). When enrollment is included as though it were exogenous and measured without error, the estimated derivative of fertility with respect to enrollment is -0.0062 (3.35), whereas when enrollment is instrumented by P and PE, the derivative of Progresa's effect on fertility is estimated to be -0.0098 (2.52), and is again significantly different from zero.

Although the reduced-form difference estimator of the youth fertility effect of the program is not significantly different from zero, the instrumental variable estimate implies the program's impact has been to reduce teenage birth rates, and this effect is significant at the 1–2% level. If Progresa is associated with a 9.2% increase in enrollment of secondary school girls (Table 5 panel sample), the instrumental variable estimate of the program's impact on fertility for this group is -0.00072 (0.092×-0.0078) which would represent a reduction in the birth rate for this group of teenagers of about 14% ($-0.00072/0.00532$). This short-run effect on fertility might exceed the long-run effect on lifetime cohort

Table B-1
Derivatives from probit estimates of enrollment: female primary school panel sample

Probit estimates	Number of observations = 33,795
	χ^2 (40) = 2354.29
	Prob > χ^2 = 0.0000
Log-likelihood = − 4675.5567	Pseudo R^2 = 0.3728
	(Standard errors adjusted for clustering on eml)

enrolled	d*F*/d*x*	Robust standard error	*z*	$P>\|z\|$	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.0005991	0.0050085	0.12	0.904	0.605326	− 0.009217	0.010416
pobre*	− 0.0091855	0.0030357	− 2.70	0.007	0.732742	− 0.015135	− 0.003236
bp*	− 0.0049181	0.0062675	− 0.80	0.422	0.454032	− 0.017202	0.007366
age6*	0.0101409	0.0021239	3.93	0.000	0.067969	0.005978	0.014304
age7*	0.010305	0.0022765	3.84	0.000	0.115283	0.005843	0.014767
age9*	− 0.0118989	0.0041963	− 3.31	0.000	0.155496	− 0.020123	− 0.003674
age10*	− 0.035829	0.007483	− 6.76	0.000	0.164729	− 0.050496	− 0.021163
age11*	− 0.0813027	0.0118946	− 11.24	0.000	0.14227	− 0.104616	− 0.05799
age12*	− 0.1866315	0.020014	− 17.49	0.000	0.098003	− 0.225858	− 0.147405
age13*	− 0.3426408	0.0309285	− 20.56	0.000	0.046516	− 0.403259	− 0.282022
age14*	− 0.5534184	0.034298	− 25.97	0.000	0.027282	− 0.620641	− 0.486196
age15*	− 0.6935964	0.0316237	− 27.83	0.000	0.014262	− 0.755578	− 0.631615
age16*	− 0.816762	0.0343812	− 22.17	0.000	0.006983	− 0.884148	− 0.749376
age1718*	− 0.7936849	0.0747091	− 10.53	0.000	0.001332	− 0.940112	− 0.647258
t345bas*	0.0005362	0.004325	0.12	0.902	0.333836	− 0.007941	0.009013
t345bp*	0.0087485	0.0036148	2.17	0.030	0.252611	0.001664	0.015833
educ1*	0.0289533	0.0023014	10.79	0.000	0.169108	0.024443	0.033464
educ2*	0.0314978	0.0023913	13.28	0.000	0.181447	0.026811	0.036185
educ3*	0.0353253	0.0023514	15.85	0.000	0.188341	0.030717	0.039934
educ4*	0.0391307	0.0025298	18.81	0.000	0.172777	0.034172	0.044089
educ5*	0.0387645	0.002495	20.58	0.000	0.16103	0.033874	0.043655
bpeduc1*	− 0.0008193	0.0068787	− 0.12	0.904	0.081107	− 0.014301	0.012663
bpeduc2*	− 0.000396	0.0059857	− 0.07	0.947	0.085841	− 0.012128	0.011336
bpeduc3*	0.0053271	0.0045679	1.06	0.291	0.087143	− 0.003626	0.01428
bpeduc4*	0.0034131	0.0051181	0.62	0.533	0.075396	− 0.006618	0.013444
bpeduc5*	0.0119702	0.0029521	3.01	0.003	0.066341	0.006184	0.017756
t2*	0.0118151	0.0014636	7.26	0.000	0.22311	0.008947	0.014684
t3*	0.0016916	0.0022367	0.74	0.462	0.194555	− 0.002692	0.006075
t4*	0.010656	0.0018899	5.10	0.000	0.194555	0.006952	0.01436
t5*	0.0096671	0.0020672	4.03	0.000	0.16248	0.005615	0.013719
nomom*	− 0.0018819	0.0051319	− 0.38	0.704	0.046605	− 0.01194	0.008176
meduc	0.0025721	0.0004662	5.45	0.000	2.8535	0.001658	0.003486
nodad*	0.0043887	0.0031354	1.26	0.206	0.102767	− 0.001757	0.010534
deduc	0.001583	0.0004843	3.24	0.001	2.92854	0.000634	0.002532
no_p*	− 0.0106948	0.0048011	− 2.42	0.016	0.311762	− 0.020105	− 0.001285
nt_p	− 0.0003066	0.0001106	− 2.73	0.006	17.0827	− 0.000523	− 0.00009
nodissec*	− 0.0171667	0.0159229	− 1.38	0.166	0.022252	− 0.048375	0.014042
dis_sec	− 0.000124	0.0006483	− 0.19	0.848	2.09873	− 0.001395	0.001147
nearest	0.0000697	0.0000293	2.38	0.017	103.716	0.000012	0.000127
distance	0.0007093	0.0002408	2.91	0.004	9.61285	0.000237	0.001181
Observed *P*	0.9422992						
Predicted *P*	0.9808284	(at $\bar{x}$)					

z and $P>|z|$ are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table B-2
Derivatives from probit estimates of enrollment: male primary school panel sample

