PROGRESSING THROUGH PROGRESA: AN IMPACT ASSESSMENT OF A SCHOOL SUBSIDY EXPERIMENT IN MEXICO*

Jere Behrman
University of Pennsylvania

Piyali Sengupta
University of Pennsylvania

Petra Todd
University of Pennsylvania and NBER

June, 2002

*We thank the International Food Policy Research Institute (IFPRI) and PROGRESA for making the data used in this study available. We are especially grateful to Monica Orozco, Daniel Hernandez, Santiago Levy, and Susan Parker for help in answering questions about the data. We also thank Moshe Buchinsky, Anne Case, William Crockett, Angus Deaton, Bo Honore, Alan Krueger, Gabriel Martinez, Robert Moffitt, Christina Paxson, John Strauss, Robert Willis, Ken Wolpin and participants at seminars at Princeton, Johns Hopkins, Brown, University of Pennsylvania, Michigan State, Penn State, Maryland, University of Chile, Catholica University, Instituto Technologico Autonomo de Mexico, and at the NBER Inter-American Seminar on Economics in Costa Rica in November, 2000 for helpful comments. This paper is part of the IFPRI PROGRESA Evaluation Project. Todd's participation in this project was funded in part by a grant from the University of Pennsylvania Research Foundation. The authors may be contacted through email at jbehrman@econ.sas.upenn.edu, psngpt@aol.com, or petra@athena.sas.upenn.edu or at University of Pennsylvania, 160 McNeil Building, 3718 Locust Walk, Philadelphia, PA 19104.

PROGRESSING THROUGH PROGRESA: AN IMPACT ASSESSMENT OF A SCHOOL SUBSIDY EXPERIMENT*

Abstract

A new anti-poverty program in Mexico, PROGRESA, provides monetary transfers to families that are contingent upon their children's regular attendance at school. The benefit levels vary with the grade level and gender of the child and are intended to offset the opportunity costs of not sending children to school. The initial phase of the program was implemented as a randomized social experiment.

This paper uses a Markov schooling transition model applied to the experimental data to assess the impact of the subsidy program on schooling attainment and on the underlying behaviors that determine schooling attainment, including ages of matriculation, dropout rates, grade repetition rates, and school reentry rates. Results show that the program increases schooling attainment effectively by reducing drop-out rates and facilitating grade progression, particularly during the transition from primary to secondary school. Many of these effects would not be clear if attention were limited to enrollments as in much of the previous literature. A simulation evaluating the effects of longer terms of exposure to the program indicates that if children were to participate between ages 6 to 14, they would experience an increase of 0.7 years in average educational attainment levels and an increase of 21% in the percentage of children attending junior secondary school, with somewhat larger effects for boys than for girls.

1. Introduction

Increasing human capital investments in children is considered to be among the most effective ways of encouraging growth and of alleviating poverty in developing countries. To stimulate such investments, many governments in Latin America and Asia have initiated programs that provide financial incentives for families to send their children to school.[1] This paper evaluates the effects of a large-scale anti-poverty and human resource program, called PROGRESA, that was introduced in Mexico in 1997.[2] The program provides aid to approximately 2.6 million poor families, which represent about 40% of rural families and 10% of all families living in Mexico. It operates in over 50,000 localities in 31 states, with an annual budget of approximately 1 billion dollars.[3] A major goal of PROGRESA is to provide households with sufficient means and resources to allow their children to complete basic education.

An important component of PROGRESA is the provision of transfer payments to families that are contingent upon their children regularly attending school. The transfers are intended to alter the private incentives to invest in education by offsetting the opportunity cost of not sending children to school. In recognition of the fact that older children are more likely to engage in family or outside work, the transfer amount increases with the child's grade level and is greatest for children in secondary school. The benefit level is also slightly higher for female children who traditionally have lower secondary school enrollment levels.[4] In addition to educational subsidies, the program also provides monetary aid for poor families and nutritional supplements for their infants and small children that are not contingent on schooling.[5] In total, the benefits that families receive through PROGRESA are often substantial relative to their income levels. The average total cash transfer is US $55 per month, which represents over a fifth of the average family income. (Skoufias and Parker, 2000)

For the purposes of evaluation, the initial phase of PROGRESA was implemented as a

---

[1] For example, such programs exist in Bangladesh, Pakistan, Argentina, Chile, Colombia, Brazil, Nicaragua, and Honduras.

[2] PROGRESA stands for Programa de Educacion, Salud, y Alimentacion.

[3] See Gomez de Leon and Parker (2000), Coady (2000), Gertler and Levy (2002) and section 3 of this paper for more information on program coverage. The program continues to be expanded subsequent to the period covered by the data that we use, and is currently being extended into more urban areas. Also, the program's name has been changed in 2002 to OPPORTUNIDADES. Its key features remain the same, though with some changes in coverage including extension of the educational subsidies to upper secondary schooling.

[4] See Table 1 for a schedule of how benefit amounts vary by child grade and gender.

