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FINAL REPORT

THE IMPACT OF PROGRESA ON SCHOOL ENROLLMENTS

by

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EXECUTIVE SUMMARY

This study assesses how the PROGRESA Program (Education, Health, and Food Program of Mexico) has affected the schooling of Mexican youth in its first 15 months of operation. PROGRESA is designed to increase school enrollment among youth in poor families in poor rural communities, by making education and food grants to mothers in poor families, if their children attend school regularly and receive periodic medical check-ups. The program thus seeks to reduce the current level of poverty in Mexico and to increase the future productivity of children from poor families that should enhance the welfare of these families in the long run.

Enrollment rates are first compared between groups of poor children who are in otherwise similar communities, only some of which communities are beneficiaries of PROGRESA's educational grants. The enrollment rate of children in objectively categorized "poor" households residing in PROGRESA and non-PROGRESA (control) localities are compared in Table 3, before and after the program started. The group difference estimator of the Program's impact on enrollment (i.e., ,D1>0, post-program in Table 3) documents that children who had completed grades 1, 2, 3, 4, 5, 6, 7, or 8 in the previous school year, reported statistically significantly higher (.05 level) enrollment rates in the PROGRESA localities than in non-PROGRESA localities.

The placement of the PROGRESA Programs across all possible poor rural communities is designed to be random, and thus presumably unrelated before the Program was initiated in the summer of 1998 to enrollments or to other characteristics of poor households. To confirm that communities selected for the placement of PROGRESA programs did not for some reason differ in their enrollment patterns, the pre-program differences in enrollment rates between the PROGRESA and non-PROGRESA poor populations are shown not to be statistically different (i.e., D1=0, pre-program in Table 3). Differences-in-differences estimators calculated over time also show the increase in enrollment rates of the poor in the PROGRESA compared with the non-PROGRESA localities between pre-program and post-program periods (i.e., DD1>0 in Table 3). They are largest in the grade when enrollment rates are lowest, between completing elementary school (grade 6) and starting junior secondary school, although they are also significant (.05 level) for those who have completed grade 4, 5, or 6.

The PROGRESA program is associates with increases in enrollment rates by 9.4 percentage points (or one seventh) for this critical transition year, whereas the gains in enrollment related to the Program are on average only 3.1 percentage points after grades 1 through 5 and 5.4 percentage points on average after grades 7 and 8. If these estimated effects of the PROGRESA program on enrollments were sustained over the period a child is of school age, one approximation for the cumulated effect on average educational attainment would be the sum of the estimated program induces changes in the probability of enrollment at each grade level for a child from a poor household. Summing these double-differenced estimates for children who have completed one to eight years of schooling implies that a poor child would reach the age of 16 having completed on average .37 more years of schooling than would a poor child residing in a community without PROGRESA. There is a disproportionate advantage for girls from the program, and according to these calculations they gain about .50 years of additional schooling from second to ninth grade, compared to boys who stand to gain .26 years at these same grade levels. Because the average Mexican worker in the metropolitan areas nearest the PROGRESA communities receive wages that are roughly 12 percent higher for each year of secondary school they have completed, these increments to the average education of poor youth in the PROGRESA communities are expected to translate into gains in lifetime earning capacity of about 6 percent for girls and 3 percent for boys.

The targeting of the educational grants only to poor households is an important feature of the PROGRESA program, and is expected to increase enrollment rates of the poor relative to the non-poor in populations served by the program (i.e., D2<0, post program in Table 4). The empirical evidence from the first year of experience with the PROGRESA program is that enrollment rates did rise more for the poor than the non-poor in PROGRESA localities. This dimension of economic inequality in school enrollment within the same communities is diminished over time by the establishment of a PROGRESA Program in the area, but the differences in differences are sufficiently small, compared to the substantial variations in levels across communities, that they are statistically significant only after grade 4 and 6 (i.e., DD2<0 in Table 4).

To confirm that these group differenced estimates are not biased by the omission of observed control variables that might be fortuitously related to which communities were designated to participate in the PROGRESA program or which households were designated "poor" and thus eligible for program educational grants, a more structured model of the enrollment outcome is estimated using the probit model which assumes the unexplained variation in enrollment is normally and randomly distributed. Conditional on this structural approach, discrete variables are used to account for the enrollment outcome at the individual child level that incorporate the distinctions previously analyzed: PROGRESA/non-PROGRESA locality, Eligible (poor)/ non-poor household, and grade completed, stratified by sex. Additional controls are then added for the child's age, the schooling of the mother and father, the student-teacher ratio in the local primary school, the distance to the secondary school, and the distance from the locality to the Cabecera and nearest metropolitan area. The estimates of a second "long-form" probit model that included four way interactions between the child's grade level and their eligibility in a post-program round were not successful in separating precisely the differential impact of the program by grade level and round.

The estimates of the short-form of the probit model of enrollment for the panel sample indicates the average program impact across the three rounds is to increase the probability of enrollment by 1.2 percentage points for girls in the primary level and .5 percentage points for boys at this level. At the secondary school level the average program impact across rounds is an increase in enrollment of 11.0 percentage points for girls and 7.5 for boys. The cumulate effects of these gains for the six years of primary school and three years of junior secondary school suggests a lifetime increment for a child in a program community of .40 years of additional schooling for girls and .26 for boys.

The estimated impact on enrollment may be sensitive to the composition of the sample. Consequently, the restriction on the “panel sample” that a child is matched in all five rounds is relaxed, and the probit model is estimated based on the “pooled sample” that includes all children age 6 to 18 who are observed with sufficient information to estimate their enrollment in any one of the five survey rounds. The pooled sample is 60 percent larger than the panel sample at the primary school level and 90 percent larger at the secondary school level. The estimates program impact on enrollment at the primary levels is increased in the pooled sample compared with the panel by three-quarters for girls and doubled for boys, but they are decreased somewhat at the secondary level. The cumulative effects on lifetime educational attainment estimated from the pooled sample imply that girls would gain .36 years of schooling and boys .26 years. Given the different modeling assumptions underlying the individual probit models with controls and the group-differenced model, the two estimation approaches yield similar estimates for the program’s impact on the educational attainments of boys and girls in the PROGRESA localities.

FINAL REPORT ON THE IMPACT OF PROGRESA ON SCHOOL ENROLLMENT

T. Paul Schultz

1. OBJECTIVES

The purpose of this study is to assess how the PROGRESA Program (Education, Health, and Food Program of Mexico) has affected the schooling of Mexican youth. PROGRESA is designed to increase school enrollment among youth in poor families in poor rural communities. By making educational and food grants to mothers in poor households, if their children attend school regularly and receive periodic medical check-ups, the program seeks to reduce the current level of poverty in Mexico and increase the future productivity and welfare of these children and their families.

The program's effect on school enrollment is evaluated at two levels: (1) differences in enrollment rates between groups of children who are eligible and not eligible for program grants, and (2) differences in enrollment outcomes at the level of the individual child between those who are program-eligible and not eligible, controlling for additional factors in the family and community. First, group differences in school enrollment rates of poor children are compared in poor rural localities that are randomly designated to participate in PROGRESA and in comparable nonparticipating (or control) localities, and these enrollment rates are followed from the year before the program started, to the years after. The "panel" sample for group comparisons of enrollment rates is restricted to children in the two groups of localities who are observed twice in the year before the program is initiated (October 1987 and March 1998), and again three times (October 1998, May 1999 and November 1999) in the 15 months after PROGRESA began to make educational grants to families. The "panel" structure of this first sample includes only children who are observed in all five survey cycles. The interpretation of differences over time between the group enrollment rates is simplified in this panel sample, because this sampling procedure holds constant the individual composition of the groups between the pre-program and post-program observations, and thereby eliminates what might otherwise appears to be a program impact over time due to some compositional changes occurring in survey respondents. Unobserved persistent characteristics of the individuals, families, and communities that might, if they changed over time, affect period-specific enrollment rates and distort such group comparisons over time are thus "differenced out" of these comparisons to estimate the impact of the program on enrollment over time.

The second level of analysis reexamines the identical panel sample of children, but considers the individual child as the unit of analysis, which permits additional variables to be controlled that could potentially affect the probability of enrollment. Two advantages are noted in performing the program evaluation with the individual analysis. If the controls are for some unexplained reason correlated with the distinction between

PROGRESA and non-PROGRESA localities, or how the poor who are eligible for benefits are designated in the two groups of localities, adding the controls at the individual level to the analysis of enrollment would reduce any omitted-variable statistical bias present when the program effects are estimated at the group level. Even if program placement across the poor rural communities is random and the eligibility rules are administered correctly, the added controls should reduce the estimation error associated with the more extensively paramaterized model, and this should increase the statistical power of the model at the individual level to isolate significant effects attributable to the program treatment, if there are any (Manning et al., 1982). Consequently, the individual analysis of the data is the preferred empirical methodology for evaluating the program, although the initial group comparisons are reported as a less parametrically structured, and therefore more transparent, check on the magnitude of the estimates of the program effects (Moffitt, 1991; Hammermesh, 1999).

A third phase of the analysis explores the robustness of the preferred individual or group estimates of program effects. All children in the surveyed populations are now included in the "pooled sample" for estimating the program effects on individual enrollment, if they are observed in one or more of the five survey cycles. This increases the size of sample by 60 percent at the primary school level, and by 90 percent at the secondary school level, and, all else equal, this might be expected to increase the precision of the estimates. The benchmark individual estimates based on the panel could also be biased from the population average program effect because of sample selection; the children migrating out of the sample of rural localities over time (i.e., movers) could respond differently to the Program treatment of educational grants than those children (i.e., stayers) who are observed in all five rounds of the survey. Adding back into the estimation sample these out-migrants may shed light on how robust the program evaluation estimates are to these issues of sample attrition and population mobility. Within the limitations of the existing data, it is not possible to implement a satisfactory sample-selection-correction model that would predict who migrates out of the sampled localities, and then use the auxiliary models of migration to correct the program impact estimates for the mobility of the population (Rosenzweig and Wolpin, 1988).[1]

In addition, the estimated effects of certain control variables at the individual level of analysis should clarify for policymakers what household and community factors are currently important constraints on enrollment probabilities for these poor rural households of Mexico, and thus provide a limited assessment of auxiliary public policies that might be effective (or ineffective) in increasing enrollments in the future, such as raising the "quality" of schooling by reducing "class size", or building additional secondary schools to reduce the average distance from a child to their nearest secondary school. To evaluate more completely the policy tradeoffs between these alternatives interventions and PROGRESA's approach of directly subsidizing the family to enroll

[1] No information is available on the location or schooling status of those children who are not matched in the follow-up surveys, and correspondingly there is no background census data on the individuals or the communities from which in-migrants came before being first observed in a subsequent round of the surveys.

their children, it would be necessary to forecast the public and private marginal costs of modifying these constraint variables impinging on family schooling behavior. A comprehensive cost-effectiveness evaluation of these schooling system alternatives would also need to assign commensurate values to the multiple impacts of PROGRESA on a variety of other welfare objectives, such as poverty reduction for the parents and improved health and nutrition for the children in families eligible for PROGRESA benefits. Such comprehensive cost-benefit comparisons are well beyond the scope of this report.

2. PROGRAM INTERVENTIONS AND PATTERNS OF SCHOOL ENROLLMENT

In a locality (i.e., a low income rural community of Mexico) that has been designated as a PROGRESA Program area, households which have been characterized by the program as being "poor" (Behrman and Todd, 1999) and have children enrolled in school in grades 3 through 9 are eligible to receive an educational grant every two months, for the amount indicated in Table 1.[2] The level of these educational grants is approximately equal to what a child could earn in the labor force or contribute to family production. The size of the educational grant doubles from 70 to 135 pesos per month from the third to the final sixth year of primary school, when enrollment rates in this population are high or about 93 percent.[3] But in the regions surveyed by the PROGRESA Program many children who have completed only the sixth grade of primary school are not enrolled in junior secondary school. The educational grant for enrolling in the first year of junior secondary school increases by half to 205 pesos, with a small advantage for girls over boys in the first three years of (junior) secondary school. However, once a child has completed the first year of junior secondary school, the likelihood that they will be enrolled in the next grade increases to about 96 percent in grades 8 and 9, after which the enrollment rate falls again (See below Table 2).

An objective of the PROGRESA program is to foster an increase in the transition of poor rural youth into the junior secondary school, and encourage them to continue their education. This is a major decision in this community, for about three-fourths of the parents would have to plan on their 12-13 year old child traveling on average 3

[2] These amounts have increased somewhat after the preliminary program design was completed, and are somewhat higher due to adjustment for inflation than those reported in early PROGRESA literature and evaluation studies, e.g., Coady and Djebbari, 1999: Table 5. There are also additional supports (cash and kind) for school materials of 120 pesos per year at the primary level and 240 pesos per year at the secondary level provided on a term by term basis for beneficiary families (PROGRESA, 1999).

[3] These figures are based on the sample surveys described later in this report that are designed to represent the poor rural community from which the PROGRESA localities were randomly drawn. See Appendix Table A-1 for sample statistics.

Table 1— Monthly Payments for PROGRESA Program

Eligible Families for Children who attend at least 85 Percent of Days[a]

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

Source: PROGRESA staff.

[a] Excluding those days for which medical or parent excuses were obtained in the last two months.
[b] Corresponds to school year first-term, September to December, 1998.

Table 2—Distribution of Children Age 6 to 16 in October 1997 Census by Age and Years of Schooling Completed

(Beneath the number of children in each cell is the proportion of that cell enrolled)

Age of Child	Years of Schooling Completed by Child												Total
	0	1	2	3	4	5	6	7	8	9	10	11 +	
6	1640 0.899	512 0.990	38 1.000	0	0	0	0	0	0	0	0	0	2190 0.922
7	477 0.801	1484 0.997	327 0.991	28 0.964	0	0	0	0	0	0	0	0	2316 0.955
8	157 0.650	677 0.993	1222 0.991	321 0.988	21 1.000	0	0	0	0	0	0	0	2398 0.969
9	65 0.477	241 0.988	658 0.977	1033 0.992	226 0.987	29 1.000	1 1.000	0	0	0	0	0	2253 0.972
10	56 0.268	101 0.941	305 0.970	728 0.974	998 0.990	262 0.981	21 0.810	0	0	0	0	0	2471 0.962
11	44 0.114	33 0.939	149 0.960	339 0.947	717 0.978	884 0.984	199 0.859	14 1.000	1 1.000	0	0	0	2380 0.948
12	42 0.214	42 0.714	84 0.821	184 0.875	385 0.951	651 0.966	816 0.745	156 0.987	11 1.000	0	0	0	2371 0.859
13	40 0.025	40 0.850	51 0.667	90 0.700	176 0.869	348 0.928	780 0.519	451 0.976	117 0.966	12 0.667	0	0	2105 0.748
14	39 0.077	22 0.682	48 0.333	62 0.516	87 0.678	192 0.818	606 0.332	314 0.949	352 0.974	75 0.653	4 1.000	0	1801 0.654
15	13 0.000	15 0.800	10 0.400	19 0.316	32 0.469	56 0.732	242 0.236	84 0.940	111 0.937	108 0.519	15 1.000	2 1.000	708 0.554
16	2 0.000	0	2 0.500	3 0.333	0	4 0.500	13 0.077	3 0.667	7 1.000	7 0.571	1 1.000	0	42 0.452
Total all ages	2575 0.785	3167 0.983	2894 0.960	2807 0.948	2642 0.956	2426 0.951	2678 0.546	1022 0.966	599 0.967	202 0.579	20 1.000	2 1.000	21,035 0.883

kilometers to another town to continue their education.[4] The economic returns to their children from continuing to enroll in secondary school are relatively large and provide the children with an opportunities to escape from poverty. The 1996 National Urban Employment Survey (Encuesta Nacional de Empleo Urbano) has been used to estimate the structure of wage in 39 of Mexico's largest metropolitan areas (Parker, 1999). Matching the rural community sample of children analyzed in this study, who are age 6 to 16, to the nearest of these Mexican metropolitan areas, it is found that the estimated average percentage increases in worker wages associated with completing one more year of secondary school in the neighboring urban areas is about 12 percent, and slightly higher for women than for men. This private rate of return to secondary school is more than twice as large as the 5 percent wage return Parker (1999) estimates for each year of primary schooling for urban Mexican workers in 1996. Thus, the financial reward associated with the initiation and continuation of secondary school is substantial in neighboring metropolitan areas to the PROGRESA localities. Wage returns to secondary school are noted to be increasing in Mexico, contributing to the increase in earnings inequality, but also signaling the importance of fostering secondary schooling among the poor as an effective mechanism to reduce future poverty (Bouillon, et el. 1999). Unfortunately, there are insufficient wage earners in the rural communities themselves to assess with confidence the productive gains enjoyed locally by workers who obtain more schooling, because most of those in the sample work as self employed farmers or as farm workers in their own families, and it is not known how their individual productivity varies by schooling. Moreover, this information on the productive benefits to education in the rural economy would neglect the returns realized by Mexicans who are born and educated in rural areas, and then work as adults in the urban economy or in other more prosperous rural areas of Mexico (Schultz, 1988).

To gain a perspective on the relative generosity of the educational grants provided by PROGRESA, it is useful to compare the size of these grants with the average wage of local workers in agriculture. In a community questionnaire collected in November 1998 in the 495 localities surveyed in this report, the mean population weighted daily wage is about 29 pesos (Appendix Table A-1). If a worker were engaged at this typical rate for 20 days in a month, his earnings would be 580 pesos per month. Thus, the PROGRESA grant of 255 pesos per month for a daughter in her third year of secondary school would represent a 44 percent supplement to the income of a single male wage earner household during the ten month school year. [5] One could imagine that there would be instances where the PROGRESA grants to poor families with children in grades 3 through 9 would

[4] This is the mean distance to the secondary school in the panel sample who do not have a secondary school in their locality. Setting the distance to zero for those with a local secondary school, the overall sample mean is almost 2.1 km. See Appendix Table A-1.

[5] Other figures prepared by PROGRESA and the evaluation project have estimated that the monetary support grants for education and food would constitute on average 260 pesos per month per beneficiary household, equivalent to about 30 percent of the average monetary income of such poor families, and the 90 pesos of cash food grants would be equivalent to about 30 percent of the expected food expenditures of these families (Coady and Djebbari, 1999; PROGRESA, 1999). Also these amounts are to be adjusted every six months according to consumer price levels published by the Banco de Mexico.

fully compensate them for urging their children to spend their time attending school rather than working in the full-time labor force. [6]

There are also other household income supplements that may be received by "poor" households in a PROGRESA locality after mid 1998. A cash transfer "for food" of 90 pesos per month per household is available, regardless of location, household size, or composition, if its family members have periodic health care visits at which nutritional supplements can be prescribed and vaccinations provided. There are additional efforts to coordinate the local community to participate in the delivery of PROGRESA grants and services, but these aspects of the joint responsibility of the community for the program's operations are not quantitatively analyzed here (Coady and Djebbari, 1999).

The likelihood that a specific child is enrolled is related to the child's age and years of schooling completed. For the typical child surveyed here, age alone does not describe with much precision the grade a child is enrolled in. Children do not all enter the school system at the "normal" age of six, nor matriculate to the next grade at the end of each school year. Table 2 illustrates the distribution by age and years of schooling completed of children between the ages of 6 and 16 in the panel sample as recorded in the October 1997 household census. Beneath the number of children in an age-education cell is the proportion of those children currently enrolled in school. Although information in a survey is reported with measurement error, Table 2 suggests that being behind your "normal" grade level for age is not uncommon and is also not associated with a large decline in current enrollment rate. Among the eleven year olds in Table 2, 884 children had completed the 5th grade, and are thus on the normal school trajectory, and report an enrollment rate of .98 . Alternatively, there are 149 11 year old children whose last completed year of schooling is only the second grade, and they report nearly the same enrollment rate of .96. A small part of the difference in progress through the school system is due to differences in age at entry, for from the right hand total column it is evident that 92 percent of the 6 year olds are enrolled, and this enrollment rates increases slightly to 97 percent among those 9 years old. Grade repetition and temporary withdrawal from school do not appear to be rare events. Consequently, of those 2380 children age 11, only about half or 1098 are prepared to enter grade 6 or a higher grade, and other half are lagging behind their "normal" progression path through the school system, or a few have already withdrawn from school.