Probit estimates	Number of observations = 36,390
	χ^2 (40) = 2318.23
	Prob> χ^2 = 0.0000
Log-likelihood = − 5353.9019	Pseudo R^2 = 0.3712
	(Standard errors adjusted for clustering on eml)

enrolled	d*F*/d*x*	Robust standard error	*z*	*P*>|*z*|	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.0039613	0.0055419	0.73	0.467	0.612751	− 0.006901	0.014823
pobre*	− 0.008804	0.0033723	− 2.45	0.014	0.734927	− 0.015414	− 0.002194
bp*	− 0.0034616	0.0065349	− 0.53	0.595	0.46227	− 0.01627	0.009347
age6*	0.0122906	0.0024264	4.05	0.000	0.063314	0.007535	0.017046
age7*	0.011284	0.0022683	4.14	0.000	0.109975	0.006838	0.01573
age9*	− 0.0160782	0.0049875	− 3.87	0.000	0.1482	− 0.025853	− 0.006303
age10*	− 0.0358795	0.0073549	− 6.63	0.000	0.156801	− 0.050295	− 0.021464
age11*	− 0.0678652	0.0101301	− 10.06	0.000	0.135367	− 0.08772	− 0.048011
age12*	− 0.1241223	0.0153646	− 13.77	0.000	0.105826	− 0.154236	− 0.094008
age13*	− 0.2786195	0.0269473	− 18.55	0.000	0.057104	− 0.331435	− 0.225804
age14*	− 0.5169159	0.0313593	− 26.25	0.000	0.035339	− 0.578379	− 0.455453
age15*	− 0.7397824	0.0293936	− 27.61	0.000	0.019951	− 0.797393	− 0.682172
age16*	− 0.8828009	0.0199916	− 27.74	0.000	0.008821	− 0.921984	− 0.843618
age1718*	− 0.8980266	0.0350824	− 14.47	0.000	0.001594	− 0.966787	− 0.829266
t345bas*	− 0.0088439	0.0058294	− 1.62	0.105	0.337868	− 0.020269	0.002581
t345bp*	0.0168128	0.0037594	3.97	0.000	0.257186	0.009445	0.024181
educ1*	0.0335857	0.0025075	13.33	0.000	0.173482	0.028671	0.0385
educ2*	0.038359	0.0027413	15.10	0.000	0.187222	0.032986	0.043732
educ3*	0.0412708	0.0029043	16.40	0.000	0.186397	0.035579	0.046963
educ4*	0.0427542	0.0028875	19.03	0.000	0.170871	0.037095	0.048414
educ5*	0.0449046	0.0029803	20.59	0.000	0.162517	0.039063	0.050746
bpeduc1*	0.0070622	0.0053909	1.14	0.254	0.085133	− 0.003504	0.017628
bpeduc2*	− 0.0005051	0.0065813	− 0.08	0.938	0.087854	− 0.013404	0.012394
bpeduc3*	0.0035112	0.005734	0.58	0.565	0.086837	− 0.007727	0.01475
bpeduc4*	0.0005793	0.0062708	0.09	0.927	0.07601	− 0.011711	0.01287
bpeduc5*	0.0039875	0.0055699	0.67	0.506	0.070431	− 0.006929	0.014904
t2*	0.01398	0.0015253	8.52	0.000	0.224512	0.010991	0.016969
t3*	0.0067593	0.0023241	2.62	0.009	0.195383	0.002204	0.011314
t4*	0.0104273	0.0023487	3.82	0.000	0.195383	0.005824	0.015031
t5*	0.0077035	0.0025246	2.79	0.005	0.162242	0.002755	0.012652
nomom*	0.0031052	0.0044933	0.65	0.513	0.048722	− 0.005702	0.011912
meduc	0.0014324	0.0005556	2.55	0.011	2.78549	0.000343	0.002521
nodad*	− 0.0002422	0.0044755	− 0.05	0.957	0.108272	− 0.009014	0.00853
deduc	0.0022635	0.0005733	3.96	0.000	2.87667	0.00114	0.003387
no_p*	− 0.0196164	0.0064108	− 3.41	0.000	0.307117	− 0.032181	− 0.007051
nt_p	− 0.0004487	0.0001467	− 3.00	0.003	17.0041	− 0.000736	− 0.000161
nodissec*	− 0.0110868	0.0106312	− 1.22	0.224	0.016763	− 0.031924	0.00975
dis_sec	− 0.0008906	0.0007367	− 1.19	0.234	2.09392	− 0.002334	0.000553
nearest	0.0001776	0.0000343	5.44	0.000	104.69	0.00011	0.000245
distance	0.0007966	0.0002481	3.18	0.001	9.51008	0.00031	0.001283
Observed *P*	0.9374279						
Predicted *P*	0.9776177	(at $\bar{x}$)					

z and *P*>|*z*| are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table B-3
Derivatives from probit estimates of enrollment: female secondary school panel sample

Probit estimates			Number of observations = 13,872				
			χ^2 (30) = 2020.18				
			Prob>χ^2 = 0.0000				
Log-likelihood = – 6029.1331			Pseudo R^2 = 0.3116				
			(Standard errors adjusted for clustering on eml)				

enrolled	dF/dx	Robust standard error	z	P>|z|	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.0405425	0.0336259	1.22	0.224	0.600058	– 0.025363	0.106448
pobre*	– 0.0436791	0.0205576	– 2.09	0.036	0.603374	– 0.083971	– 0.003387
bp*	– 0.03463	0.0550528	– 0.64	0.525	0.369377	– 0.142531	0.073271
age12*	– 0.1514779	0.0331496	– 4.87	0.000	0.162053	– 0.21645	– 0.086506
age13*	– 0.3322022	0.0337203	– 10.33	0.000	0.248558	– 0.398293	– 0.266112
age14*	– 0.5142785	0.0325735	– 15.02	0.000	0.246035	– 0.578121	– 0.450436
age15*	– 0.6600854	0.0265272	– 19.16	0.000	0.189735	– 0.712078	– 0.608093
age16*	– 0.7124782	0.0206369	– 20.18	0.000	0.103518	– 0.752926	– 0.672031
age1718*	– 0.7155139	0.0152419	– 17.88	0.000	0.018599	– 0.745387	– 0.68564
t345bas*	– 0.02322	0.0287075	– 0.81	0.417	0.430868	– 0.079486	0.033046
t345bp*	0.1155239	0.0250349	4.32	0.000	0.269319	0.066456	0.164591
educ6*	– 0.2409196	0.0267488	– 8.84	0.000	0.55731	– 0.293346	– 0.188493
educ7*	0.2486267	0.0200901	8.75	0.000	0.199611	0.209251	0.288002
educ8*	0.2699232	0.0135224	12.40	0.000	0.139057	0.24342	0.296427
bpeduc6*	0.0321777	0.0432794	0.73	0.467	0.220949	– 0.052648	0.117004
bpeduc78*	– 0.023793	0.055627	– 0.44	0.663	0.120963	– 0.13282	0.085234
t2*	0.0838579	0.0084327	9.34	0.000	0.140787	0.06733	0.100386
t3*	0.0634646	0.0163512	3.68	0.000	0.210352	0.031417	0.095512
t4*	0.1185385	0.0157959	6.73	0.000	0.210352	0.087579	0.149498
t5*	0.0421372	0.0179992	2.29	0.022	0.295776	0.00686	0.077415
nomom*	0.0443595	0.0264707	1.60	0.110	0.047217	– 0.007522	0.096241
meduc	0.0132353	0.0028711	4.65	0.000	2.70603	0.007608	0.018863
nodad*	0.0188146	0.0217676	0.85	0.396	0.107987	– 0.023849	0.061478
deduc	0.0142401	0.0030241	4.69	0.000	2.75353	0.008313	0.020167
no_p*	– 0.0057304	0.0301929	– 0.19	0.849	0.312356	– 0.064907	0.053446
nt_p	– 0.0003558	0.0009758	– 0.36	0.715	15.8167	– 0.002268	0.001557
nodissec*	0.0055239	0.0814528	0.07	0.946	0.008578	– 0.154121	0.165168
dis_sec	– 0.0248183	0.0056558	– 4.43	0.000	2.02796	– 0.035903	– 0.013733
nearest	0.0011056	0.0002294	4.84	0.000	103.782	0.000656	0.001555
distance	0.000032	0.0015741	0.02	0.984	9.75085	– 0.003053	0.003117
Observed P	0.6738754						
Predicted P	0.7523877	(at $\bar{x}$)					