[5] Some of this aid is contingent on visiting a health clinic.

randomized social experiment, in which 506 rural villages were randomly assigned to either participate in the program or serve as controls. Randomization, under ideal conditions, allows mean program impacts to be assessed through simple comparisons of outcomes for the treatment and control groups. Schultz (2000a, 2000b) investigates the program's impact on school enrollment and finds significant impacts on enrollment, particularly for the first year of junior secondary school (7th grade).

This paper provides a disaggregated assessment of the impacts of PROGRESA on education. Our aim is to understand not only the gross impact on enrollments, but also how the program affects the underlying components that determine enrollments–ages of school matriculation, grade repetition and drop-out rates and school reentry rates among dropouts.[6] To distinguish empirically among these separate components of the overall impacts, we use a Markov schooling transition model. Similar statistical models have been used in both the sociology and economics literatures, usually in studying the influence of family background on schooling and not in examining the effects of social programs.[7]

This paper also examines whether program impacts differ by children's propensities to attend school, and it considers the evidence for spillover effects on children who reside in the same communities as program participants but do not satisfy the program eligibility criteria. This paper further investigates the sources of gender differences in educational attainment levels. We find that girls on average tend to progress more quickly than boys through elementary grades and have higher schooling on average, but they are more likely to drop out after completing primary school and are less likely to return after having dropped out. We also find that, although the program scholarship schedule favors girls at the secondary level, the impact of the program on school attainment is somewhat larger for boys.

The data we analyze cover only the first two years of the operation of PROGRESA, so all children are observed participating in the program for at most two years after program initiation. This makes it impossible to assess directly the impact of long-term participation in the program. However, long-term impacts are of key interest, as the policy change being considered is that of making PROGRESA a permanent program. We therefore propose a way of simulating the effects of longer-term exposure to the program that can be implemented even when children are only observed only over a short time interval, as is often the case in the evaluation of new social programs. Results based on our simulation procedure indicate that if children were to participate over an eight-year time period starting at age 6, their educational attainment distribution would

---

[6] Some previous studies for developing countries find that enrollment rates are fairly limited and possibly a misleading indicator of schooling progress. For example, Behrman and Knowles (1999) report fairly small enrollment differentials associated with parental income in Vietnam and much larger grade repetition differences.

[7] See Heckman and Cameron (1998) for a recent discussion of the use of Markov schooling transition models in economics and sociology.

change substantially. In particular, average educational attainment would increase by 0.7 years and 21% more children would attend some secondary school grades.

The plan of the paper is as follows. Section two describes the key parameters of interest in this study, the Markov schooling transition model, and the assumptions required to apply the simulation method used to evaluate the effects of long-term program participation. Section three provides additional information on the PROGRESA program and data subsamples. Section four presents the empirical findings. Simple comparisons of enrollment rates reveal effects of the program only for older children, with the greatest changes observed in the age ranges and grade levels for which the school subsidies are greatest, consistent with reported findings in Schultz (2000a, 2000b). However, when we disaggregate the data more finely, using the schooling transition model, it becomes clear that younger children are also affected by the program. For children age 6 to 10 years, program participation is associated with less grade repetition and better grade progression. For children age 11 to 14, the program decreases the dropout rate, particularly during the transition from primary to secondary school, and encourages school reentry among those who have dropped out. Section 4.2 of the paper presents results from our simulation of the effects of longer exposures to the program. The simulated long-term impact of the program is to increase average schooling attainment by an average of about 0.7 grades, with somewhat larger increases for boys than girls. These estimates imply cost-benefit ratios of at least 1.7, suggesting that from a social point of view the program is productive for those who participate, and we are not able to find any evidence of spillover effects on others. Section five concludes.

2. Parameters of Interest and Estimation Methods

A key parameter of interest in many evaluations of social programs is the so-called *treatment-on-the-treated* parameter, which gives the average impact of the program on persons who participate in the program. To define this parameter, let e_1 and e_0 be indicator variables that denote some outcome of interest, for example school enrollment, in the two potential outcome states corresponding to with and without treatment, where treatment is participation in the PROGRESA program. The treatment effect for an individual is given by $\Delta = e_1 - e_0$. Let T equal 1 if a child resides in a treatment village and let D be an indicator for whether the child is eligible for the program.[8] Children for whom $T = 1$ and $D = 1$ participate in the program.[9] Let X denote additional conditioning variables, such as the age or sex of the child. The difference

$$\Delta_{TT} = E(e_1| \ X, T = 1, D = 1) - E(e_0 \mid X, T = 1, D = 1)$$

---

[8] As described below, in section 3.1, not all children residing in a treatment community are eligible for the program–only those living in poorer households. Therefore, the distinction must be made between D and T.

[9] Participation is almost universal among eligible families for whom $T = 1$ and $D = 1$.

gives the average program impact on enrollment for children participating in the program (this parameter may be defined with or without conditioning on X). Because e is binary, Δ_{TT} corresponds to the mean difference in the conditional probability of enrollment.