As a consequence of the range in ages of children prepared to enter the school system at each grade, the age-specific enrollment rates remain high, above 95 percent, until age 11, and then gradually decline thereafter by about ten percentage points a year, as seen in the right-hand total column in Table 2. But the enrollment rates by years of schooling

[6] To my knowledge, only in the case of a secondary school fellowship program for girls in Bangladesh, introduced in the early 1990s, has another low-income country designed an enrollment subsidy program as generous as that envisioned by PROGRESA (World Bank, 1998; Arend-Kuenning and Amin, 1998). In Bangladesh, some of the educational fellowships were deposited in a savings account to assist the youth in their later life as independent adults. The PROGRESA Program might also consider means to allocate part of the educational grant to support the child=s later investments in other complementary forms of human capital, including migration and job search to improve their employment prospect.

completed, reported in the bottom row in Table 2, reflect more clearly the critical thresholds in the enrollment process for a Mexican child in these poor rural areas. The enrollment rate is above 95 percent once the child has completed the first year of schooling, which almost all do, but then markedly declines to 55 percent after completing the 6th and final year of primary schooling. Once the child has completed a year of junior secondary school, enrollment rates return to 96 percent until the next transition after completing the 9th grade, and the enrollment rate in the first year of senior secondary school falls to 58 percent of those qualified to enter.[7]

Two samples are drawn for the analysis of school enrollment: a balanced "panel" of children for which there is information on all five survey cycles, and a "pooled" series of cross sections that includes all children observed in any survey. The first sample includes all children age 5 to 16 at the time of the first round (Household Census October 1997), who completed the age, schooling, and enrollment questions, for whom the schooling of coresident parents is reported, and the locality is matched to link the child to other sources of community information. The panel sample is further restricted to include only those children who could be followed and matched in the subsequent preprogram survey round in March 1998, and then in the postprogram surveys in October 1998, May 1999 and November 1999, which occurred after the PROGRESA Program had started to provide education and food grants in the fall of 1998. The second larger "pooled sample" includes all children age 6 to 18 who are observed at least once and can be linked with sufficient household data to estimate the basic enrollment model.[8] The children are matched on the basis of their family (folio) and individual (identification) numbers, and they must live in a locality for which there is matchable information from the community survey, school facility survey, and government files on communities and their socioeconomic and geographic characteristics. Dummy variables are included in the regression when a specific variable is not reported by the household or could not be

[7] The truncation of the sample of school-aged children from age 5 to 16 in October 1997, and from 6 to 16 in subsequent surveys, until the fifth survey when children 6 to 18 are interviewed, implies that the panel sample in the lowest age group and highest schooling grade need not be representative of the entire population of school-aged children. It is not clear how to correct for this minor limitation of the sample frame, but caution should be used in interpreting changes over time (D11 or D22) in the classes of children who have completed "no years of schooling" or "9 or more years of schooling". The pooled sample may be less affected by this problem, particularly among the youngest children. But censoring of those children over age 16 who could potentially or actually be enrolled in senior secondary school suggests this would still present a minor problem for the highest educational-grade-completed group, even for comparisons within a time period, such as D1 or D2. Fortunately, the youngest and oldest groups are not targeted by the PROGRESA program, and do not therefore enter centrally into the program evaluations findings reported below.

[8] In the fifth round of the survey, information was collected on resident children up to age 18, rather than only to age 16 as in the previous four rounds. These observations on older children are retained in this analysis, and an age dummy for being age 17 or 18 is included along with other age control variables in the individual level probit analysis. This should mitigate the censorship of the sample at the higher ages which otherwise occurred as children who were 15 or 16 in the earlier rounds "aged out" of the working sample. Nonetheless, estimates that describe the enrollment behavior of children who have completed no years of schooling and those who have completed 9 or more years of schooling should be interpreted with caution, due to the unrepresentativeness of the samples of youngest and oldest children who remain in the working samples. This may be a more serious problem with regard to the panel sample.

matched across the 495 localities examined. The working sample includes 314 localities where PROGRESA began delivering cash grants and in-kind transfers after the summer of 1998 and 181 non-PROGRESA (control) localities. These 495 localities are located in seven state in Mexico: Hildalgo, Michoacan, Puebla, Queretaro, San Luis Potosi, Veracruz, and Guerrero.[9] By March 1999 PROGRESA had initiated programs in 41,438 localities in 30 states of Mexico, that encompassed about one-tenth of all the families in Mexico (National Coordination of PROGRESA, 1999). In the sample of localities examined here, about two-thirds of the children between the age of 6 and 16 in 1997 were designated as living in a "poor" household, and thus eligible for educational grants if they were enrolled in the appropriate grade, 3 through 9 and attending at least 85 percent of the days in the last two months.

The number of children age 5 to 16 observed in the first survey is 40,959, and the number for which all five survey observations are matched, and age, education , and parental education, are reported, and satisfactory linkages are made to schools and locality characteristics is 19,716. Appendix Table A-1 reports the average and standard deviations of the central variables used in the analysis of the panel matched sample of the four surveys, males and females, separately for by primary and secondary school levels.

Each of the localities (community) contains its own primary school. As already noted, only about a quarter of the localities have their own secondary schools, and government data files are used to impute the distance to the nearest secondary school in the communities without schools (PROGRESA, 1999). Children are not required to attend a particular school according to where they reside, and they can qualify for the PROGRESA educational grant by attending any public school, if they reside in a PROGRESA Program locality (PROGRESA, 1999). Consequently, some students attend schools outside of their locality that are not the school distinguished in government records as the closest secondary school. The distances to secondary school may therefore slightly under-state the actual distance to the school the child has chosen (or considered).

An important limitation of the panel matched sample is that it excludes in- and out-migrants. It is generally not possible to describe the family and origin environment of the children of in-migrant families. The out-migrant families are indistinguishable from those who for some other reason cannot be matched, perhaps due to some clerical error in reporting or coding the family, individual, or community identifiers. The question regarding the child's last year of completed school is collected in the first, third and fifth rounds, and it is assumed that the responses in the first and third rounds are still valid

[9] The procedures used in the program to select a locality as a PROGRESA program areas or not, and the procedures used to designate a household as poor and thus eligible to be a beneficiary from the PROGRESA grants is described and analyzed in detail in other evaluation studies (Skoufias, et al. 1999; Behrman and Todd, 1999; Coady and Djebbari, 1999).

about five months later in March 1998 and May 1999, respectively, as the paired surveys occur during the same school year.[10]

3. FRAMEWORK FOR ANALYSIS OF SCHOOL ENROLLMENT AT THE INDIVIDUAL LEVEL

The human capital framework hypothesizes that schooling is acquired at a private cost by individuals and families partly to increase the student's future productivity, and consequently, at the margin, the decision to remain enrolled in school is affected by the balance between the current opportunity costs of enrollment and the anticipated future productive gains from acquiring additional schooling. Most public policies formulated to increase schooling have focused on improving access and quality of public sector schools and thereby to increase enrollments. It is often also assumed that these public policies call forth increased private investment by families and students in schooling to complement the increased public expenditures to improve the supply of school services (e.g., Birdsall, 1985; Schultz, 1988,1999b; Case and Deaton, 1999). These supply-side interventions may be particularly effective among the economically disadvantaged groups, who are more likely to be constrained from investing in the schooling of their children at a socially desired level because of limitations on their borrowing or information. Information about the magnitude of the private returns to schooling may not be adequately appreciated in low-income rural Mexican families, but research has not identified a replicable mechanism to increase enrollment by improving at a reasonable cost the supply of credit or information to households.

The PROGRESA program has taken a radically different approach to this social problem by transferring public resources directly to poor families that invest in their children's health, nutrition, and schooling. The effectiveness of this direct targeted demand subsidy to children who are poor and who stay in school has not been empirically evaluated, to my knowledge, in a setting where access to the program is varied randomly. The program targets the enrollment subsidy only to poor localities, and within them only to the poorest families based on multiple objective criteria, and then only to those children who are certified as enrolled and attending regularly in the final four years in primary and first three years of secondary school (See Table 1). In principle, this focused targeting could achieve a relatively large impact on the enrollment of the poor for a specified level of public expenditure. Only empirical evaluation of the current program can answer whether this has occurred in practice. The initial community placement of the PROGRESA programs was designed to facilitate the form of statistical program evaluation described in this report. Whether the program is sufficiently effective in promoting increases in the investment in the health and education of the children of these

[10] There are a few children observed in round four who were not matched in round three, and for those who were observed in round 1, their completed years of schooling was assumed to have increased by one year from October 1997 to October 1998, if they were still enrolled at the later date.

rural poor families in Mexico to warrant public support is, of course, a complex and ultimately political question than cannot be answered here.

To guide the program evaluation analysis, a framework is outlined that includes individual, community, and program administrative variables that are expected to influence the likelihood that a child is enrolled. Virtually all of the reported variation in school attendance is due to the variation in enrollment that is analyzed here.[11] The same framework helps to interpret differences between groups of children in their enrollment rates, which provide an alternative basis for estimating program effects that is parallel, but not identical, to the individual analysis. Let the probability of being enrolled in school for the i th child at the time of a survey be denoted as S_i. This likelihood of enrollment is affected by family demand for schooling, which may respond to many factors, including school quality and access, the opportunity cost of the student's time minus attendance subsidies provided after the start of PROGRESA, by household endowments and parental education, and factors affecting the local and regional labor market wage returns to schooling. If the process determining enrollment outcomes is approximated by the probit model with its assumed normally distributed error, the parameters to the relationship determining enrollment can be estimated by maximum likelihood methods. The standard errors of the estimates are adjusted for the clustering at the locality level (495) of the explanatory variables representing the program, school and other community characteristics (Huber, 1967), which is analogous to the White adjustment for heteroscedasticity.

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

$$S_i = a_0 + a_1 P_i + a_2 E_i + a_3 P_i E_i + \sum_{k=1}^{K} g_{ki} C_{ki} + \sum_{k=1}^{K} d_{ki} C_{ki} P_i E_i + \sum_{k=1}^{K} b_j X_{ji} + e_i, \quad i = 1,2...,n \qquad (1)$$

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.

First, there may be an effect on enrollments, α_1 , because an individual resides in a PROGRESA locality , P_i =1 (otherwise zero) , although the random placement of the initial community locations of PROGRESA was designed to minimize any such difference before the program started to inform the community of the nature of the program and who its beneficiaries would be. There may also be an effect, α_2 , of being designated as a child from a poor (pobre) household, E_i = 1 (otherwise zero), who is 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

[11] A parallel "note" (Schultz, 2000b) describes how the same factors as are included in equation (1) and (2) are observed to be related to the school attendance rates reported by household respondents, conditional on the boy or girl being enrolled in school, between the first two rounds of preprogram surveys and third and fourth rounds of postprogram surveys. See Appendix Table A-1 for magnitudes of attendance for the entire sample.

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 effect on enrollment denoted α_3 , which should be approximately zero until the Program transfer payments are announced, after which it is expected to be positive. Having controlled for the two interaction effects, 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 rich and poor families in the same community and errors in program administration.

Because enrollment rates vary across grades in a school system, a control is needed for the grade level to which the child would be qualified to enroll. The variable C_k is defined as 1 if the child has completed precisely k years of school, k= 0, 1, ..., 8, 9 (or more), which would qualify the child to enroll in the k+1 grade. These grade completion dummies are used to estimate the effect on enrollment by grade level, γ_k , which Table 2 bottom row suggested could be important. There is finally a pure program effect , δ_k , due to the administrative targeting of the educational grants only for children from poor families, in PROGRESA localities, attending grades 3 through 9. This targeting effect is therefore contingent on the child having completed k = 2 to 8 years of school by the end of the previous school year. The coefficient on the three way interaction of P, E, and C_k will suggest whether the enrollment effects of the program after transfers are initiated are concentrated in the targeted range as specified by PROGRESA, or exert household income spillover effects at earlier or later grades, as might occur between younger and older siblings in the same family. Finally, the model includes a variety of additional controls, X_j, for the child's characteristics, the parents' characteristics, and those of the community and local schools.

Over time, some variables that explain the probability of enrollment in equation (1) may change, such as C which would increase if a child completes one grade of schooling and is thus qualified to enroll in the next grade. The net effect of all unobserved variables that change over time could be partially captured by a shift in the estimated intercept for each time period or survey cycle. In other words, α_{0t} is allowed to vary in each round of the survey, where t = 1,2,3,4,5. Because PROGRESA grants only started in September 1998, and the beneficiaries were only identified in the previous summer, the program effects on enrollments represented by the coefficients on P, PE, and C_k PE are estimated as an additional set of interaction effects for the periods surveyed after the start of the program in October 1998, May 1999 and November 1999 (t = 3, 4 and 5, respectively), and the estimated program effects are distinguished by asterisks in the enrollment equation (2) that combines all five cross sections:

$$S_{it} = \sum_{t=1}^{5} \alpha_{ot} + \alpha_1 P_i + \alpha_2 E_i + \alpha_3 (P_i E_i) + \sum_{k=1}^{K} \gamma_k C_{kit} + \sum_{k=1}^{K} \delta_k (C_{kit} P_i E_i) + \sum_{t=3}^{5} (\alpha^*_{1t} P_i + \alpha^*_{3t} (P_i E_i)) + \sum_{k=1}^{K} \delta^*_{kt} (C_{kit} P_i E_i) + \sum_{j=1}^{J} \beta_j X_{jit} + e_{it} \quad (2)$$

Equation (2) is estimated separately for boys and girls, as the 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 reduction in enrollment at the secondary level, the two levels are estimated separately. The primary sample is defined as all children age 6 to 16 who report $C_{kt} < 6$, indicating that they have not yet completed primary school, and the secondary sample as all children age 6 to 16 who report $C_{kt} > 5$. The first "short-model" specification simplifies equation (2) slightly by assuming that PROGRESA's effect on enrollment is uniform by school level, and thus δ_{kt}* are zero, whereas the second "long-model" specification estimates 15 additional primary enrollment program parameters (five levels for three survey cycles) and 6 additional secondary enrollment program parameters (two distinguishable levels for three survey cycles). The small size of the samples of older children who are prepared to enter the more advanced grades of secondary school place limits on the estimation of the second fully interacted "long-model" specification and is the justification for combining grades 7 and 8 in one C class. The Probit model parameters are summarized in terms of the implied derivatives of enrollment with respect to the explanatory variables evaluated at the sample means, analogous to the linear approximation in equation (2). The sum of program effects on average enrollment for the entire population could then be calculated as a weighted sum of the derivatives of the probit estimates, where the weights are the sample average values (denoted by the underlined variables C and E) in the following expression:

$$dS_{kt} / dP = \alpha_{1t}^* + \alpha_{3t}^* \underline{E} + \delta_{kt}^* \underline{C_{kt}} \underline{E} , \qquad t = 3, 4 \text{ and } k = 1,2, ..., 8 . \qquad (3)$$

Different sets of control variables were included to assess the sensitivity of the estimates of program effects. Among the control variables, or X_{ji} included in equation (2), are dummies for the age of the child from 6 to 18 years at the time of the observation, whether the locality has a secondary school, and if it does not, how many kilometers is the nearest secondary school that the children in the locality are likely to attend and a dummy variable to indicate no distance reported in the government database (mean.008), the student-teacher ratio in the local primary school, and a dummy variable to indicate if this information was not available, as was the case for almost a third of the sample. Two variables are also included to capture the remoteness of the community from an urban labor market in which workers may receive a larger wage. Greater distances may translate into poorer local job opportunities and thus less competition for the time of children of school age who live with their parents: (1) the distance from the community to the Cabeceras or municipal county seat of government (average 10 km) and (2) the distance from the community to the nearest major metropolitan center (average 146 km).[12] It is expected that these distances would lower the opportunity cost of the child's time to attend school in the community where her family resides, but it could also raise the costs of migration to the city to seek a job that rewards more highly the better educated worker. Rural-urban migration is more likely to occur the greater is the educational attainment of the rural born man or woman, and this empirical regularity is observed in both high- and low-income countries (Schultz, 1988).[13]

The demands of parents for their children's schooling are often predicted on the basis of the years of education completed by the mother and father. In this case, two additional dummy variables are also needed in the model to indicate if the parental education information is not available, because the mother or father is not enumerated in the household (Cf. Tansel, 1997; Thomas, 1994; Behrman and Wolfe, 1984; Lam and Duryea, 1999). This procedure controls for the effects of lone parents, although it would be preferable to deal with this variation in household composition as another jointly determined aspect of the coping strategies of women and their families.[14]

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[12] Localities in Hildalgo (State) were allocated to the nearest of four major cities (Queretaro, Puebla, Tampico, or Mexico City), in Michoacan (State) to Morelia (its Capital), in Puebla to Puebla (Capital), in Queretaro to Queretaro (Capital), in San Luis Potosi to San Luis Potosi (Capital), in Veracruz to Veracruz (Capital), and in Guerrero to Acapulco (largest city in State). Thirty-nine cities were considered as sources of information on the local wage returns to primary and secondary education for men and women working in these cities. These estimates of educational returns were reasonable and represented wages on average being about 5 percent higher per additional year of primary school completed by the worker, and 12 percent higher per additional year of secondary school completed, and somewhat higher for women than for men. Potential sample selection bias was corrected because these estimates are based on wage earners (Susan Parker (1999)prepared these estimates from micro data files and linked them to the locality sample considered here.) Although the returns varied substantially across cities, being somewhat higher in the Southern cities than in the Northern cities, they did not account for a significant share of variation in enrollment rates in this sample of rural communities.

[13] A community questionnaire was collected in November 1998 that inquired about the local wage for workers of various types, but no response was reported for a majority of communities with regard to female and child wages, or for the wages of more specialized occupations. But in the case of the male daily wage in agriculture, about 98 percent of the communities provided a response, and the distribution of wages is broadly consistent with the government's minimum wage in the region. Moreover, responses to the community questionnaire in 1999 produced a wage variable that is correlated at .76 with that for the previous year, suggesting at least some consistency in responses. This male agricultural daily wage reported by locality in these two years may provide a rough indicator of both the local opportunity cost of child labor that could reduce the incentive to send children to school, and also measure the general level of income that might contribute to greater investment in the schooling of children. The enrollment effect of the local male agricultural wage was not statistically significant, and is not reported in this final report.

[14] The estimated effect on enrollment of a child having only one parent at home should be (cautiously) contrasted with the effect of having a father there who has an average education (2.7 years in this sample) or correspondingly having a mother with an average education (2.6 years). Because this variation in family composition is probably affected by unobserved variables that could reasonably affect the child's schooling decisions in other ways, these lone-parent variables may be endogenous to the enrollment process, and their apparent effects estimated here on the basis of the working assumption that they are exogenous should therefore be interpreted with caution. Exclusion of children without fathers in the household would reduce the size of the estimation sample of children by 12 percent, and exclusion of those without mothers of the child would have reduced the sample by 5 percent. The exclusion of these groups would also have introduced a sample selection problem, that could be a source of parameter bias.

Information on family income is not 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. This realized income would then be behaviorally interrelated with the family's school enrollment decisions. A latent variable index for household income and well-being was constructed by PROGRESA on the basis of many characteristics of the economic endowments of the family and its well-being that were measured in the original household censuses (ENCASEH97). This index was then converted into a binary indicator of whether the family is designated as "Poor" and therefore eligible for benefits from the PROGRESA program when the Program is established in the locality (Behrman and Todd, 1999). Equation (2) already includes this discrete indicator of poverty, E , because it is a qualification for program eligibility, but its inclusion also permits us to estimate in the control and in the PROGRESA communities before the onset of the program the net effect of this measure of poverty on child enrollment.[15]

4. DIFFERENCES IN ENROLLMENT RATES BETWEEN GROUPS

If the population at time t is divided into four groups, according to whether they are in poor households (E=1) , and whether they live in a PROGRESA locality (P=1) , the enrollment rates (S_{gt}) for each group, g= 1 ,2 ,3 ,4 , is defined in Figure 1. The difference between school enrollment rates of children eligible for program grants in localities participating in PROGRESA and enrollment rates of children in similarly poor (i.e., eligible) households in localities designated as control localities represents the "first difference-estimator" of the Program's effect on the schooling of poor children, represented in Figure 2 by $D1= S_1 - S_2$. The unbiasedness of this estimator depends critically on the random assignment of program participation and control localities such that observed and unobserved factors that might otherwise influence school enrollment are unrelated to program placement. Even if the randomization of Program placement were soundly designed, statistical correlations between program designated areas and pre-program (t-1) enrollments might exist fortuitously. If the pre-program regional differences between eligible PROGRESA and control children were due to factors that do not change over time in their impact on enrollments, the baseline observations in t-1 could be differenced and subtracted from the subsequent panel observations in t to construct a "program fixed-effect estimator", adjusted for any persistent form of the initial heterogeneity between localities, or a "double-differenced estimator", DD1 (Cf. Figure 2) (Moffitt, 1991; Hammermesh, 1999). This difference in differences estimator of

[15] Ethnicity of the parents is thought to be a factor in access to and the acquisition of education, and it is certainly correlated with poverty in Mexico, as it is in most societies. But the available survey information only reports whether the mother and father enumerated in the benchmark Census speak a native dialect, and in addition whether they do not speak Spanish. Four ethnic variables were thus constructed and added to the enrollment equation. They were not separately or jointly significant in explaining general enrollment patterns, once parent education is controlled, and are therefore omitted from this analysis, although these findings were presented in the previous report (Schultz, 1999a).