z and P>|z| are the test of the underlying coefficient being 0.

* dF/dx is for discrete change of dummy variable from 0 to 1.

Table B-4
Derivatives from probit estimates of enrollment: male secondary school panel sample

Probit estimates			Number of observations = 14,523				
			χ^2 (30) = 1938.30				
			Prob>χ^2 = 0.0000				
Log-likelihood = – 5947.3023			Pseudo R^2 = 0.2979				
			(Standard errors adjusted for clustering on eml)				
enrolled	d*F*/d*x*	Robust standard error	*z*	*P*>|*z*|	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.0559566	0.0307648	1.86	0.063	0.624595	– 0.004341	0.116255
pobre*	0.0003513	0.0181602	0.02	0.985	0.621979	– 0.035242	0.035945
bp*	– 0.0705952	0.0420544	– 1.71	0.087	0.407974	– 0.15302	0.01183
age12*	– 0.1304121	0.0338961	– 4.28	0.000	0.142326	– 0.196847	– 0.063977
age13*	– 0.2635642	0.0361537	– 8.26	0.000	0.224678	– 0.334424	– 0.192704
age14*	– 0.4532515	0.036618	– 13.29	0.000	0.254217	– 0.525021	– 0.381482
age15*	– 0.6063485	0.0341645	– 16.67	0.000	0.203264	– 0.67331	– 0.539387
age16*	– 0.7167952	0.0273934	– 18.63	0.000	0.123597	– 0.770485	– 0.663105
age1718*	– 0.7580052	0.0172008	– 18.32	0.000	0.019899	– 0.791718	– 0.724292
t345bas*	0.0048251	0.0263577	0.18	0.855	0.450045	– 0.046835	0.056485
t345bp*	0.0571945	0.0219054	2.51	0.012	0.295876	0.014261	0.100128
educ6*	– 0.1482841	0.0232963	– 6.51	0.000	0.503684	– 0.193944	– 0.102624
educ7*	0.200679	0.0142887	10.91	0.000	0.220134	0.172674	0.228684
educ8*	0.2220405	0.0110085	13.60	0.000	0.159953	0.200464	0.243617
bpeduc6*	0.0261661	0.031101	0.82	0.412	0.218825	– 0.034791	0.087123
bpeduc78*	– 0.0230549	0.0350141	– 0.67	0.500	0.152792	– 0.091681	0.045571
t2*	0.0618833	0.0078104	7.57	0.000	0.141362	0.046575	0.077192
t3*	0.0166851	0.01706	0.97	0.334	0.21435	– 0.016752	0.050122
t4*	0.0529807	0.0163211	3.11	0.002	0.21435	0.020992	0.084969
t5*	0.0036213	0.0177548	0.20	0.839	0.290436	– 0.031177	0.03842
nomom*	0.0206746	0.0239201	0.84	0.402	0.047993	– 0.026208	0.067557
meduc	0.008676	0.0029834	2.89	0.004	2.6237	0.002829	0.014523
nodad*	0.0430884	0.0156975	2.58	0.010	0.113888	0.012322	0.073855
deduc	0.0187216	0.0028816	6.45	0.000	2.7798	0.013074	0.024369
no_p*	– 0.0602337	0.0264546	– 2.34	0.019	0.312608	– 0.112084	– 0.008384
nt_p	– 0.0017076	0.0006839	– 2.48	0.013	15.9699	– 0.003048	– 0.000367
nodissec*	– 0.1366813	0.0753636	– 2.04	0.041	0.008882	– 0.284391	0.011029
dis_sec	– 0.0177838	0.0041573	– 4.30	0.000	2.05403	– 0.025932	– 0.009636
nearest	0.0011736	0.000172	6.76	0.000	105.207	0.000837	0.001511
distance	– 0.000171	0.0013106	– 0.13	0.896	9.42102	– 0.00274	0.002398
Observed *P*	0.7300145						
Predicted *P*	0.8097157	(at $\bar{x}$)					

z and *P*>|*z*| are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table B-5
Derivatives from probit estimates of enrollment: female primary school pooled sample

