INTERNATIONAL
FOOD
POLICY
RESEARCH
INSTITUTE

FINAL REPORT

THE IMPACT OF PROGRESA ON ACHIEVEMENT TEST SCORES IN THE FIRST YEAR

Jere R. Behrman
Piyali Sengupta
Petra Todd

International Food Policy Research Institute
Food Consumption and Nutrition Division
2033 K Street, NW
Washington, DC, 20006, USA
Tel (202) 862-5600
Fax (202) 467-4439

September 20, 2000

CONTENTS

ACKNOWLEDGMENTS

This research report is part of the PROGRESA Evaluation Project of IFPRI. Behrman, Sengupta, and Todd all worked on this report as consultants to the project. They, and not IFPRI or PROGRESA, are responsible for all contents of this report. Correspondence regarding this paper should be sent to Jere R. Behrman, Economics, McNeil 160, 3718 Locust Walk, University of Pennsylvania, Philadelphia, PA 19104-6297, USA; telephone 215 898 7704, fax 215 573 2057, e-mail:jbehrman@econ.sas.upenn.edu.

EXECUTIVE SUMMARY

Increasing human capital investments in children is considered to be among the most effective ways of alleviating poverty and encouraging growth in developing countries. One possibly important channel through which such investments may have such impacts is through increasing cognitive achievement of children. Previous literature suggests that improved cognitive achievement has payoffs in terms of greater wages and perhaps productivities in labor markets in developing countries.

This paper evaluates the short-run effects on children's cognitive achievements of PROGRESA. One major component of PROGRESA is transfer payments to poor families with children enrolled in grades 3-6 of primary school and grades 1-3 of secondary school. Other components of PROGRESA include general transfers to such families and explicit nutrition supplements and health support for infants and small children. PROGRESA might have impact on children's cognitive achievement through a number of channels, some of which are relatively short run and others are relatively longer run. Previous IFRPI-PROGRESA Evaluation Project papers have considered some aspects of possible changes induced by PROGRESA that may have led or may eventually lead to improved cognitive achievement. But none of these studies consider the effects on child achievement test scores.

Section 1 introduces the topic. Section 2 describes the PROGRESA program, with emphasis on the features directly related to schooling, which focus on enrollment and attendance. The payment schedule for transfers for school attendance is presented and discussed, with reference to the increasing payments with grade level because of perceived increasing opportunity costs with grade levels and higher payments for females than for males at the secondary level because of perceived lower enrollment rates for females than males at those levels. Section 3 outlines methods for estimating the impact of PROGRESA on test scores. These include methods to control to the extent that the data permit for changing composition of those who take the tests. If, for example, PROGRESA induces greater enrollments of marginal students, the change in composition of the students taking the tests in itself will cause test scores to fall.

Section 4 presents data on enrollments and summarizes the patterns in enrollment because the program is directly conditional on enrollment, as well as patterns in age-specific schooling attainment gaps. These estimates address three general points:

1. The Relation Between Pre-program Enrollments and Differential Incentives by Age and Sex in the Payment Structure:

    (i) In the pre-PROGRESA 1997 data, enrollment rates averaged over 90% for the 7-11 age range, and then fell by about 10 percent for every added year of age. This is consistent with the general pattern of incentives from transfers becoming positive for later grades of primary school and increasing with the grade level But it suggests that the incentives could have little effect for primary grades 3, 4 and possibly 5 because most children in these grades are in the 8-11 age range and over 90% of this age range were enrolled prior to PROGRESA. Thus, from looking at the pre-program data alone, it would appear that the same resources could have been used more effectively to increase enrollments by directing them more to higher grades and older children.

(ii) In the pre-PROGRESA 1997 data, enrollment rates fell substantially for ages right above those at which most children finished primary school – ages 12-14, which relates to the rationale for the large percentage increase in the transfers for secondary grade 1. But enrollments continued to fall fairly sharply for still older ages, at which ages many children who are still in school are in secondary grade 2 or higher. So it is not clear that such a sharp increase in the transfer between primary grade 6 and secondary grade 1 compared to the increases for advancing in secondary school beyond grade 1 is warranted as is embodied in the program.

(iii) In the pre-PROGRESA 1997 data, there was not a systematic difference in enrollment rates between females and males for the 6-10 age range. For ages 11 and older enrollment rates were higher for males than for females. These data, thus, *prima facie* seem consistent with the rationale for higher transfers for females than for males because the former have lower enrollment rates for secondary school ages that are embodied in the PROGRESA transfer schedule. But part of this gender difference in age-specific enrollment rates does <u>not</u> reflect that females are receiving less schooling, but that males lag behind the standard grade progression rate more than do females because of lower grade progression rates. The average gaps in schooling attainment were in fact generally <u>larger</u> for males than for females. Thus there seems to be no basis in terms of schooling gaps or schooling attainment prior to PROGRESA for favoring females relative to males. Males apparently had higher enrollment rates in the age range 11-18 because they were on the average behind the females in age-specific schooling attainment, not ahead. So if it were desirable to have differential incentives by gender, it would seem that they should have favored males, not females.

2. Whether Enrollments and Schooling Gaps Were Different Between the Treatment and Control Samples Prior to PROGRESA: There was random assignment of localities between treatment and control groups. But that does not mean that households or individuals are randomly assigned between treatment and control groups. In fact, a previous project study finds that although on the locality level characteristics appear to be random between control and treatment samples, on the household and individual levels they differ systematically more often than would be expected were there random assignment by household or individuals. But prior to PROGRESA enrollment rates and schooling gap rates indeed were not different between the

control and treatment groups at the individual level.

3. The Impact of PROGRESA on Enrollments and Schooling Gaps: These estimates indicate significant enrollment rate increases for the treatment group beyond those for the control group between 1997 and 1999 for 12-14 year olds. For each of these ages for which the estimates are significant the estimates are larger for females than are those for males -- 11.9% versus 2.1% for 12 year olds, 8.8% versus 7.2% for 13 year olds, and 13.2% versus 5.8% for 14 year olds – and only the estimates for females, not for males, are significantly nonzero. Therefore PROGRESA seems to have been effective basically in increasing enrollments of females in the 12-14 age range. This success is consistent with the intent to focus on the ages related to the transition from primary to secondary school and the initial grades of secondary school for which previous dropout rates were high and with the intent to favor females. This latter success, however, may have exacerbated the gender gap in schooling attainment because, as noted above, even though age-specific enrollment rates prior to the program were lower for females than for males, age-specific schooling gaps were higher (and schooling attainment lower) for males than for females apparently because females had higher promotion rates than males. The lack of significant impact on enrollments of individuals younger than 12 is not surprising given the observation above about the high enrollment rates for such individuals in the pre-PROGRESA data – but it reinforces the point made above about whether the resources for transfers currently used for enrollment in primary 3 and 4 and perhaps 5 could be more effective if they were used for higher grades of school. With regard to the schooling gap, the overall estimates indicate significant reductions of this gap in the treatment relative to the control group of -0.15 grades for 11 year olds and of -0.31 grades for 15 year olds. Thus the latter provides some evidence of effects on teenagers, but for fewer ages than for enrollments. This is plausible both because the program is focused more on enrollments than attainment and because enrollments reflect shorter-run decisions and outcomes at the start of the year rather than for the whole year as for grade completion. The suggestion of an impact on 11 year olds is interesting because there is no evidence of significant impact on enrollments for that age or younger individuals. That means that any effect on the schooling gap for that age was due to a higher grade progression rate. The magnitudes of the estimates for females and males separately for these two ages, finally, indicate that the effects are the same by gender (though only those for 15 year olds are significantly nonzero at the 5% level). This contrasts with the enrollment results and also means that for the limited time period for which schooling gap estimates can be made, so far PROGRESA does not seem to have exacerbated gender gaps despite the results for a longer period for enrollment that suggests that it may have done so.

Section 5 presents data on achievement and estimates of the impact of the program on achievement test scores. To permit evaluation of the impact of PROGRESA on achievement test scores, PROGRESA arranged for the Secretary of Pubic Education (Secretaría de Educación Pública, SEP) to administer the same tests for students in schools attended by individuals in the PROGRESA Evaluation Sample as

SEP administers annually to a national sample of schools. These tests were administered to students in about 500 primary and secondary schools in the localities in the PROGRESA Evaluation Sample plus schools close to these localities.

These test data have some important limitations for the analysis of this paper: (1) Only the shorter-run effects -- basically for the first year -- can be investigated even though some of the more important impact of PROGRESA on cognitive achievement, at least based on other studies, may be longer run (e.g., improved school performance of current infants and small children when they become of school age due to better current health and nutrition). (2) The actual timing of the tests makes their interpretation somewhat murkier than intended because, instead of being given in the first quarter of each academic year to permit an assessment of cognitive achievement at the end of the previous year, they were given close to half way through the academic year. (3) Because PROGRESA was administered as a randomized experiment at the community level, impacts on outcomes such as school enrollment can be assessed simply by comparing mean outcomes for eligible children in treatment and control communities. However, the data that are available on student achievement were gathered from tests administered at the schools, so scores are only observed for children enrolled in school. Because the program induces children to enroll who otherwise would have delayed enrollment or dropped out, average test scores among test-takers will potentially be affected by selective enrollments. Similarly, the program may alter the age-composition of children at each grade. (4) The tests actually were given only to a subsample of those in the Evaluation Sample who were enrolled in school, and this subsample was not selected to be a random sample. A related point is that success in linking achievement test scores to children in the evaluation samples, particularly in the control sample, has been limited to fairly small proportions of the children in the Evaluation Sample household surveys, which limits severely in practice the possibility of controlling for possible selectivity in test taking. (5) There is no direct basis for knowing to what extent these particular achievement test scores relate to post-schooling productivities and income-generation capacities.

Statistical tests using these test data and examination of their distributions for treatment and control groups, first of all, indicate that there are not significant differences between treatment and control groups prior to PROGRESA. Even though the selection from the larger PROGRESA evaluation sample for the subsample for which test scores are available was not designed to be a random selection, systematic biases in test scores between the control and treatment groups were not introduced by this selection. The result of primary interest, however, is that after almost a school year and a half of exposure to PROGRESA, there is no significant positive impacts of PROGRESA on the achievement test scores. There are somewhat more cases in which the control group test scores exceed the treatment group scores than would be expected by chance, which to a very limited degree may be related to compositional changes But, in any case, there is not evidence of significantly positive effects even when compositional effects are taken into account in so far as they can be with the available data. Possibly this may reflect the limitations in the data noted above, particularly regarding the possibility of evaluating on the effect after a little more than a year of exposure to the program, but also possibly the relatively small sample sizes and the limited number of observations that could be merged with household survey data.

THE IMPACT OF PROGRESA ON ACHIEVEMENT TEST SCORES IN THE FIRST YEAR

Jere R. Behrman, Piyali Sengupta, and Petra Todd

1. INTRODUCTION

Increasing human capital investments in children is considered to be among the most effective ways of alleviating poverty and encouraging growth in developing countries. One possibly important channel through which such investments may have such impacts is through increasing cognitive achievement of children. There is a limited literature based on nonexperimental socioeconomic data that suggests that improved cognitive achievement has payoffs in terms of greater wages and perhaps productivities in labor markets in developing countries. These studies tend to find more substantial effects of cognitive skills than of schooling attainment if both are included, though of course schooling attainment may be affecting wages in part through cognitive achievement.[1]

This paper evaluates the short-run effects on children's cognitive achievements of PROGRESA, a relatively new, large-scale ani-poverty and human resource investment program in Mexico that provides aid to poor families with initial focus on those living in rural areas. One major component of PROGRESA is transfer payments to poor families with children enrolled in grades 3-6 of primary school and grades 1-3 of secondary school. Other components of PROGRESA include general transfers to such families and explicit nutrition supplements and health support for infants and small children. PROGRESA might have impact on children's cognitive achievement through a number of channels, some of which are relatively short run and others are relatively longer run. Previous IFRPI-PROGRESA Evaluation Project papers have considered some aspects of possible changes induced by PROGRESA that may have led or may eventually lead to improved cognitive achievement. Schultz

[1] Alderman, Behrman, Ross, and Sabot (1996) for rural Pakistan; Boissiere, Knight and Sabot (1985) for urban Kenya and Tanzania; Glewwe (1996) for Ghana; Lavy, Spratt and Leboucher (1997) for Morocco; Psacharopoulos and Velez (1992) for Colombia. There also is a large literature that finds associations between schooling attainment and wages or other measures of productivity in developing countries (e.g., Psacharopoulos 1994, Rosenzweig 1995, Strauss and Thomas 1995), but most of the data sets used in this literature do not include information on cognitive achievement. The literature on wage functions for developed countries likewise includes a few studies that report significant effects of cognitive achievement (e.g., Murnane, Willett and Levy 1995), but most of the data sets that have been used to investigate the impact of schooling on wages does not include such information.

(2000a,b), for example, has investigated the impact on school enrollments and school attendance for those enrolled. He finds significant impacts on school enrollment, particularly for the first year of junior secondary school, which imply increased schooling on the average of about 0.4 grades for poor children due to PROGRESA (about 0.5 for girls, 0.3 for boys). Behrman, Sengupta and Todd (2000) also find significant program effects on transitions across school grades. With regard to the longer-run effects, Behrman and Hoddinott (2000) further find that PROGRESA increased the annual growth of children aged 12-36 months by about 15%. But none of these studies consider the effects on child achievement test scores.

In this paper we investigate the short-run -- effectively the first-year -- impact of PROGRESA on school children's achievement test scores. In Section 2 we first describe the PROGRESA program, with emphasis on the features directly related to schooling, which focus on enrollment and attendance. In Section 3 we outline methods for estimating this impact. In Section 4 we present data on enrollments and summarize the patterns in enrollment because the program is directly conditional on enrollment. We also consider patterns in age-specific schooling attainment gaps. In Section 5 we present data on achievement and estimates of the impact of the program on achievement test scores.

2. PROGRESA PROGRAM IN GENERAL AND CONDITIONAL SCHOOLING ENROLLMENT TRANSFERS IN PARTICULAR

PROGRESA is a multifaceted program aimed at improving education, nutrition, and health outcomes of families, as well as alleviating immediate poverty, with initial focus on poorer households in rural Mexico. PROGRESA identified localities as being eligible to participate in the program because of a "high degree of marginality" that was determined primarily on the bases of analysis of data in the 1990 and 1995 population censuses (1990 Censo, 1995 Conteo), in addition to being small rural communities with access to social sector facilities necessary to benefit from some of the components of PROGRESA. PROGRESA used discriminant analysis of information from a 1997 census, modified by other information including community inputs, to determine which households were eligible for PROGRESA in these communities.[2]

PROGRESA has the following objectives:

- To substantially improve the conditions of education, health and nutrition of poor families,

[2] For extensive details regarding the selection process and an evaluation of the extent of success in targeting poorer households, see Skoufias, Davis, and Behrman (1999).

particularly children and their mothers, by providing sufficient quality services in the areas of education and health, as well as providing monetary assistance and nutrition supplements.

- Integrate these actions so that educational achievement is not affected by poor health or malnutrition in children and young people, or because they carry out work that makes school attendance difficult.

- Ensure that households have sufficient means and resources available so that their children can complete their basic education.

- Encourage the responsibility and active participation of parents and all family members in improving the education, health and nutrition of children and young people.

- Promote community participation and support for the actions of PROGRESA, so that educational and health services benefit all families in the localities where it operates, as well as uniting and promoting community efforts and initiatives in actions that are similar or complementary to the Program.

To pursue these objectives, PROGRESA has three components that are closely linked to each other:

- Educational grants to facilitate and encourage the educational aspirations of children and young people by fostering their enrollment and regular school attendance, and promoting parents' appreciation of the advantages of their children's education. At the same time, actions are to be carried out to improve the quality of education.

- Basic health care for all members of the family and strengthening the quality of services as well as reorienting individuals and health services towards taking preventive actions towards health care and nutrition.