Data from a randomized social experiment allows for direct estimation Δ_{TT}. The treatment group provides the data required to estimate $E(e_1| \ X, T = 1, D = 1)$, and the randomized-out control group provides the data to estimate $E(e_0 \mid X, T = 1, D = 1)$.[10]

Spillover effects of the program on children who do not directly participate in the program but who reside in treatment communities can be assessed by comparing outcomes of ineligibles in treatment and control communities. This comparison yields an estimator for

$$\Delta_S = E(e_1| \ X, T = 1, D = 0) - E(e_0 \mid X, T = 1, D = 0),$$

which we use to examine whether treatment has spillover effects on ineligible children. Below, we report estimates of the above treatment impact parameters within the context of a Markov schooling transition model.

2.1. A Markov Model of Schooling Transitions

There are multiple channels through which the school subsidy program may affect educational attainment levels. For example, the program may discourage children from dropping out of school and/or encourage school entry among dropouts or among those who have never enrolled in school. The program may also decrease grade repetition and thereby increase educational attainment levels, without necessarily changing enrollment rates. For this reason, program impacts on enrollment rates provide only limited information about the program's overall effectiveness and the mechanisms through which the program works.

We next describe a Markov schooling transition model that can be used to distinguish empirically different kinds of program impacts, including effects on the initial age of school entry, dropout rates, grade repetition rates, and the rate of school reentry among dropouts. The transition model provides a convenient framework for studying the dynamics of educational progression and for analyzing the experimental program impact along these various dimensions. The model will also be used later in the paper to simulate long-term program impacts by the method described below.

Let f_g^a denote the proportion of children age a enrolled in grade g, f_{ne}^a the proportion never enrolled and f_{drop}^a the proportion who were enrolled at some time in the past but whose current status is "dropped out." For six-year-old children, there are three possible schooling states (not

---

[10] Randomization implies that the mean enrollment of the randomized-out control group $E(e_0 \mid X, T = 0, D = 1)$ equals the counterfactual mean no-program enrollment of the treatment group $E(e_0 \mid X, T = 1, D = 1)$. See Heckman, Lalonde, and Smith (1999) for further discussion of the *treatment-on-treated* parameter as well as other parameters of interest in evaluation studies.

yet enrolled, enrolled in grade one, or enrolled in grade two) with the majority of six-year-olds enrolled in grade one.[11] For seven-year-olds, the number of states increases to six (enrolled in grade three, grade two, grade one, dropped out after being enrolled as a six-year-old, and not yet enrolled), with enrolled in grade two being the most common state.

A transition probability matrix describes the transition from the six-year-old schooling states to the seven-year-old states. That is, given the initial distribution of six-year-olds in each of the three states (the elements in the 3×1 vector below), the distribution of seven-year-olds can be obtained by applying the 5×3 transition matrix:

$$\begin{pmatrix} f_3^7 \\ f_2^7 \\ f_1^7 \\ f_{drop}^7 \\ f_{ne}^7 \end{pmatrix}_{5\times1} = \begin{pmatrix} p_{11}^6 & p_{12}^6 & p_{13}^6 \\ p_{21}^6 & p_{22}^6 & p_{23}^6 \\ 0 & p_{32}^6 & p_{33}^6 \\ p_{41}^6 & p_{42}^6 & 0 \\ 0 & 0 & p_{53}^6 \end{pmatrix}_{5\times3} \begin{pmatrix} f_2^6 \\ f_1^6 \\ f_{ne}^6 \end{pmatrix}_{3\times1}$$

For example, the element p_{22}^6 denotes the conditional probability that a 6-year-old enrolled in first grade advances to second grade. Similarly, p_{32}^6 is the probability of repeating first grade. The cells set equal to zero impose the restrictions that students cannot regress in grades and, once enrolled, they can no longer enter the state of being "never enrolled" (ne). We allow students to skip a grade because it is not uncommon in our data for children entering school for the first time to be placed in grade 2 or 3 along with the rest of their age cohort.[12]

Let A^a denote the transition matrix for children of age a and f^a denote the vector of schooling state proportions. In this notation, the last equation can be written as:

$$f^7 = A^6 f^6.$$

The number of rows of the A matrix increases with age as the number of potential grade levels increases.[13] In specifying the A matrices, we allow for reentry into grades from the dropout state, which is an important phenomenon in our data. For example, 86% of children in the dropout state at age eight reenter school.

The procedure by which the transition matrices and vectors of state proportions are estimated is nonparametric. Conditional on being in a given state at a given age, the next state is the outcome of a draw from a multinomial distribution. We denote by $\hat{A}^a_{T=1}$ the estimated transition matrix for treatment group children of age a. $\hat{f}^a_{T=1}$ denotes the corresponding vector

---

[11] We treat kindergarten and not enrolled as the same state.

[12] However, it is uncommon in our data to observe skipping of more than 3 grades, so we do not allow skipping of more than 3 grades.

[13] Going from age six to age seven the number of rows increases by two because there is no drop-out state at the first age, age six. At later ages, the number of rows increases by one for every additional year.

of state proportions, estimated by sample proportions. $\hat{A}^a_{T=0}$ and $\hat{f}^a_{T=0}$ denote the analogous objects for the control group.