Probit estimates			Number of observations = 55,396				
			χ^2 (40) = 5865.94				
			Prob > χ^2 = 0.0000				
Log-likelihood = − 10,425.151			Pseudo R^2 = 0.4340				
			(Standard errors adjusted for clustering on eml)				
enrolled	dF/dx	Robust standard error	z	P>|z|	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.000215	0.0065292	0.03	0.974	0.611506	− 0.012582	0.013012
pobre*	− 0.0122756	0.0039773	− 2.92	0.003	0.72628	− 0.020071	− 0.00448
bp*	− 0.0042938	0.007191	− 0.60	0.548	0.448426	− 0.018388	0.0098
age6*	0.0236569	0.0021514	9.10	0.000	0.089934	0.01944	0.027874
age7*	0.0196671	0.0025247	6.49	0.000	0.124847	0.014719	0.024615
age9*	− 0.0109849	0.0048646	− 2.47	0.014	0.133493	− 0.020519	− 0.00145
age10*	− 0.0468052	0.007541	− 8.13	0.000	0.1427	− 0.061585	− 0.032025
age11*	− 0.0962844	0.0112957	− 12.62	0.000	0.123493	− 0.118424	− 0.074145
age12*	− 0.2205828	0.0177372	− 21.07	0.000	0.088996	− 0.255347	− 0.185819
age13*	− 0.3944413	0.0241682	− 26.71	0.000	0.046989	− 0.44181	− 0.347073
age14*	− 0.6185884	0.0256296	− 32.39	0.000	0.032421	− 0.668822	− 0.568355
age15*	− 0.7968037	0.0166664	− 42.61	0.000	0.024153	− 0.829469	− 0.764138
age16*	− 0.8879588	0.0106488	− 43.81	0.000	0.019568	− 0.90883	− 0.867087
age1718*	− 0.904379	0.0142368	− 27.36	0.000	0.005813	− 0.932283	− 0.876475
t345bas*	0.0027867	0.0065432	0.42	0.672	0.339393	− 0.010038	0.015611
t345bp*	0.0098735	0.005825	1.58	0.114	0.252744	− 0.001543	0.02129
educ1*	0.0567026	0.003077	17.29	0.000	0.174507	0.050672	0.062733
educ2*	0.0579087	0.003013	21.01	0.000	0.16725	0.052003	0.063814
educ3*	0.0637999	0.0031698	21.92	0.000	0.171384	0.057587	0.070013
educ4*	0.0667777	0.0032523	24.20	0.000	0.155481	0.060403	0.073152
educ5*	0.069643	0.0033603	26.42	0.000	0.148314	0.063057	0.076229
bpeduc1*	− 0.0020465	0.0084322	− 0.25	0.805	0.081468	− 0.018573	0.01448
bpeduc2*	0.0030003	0.0069228	0.42	0.674	0.076793	− 0.010568	0.016569
bpeduc3*	0.0030168	0.0064972	0.45	0.651	0.077767	− 0.009717	0.015751
bpeduc4*	0.0072016	0.0068037	0.99	0.324	0.067027	− 0.006133	0.020537
bpeduc5*	0.0159785	0.004976	2.69	0.007	0.060744	0.006226	0.025731
t2*	0.0241022	0.0021865	10.17	0.000	0.182161	0.019817	0.028388
t3*	0.0079444	0.0030706	2.47	0.013	0.201278	0.001926	0.013963
t4*	0.0238663	0.002518	8.20	0.000	0.188245	0.018931	0.028801
t5*	0.0262796	0.0024784	8.87	0.000	0.165283	0.021422	0.031137
nomom*	− 0.0041534	0.0053494	− 0.81	0.421	0.062225	− 0.014638	0.006331
meduc	0.004514	0.0006982	6.61	0.000	2.70655	0.003146	0.005882
nodad*	0.0025438	0.0040231	0.62	0.537	0.126652	− 0.005341	0.010429
deduc	0.0028315	0.0006209	4.54	0.000	2.80091	0.001615	0.004048
no_p*	− 0.0252227	0.0072307	− 3.91	0.000	0.324554	− 0.039395	− 0.011051
nt_p	− 0.0005978	0.0001448	− 4.12	0.000	17.0097	− 0.000882	− 0.000314
nodissec*	− 0.0492674	0.0209471	− 3.12	0.002	0.028576	− 0.090323	− 0.008212
dis_sec	− 0.0011998	0.0011456	− 1.05	0.293	2.16133	− 0.003445	0.001046
nearest	0.0001795	0.0000425	4.24	0.000	102.918	0.000096	0.000263
distance	0.0006494	0.0004519	1.42	0.155	9.62955	− 0.000236	0.001535
Observed P	0.896599						
Predicted P	0.9611571	(at $\bar{x}$)					

z and P>|z| are the test of the underlying coefficient being 0.

* dF/dx is for discrete change of dummy variable from 0 to 1.

Table B-6
Derivatives from probit estimates of enrollment: male primary school pooled sample

Probit estimates	Number of observations = 59,344
	χ^2 (40) = 4983.56
	Prob> χ^2 = 0.0000
Log-likelihood = – 11,407.388	Pseudo R^2 = 0.4179
	(Standard errors adjusted for clustering on eml)

enrolled	d*F*/d*x*	Robust standard error	*z*	*P*>|*z*|	$\bar{x}$	[95% Confidence	Intervals]
basal*	0.0024683	0.0065654	0.38	0.705	0.618041	– 0.0104	0.015336
pobre*	– 0.014861	0.0044989	– 3.13	0.002	0.73052	– 0.023679	– 0.006043
bp*	0.0009888	0.0079176	0.12	0.901	0.456491	– 0.014529	0.016507
age6*	0.022978	0.002435	7.76	0.000	0.082805	0.018206	0.02775
age7*	0.0207692	0.002382	7.26	0.000	0.120484	0.016101	0.025438
age9*	– 0.0230648	0.0056482	– 4.74	0.000	0.129179	– 0.034135	– 0.011995
age10*	– 0.0518782	0.0076619	– 8.51	0.000	0.13961	– 0.066895	– 0.036861
age11*	– 0.0897213	0.0101697	– 12.26	0.000	0.119995	– 0.109654	– 0.069789
age12*	– 0.1680507	0.014857	– 17.80	0.000	0.096067	– 0.19717	– 0.138931
age13*	– 0.3401349	0.0220042	– 24.80	0.000	0.054193	– 0.383262	– 0.297008
age14*	– 0.5625444	0.0240447	– 32.54	0.000	0.036617	– 0.609671	– 0.515418
age15*	– 0.773629	0.0190071	– 36.85	0.000	0.028074	– 0.810882	– 0.736376
age16*	– 0.8954845	0.0103984	– 41.73	0.000	0.022429	– 0.915865	– 0.875104
age1718*	– 0.9260433	0.0092627	– 30.28	0.000	0.006791	– 0.944198	– 0.907889
t345bas*	– 0.0060709	0.0061841	– 1.00	0.316	0.344213	– 0.018191	0.00605
t345bp*	0.0179133	0.0048805	3.32	0.000	0.258206	0.008348	0.027479
educ1*	0.0576766	0.0028894	19.36	0.000	0.18108	0.052014	0.06334
educ2*	0.062924	0.00302	23.61	0.000	0.170211	0.057005	0.068843
educ3*	0.0669717	0.0032705	25.10	0.000	0.17193	0.060562	0.073382
educ4*	0.0688342	0.0032409	28.02	0.000	0.155163	0.062482	0.075186
educ5*	0.0726611	0.0034219	30.08	0.000	0.149333	0.065954	0.079368
bpeduc1*	0.0113956	0.0056359	1.81	0.071	0.087271	0.000349	0.022442
bpeduc2*	0.0016114	0.0066543	0.24	0.811	0.078643	– 0.011431	0.014654
bpeduc3*	0.0082768	0.0065019	1.17	0.241	0.078593	– 0.004467	0.02102
bpeduc4*	0.0001988	0.0077422	0.03	0.980	0.067808	– 0.014976	0.015373
bpeduc5*	0.0052754	0.0071048	0.70	0.481	0.063831	– 0.00865	0.019201
t2*	0.0259006	0.0020088	11.83	0.000	0.182984	0.021963	0.029838
t3*	0.0119313	0.0027544	3.99	0.000	0.201031	0.006533	0.01733
t4*	0.0205242	0.0025818	6.67	0.000	0.188983	0.015464	0.025585
t5*	0.0225325	0.0026449	7.84	0.000	0.167414	0.017348	0.027716
nomom*	– 0.0041005	0.0049898	– 0.85	0.395	0.061641	– 0.01388	0.005679
meduc	0.0034414	0.0007186	4.84	0.000	2.67951	0.002033	0.00485
nodad*	0.0026547	0.0041204	0.63	0.528	0.12532	– 0.005421	0.01073
deduc	0.0036605	0.0006614	5.51	0.000	2.75674	0.002364	0.004957
no_p*	– 0.0343626	0.0079701	– 4.80	0.000	0.321262	– 0.049984	– 0.018741
nt_p	– 0.0006979	0.0001776	– 3.88	0.000	16.9726	– 0.001046	– 0.00035
nodissec*	– 0.0285952	0.0210231	– 1.66	0.097	0.024468	– 0.0698	0.012609
dis_sec	– 0.0008256	0.0010849	– 0.76	0.448	2.15021	– 0.002952	0.001301
nearest	0.0002394	0.0000444	5.51	0.000	103.94	0.000152	0.000326
distance	0.0007646	0.0003953	1.91	0.056	9.59027	– 0.00001	0.001539
Observed *P*	0.8976476						
Predicted *P*	0.9594994	(at $\bar{x}$)					