- Monetary transfers and nutrition supplements to improve the food consumption and nutritional state of poor families, emphasizing that the purpose of this is to improve the family's food intake, particularly of children and women, who are generally the members of households who are perceived to suffer most from nutritional deficiencies.

PROGRESA has attempted to permit systematic evaluations of various components of the program by collecting longitudinal data on an evaluation sample that includes baseline (pre-PROGRESA initiation) and follow-up rounds of data for households in over 500 communities that were randomly assigned to either participate in the program or to serve as controls. Some aspects of these data are described in Section 4 below.

The first of the three PROGRESA program components is of central interest for this paper. The educational grants are "demand-side" transfer payments to families that are eligible for PROGRESA (deemed "poor") that are contingent upon their children attending school in certain grade levels (3rd-6th grade of primary school and 1st-3rd year of secondary school). The actions to improve the quality of education are "supply-side" incremental resources for schools. Children whose families do not receive transfers because they are ineligible for the program may nonetheless be affected by the influx of additional resources into their schools.

The educational grants are intended to alter the private incentives for families to invest in their children's education by offsetting the opportunity cost of not sending them to school. Table 1 summarizes the payment schedule. In recognition of the fact that older children are more likely to be more productive in work, payments begin at primary grade 3 at 70 pesos per month, increase by 14% for primary 4, and about 30% more for each of primary 5 and 6, so that the primary 6 payment is almost double (193%) of the payment for primary grade 3. Because the greatest dropout rate was perceived to have been between primary and secondary school, but the dropout rates have been relatively low between secondary grade 1 and 2 and 2 and 3, the payment for secondary 1 is about 50% above that for primary 6, but the percentage increases for secondary 2 relative to secondary 1 and for secondary 3 relative to secondary 2 are about 10% each. These payments are for enrollment with unexcused absences less than 15% of school days. They do not include payments directly for performance on the achievement tests that are the principle subject of this paper. But because they are conditional on the grade in which a student is enrolled, they do indirectly include a reward for sufficiently satisfactorily achievement performance to be promoted to the next grade in order to be eligible for the higher payment for the next grade during the next year. The payment schedule also recognizes a widely-held perception that schooling attainment levels historically have been lower for girls than for boys due to higher dropout rates for the former than the latter particularly after primary school.[3] The payment therefore is higher for females than for males by 5%, 12% and 13% for secondary grades 1, 2, and 3. To provide perspective on these magnitudes, Schultz (2000a) calculates that the PROGRESA transfer for a female enrolled in secondary grade 3 is equal to 44% of average adult male monthly wages in the communities in the PROGRESA Evaluation Sample.

We do not have information on the resources used to improve the quality of education from the supply side, nor on the distribution of these resources between control and treatment groups. If they were distributed only to the schools that treatment children attended and were well used, they would reinforce

[3] Though on the average in Mexico, as in most other Latin American countries, the gender gap in schooling attainment has been eliminated or even reversed in recent decades (Behrman, Duryea, and Szekely 1999). The average schooling attainment in Mexico for 18 year olds based on a household survey for 1996, for example, was 8.2 grades for males and 8.3 grades for females.

the demand-side incentive effects to increase school enrollments and improve school performance, including cognitive achievement. If they were equally distributed among the schools attended by control and treatment children, they would tend not to have any differential effects between the two groups. Because the achievement data indicate that some schools were attended by both treatment and control children, at least some control and treatment children had equal exposure to whatever supply-side actions were taken.

In addition to the first PROGRESA program component that is focused directly on schooling, the second and third components give direct monetary transfers and nutritional supplements (the latter with emphasis on infants and small children) and health care to eligible families, none of which are conditional on school attendance. These forms of support may improve the health of the children and thereby also indirectly influence their schooling outcomes. The supplements are not likely to have a direct effect on the schooling of children currently of school-age because they are directed towards infants and small children (though there may be an indirect effect if households respond to the supplements for infants and small children in part by redirecting resources to school-age children). In total, the cash transfers that families receive through PROGRESA are often substantial and constitute about a third of monthly family monetary income. Because the benefits are generous relative to income, most families eligible for the program choose to participate in it.

3. METHODS

The primary interest in this paper is in assessing whether PROGRESA affected child achievement test performance. We here summarize the methods that we use to make this assessment.

Let A denote the achievement test score of a child and let E_g be an indicator variable that equals 1 if the child is enrolled at school in grade g, where we assume that achievement test scores are only observed for children who are enrolled, as is the case given the way these data were collected (Section 5). Let P be an indicator that equals 1 if the child is eligible for the program and let T be an indicator that equals 1 if the child resides in a treatment locality. Only the families with children for whom P=1 and T=1 receive program benefits. Let X denote a set of variables that we use as conditioning variables. We assume that the distributions of these variables are not affected by the program (X may represent characteristics such as the age and sex of the child).

From the data on achievement scores, we can directly estimate the following means that correspond to the mean test scores for children enrolled in school, who reside in treatment or control villages and who are either eligible or not eligible for the PROGRESA program:

$$E(A|E_g=1,T=1,P=1)$$
$$E(A|E_g=1,T=0,P=1)$$
$$E(A|E_g=1,T=1,P=0)$$
$$E(A|E_g=1,T=0,P=0)$$

PROGRESA could have a direct effect on achievement, which may occur, for example, from improvements in the quality of schools or improvements in the health of children. The program may also have an effect on school enrollment due to the increased incentives to attend school in certain grades. The mean difference in achievement scores for eligible treatment and control children,

$$a_g = E(A|E_g=1,T=1,P=1)-E(A|E_g=1,T=0,P=1)$$

does not provide an unbiased estimate of the mean impact on achievement, because it does not control for changes in the composition of children enrolled in school arising from the program. This mean confounds the effect of the program on achievement with the compositional effect of who goes to school and therefore who takes the test. Indeed, it is possible for the mean difference in test scores to be negative even if the program is having a positive effect, as might occur if children whose achievement scores are below average are induced by the program to enroll. We want to separate out two distinct effects, the change in achievement scores arising from the direct effect on achievement, holding the composition of children fixed, and the change in achievement scores arising from the compositional effect on who enrolls in school. We now describe a way for separating out these effects.

Let X be a set of conditioning variables. From the data, we can estimate the difference in the probability of enrollment for treatment and control eligible children and for any X by:

$$Pr(E_g=1|X,T=1,P=1)-Pr(E_g=1|X,T=0,P=1).$$

The overall effect of the program on enrollment is given by

$$Pr(E_g=1|T=1,P=1)-Pr(E_g=1|T=0,P=1).$$

We can write the mean achievement test score for eligible participating children enrolled in grade g as:

$$E(A \mid E_g=1, T=1,P=1)=$$
$$\{\int_{X|E=1,T=1,P=1} E(A \mid E_g=1, T=1,P=1,X)\, f(X|E_g=1, T=1,P=1)dX \}/$$
$$\{\int_{X|E=1,T=1,P=1} f(X|E_g=1, T=1,P=1)dX\}$$

Similarly, the mean achievement test scores for eligible children in control communities can be written as

$$E(A \mid E_g=1, T=0, P=1)=$$
$$\left\{\int_{X|E=1,T=1,P=1} E(A \mid E_g=1, T=0,P=1,X)f(X|E_g=1, T=0,P=1)dX \right\}$$
$$/\left\{\int_{X|E=1,T=1,P=1} f(X|E_g=1, T=0,P=1)dX\right\}$$

If the program induces changes in the composition of children going to school, then we generally expect

$$f(X|E_g=1, T=1,P=1) \neq f(X|E_g=1, T=0,P=1) \text{ for some } X,$$

even though $f(X|T=1,P=1)=f(X|T=0,P=1)$ by virtue of randomization (and under the assumption that we choose conditioning variables X that are not affected by treatment).

Using data on randomized-out controls, we can construct an estimate of the average achievement test score for treated individuals, holding constant the composition of children (as measured by the distribution of X) at the pattern observed for controls. This average achievement test score, holding the distribution of X constant at that observed for controls, is given by

$$\Delta_{g,A} = \int_{X|E=1,T=1,P=1} E(A|E_g=1,T=1,P=1,X)\{f(X|E_g=1, T=0,P=1)/f(X|E_g=1,T=1,P=1)\}dX$$
$$-E(A|E_g=1,T=0,P=1)$$

The change in average achievement test scores due to changing composition of the sample at that grade is therefore given by

$$\Delta_{g,C} = \Delta_g - \Delta_{g,A},$$

where Δ_g was defined above as the mean difference in achievement scores among treatment and control eligibles. When the X are discrete, an estimator for $\Delta_{g,A}$ is given by

$$\sum_{i \in R_1} A_i \times \hat{Pr}(x_i=X|E_g=1,T=0,P=1,x=X)/\hat{Pr}(x_i=X|E_g=1,T=1,P=1,x=X)$$

$$-\frac{1}{n_0}\sum_{i \in R_0} A_i$$

where R_1 is the set of eligible children in the treatment group with characteristics X and R_0 is the set of eligible children in the control group with characteristics X. $Pr(x_i=X|E_g=1,T=0,P=1)$ is the fraction of eligible control children with characteristics X who are enrolled in school and $\Pr(x_i=X|E_g=1,T=1,P=1)$ the corresponding fraction of treatment children.

This decomposition depends on which variables enter into the conditioning set X, that is how we define a compositional change in the sample. In our empirical work, the variables included in X are the age and sex of the child. We are not able to include a broader set of family background variables in X because of the limited extent to which the test achievement data can be linked to the household surveys (Section 5). To implement the decomposition, we need to know (a) grade enrollment rates and average achievement test scores within cells of observations on eligible children defined by X and by treatment status and (b) the proportions in each X cell.

4. SCHOOLING ENROLLMENT AND SCHOOLING ATTAINMENT GAPS: DATA AND IMPACTS

The datasets gathered as part of the PROGRESA experiment provide rich information on variables related to the schooling, health, and consumption patterns of households. Currently, household survey data are available from two baseline surveys and four follow-up surveys that were administered approximately every six months. Data are available at the household and individual levels, but random assignment was performed at the community level, because of the broader geographic nature of some of PROGRESA benefits, such as improvements in local schools and health facilities.

The basic PROGRESA datasets that we use in this section consist of a baseline household survey (census) administered in October 1997 and follow-up surveys approximately one (October 1998) and two (November 1999) years later. The household surveys were conducted in 320 randomly selected treatment localities (in which treatment was initiated soon after the baseline survey) and 186 control localities (in which there had been no treatment prior to collection of these data) All 506 of these localities were selected in a stratified random selection procedure (with stratification by populations of localities) from the small rural localities identified by PROGRESA as being eligible to participate in the program, as noted above, because of a "high degree of marginality" that was determined primarily on the bases of analysis of data in the 1990 and 1995 population censuses (1990 Censo, 1995 Conteo) and access to social services necessary for some components of PROGRESA.

We here use these data to summarize age and gender patterns in enrollments and, to a lesser extent, in gaps in schooling attainment and possible differences by treatment status. The enrollment rates are of interest because the PROGRESA transfers for schooling are conditional on enrollment (and at least 85% attendance), with the schedule described in Table 1. In addition, as noted in Section 3, there may be important effects on the means for achievement test scores if PROGRESA induces compositional changes in the students who take these tests. The gaps in schooling attainment are defined as the difference between what schooling grade would have been completed if an individual entered school at age six and progressed one grade each year and what actually was attained. These gaps can be positive because individuals started school when they were older than six, repeated grades or dropped out of school.[4] These gaps are of interest because they indicate directly what are age-specific shortfalls in schooling attainment.

The first three sets of columns Tables 2.1 give the percentages enrolled by age for the 1997, 1998 and 1999 fall data rounds, respectively, for control and treatment children, as well as the differences between these enrollment rates for control and treatment children. The fourth set of columns gives the change from 1997 to 1999 for the control, treatment and the difference between them (the last is the "difference-in-difference" estimator). Tables 2.2 and 2.3 give parallel information on enrollments for females and for males, respectively. Tables 2.1-3 give similar information for gaps in schooling attainment. These tables provide some insight into three topics: the relation between pre-program enrollments and differential incentives by age and sex in the payment structure, whether enrollments and schooling gaps were different between the treatment and control samples prior to PROGRESA, and the impact of PROGRESA on enrollments and schooling gaps.

The Relation Between Pre-program Enrollments and Differential Incentives by Age and Sex in the Payment Structure: There are three points about how these data relate to the differential incentives by age and sex that are reflected in the transfer schedule in Table 1:

1. In the pre-PROGRESA 1997 data, enrollment rates averaged over 90% for the 7-11 age range and almost that high for six year olds (though there apparently was some late initial enrollment), and then fell by about 10 percent for every added year of age (about 80% for 12 year olds, about 70% for 13 year olds, etc.). This is consistent with the general pattern of incentives from transfers becoming positive for later grades of primary school and increasing with the grade level in Table 1. But it suggests that the incentives could have little effect for primary grades 3, 4 and possibly 5 because most children in these grades are in the 8-11 age range and, as noted, over 90% of this age range were enrolled prior to PROGRESA. This point is reinforced by the enrollment rates for the control sample for 1998 and 1999 – 95% or over for the age range 7-10 for 1998 (and over 90% for 6 and 11) and for the age range 6-11 for 1999. Thus, from looking at the pre-program data alone, it would appear that the same resources could have been used

[4] They can be negative if individuals start school when they are younger than six years old or if they skip grades. The data include a small proportion of such cases.

more effectively to increase enrollments by directing them more to higher grades and older children.

2. In the pre-PROGRESA 1997 data, enrollment rates fell substantially for ages right above those at which most children finished primary school – ages 12-14, which relates to the rationale for the large percentage increase in the transfers in Table 1 for secondary grade 1. But enrollments continued to fall fairly sharply for still older ages, at which ages many children who are still in school are in secondary grade 2 or higher. So it is not clear that such a sharp increase in the transfer between primary grade 6 and secondary grade 1 compared to the increases for advancing in secondary school beyond grade 1 is warranted as is in Table 1 – though if the objective is to match the increasing opportunity cost of time with age the desired transfer dependent on age clearly depends on how that opportunity cost of children's time increases with their ages.

3. In the pre-PROGRESA 1997 data, there was not a systematic difference in enrollment rates between females and males for the 6-10 age range. The enrollment rates were higher for boys than for girls in the 7-10 age range in the control sample and *vice versa* in the treatment sample. In both samples, however, for ages 11 and older enrollment rates were higher for males than for females. These data, thus, *prima facie* seem consistent with the rationale for higher transfers for females than for males because the former have lower enrollment rates for secondary school ages that are embodied in the transfer schedule in Table 1. But part of this gender difference in age-specific enrollment rates does <u>not</u> reflect that females are receiving less schooling, but that males lag behind the standard grade progression rate more than do females because of lower grade progression rates. Tables 3.2 and 3.3 provide some information on this topic in the form of the average gap in completed grades. For both the control and the treatment samples in 1997 for all but two ages in the range 7 through 18 the average gaps are <u>larger</u> for males than for females and the two exceptions in each case have fairly small differences between the gaps for males and females.[5] Thus there seems to be no basis in terms of schooling gaps or schooling attainment prior to PROGRESA for favoring females relative to males. Males apparently had higher enrollment rates in the age range 11-18 because they were on the average behind the females in age-specific schooling

[5] For the control group for ages 17 and 18 they are larger for females only by 0.01 and 0.13 grades, respectively, and for the treatment group they are larger for females for ages 7 and 16 by only 0.01 and 0.02 grades, respectively.

attainment, not ahead. So if it were desirable to have differential incentives by gender, it would seem that they should have favored males, not females.