2.2. Estimating Program Impacts

2.2.1. One-year impacts

The short-run, one-year experimental impact of the program on children of a given age a can be assessed by comparing the age-specific transition matrix estimated for treatments and controls:

$$\hat{A}^a_{T=1} - \hat{A}^a_{T=0}.$$

This comparison is informative on how short-term participation in the program affects ages of matriculation, grade progression, dropping out and school reentry (among dropouts) at each age, as well as the overall impact on enrollments.

We use Pearson chi-squared tests to test for whether observed treatment and control differences are statistically significant. In the empirical work, we perform two kinds of tests: tests of the equivalence between the treatment and control transition matrices and tests of equivalence between the individual columns of the matrices. The test statistic for testing equivalence between two matrices (for $T = 1$ and for $T = 0$) is given by:

$$\sum_{T\in\{0,1\}} \sum_{r,c} \frac{(\widehat{p}^a_{T,r,c} - \widehat{p}^a_{r,c})^2}{\widehat{p}^a_{r,c}} \quad \sim \quad \chi^2(N_c N_r - N_c - N_Z)$$

where r, c denotes the rth row and cth column. N_r and N_c denote the number of rows and columns of the transition matrices, respectively. N_Z denotes the number of elements set equal to zero. $\widehat{p}^a_{T,r,c}$ are the transition probabilities estimated conditional on treatment status, T, and $\widehat{p}^a_{r,c}$ the probabilities estimated unconditionally (i.e. combining the treatment and control data). The Pearson test compares the conditional and unconditional transition probabilities, which would be expected to be equal under the null hypothesis of no treatment effect.

2.3. Simulating the Impact of Longer Term Exposure to the Program

In addition to evaluating the short-term impact of the program, we are also interested in the effects of longer-term participation. In fact, long-term impact is of greater interest than short-run program impact because the policy change being considered is that of making PROGRESA a permanent program. However, children in our dataset are observed for at most two years after program initiation, so we lack the data that would allow direct estimation of the effects of longer exposures to the program. Waiting until a cohort has participated in the program for many years would require too great a delay in evaluating the impact of the program. Therefore, we propose a simulation approach that uses the Markov schooling model to predict the effects of multiple years of exposure to the program. To simulate the impacts for a synthetic cohort from data on a cross-section (or a short panel) of children of different ages, we require the following additional assumptions:

(A-1) The transition probabilities at each age depend on whether the child is currently participating and on the child's current grade level.

(A-2) The age-specific transition matrices are stable over time.

Assumption (A-1) can be expressed formally as:

$$\Pr(s^{a+1} \mid s^a, T^a, H^a) = \Pr(s^{a+1} \mid s^a, T^a) \tag{2.1}$$

where s^{a+1} and s^a are the schooling levels at ages $a+1$ and a and where T^a denotes whether the child participates in the program at age a. H^a is a vector summarizing the schooling and program participation history up until age a. Assumption (A-1) allows transition probabilities to depend on program participation history, but it assumes that participation history matters only through its effect on the current schooling level.[14]

Under assumptions (A-1) and (A-2) and given an initial vector of state proportions at some age, the predicted schooling state proportions at any later age can be obtained by the product of the intermediate age transition matrices. For example, the predicted state proportions for eight-year-old children who participated in the program since age 6 can be obtained by

$$\tilde{f}^{8}_{T=1} = \hat{A}^{7}_{T=1}\ \hat{A}^{6}_{T=1}\hat{f}^{6}_{T=1},$$

where we denote the predicted proportions with a "$\tilde{}$" and objects that are directly estimated from the data with a "$\hat{}$". More generally, the predicted grade proportions at any age a is given by

$$\hat{f}^{a}_{T=t} = \left(\prod_{s=a_s}^{a-1} \hat{A}^{s}_{T=t}\right)\hat{f}^{a_s}_{T=t},$$

---

[14] Assumption (A-1) could be relaxed to allow the transition probability to depend in an explicit way on number of years participating in the program, conditional on current attainment level. For example, the transition probabilities could be estimated using a multinomial logit model that includes length of time in the program as a conditioning variable. Within such a framework, parametric assumptions allow extrapolation to length of exposure times that are outside the range of those observed in the data. However, given that in our data we only observe children for two years after program initiation, we do not attempt to estimate this type of dependence. Therefore, we assume that participation history matters only through its effect on the child's current schooling attainment level.

where a_s is an age prior to age a.

Determining how participation in the program affects the entire educational attainment distribution at any age requires keeping track of the highest grade completed among dropouts. To do this, we partition the dropout state according to the grade at which the child dropped out of school. For example, at age 8 the dropout state is partitioned into three distinct states: dropped out after completing grade 3, dropped out after completing grade 2 and dropped out after completing grade 1. The proportion of children that obtain any grade level G at age A can then be obtained by summing the predicted proportion currently enrolled in G and the predicted proportion of children who dropped out after having completed grade G. Performing this summation for each different grade level generates the simulated education distribution at age A. From the simulated education distribution, we calculate summary statistics, such as the average educational attainment level with and without participation in the program for varying lengths of participation.