z and *P*>\|*z*\| are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table B-7
Derivatives from probit estimates of enrollment: female secondary school pooled sample

Probit estimates	Number of observations = 25,761
	χ^2 (30) = 4388.92
	Prob > χ^2 = 0.0000
Log-likelihood = − 11,689.394	Pseudo R^2 = 0.3336
	(Standard errors adjusted for clustering on eml)

enrolled	d*F*/d*x*	Robust standard error	*z*	*P*>|*z*|	$\bar{x}$	[95% Confidence Intervals]	
basal*	0.039025	0.027427	1.43	0.154	0.605799	− 0.014731	0.092781
pobre*	− 0.0435689	0.0186049	− 2.33	0.020	0.586856	− 0.080034	− 0.007104
bp*	− 0.051462	0.0401413	− 1.29	0.198	0.361787	− 0.130137	0.027214
age12*	− 0.170714	0.0287113	− 6.01	0.000	0.121424	− 0.226987	− 0.114441
age13*	− 0.3745937	0.0257298	− 13.55	0.000	0.192151	− 0.425023	− 0.324164
age14*	− 0.5451964	0.0225147	− 19.03	0.000	0.206785	− 0.589324	− 0.501068
age15*	− 0.6613944	0.0171385	− 24.67	0.000	0.210978	− 0.694985	− 0.627804
age16*	− 0.7071438	0.0136358	− 27.74	0.000	0.187105	− 0.733869	− 0.680418
age1718*	− 0.6634658	0.0088608	− 29.80	0.000	0.057568	− 0.680833	− 0.646099
t345bas*	− 0.01476	0.0239871	− 0.62	0.538	0.387407	− 0.061774	0.032254
t345bp*	0.086097	0.0216085	3.88	0.000	0.238461	0.043745	0.128449
educ6*	− 0.3283489	0.0222507	− 13.88	0.000	0.551221	− 0.371959	− 0.284738
educ7*	0.275769	0.0198256	11.13	0.000	0.165677	0.236911	0.314627
educ8*	0.3443764	0.0145285	16.35	0.000	0.135204	0.315901	0.372852
bpeduc6*	0.0677126	0.0336769	1.97	0.049	0.214549	0.001707	0.133718
bpeduc78*	0.0287906	0.0373154	0.76	0.446	0.105935	− 0.044346	0.101927
t2*	0.1079754	0.0085769	12.10	0.000	0.150576	0.091165	0.124786
t3*	0.0664699	0.0154561	4.21	0.000	0.210046	0.036176	0.096763
t4*	0.1488357	0.0152808	9.00	0.000	0.162144	0.118886	0.178786
t5*	0.0750887	0.0161298	4.56	0.000	0.264974	0.043475	0.106702
nomom*	0.0010356	0.0216961	0.05	0.962	0.068437	− 0.041488	0.043559
meduc	0.0141195	0.0024856	5.68	0.000	2.50196	0.009248	0.018991
nodad*	0.0209539	0.017597	1.18	0.237	0.131711	− 0.013535	0.055443
deduc	0.0165544	0.0026562	6.22	0.000	2.57948	0.011348	0.02176
no_p*	− 0.0311407	0.0284207	− 1.10	0.271	0.315555	− 0.086844	0.024563
nt_p	− 0.0012632	0.0008932	− 1.41	0.157	15.845	− 0.003014	0.000487
nodissec*	0.0154711	0.0949567	0.16	0.871	0.010597	− 0.170641	0.201583
dis_sec	− 0.022742	0.0052547	− 4.35	0.000	2.07368	− 0.033041	− 0.012443
nearest	0.0013093	0.0002096	6.24	0.000	103.622	0.000898	0.00172
distance	− 0.0002782	0.0013918	− 0.20	0.842	9.78537	− 0.003006	0.00245
Observed *P*	0.578122						
Predicted *P*	0.6282776	(at $\bar{x}$)					

z and *P*>\|*z*\| are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table B-8
Derivatives from probit estimates of enrollment: male secondary school pooled sample