Whether Enrollments and Schooling Gaps Were Different Between the Treatment and Control Samples Prior to PROGRESA: As noted above, there was random assignment of localities between treatment and control groups. But that does not mean that households or individuals are randomly assigned between treatment and control groups. In fact, Behrman and Todd (1999) find that although on the locality level characteristics appear to be random between control and treatment samples, on the household and individual levels they differ systematically more often than would be expected were there random assignment by household or individuals. The third column in the first set of estimates in Tables 2.1-3 and 3.1-3 gives the differences in the means between the treatment and control groups for 1997, and the standard deviations of those differences. For overall enrollment and for female and male enrollments, none of the differences are significantly nonzero at the 5% level (or even the 10% level). For the school gaps, the gaps are significantly greater at the 5% level for the control sample than for the treatment sample for eight year olds overall and females and for 13 year old males. None of the rest of the differences are significantly nonzero at the 5% level. Thus, there are significant differences at the 5% level in the means of enrollment rates and schooling gaps for 3.8% of the cases for 1997 in Tables 2.1-3 and 3.1-3, which is about what would be expected by chance if the outcomes considered are distributed randomly between individual treatment and control samples.

The Impact of PROGRESA on Enrollments and Schooling Gaps: Finally there is the question regarding what the data say about the effectiveness of PROGRESA in inducing higher enrollments and lower schooling gaps. Just looking at the changes in the enrollment rates between 1997 and 1998/1999 for treatment children (penultimate columns in Tables 2.1-3) is not informative about the enrollment question because there were secular upward trends in enrollments independent of PROGRESA, as is reflected in the increased enrollment rates for the control children in 1998 and 1999 (second and third sets of estimates in Tables 2.1-3). The difference-in-difference estimates in the last columns of Tables 2.1-3 control for such common secular trends by subtracting the change in the control enrollment rates over time from the change in the treatment enrollment rates over time. These estimates indicate significant enrollment rate increases for the treatment group beyond those for the control group between 1997 and 1999 of 6.9% for 12 year olds, 7.7% for 13 year olds, and 8.9% for 14 year olds (and positive, though not significant at the 5% level increases for all other ages in the 10-17 range). For each of these ages for which the estimates are significant the differences in the differences are larger for females than are those for males -- 11.9% versus 2.1% for 12 year olds, 8.8% versus 7.2% for 13 year olds, and 13.2% versus 5.8% for 14 year olds – and only the differences-in-the-differences for females, not for males, are significantly nonzero at the 5% level. Therefore PROGRESA seems to have been effective basically in increasing enrollments of females in the 12-14 age range.

This success is consistent with the intent to focus on the ages related to the transition from primary to secondary school and the initial grades of secondary school for which previous dropout rates were high, as reflected in the pattern of grade-specific transfers in Table 1. It is also consistent with the intent to favor females, as also is reflected in the slight advantages for females in the transfers for secondary school in Table 1. This latter success, however, may have exacerbated the gender gap in schooling attainment because, as noted above, even though age-specific enrollment rates prior to the program were lower for females than for males, age-specific schooling gaps were higher (and schooling attainment lower) for males than for females apparently because females had higher promotion rates than males. The lack of significant impact on enrollments of individuals younger than 12, finally, is not surprising given the observation above about the high enrollment rates for such individuals in the pre-PROGRESA data – but it reinforces the point made above about whether the resources for transfers currently used for enrollment in primary 3 and 4 and perhaps 5 could be more effective if they were used for higher grades of school.

With regard to the schooling gap, the overall estimates indicate significant reductions of this gap in the treatment relative to the control group of -0.15 grades for 11 year olds and of -0.31 grades for 15 year olds. Thus the latter provides some evidence of effects on teenagers, but for fewer ages than for enrollments. This is plausible both because the program is focused more on enrollments than attainment (though, as noted, that transfers increase with higher grades creates some additional incentives for successfully completing grades of school) and because enrollments reflect shorter-run decisions and outcomes at the start of the year rather than for the whole year as for grade completion. In fact, because the PROGRESA program only started to affect the 1998-9 school year, currently data are available on the impact on grade completion only for that year, in contrast to data being available for enrollments for both that year and the 1999-2000 school year. The suggestion of an impact on 11 year olds is interesting because there is no evidence of significant impact on enrollments for that age or younger individuals. That means that any effect on the schooling gap for that age was due to a higher grade progression rate. The magnitudes of the estimates for females and males separately for these two ages, finally, indicate that the effects are the same by gender (though only those for 15 year olds are significantly nonzero at the 5% level).[6] This contrasts with the enrollment results and also means that for the limited time period for which schooling gap estimates can be made, so far PROGRESA does not seem to have exacerbated gender gaps despite the results for a longer period for enrollment that suggests that it may have done so.

---

[6] For nine-year old males the estimates also indicate a significantly greater drop in the gap of 0.14 for the control than for the treatment sample, though no significant effects are indicated either for nine-year old females or nine-year olds overall. We have no explanation for this estimate.

5. ACHIEVEMENT TEST SCORES: DATA AND IMPACTS

PROGRESA might have impact on children's cognitive achievement through a number of channels, some of which are relatively short run and others are relatively longer run. If cognitive achievement can be considered to be produced by a number of school, individual and home inputs, among the leading relatively short-run possibilities for current school-age children include: (1) increasing enrollment, (2) increasing attendance conditional on enrollment, (3) increasing learning conditional on enrollment and attendance because students are healthier and have better short-term nutritional status, (4) increasing learning conditional on enrollment and attendance because students have more time and energy for homework and other out-of-school learning that complements what they learn in school, and (5) increasing learning conditional on enrollment and attendance because schools are improved.[7] Of course if there are students who enroll or attend who would not otherwise have attended, the added congestion and pressure on school resources may work in the other direction, to reduce, rather than to increase gains in cognitive achievement of students who would have been in school in any case. If the students who are induced to enroll and attend and take cognitive achievement tests are selected in a way in which they on average perform less well on cognitive achievement tests than do the students who would have taken the tests in the absence of the PROGRESA intervention, which seems likely, then this selectivity will tend to result, *ceteris paribus*, in a drop in average test scores. In Section 4 we have documented that PROGRESA did induce significantly increased school enrollments among 12-14 year old females, so there may be some of these compositional effects.

The longer-run effects, for example, may include the impact of improved health and nutrition of infants and small children on their longer-run nutritional status and learning capacities, with results such as starting school earlier, progressing through school more rapidly, and learning more while in school. A number of recent studies based on longitudinal data suggest that such effects of better nutrition for infants and small children might be considerable – Alderman, Behrman, Lavy and Menon (2001) for rural Pakistan; Galler (1984) and Galler, *et al.* (1983) for Barbados; Glewwe, Jacoby and King (2000) and Glewwe and King (2000) for the Philippines, Martorell (1997, 1999) for rural Guatemala; and Todd, Behrman and Cheng (2000) for urban Bolivia. These are other related studies are reviewed in a IFRPI-PROGRESA Evaluation Project paper (Behrman 2000).

[7] Todd and Wolpin (2000) discuss issues in the estimation of cognitive achievement production functions and related interpretation issues.

Achievement Test Score Data:[8] In order to permit evaluation of the impact of PROGRESA on achievement test scores, PROGRESA arranged for the Secretary of Pubic Education (Dirección General de Evaluación del Proceso Educativo de la Secretaría de Educación Pública, SEP) to administer the same tests for students in schools attended by individuals in the PROGRESA Evaluation Sample as SEP administers annually to a national sample of schools (which, however, does not include schools attended by children in the PROGRESA Evaluation Sample). The advantages of using the same tests include that these tests have been validated and that they permit comparisons with the other schools in the SEP sample. These tests were administered to students in about 500 primary and secondary schools in the localities in the PROGRESA Evaluation Sample plus schools close to these localities with at least five students receiving PROGRESA educational transfers (the latter group is primarily at the secondary level because only 18% of the localities in the Evaluation Sample had secondary schools).

Two tests were given: (1) ***Evaluation of Primary Education*** (Evaluación de la Educación Primaria, "EVEP") and (2) ***National Standards*** (Estándares Nacionales, "Estándares"). Both tests include components on Spanish and on Mathematics. A decision was made to give both tests in the first quarter of the school year (which starts in August) so that what they evaluate is the level of achievement at the end of the previous school year though in fact the tests were given in December in 1997 and in 1998 and in November in 1999. Both tests are designed to evaluate achievement conditional on grade of schooling completed. The EVEP was given to evaluate selected primary grades in the range 3 to 6 in 1997, 1998 and 1999. The Estándares was given to evaluate selected primary grades in the same range and secondary grades 1-3 in 1998 and 1999. Both tests are scheduled to be given in 2000, but these data are not yet available. Table 4 is reproduced from PROGRESA (2000a) to show in what grades the tests were given and what grades the tests are evaluating, with arrows to indicate comparisons possible with tests across years. Both tests were calibrated by SEP using the Rasch model so that the scores for a given grade level range from 20 to 80 with an equal probability of obtaining above and below 50 for the reference sample used for the calibration and with greater weights placed on the questions that discriminate better among members of the reference sample.

We note several limitations of these test data for our analysis:

1. We can only consider the shorter-run effects, among those noted at the start of this section, because the PROGRESA evaluation panel data are not for long enough duration to permit following infants and small children to school age even though the literature referred to in the discussion of longer-run effects at the start of this section suggests the possibility that there may be important effects on cognitive achievement

[8] For further details concerning these tests, including the calibration procedures, see PROGRESA (2000a,b).

through this mechanism. In fact, though the available PROGRESA evaluation data currently cover about 3.5 years (October/November 1997 - May/June 2000), given the intended timing of the school cognitive achievement tests that are available for analysis (i.e., in the first trimester of the academic year so they basically assess what was known at the end of the previous year adjusted for learning/forgetting between the end of the previous school year and the start of the current year), there is only one true post-PROGRESA exposure achievement test currently available. So, even though it is possible to consider, for example, enrollment decisions for two years subsequent to the initiation of PROGRESA, effectively the achievement tests can be considered only for one year subsequent to the initiation of PROGRESA – so they, like the schooling attainment gaps measures used in Section 4, are indeed short run.

2. The actual timing of the tests makes their interpretation somewhat murkier than intended. Instead of being given in the first quarter of each academic year, they were given close to half way through the academic year (December in 1997 and 1998, November in 1999). Therefore for 1998 they include about a half year of school after PROGRESA began in the Evaluation Sample.

3. Because PROGRESA was administered as a randomized experiment at the community level, impacts on outcomes such as school enrollment can be assessed simply by comparing mean outcomes for eligible children in treatment and control communities. However, the data that are available on student achievement were gathered from tests administered at the schools, so scores are only observed for children enrolled in school. If the program induces children to enroll who otherwise would have delayed enrollment or dropped out as previously studies and the analysis in Section 4 suggest is the case (e.g., Schultz 2000a, Behrman, Sengupta, and Todd 2000), then average test scores among test-takers will potentially be affected by selective enrollments. Similarly, the program may alter the age-composition of children at each grade. Thus, even though participation is randomly assigned at the community level, selectivity into the pool of test-takers and into alternative grade-levels needs to be taken into account in evaluating grade-specific impacts on student achievement. Section 3 considers how to take such selectivity into account in the evaluation; our "weighted" estimates in Tables 6 and 7 below use these methods.

4. The tests actually were given only to a subsample of those in the Evaluation Sample who were enrolled in school, and this subsample was not selected to be a random sample. Table 5 summarizes the numbers in the longitudinal sample for EVEP (that permits estimates down the diagonal of Table 4), the cross sections for Estándares, and the cross sections for the Evaluation Sample household surveys. For these EVEP data the number of treatment observations is 38% and the number of control observations in

primary grade 4 in 1997 is 13% of those in the 1997 household survey. For the Estándares data the number of treatment children across all the includes grades is 59% in 1998 and 41% in 1999 of those in the Evaluation Sample household surveys, and only 17% and 11% respectively for the control children. A related point is that success in linking achievement test scores to children in the evaluation samples, particularly in the control sample, has been limited to significantly smaller numbers than in Tables 5.1 for the EVEP data (see the numbers in parentheses in the table).

5. We have no direct basis for knowing to what extent these achievement test scores relate to post-schooling productivities and income-generation capacities. There is some evidence, as noted in the introduction, that in other developing country contexts similar achievement tests are associated with wages and productivities, and often associated with more of the variances in wages than are school attainments. But we are not aware of any estimates using these tests for Mexico, and the PROGRESA data do not contain information with which to estimate such relations.

Achievement Test Results: Tables 6 and 7 summarize the EVEP and Estándares data. For each data set information is presented by schooling level and grade for all children in the PROGRESA Evaluation Sample for whom information is available, and then separately. The EVEP data includes the observations in the panel – along the diagonal in Table 4.[9] The information presented includes (i) the means for the treatment and control groups and the difference and percentage difference in these means and standard deviations of these differences, and (ii) the differences in the means and the percentage differences if the treatment group is weighted to match the control group on observable characteristics (i.e., age, sex).[10] A number of figures also are presented that give the whole distributions the respect test scores (by year, school grade, and for all children together as well as for females and males separately, for both tests) for treatment versus control eligible children. These figures may reveal differences in the distributions that are not revealed in the mean comparisons in the tables. These tables and figures suggest two basic points:

1. ***Differences between treatment and control groups prior to PROGRESA***: The 1997 data are for such a period, but unfortunately cover only two grades. They indicate

[9] We also have done similar estimates for the cross-sections for primary grade 6. The results are very similar to those that are summarized below.

[10] Because the weights are estimated the standard deviations can not be calculated in the latter case. But the weighted mean differences in fact are fairly close to the unweighted mean differences. Therefore the discussion in the text on the significance of the differences in the means applies approximately to the weighted comparisons as well as to the unweighted comparisons.

no significant differences. The 1998 data were supposed to represent achievement at the end of the 1997-8 school year, before PROGRESA started for the Evaluation Sample, though they were late enough in the academic year that there was about half a school year exposure to PROGRESA. The 1998 data cover more grades and both tests. They also do not indicate any significant differences in the means. Examination of the figures reinforces the conclusion from the mean comparisons in the tables that there are no significant differences between the control and the treatment subsamples prior to PROGRESA. This is good news. It means that, although the selection from the larger PROGRESA evaluation sample for the subsample for which test scores are available was not designed to be a random selection, systematic biases in test scores between the control and treatment groups were not introduced by this selection.

2. ***Impact of PROGRESA on achievement scores***: The 1999 data record achievement after almost a school year and a half of exposure to PROGRESA. They indicate no significant (at the 5% level) positive means impacts of PROGRESA on the achievement test scores. The figures also do not reveal any important positive impacts on other parts of the distribution. Tables 6 and 7 indicate, however, seven cases in 1999 for which the estimates for the control group are significantly greater than the estimates for the treatment group: for the EVEP test -- mathematics for males for primary grade 6; for the Estándares test – mathematics and Spanish for all students and for females alone for primary grade 4, Spanish for all students and for females alone for secondary grade 3. This includes 19.4% of the cases, which is higher than would be expected by chance were these independent estimates.[11] As noted above, compositional changes might result in declines in the average test scores if, for example, PROGRESA induces students to enroll who otherwise would not have enrolled and who perform less well on the tests than the students who would have enrolled whether or not PROGRESA was in effect. That all of these significant cases for the Estándares test are for females (though not the one case for the EVEP test) and only for females are significant enrollment effects found in Section 4 *prima facie* seems consistent with the possibility that such compositional changes might underlie these estimated lower means for the treatment than for the control group. But the age patterns seem to undercut such an interpretation because only one of these significant effects on females' test scores is for a grade higher than primary 4, but the significant enrollment effects for females in Section 4 are for females aged 12-14, an age range that is above the age for most females in

[11] They obviously are not independent because the category for all students includes both females and males. If only the separate estimates for females and males are considered, four of the 24 (16.7%) are significantly negative at the 5% level.

primary grade 4. The comparisons between the unweighted and the weighted estimates also do not lend much support to such an interpretation because in only half of the cases is the weighted estimate less in absolute magnitude than the unweighted estimates (and in these cases only a little less), as would be expected were the composition effect important. Thus, there are somewhat more cases in which the control scores average higher than the treatment scores than would be expected by chance or than appear to be explained by compositional effects. But, in any case, there is not evidence of significantly positive effects even when compositional effects are taken into account in so far as they can be with the available data. Possibly this may reflect the limitations in the data noted above, particularly regarding the possibility of evaluating on the effect after a little more than a year of exposure to the program, but also possibly the relatively small sample sizes and the limited number of observations that could be merged with household survey data.