Figure1— Schematic Comparison of the Proportion of Children Enrolled in School at Time Period t

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

Figure 2 — Group Differences Representing Effects of Program Grants

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 factors affecting or correlated with outcomes variables

II. Double-Differenced Estimator of Change in Outcomes Between Program-Control Eligible Groups Over Time

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

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

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

IV. Double-Differenced Estimator of Change in Inequality in Outcome Over Time

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

Assumes all factors affecting economic group differences in Program and Control regions do not change over time program effects is analogous to the derivative of the individual probability of enrollment in the postprogram period in equation (3), derived from the asterisked parameters in the individual regression model (2) based on a series of cross sectional surveys that include pre-program (t=1, 2) and post-program (t=3,4 and 5) observations, but without controls.

These group differences, therefore, may be compared within relevant subgroups of the population, if the groups are independent of the designation of which localities are initially awarding PROGRESA educational grants. One informative way to subdivide the children is by years of schooling completed, which can distinguish which children qualify, and which do not, for PROGRESA educational grants. Therefore, the previous four-way division of the population may be repeated within cells defined by the years of schooling completed by the child, with the restricted program evaluation designed to test whether the program impact on enrollment is concentrated among groups of children who have completed 2 to 8 years of schooling in a PROGRESA locality after the program starts in the summer of 1998.

Eligibility for the Program's transfers may also change the distribution of schooling within the locality by income, helping the children in disadvantaged families catch up. Enrollment of children can be compared who appear to be observationally eligible for the program with those who appear to be ineligible (non-poor), to assess how effectively the program benefits are targeted to the poor population. Referring to Figures 1 and 2, a measure of this form of program redistribution through focused targeting could involve the following comparison, $D2 = (S_3 - S_1) - (S_4 - S_2)$. Before the start of the program, the poor are expected to enroll their children less often in school than the non-poor, or, $(S_3 - S_1) > 0$; $(S_4 - S_2) > 0$, because the poor are more constrained in borrowing to invest in their children, or because they have less information about economic returns to schooling, or they have other more pressing consumption needs or investment opportunities. In any case, it is expected that the quantities in parentheses will tend to be positive, representing economic-differentials or inequalities in school enrollment. It might be anticipated that the PROGRESA means-tested grants would reduce these gaps and possibly even reverse the gap, and D2 would then be negative. As in the previous case of evaluating D1, the educational differentials between economic classes by region could also be observed before the program started, and if these economic gaps were assumed to represent persistent sources of regional variation, they could be removed by differencing the gap after the program from the inequality which existed before the program, and thus define DD2 in Figure 2, or a second difference in differences estimator of the programs impact on educational inequality.

5. INTERPRETATION OF EVIDENCE OF GROUP DIFFERENCES IN ENROLLMENT

Enrollment rates for groups of children in the panel sample are classified four ways as in Figure 1: (1) whether or not they resided in a locality where PROGRESA was placed, (2) whether or not the child came from a household designated as “poor” and thus eligible for

PROGRESA educational grants, (3) whether the observation on the child's enrollment came from the first two survey rounds before the program benefits started, or from the last three survey rounds when PROGRESA grants were introduced, and (4) by the number of years of schooling the child had completed by the end of the previous school year. Following the definitions of the group differences in enrollment, as outlined in Figures 1 and 2, several comparisons are reported in Table 3 for the program impact on enrollment levels of the poor in pre-program and post-program differences (D1), and difference in differences over time (D11), and in Table 4 for the program impact on enrollment inequality across income groups (D2), and difference in differences over time (DD2). The subsequent discussion focuses on these differences for all children (boys and girls combined) by grade level, which are more stable than those reported separately for boys and girls.

The first issue is whether the assignment of PROGRESA localities and non-PROGRESA localities are approximately random with regard to enrollment levels? This is explored by considering D1, the difference in enrollment of poor children between the PROGRESA and non-PROGRESA localities before the program benefits were announced (pre-program). None of the ten values of preprogram D1 in the first column of Table 3 is statistically significantly different from zero, at the five percent confidence level, and seven out of ten grade levels are positive[16]. There is thus no indication before their entry into the program that the localities selected to participate initially in the PROGRESA program exhibited distinctly different enrollment levels than the controls.

With regard to the differences in enrollment inequality between the PROGRESA and non-PROGRESA localities, Table 4 reports that in the pre-program year these differences (D2) were negative in half of the grades, and significantly different from zero for only one out of ten grades, specifically for children who had completed only the sixth grade. This does not suggest a systematic inequality pattern between the localities selected for the initial participation in PROGRESA and controls.

In a previous report (Schultz, 2000a) the underlying enrollment rates for the first survey cycles were summarized. Enrollment rates were, as expected, lower for children from

[16] To test whether the group differences are statistically significant, the enrollment rates for the groups defined in Figure 1 are fit (perfectly) by a saturated linear regression model for enrollment of all of the children in the panel sample, which included 39 dummy variables and a constant in one period. It is then possible to test whether the estimated coefficients for the PROGRESA variables and their interactions that define the relevant group differences are jointly significantly different from zero, based on a joint F test. These are reported in parentheses beneath the differences in Tables 3 and 4. More specifically the individual regression includes the variables P, E, PE, and C_k , k=1, 2, ... , 9 , and the interactions between P, E, PE, and the k various schooling completed levels, C , which with the overall constant sum to 40 fitted (linear additive) parameters in both the preprogram and postprogram periods. The tests for the significance of the Program impact on enrollment in terms of D1 are that the P and PE coefficients and the interactions between PE and C are jointly different from zero. The test of the significance of the inequality reducing effect of the program represented by D2 are that the PE and interactions between PE and C are jointly different from zero.

Table 3— Difference in Differences for Enrollment Rates Between PROGRESA and Non PROGRESA Children[a]

(Significance levels in parentheses beneath differences)[b]

Years of Schooling Completed	Pre-Program Difference of Poor PROGRESA – Non PROGRESA D1			Post-Program Difference of Poor PROGRESA – Non PROGRESA D1			Post-Pre Program Differences in Differences D11		
	All	Female	Male	All	Female	Male	All	Female	Male
0	.009 (.36)	.010 (.44)	.007 (.62)	.001 (.94)	-.013 (.41)	.015 (.34)	-.008 (.60)	-.024 (.26)	.008 (.69)
1	.001 (.42)	-.009 (.82)	.011 (.38)	.018 (.0009)	.006 (.078)	.029 (.0023)	.017 (.065)	.015 (.28)	.019 (.10)
2	-.004 (.28)	-.013 (.40)	.005 (.51)	.017 (.0055)	.020 (.33)	.015 (.0027)	.021 (.22)	.033 (.92)	.009 (.086)
3	.015 (.29)	.025 (.17)	.005 (.88)	.031 (.0171)	.009 (.58)	.052 (.0035)	.016 (.35)	-.016 (.55)	.046 (.044)
4	.008 (.51)	-.016 (.84)	.030 (.27)	.041 (.0001)	.047 (.018)	.036 (.0022)	.034 (.022)	.063 (.071)	.006 (.13)
5	.015 (.14)	.005 (.55)	.024 (.13)	.092 (.0000)	.106 (.0005)	.079 (.0001)	.077 (.011)	.101 (.045)	.054 (.078)
6	.024 (.035)	.048 (.44)	-.019 (.0025)	.118 (.0007)	.174 (.0000)	.054 (.80)	.094 (.0024)	.126 (.012)	.073 (.021)
7	-.012 (.90)	-.005 (.86)	-.015 (.96)	.037 (.0104)	.043 (.74)	.032 (.0007)	.049 (.11)	.048 (.94)	.047 (.025)
8	-.030 (.91)	-.051 (.93)	-.016 (.84)	.028 (.0396)	.077 (.0008)	-.009 (.99)	.058 (.19)	.128 (.042)	.006 (.86)
9 or more	.103 (.54)	.327 (.0014)	-.156 (.0071)	.024 (.72)	.012 (.89)	.030 (.82)	-.079 (.48)	-.315 (.004)	.187 (.023)

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

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

Table 4— Difference in Differences for Schooling Inequality Between PROGRESA and Non PROGRESA Localities[a]

(Significance levels in parentheses beneath differences)[b]

Years of Schooling Completed	Pre-Program Differences D2			Post-Program Differences D2			Difference in Differences DD2		
	All	Female	Male	All	Female	Male	All	Female	Male
0	.010	.009	.011	.050	.037	.068	.040	.029	.057
	(.62)	(.76)	(.69)	(.033)	(.25)	(.047)	(.19)	(.50)	(.19)
1	-.002	.010	-.013	-.038	-.041	-.035	-.036	-.052	-.021
	(.91)	(.71)	(.61)	(.026)	(.092)	(.14)	(.16)	(.16)	(.54)
2	-.009	-.012	-.006	-.022	-.012	-.029	-.013	.000	-.023
	(.64)	(.65)	(.82)	(.18)	(.61)	(.18)	(.61)	(1.00)	(.50)
3	-.009	-.032	.012	-.011	.004	-.027	-.002	.035	-.039
	(.64)	(.25)	(.65)	(.48)	(.86)	(.22)	(.93)	(.32)	(.26)
4	.002	.026	-.022	-.050	-.061	-.034	-.051	-.088	-.012
	(.94)	(.34)	(.41)	(.0021)	(.0074)	(.14)	(.040)	(.014)	(.73)
5	-.020	-.003	-.037	-.081	-.102	-.062	-.061	-.100	-.025
	(.30)	(.91)	(.17)	(.0000)	(.0000)	(.0042)	(.015)	(.0065)	(.46)
6	.042	-.009	.124	-.032	-.126	.073	-.074	-.117	-.051
	(.026)	(.74)	(.000)	(.014)	(.0000)	(.0001)	(.0012	(.0003)	(.12)
7	.014	.010	.015	-.025	.010	-.060	-.039	.000	-.075
	(.63)	(.82)	(.71)	(.24)	(.74)	(.038)	(.28)	(.99)	(.13)
8	.023	.024	.029	-.014	-.126	.082	-.037	-.150	.053
	(.55)	(.67)	(.58)	(.56)	(.0003)	(.011)	(.41)	(.024)	(.39)
9 or more	-.022	-.284	.266	.063	.031	.094	.085	.315	-.172
	(.73)	(.0020)	(.0029)	(.027)	(.47)	(.015)	(.22)	(.0019)	(.08)

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

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

poor households in 18 out of 20 groups defined before the program started, by ten grades and whether the locality participated in PROGRESA. These poor/non-poor differences in enrollment are statistically significant in a majority of groups, and highly significant jointly. But for grades 2 to 8 in the localities participating in the PROGRESA program, the enrollment rates of the poor increased during the first year that the program offered educational grants and actually began to exceed those of the non-poor in these rural areas (Schultz, 2000a,Table3). In the three post-program rounds (t = 3, 4, and 5) the levels of enrollment among the poor in the PROGRESA localities increased relative to those in the non-PROGRESA localities at every grade level, as seen by the post-program D1 values reported in Table 3. From grade 1 to grade 8 these positive impacts of the program are statistically significant, and the lack of significance for the lowest and highest grade group may be attributed to the lack of educational grants for these grades or the limitations of age censoring of the sample design for these extreme age groups. The impact on enrollment rates is largest for children who have completed just the sixth grade and are thus qualified to enroll in the junior secondary school, increasing 11.8 percentage points for both sexes combined, or by 17.4 percentage points for girls and 5.4 for boys. The program effects are also large for grade 5, and substantial from grade 3 to 8 for both sexes combined. If these estimated effects of the program on enrollment were sustained over the period a child is of school age, one approximation for the cumulated effect on educational attainment for an average child from a poor household would be the sum of the estimated changes in the probability of enrollment at each grade level. Summing these D1 values from grade 1 to 8 suggests that the program is expected to increase educational attainment of poor youth by .38 years, but girls gain more than boys, .48 compared with .29 years of additional schooling.

The difference in differences of enrollment levels over time (D11), as reported in the third set of columns in Table 3, confirms a similar pattern of program effects. The largest impact is on the enrollments for children who have completed grades 5 through 8, and if program effects are summed over grades 1 to 8, the D11 estimates imply that girls would gain .50 years of schooling while boys would gain .26 , or a gain of .37 for the estimates that combine boys and girls. Because the average Mexican worker in the metropolitan areas nearest to the communities participating in PROGRESA receive a wage that are roughly 12 percent higher for each additional year of secondary school they have completed (Parker, 1999), the program's impact on the schooling of poor youth in these regions is likely to translate into gains in lifetime earnings capacity of girls of about 6 percent, and for boys of about 3 percent.

The impact of the program on the inequality in educational enrollments between poor and non-poor children within the PROGRESA localities is also evident in Table 4, where the post-program values of D2 indicate that the gap in enrollment probabilities between income groups has narrowed more in the PROGRESA localities than in the non-PROGRESA localities. The value of D2 is negative in all grades from 1 to 8, and is statistically significantly negative for those who have completed grade 1, 4, 5, and 6, for both sexes combined. The reduction in the inequality of enrollment over time (DD2) is also negative from grade 1 to 8, and is statistically significant for children who have completed grades 4 through 6.

The targeting of the educational grants only to poor households is an important feature of the PROGRESA program, and the analysis of both D1 and D2 confirms that PROGRESA has increased the enrollments of those populations that were targeted, and raised their enrollments to the levels of the non-poor in the same communities. It should be clear, however, that reducing educational inequality within these poor rural communities will still leave the educational attainments of poor and non-poor in these rural areas markedly behind those in the rest of Mexico. Thus, the magnitude of the program's impact on raising enrollment rates of the poor should be the main criterion for evaluating the effectiveness of PROGRESA, and one may recall that about two-thirds of these communities qualify as being "poor" (Table A-1).

Given the larger magnitude of the enrollment effects on children who have completed grades 5 to 8, compared with those who have completed grades 2 to 4, PROGRESA might consider redirecting its resources for education from grants for children having completed grades 2 to 4, to grants that would help poor households send their children on to grade 9, and encourage them to make the transition to senior secondary school. The transition years after grade 6 and 8 are critical thresholds in the schooling process to which PROGRESA might target more of its funds (Cf. Table 2, bottom row continuation rates). Assuring these rural communities have access within a reasonable distance to senior secondary schools is, of course, also necessary. The next section of this report extends this analysis of group differences in enrollment rates to consider the individual child as the unit of anlaysis, in order to control statistically for additional characteristics of the child (age), and households (mother's and father's schooling), local schools (student-teacher ratio in primary schools and distance to secondary schools), and proximity to the urban economy of Mexico, all of which might influence the partial relationship assessed here between the PROGRESA program and the probability of enrollment for children in poor families.

6. EVIDENCE OF PROGRAM EFFECTS ON INDIVIDUAL ENROLLMENT

The means and standard deviations for the variables analyzed in this section are reported in Appendix Tables A-1 for the panel and the pooled samples, for the primary and secondary school levels, by sex of children age 6 to 18. The definitions and abbreviated names of the variables are given in Table A-2 that are used in Appendix Tables B-1 to B-16 which report the derivatives of enrollment with respect to all of the explanatory variables in the probit model. Two specifications of the probit model are estimated by maximum likelihood methods, where the robust standard errors take account of the clustered design of the sample and community variables. The first short-form specification measures the impact of the PROGRESA program on enrollment of children from poor (i.e., program eligible) households residing in a PROGRESA locality, in the post-program rounds of the survey (i.e., 3, 4, or 5), but implicitly ignores differential program effects across grade levels within a school level. The second long form specification adds interactions between poor*PROGRESA*round3/4/5 and grade levels completed as of the previous school year. In other words, $*_{kt}^{*}$, t=3, 4, 5 are assumed in equation (2) to be equal to zero in the short-form probit model, and they are all estimated in the long-form for each relevant grade level in the school level. Table 5 reports in rows

1 and 2 the program impact parameters associated with PROGRESA (P) and PROGRESA-Eligible (P*E) by post-program round of November 1998 (t3), May 1999 (t4) and November 1999 (t5), which are summed as the third rows to approximate the net impact of the program on enrollment rates of the poor in each specific round. In brackets beneath the round-specific program net impact on the poor is the statistical probability that this net impact of the program is actually zero, according to a χ^2 test. At the bottom of the table there is reported the joint test of significance of all the program coefficients across rounds being zero. In 22 of the 24 possible round/sex/school-level/sample tests of the program's net impact on enrollment these joint effects are statistically significant at the 5 percent level. Thus, there appears to be a general positive enrollment effect of the program in the post-program surveys for both genders, in both samples, and at both school levels. But in general, the program effects are weakest for the primary school panel sample, and strongest for the secondary school panel sample.

The size of the probit estimated impact of the PROGRESA program in Table 5 on primary school enrollment is moderate, adding only slightly to the 93 percent enrollment levels in the panel sample that is restricted to children who could be matched across all five surveys. The net program impact on girl's enrollment probability is .7 percentage points higher in round 3, .8 points higher in round 4, and 2.0 points higher in round 5. Among boys the program net impact is initially smaller and increases to a similar level as for girls, from .2, .3 , and 1.1 percentage points higher in the three post-program rounds of the survey, respectively.

The pooled sample includes also children who might have migrated out of, or into, the sampled communities during the 25 months that are spanned by the first household Census of October 1997 and the last available survey interview from November 1999, or those who happened to not be matched in all rounds for any reason, such as missing some essential data. Primary school enrollment in the pooled sample is substantially lower than in the panel, about 89 percent, and this may perhaps explain why the program impact on enrollment is relatively larger in the pooled than in the panel sample: 1.3, 1.4, and 2.0 percentage points for girls, and 1.3, .5 and 1.6 percentage points for boys, for the three post-program survey rounds, respectively. The panel sample, however, remains a benchmark because it replicates the sample needed for the earlier group-differenced estimator.