Probit estimates	Number of observations = 26,696
	χ^2 (30) = 4373.80
	Prob>χ^2 = 0.0000
Log-likelihood = − 11,856.423	Pseudo R^2 = 0.3231
	(Standard errors adjusted for clustering on eml)

enrolled	d*F*/d*x*	Robust standard error	*z*	*P*>|*z*|	$\bar{x}$	[95% Confidence	Intervals]
basal*	0.0308341	0.0255824	1.21	0.226	0.628521	− 0.019306	0.080975
pobre*	− 0.0005255	0.017341	− 0.03	0.976	0.591849	− 0.034513	0.033462
bp*	− 0.0196438	0.0336946	− 0.58	0.559	0.382529	− 0.085684	0.046396
age12*	− 0.134337	0.0316387	− 4.46	0.000	0.107057	− 0.196348	− 0.072326
age13*	− 0.2967558	0.0301573	− 10.12	0.000	0.172723	− 0.355863	− 0.237649
age14*	− 0.5068903	0.0267636	− 17.07	0.000	0.21138	− 0.559346	− 0.454435
age15*	− 0.6441806	0.0222895	− 21.57	0.000	0.215388	− 0.687867	− 0.600494
age16*	− 0.7264521	0.0169764	− 25.76	0.000	0.204225	− 0.759725	− 0.693179
age1718*	− 0.7292976	0.0089153	− 29.06	0.000	0.064392	− 0.746771	− 0.711824
t345bas*	0.0165596	0.020787	0.79	0.427	0.406016	− 0.024182	0.057301
t345bp*	0.035322	0.0193392	1.80	0.072	0.254533	− 0.002582	0.073226
educ6*	− 0.2296758	0.0204386	− 11.26	0.000	0.491422	− 0.269735	− 0.189617
educ7*	0.2532479	0.0136579	14.55	0.000	0.184709	0.226479	0.280017
educ8*	0.3016449	0.0117375	18.77	0.000	0.156877	0.27864	0.32465
bpeduc6*	0.0140805	0.0279529	0.50	0.617	0.199805	− 0.040706	0.068867
bpeduc78*	− 0.0269772	0.0304278	− 0.90	0.369	0.131855	− 0.086615	0.03266
t2*	0.0750197	0.0078931	9.14	0.000	0.1474	0.05955	0.09049
t3*	0.0270008	0.0149314	1.78	0.074	0.210518	− 0.002264	0.056266
t4*	0.1011196	0.0144506	6.53	0.000	0.165043	0.072797	0.129442
t5*	0.0381174	0.0165227	2.27	0.023	0.270453	0.005733	0.070501
nomom*	0.0288818	0.0191256	1.48	0.139	0.061208	− 0.008604	0.066367
meduc	0.0115454	0.0026545	4.34	0.000	2.46962	0.006343	0.016748
nodad*	0.0336957	0.0152836	2.16	0.031	0.129945	0.00374	0.063651
deduc	0.0215693	0.0026077	8.24	0.000	2.60447	0.016458	0.02668
no_p*	− 0.07962	0.02815	− 2.86	0.004	0.312931	− 0.134793	− 0.024447
nt_p	− 0.002798	0.0007839	− 3.55	0.000	15.9984	− 0.004334	− 0.001262
nodissec*	− 0.0827008	0.0644486	− 1.34	0.182	0.010039	− 0.209018	0.043616
dis_sec	− 0.0186885	0.0043686	− 4.27	0.000	2.08128	− 0.027251	− 0.010126
nearest	0.0012774	0.0001872	6.76	0.000	104.609	0.000911	0.001644
distance	− 0.0006908	0.0014184	− 0.49	0.626	9.54015	− 0.003471	0.002089
Observed *P*	0.6352637						
Predicted *P*	0.7001403	(at $\bar{x}$)					

z and *P*>\|*z*\| are the test of the underlying coefficient being 0.

* d*F*/d*x* is for discrete change of dummy variable from 0 to 1.

Table C-1
Derivatives from probit estimation of the likelihood that a child in October 1997 will be observed in the panel sample: female primary sample

Probit estimates	Number of observations = 14,571
	χ^2 (33) = 1397.53
	Prob>χ^2 = 0.0000
Log-likelihood = − 8977.9197	Pseudo R^2 = 0.1098
	(Standard errors adjusted for clustering on eml)

In Panel Sample	dF/dx	Robust standard error	z	P>|z|	$\bar{x}$	[95% Confidence Interval]	
basal*	− 0.0935421	0.0276217	− 3.37	0.000	0.613342	− 0.14768	− 0.039405
pobre*	− 0.0304338	0.0200523	− 1.52	0.130	0.719237	− 0.069736	0.008868
bp*	0.0708282	0.0308951	2.29	0.022	0.443346	0.010275	0.131382
age6*	0.2725063	0.01457	16.22	0.000	0.119278	0.24395	0.301063
age7*	0.1780456	0.0157288	10.68	0.000	0.118592	0.147218	0.208874
age9*	0.1081719	0.0172996	6.11	0.000	0.113994	0.074265	0.142078
age10*	0.0873269	0.0197717	4.35	0.000	0.12168	0.048575	0.126079
age11*	0.0579435	0.0204649	2.81	0.005	0.110905	0.017833	0.098054
age12*	0.0172356	0.0237905	0.72	0.469	0.066571	− 0.029393	0.063864
age13*	− 0.067541	0.0266009	− 2.52	0.012	0.040903	− 0.119678	− 0.015404
age14*	− 0.175632	0.0292484	− 5.66	0.000	0.030403	− 0.232958	− 0.118306
age15*	− 0.3890883	0.0204285	− 12.52	0.000	0.024638	− 0.429128	− 0.349049
age16*	− 0.5024563	0.0141405	− 11.49	0.000	0.021001	− 0.530171	− 0.474741
educ1*	0.1938106	0.0174116	10.55	0.000	0.159083	0.159685	0.227937
educ2*	0.2876669	0.0172156	14.73	0.000	0.151671	0.253925	0.321409
educ3*	0.2902721	0.0199873	12.79	0.000	0.147416	0.251098	0.329446
educ4*	0.3057762	0.0209193	12.68	0.000	0.135063	0.264775	0.346777
educ5*	0.3380223	0.021034	13.53	0.000	0.133004	0.296796	0.379248
bpeduc1*	0.0677081	0.027576	2.43	0.015	0.073296	0.01366	0.121756
bpeduc2*	0.0178647	0.0267027	0.67	0.504	0.069453	− 0.034472	0.070201
bpeduc3*	0.0164747	0.0261955	0.63	0.530	0.065335	− 0.034868	0.067817
bpeduc4*	0.009227	0.0303658	0.30	0.761	0.054698	− 0.050289	0.068743
bpeduc5*	− 0.0012412	0.0289688	− 0.04	0.966	0.052707	− 0.058019	0.055537
nomom*	− 0.0911609	0.0244707	− 3.68	0.000	0.073296	− 0.139123	− 0.043199
meduc	0.0060197	0.0026046	2.31	0.021	2.60593	0.000915	0.011125
nodad*	− 0.1069287	0.0184244	− 5.73	0.000	0.140279	− 0.14304	− 0.070818
deduc	− 0.0027555	0.0025144	− 1.10	0.273	2.70585	− 0.007684	0.002173
no_p*	− 0.0529247	0.0304837	− 1.73	0.083	0.327294	− 0.112672	0.006822
nt_p	− 0.0011144	0.0010143	− 1.10	0.272	17.0277	− 0.003102	0.000874
nodissec*	− 0.1291106	0.0622992	− 2.01	0.044	0.031226	− 0.251215	− 0.007006
dis_sec	− 0.0133874	0.0052333	− 2.56	0.011	2.18868	− 0.023645	− 0.00313
nearest	0.0002649	0.0002155	1.23	0.219	103.047	− 0.000158	0.000687
distance	− 0.0017093	0.0017943	− 0.95	0.341	9.70101	− 0.005226	0.001807
Observed P	0.5225448						
Predicted P	0.5093702	(at $\bar{x}$)					

z and P>|z| are the test of the underlying coefficient being 0.