REFERENCES

Alderman, H., J. R. Behrman, D. Ross, and R. Sabot. 1996. The returns to endogenous human capital in Pakistan's rural wage labor market. *Oxford Bulletin of Economics and Statistics* 58:1, no. February : 29-55.

Alderman, H., J. R. Behrman, V. Lavy, and R. Menon. Forthcoming (2001). Child nutrition, child health, and school enrollment: A longitudinal analysis. *Journal of Human Resources*.

Bacolod, M., and E. M. King. 2000. The effects of family background and school quality on low and high achievers: Determinants of academic achievement in the Philippines. Washington, DC: World Bank. Mimeo.

Behrman, J. R. 1998. General conceptual framework for micro analysis of PROGRESA impact. Washington, DC: International Food Policy Research Institute.

Behrman, J. R. 2000. Literature review on interactions between health, education, and nutrition and the potential benefits of intervening simultaneously in all three. Philadelphia, PA: University of Pennsylvania. Paper prepared for IFPRI-PROGRESA Evaluation Project. Mimeo.

Behrman, J. R., S. Duryea, and M. Székely. 1999. Schooling investments and aggregate conditions: A household-survey-based approach for Latin America and the Caribbean. Washington, DC: Inter-American Development Bank. Background Paper for IPES 2000 for IDB. Mimeo.

Behrman, J. R., and J. Hoddinott. 2000. Preliminary evaluation of the impact of PROGRESA on child growth. Washington, DC: International Food Policy Research Institute. Report submitted to PROGRESA. Mimeo.

Behrman, J. R., P. Sengupta, and P. Todd. 2000. The impact of a schooling transfer program on educational outcomes: Evidence from Mexico’s PROGRESA experiment. Washington, DC: International Food Policy Research Institute. Mimeo.

Behrman, J. R., and P. E. Todd. 1999. Randomness in the experimental samples of PROGRESA (education, health, and nutrition program). Report submitted to PROGRESA. Washington, DC: International Food Policy Research Institute.

Boissiere, M., J. B. Knight, and R. H. Sabot. 1985. Earnings, schooling, ability and cognitive skills. *American Economic Review* 75: 1016-1030.

Galler, J.R. 1984. Behavioral consequences of malnutrition in early life. In *Nutrition and Behavior,* ed., J.R Galler. New York: Plenum Press.

Galler, J.R., F. Ramsey, G. Solimano, and W.E. Lowell. 1983. The influence of early malnutrition on subsequent behavioral development and classroom behavior. *Journal of the American Academy of Child Psychiatry* 22:16-22.

Glewwe, P. 1996. The relevance of standard estimates of rates of return to schooling for education policy: A critical assessment. *Journal of Development Economics* 51: 2 (December): 267-90.

Glewwe, P., and H. Jacoby. 1995. An economic analysis of delayed primary school enrollment and childhood malnutrition in a low income country. *Review of Economics and Statistics* 77:1, (February): 156-69.

Glewwe, P., H. Jacoby, and E. King. Forthcoming (2000). Early childhood nutrition and academic achievement: A longitudinal analysis. *Journal of Public Economics*.

Glewwe, P., and E. King. Forthcoming (2000). The impact of early childhood nutrition status on cognitive achievement: Does the timing of malnutrition matter? *World Bank Economic Review*.

Lavy, V., J. Spratt, and N. Leboucher. 1997. Patterns of incidence and change in Moroccan literacy. *Comparative Education Review* 41:2 (May).

Martorell, R. 1997. Undernutrition during pregnancy and early childhood and its consequences for cognitive and behavioral development. In *Early child development: Investing in our children's future*, ed., M.E. Young. Amsterdam: Elsevier.

Martorell, R. 1999. The nature of child malnutrition and its long-term implications. *Food and Nutrition Bulletin* 20: 288-292.

Murnane, R. J., J. B. Willet, and F. Levy. 1995. The growing importance of cognitive skills in wage determination. *Review of Economics and Statistics* 77: 2 (May), 251-266.

Pollitt, E., K. S. Gorman, P. L. Engle, R. Martorell, and J. Rivera. 1993. Early supplementary feeding and cognition. *Monographs of the Society for Research in Child Development Serial No. 235* 58:7.

PROGRESA. 2000a. Nota sobre las pruebas de logro educativo aplicadas por PROGESA-SEP Ciclos 1997-1998; 1998-1999; 1999-2000. Mexico City: PROGRESA. Mimeo.

PROGRESA. 2000b. Nota técnica de las pruebas aplicadas en las evaluaciones del aprovechamiento escolar. 1997, 1998 Y 1999, escuelas primarias y secundarias, SEP - PROGRESA. Mexico City: PROGRESA. Mimeo.

Psacharopoulos, G. 1994. Returns to investment in education: A global update. *World Development* 22:9, no. September: 1325-44.

Psacharopoulos, G., and E. Velez. 1992. Schooling, ability and earnings in Colombia, 1988. *Economic Development and Cultural Change* 40: 629-643.

Rosenzweig, Mark R., 1995. Why are there returns in schooling? *American Economic Review* 85:2 (May), 153-158.

Schultz, T. P. 2000a. The impact of PROGRESA on school enrollments. Washington, DC: International Food Policy Research Institute. Report submitted to PROGRESA.

Schultz, T. P. 2000b. The impact of PROGRESA on school attendance rates in the sampled population. Washington, DC: International Food Policy Research Institute. Report submitted to PROGRESA.

Skoufias, E., B. Davis, and J. R. Behrman. 1999. A preliminary evaluation of the selection of beneficiary households in the education, health, and nutrition program (PROGRESA) of Mexico. Report submitted to PROGRESA. Washington, DC: IFPRI/PROGRESA Research Report.

Strauss, J., and D. Thomas. 1995. Human resources: Empirical modeling of household and family decisions. In *Handbook of development economics,* eds. J. R. Behrman, and T.N. Srinivasan, 1883-2024. Vol. 3A. Amsterdam: North-Holland Publishing Company.

Todd, P.E., J. R. Behrman, and Y. Cheng. 2000. Evaluating preschool programs when length of exposure to the program varies: A nonparametric approach. Philadelphia, PA: University of Pennsylvania.

Todd, P. E., and K. I. Wolpin. 2000. On the specification and estimation of the production function for cognitive achievement. Philadelphia, PA: University of Pennsylvania.

Table 1. Monthly Transfers for School Attendance by PROGRESA[a]

School Level	Grade	Monthly Payment in Pesos		Payment/Payment for Previous Grade (%)		Females/Males (%)
		Females	Males	Females	Males	
Primary	3	70	70	--	--	100
	4	80	80	114	114	100
	5	105	105	131	131	100
	6	135	135	129	129	100
Secondary	1	210	200	156	148	105
	2	235	210	112	105	112
	3	255	225	109	107	113

a Source: Schultz (1999a, Table 1). Corresponds to first term of the 1998-1999 school year. To receive these transfers children must be from eligible families and must attend at least 85 percent of school days (excluding those days for which medical or parental excuses were obtained in the last two months).

Table 2.1
Fraction enrolled, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
5	75	76	0.30	.	.	.	.	.	.	.	.	.
			(2.02)			.			.			.
6	88	90	1.46	90	91	1.17	96	96	0.48	7.43	6.45	-.99
			(1.33)			(1.30)			(0.96)			(1.64)
7	93	93	-.14	95	96	0.33	97	98	1.00	3.40	4.54	1.14
			(1.03)			(0.88)			(0.73)			(1.27)
8	94	95	1.11	96	96	0.61	98	98	0.80	3.82	3.52	-.30
			(0.96)			(0.82)			(0.58)			(1.12)
9	95	95	0.38	95	97	2.41	98	98	0.05	3.31	2.98	-.34
			(0.93)			(0.88)			(0.55)			(1.08)
10	93	94	1.02	95	95	0.91	96	98	1.99	3.44	4.41	0.97
			(1.06)			(0.95)			(0.73)			(1.29)
11	91	92	0.58	92	95	2.77	95	97	1.95	3.40	4.76	1.36
			(1.19)			(1.11)			(0.94)			(1.52)
12	82	81	-.95	80	88	8.02	86	92	5.93	3.48	10.4	6.88
			(1.80)			(1.71)			(1.46)			(2.32)
13	67	70	2.76	73	78	5.44	74	84	10.5	6.24	14.0	7.74
			(2.47)			(2.20)			(2.08)			(3.23)
14	54	54	0.55	53	67	14.7	63	73	9.47	9.44	18.4	8.92
			(3.03)			(2.88)			(2.58)			(3.97)
15	36	39	2.22	39	45	5.42	44	53	9.31	7.76	14.9	7.10
			(3.70)			(3.55)			(3.49)			(5.08)
16	28	31	3.06	26	31	5.16	32	40	7.12	4.71	8.78	4.06
			(4.45)			(4.12)			(4.13)			(6.07)
17	16	19	2.82	.	.	.	23	27	3.75	6.75	7.69	0.94
			(5.03)			.			(4.74)			(6.91)
18	11	13	1.73	.	.	.	16	17	0.67	4.66	3.61	-1.1
			(5.61)			.			(5.60)			(7.93)

Table 2.2
Fraction females enrolled, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
5	77	74	-3.4	.	.	.	.	.	.	.	.	.
			(2.85)			.			.			.
6	88	89	0.44	89	91	1.44	96	97	0.30	7.97	7.82	-.15
			(1.90)			(1.87)			(1.28)			(2.29)
7	93	94	0.72	95	95	-.22	96	98	1.35	3.23	3.85	0.62
			(1.48)			(1.27)			(1.12)			(1.85)
8	94	95	0.81	95	96	0.88	98	99	1.00	3.59	3.78	0.19
			(1.35)			(1.28)			(0.82)			(1.58)
9	94	96	2.08	95	97	1.54	99	98	-.77	4.90	2.06	-2.8
			(1.34)			(1.23)			(0.67)			(1.50)
10	92	94	2.59	94	96	1.58	97	97	0.24	5.59	3.24	-2.3
			(1.57)			(1.39)			(0.97)			(1.85)
11	91	91	0.15	93	94	1.59	95	96	1.25	3.45	4.56	1.11
			(1.74)			(1.59)			(1.42)			(2.24)
12	80	76	-4.5	77	87	9.93	83	90	7.34	2.62	14.5	11.9
			(2.83)			(2.59)			(2.24)			(3.61)
13	64	66	2.31	66	73	6.51	71	82	11.1	7.52	16.3	8.75
			(3.72)			(3.48)			(3.06)			(4.81)
14	49	48	-.84	50	63	13.0	60	72	12.3	11.0	24.2	13.2
			(4.65)			(4.32)			(3.88)			(6.05)
15	32	34	1.41	32	38	5.48	42	53	11.0	9.64	19.2	9.54
			(5.48)			(5.41)			(5.14)			(7.52)
16	20	26	6.12	24	30	6.60	28	37	9.37	7.92	11.2	3.25
			(6.65)			(6.05)			(6.41)			(9.23)
17	14	17	2.97	.	.	.	24	26	2.71	9.74	9.48	-.26
			(7.00)			.			(7.03)			(9.92)
18	8	12	3.82	.	.	.	13	17	3.76	4.99	4.93	-.06
			(7.91)			.			(8.37)			(11.52)

Table 2.3
Fraction males enrolled, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
5	73	77	3.68	.	.	.	.	.	.	.	.	.
			(2.85)			.			.			.
6	88	91	2.43	90	91	0.91	95	96	0.68	6.90	5.15	-1.7
			(1.88)			(1.82)			(1.43)			(2.36)
7	94	93	-.98	95	96	0.83	97	98	0.66	3.60	5.23	1.63
			(1.45)			(1.22)			(0.96)			(1.74)
8	94	95	1.20	96	97	0.44	98	98	0.61	3.87	3.28	-.59
			(1.34)			(1.04)			(0.83)			(1.58)
9	96	95	-1.3	94	98	3.24	98	99	0.90	1.63	3.86	2.23
			(1.28)			(1.25)			(0.88)			(1.55)
10	94	93	-.46	95	95	0.26	95	99	3.68	1.35	5.49	4.14
			(1.42)			(1.30)			(1.10)			(1.80)
11	92	93	1.02	92	95	3.86	95	98	2.61	3.33	4.92	1.59
			(1.64)			(1.56)			(1.24)			(2.05)
12	84	86	2.32	83	89	6.11	88	93	4.39	4.55	6.61	2.07
			(2.28)			(2.24)			(1.88)			(2.96)
13	71	74	2.76	79	83	4.16	76	86	9.92	4.82	12.0	7.15
			(3.27)			(2.72)			(2.81)			(4.31)
14	59	60	1.05	55	72	16.3	66	73	6.94	7.65	13.5	5.88
			(3.95)			(3.85)			(3.43)			(5.23)
15	40	43	2.56	47	51	4.07	46	54	7.76	5.85	11.0	5.20
			(4.99)			(4.66)			(4.76)			(6.89)
16	36	36	-.21	29	32	3.62	36	42	5.02	0.73	5.97	5.23
			(5.94)			(5.62)			(5.38)			(8.02)
17	19	22	2.27	.	.	.	23	27	4.61	3.37	5.71	2.34
			(7.23)			.			(6.43)			(9.68)
18	15	14	-.57	.	.	.	18	16	-2.0	3.70	2.29	-1.4
			(7.96)			.			(7.54)			(10.96)

Table 3.1
Average Gap in Completed Grades, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
6	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			(0.00)			(0.00)			(0.00)			(0.00)
7	0.20	0.18	-.02	0.25	0.21	-.04	0.27	0.24	-.03	0.07	0.06	-.02
			(0.02)			(0.02)			(0.02)			(0.02)
8	0.44	0.40	-.05	0.43	0.39	-.04	0.46	0.42	-.04	0.02	0.03	0.01
			(0.02)			(0.02)			(0.02)			(0.03)
9	0.66	0.62	-.03	0.61	0.54	-.07	0.59	0.59	0.00	-.07	-.03	0.04
			(0.03)			(0.03)			(0.03)			(0.05)
10	0.84	0.79	-.05	0.89	0.69	-.20	0.89	0.80	-.09	0.06	0.02	-.04
			(0.04)			(0.04)			(0.04)			(0.06)
11	0.99	1.00	0.01	0.93	0.89	-.05	1.03	0.89	-.14	0.04	-.11	-.15
			(0.05)			(0.05)			(0.05)			(0.07)
12	1.29	1.27	-.03	1.09	1.07	-.02	1.11	1.10	-.01	-.18	-.17	0.02
			(0.06)			(0.05)			(0.06)			(0.08)
13	1.70	1.57	-.13	1.46	1.38	-.07	1.46	1.36	-.11	-.23	-.21	0.02
			(0.07)			(0.07)			(0.07)			(0.09)
14	2.13	2.01	-.12	1.83	1.69	-.14	1.90	1.69	-.20	-.23	-.31	-.08
			(0.08)			(0.07)			(0.08)			(0.11)
15	2.62	2.62	0.00	2.31	2.27	-.04	2.54	2.23	-.31	-.08	-.39	-.31
			(0.09)			(0.09)			(0.10)			(0.13)
16	3.30	3.29	-.01	2.99	2.82	-.17	3.04	3.08	0.05	-.26	-.20	0.06
			(0.11)			(0.11)			(0.11)			(0.16)
17	4.32	4.10	-.22	.	.	.	4.07	3.78	-.29	-.26	-.33	-.07
			(0.14)			.			(0.13)			(0.19)
18	5.16	5.05	-.11	.	.	.	4.83	4.48	-.35	-.33	-.56	-.24
			(0.17)			.			(0.18)			(0.25)