At the secondary school level, the program impacts estimated by the probit short-form model as reported in Table 5 from the panel sample, suggests the enrollment of girls increased by one-sixth from the sample mean of 67 percent, or by 10.6 percentage points in round 3, 11.5 in round 4, and 10.8 in round 5. Among boys in the panel sample who on average were enrolled 72 percent of the time in secondary school, the program's effect is estimated in the panel sample as 9.4 percentage points in round 3, 7.3 in round 4, and 5.7 in round 5. In contrast with the approximately constant impact of the program over time on girl's enrollment, the program impact on boy's enrollment appears to have peaked in the first year and fallen thereafter. But it may be noted that the decline occurs in the

Table 5 — Estimated Impacts of PROGRESA Program on Enrollment in Short Specification of Probit Model, for Panel, and Pooled Samples[a]

	Panel Sample				Pooled Sample			
	Primary		Secondary		Primary		Secondary	
	Female	Male	Female	Male	Female	Male	Female	Male
Sample Size	34,934	37,541	12,733	13,372	56,923	60,894	24,234	25,146
Pseudo R^2	.3528	.3535	.2557	.2517	.4105	.4018	.2771	.2772
Derivatives of Explanatory Variables:								
1. Round 3* PROGRESA	-.0029 (.43)	-.0140 (1.86)	.0049 (.16)	.0411 (1.40)	-.0002 (.02)	-.0045 (.58)	.0041 (.15)	.0362 (1.56)
2. Round 3* PROGRESA Eligible (Poor)	.0101 (1.83)	.0165 (3.50)	.1105 (3.95)	.0530 (2.05)	.0127 (1.70)	.0174 (2.67)	.0805 (3.36)	.0424 (1.91)
3.Impact for Poor Round 3 $\chi^2_{(2)}$ [prob. impact zero]	.0072 [.0554]	.0020 [.0329]	.1154 [.0000]	.0941 [.0000]	.0125 [.0186]	.0129 [.0067]	.0846 [.0006]	.0786 [.0003]
1. Round 4 * PROGRESA	-.0035 (.49)	-.0141 (1.77)	-.0190 (.56)	.0098 (.33)	.0025 (.27)	-.0137 (1.52)	-.0004 (.01)	.0159 (.57)
2. Round 4 PROGRESA*Eligible (Poor)	.0118 (2.29)	.0169 (3.59)	.1337 (4.51)	.0636 (2.54)	.0114 (1.39)	.0191 (2.79)	.1052 (3.80)	.0473 (1.69)

	Panel Sample				Pooled Sample			
	Primary		Secondary		Primary		Secondary	
	Female	Male	Female	Male	Female	Male	Female	Male
3. Impact for Poor Round 4 $\chi^2_{(2)}$[prob. impact zero]	.0083 [.0138]	.0027 [.0229]	.1147 [.0000]	.0734 [.0015]	.0139 [.0157]	.0054 [.132]	.1048 [.0002]	.0632 [.0119]
1. Round 5 *PROGRESA	-.0063 (.92)	-.0025 (.36)	-.0215 (.57)	-.0194 (.59)	.0003 (.03)	.0018 (.23)	-.0424 (1.39)	-.0159 (.63)
2. Round 5 * PROGRESA Eligible (poor)	.0167 (3.87)	.0137 (2.79)	.1293 (4.06)	.0767 (2.97)	.0194 (2.51)	.0142 (2.17)	.1133 (4.18)	.0635 (2.94)
3. Impact for Poor Round 5 $\chi^2_{(2)}$ [prob. impact zero]	.0104 [.0002]	.0112 [.0027]	.1078 [.0005]	.0513 [.0173]	.0197 [.0003]	.0160 [.0054]	.0709 [.0145]	.0476 [.0451]
Joint Impact in all Three Rounds χ2 (3) [prob. impact zero]	[.0023]	[.0023]	[.0000]	[.0006]	[.0015]	[.0100]	[.0019]	[.0038]

[a] Derivatives of the enrollment probability with respect to the program explanatory variables, derived from the probit model estimates reported in Appendix Tables B-1 to B-6, evaluated at the sample means. Beneath the derivatives in parentheses are the absolute values of the z statistics or the ratio of the probit coefficient to its Huber-White robust standard errors that correct for the community aggregation structure of the sample and variables representing community characteristics, such as the program.

estimated program impact on boys that affects both the poor and non-poor children residing in PROGRESA localities, which falls from 4.1 percentage points in round 3, to 1.0 in round 4, to -1.9 in round 5. Non-poor families in PROGRESA localities increased the secondary school enrollment of their sons when the program initially started, but perhaps as they learned that they would not quality for the program educational grants, they subsequently decided to reduce their son's enrollment rates.

In the larger pooled sample, the secondary enrollment effects for girls are somewhat smaller and more variable across rounds than in the panel, namely, 8.5, 10.5, and 7.1 percentage points in rounds 3, 4, and 5, respectively. Among boys the pooled sample implies estimates of the program impact of 7.9, 6.3, and 4.8 percentage points, respectively, indicating the same downward trend as in the panel sample, and again this trend is directly related to the decline in the derivative of the PROGRESA program effect that "spills-over" to affect enrollments of both the eligible and non-eligible households. In sum, these short-form estimates of the probit model summarized in Table 5 imply, as did the previous report, that the impact of the program on enrollment rates appears to have been on the order of 1 to 2 percentage points at the primary level, and 6-11 percentage points for girls, and 5-9 percentage points for boys, at the secondary level.

As observed in the previous report on enrollment, the extra cycle of the fifth survey does not improve the precision of the probit estimates of the differential impact of the PROGRESA program by grade level, even though they were reasonably well defined in the double-differenced model with boys and girls combined (Table 3). Table 6 and 7 reports for the primary and secondary school samples the long form of the probit model and specifically the derivatives of enrollment with respect to the program and program interaction variables, including now interactions by grade level of the PROGRESA and Eligible variables by post-program round. There are few regular patterns in the program effects across grades at the primary school level in Table 6, except perhaps for the increased enrollment effects, for girls in the panel sample in round 3 : essentially no effect on children with zero or one year of school completed, up to a larger effect for those with five years of 1.21 percentage points (i.e., -.0026 - .0007 +.0154 = .0121). Few conclusions are drawn from these estimates.

The effort to differentiate the program impact by grade level using the probit model and more control variables is also not much more successful at the secondary school level as shown in Table 7. Adding together the first and second derivatives provides the estimate of the omitted grade category, which is now children with 9 or more years of schooling, for which there is no PROGRESA educational grant and a relatively small and perhaps representative sample of children.[17] For children who had completed only grade 6, the enrollment rate in the panel sample is estimated to increase for girls by 5.6 percentage

[17] For example, for girls with 9 or more years of schooling the net program effect in round 3, based on the panel sample, is -.147 in Table 3, which is equal to the sum of the derivatives with respect to PROGRESA and the PROGRESA*Eligible interaction, i.e., .0050-.1517=-.1467, whereas the effect for those who had 6 or 7/8 years of education completed would be augmented by the additional interaction with grade derivative.

Table 6—Primary School Enrollment of PROGRESA by Grade Completed and Survey Round

	Derivatives				Net Program Effects [p<0]			
	Panel Sample		Pooled Sample		Panel Sample		Pooled Sample	
	Female	Male	Female	Male	Female	Male	Female	Male
Pseudo R^2	.3599	.3587	.4160	.4064				
Round 3 PROGRESA	-.0026 (.42)	-.0137 (1.85)	-.0002 (.02)	-.0045 (.59)				
Round 3 PROGRESA *Eligible: (Grade 0 Omitted)	-.0007 (.09)	.0137 (2.10)	.0091 (1.06)	.0131 (1.61)	-.0033 [.59]	.0000 [.44]	.0089 [.18]	.0086 [.19]
Grade 1 Completed	.0089 (1.16)	.0138 (1.22)	-.0066 (.54)	.0166 (1.24)	.0056 [.43]	.0138 [.082]	.0023 [.79]	.0252 [.046]
Grade 2 Completed	.0099 (1.43)	-.0012 (.11)	-.0003 (.03)	.0035 (.28)	.0066 [.23]	-.0012 [.60]	.0086 [.28]	.0121 [.21]
Grade 3 Completed	.0105 (1.53)	.0147 (1.75)	-.0036 (.34)	.0145 (1.33)	.0072 [.25]	.0147 [.002]	.0053 [.53]	.0231 [.016]
Grade 4 Completed	.0133 (1.94)	-.0097 (.83)	.0101 (.98)	.0002 (.02)	.0100 (.11)	-.0097 [.77]	.0190 [.065]	.0088 [.30]
Grade 5 Completed	.0154 (2.42)	-.0005 (.05)	.0133 (1.30)	-.0027 (.22)	.0121 [.03]	-.0005 [.52]	.0222 [.034]	.0059 [.47]
Round 4 PROGRESA	-.0034 (.50)	-.0138 (1.77)	.0025 (.26)	-.0135 (1.53)				
Round 4 PROGRESA *Eligible (Grade 0 Omitted)	.0133 (2.01)	.0208 (3.42)	.0262 (3.14)	.0318 (4.06)	.0099 [.04]	.0070 [.017]	.0287 [.000]	.0182 [.0019]
Grade 1 Completed	-.0047 (.38)	-.0267 (1.43)	-.1265 (5.86)	-.1087 (4.41)	.0052 [.33]	-.0197 [.90]	-.0978 [.0048]	-.0905 [.012]
Grade 2 Completed	-.0046 (.44)	-.0082 (.68)	-.0258 (1.73)	-.0423 (2.56)	.0053 [.21]	-.0012 [.10]	.0029 [.17]	-.0241 [.94]
Grade 3 Completed	-.0069 (.63)	.0011 (.09)	-.0406 (2.47)	-.0151 (.90)	.0030 [.34]	.0081 [.016]	-.0119 [.61]	.0031 [.088]
Grade 4 Completed	-.0041 (.36)	-.0288 (1.85)	-.0164 (1.14)	-.0306 (1.96)	.0058 [.25]	-.0218 [.96]	.0098 [.049]	-.0124 [.44]

	Derivatives				Net Program Effects [p<0]			
	Panel Sample		Pooled Sample		Panel Sample		Pooled Sample	
	Female	Male	Female	Male	Female	Male	Female	Male
Grade 5 Completed	-.0067 (.60)	-.0366 (2.19)	-.0364 (2.35)	-.0426 (2.19)	.0032 [.37]	-.0296 [.65]	-.0077 [.46]	-.0244 [.95]
Round 5 PROGRESA	-.0062 (.92)	-.0025 (.37)	.0002 (.02)	.0017 (.21)				
Round 5 PROGRESA *Eligible (Grade 0 Omitted)	.0255 (6.14)	.0273 (5.12)	.0479 (7.84)	.0454 (7.50)	.0193 [.000]	.0248 [.000]	.2481 [.000]	.0471 [.0000]
Grade 1 Completed	-.0254 (1.63)	-.0211 (1.06)	-.2173 (7.31)	-.1274 (4.63)	-.0061 [.002]	.0037 [.008]	-.1692 [.79]	-.0803 [.18]
Grade 2 Completed	-.0197 (1.38)	-.0613 (2.81)	-.0977 (3.71)	-.1091 (5.00)	-.0004 [.000]	-.0365 [.023]	-.0496 [.0026]	-.0620 [.017]
Grade 3 Completed	-.0406 (2.32)	-.0448 (2.29)	.1387 (5.71)	-.1045 (4.42)	-0213 [.013]	-.0200 [.002]	.1868 [.047]	-.0574 [.020]
Grade 4 Completed	-.0221 (1.63)	-.0843 (3.71)	-.1058 (4.81)	-.1417 (6.22)	-.0028 [.000]	-.0595 [.11]	-.0577 [.0015]	-.0946 [.19]
Grade 5 Completed	-.238 (7.41)	-.2441 (6.53)	-.3740 (10.4)	.3381 (9.92)	-.0045 [.001]	-.2193 [.003]	-.3259 [.0000]	-.2910 [.0000]
Joint Test all 21 Coefficients Zero	[.0000]	[.0000]	[.0000]	[.0000]				

Table 7— Secondary School Enrollment Impacts of PROGRESA by Grade Completed and Survey Round

	Derivatives (t)				Net Program Effects [p<]			
	Panel Sample		Pooled Sample		Panel Sample		Pooled Sample	
	Female	Male	Female	Male	Female	Male	Female	Male
Pseudo R^2	.2625	.2575	.2855	.2844				
Round 3 PROGRESA	.0050 (.16)	.0406 (1.40)	.0046 (.17)	.0361 (1.56)				
Round 3 PROGRESA *Eligible (Grade 9+ Omitted)	-.1518 (1.37)	.0689 (.95)	-.0199 (.39)	.0107 (.24)	-.147 [.19]	.110 [.13]	-.015 [.76]	.047 [.28]
Grade 6	.2023 (2.64)	-.0483 (.58)	.1210 (2.39)	.0417 (.99)	.056 [.000]	.061 [.006]	.106 [.000]	.089 [.0002]
Grade 7 or 8	.1899 (2.22)	.0514 (.61)	.0519 (.86)	.0339 (.62)	.043 [.056]	.161 [.001]	.037 [.38]	.081 [.054]
Round 4 PROGRESA	-.0192 (.57)	.0095 (.33)	-.0001 (.00)	.0155 (.56)				
Round 4 PROGRESA *Eligible (Grade 9+ Omitted)	-.1905 (1.69)	.0614 (.81)	-.0575 (.94)	-.0003 (.000)	-.210 [.064]	.071 [.34]	-.058 [.34]	.015 [.78]
Grade 6	.2415 (3.50)	.0197 (.26)	.2189 (4.13)	.1022 (2.07)	.032 [.000]	.091 [.000]	.161 [.000]	.117 [.000]
Grade 7 or 8	.1597 (1.68)	-.0705 (.75)	-.0785 (1.10)	-.0994 (1.53)	-.050 [.92]	.000 [.81]	-.136 [.009]	-.084 [.049]
Round 5 PROGRESA	-.0223 (.60)	-.0196 (.60)	-.0440 (1.45)	-.0167 (.67)				
Round 5 PROGRESA *Eligible (Grade 9+ Omitted)	-.2872 (2.30)	.0843 (1.12)	.0199 (.42)	.0799 (1.90)	-.310 [.012]	.065 [.37]	-.024 [.64]	-.063 [.14]
Grade 6	.2811 (4.76)	.0701 (.92)	.2463 (5.54)	.1357 (3.23)	-.028 [.000]	.135 [.000]	.222 [.0000]	.073 [.0000]

	Derivatives (t)				Net Program Effects [p<]			
	Panel Sample		Pooled Sample		Panel Sample		Pooled Sample	
	Female	Male	Female	Male	Female	Male	Female	Male
Grade 7 or 8	.0962 (.87)	-.2248 (2.15)	-.3068 (5.04)	-.3311 (5.31)	-.213 [.006]	-.160 [.009]	-.331 [.000]	-.394 [.000]
Joint Test on all 12 Coefficients Zero	[.0000]	[.0000]	[.0000]	[.0000]				

points in round 3, by 3.2 in round 4, and by 2.8 in round 5. But in the pooled sample, the estimated increases for girls' secondary school enrollment rates are 10.6 in round 3, 16.1 in round 4, and 27 in round 5. These enrollment effects for girls become smaller among those who have completed grades 7 or 8, or grade 9 or more. For boys the panel sample estimates suggest a 6.1 percentage point program associated gain for those who have completed 6th grade in round 3, 9.1 in round 4, and 13.5 in round 5. Having completed grades 7 or 8, the enrollment rates for boys in the panel sample increase by 6.1 percentage points in round 3, zero in round 4, and decrease by 16 percentage points in round 5. The pooled sample suggests the program impacts among boys who completed grade 6 are 8.9 percentage points higher in round 3, 11.7 in round 4, and 7.3 in round 5. Again, these estimated program effects become more unstable across rounds for the children who have completed 7th or 8th grade. When a χ^2 test is performed to determine whether the net effects of the sum of grade specific probit coefficients are jointly significant and positive, they pass this test usually only in the case of the 6th grade graduates. In conclusion, the grade-specific estimates from the probit specification are not well defined, and therefore the short-form specification of the probit and the group-differenced estimates are the preferred basis for evaluating the impact of PROGRESA on school enrollment.

7. ENROLLMENT EFFECTS OF CONTROL VARIABLES IN THE PROBIT MODELS

According to the probit estimates of the short-form specification based on the panel sample (Table B-1 and B-3), the school attainment of both the mother and the father are significantly positively associated with the enrollment of their children. The estimated effect of one more year of mother's education is .49 percentage points higher primary school enrollment probability for daughters and .40 points for sons, whereas the schooling of the father is associated with .29 percentage points higher for daughters and .36 for sons. At the secondary school level, the impacts are larger as the probability of enrollment falls on average. An additional year of mother's education increases her daughter's probability of being enrolled by 1.6 percentage points and her son by 1.5, whereas an additional year of schooling of the father is associated with a increase in his daughter's enrollment by 1.6 percentage points and his son's by 1.9 points.

Distance to secondary school is associated with lower primary and secondary school enrollments, whereas the distance to the Cabeceras and to the nearest Metropolitan areas are significantly associated with increasing enrollment probabilities. Residing in a town that is only 50 kilometers from a metropolitan areas, rather than the sample mean distance of 150 kilometers is associated with a secondary school enrollment rate being 8.9 percentage points lower for girls and 10.7 points lower for boys. The emerging pattern from these data is that efforts to close the "gap" between these rural areas and the more prosperous urban area of Mexico may work to reduce rural enrollment rates, unless rural incomes rise, rural parents become better educated, or PROGRESA educational grants compensate for the growing access of the rural children to urban employment opportunities. Nearby cities can dissuade rural children from enrolling in school, as shown here, and reduce their attendance after they are enrolled (see Schultz, 2000b).

The household poverty indicator constructed by the PROGRESA program to target program eligibility to the more impoverished population is associated in the panel sample with a reduction in enrollment rates of 1 to 2 percent at the primary level and 6.7 percentage points for girls at the secondary school level and 2.5 percentage points for boys. This regularity implies that improving the income levels of these poor households will probably increase secondary school enrollment rates of girls more than of boys, and thus help to close the gender gap in enrollment that is evident in Mexico (Cf. Table A-1). Some of the control variables appear to exert stronger effects at the secondary level in the pooled sample. For example, in the pooled sample there emerges a weaker tendency for increased schooling of the mother to improve the secondary school enrollment prospects of a daughter relative to that of a son (i.e., .0028>.0018 at primary; and .0156>.0126 at secondary) and conversely for the schooling of the father to improve the enrollment prospect of a son relative to a daughter (i.e., .0022>.0016 at primary, and .0170>.0133). Such gender differences in the family transmission of schooling from parent to child are also noted in other cultures as well (Thomas, 1994).

To gain some perspective on what alternative public policies might increase enrollments and could be weighed as alternatives to the PROGRESA educational grants, the estimated effects of these control variables suggest two possibilities, but the magnitude of the effects are not promising. Improvement in access to secondary schools would stimulate greater enrollment. According to the short-form estimates of the secondary school enrollment model (Table B-5 and B-7) one can imagine the consequences of constructing sufficient schools and staffing them to provide all children in the sampled population with a secondary school within 4 kilometers of their own locality, which would reduce the distance to secondary schools in 78 out of the 492 localities considered here, and benefit 12 percent of panel sample of girls qualified to attend secondary school. The resulting reduction in mean distance from 2.05 km to 1.89 is expected to increase the percentage of girls enrolled from 66.7 to 67.1, or by .46 percentage points. The increased access to secondary schools would benefit 13 percent of the boys in the panel sample and would lead to an expected increase in their enrollment from 72.2 to 72.6 percent, or by .34 percentage points.

Improvements in school quality is another area in which public policy can presumably induce an increase in household demand for schooling and thereby raise enrollment rates. The most commonly considered measure of school quality is the student-teacher ratio, or approximately the average class size. At the local primary school level the probit model estimates based on the pooled sample (Tables B-9 and B-11) find this student-teacher ratio is significantly associated with lower enrollment rates, suggesting parents view smaller classes as a more promising investment of their children’s time in school. The sample average teacher-student ratio is 25, whereas about 20 percent of the schools report a student-teacher ratio greater than 30. If these relatively “low quality” schools were able to bring down their student-teacher ratio to 30, the probit estimates imply the enrollment rate of girls and boys would both increase by .10 percentage points. The capital and salary costs that would be required to realize this limit on class size cannot be assessed here. In contrast with the targeted PROGRESA educational grants to poor families, these alternative options designed to increase enrollment by (1) constructing secondary schools closer to the dispersed rural population and, (2) reducing the size of larger classes in primary schools, do not appear likely to induce large enrollment increments, although a full evaluation of these alternatives would, of course, require information on the cost-effectiveness of specific policy options and their distributional consequences between the poor and non-poor.

8. SUMMARY AND CONCLUSIONS

Enrollment rates are compared for groups of poor children who are in otherwise similar rural communities, two-thirds of which communities are randomly selected to participate in the PROGRESA program and offered educational grants for children who are enrolled in school. Comparisons before and after the program became operational are analyzed in this report to assess how the availability of program benefits influenced school enrollments. First, the level of enrollment rates of comparably poor children in PROGRESA and non-PROGRESA (control) localities after the program was in operation in October 1998, May 1999, and November 1999 are compared in Table 3. These calculations indicate that for those children who had completed in the previous year grades 1 through 8, the enrollment rates are statistically significantly higher in the PROGRESA localities than in the control localities (i.e., D1 (post-program)>0 in Table 3). Program educational grants were available for children who had completed grades 2 through 8.