* dF/dx is for discrete change of dummy variable from 0 to 1.

Table C-2
Derivatives from probit estimation of the likelihood that a child in October 1997 will be observed in the panel sample: male primary sample

Probit estimates

Log-likelihood = − 9524.1246

Number of observations = 15,405
χ^2 (33) = 1485.62
Prob>χ^2 = 0.0000
Pseudo R^2 = 0.1064
(Standard errors adjusted for clustering on eml)

In Panel Sample	dF/dx	Robust standard error	z	P>|z|	$\bar{x}$	[95% Confidence	Interval]
basal*	− 0.0863939	0.0268401	− 3.20	0.001	0.620643	− 0.139	− 0.033788
pobre*	− 0.0336661	0.0198054	− 1.70	0.090	0.721973	− 0.072484	0.005152
bp*	0.0739311	0.0327134	2.25	0.024	0.451672	0.009814	0.138048
age6*	0.2880241	0.0121992	19.78	0.000	0.107627	0.264114	0.311934
age7*	0.1782359	0.0140021	12.04	0.000	0.116586	0.150792	0.20568
age9*	0.0462501	0.0169492	2.71	0.007	0.109445	0.01303	0.07947
age10*	0.0288645	0.0186569	1.54	0.123	0.122233	− 0.007702	0.065431
age11*	0.0218182	0.0222061	0.98	0.327	0.10224	− 0.021705	0.065341
age12*	0.000635	0.0241597	0.03	0.979	0.07952	− 0.046717	0.047987
age13*	− 0.0636945	0.027767	− 2.28	0.023	0.047777	− 0.118117	− 0.009272
age14*	− 0.1169052	0.0272544	− 4.19	0.000	0.034599	− 0.170323	− 0.063488
age15*	− 0.3696306	0.0218828	− 12.11	0.000	0.02629	− 0.41252	− 0.326741
age16*	− 0.5229931	0.0111891	− 11.33	0.000	0.023109	− 0.544923	− 0.501063
educ1*	0.2316899	0.0177284	12.07	0.000	0.170399	0.196943	0.266437
educ2*	0.3067286	0.0170741	15.40	0.000	0.148328	0.273264	0.340193
educ3*	0.3294753	0.0169449	16.41	0.000	0.15099	0.296264	0.362687
educ4*	0.3402107	0.0190043	14.90	0.000	0.140019	0.302963	0.377459
educ5*	0.3539892	0.0208973	13.79	0.000	0.129503	0.313031	0.394947
bpeduc1*	0.0287696	0.0268839	1.07	0.286	0.081532	− 0.023922	0.081461
bpeduc2*	0.0349261	0.0294001	1.18	0.236	0.068614	− 0.022697	0.092549
bpeduc3*	− 0.0015495	0.0280127	− 0.06	0.956	0.067705	− 0.056453	0.053354
bpeduc4*	0.045759	0.0267302	1.70	0.088	0.058877	− 0.006631	0.098149
bpeduc5*	0.0111471	0.0307773	0.36	0.717	0.052061	− 0.049175	0.071469
nomom*	− 0.1123482	0.0211754	− 5.21	0.000	0.068225	− 0.153851	− 0.070845
meduc	0.0000927	0.0023948	0.04	0.969	2.60493	− 0.004601	0.004786
nodad*	− 0.0772373	0.0174975	− 4.39	0.000	0.135281	− 0.111532	− 0.042943
deduc	0.0005755	0.0024053	0.24	0.811	2.67011	− 0.004139	0.00529
no_p*	− 0.0722208	0.0322199	− 2.24	0.025	0.325154	− 0.135371	− 0.009071
nt_p	− 0.0014544	0.0011058	− 1.32	0.188	16.9203	− 0.003622	0.000713
nodissec*	− 0.1663174	0.0629267	− 2.51	0.012	0.027459	− 0.289652	− 0.042983
dis_sec	− 0.0127885	0.0057154	− 2.24	0.025	2.17139	− 0.023991	− 0.001586
nearest	0.0001621	0.0002199	0.74	0.461	103.989	− 0.000269	0.000593
distance	− 0.0011448	0.0016277	− 0.70	0.482	9.63476	− 0.004335	0.002045
Observed P	0.5255437						
Predicted P	0.5117337	(at $\bar{x}$)					

z and $P>|z|$ are the test of the underlying coefficient being 0.
* dF/dx is for discrete change of dummy variable from 0 to 1.

Table C-3
Derivatives from probit estimation of the likelihood that a child in October 1997 will be observed in the panel sample: female secondary sample

Probit estimates	Number of observations = 5468
	χ^2 (23) = 1021.19
	Prob> χ^2 = 0.0000
Log-likelihood = − 2712.3028	Pseudo R^2 = 0.2423
	(Standard errors adjusted for clustering on eml)