3. Description of the program and of the data

3.1. The Datasets

The datasets gathered as part of the PROGRESA experiment provide rich information on variables related to the schooling, health, and consumption patterns of households. The datasets that we use were gathered from baseline household surveys administered in October, 1997 and March, 1998 and from two follow-up surveys administered at approximately one-year intervals. The program was started in the summer of 1998 and households began receiving transfer checks for schooling attendance in the fall of 1998.

Data are available at the individual and household level, but random assignment was performed at the community level because of the broader geographic nature of some of program benefits, such as improvements in local schools and health facilities, and because it was perceived that random assignment within small communities would be politically unpalatable. Household surveys were conducted in 320 randomly selected treatment localities (in which treatment was initiated soon after the baseline survey) and in 186 control localities (in which there was no treatment over the time period covered by our data).[15] As the program has recently been expanded into many of the control localities, it is possible that the behavior of the control groups over the time period when we observe them was influenced by their expectation of eventually

---

[15] All 506 of these localities were selected in a stratified random selection procedure (with stratification by populations of localities) from the localities identified by PROGRESA as being eligible to participate in the program, because of a "high degree of marginality" (determined primarily on the basis of analysis of data in the 1990 and 1995 population censuses (1990 Censo, 1995 Conteo)).

receiving benefits. However, they were not told during the period in which the data we use were collected that they would receive benefits and we assume in this paper that they constitute a valid comparison group.[16] As discussed below, the data show that the groups were highly comparable prior to the program.

Within treatment localities, only households that satisfy eligibility criteria receive the monetary transfers under the program, where eligibility is determined on the basis of a marginality index designed to identify the poorest families within each community.[17] Because program benefits are generous relative to families' incomes, most families deemed eligible for the program decide to participate in at least some aspects of it, though not all families are induced by the school-contingent transfers to send their children to school.

In sum, there are over 30,000 eligible children participating in the experiment. However, our datasets pertain to over 75,000 children because, within each community, data collection was exhaustive and data are available on children from families that were ineligible for the subsidies. These families might still be affected by the operation of the program in their communities through changes that occur in the quality of their schools and/or health services or through spillover effects from those students who were directly affected by the program. For example, rising enrollments of eligible children might lead to a deterioration in the quality of schooling for noneligible children, as measured by quality indicators such as pupil-teacher ratios and per pupil expenditures. To prevent such a deterioration, the PROGRESA program provided additional resources to the schools. We show below that there is no strong support for spillover effects on the education of children from ineligible families, suggesting that the program's impact mainly came through demand-side incentive effects rather than supply-side schooling changes. There may have been quality improvements that just offset any congestion effects.

3.2. Program objectives and benefit levels

The broad objective of PROGRESA is to improve the conditions of education, health and nutrition for poor families, particularly for children and their mothers, by providing services in the areas of education and health, as well as providing monetary assistance and nutritional

---

[16] If the control group had anticipated eventually receiving program benefits, this would not necessarily invalidate their use as a comparison group. However, the nature of the treatment effect would need to be redefined. If controls anticipated being brought into the program, the treatment would then correspond to being randomly denied program benefits for a certain length of time.

[17] Eligibility is based in part on discriminant analysis of a 1997 census conducted in the localities that had been determined to be eligible for participation in the program. The discriminant score is a function of characteristics of the household head (education, occupation, and age), measures of family assets, characteristics of the family's home (a measure of crowding, floor and wall characteristics, water access, lavatory), number of school eligible children, number of children, and number of school-age children who are not in school.

supplements. The program is made up of three components:

(i) Educational grants to facilitate and encourage the education of children by fostering their enrollment and regular school attendance, and to promote parents' appreciation of the advantages of their children's education.[18] At the same time, actions are taken to improve the quality of education and to ensure that school quality does not fall as a result of higher enrollments due to the program.

(ii) A strengthening of the quality of health services for all members of the family as well as efforts to reorient individuals and health service providers towards taking preventive actions towards health care and nutrition.

(iii) Monetary transfers in the amount of approximately $12/month and nutrition supplements aimed at improving the food consumption and nutritional state of poor families, particularly that of children and women who are generally the members of households perceived to suffer most from nutritional deficiencies. Nutritional supplements are primarily targeted at children 4 months-2 years old and to breast-feeding and pregnant women. They are also given to children age 2-5 years who exhibit signs of malnutrition. (Gomez de Leon and Parker, 2000) The requirement for receiving the benefits is attendance at a health clinic for preventative health checks.[19]

In this paper, our focus is on educational outcomes, and we expect the school-contingent transfers to play the greatest role in changing schooling attendance and enrollment patterns. Table 1 shows how the benefit schedule varies by grade and sex of the child. The benefit amount is increasing in grade and, at secondary school grade levels, the benefit is greater for female children. The greatest marginal increase in the benefit level comes at the transition from primary school to secondary school, at which transition many poor children in Mexico drop out of school. The transfer amount also increases from secondary grades 1 through 3, but the change in benefit levels is less steep than the change from primary grade 6 to the first year of secondary school.