To confirm the independence of the placement of PROGRESA programs and enrollments, it is also shown that the poor children in PROGRESA localities did not exhibit a tendency toward higher enrollments than did those in the control localities in the year before the program started (i.e., D1 (pre-program) = 0 in Table 3). Double-difference estimates combine the post-program differences minus the pre-program differences, and thereby allow for possible persistent unobserved sources of regional variation. The analysis of these difference in differences again confirm a marked increase in enrollment rates of the poor, especially in the transition year of entering junior high school after completing the 6^{th} grade (i.e., D11>0 in Table 3). If these estimates of the impact of the program on enrollment were sustained over the period a child is of school age, one approximation for the cumulated effect on educational attainment for an average child from a poor household would be the sum of these estimated program related changes in the probabilities of enrollment at each grade level. Summing these values of D1 in the post-program period from grade 1 to 8, implies that the program would increase educational attainment of girls from poor households by .48 years, and boys by .29. Summing the values of D11 implies girls would gain .50 years and boys .26 years of schooling. The PROGRESA program appears to be increasing enrollments while closing the initial gender gap in school enrollments.

An important feature of the PROGRESA program is the targeting of the educational grants to the poor, even in the geographically restricted context of poor rural communities. Does this targeting increase the enrollment rates of the poor relative to the enrollment rates of the non-poor in the localities participating in the PROGRESA program (i.e., D2<0 in Table 4)? The pattern is one of enrollment rates rising more for the poor than for the non-poor after the program starts, and indeed in most of the grade comparisons the enrollment rate of the poor exceed those of the non-poor in the PROGRESA localities in the 15 months after the program started. Although declines in inequality of enrollment are relatively small compared with the high levels of enrollment, these differences in inequality are nonetheless statistically significant after completing grades 1 through 6. Changes in the inequality of enrollment over time (DD2) are also uniformly negative after completing grade 1 through 8, and statistically significant after grade 1, 4, 5, and 6.

To confirm that these estimates based on group comparisons are not biased by the omission of some control variable that might be fortuitously related to which communities were designated to participate in the PROGRESA program, or which households were designated as "poor" and thus eligible for program educational grants, a more structured model of the enrollment outcome is estimated using the probit model which assumed the unexplained variation is normally distributed. Conditional on this structural approach, discrete variables are used to account for the enrollment outcome at the individual child level that capture the distinctions previously analyzed: PROGRESA/non-PROGRESA, Eligible (poor)/non-poor, and grade completed, and stratified by sex. Additional controls are then added for the age of the child, the schooling of the mother and father, the student-teacher ratio in the local primary school, the distance to the secondary school, and the distance from the locality to the Cabeceras (municipal seat of local government) and nearest metropolitan area. Finally, controls are included for the five survey rounds, and for three-way interactions between the PROGRESA locality, the household's eligibility, and the post-program round. A second "long-form" specification of this probit model for enrollment adds four way interactions between the previous three-way interaction and the child's grade level. The estimates of this second specification did not yield precise or stable program effects across rounds, and it may be concluded that the policy relevant parameters are adequately identified by the available data.

The short form of the probit model was first estimated using the same "panel sample" required for the group-difference estimator. This panel sample includes 19,716 children in each of the five surveys for a total of 98,580 (Table A-1). The same specification of the probit model is then estimated for the "pooled sample" that includes all children in the age limits who are observed in one or more of the surveys, which includes a total of 167,197 children in the five survey rounds combined (Table A-1). The average of the three survey rounds of estimates for the program's impact on primary school enrollments for girls, based on the panel sample, is a gain of 1.2 percentage points and for boys .5 points. At the secondary school level the round average of the program impact is an increase in the enrollment rate of 11.0 percentage points for girls and 7.5 for boys. The cumulative effect of these panel sample gains for the six years of primary school and three years of junior secondary school suggests a lifetime increment for a child in a PROGRESA community of .40 years of additional schooling for girls, and .26 years for boys. The estimates of the short-form probit model from the pooled sample imply a larger program effect at the primary levels and smaller effect at the secondary, with a cumulative lifetime gain of .36 years of additional schooling for girls due to the PROGRESA program, and .26 years gain for boys. Given the different modeling assumptions underlying the individual probit model and the group-differenced model, the two estimation approaches yield similar estimates for the program's impact on enrollment for boys and girls, and the shift between the panel to the pooled sample has a limited effect on the estimated impact of PROGRESA on enrollments over a child's entire lifetime.

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APPENDIX TABLES

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]

	Sample 1 - Panel				Sample 2 - Pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
Sample Size	34934	37541	12733	13372	56923	60894	24234	25146
Enrollment	0.936	0.934	0.667	0.722	0.893	0.895	0.567	0.625
Attendance	0.973	0.972	0.979	0.979	0.970	0.969	0.980	0.977
PROGRESA Locality	0.604	0.612	0.603	0.627	0.610	0.618	0.608	0.630
Eligible (Poor)	0.731	0.732	0.596	0.619	0.725	0.729	0.580	0.588
PROGRESA* Eligible	0.452	0.461	0.367	0.407	0.447	0.456	0.359	0.380
Completed Schooling								
0	0.136	0.127	0.000	0.000	0.193	0.181	0.000	0.000
1	0.180	0.183	0.000	0.000	0.183	0.188	0.000	0.000
2	0.179	0.187	0.000	0.000	0.162	0.167	0.000	0.000
3	0.183	0.179	0.000	0.000	0.167	0.167	0.000	0.000
4	0.167	0.165	0.000	0.000	0.151	0.151	0.000	0.000
5	0.155	0.158	0.000	0.000	0.145	0.146	0.000	0.000
6	0.000	0.000	0.588	0.525	0.000	0.000	0.557	0.492
7	0.000	0.000	0.195	0.226	0.000	0.000	0.176	0.201
8	0.000	0.000	0.136	0.159	0.000	0.000	0.135	0.158
9	0.000	0.000	0.081	0.090	0.000	0.000	0.133	0.150
Age of Child:								
6	0.066	0.061	0.000	0.000	0.088	0.081	0.000	0.000
7	0.111	0.107	0.000	0.000	0.121	0.117	0.000	0.000
8	0.147	0.146	0.000	0.000	0.134	0.132	0.000	0.000

	Sample 1 - Panel				Sample 2 - Pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
9	0.150	0.144	0.000	0.000	0.130	0.126	0.000	0.000
10	0.159	0.152	0.002	0.001	0.139	0.136	0.002	0.001
11	0.141	0.135	0.022	0.023	0.123	0.120	0.016	0.017
12	0.109	0.113	0.138	0.124	0.098	0.103	0.102	0.092
13	0.054	0.064	0.247	0.220	0.053	0.060	0.188	0.166
14	0.030	0.039	0.259	0.262	0.035	0.040	0.213	0.215
15	0.015	0.021	0.203	0.215	0.025	0.029	0.221	0.225
16	0.008	0.009	0.109	0.132	0.021	0.023	0.195	0.215
17	0.001	0.002	0.019	0.020	0.003	0.004	0.037	0.041
18	0.000	0.000	0.000	0.001	0.002	0.002	0.025	0.027
Mother's Schooling[d]	2.85 (2.64)	2.78 (2.64)	2.70 (2.47)	2.61 (2.49)	2.70 (2.70)	2.68 (2.68)	2.49 (2.46)	2.45 (2.46)
Father's Schooling[d]	2.92 (2.77)	2.87 (2.72)	2.74 (2.57)	2.77 (2.69)	2.80 (2.81)	2.75 (2.75)	2.56 (2.56)	2.59 (2.63)
Mother Not Present	0.047	0.049	0.047	0.047	0.062	0.061	0.069	0.062
Father Not Present	0.103	0.108	0.107	0.114	0.126	0.125	0.133	0.130
School Characteristics:								
Primary School Student/ Teacher Ratio[a]	17.4 (14.1)	17.3 (13.8)	16.6 (13.3)	16.7 (13.5)	17.4 (14.2)	17.1 (14.1)	16.5 (13.4)	16.6 (13.5)
No Information on Primary School	0.293	0.290	0.302	0.295	0.300	0.297	0.300	0.296
Distance to Secondary School (km.)[d]	2.10 (1.90)	2.09 (1.86)	2.01 (1.84)	2.05 (1.85)	2.16 (1.93)	2.15 (1.92)	2.07 (1.87)	2.07 (1.86)

	Sample 1 - Panel				Sample 2 - Pooled			
	Primary[b]		Secondary[c]		Primary[b]		Secondary[c]	
	Female	Male	Female	Male	Female	Male	Female	Male
No Distance to Secondary School	0.022	0.016	0.009	0.009	0.028	0.024	0.010	0.010
Community Characteristics								
Distance to Cabeceras (km)	9.62 (6.18)	9.52 (5.97)	9.73 (6.29)	9.36 (5.67)	9.64 (6.05)	9.59 (5.96)	9.77 (6.30)	9.52 (5.89)
Distance to Nearest Metro Area (km)[e]	150 (78.3)	150 (77.8)	146 (76.9)	150 (77.1)	146 (76.9)	146 (76.2)	145 (77.2)	147 (76.7)
Community Daily Agricultural Wage:								
For Men[d]	29.2 (10.3)	29.3 (10.4)	31.1 (10.8)	29.7 (10.5)	29.0 (10.7)	29.0 (10.9)	30.2 (10.9)	29.5 (10.7)
For Women[d]	11.5 (14.3)	11.3 (14.3)	11.5 (15.0)	11.4 (14.5)	11.8 (14.4)	11.4 (14.3)	11.5 (14.8)	11.5 (14.6)
No wage for Men	0.021	0.022	0.018	0.026	0.029	0.032	0.027	0.031
No wage for Women	0.562	0.569	0.586	0.577	0.549	0.565	0.577	0.568

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

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

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

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

[e] Distance measured from locations in Hildalgo (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 Postosi, in Veracruz and Veracruz, and in Guerrero from Acapulco (largest city in State).

Table A-2— Computer Variable Names as Reported in Appendix B Tables

Dependent Variables	
Enrollment in school dummy variable	inschl
Attendance in school as proportion of days attended in the last month (20 – days missed) / 20	attend
Explanatory variables	
Date of survey cycle	
October 1997	T1
March 1998	T2
October 1998	T3
May 1999	T4
November 1999	T5
Age of child at interview	age 1 = 1 (otherwise 0), . . ., age 16
Years of schooling completed by child at end of prior school year	educ 0 =1 if no schooling, . . ., educ 9 or more
Locality designated for PROGRESA program summer 1998	basal = 1
Household eligibility for PROGRESA benefits due to poverty of household	pobre = 1
Interaction of PROGRESA locality and eligible child	bp = 1
Interaction of postprogram survey cycles and PROGRESA locality	T3 basal, T4bp
Interactions of postprogram survey cycles and PROGRESA locality and eligible child	T3bp, T4 bp
Interactions of postprogram survey cycles and PROGRESA locality, eligible child, and years of schooling	T3bpeduc 1, …, T3bpeduc 9, T4bpeduc1, …, 4pbeduc9
Mother's schooling in years completed	meduc
Mother not present in household	nonmom = 1
Father's schooling in years completed	deduc
Father not present in household	nodad = 1
Distance in kilometers to nearest secondary school	dis_sec
Distance to nearest secondary school squared	dis_sec2
No data on secondary school in matched file	nodissec =1
Student-teacher ratio in local primary school	st_p
No data on primary school in matched file	no_p = 1
Distance to cabecera in kilometers (administrative center of municipality)	distance
Distance to nearest metropolitan area in kilometers	capital
Daily wage in locality for males in agriculture	mwage
Male wage not reported in locality	nomwage = 1
Daily wage in locality for females in agriculture	fwage
Female wage not reported in locality	nofwage = 1

*Pooled Sample Female Primary Enrollment Short Specification
Table B-1

Probit Estimates

Number of obs = 56923
chi2(44) =5959.95
Prob > chi2 = 0.0000
Log Likelihood = -11434.722
Pseudo R2 = 0.4105

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.]	
nodissec*	-.0446546	.0198925	-2.86	0.004	.028161	-.083643	-.005666
dis_sec	-.0020809	.0011625	-1.80	0.072	2.16042	-.004359	.000198
capital	.0001445	.0000322	4.49	0.000	145.866	.000081	.000208
distance	.0009175	.0004648	1.96	0.050	9.64019	6.5e-06	.001829
t2*	.0274224	.0023464	10.65	0.000	.177275	.022824	.032021
t3*	.0103016	.0034387	2.80	0.005	.195879	.003562	.017041
t4*	.0269338	.0031261	7.29	0.000	.183195	.020807	.033061
t5*	.0230704	.0037755	5.64	0.000	.187675	.015671	.03047
basal*	.0021718	.0068986	0.32	0.752	.610491	-.011349	.015693
age6*	.0227994	.0025371	7.63	0.000	.087522	.017827	.027772
age7*	.0224809	.0026484	6.98	0.000	.12148	.01729	.027672
age9*	-.0099502	.0050749	-2.10	0.035	.129895	-.019897	-3.6e-06
age10*	-.0478496	.0077464	-7.96	0.000	.138802	-.063032	-.032667
age11*	-.0997931	.0115786	-12.40	0.000	.123184	-.122487	-.0771
age12*	-.2448826	.0182184	-22.16	0.000	.09815	-.28059	-.209175
age13*	-.4210741	.0234211	-28.20	0.000	.052773	-.466979	-.37517
age14*	-.6215671	.0247218	-32.68	0.000	.034643	-.670021	-.573113
age15*	-.794876	.0159453	-43.44	0.000	.024858	-.826128	-.763624
age16*	-.8767538	.0111453	-42.88	0.000	.020501	-.898598	-.854909
age1718*	-.9196827	.0098211	-29.45	0.000	.005095	-.938932	-.900434
pobre*	-.0147374	.0039644	-3.57	0.000	.725401	-.022508	-.006967
bp*	-.0070867	.0076613	-0.93	0.352	.447376	-.022103	.007929
nomom*	-.0035426	.0056272	-0.65	0.518	.062137	-.014572	.007486
meduc	.0049402	.0007487	6.90	0.000	2.70399	.003473	.006408
nodad*	.0029896	.0041399	0.71	0.481	.126434	-.005125	.011104
deduc	.0028713	.0006174	4.63	0.000	2.80187	.001661	.004081
no_p*	-.0160892	.0093869	-1.79	0.073	.300318	-.034487	.002309
st_p	-.0003324	.0002554	-1.28	0.199	17.1401	-.000833	.000168
t3basal*	-.0001874	.008367	-0.02	0.982	.120057	-.016586	.016212
t3bp*	.012747	.0066065	1.70	0.089	.08847	-.000201	.025695
t4basal*	.0025334	.0093624	0.27	0.790	.112169	-.015817	.020883
t4bp*	.0113717	.0073303	1.39	0.166	.084026	-.002995	.025739
t5basal*	.0003277	.0094798	0.03	0.972	.113452	-.018252	.018908
t5bp*	.0193629	.0062359	2.51	0.012	.084447	.007141	.031585
educ1*	.0635972	.0033468	16.81	0.000	.182615	.057038	.070157
educ2*	.0633763	.0032146	20.04	0.000	.162377	.057076	.069677
educ3*	.069054	.0031864	20.95	0.000	.166857	.062809	.075299
educ4*	.072039	.0033595	23.61	0.000	.15073	.065454	.078623
educ5*	.0700366	.0033861	23.63	0.000	.144546	.0634	.076673
bpeduc1*	-.0054972	.0096255	-0.59	0.552	.085361	-.024363	.013369
bpeduc2*	.0036159	.0078923	0.44	0.657	.074733	-.011853	.019085
bpeduc3*	.0051625	.007175	0.69	0.490	.076068	-.0089	.019225
bpeduc4*	.0115872	.0069651	1.50	0.134	.064174	-.002064	.025239
bpeduc5*	.0205176	.0046205	3.60	0.000	.058553	.011462	.029573
obs. P	.8927323						
pred. P	.9560163	(at x-bar)					

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

*

Pooled Sample Female Primary Enrollment with Schooling Interactions
Table B-2

Probit Estimates

Number of obs = 56923
chi2(59) =6847.45
Prob > chi2 = 0.0000
Pseudo R2 = 0.4160

Log Likelihood = -11328.035

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95%	C.I.]
nodissec*	-.0422124	.0190058	-2.82	0.005	.028161	-.079463	-.004962
dis_sec	-.0020372	.0011489	-1.78	0.075	2.16042	-.004289	.000215
t3bped1*	-.0066462	.0130265	-0.54	0.590	.01567	-.032178	.018885
t3bped2*	-.0002965	.0092504	-0.03	0.974	.015073	-.018427	.017834
t3bped3*	-.0035937	.0108702	-0.34	0.733	.016584	-.024899	.017711
t3bped4*	.0101115	.009147	0.98	0.325	.013931	-.007816	.028039
t3bped5*	.013261	.0087695	1.30	0.194	.012227	-.003927	.030449
t4bped1*	-.126507	.0326811	-5.86	0.000	.017901	-.190561	-.062453
t4bped2*	-.0257753	.0176611	-1.73	0.084	.014177	-.06039	.00884
t4bped3*	-.040559	.0208576	-2.47	0.014	.015231	-.081439	.000321
t4bped4*	-.0164082	.0162219	-1.14	0.253	.012877	-.048202	.015386
t4bped5*	-.0363558	.0192487	-2.35	0.019	.011366	-.074083	.001371
t5bped1*	-.2173422	.0481375	-7.31	0.000	.018411	-.31169	-.122995
t5bped2*	-.0977369	.0380105	-3.71	0.000	.014282	-.172236	-.023238
t5bped3*	-.1386701	.0371972	-5.71	0.000	.015214	-.211575	-.065765
t5bped4*	-.1057671	.0326286	-4.81	0.000	.01307	-.169718	-.041816
t5bped5*	-.3739703	.0562143	-10.44	0.000	.011384	-.484148	-.263792
capital	.0001415	.0000315	4.50	0.000	145.866	.00008	.000203
distance	.0008975	.0004561	1.95	0.051	9.64019	3.5e-06	.001792
t2*	.026404	.0023056	10.33	0.000	.177275	.021885	.030923
t3*	.0100355	.0033964	2.76	0.006	.195879	.003379	.016692
t4*	.0265482	.0030679	7.30	0.000	.183195	.020535	.032561
t5*	.0226878	.0037028	5.64	0.000	.187675	.01543	.029945
basal*	.0021876	.0067824	0.32	0.746	.610491	-.011106	.015481
age6*	.0239028	.0024839	8.02	0.000	.087522	.019034	.028771
age7*	.0203661	.0028113	6.02	0.000	.12148	.014856	.025876
age9*	-.0125794	.0052728	-2.60	0.009	.129895	-.022914	-.002245
age10*	-.0513926	.0079705	-8.42	0.000	.138802	-.067014	-.035771
age11*	-.1059004	.0119315	-12.91	0.000	.123184	-.129286	-.082515
age12*	-.2509488	.0187424	-22.07	0.000	.09815	-.287683	-.214214
age13*	-.4260152	.0237988	-28.00	0.000	.052773	-.47266	-.37937
age14*	-.6284461	.0248803	-32.57	0.000	.034643	-.677211	-.579682
age15*	-.8024492	.0157784	-43.30	0.000	.024858	-.833374	-.771524
age16*	-.8827284	.0106622	-43.41	0.000	.020501	-.903626	-.861831
age1718*	-.9193849	.0099405	-29.88	0.000	.005095	-.938868	-.899902
pobre*	-.0144156	.0038942	-3.55	0.000	.725401	-.022048	-.006783
bp*	-.0152713	.0078906	-1.97	0.049	.447376	-.030737	.000194
nomom*	-.0034313	.0055371	-0.64	0.524	.062137	-.014284	.007421
meduc	.0049488	.0007416	7.00	0.000	2.70399	.003495	.006402
nodad*	.0028951	.0040785	0.69	0.488	.126434	-.005099	.010889
deduc	.0028071	.0006102	4.57	0.000	2.80187	.001611	.004003
no_p*	-.0159377	.0091941	-1.82	0.069	.300318	-.033958	.002082
st_p	-.0003355	.0002492	-1.33	0.184	17.1401	-.000824	.000153
t3basal*	-.000152	.0082495	-0.02	0.985	.120057	-.016321	.016017
t3bp*	.0090841	.0078928	1.06	0.290	.08847	-.006386	.024554
t4basal*	.0024783	.0092262	0.26	0.792	.112169	-.015605	.020561
t4bp*	.0262326	.0060593	3.14	0.002	.084026	.014357	.038109
t5basal*	.0001709	.0093284	0.02	0.985	.113452	-.018112	.018454
t5bp*	.0479151	.0029783	7.84	0.000	.084447	.042078	.053752
educ1*	.0627291	.0032899	16.90	0.000	.182615	.056281	.069177