In Panel Sample	dF/dx	Robust standard error	z	P>|z|	$\bar{x}$	[95% Confidence Interval]	
basal*	− 0.0190097	0.0269597	− 0.71	0.480	0.604243	− 0.07185	0.03383
pobre*	0.0111123	0.0233556	0.47	0.635	0.555962	− 0.034664	0.056888
bp*	− 0.0364028	0.0603408	− 0.60	0.551	0.342904	− 0.154669	0.081863
age12*	− 0.002039	0.0384345	− 0.05	0.958	0.131858	− 0.077369	0.073291
age13*	− 0.0577414	0.0341185	− 1.63	0.103	0.195318	− 0.124612	0.00913
age14*	− 0.1424794	0.0308426	− 4.15	0.000	0.211778	− 0.20293	− 0.082029
age15*	− 0.3256841	0.0219688	− 11.10	0.000	0.227323	− 0.368742	− 0.282626
age16*	− 0.4726816	0.0133919	− 17.78	0.000	0.203365	− 0.498929	− 0.446434
educ6*	− 0.0086754	0.030021	− 0.29	0.772	0.596928	− 0.067515	0.050165
educ7*	0.0733549	0.0374756	2.03	0.042	0.14466	− 0.000096	0.146806
educ8*	0.0385197	0.0367696	1.07	0.284	0.130029	− 0.033547	0.110587
bpeduc6*	0.0618872	0.0595005	1.07	0.287	0.228237	− 0.054732	0.178506
bpeduc78*	0.0182759	0.0584815	0.32	0.752	0.081017	− 0.096346	0.132898
nomom*	− 0.0920657	0.0277672	− 3.02	0.003	0.088881	− 0.146488	− 0.037643
meduc	0.0021964	0.0032905	0.67	0.505	2.39539	− 0.004253	0.008646
nodad*	− 0.0486588	0.0226265	− 2.07	0.038	0.151061	− 0.093006	− 0.004312
deduc	0.0033004	0.0033759	0.98	0.328	2.4861	− 0.003316	0.009917
no_p*	− 0.0242132	0.0300378	− 0.80	0.423	0.316386	− 0.083086	0.03466
nt_p	− 0.0005076	0.0010017	− 0.51	0.612	15.6666	− 0.002471	0.001456
nodissec*	− 0.1060691	0.0762283	− 1.20	0.231	0.008961	− 0.255474	0.043336
dis_sec	− 0.0086714	0.0048077	− 1.81	0.071	2.08329	− 0.018094	0.000752
nearest	− 0.000154	0.0001918	− 0.80	0.422	103.052	− 0.00053	0.000222
distance	− 0.0011035	0.001143	− 0.96	0.335	9.7554	− 0.003344	0.001137
Observed P	0.3621068						
Predicted P	0.2755786	(at $\bar{x}$)					

z and P>|z| are the test of the underlying coefficient being 0.

* dF/dx is for discrete change of dummy variable from 0 to 1.

Table C-4
Derivatives from probit estimation of the likelihood that a child in October 1997 will be observed in the panel sample: male secondary sample

Probit estimates	Number of observations = 5515
	χ^2 (23) = 1037.44
	Prob > χ^2 = 0.0000
Log-likelihood = – 2699.6436	Pseudo R^2 = 0.2555
	(Standard errors adjusted for clustering on eml)

In Panel Sample	dF/dx	Robust standard error	z	$P>\|z\|$	$\bar{x}$	[95% Confidence Interval]	
basal*	– 0.0775564	0.0246832	– 3.18	0.001	0.6301	– 0.125935	– 0.029178
pobre*	– 0.0309007	0.0229996	– 1.35	0.178	0.55612	– 0.075979	0.014178
bp*	0.0037308	0.054659	0.07	0.946	0.359021	– 0.103399	0.11086
age12*	0.044145	0.0386862	1.17	0.241	0.118948	– 0.031679	0.119969
age13*	– 0.033397	0.0362902	– 0.90	0.368	0.179873	– 0.104524	0.03773
age14*	– 0.08146	0.0337695	– 2.30	0.022	0.215231	– 0.147647	– 0.015273
age15*	– 0.2933524	0.0237834	– 9.78	0.000	0.234633	– 0.339967	– 0.246738
age16*	– 0.4833607	0.0149552	– 17.11	0.000	0.219764	– 0.512672	– 0.454049
educ6*	0.0088152	0.0333212	0.26	0.791	0.525113	– 0.056493	0.074124
educ7*	0.0901386	0.0377124	2.48	0.013	0.177153	0.016224	0.164053
educ8*	0.0304828	0.0348191	0.89	0.373	0.151768	– 0.037761	0.098727
bpeduc6*	0.1072706	0.0572334	1.95	0.051	0.206346	– 0.004905	0.219446
bpeduc78*	0.0766199	0.0587223	1.36	0.175	0.115141	– 0.038474	0.191713
nomom*	– 0.0779137	0.0293957	– 2.44	0.015	0.069266	– 0.135528	– 0.020299
meduc	0.0024015	0.0035042	0.69	0.493	2.40508	– 0.004467	0.00927
nodad*	– 0.0408376	0.0218892	– 1.81	0.070	0.140888	– 0.08374	0.002065
deduc	0.0040803	0.0032732	1.25	0.212	2.54034	– 0.002335	0.010496
no_p*	– 0.0052729	0.0265313	– 0.20	0.843	0.31786	– 0.057273	0.046727
nt_p	– 0.0006303	0.0008608	– 0.73	0.465	15.7543	– 0.002317	0.001057
nodissec*	– 0.0495879	0.055101	– 0.85	0.393	0.007978	– 0.157584	0.058408
dis_sec	– 0.0053094	0.0043754	– 1.22	0.224	2.07994	– 0.013885	0.003266
nearest	0.0001697	0.0002056	0.82	0.410	104.598	– 0.000233	0.000573
distance	– 0.00281	0.001252	– 2.24	0.025	9.64318	– 0.005264	– 0.000356
Observed *P*	0.3673617						
Predicted *P*	0.2739821	(at $\bar{x}$)					

z and $P>|z|$ are the test of the underlying coefficient being 0.
* dF/dx is for discrete change of dummy variable from 0 to 1.

Table D-1
Comparison of mean pre-program characteristics of the panel samples of poor children

Variables	"Poor" potentially eligible children age 6–16, in pre-program surveys from panel samples	
	Resident in Progresa locality (treatment)	Resident in non-Progresa locality (control)
Sample size	17,286	10,278
Enrollment rate	0.896	0.891
Mother's years of education	2.60	2.62
No resident mother	0.039	0.043
Father's years of education	2.77	2.73
No resident father	0.090	0.096

Table D-1 (*continued*)

Variables	"Poor" potentially eligible children age 6–16, in pre-program surveys from panel samples	
	Resident in Progresa locality (treatment)	Resident in non-Progresa locality (control)
Student/teacher ratio in local primary school	17.9	17.6
Distance to secondary school (km)	2.08	2.10
No distance data on school	0.024	0.018
Distance to Cabeceras (km)	9.15	9.70
Distance to metropolitan area (km)	107	105

fertility, because Progresa could have a larger effect increasing the opportunity cost of time of the girl who could now attend subsidized school, and exert a smaller effect on the woman's subsequent lifetime wage opportunities, which is expected to reduce her total number of births. This form of inter-temporal substitution effect on the timing of fertility has also been estimated as a side-effect of adolescent job opportunities programs implemented in the United States during the 1970s (Olsen and Farkas, 1985, 1990).

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