Table 3.2
Average Gap in Completed Grades for females, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
6	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			(0.00)			(0.00)			(0.00)			(0.00)
7	0.18	0.19	0.00	0.22	0.21	-.01	0.27	0.25	-.02	0.09	0.06	-.02
			(0.02)			(0.02)			(0.03)			(0.04)
8	0.42	0.36	-.06	0.40	0.35	-.05	0.41	0.40	-.01	-.00	0.04	0.05
			(0.03)			(0.03)			(0.04)			(0.05)
9	0.58	0.59	0.01	0.58	0.52	-.06	0.60	0.54	-.06	0.02	-.04	-.06
			(0.04)			(0.05)			(0.05)			(0.07)
10	0.79	0.73	-.06	0.81	0.66	-.15	0.81	0.75	-.06	0.02	0.02	0.00
			(0.06)			(0.06)			(0.06)			(0.08)
11	0.94	0.93	-.01	0.82	0.83	0.01	0.94	0.78	-.16	-.00	-.15	-.15
			(0.06)			(0.07)			(0.07)			(0.10)
12	1.13	1.17	0.04	1.05	0.97	-.08	0.96	1.07	0.11	-.17	-.09	0.08
			(0.08)			(0.07)			(0.07)			(0.11)
13	1.61	1.54	-.08	1.40	1.37	-.04	1.37	1.32	-.06	-.24	-.22	0.02
			(0.09)			(0.09)			(0.09)			(0.13)
14	2.09	1.94	-.14	1.75	1.74	-.01	1.83	1.73	-.10	-.26	-.21	0.05
			(0.11)			(0.10)			(0.11)			(0.16)
15	2.47	2.60	0.13	2.35	2.27	-.09	2.33	2.17	-.16	-.14	-.43	-.28
			(0.12)			(0.13)			(0.13)			(0.18)
16	3.22	3.30	0.08	2.80	2.75	-.05	2.99	3.11	0.12	-.23	-.19	0.04
			(0.16)			(0.15)			(0.16)			(0.22)
17	4.33	4.09	-.24	.	.	.	4.19	3.63	-.56	-.14	-.46	-.32
			(0.19)			.			(0.19)			(0.27)
18	5.22	5.02	-.21	.	.	.	4.90	4.47	-.42	-.32	-.54	-.22
			(0.24)			.			(0.26)			(0.36)

Table 3.3
Average Gap in Completed Grades for males, by age and treatment status

	Oct. 97			Oct. 98			Nov. 99			Change from 97 to 99		
age	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**	**con**	**trt**	**diff**
6	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			(0.00)			(0.00)			(0.00)			(0.00)
7	0.21	0.18	-.03	0.29	0.22	-.07	0.28	0.23	-.04	0.06	0.05	-.01
			(0.02)			(0.03)			(0.03)			(0.03)
8	0.47	0.43	-.04	0.47	0.42	-.04	0.50	0.44	-.06	0.03	0.01	-.02
			(0.03)			(0.03)			(0.04)			(0.05)
9	0.74	0.66	-.08	0.65	0.56	-.08	0.57	0.63	0.06	-.17	-.03	0.14
			(0.05)			(0.05)			(0.05)			(0.07)
10	0.88	0.84	-.04	0.97	0.72	-.25	0.97	0.85	-.12	0.10	0.02	-.08
			(0.05)			(0.06)			(0.06)			(0.08)
11	1.05	1.06	0.02	1.05	0.94	-.11	1.12	1.00	-.13	0.08	-.06	-.14
			(0.07)			(0.07)			(0.07)			(0.10)
12	1.44	1.36	-.08	1.13	1.17	0.04	1.26	1.13	-.13	-.18	-.23	-.05
			(0.08)			(0.08)			(0.08)			(0.12)
13	1.78	1.60	-.18	1.51	1.40	-.11	1.56	1.40	-.16	-.22	-.20	0.02
			(0.09)			(0.09)			(0.10)			(0.14)
14	2.16	2.06	-.10	1.91	1.64	-.27	1.96	1.66	-.30	-.21	-.40	-.19
			(0.11)			(0.10)			(0.11)			(0.15)
15	2.75	2.63	-.12	2.26	2.26	-.00	2.73	2.28	-.46	-.02	-.36	-.34
			(0.12)			(0.12)			(0.14)			(0.19)
16	3.38	3.28	-.10	3.18	2.88	-.30	3.07	3.06	-.01	-.31	-.22	0.09
			(0.17)			(0.15)			(0.15)			(0.23)
17	4.32	4.13	-.19	.	.	.	3.96	3.89	-.07	-.36	-.23	0.12
			(0.20)			.			(0.18)			(0.27)
18	5.09	5.08	-.00	.	.	.	4.77	4.49	-.28	-.31	-.59	-.28
			(0.25)			.			(0.25)			(0.36)

Table 4. Years and Grades of PROGRESA-SEP Achievement Tests[a]

Year	Primary			Secondary		
School grade when test taken	4°	5°	6°	1°	2°	3°
School grade being evaluated	3°	4°	5°	6°	1°	2°
1997	**EVEP** ↘		**EVEP** ↕			
1998	*Estándares* ↕	*Estándares* ↘ **EVEP**	*Estándares* ↘ ***EVEP*** ↕ ↘	*Estándares* ↘	*Estándares* ↘	*Estándares*
1999	*Estándares*		*Estándares* **EVEP** ↕	*Estándares* ↕ **EVEP**	*Estándares* ↕	*Estándares* ↕

[a] Source: PROGRESA (2000a). Tests are defined and explanations given in the text of Section 5 of this paper.

Table 5.1
Number of Observations in Achievement Test Samples
Test: Evep Test, Boys and Girls Combined, eligible population only (pobre=1)
(number of observations for which folio variable is available shown in parentheses)

Grade	1997		1998		1999	
	treatment	control	treatment	control	treatment	controls
4	813	170	59	0	…	…
	(808)	(1)	(59)	(0)		
5	…	…	754	170	…	…
			(749)	(1)		
6	…	…	…	…	644	170
					(639)	(1)
missing grade	0	0	0	0	169	0
					(169)	(0)
total	813	170	813	170	813	170
	(808)	(1)	(808)	(1)	(808)	(1)

Table 5.2
Number of Observations in Achievement Test Samples
Test: Estandares Test, Boys and Girls Combined, eligible population only (pobre=1)
(number of observations for which folio variable is available shown in parentheses)

Grade	1998		1999	
	treatment	control	treatment	controls
4 (primary)	1376	241	1188	216
	(1376)	(241)	(1188)	(216)
5 (primary)	1284	272	…	…
	(1284)	(272)		
6 (primary)	1158	264	1110	237
	(1158)	(264)	(1110)	(237)
1 (second.)	749	54	402	61
	(749)	(54)	(402)	(61)
2 (second.)	466	42	709	61
	(466)	(42)	(709)	(61)
3 (second.)	359	29	461	53
	(359)	(29)	(461)	(53)
total	5392	902	3870	628

Table 5.3
Number of Observations in Progresa Samples
Test: Boys and Girls Combined, eligible population only (pobre=1)

Grade	1997		1998		1999	
	treatment	control	treatment	control	treatment	control
4 (primary)	2117	1271	2026	1158	1864	1195
5 (primary)	1833	1143	1698	1007	1646	968
6 (primary)	1681	1040	1576	931	1500	907
1 (second.)	3061	1779	2509	1494	2666	1801
2 (second.)	642	392	753	434	1072	466
3 (second.)	567	329	633	351	804	409
total	9901	5954	9195	5375	9552	5746

--   --

Table 6.1
Mean Impacts on Evep Achievement Tests †
Sample: Boys and Girls Combined (all treatments and controls)

	Fall 1997 (4th grade)				Fall 1998 (5th grade)				Fall 1999 (6th grade)			
test	**trt***	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**
Spanish	47.4	46.9	0.57	0.62	51.4	51.7	-.34	0.20	50.3	50.2	0.07	0.28
			(0.68,1.22%)	(1.33%)			(0.82,-.66%)	(0.38%)			(0.59,0.13%)	(0.55%)
Math	42.9	43.0	-.11	0.10	45.2	45.2	0.03	0.79	48.2	48.8	-.60	-.56
			(0.71,-.25%)	(0.23%)			(0.84,0.07%)	(1.75%)			(0.57,-1.2%)	(-1.2%)

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

--   --

Table 6.2
Mean Impacts on Evep Achievement Tests †
Sample: Girls (all pobre=1 treatments and controls)

	Fall 1997 (4th grade)				Fall 1998 (5th grade)				Fall 1999 (6th grade)			
test	**trt***	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**
Spanish	47.7	47.0	0.74	1.20	52.5	52.8	-.36	0.14	50.7	50.8	-.11	0.16
			(0.95,1.58%)	(2.56%)			(1.08,-.68%)	(0.27%)			(0.76,-.21%)	(0.31%)
Math	43.2	42.9	0.28	0.65	45.5	44.9	0.59	1.54	48.1	47.9	0.18	0.25
			(1.00,0.65%)	(1.51%)			(1.25,1.32%)	(3.44%)			(0.82,0.37%)	(0.52%)

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

Table 6.3
Mean Impacts on Evep Achievement Tests †
Sample: Boys (all pobre=1 treatments and controls)

	Fall 1997 (4th grade)				Fall 1998 (5th grade)				Fall 1999 (6th grade)			
test	**trt***	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**	**trt**	**con**	**mean diff**	**est. impact**
Spanish	47.2	46.7	0.45	-.10	50.4	50.4	-.01	0.30	50.0	49.6	0.39	0.45
			(1.00,0.96%)	(-.20%)			(1.25,-.01%)	(0.60%)			(0.92,0.78%)	(0.90%)
Math	42.5	43.0	-.46	-.57	44.9	45.4	-.56	-.10	48.3	49.9	-1.6	-1.5
			(1.02,-1.1%)	(-1.3%)			(1.10,-1.2%)	(-.23%)			(0.75,-3.2%)	(-3.1%)

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

Table 7.1a
Mean Impacts on Estandares Achievement Tests †
Sample: Boys and Girls Combined, Test: Math

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	46.3	46.9	-.55	-1.2%	-.86	-1.8%
					(0.63)			
1998	1	5	47.0	48.0	-1.0	-2.1%	-1.0	-2.2%
					(0.52)			
1998	1	6	47.1	46.7	0.38	0.81%	0.59	1.26%
					(0.52)			
1998	2	1	46.9	47.0	-.16	-.33%	0.53	1.13%
					(0.80)			
1998	2	2	46.2	45.6	0.60	1.32%	0.80	1.75%
					(1.02)			
1998	2	3	46.6	45.1	1.54	3.43%	1.55	3.44%
					(1.59)			
1999	1	4	39.5	40.8	-1.3	-3.1%	-1.7	-4.1%
					(0.67)			
1999	1	6	43.1	42.9	0.17	0.40%	0.30	0.70%
					(0.46)			
1999	2	1	44.6	45.6	-.94	-2.1%	-1.2	-2.5%
					(0.81)			
1999	2	2	47.9	48.0	-.05	-.11%	0.33	0.69%
					(0.87)			
1999	2	3	49.8	50.9	-1.1	-2.2%	-.63	-1.2%
					(1.00)			

Table 7.1b
Mean Impacts on Estandares Achievement Tests †
Sample: Boys and Girls Combined, Test: Spanish

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	45.0	45.4	-.39	-.85%	-.79	-1.7%
					(0.69)			
1998	1	5	45.5	47.2	-1.7	-3.7%	-1.8	-3.9%
					(0.70)			
1998	1	6	46.3	45.3	0.98	2.16%	1.05	2.31%
					(0.69)			
1998	2	1	46.0	46.0	0.08	0.17%	0.99	2.16%
					(1.26)			
1998	2	2	45.2	44.1	1.10	2.49%	1.30	2.95%
					(1.52)			
1998	2	3	44.1	44.1	0.04	0.08%	0.01	0.02%
					(2.05)			
1999	1	4	41.0	42.3	-1.2	-2.9%	-1.5	-3.5%
					(0.53)			
1999	1	6	41.9	41.8	0.13	0.32%	0.23	0.54%
					(0.55)			
1999	2	1	42.3	42.3	0.02	0.05%	0.34	0.80%
					(1.23)			
1999	2	2	47.2	48.6	-1.4	-2.9%	-1.0	-2.1%
					(0.93)			
1999	2	3	48.9	52.0	-3.1	-6.0%	-2.6	-5.0%
					(1.33)			

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

*Level=1 refers to Primary School and Level=2 refers to Secondary school.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

Table 7.2a
Mean Impacts on Estandares Achievement Tests †
Sample: Girls, Test: Math

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	46.6	46.4	0.22 (0.91)	0.47%	-.07	-.15%
1998	1	5	47.1	48.3	-1.2 (0.79)	-2.4%	-1.3	-2.6%
1998	1	6	46.7	46.0	0.72 (0.81)	1.56%	0.88	1.91%
1998	2	1	47.1	47.2	-.07 (1.10)	-.14%	0.83	1.75%
1998	2	2	46.1	45.5	0.59 (1.16)	1.29%	0.77	1.69%
1998	2	3	47.5	45.7	1.76 (2.38)	3.85%	1.65	3.61%
1999	1	4	38.6	40.9	-2.2 (1.01)	-5.4%	-2.6	-6.4%
1999	1	6	43.2	42.8	0.32 (0.66)	0.74%	0.48	1.11%
1999	2	1	44.4	45.6	-1.2 (0.98)	-2.7%	-1.5	-3.4%
1999	2	2	47.6	47.8	-.23 (1.19)	-.47%	-.06	-.12%
1999	2	3	48.7	49.9	-1.1 (1.40)	-2.3%	-.99	-2.0%

Table 7.2b
Mean Impacts on Estandares Achievement Tests †
Sample: Girls, Test: Spanish

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	45.0	45.3	-.28 (0.93)	-.62%	-.82	-1.8%
1998	1	5	45.7	47.3	-1.6 (0.98)	-3.4%	-1.8	-3.7%
1998	1	6	46.1	45.2	0.96 (1.03)	2.12%	0.78	1.73%
1998	2	1	46.7	44.8	1.89 (1.94)	4.21%	3.27	7.29%
1998	2	2	45.4	40.1	5.25 (2.47)	13.1%	5.92	14.8%
1998	2	3	45.5	45.3	0.17 (2.93)	0.38%	-.19	-.42%
1999	1	4	40.5	42.9	-2.4 (0.76)	-5.6%	-2.5	-5.9%
1999	1	6	42.5	42.2	0.32 (0.82)	0.76%	0.37	0.88%
1999	2	1	42.8	43.6	-.73 (1.65)	-1.7%	-.16	-.37%
1999	2	2	48.2	49.4	-1.1 (1.29)	-2.3%	-.81	-1.6%
1999	2	3	49.3	56.0	-6.7 (2.40)	-12%	-6.6	-12%

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

*Level=1 refers to Primary School and Level=2 refers to Secondary School.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

Table 7.3a
Mean Impacts on Estandares Achievement Tests †
Sample: Boys, Test: Math

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	46.0	47.3	-1.3	-2.8%	-1.8	-3.7%
					(0.88)			
1998	1	5	46.9	47.8	-.83	-1.7%	-.78	-1.6%
					(0.68)			
1998	1	6	47.4	47.4	0.05	0.10%	0.31	0.66%
					(0.65)			
1998	2	1	46.6	46.9	-.23	-.50%	0.16	0.35%
					(1.18)			
1998	2	2	46.3	45.7	0.62	1.36%	0.78	1.72%
					(1.53)			
1998	2	3	46.0	43.8	2.21	5.05%	1.74	3.97%
					(1.20)			
1999	1	4	40.3	40.7	-.45	-1.1%	-.89	-2.2%
					(0.89)			
1999	1	6	43.1	43.1	0.01	0.02%	0.08	0.19%
					(0.65)			
1999	2	1	44.9	45.6	-.67	-1.5%	-.70	-1.5%
					(1.34)			
1999	2	2	48.3	48.2	0.19	0.39%	0.80	1.67%
					(1.28)			
1999	2	3	50.5	51.5	-.99	-1.9%	-.52	-1.0%
					(1.38)			

Table 7.3b
Mean Impacts on Estandares Achievement Tests †
Sample: Boys, Test: Spanish

Year	Level*	Grade	trt	con	mean diff	percent	est.	percent
1998	1	4	45.1	45.6	-.49	-1.1%	-.71	-1.6%
					(1.01)			
1998	1	5	45.3	47.1	-1.8	-3.8%	-1.9	-4.1%
					(1.02)			
1998	1	6	46.4	45.4	0.99	2.19%	1.29	2.83%
					(0.93)			
1998	2	1	45.4	47.2	-1.8	-3.9%	-1.6	-3.4%
					(1.59)			
1998	2	2	45.1	46.8	-1.7	-3.7%	-1.7	-3.6%
					(1.77)			
1998	2	3	43.2	41.8	1.46	3.50%	0.91	2.18%
					(2.19)			
1999	1	4	41.5	41.8	-.33	-.78%	-.67	-1.6%
					(0.73)			
1999	1	6	41.3	41.3	0.02	0.05%	-.04	-.10%
					(0.73)			
1999	2	1	41.8	40.9	0.90	2.21%	0.82	2.01%
					(1.84)			
1999	2	2	46.0	47.9	-1.9	-3.9%	-1.4	-3.0%
					(1.34)			
1999	2	3	48.6	49.4	-.77	-1.6%	-.02	-.03%
					(1.39)			

† The estimated impact column controls for selectivity into test-taking based on the age and sex of the child. It reweights the treatment data to align the treatment distribution of age and sex to that of the controls.