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Table B-2 Continued

educ2*	.0626125	.0031596	20.22	0.000	.162377	.05642	.068805
educ3*	.0682982	.003137	21.19	0.000	.166857	.06215	.074447
educ4*	.071121	.0033076	23.87	0.000	.15073	.064638	.077604
educ5*	.0691009	.0033329	24.05	0.000	.144546	.062569	.075633
bpeduc1*	.0177063	.0079637	1.87	0.061	.085361	.002098	.033315
bpeduc2*	.0094548	.0086523	1.00	0.317	.074733	-.007503	.026413
bpeduc3*	.0146201	.0071461	1.79	0.074	.076068	.000614	.028626
bpeduc4*	.013819	.0072306	1.68	0.093	.064174	-.000353	.027991
bpeduc5*	.0334244	.0038162	5.62	0.000	.058553	.025945	.040904
obs. P	.8927323						
pred. P	.9570982	(at x-bar)					

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

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Pooled Sample Male Primary Enrollment Short Specification
Table B-3

Probit Estimates

Number of obs = 60894
chi2(44) =5215.07
Prob > chi2 = 0.0000
Pseudo R2 = 0.4018

Log Likelihood = -12204.916

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0211103	.0190341	-1.29	0.198	.02414	-.058416	.016196
dis_sec	-.0017193	.0010517	-1.62	0.106	2.15021	-.003781	.000342
capital	.0002009	.0000254	8.59	0.000	146.354	.000151	.000251
distance	.0008829	.000388	2.26	0.024	9.59595	.000122	.001643
t2*	.028058	.0020545	12.20	0.000	.178326	.024031	.032085
t3*	.0127176	.0029344	3.98	0.000	.195914	.006966	.018469
t4*	.0243088	.0032758	6.19	0.000	.184172	.017888	.030729
t5*	.0242205	.0033484	6.58	0.000	.188606	.017658	.030783
basal*	.003037	.0067206	0.46	0.649	.617778	-.010135	.016209
age6*	.0212507	.0026623	6.59	0.000	.080665	.016033	.026469
age7*	.0218888	.0024859	7.22	0.000	.117417	.017017	.026761
age9*	-.0227921	.0058765	-4.47	0.000	.125874	-.03431	-.011274
age10*	-.053415	.0079044	-8.44	0.000	.13599	-.068907	-.037923
age11*	-.0876543	.0102236	-11.71	0.000	.119913	-.107692	-.067616
age12*	-.1843512	.015554	-18.76	0.000	.102752	-.214836	-.153866
age13*	-.3536801	.0218529	-25.66	0.000	.059809	-.396511	-.310849
age14*	-.567986	.0232724	-33.76	0.000	.039741	-.613599	-.522373
age15*	-.76549	.0196254	-36.06	0.000	.028985	-.803955	-.727025
age16*	-.8858477	.0108887	-41.89	0.000	.022777	-.907189	-.864506
age1718*	-.9376511	.0062637	-31.22	0.000	.006487	-.949928	-.925374
pobre*	-.0177788	.0043699	-3.83	0.000	.728676	-.026344	-.009214
bp*	.0011159	.0079916	0.14	0.889	.455661	-.014547	.016779
nomom*	-.0038067	.0052344	-0.75	0.453	.061418	-.014066	.006452
meduc	.0039505	.0006898	5.80	0.000	2.68173	.002599	.005302
nodad*	.0021792	.0042489	0.51	0.613	.125234	-.006149	.010507
deduc	.0035622	.0006704	5.33	0.000	2.75659	.002248	.004876
no_p*	-.0234969	.0097448	-2.56	0.010	.297386	-.042596	-.004398
st_p	-.0003902	.0002794	-1.38	0.167	17.1068	-.000938	.000157
t3basal*	-.0044979	.0079358	-0.58	0.559	.120997	-.020052	.011056
t3bp*	.0174343	.0054705	2.67	0.008	.08973	.006712	.028156
t4basal*	-.0136861	.0098284	-1.52	0.129	.114576	-.032949	.005577
t4bp*	.0191411	.0056072	2.79	0.005	.08656	.008151	.030131
t5basal*	.0018206	.0079086	0.23	0.820	.115348	-.01368	.017321
t5bp*	.0142271	.0056922	2.17	0.030	.086133	.003071	.025384
educ1*	.0617752	.0028878	19.94	0.000	.187835	.056115	.067435
educ2*	.0649037	.0029977	21.91	0.000	.16688	.059028	.070779
educ3*	.0696249	.0033009	23.77	0.000	.166831	.063155	.076095
educ4*	.0712756	.0031901	26.50	0.000	.151312	.065023	.077528
educ5*	.0712746	.0033169	27.00	0.000	.145778	.064774	.077776
bpeduc1*	.0090754	.0056868	1.48	0.139	.090731	-.00207	.020221
bpeduc2*	.0039067	.0070362	0.54	0.590	.076625	-.009884	.017697
bpeduc3*	.0097988	.0067886	1.32	0.188	.076428	-.003507	.023104
bpeduc4*	-.0006961	.0080569	-0.09	0.931	.065671	-.016487	.015095
bpeduc5*	.0102371	.0063487	1.46	0.143	.061747	-.002206	.02268
obs. P	.8954084						
pred. P	.9564464	(at x-bar)					

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

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Pooled Sample Male Primary Enrollment with Schooling Interactions
Table B-4

Probit Estimates | Number of obs = 60894
chi2(59) =5905.73
Prob > chi2 = 0.0000
Log Likelihood = -12110.836 | Pseudo R2 = 0.4064

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0206098	.0187972	-1.27	0.203	.02414	-.057452	.016232
dis_sec	-.0016624	.0010362	-1.59	0.112	2.15021	-.003693	.000369
t3bped1*	.0166287	.0109305	1.24	0.213	.016126	-.004795	.038052
t3bped2*	.0034684	.0117995	0.28	0.776	.016044	-.019658	.026595
t3bped3*	.0144695	.0091975	1.33	0.183	.016323	-.003557	.032496
t3bped4*	.0001909	.010862	0.02	0.986	.01386	-.021098	.02148
t3bped5*	-.0026624	.012134	-0.22	0.822	.013466	-.026445	.02112
t4bped1*	-.1087372	.0366786	-4.41	0.000	.019148	-.180626	-.036848
t4bped2*	-.0422614	.0211086	-2.56	0.011	.015223	-.083633	-.000889
t4bped3*	-.0150725	.0187924	-0.90	0.369	.015092	-.051905	.02176
t4bped4*	-.0306188	.0189749	-1.96	0.050	.012809	-.067809	.006571
t4bped5*	-.0425652	.0247439	-2.19	0.029	.012399	-.091062	.005932
t5bped1*	-.1273674	.0416387	-4.63	0.000	.019099	-.208978	-.045757
t5bped2*	-.1091421	.0323768	-5.00	0.000	.014763	-.1726	-.045685
t5bped3*	-.1044611	.0349891	-4.42	0.000	.01524	-.173039	-.035884
t5bped4*	-.141717	.0350219	-6.22	0.000	.012924	-.210359	-.073075
t5bped5*	-.3380995	.0541005	-9.92	0.000	.012514	-.444134	-.232064
capital	.0001977	.0000249	8.62	0.000	146.354	.000149	.000247
distance	.0008783	.0003825	2.28	0.023	9.59595	.000129	.001628
t2*	.0269935	.0020381	11.90	0.000	.178326	.022999	.030988
t3*	.0122697	.0028973	3.90	0.000	.195914	.006591	.017948
t4*	.0238746	.0032128	6.19	0.000	.184172	.017578	.030171
t5*	.0237734	.0032833	6.59	0.000	.188606	.017338	.030209
basal*	.0030625	.0066097	0.47	0.641	.617778	-.009892	.016017
age6*	.0225586	.0025511	7.24	0.000	.080665	.017559	.027559
age7*	.0203934	.0026113	6.48	0.000	.117417	.015275	.025511
age9*	-.0255794	.0060282	-4.96	0.000	.125874	-.037394	-.013764
age10*	-.0565894	.0081135	-8.80	0.000	.13599	-.072492	-.040687
age11*	-.0926568	.010367	-12.32	0.000	.119913	-.112976	-.072338
age12*	-.1884503	.0156166	-19.08	0.000	.102752	-.219058	-.157842
age13*	-.3585764	.0219186	-25.83	0.000	.059809	-.401536	-.315617
age14*	-.5749099	.022875	-34.41	0.000	.039741	-.619744	-.530076
age15*	-.7721269	.0193406	-36.29	0.000	.028985	-.810034	-.73422
age16*	-.8899423	.0104545	-42.65	0.000	.022777	-.910433	-.869452
age1718*	-.9381983	.0062847	-31.72	0.000	.006487	-.950516	-.92588
pobre*	-.0174064	.0042936	-3.82	0.000	.728676	-.025822	-.008991
bp*	-.0066539	.0082764	-0.81	0.420	.455661	-.022875	.009568
nomom*	-.0042239	.0051709	-0.85	0.398	.061418	-.014359	.005911
meduc	.0038668	.000681	5.75	0.000	2.68173	.002532	.005202
nodad*	.0023524	.0041693	0.55	0.579	.125234	-.005819	.010524
deduc	.0035554	.0006595	5.39	0.000	2.75659	.002263	.004848
no_p*	-.0231993	.0095338	-2.59	0.010	.297386	-.041885	-.004513
st_p	-.000382	.0002724	-1.39	0.165	17.1068	-.000916	.000152
t3basal*	-.0044798	.0078312	-0.59	0.555	.120997	-.019829	.010869
t3bp*	.0130559	.0070996	1.61	0.106	.08973	-.000859	.026971
t4basal*	-.0135662	.009706	-1.53	0.127	.114576	-.03259	.005457
t4bp*	.0317575	.0052002	4.06	0.000	.08656	.021565	.04195
t5basal*	.0016706	.0077751	0.21	0.832	.115348	-.013568	.01691
t5bp*	.0454288	.003137	7.50	0.000	.086133	.03928	.051577
educ1*	.0609365	.0028556	20.04	0.000	.187835	.05534	.066533

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Table B-4 Continued

educ2*	.0641062	.0029599	22.19	0.000	.16688	.058305	.069907
educ3*	.0687911	.0032524	24.20	0.000	.166831	.062417	.075166
educ4*	.070315	.0031446	26.95	0.000	.151312	.064152	.076478
educ5*	.0702651	.0032618	27.37	0.000	.145778	.063872	.076658
bpeduc1*	.0210806	.0063971	2.67	0.008	.090731	.008542	.033619
bpeduc2*	.0114238	.0071888	1.43	0.152	.076625	-.002666	.025514
bpeduc3*	.0116267	.0080156	1.29	0.196	.076428	-.004084	.027337
bpeduc4*	.0086956	.0081096	0.99	0.323	.065671	-.007199	.02459
bpeduc5*	.027743	.0049992	3.87	0.000	.061747	.017945	.037541
obs. P	.8954084						
pred. P	.9575505	(at x-bar)					

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

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Pooled Sample Female Secondary Enrollment Short Specification
Table B-5

Probit Estimates

Number of obs = 24234
chi2(34) =3232.24
Prob > chi2 = 0.0000
Log Likelihood = -11983.921
Pseudo R2 = 0.2771

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	.0335302	.0893045	0.37	0.711	.01044	-.141503	.208564
dis_sec	-.0266749	.0054669	-4.90	0.000	2.07028	-.03739	-.01596
capital	.00089	.0001209	7.36	0.000	144.971	.000653	.001127
distance	.0000918	.0013994	0.07	0.948	9.77018	-.002651	.002835
t2*	.1063389	.0084312	12.21	0.000	.160064	.089814	.122864
t3*	.0599181	.0165855	3.56	0.000	.223281	.027411	.092425
t4*	.1387325	.017306	7.58	0.000	.172361	.104813	.172652
t5*	.1176058	.0205551	5.52	0.000	.21866	.077319	.157893
basal*	.0355457	.027613	1.29	0.197	.607824	-.018575	.089666
age12*	-.1571766	.0334362	-4.71	0.000	.101964	-.22271	-.091643
age13*	-.3212211	.0312544	-9.68	0.000	.187711	-.382479	-.259964
age14*	-.4796906	.0282355	-14.09	0.000	.212553	-.535031	-.42435
age15*	-.6198584	.0211546	-19.79	0.000	.221094	-.661321	-.578396
age16*	-.6737108	.0168087	-22.52	0.000	.195469	-.706655	-.640766
age1718*	-.6519086	.0089799	-25.75	0.000	.062515	-.669509	-.634308
pobre*	-.0672377	.0195403	-3.42	0.000	.580135	-.105536	-.028939
bp*	-.051305	.0433633	-1.19	0.235	.358793	-.136295	.033685
nomom*	-.0018712	.0222559	-0.08	0.933	.069035	-.045492	.04175
meduc	.0159944	.0025828	6.20	0.000	2.49509	.010932	.021057
nodad*	.0213537	.0190047	1.12	0.264	.132541	-.015895	.058602
deduc	.0159684	.0028727	5.55	0.000	2.56326	.010338	.021599
no_p*	-.0176158	.0327173	-0.54	0.590	.29995	-.081741	.046509
st_p	-.0006956	.001099	-0.63	0.527	16.5063	-.00285	.001458
t3basal*	.0040853	.026893	0.15	0.879	.137369	-.048624	.056795
t3bp*	.0804507	.0231464	3.36	0.000	.082941	.035085	.125817
t4basal*	-.0004344	.0299118	-0.01	0.988	.105761	-.05906	.058192
t4bp*	.1052273	.0263107	3.80	0.000	.065156	.053659	.156795
t5basal*	-.0423625	.0306957	-1.39	0.164	.132541	-.102525	.0178
t5bp*	.1133144	.0256599	4.18	0.000	.079599	.063022	.163607
educ6*	-.3029016	.0251937	-11.35	0.000	.556738	-.35228	-.253523
educ7*	.2258861	.0226411	8.83	0.000	.175704	.18151	.270262
educ8*	.2726494	.0177003	12.92	0.000	.134893	.237958	.307341
bpeduc6*	.0804634	.0383521	2.05	0.040	.218536	.005295	.155632
bpeduc78*	.0159624	.0390251	0.41	0.684	.103326	-.060525	.09245
obs. P	.5671371						
pred. P	.5979567	(at x-bar)					

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

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Pooled Sample Female Secondary Enrollment with Schooling Interactions

Table B-6

Probit Estimates

Number of obs = 24234
chi2(40) =3348.56
Prob > chi2 = 0.0000
Log Likelihood = -11845.38
Pseudo R2 = 0.2855

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	.0409274	.0881338	0.46	0.648	.01044	-.131812	.213666
dis_sec	-.026633	.0055238	-4.85	0.000	2.07028	-.037459	-.015807
t3bped6*	.1209945	.0471421	2.39	0.017	.050054	.028598	.213391
t3bped78*	.0518669	.0590618	0.86	0.391	.023851	-.063892	.167626
t4bped6*	.2189076	.0429961	4.13	0.000	.03883	.134637	.303178
t4bped78*	-.0784987	.0724909	-1.10	0.273	.02121	-.220578	.063581
t5bped6*	.2463248	.0341743	5.54	0.000	.042213	.179344	.313305
t5bped78*	-.3067722	.0552094	-5.04	0.000	.027193	-.414981	-.198564
capital	.0008854	.0001213	7.30	0.000	144.971	.000648	.001123
distance	.0000535	.001422	0.04	0.970	9.77018	-.002734	.002841
t2*	.1088563	.0085965	12.21	0.000	.160064	.092007	.125705
t3*	.0604199	.0165725	3.59	0.000	.223281	.027938	.092901
t4*	.1396294	.0172281	7.64	0.000	.172361	.105863	.173396
t5*	.1108537	.0205134	5.23	0.000	.21866	.070648	.151059
basal*	.0354865	.0275799	1.29	0.197	.607824	-.018569	.089542
age12*	-.1595047	.0336123	-4.76	0.000	.101964	-.225384	-.093626
age13*	-.3350739	.0310216	-10.11	0.000	.187711	-.395875	-.274273
age14*	-.4934923	.0278364	-14.53	0.000	.212553	-.548051	-.438934
age15*	-.6287048	.0209039	-20.08	0.000	.221094	-.669676	-.587734
age16*	-.681556	.0165166	-22.85	0.000	.195469	-.713928	-.649184
age1718*	-.652943	.0091866	-25.22	0.000	.062515	-.670948	-.634938
pobre*	-.0667263	.0194882	-3.40	0.000	.580135	-.104923	-.02853
bp*	.024614	.0496182	0.49	0.621	.358793	-.072636	.121864
nomom*	-.0012701	.0223472	-0.06	0.955	.069035	-.04507	.04253
meduc	.0159851	.0025927	6.17	0.000	2.49509	.010903	.021067
nodad*	.0206616	.0191074	1.07	0.282	.132541	-.016788	.058111
deduc	.015907	.0029053	5.47	0.000	2.56326	.010213	.021601
no_p*	-.0185295	.0327684	-0.57	0.571	.29995	-.082754	.045695
st_p	-.0007667	.0011014	-0.70	0.486	16.5063	-.002925	.001392
t3basal*	.0046018	.0268652	0.17	0.864	.137369	-.048053	.057257
t3bp*	-.0198871	.0506982	-0.39	0.694	.082941	-.119254	.079479
t4basal*	-.0001154	.0298646	-0.00	0.997	.105761	-.058649	.058418
t4bp*	-.0575128	.0615556	-0.94	0.345	.065156	-.17816	.063134
t5basal*	-.0440056	.0306156	-1.45	0.147	.132541	-.104011	.016
t5bp*	.0198569	.0473606	0.42	0.677	.079599	-.072968	.112682
educ6*	-.2990026	.0251527	-11.24	0.000	.556738	-.348301	-.249704
educ7*	.2307964	.0224256	9.04	0.000	.175704	.186843	.27475
educ8*	.2698887	.0174846	12.93	0.000	.134893	.23562	.304158
bpeduc6*	-.0523225	.0509376	-1.03	0.301	.218536	-.152158	.047513
bpeduc78*	.1376753	.0506507	2.53	0.012	.103326	.038402	.236949
obs. P	.5671371						
pred. P	.6021808	(at x-bar)					

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

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Pooled Sample Male Secondary Enrollment Short Specification
Table B-7