The decline in attendance at secondary schools is partly due to the lack of school availability. All localities have one or more primary schools, but most do not have secondary schools. Therefore, attendance at secondary schools often requires traveling longer distances or attend-

---

[18] Receipt of benefits is contingent on children attending school at least 85% of the time, which is verified by school personnel.

[19] See Handa and Huerta, 2000, for an analysis of how the program affects clinic attendance, Behrman and Hodinott, 2001, for an analysis of how the program affects the nutritional status of infants, and Gertler (2000) for an analysis of how the program affects a range of health outcomes.

ing classes by telecommunication ("telesecondaria" schools).[20] Although compulsory schooling laws require that children attend secondary school (through grade 9), the laws are not strictly enforced and many children drop out earlier.[21] Grade progression is typically determined by grades during the school year as well as performance on national standardized tests. A certificate is awarded upon completion of primary school (grade 6).

Table 2 examines the relationship between working for pay and school attendance. The table gives the percentage of control group children age 6-16 who work for pay, the percentage of children who attend school among those working for pay and the average monthly earnings in pesos for working children. Mexican child labor laws allow paid work only for children age 14 or older, with restrictions on the type of work and number of hours worked for children age 14-16. However, these laws are not strictly enforced, particularly in the rural communities in our sample where we observe children working for pay as early as age 8. However, the participation rate is relatively low in the ages 8-11, ranging from 1-4%. At all ages, a higher fraction of boys than girls report working for pay. As expected, average monthly earnings generally increase with age. At the secondary grade levels, the schooling subsidy represents a little less than 40% of the average monthly earnings of children in the relevant age ranges. As shown in the second column, working for pay does not necessarily preclude attending school, but older children rarely combine school attendance with work for pay.

Gomez de Leon and Parker (2000) and Skoufias and Parker (2001) analyze cross-sectional data on children's time use in PROGRESA communities and find that girls on average devote more time to domestic work than boys, while boys spend more time on average in farm and market work. Children participating in domestic work often do so part-time for about 3 hours per day and continue to attend school. Participation in PROGRESA is associated with a significant decrease in time spent in domestic work for girls but no change in participation in other kinds of work. For boys of secondary school age, participation in the program is associated with a significant reduction in participation in market and farm work.[22]

---

[20] The distance learning classes are taken under the guidance of a single teacher who often acts as a tutor for children in multiple grades.

[21] The law was changed in 1992, when compulsory schooling was increased from 6 to 9 years.

[22] Ravaillon and Wodon (2000) evaluate the impact of a similar educational subsidy program in Bangladesh that provides in-kind transfers of food to families that send their children to school. They find that the subsidy program significantly decreases child labor, but that the lower incidence of child labor only accounts for about a quarter of the increase in school enrollment for boys and about an eighth for girls. They conclude that parents substitute other uses of their children's time, such as liesure, for schooling and that the program does not have a very strong effect on child labor.

4. Empirical Results

4.1. Impacts on Enrollment Rates and Schooling Lags

Figure 1 shows the percentages of PROGRESA-eligible children enrolled in school by age, sex and treatment status for the 1997, 1998, and 1999 fall data rounds. The 1997 year is pre-program, so randomized assignment implies that the enrollment rates for treatments and controls should be equal. The 1998 and 1999 years occur after program initiation when treatment and control differences can be attributed to the program.

As seen in the figure, enrollment rates fall substantially around the ages when most children finish primary school (ages 12 to 14), providing a rationale for the large percentage increase in the transfer amounts that occur at secondary grades (see the benefit schedule in Table 1). The enrollment rates continue to fall through age 16. Figure 2 shows the treatment-control difference in enrollment rates in each of the post-program years for the same data subsamples, where the dotted lines in the figures show pointwise 90% confidence intervals. The figures suggest a pattern of zero treatment impact at ages younger than eleven and a positive impact at older ages.[23]

Tables 3(a)-(c) present the corresponding regression coefficients obtained from a regression of enrollment proportions on a set of age indicators interacted with an indicator for whether the child is in the treatment group. The coefficients associated with the treatment interactions give the estimated age-specific program impacts. The bottom two rows of each table report p-values from tests of the hypothesis that the treatment impact is zero over the full age range and over the age range restricted to children twelve and older. Consistent with random assignment, we do not find evidence of pre-program enrollment differences; the hypothesis that treatment and control enrollment proportions are equal in the preprogram year (1997) cannot be rejected. However, in both post-program years, a joint F-test of zero treatment impact across the two age ranges rejects the hypothesis with p-values less than 0.0001.

4.1.1. Enrollment Impacts by Gender

Figure 1 also gives the enrollment rate in the preprogram period for girls and boys. The enrollment rates were higher for boys than girls at ages 11 and higher, apparently justifying the greater subsidy for girls than for boys in the program. But examination of how grade progression patterns differ for girls and boys, through estimation of schooling transition matrices according to the method described in section two, reveals a somewhat more complicated pattern.[24]

---

[23] Estimates based on a difference-in-difference comparison of post-program minus pre-program differences lead to the same inferences.