*Level=1 refers to Primary School and Level=2 refers to Secondary School.

The treatment sample includes all treatment children. Some may be attending schools in control localities. The control sample includes all control children, some of whom may be attending schools in treatment localities

Figure 1.1: Plot of Empirical Cdf's of Unadjusted Test Scores
Comparing Poor in Treatment and Control Localities

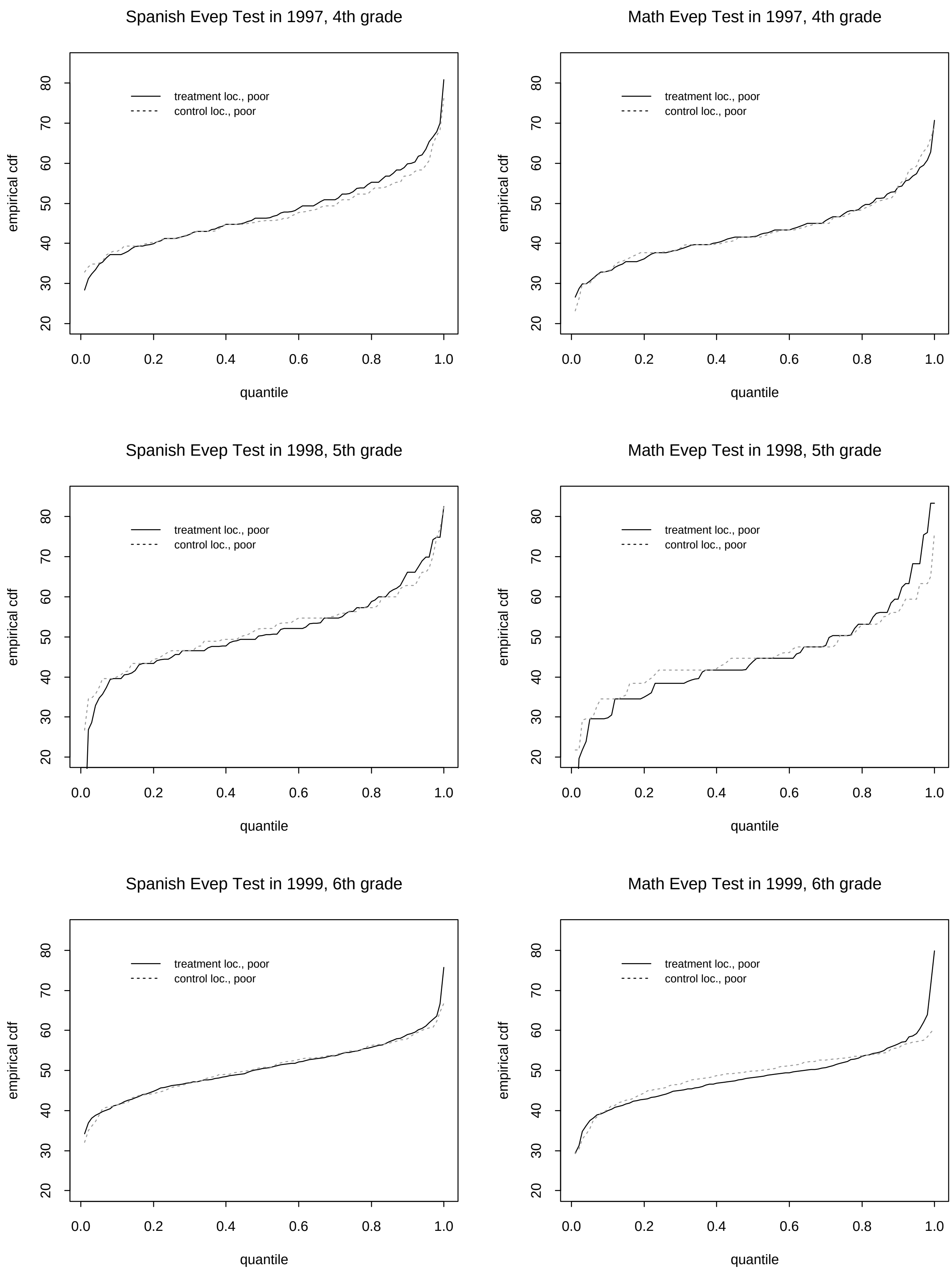


Figure 1.2: Plot of Empirical Cdf's of Unadjusted Test Scores
Comparing Poor in Treatment and Control Localities, Girls Only

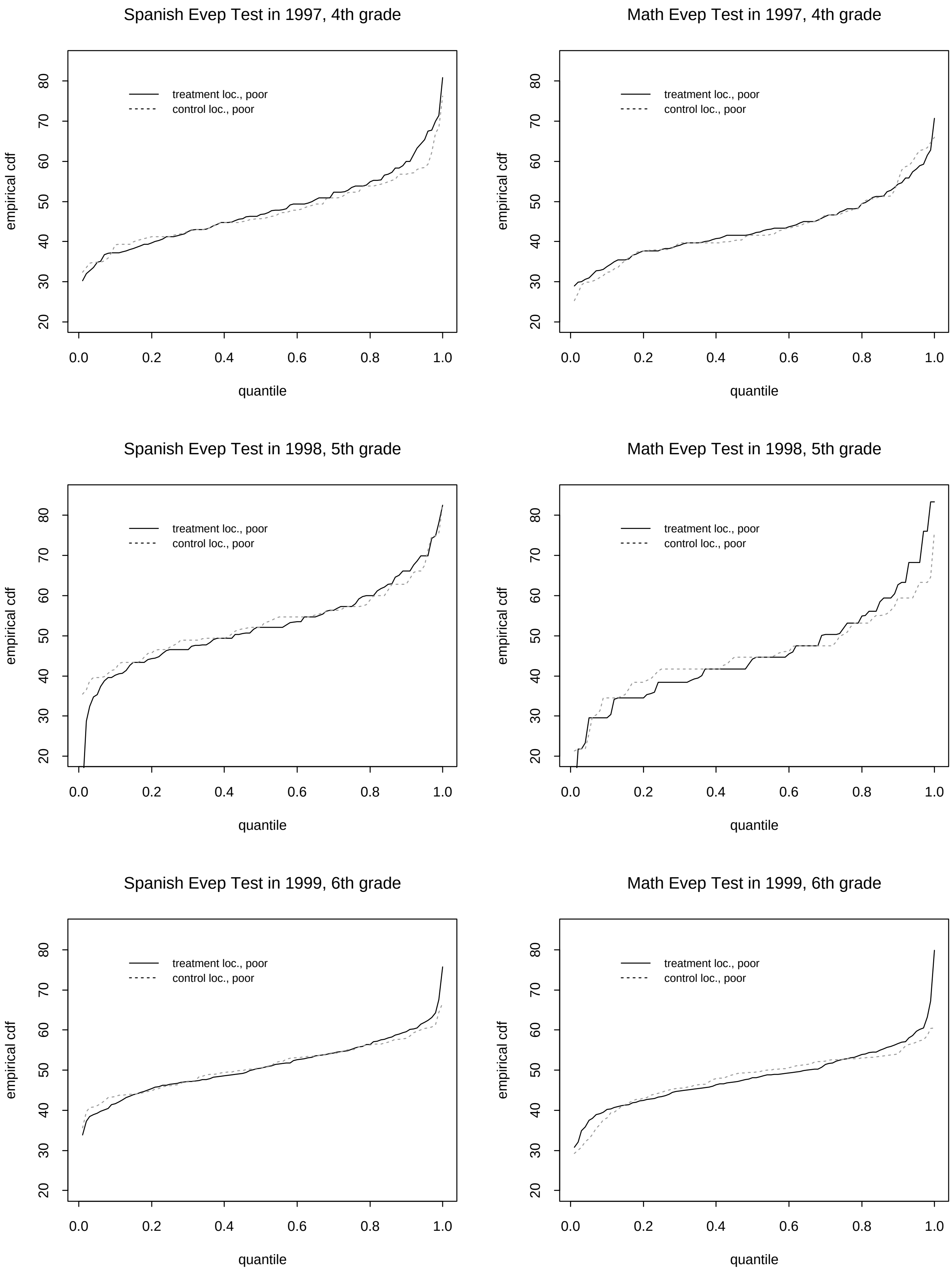


Figure 1.3: Plot of Empirical Cdf's of Unadjusted Test Scores
Comparing Poor in Treatment and Control Localities, Boys Only