Probit Estimates

Number of obs = 25146
chi2(34) =3612.86
Prob > chi2 = 0.0000
Pseudo R2 = 0.2772

Log Likelihood = -12028.286

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0649473	.0712705	-0.94	0.349	.009942	-.204635	.07474
dis_sec	-.0225639	.0041001	-5.49	0.000	2.07704	-.0306	-.014528
capital	.0010748	.000119	8.97	0.000	146.747	.000842	.001308
distance	-.0003659	.001462	-0.25	0.802	9.52328	-.003231	.0025
t2*	.0763743	.007888	9.36	0.000	.156486	.060914	.091835
t3*	.0156668	.0156699	0.99	0.320	.223495	-.015046	.046379
t4*	.1035709	.0165043	5.91	0.000	.175217	.071223	.135919
t5*	.0479343	.0188775	2.50	0.013	.225483	.010935	.084934
basal*	.0297385	.0250763	1.19	0.233	.629802	-.01941	.078887
age12*	-.0749889	.0350863	-2.20	0.028	.091545	-.143757	-.006221
age13*	-.2145233	.0340462	-6.47	0.000	.166428	-.281253	-.147794
age14*	-.4148045	.0319218	-12.28	0.000	.214587	-.47737	-.352239
age15*	-.5783754	.0272411	-17.08	0.000	.224728	-.631767	-.524984
age16*	-.6734721	.0216491	-20.82	0.000	.214587	-.715904	-.631041
age1718*	-.7094619	.0097497	-24.69	0.000	.068679	-.728571	-.690353
pobre*	-.025361	.0176405	-1.43	0.152	.587767	-.059936	.009214
bp*	-.005325	.0366798	-0.15	0.884	.379981	-.077216	.066566
nomom*	.0262141	.0209112	1.23	0.217	.06172	-.014771	.067199
meduc	.0147933	.0027508	5.36	0.000	2.45132	.009402	.020185
nodad*	.0363124	.0164899	2.16	0.031	.130438	.003993	.068632
deduc	.0190094	.0026389	7.22	0.000	2.59544	.013837	.024182
no_p*	-.0373886	.0321123	-1.17	0.242	.295872	-.100328	.02555
st_p	-.0015767	.0010209	-1.54	0.123	16.6888	-.003578	.000424
t3basal*	.0362066	.0227989	1.56	0.119	.141374	-.008478	.080892
t3bp*	.0424022	.0216538	1.91	0.056	.087052	-.000039	.084843
t4basal*	.0158931	.0277378	0.57	0.570	.110356	-.038472	.070258
t4bp*	.0472575	.0270662	1.69	0.090	.070429	-.005791	.100306
t5basal*	-.015878	.025359	-0.63	0.528	.141852	-.065581	.033825
t5bp*	.0634866	.0207955	2.94	0.003	.086614	.022728	.104245
educ6*	-.2104627	.0234329	-8.93	0.000	.491768	-.25639	-.164535
educ7*	.2350244	.016724	11.72	0.000	.200509	.202246	.267803
educ8*	.2373068	.0145963	13.68	0.000	.157798	.208699	.265915
bpeduc6*	.0151208	.0332046	0.45	0.651	.201623	-.049959	.080201
bpeduc78*	-.0332193	.0335375	-1.00	0.315	.134614	-.098952	.032513
obs. P	.6245128						
pred. P	.6725663	(at x-bar)					

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

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Pooled Sample Male Secondary Enrollment with Schooling Interactions
Table B-8

Probit Estimates

Number of obs = 25146
chi2(40) =3587.17
Prob > chi2 = 0.0000
Pseudo R2 = 0.2844

Log Likelihood = -11909.441

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95%	C.I.]
nodissec*	-.0591877	.06958	-0.87	0.382	.009942	-.195562	.077187
dis_sec	-.0222387	.0041347	-5.37	0.000	2.07704	-.030343	-.014135
t3bped6*	.0416604	.0410752	0.99	0.324	.046091	-.038846	.122166
t3bped78*	.0338936	.0537822	0.62	0.538	.029985	-.071518	.139305
t4bped6*	.1022265	.0451187	2.07	0.039	.037421	.013795	.190658
t4bped78*	-.0993843	.0673644	-1.53	0.126	.027042	-.231416	.032647
t5bped6*	.1357012	.0365383	3.23	0.001	.040881	.064087	.207315
t5bped78*	-.3311091	.0505203	-6.31	0.000	.03261	-.430127	-.232091
capital	.0010732	.0001186	8.99	0.000	146.747	.000841	.001306
distance	-.000359	.0014503	-0.25	0.805	9.52328	-.003201	.002484
t2*	.0762904	.0080548	9.15	0.000	.156486	.060503	.092078
t3*	.0154626	.015611	0.98	0.325	.223495	-.015134	.04606
t4*	.1033302	.016402	5.92	0.000	.175217	.071183	.135478
t5*	.0437951	.0188156	2.29	0.022	.225483	.006917	.080673
basal*	.0294604	.0249653	1.19	0.236	.629802	-.019471	.078391
age12*	-.0785357	.035477	-2.28	0.023	.091545	-.148069	-.009002
age13*	-.2281374	.0343995	-6.81	0.000	.166428	-.295559	-.160716
age14*	-.4311959	.0321269	-12.59	0.000	.214587	-.494164	-.368228
age15*	-.5879267	.0274606	-17.09	0.000	.224728	-.641749	-.534105
age16*	-.6825142	.0216653	-20.82	0.000	.214587	-.724977	-.640051
age1718*	-.7121842	.010008	-24.44	0.000	.068679	-.7318	-.692569
pobre*	-.025692	.0175354	-1.46	0.144	.587767	-.060061	.008677
bp*	.0048318	.0443087	0.11	0.913	.379981	-.082012	.091675
nomom*	.0266262	.021009	1.25	0.212	.06172	-.014551	.067803
meduc	.0147219	.002748	5.33	0.000	2.45132	.009336	.020108
nodad*	.0360244	.0164466	2.15	0.032	.130438	.00379	.068259
deduc	.0190489	.0026405	7.23	0.000	2.59544	.013874	.024224
no_p*	-.0381125	.0321008	-1.19	0.232	.295872	-.101029	.024804
st_p	-.0016027	.0010217	-1.56	0.118	16.6888	-.003605	.0004
t3basal*	.0361436	.0226962	1.56	0.118	.141374	-.00834	.080627
t3bp*	.0107468	.0446723	0.24	0.811	.087052	-.076809	.098303
t4basal*	.0155752	.0276228	0.56	0.576	.110356	-.038565	.069715
t4bp*	-.0002593	.0597551	-0.00	0.997	.070429	-.117377	.116859
t5basal*	-.0167293	.0252184	-0.67	0.504	.141852	-.066156	.032698
t5bp*	.0799377	.0395773	1.90	0.057	.086614	.002368	.157508
educ6*	-.2079013	.0233677	-8.86	0.000	.491768	-.253701	-.162101
educ7*	.2355614	.0166009	11.76	0.000	.200509	.203024	.268099
educ8*	.2355494	.0143531	13.80	0.000	.157798	.207418	.263681
bpeduc6*	-.0427368	.043774	-0.99	0.322	.201623	-.128532	.043059
bpeduc78*	.0823551	.0416987	1.87	0.061	.134614	.000627	.164083
obs. P	.6245128						
pred. P	.6771394	(at x-bar)					

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

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Panel Sample Female Primary Enrollment Short Specification

Table B-9

Probit Estimates

Number of obs = 34934
chi2(44) =2597.30
Prob > chi2 = 0.0000
Pseudo R2 = 0.3528

Log Likelihood = -5370.9739

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0186685	.0170191	-1.38	0.167	.021698	-.052025	.014688
dis_sec	-.0007746	.0006642	-1.16	0.245	2.10089	-.002076	.000527
capital	.0000616	.0000219	2.76	0.006	150.01	.000019	.000104
distance	.0008508	.0002736	3.09	0.002	9.62474	.000315	.001387
t2*	.0139003	.0016537	7.39	0.000	.215836	.010659	.017141
t3*	.0034905	.0025379	1.31	0.189	.188212	-.001484	.008465
t4*	.0131434	.0022793	5.01	0.000	.188212	.008676	.017611
t5*	.0044055	.0031814	1.33	0.182	.189786	-.00183	.010641
basal*	.0014667	.0053006	0.28	0.780	.604168	-.008922	.011856
age6*	.0087392	.0027924	2.74	0.006	.065753	.003266	.014212
age7*	.0110565	.0026784	3.56	0.000	.111496	.005807	.016306
age9*	-.0113115	.0046855	-2.76	0.006	.150427	-.020495	-.002128
age10*	-.0385527	.0082034	-6.56	0.000	.1593	-.054631	-.022474
age11*	-.0859454	.012724	-10.90	0.000	.141438	-.110884	-.061007
age12*	-.2061245	.0206184	-18.20	0.000	.108719	-.246536	-.165713
age13*	-.379959	.0303415	-22.01	0.000	.053701	-.439427	-.320491
age14*	-.5601072	.0332246	-25.98	0.000	.029799	-.625226	-.494988
age15*	-.6862514	.0306324	-28.31	0.000	.015143	-.74629	-.626213
age16*	-.8193577	.030711	-23.69	0.000	.008044	-.87955	-.759165
age1718*	-.8590394	.0451212	-13.96	0.000	.001517	-.947475	-.770604
pobre*	-.0123704	.0029835	-3.77	0.000	.731236	-.018218	-.006523
bp*	-.0073465	.0067698	-1.11	0.267	.452224	-.020615	.005922
nomom*	-.0000267	.0053289	-0.00	0.996	.046774	-.010471	.010418
meduc	.0028052	.0005043	5.68	0.000	2.84989	.001817	.003794
nodad*	.0055832	.0034575	1.45	0.146	.103366	-.001193	.01236
deduc	.0016366	.0004935	3.27	0.001	2.92546	.000669	.002604
no_p*	-.0135084	.0052606	-2.84	0.004	.292952	-.023819	-.003198
st_p	-.0003572	.0001255	-2.86	0.004	17.4063	-.000603	-.000111
t3basal*	-.0026896	.0064177	-0.43	0.664	.113671	-.015268	.009889
t3bp*	.0100757	.0044717	1.83	0.067	.085619	.001311	.01884
t4basal*	-.0034582	.0073415	-0.49	0.622	.113671	-.017847	.010931
t4bp*	.0117895	.0040637	2.29	0.022	.085619	.003825	.019754
t5basal*	-.0063042	.0074698	-0.92	0.358	.114187	-.020945	.008336
t5bp*	.0166591	.0030235	3.87	0.000	.086134	.010733	.022585
educ1*	.0346706	.0026413	10.80	0.000	.180397	.029494	.039847
educ2*	.0354393	.0028031	11.70	0.000	.179195	.029945	.040933
educ3*	.0392785	.0024732	15.42	0.000	.182573	.034431	.044126
educ4*	.043531	.0027016	18.81	0.000	.1666	.038236	.048826
educ5*	.0386683	.0024737	17.90	0.000	.155436	.03382	.043517
bpeduc1*	-.0025819	.0084758	-0.32	0.752	.086248	-.019194	.01403
bpeduc2*	.0016243	.0068228	0.23	0.817	.085075	-.011748	.014997
bpeduc3*	.0070946	.0051265	1.23	0.217	.084359	-.002953	.017142
bpeduc4*	.0064474	.0054719	1.05	0.296	.071363	-.004277	.017172
bpeduc5*	.0139864	.0029654	3.54	0.000	.063548	.008174	.019799
obs. P	.9361081						
pred. P	.9771317	(at x-bar)					

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

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Panel Sample Female Primary Enrollment with Schooling Interactions
Table B-10

Probit Estimates — Number of obs = 34934; chi2(59) = 3122.55; Prob > chi2 = 0.0000; Pseudo R2 = 0.3599

Log Likelihood = -5311.8854

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0178757	.0165022	-1.36	0.173	.021698	-.050219	.014468
dis_sec	-.0007062	.0006433	-1.09	0.275	2.10089	-.001967	.000555
t3bped1*	.0088663	.0061558	1.16	0.248	.015887	-.003199	.020931
t3bped2*	.0098872	.0054132	1.43	0.153	.01666	-.000722	.020497
t3bped3*	.01049	.0052657	1.53	0.127	.017232	.000169	.02081
t3bped4*	.0133206	.0045404	1.94	0.053	.014799	.004422	.02222
t3bped5*	.0153614	.0037771	2.42	0.015	.012853	.007958	.022764
t4bped1*	-.004712	.013338	-0.38	0.702	.015887	-.030854	.02143
t4bped2*	-.0046039	.0111997	-0.44	0.657	.01666	-.026555	.017347
t4bped3*	-.0068722	.0121341	-0.63	0.529	.017232	-.030655	.01691
t4bped4*	-.0041052	.012266	-0.36	0.720	.014799	-.028146	.019936
t4bped5*	-.0067085	.012538	-0.60	0.551	.012853	-.031283	.017866
t5bped1*	-.0254025	.0208527	-1.63	0.104	.015887	-.066273	.015468
t5bped2*	-.0196788	.0181039	-1.38	0.168	.01686	-.055162	.015804
t5bped3*	-.0406197	.0251972	-2.32	0.020	.017204	-.090005	.008766
t5bped4*	-.0220891	.0175726	-1.63	0.103	.014828	-.056531	.012352
t5bped5*	-.2374957	.0595291	-7.41	0.000	.013025	-.354171	-.120821
capital	.0000601	.0000214	2.76	0.006	150.01	.000018	.000102
distance	.0008248	.0002645	3.10	0.002	9.62474	.000306	.001343
t2*	.0134626	.0016204	7.35	0.000	.215836	.010287	.016639
t3*	.0035855	.0024616	1.39	0.165	.188212	-.001239	.00841
t4*	.0130075	.0022083	5.12	0.000	.188212	.008679	.017336
t5*	.0046052	.0030695	1.44	0.150	.189786	-.001411	.010621
basal*	.0014856	.0051488	0.29	0.771	.604168	-.008606	.011577
age6*	.0102026	.0025719	3.35	0.000	.065753	.005162	.015243
age7*	.0116321	.0025497	3.87	0.000	.111496	.006635	.016629
age9*	-.0121524	.0046815	-3.00	0.003	.150427	-.021328	-.002977
age10*	-.039555	.008334	-6.71	0.000	.1593	-.055889	-.023221
age11*	-.0890281	.0129397	-11.31	0.000	.141438	-.114389	-.063667
age12*	-.2074525	.0209822	-18.18	0.000	.108719	-.248577	-.166328
age13*	-.3780244	.0306923	-21.79	0.000	.053701	-.43818	-.317869
age14*	-.5634623	.0335147	-25.89	0.000	.029799	-.62915	-.497775
age15*	-.6889117	.0310048	-28.09	0.000	.015143	-.74968	-.628143
age16*	-.8214224	.0305863	-23.85	0.000	.008044	-.88137	-.761474
age1718*	-.8623209	.042935	-14.56	0.000	.001517	-.946472	-.77817
pobre*	-.011963	.0029	-3.75	0.000	.731236	-.017647	-.006279
bp*	-.0094154	.0068255	-1.42	0.154	.452224	-.022793	.003962
nomom*	-.0000963	.0052274	-0.02	0.985	.046774	-.010342	.010149
meduc	.0027765	.000493	5.75	0.000	2.84989	.00181	.003743
nodad*	.005439	.0033715	1.45	0.147	.103366	-.001169	.012047
deduc	.0016326	.0004839	3.32	0.000	2.92546	.000684	.002581
no_p*	-.0132694	.0051585	-2.85	0.004	.292952	-.02338	-.003159
st_p	-.0003523	.0001218	-2.90	0.004	17.4063	-.000591	-.000113
t3basal*	-.0025518	.0062573	-0.42	0.672	.113671	-.014816	.009712
t3bp*	-.0006769	.0075049	-0.09	0.927	.085619	-.015386	.014032
t4basal*	-.0033861	.0071693	-0.50	0.620	.113671	-.017438	.010665
t4bp*	.013247	.0049217	2.01	0.044	.085619	.003601	.022893
t5basal*	-.0061767	.0072939	-0.92	0.356	.114187	-.020472	.008119
t5bp*	.0254613	.0022095	6.14	0.000	.086134	.021131	.029792
educ1*	.0338859	.0026003	10.87	0.000	.180397	.028789	.038982

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Table B-10 Continued

educ2*	.0348033	.0027552	11.89	0.000	.179195	.029403	.040203
educ3*	.0385871	.002464	15.64	0.000	.182573	.033758	.043416
educ4*	.0426498	.0026921	19.05	0.000	.1666	.037373	.047926
educ5*	.0378408	.002456	18.14	0.000	.155436	.033027	.042655
bpeduc1*	-.0021012	.0088966	-0.24	0.807	.086248	-.019538	.015336
bpeduc2*	.0011289	.0083046	0.13	0.894	.085075	-.015148	.017406
bpeduc3*	.0078718	.0058455	1.17	0.244	.084359	-.003585	.019329
bpeduc4*	.0046695	.0061898	0.69	0.488	.071363	-.007462	.016801
bpeduc5*	.0199634	.0024399	4.84	0.000	.063548	.015181	.024746
obs. P	.9361081						
pred. P	.9779759	(at x-bar)					

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

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Panel Sample Male Primary Enrollment Short Specification
Table B-11

Probit Estimates

Number of obs = 37541
chi2(44) =2477.00
Prob > chi2 = 0.0000
Pseudo R2 = 0.3535

Log Likelihood = -5905.4546

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0090038	.0098044	-1.04	0.300	.016462	-.02822	.010213
dis_sec	-.001514	.0007068	-2.09	0.036	2.09418	-.002899	-.000129
capital	.0001495	.0000188	8.46	0.000	150.495	.000113	.000186
distance	.0007817	.0002377	3.27	0.001	9.52723	.000316	.001248
t2*	.0152313	.0015819	8.63	0.000	.217629	.012131	.018332
t3*	.008733	.0025748	3.06	0.002	.189393	.003687	.013779
t4*	.011955	.0028216	3.61	0.000	.189393	.006425	.017485
t5*	.0071243	.0030268	2.20	0.028	.187928	.001192	.013057
basal*	.0056435	.0059071	0.98	0.327	.612397	-.005934	.017221
age6*	.0102223	.0029006	3.00	0.003	.061373	.004537	.015907
age7*	.0106769	.0025593	3.56	0.000	.106603	.005661	.015693
age9*	-.0173637	.0055022	-3.77	0.000	.14363	-.028148	-.006579
age10*	-.0372148	.0079946	-6.23	0.000	.151967	-.052884	-.021546
age11*	-.0663468	.0105805	-9.08	0.000	.134866	-.087084	-.045609
age12*	-.1390053	.0164424	-14.18	0.000	.113369	-.171232	-.106779
age13*	-.2886525	.0267669	-18.92	0.000	.06377	-.341115	-.23619
age14*	-.5097067	.030948	-26.16	0.000	.039237	-.570364	-.44905
age15*	-.7305961	.0306686	-26.54	0.000	.02123	-.790705	-.670487
age16*	-.8469168	.0238875	-27.00	0.000	.009456	-.893735	-.800098
age1718*	-.9280742	.025421	-14.17	0.000	.001625	-.977898	-.87825
pobre*	-.0105593	.0032771	-2.98	0.003	.732399	-.016982	-.004136
bp*	-.0036131	.0068567	-0.53	0.596	.461069	-.017052	.009826
nomom*	.0037959	.0046379	0.77	0.443	.04888	-.005294	.012886
meduc	.001795	.000536	3.29	0.001	2.78543	.000745	.002846
nodad*	-.0002369	.0044934	-0.05	0.958	.108441	-.009044	.00857
deduc	.002247	.0005775	3.92	0.000	2.8763	.001115	.003379
no_p*	-.0163051	.0060858	-2.95	0.003	.290323	-.028233	-.004377
st_p	-.0003562	.0001635	-2.18	0.029	17.3428	-.000677	-.000036
t3basal*	-.0140073	.0088602	-1.86	0.063	.115474	-.031373	.003358
t3bp*	.0164725	.0034595	3.50	0.000	.087584	.009692	.023253
t4basal*	-.0140789	.0092958	-1.77	0.077	.115474	-.032298	.004141
t4bp*	.016872	.0034911	3.59	0.000	.087584	.01003	.023714
t5basal*	-.0024684	.0070185	-0.36	0.717	.114994	-.016224	.011288
t5bp*	.0136543	.0037691	2.79	0.005	.087105	.006267	.021042
educ1*	.0373587	.0025643	13.15	0.000	.18308	.032333	.042385
educ2*	.0402081	.0026871	13.88	0.000	.186942	.034942	.045475
educ3*	.0425624	.0028101	15.35	0.000	.179031	.037055	.04807
educ4*	.0444174	.0027851	17.27	0.000	.165499	.038959	.049876
educ5*	.0436512	.002765	18.18	0.000	.158493	.038232	.04907
bpeduc1*	.0052375	.0061019	0.79	0.430	.090168	-.006722	.017197
bpeduc2*	-.0003703	.0070766	-0.05	0.958	.087185	-.01424	.0135
bpeduc3*	.0037731	.0061503	0.58	0.563	.083722	-.008281	.015828
bpeduc4*	-.0007093	.0069359	-0.10	0.918	.072987	-.014303	.012885
bpeduc5*	.0045938	.0054107	0.79	0.431	.067899	-.006011	.015199
obs. P	.9339389						
pred. P	.9753408	(at x-bar)					

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

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Panel Sample Male Primary Enrollment with Schooling Interaction
Table B-12

Probit Estimates — Number of obs = 37541; chi2(59) =2942.00; Prob > chi2 = 0.0000; Pseudo R2 = 0.3587