[24] For the sake of brevity, the transition matrix estimates are not shown here. They are included in Appendix A, which is available on request.

Estimates based on samples of boys and girls in the control group data allow a comparison of their educational accumulation patterns in the absence of any program intervention.[25] The transition matrix estimates show a general pattern of girls progressing *more* quickly than boys through primary school grades, but then dropping out of school at earlier ages. For example, at age 6, girls are more likely to progress from grade 1 to grade 2, a pattern that continues at age 7 when girls have a 12% higher advancement rate from grade 2 to 3. At ages 8, 9, and 10, advancement rates for girls again exceed those of boys by about 5%, but the differences are not statistically significant. At ages 12 and 13, when many children complete primary school, the educational advantage for girls reverses as girls begin dropping out of school in greater numbers. For example, 30% of twelve-year-old girls enrolled in grade 6 drop out in comparison to 15% of boys. Once girls drop out, they are less likely than boys to reenter. At age 13, only 10% of girls reenter school as compared with 26% of boys.

One measure of the overall differences in schooling by gender is provided by the differences in schooling gaps by gender, where the gaps are defined as the difference between the schooling grade that could have been completed if an individual had entered school at age six and had progressed one grade each year and the average schooling grade actually attained. Figure 3 shows the average gap in completed grades for girls and boys. For both the control and treatment samples in 1997 for all but two ages in the range 7 through 18, average schooling gaps are *larger* for boys than for girls, and the two exceptions in each case have fairly small differences between the gaps.[26] Thus, part of the gender difference in age-specific enrollment rates reflects the greater tendency for boys to lag behind the standard grade progression rate due to grade repetition and therefore to require more years of enrollment to achieve a given level of schooling.

These preprogram gender patterns suggest that it is of interest to investigate the impact of the program not only on gender differences in enrollment, but also on other schooling measures. Figure 2 (columns two and three) shows that program impacts on enrollment rates are greater for girls, which could be due to the higher subsidy level given to girls in the secondary school grade levels. (See Table 1) Figure 4 shows the impact of the program on the schooling gap, with a negative treatment-control difference corresponding to a positive effect of treatment. Beginning around age 10, the treatment group tends to have smaller schooling gaps than the control group, with apparently greater effects for boys versus girls.[27]

---

[25] Pearson chi-squared tests, reported in Table 5, do not reject the null hypothesis that the overall estimated transition matrices do not differ by gender. However, when the same tests are applied to individual columns of the matrices, they often reject equality at conventional significance levels.

[26] 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.

[27] For reference, Tables A.1(a)-(c) in Appendix A (available from the authors on request) tabulate the schooling gaps shown in the figures.

Thus, the preprogram gender differences are more complicated than is revealed by enrollment rates. Though boys have higher enrollment rates, because they proceed through the primary grades more quickly, girls have smaller schooling gaps. Also, the impact of the program on gender differences is more complicated than is revealed by the impact on enrollment rates. Enrollment rates increase more for girls, but schooling gaps are reduced more for boys. Therefore, to evaluate the program impact on gender differences comprehensively, it is necessary to go beyond analysis of enrollment rates, which we do by estimating impacts based on transition matrices in section 4.2.4.

4.1.2. Enrollment Impacts by Children's Enrollment Propensities

So far we have considered how program impacts vary with the age and sex of the child but not how they vary with other characteristics of the child, such as the family background or distance from school. For example, one question of interest is whether program impacts are concentrated among the children from the most well off or least-well off of the program-eligible families. We now examine the evidence for impact heterogeneity, using the conditional probability of enrolling in school in the absence of the program as a summary measure of child's background. The enrollment propensity is estimated using preprogram characteristics as conditioning variables. That is, for each age level, we estimate a logistic model for the probability of enrollment and then estimate program impacts conditional on the predicted propensity to enroll in school.

The logistic model includes an indicator for child's gender, indicators for mother's and father's education (ever enrolled in school, education > 6 years, and education > 9 years), distance to the nearest secondary school and its square, indicators for geographic location of the village (state or *entidad*), and indicators for housing characteristics (has a bathroom, has electricity, has more than one room). In addition, the model includes the proportion of children in the locality who report working for pay. The coefficients that are most often significant at conventional levels are the parental education variables (both mother's and father's) and geographic location effects.[28] For children age 11 or older, a higher proportion of children working for pay in the locality significantly lowers the probability of enrolling in school. For children 12 and older, a greater distance to secondary school also lowers the probability of enrollment. The percent correctly classified under the model as being enrolled or not enrolled ranges from 68.1% to 80%.