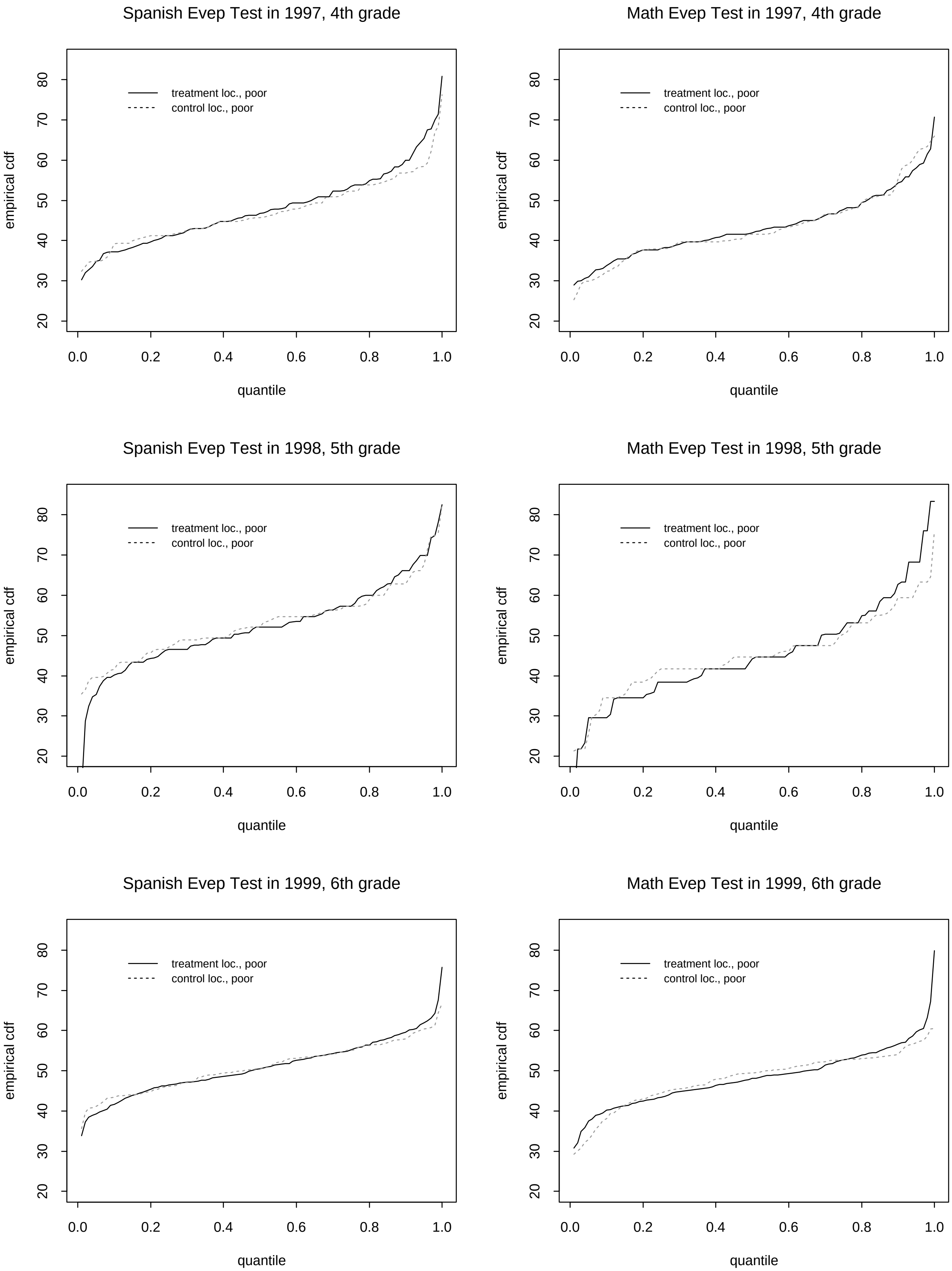


Figure 2.1: Empirical cdf of Estandares Test Scores, Boys and Girls Combined

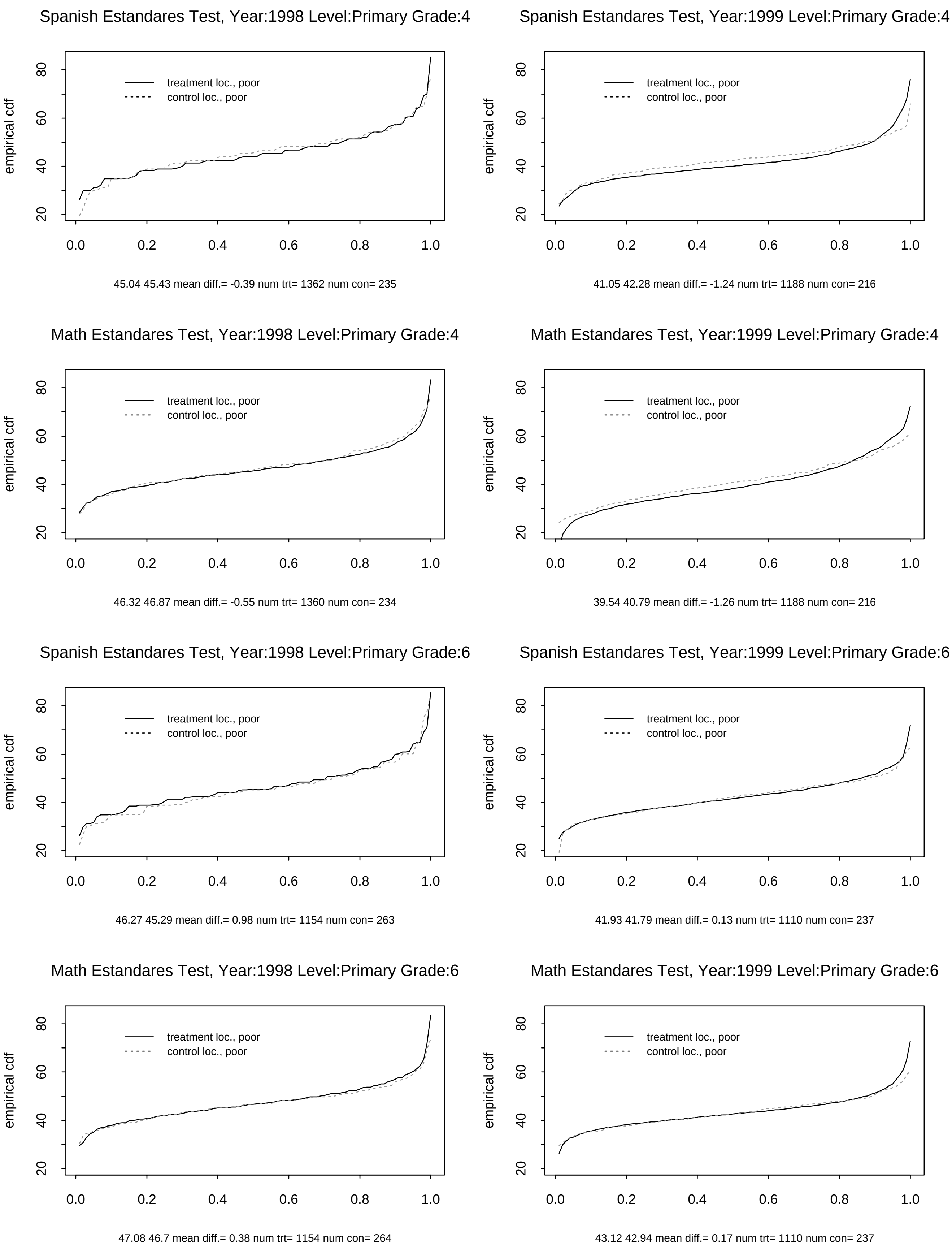


Figure 2.1: Empirical cdf of Estandares Test Scores, Boys and Girls Combined

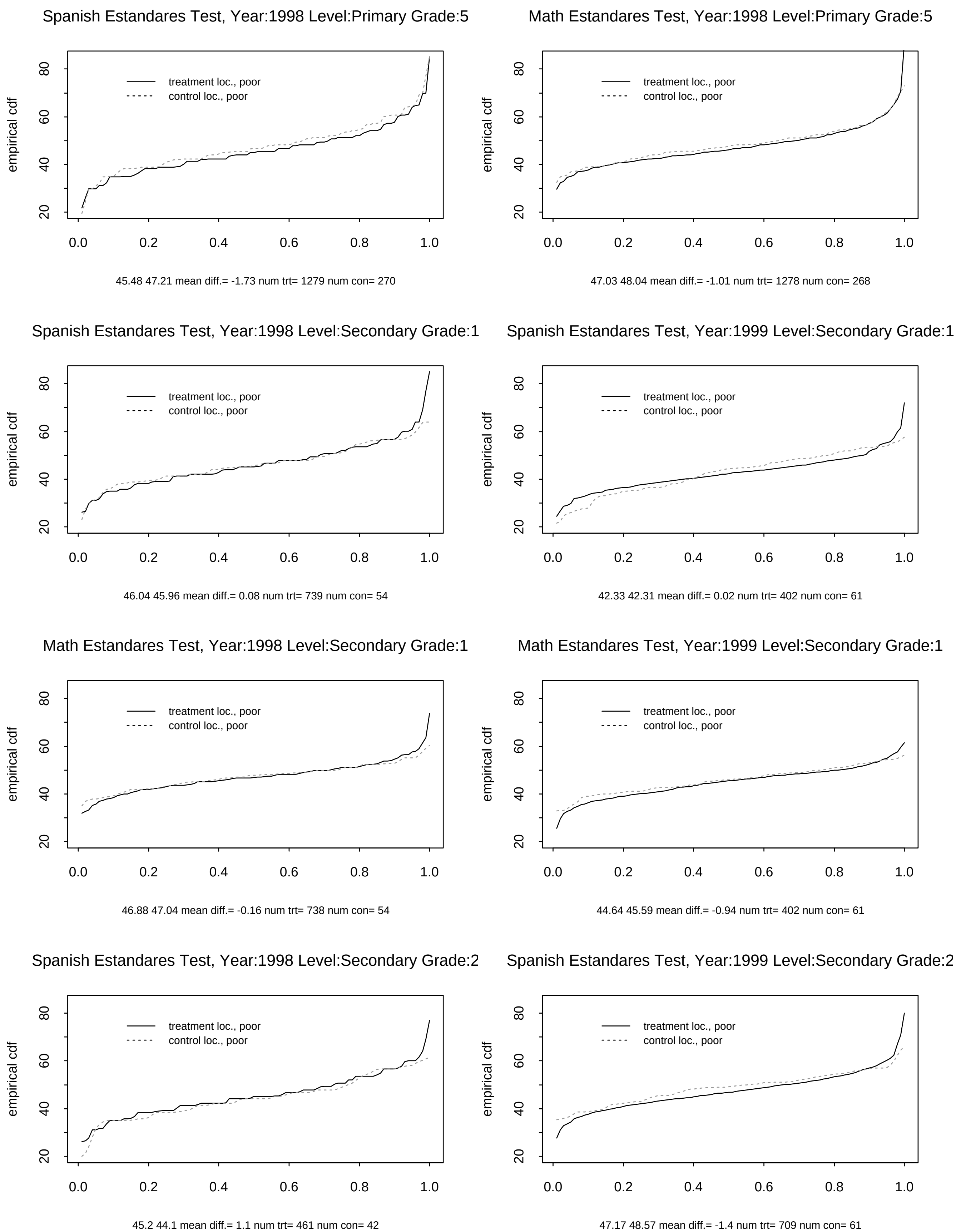


Figure 2.1: Empirical cdf of Estandares Test Scores, Boys and Girls Combined

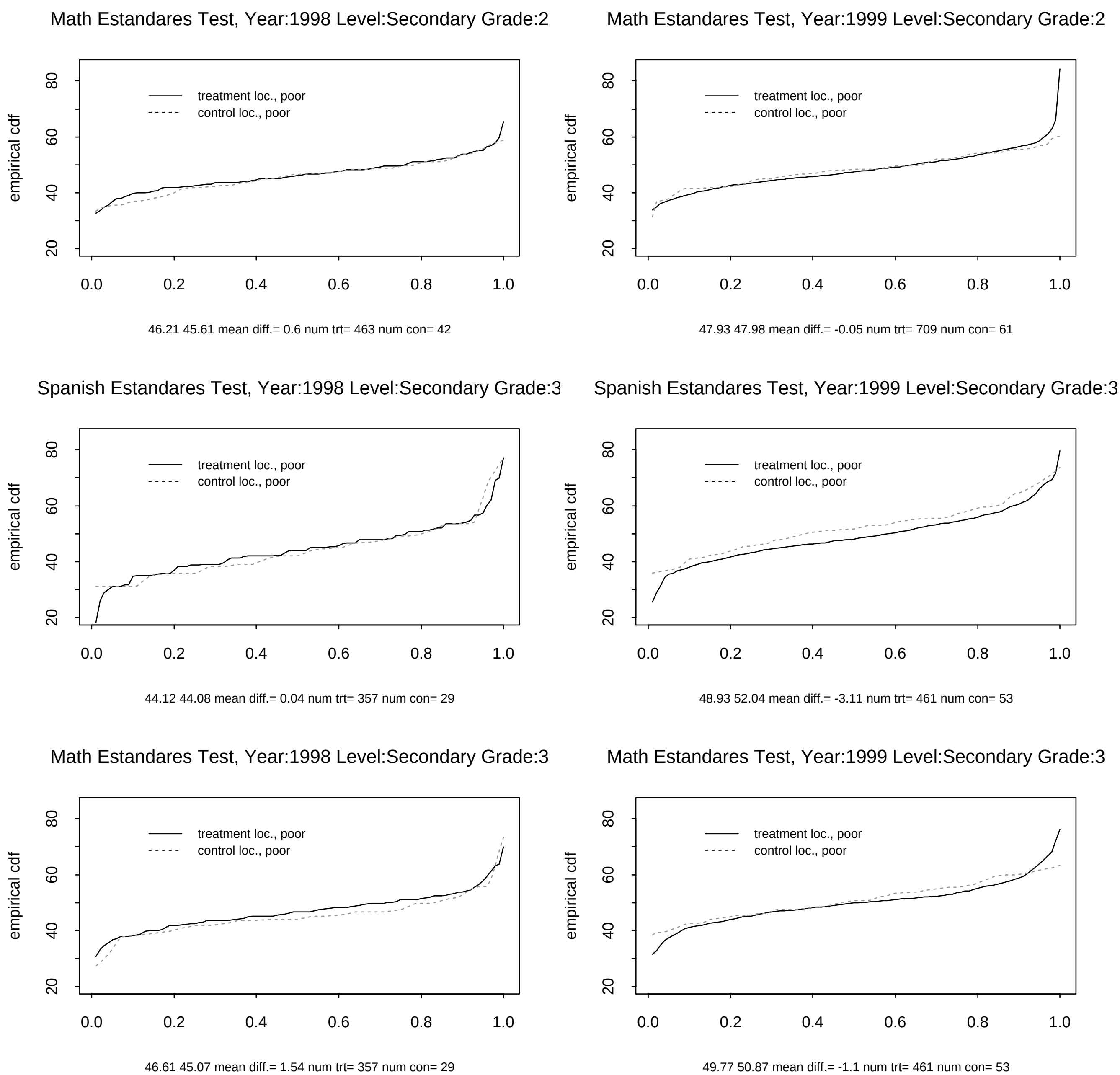


Figure 2.2: Empirical cdf of Estandares Test Scores, Girls

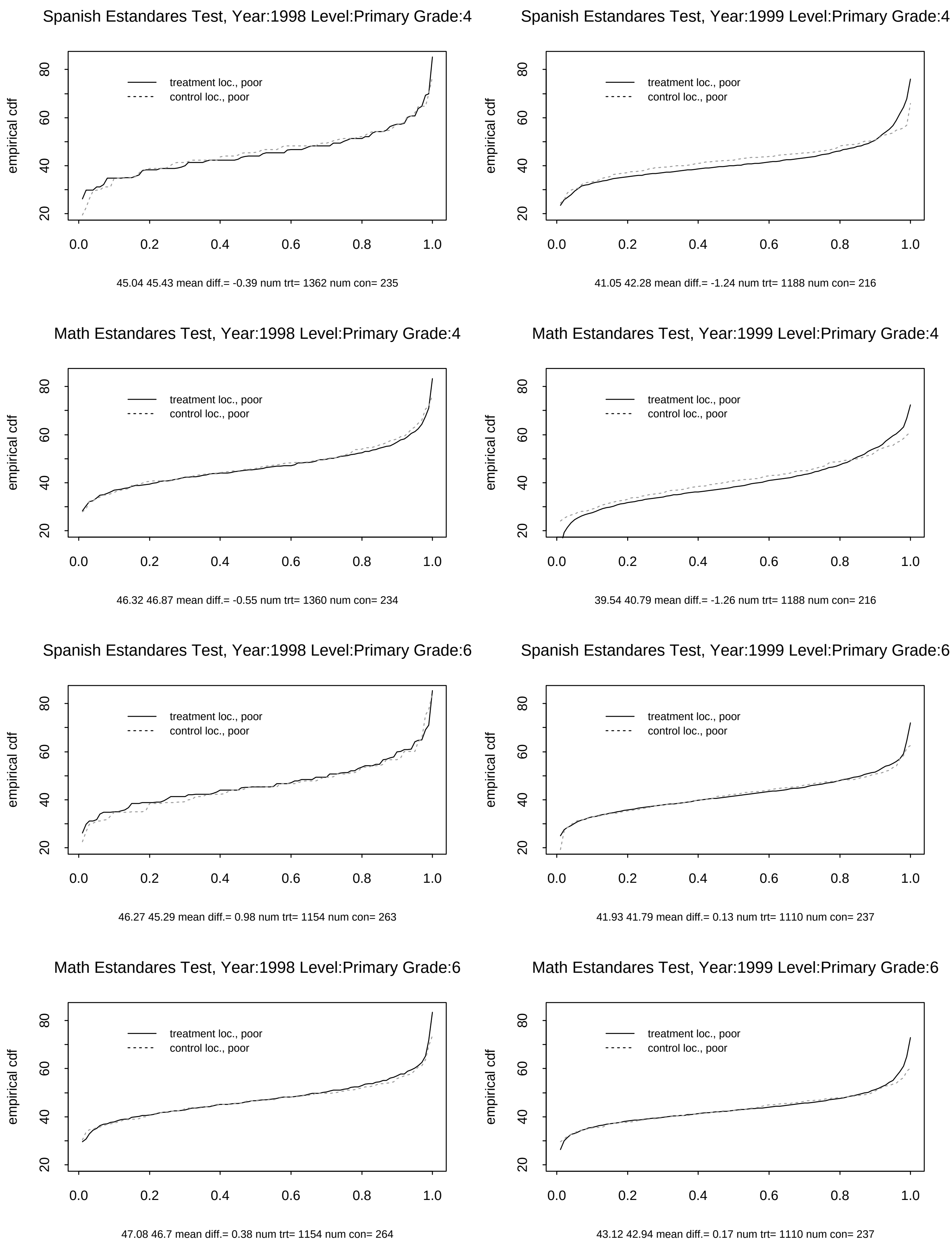


Figure 2.2: Empirical cdf of Estandares Test Scores, Girls

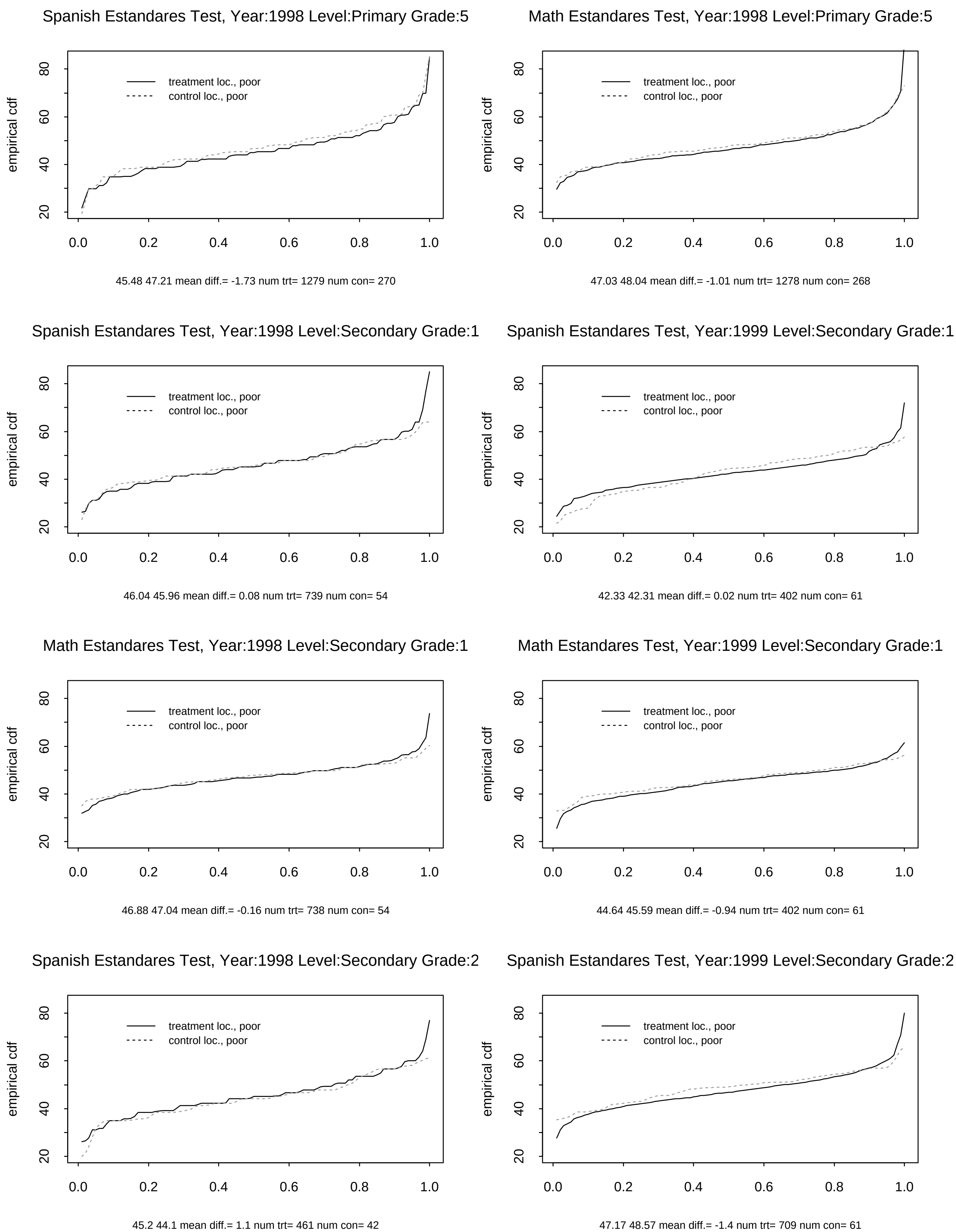
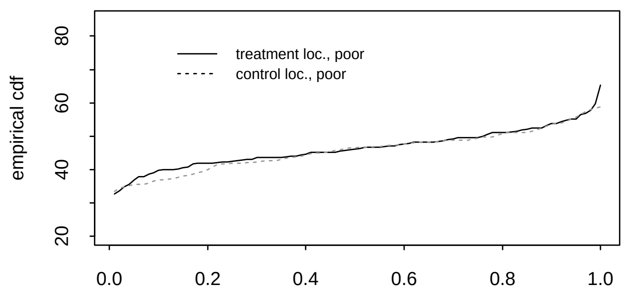