Log Likelihood = -5858.2609

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.0083507	.0096469	-0.97	0.330	.016462	-.027258	.010557
dis_sec	-.0014618	.0006885	-2.07	0.038	2.09418	-.002811	-.000112
t3bped1*	.0138608	.007655	1.22	0.224	.016382	-.001143	.028864
t3bped2*	-.0012369	.011952	-0.11	0.916	.017607	-.024662	.022189
t3bped3*	.0146559	.0054997	1.75	0.080	.017048	.003877	.025435
t3bped4*	-.0096806	.0132255	-0.83	0.404	.014278	-.035602	.016241
t3bped5*	-.000526	.0112122	-0.05	0.962	.014438	-.022501	.021449
t4bped1*	-.0267383	.0248326	-1.43	0.153	.016382	-.075409	.021933
t4bped2*	-.0081979	.0135422	-0.68	0.498	.017607	-.03474	.018344
t4bped3*	.0010964	.0116514	0.09	0.926	.017048	-.02174	.023933
t4bped4*	-.0288279	.0207606	-1.85	0.064	.014278	-.069518	.011862
t4bped5*	-.0366131	.023315	-2.19	0.029	.014438	-.08231	.009083
t5bped1*	-.0211156	.0254074	-1.06	0.290	.016382	-.070913	.028682
t5bped2*	-.061303	.0335038	-2.81	0.005	.017421	-.126969	.004363
t5bped3*	-.044821	.0284102	-2.29	0.022	.017075	-.100504	.010862
t5bped4*	-.0843436	.0370884	-3.71	0.000	.014251	-.157035	-.011652
t5bped5*	-.244121	.0682434	-6.53	0.000	.014278	-.377875	-.110366
capital	.0001457	.0000185	8.47	0.000	150.495	.000109	.000182
distance	.0007815	.0002325	3.33	0.000	9.52723	.000326	.001237
t2*	.0146954	.0015844	8.52	0.000	.217629	.01159	.017801
t3*	.0085796	.0025034	3.09	0.002	.189393	.003673	.013486
t4*	.0118626	.0027262	3.70	0.000	.189393	.006519	.017206
t5*	.0072488	.0029168	2.31	0.021	.187928	.001532	.012966
basal*	.0055377	.0057479	0.99	0.322	.612397	-.005728	.016803
age6*	.0119735	.0026535	3.69	0.000	.061373	.006773	.017174
age7*	.011341	.0024378	3.92	0.000	.106603	.006563	.016119
age9*	-.0179675	.0054292	-3.98	0.000	.14363	-.028608	-.007327
age10*	-.0377537	.0078801	-6.45	0.000	.151967	-.053198	-.022309
age11*	-.0685112	.0105202	-9.50	0.000	.134866	-.08913	-.047892
age12*	-.1406044	.0162784	-14.47	0.000	.113369	-.172509	-.108699
age13*	-.2900085	.0265086	-19.13	0.000	.06377	-.341964	-.238053
age14*	-.5134145	.0304983	-26.56	0.000	.039237	-.57319	-.453639
age15*	-.7322941	.0302589	-26.85	0.000	.02123	-.7916	-.672988
age16*	-.8501749	.0230244	-27.76	0.000	.009456	-.895302	-.805048
age1718*	-.9293295	.0238997	-15.03	0.000	.001625	-.976172	-.882487
pobre*	-.0102558	.0031968	-2.97	0.003	.732399	-.016521	-.00399
bp*	-.0083479	.0072179	-1.18	0.239	.461069	-.022495	.005799
nomom*	.0037792	.0044981	0.79	0.432	.04888	-.005037	.012595
meduc	.0017202	.0005242	3.23	0.001	2.78543	.000693	.002748
nodad*	-.000363	.004413	-0.08	0.934	.108441	-.009012	.008286
deduc	.0022212	.000563	3.97	0.000	2.8763	.001118	.003325
no_p*	-.0158031	.0059518	-2.92	0.003	.290323	-.027469	-.004138
st_p	-.0003413	.0001598	-2.13	0.033	17.3428	-.000654	-.000028
t3basal*	-.0137134	.0086999	-1.85	0.064	.115474	-.030765	.003338
t3bp*	.0137327	.0050457	2.10	0.036	.087584	.003843	.023622
t4basal*	-.0138077	.0091198	-1.77	0.076	.115474	-.031682	.004067
t4bp*	.0207599	.0037676	3.42	0.000	.087584	.013376	.028144
t5basal*	-.0024891	.0068593	-0.37	0.708	.114994	-.015933	.010955
t5bp*	.0272704	.0025946	5.12	0.000	.087105	.022185	.032356
educ1*	.0365834	.0025615	13.21	0.000	.18308	.031563	.041604

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Table B-12 Continued

educ2*	.0395701	.002687	14.18	0.000	.186942	.034304	.044836
educ3*	.0418888	.0028023	15.79	0.000	.179031	.036396	.047381
educ4*	.0436479	.0027856	17.78	0.000	.165499	.038188	.049108
educ5*	.0428619	.0027552	18.61	0.000	.158493	.037462	.048262
bpeduc1*	.0063788	.007017	0.82	0.413	.090168	-.007374	.020132
bpeduc2*	.0035904	.0073774	0.46	0.645	.087185	-.010869	.01805
bpeduc3*	.0017354	.0076308	0.22	0.825	.083722	-.013221	.016692
bpeduc4*	.0075177	.0061786	1.07	0.287	.072987	-.004592	.019627
bpeduc5*	.0173162	.0036798	3.15	0.002	.067899	.010104	.024528
obs. P	.9339389						
pred. P	.976207	(at x-bar)					

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

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Panel Sample Female Secondary Enrollment Short Specification
Table B-13

Probit Estimates

Number of obs = 12733
chi2(34) =1527.64
Prob > chi2 = 0.0000
Pseudo R2 = 0.2557

Log Likelihood = -6030.8986

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	.0359622	.0674742	0.51	0.607	.008875	-.096285	.168209
dis_sec	-.0289146	.0060308	-4.86	0.000	2.01568	-.040735	-.017094
capital	.0007945	.000132	6.07	0.000	145.876	.000536	.001053
distance	.0002311	.0015827	0.15	0.884	9.73059	-.002871	.003333
t2*	.0839524	.0087503	9.15	0.000	.153381	.066802	.101103
t3*	.057586	.0171467	3.24	0.001	.229168	.023979	.091193
t4*	.1186758	.0172892	6.33	0.000	.229168	.08479	.152562
t5*	.0809908	.0217258	3.55	0.000	.232781	.038409	.123573
basal*	.0338408	.0348076	0.98	0.328	.602764	-.034381	.102063
age12*	-.1360538	.0403671	-3.54	0.000	.138381	-.215172	-.056936
age13*	-.2761577	.0416015	-6.89	0.000	.246917	-.357695	-.19462
age14*	-.4371635	.040673	-10.48	0.000	.258698	-.516881	-.357446
age15*	-.613268	.0338362	-14.62	0.000	.203016	-.679586	-.54695
age16*	-.6749823	.0270227	-15.50	0.000	.109244	-.727946	-.622019
age1718*	-.6949927	.0171942	-14.59	0.000	.019634	-.728693	-.661293
pobre*	-.0652908	.0230393	-2.78	0.005	.595932	-.110447	-.020135
bp*	-.0760537	.0676816	-1.14	0.254	.366764	-.208707	.0566
nomom*	.0427032	.0294931	1.39	0.163	.046808	-.015102	.100509
meduc	.0155948	.0032214	4.88	0.000	2.70274	.009281	.021909
nodad*	.0209925	.0242085	0.85	0.393	.106809	-.026455	.06844
deduc	.0132765	.0033552	3.96	0.000	2.74633	.0067	.019853
no_p*	-.0051703	.0368654	-0.14	0.888	.302285	-.077425	.067084
st_p	-.000298	.0012468	-0.24	0.811	16.5999	-.002742	.002146
t3basal*	.004881	.0308956	0.16	0.875	.138695	-.055673	.065435
t3bp*	.1105391	.0249617	3.95	0.000	.085369	.061615	.159463
t4basal*	-.019043	.0342184	-0.56	0.573	.138695	-.08611	.048024
t4bp*	.1337271	.0253885	4.51	0.000	.085369	.083967	.183488
t5basal*	-.0215077	.038123	-0.57	0.568	.141051	-.096227	.053212
t5bp*	.1292883	.0274324	4.06	0.000	.087018	.075522	.183055
educ6*	-.2236812	.0323385	-6.60	0.000	.587528	-.287063	-.160299
educ7*	.2470275	.0248524	7.42	0.000	.195084	.198318	.295737
educ8*	.1945269	.0210358	7.44	0.000	.136103	.153298	.235756
bpeduc6*	.0860843	.0529246	1.55	0.122	.236865	-.017646	.189815
bpeduc78*	.0243715	.0611776	0.39	0.696	.108772	-.095534	.144278
obs. P	.6668499						
pred. P	.7249791	(at x-bar)					

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

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Panel Sample Female Secondary Enrollment with Schooling Interactions
Table B-14

Probit Estimates	Number of obs = 12733
	chi2(40) =1571.11
	Prob > chi2 = 0.0000
Log Likelihood = -5976.1332	Pseudo R2 = 0.2625

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	.0277696	.0685155	0.39	0.693	.008875	-.106518	.162058
dis_sec	-.0289382	.0060341	-4.86	0.000	2.01568	-.040765	-.017112
t3bped6*	.2022958	.0505806	2.64	0.008	.052698	.10316	.301432
t3bped78*	.1898898	.0567509	2.22	0.027	.026859	.07866	.301119
t4bped6*	.2414627	.0367471	3.50	0.000	.052698	.16944	.313486
t4bped78*	.1597268	.0714083	1.68	0.094	.026859	.019769	.299684
t5bped6*	.2810833	.0219301	4.76	0.000	.053954	.238101	.324065
t5bped78*	.0961994	.0976905	0.87	0.385	.027016	-.09527	.287669
capital	.0007955	.0001324	6.05	0.000	145.876	.000536	.001055
distance	.0002195	.001587	0.14	0.890	9.73059	-.002891	.00333
t2*	.0851325	.0089116	9.07	0.000	.153381	.067666	.102599
t3*	.0578792	.0170666	3.27	0.001	.229168	.024429	.091329
t4*	.1190199	.0171513	6.39	0.000	.229168	.085404	.152636
t5*	.0802435	.0215747	3.54	0.000	.232781	.037958	.122529
basal*	.0336232	.0345913	0.98	0.328	.602764	-.034175	.101421
age12*	-.136886	.0403403	-3.58	0.000	.138381	-.215952	-.05782
age13*	-.2866618	.0415983	-7.16	0.000	.246917	-.368193	-.205131
age14*	-.4507843	.0405643	-10.78	0.000	.258698	-.530289	-.37128
age15*	-.6231822	.0335107	-14.85	0.000	.203016	-.688862	-.557502
age16*	-.6800075	.0268208	-15.66	0.000	.109244	-.732575	-.62744
age1718*	-.692478	.018581	-14.32	0.000	.019634	-.728896	-.65606
pobre*	-.0649943	.0228781	-2.79	0.005	.595932	-.109835	-.020154
bp*	.1912944	.0935826	1.90	0.058	.366764	.007876	.374713
nomom*	.0425831	.0293631	1.40	0.163	.046808	-.014967	.100134
meduc	.0156625	.0032179	4.90	0.000	2.70274	.009356	.021969
nodad*	.0206324	.0241032	0.84	0.399	.106809	-.026609	.067874
deduc	.0133785	.0033738	3.97	0.000	2.74633	.006766	.019991
no_p*	-.0037728	.0368231	-0.10	0.918	.302285	-.075945	.068399
st_p	-.0003169	.0012469	-0.25	0.799	16.5999	-.002761	.002127
t3basal*	.004987	.0307494	0.16	0.872	.138695	-.055281	.065255
t3bp*	-.1517936	.117655	-1.37	0.170	.085369	-.382393	.078806
t4basal*	-.0192335	.0340609	-0.57	0.568	.138695	-.085992	.047525
t4bp*	-.1904963	.1198847	-1.69	0.091	.085369	-.425466	.044473
t5basal*	-.0223727	.0379537	-0.60	0.550	.141051	-.096761	.052015
t5bp*	-.2871804	.1303615	-2.30	0.021	.087018	-.542684	-.031677
educ6*	-.2200369	.0321652	-6.54	0.000	.587528	-.283079	-.156994
educ7*	.2458518	.0245396	7.43	0.000	.195084	.197755	.293949
educ8*	.1957502	.0205655	7.62	0.000	.136103	.155443	.236058
bpeduc6*	-.2429223	.1194301	-2.12	0.034	.236865	-.477001	-.008844
bpeduc78*	-.1214092	.1338932	-0.96	0.338	.108772	-.383835	.141017
obs. P	.6668499						
pred. P	.7290743	(at x-bar)					

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

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Panel Sample Male Secondary Enrollment Short Specifications
Table B-15

Probit Estimates

Number of obs = 13372
chi2(34) =1516.03
Prob > chi2 = 0.0000
Pseudo R2 = 0.2517

Log Likelihood = -5915.0549

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95%	C.I.]
nodissec*	-.1055907	.0777653	-1.48	0.138	.009049	-.258008	.046827
dis_sec	-.0212749	.0038893	-5.47	0.000	2.04987	-.028898	-.013652
capital	.0009328	.0001224	7.41	0.000	150.09	.000693	.001173
distance	.0001465	.0014194	0.10	0.918	9.36521	-.002635	.002928
t2*	.0633106	.0080953	7.47	0.000	.15353	.047444	.079177
t3*	.007727	.0185967	0.41	0.679	.2328	-.028722	.044176
t4*	.0622319	.0179804	3.31	0.000	.2328	.026991	.097473
t5*	.0213453	.0215671	0.98	0.329	.22936	-.020925	.063616
basal*	.0509413	.0315695	1.64	0.101	.626608	-.010934	.112816
age12*	-.0793044	.0384511	-2.20	0.028	.12429	-.154667	-.003942
age13*	-.1924441	.0414194	-5.08	0.000	.220386	-.273625	-.111264
age14*	-.3704615	.0429013	-9.24	0.000	.262115	-.454546	-.286377
age15*	-.538932	.0415833	-12.58	0.000	.21545	-.620434	-.45743
age16*	-.6642123	.0351318	-14.62	0.000	.131693	-.733069	-.595355
age1718*	-.7273866	.0217404	-14.96	0.000	.021388	-.769997	-.684776
pobre*	-.0219153	.0191908	-1.13	0.256	.619354	-.059529	.015698
bp*	-.0631299	.0510789	-1.25	0.210	.406671	-.163243	.036983
nomom*	.0284243	.0267147	1.02	0.306	.047487	-.023935	.080784
meduc	.0125808	.0032491	3.85	0.000	2.60993	.006213	.018949
nodad*	.0446015	.0179107	2.35	0.019	.113895	.009497	.079706
deduc	.0170173	.0031525	5.41	0.000	2.77251	.010839	.023196
no_p*	-.023024	.0306904	-0.76	0.448	.295244	-.083176	.037128
st_p	-.0006199	.000936	-0.66	0.508	16.7038	-.002454	.001215
t3basal*	.0411143	.0280754	1.40	0.161	.146425	-.013913	.096141
t3bp*	.0530139	.0240722	2.05	0.040	.095498	.005833	.100195
t4basal*	.0097858	.0290832	0.33	0.739	.146425	-.047216	.066788
t4bp*	.0636247	.0228856	2.54	0.011	.095498	.01877	.10848
t5basal*	-.0193613	.0334414	-0.59	0.556	.144182	-.084905	.046183
t5bp*	.0766672	.0230965	2.97	0.003	.093928	.031399	.121935
educ6*	-.1214202	.0290179	-4.18	0.000	.525277	-.178294	-.064546
educ7*	.2153389	.0187117	8.73	0.000	.225845	.178665	.252013
educ8*	.1723819	.0165233	8.24	0.000	.158989	.139997	.204767
bpeduc6*	.0293486	.0422394	0.68	0.497	.228687	-.053439	.112136
bpeduc78*	-.021	.0444398	-0.48	0.630	.151885	-.1081	.0661
obs. P	.7219563						
pred. P	.7865362	(at x-bar)					

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

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Panel Sample Male Secondary Enrollment with Schooling Interactions
Table B-16

Probit Estimates

Number of obs = 13372
chi2(40) =1462.49
Prob > chi2 = 0.0000
Pseudo R2 = 0.2575

Log Likelihood = -5869.1199

(standard errors adjusted for clustering on eml)

inschl	dF/dx	Robust Std. Err.	z	P>|z|	x-bar	[95% C.I.	]
nodissec*	-.1063476	.0773454	-1.51	0.132	.009049	-.257942	.045247
dis_sec	-.0210259	.0038921	-5.40	0.000	2.04987	-.028654	-.013398
t3bped6*	-.0482484	.0877867	-0.58	0.563	.051226	-.220307	.12381
t3bped78*	.0514003	.0770053	0.61	0.540	.036793	-.099527	.202328
t4bped6*	.019701	.0750104	0.26	0.798	.051226	-.127317	.166719
t4bped78*	-.0705324	.1014284	-0.75	0.456	.036793	-.269328	.128264
t5bped6*	.0700968	.067195	0.92	0.355	.05003	-.061603	.201797
t5bped78*	-.2247694	.1173408	-2.15	0.032	.036644	-.454753	.005214
capital	.0009251	.0001216	7.40	0.000	150.09	.000687	.001163
distance	.0001952	.0014021	0.14	0.889	9.36521	-.002553	.002943
t2*	.0630071	.0081693	7.36	0.000	.15353	.046995	.079019
t3*	.0075338	.0184158	0.41	0.684	.2328	-.02856	.043628
t4*	.0615303	.0177821	3.30	0.001	.2328	.026678	.096383
t5*	.0206092	.0213543	0.95	0.341	.22936	-.021244	.062463
basal*	.0502217	.0312383	1.64	0.102	.626608	-.011004	.111448
age12*	-.0794428	.0385497	-2.20	0.027	.12429	-.154999	-.003887
age13*	-.2006647	.0418107	-5.28	0.000	.220386	-.282612	-.118717
age14*	-.3822946	.0433594	-9.44	0.000	.262115	-.467277	-.297312
age15*	-.5450989	.0419805	-12.60	0.000	.21545	-.627379	-.462819
age16*	-.6686106	.0353353	-14.65	0.000	.131693	-.737867	-.599355
age1718*	-.7291464	.0220952	-15.08	0.000	.021388	-.772452	-.685841
pobre*	-.0218915	.0189509	-1.15	0.251	.619354	-.059035	.015252
bp*	-.0699324	.081767	-0.87	0.385	.406671	-.230193	.090328
nomom*	.0288094	.0264839	1.04	0.296	.047487	-.023098	.080717
meduc	.0123567	.0032265	3.80	0.000	2.60993	.006033	.018681
nodad*	.0439093	.0177402	2.33	0.020	.113895	.009139	.07868
deduc	.0169089	.0031353	5.41	0.000	2.77251	.010764	.023054
no_p*	-.0226143	.0304748	-0.75	0.453	.295244	-.082344	.037115
st_p	-.0006094	.0009289	-0.65	0.513	16.7038	-.00243	.001211
t3basal*	.040569	.0277489	1.40	0.162	.146425	-.013818	.094956
t3bp*	.0689064	.0652128	0.95	0.340	.095498	-.058908	.196721
t4basal*	.0095105	.0287673	0.33	0.743	.146425	-.046872	.065893
t4bp*	.0613599	.0690694	0.81	0.416	.095498	-.074014	.196733
t5basal*	-.0195838	.0331083	-0.60	0.547	.144182	-.084475	.045307
t5bp*	.0842653	.0657716	1.12	0.263	.093928	-.044645	.213175
educ6*	-.1187285	.028689	-4.14	0.000	.525277	-.174958	-.062499
educ7*	.2125953	.0184367	8.73	0.000	.225845	.17646	.24873
educ8*	.1713602	.0161719	8.36	0.000	.158989	.139664	.203057
bpeduc6*	.0189477	.0743575	0.25	0.802	.228687	-.12679	.164686
bpeduc78*	.0539005	.0727225	0.70	0.487	.151885	-.088633	.196434
obs. P	.7219563						
pred. P	.7911446	(at x-bar)					

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