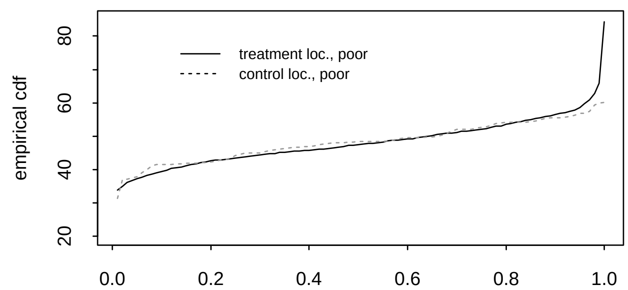
Figure 2.2: Empirical cdf of Estandares Test Scores, Girls

Math Estandares Test, Year:1998 Level:Secondary Grade:2



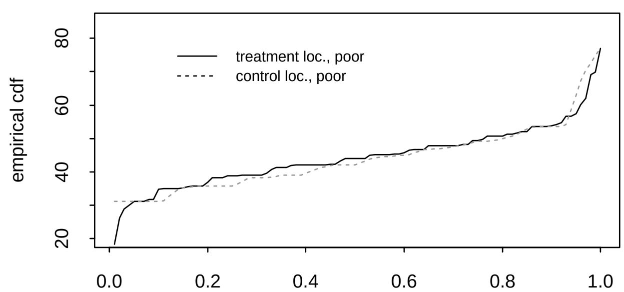
46.21 45.61 mean diff.= 0.6 num trt= 463 num con= 42

Math Estandares Test, Year:1999 Level:Secondary Grade:2



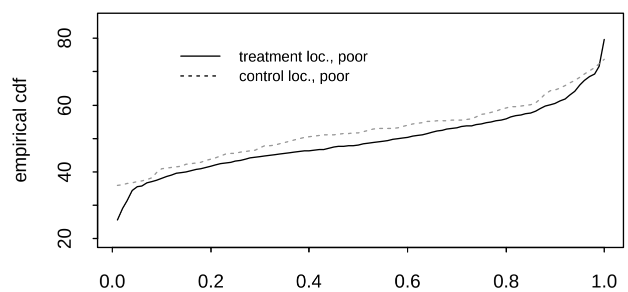
47.93 47.98 mean diff.= -0.05 num trt= 709 num con= 61

Spanish Estandares Test, Year:1998 Level:Secondary Grade:3



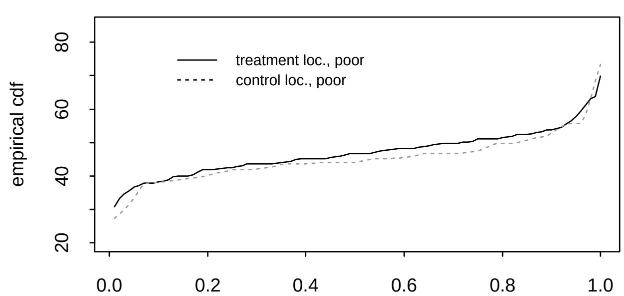
44.12 44.08 mean diff.= 0.04 num trt= 357 num con= 29

Spanish Estandares Test, Year:1999 Level:Secondary Grade:3



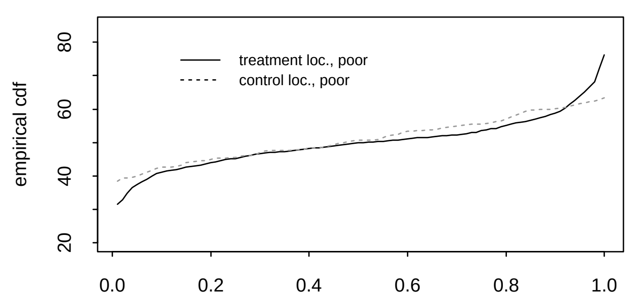
48.93 52.04 mean diff.= -3.11 num trt= 461 num con= 53

Math Estandares Test, Year:1998 Level:Secondary Grade:3



46.61 45.07 mean diff.= 1.54 num trt= 357 num con= 29

Math Estandares Test, Year:1999 Level:Secondary Grade:3



49.77 50.87 mean diff.= -1.1 num trt= 461 num con= 53

Figure 2.3: Empirical cdf of Estandares Test Scores, Males

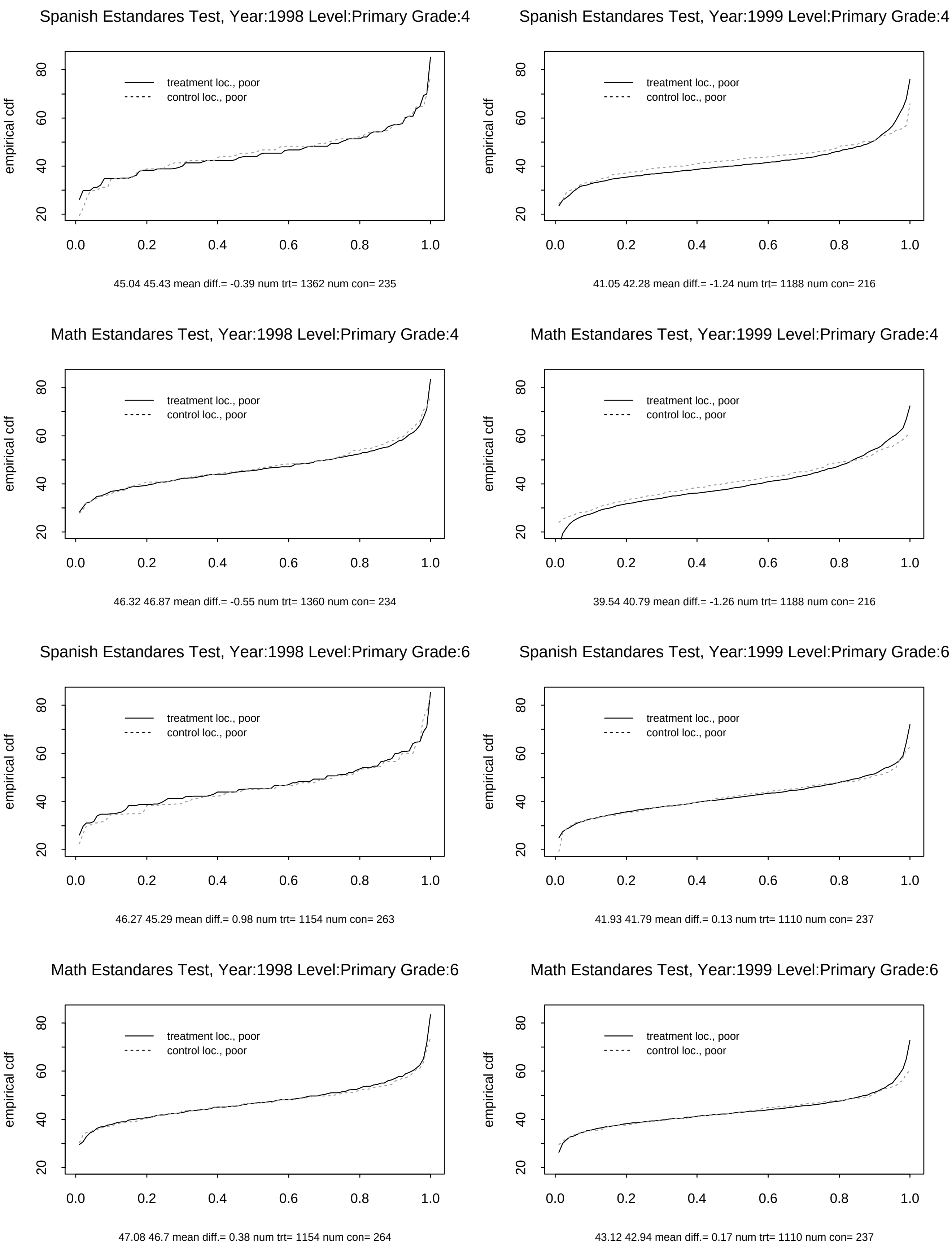
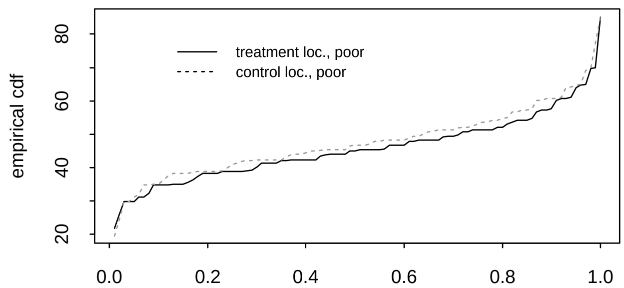


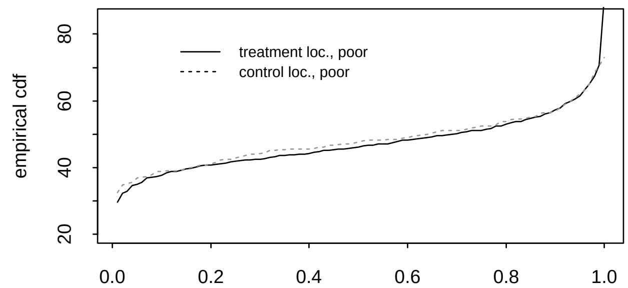
Figure 2.3: Empirical cdf of Estandares Test Scores, Males

Spanish Estandares Test, Year:1998 Level:Primary Grade:5



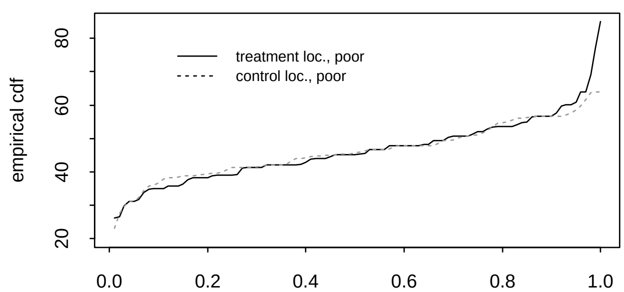
45.48 47.21 mean diff.= -1.73 num trt= 1279 num con= 270

Math Estandares Test, Year:1998 Level:Primary Grade:5



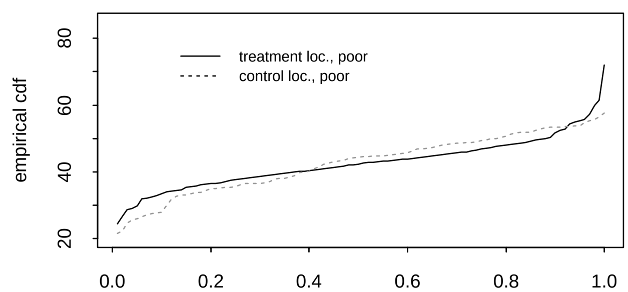
47.03 48.04 mean diff.= -1.01 num trt= 1278 num con= 268

Spanish Estandares Test, Year:1998 Level:Secondary Grade:1



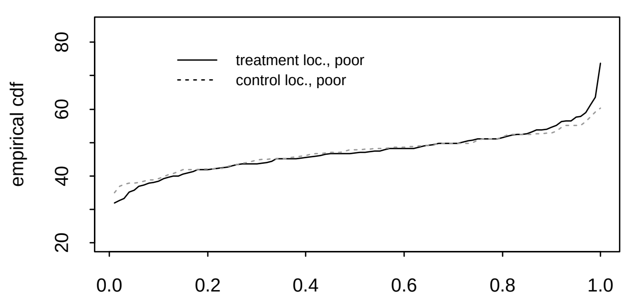
46.04 45.96 mean diff.= 0.08 num trt= 739 num con= 54

Spanish Estandares Test, Year:1999 Level:Secondary Grade:1



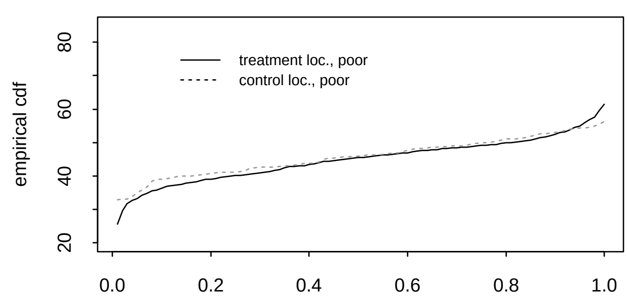
42.33 42.31 mean diff.= 0.02 num trt= 402 num con= 61

Math Estandares Test, Year:1998 Level:Secondary Grade:1



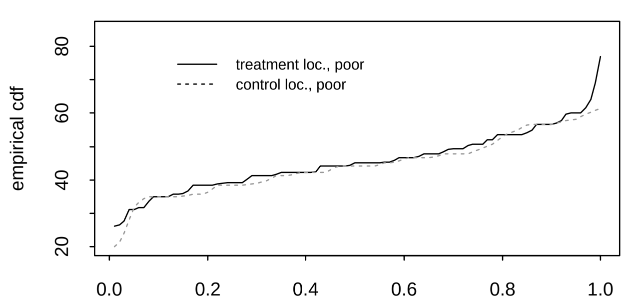
46.88 47.04 mean diff.= -0.16 num trt= 738 num con= 54

Math Estandares Test, Year:1999 Level:Secondary Grade:1



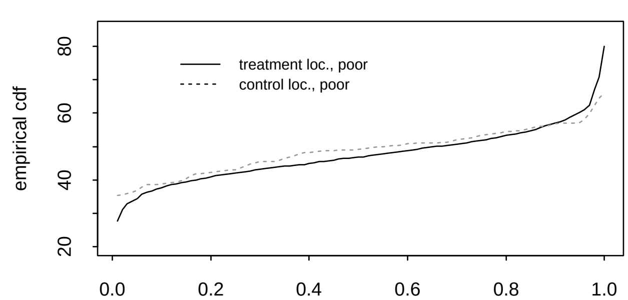
44.64 45.59 mean diff.= -0.94 num trt= 402 num con= 61

Spanish Estandares Test, Year:1998 Level:Secondary Grade:2



45.2 44.1 mean diff.= 1.1 num trt= 461 num con= 42

Spanish Estandares Test, Year:1999 Level:Secondary Grade:2



47.17 48.57 mean diff.= -1.4 num trt= 709 num con= 61

Figure 2.3: Empirical cdf of Estandares Test Scores, Males