Figure 5 displays the program impacts conditional on the predicted propensity to enroll in the absence of the program for all children, girls and boys, where we have classified the enrollment propensities by quartile. The figures indicate greater impacts for children who are least likely to enroll in the absence of the program (i.e. children whose enrollment propensities put them

---

[28] Coefficient estimates for the logistic regressions are reported in Appendix B, available on request.

in the first and second quartiles), particularly for children in the older age ranges. The greater response of poorer households to the program is consistent with these households having more constrained access to capital markets and with the subsidies alleviating these constraints.[29]

4.2. Impact Estimates based on the Schooling-transition Model

As described in section two, there are multiple channels through which the educational subsidy program can impact children's educational attainment levels, not only through changes in enrollment rates. We next use the schooling transition model that was described in section two to study how participation in PROGRESA affects the process by which children enter school and pass through the grades. First, we estimate the short-term impact of the program through a comparison of educational transition patterns for treatment and control children. Second, we examine the evidence for spillover effects of the program on children living in PROGRESA communities who are ineligible for program benefits. Third, we simulate the long-term impacts of the program using the simulation method that was proposed in section two. Fourth, we investigate the sources of gender differentials in schooling by comparing transition patterns for girls and boys. Lastly, we use the estimated impacts to perform a cost-benefit analysis of the program.

4.2.1. Comparison of Treatments and Controls

Table 4 summarizes the overall average effects, by age, on the probability of repeating a grade, dropping out, and reentering school obtained by comparing enrollment status in 1997 and 1998. This table shows that some of the program effects are substantial and that some important effects occur at ages prior to program eligibility. Across almost the entire age range, we observe significant decreases in the probability of repeating a grade. Also notable are reductions in the probability of dropping out and increases in the probability of reentering school after having dropped out for older children, ages 11-14.

Tables 5(a)-(i) provide greater detail about the underlying changes in behavior. They show the estimates of the schooling transition matrices for children of ages 6 to 14. For example, Table 5(a) gives the estimated probabilities of transiting from three potential schooling states at age 6 to five potential schooling states at age 7. "G" denotes the source state, which corresponds either to a grade level or to the states of being "never enrolled" or "dropout." The age 7 destination states are shown in the first column of the table. In each table, the top panel gives the transition matrix for the treated group, the second panel that of the control group, and the

---

[29] Using a different estimation approach based on a structural model of school enrollment decisions, Sadoulet, Finan and Janvry (2001) similarly find greater impacts on school achievement for children from the poorest households.

last panel gives treatment-control differences. The row labeled 'No. obs' gives the number of observations in each of the age 6 states and the row labeled 'P(G)' gives the proportion of the total number of observations in each state. As treatment status was randomly assigned and no program had been implemented at the time of the first observation (Oct. 1997), randomization would imply that the unconditional probabilities, shown in the last row of the top two panels, are equal for treatments and controls, which is largely supported by the data. Finally, the last row of the third panel (labeled 'p-value') reports the p-values from Pearson chi-squared tests of the equality of each of the columns of the treatment and control matrices. Table 6 reports the p-values from tests of the equivalence of the entire matrices.

Impacts on Primary School Age Children In Table 5(a), we see that most 6-year-olds are enrolled in grade one, but roughly 10% have not yet enrolled in school. Grade repetition is common, and about one third of the children in the control group enrolled in grade one repeat the grade. Grade repetition may mean that children's attendance at school was not sufficiently regular to fulfill the requirements for completing the grade, in which case repeating a grade may not be much different from dropping out and then reentering school. In the treatment group, the repetition rate is about 8% lower and the probability of transiting to the second grade 11% higher than in the control group. Thus, participation in the program appears to foster grade progression and reduce grade repetition, even in the early grades when families do not yet receive monetary transfers for school attendance. These impacts could be due to the health component of the program, to the presence of older siblings in the program, or to forward-looking behavior on the part of the parents, who anticipate future program benefits once the child attains grade three. When we compare grade repetition rates of 6 year-olds who have older siblings in grades 3 through 9 to those without older siblings in these grades, we find that the repetition rates are insignificantly different across the two groups, both within treatment and control samples. This finding suggests that benefits paid to older siblings do not account for the decrease in grade repetition. Finally, among 6-year-olds, the overall school enrollment rates for treatment and controls are very similar, so focusing only on enrollment rates does not reveal the impact of the program that operates mainly in reducing grade repetition rates.[30]

Table 5(b) shows similar results for the age 7 → age 8 transitions, for which there is an additional state corresponding to dropping out after having been enrolled in school at age 6. At age 7, about 6% of children have yet to enroll in school. Most 7-year-olds (63%) are enrolled in second grade, though a substantial fraction (20%) are enrolled in first grade. Again, participation in the program is associated with better grade progression rates and lower grade repetition

---

[30] About 19% of children report being enrolled in grade 2, which is a higher grade than expected for a 6-year-old and may reflect misreporting of grades. Among these children, over 80% repeat the grade, which suggests that in many cases among these children the grade may have been misreported.

rates, both for first graders and second graders. Again, for 7-year-olds grade repetition rates are independent of having older siblings receiving scholarship benefits.

The patterns for the age 8 → age 9 and for the age 9 → age 10 transitions are very similar. For children enrolled in grade 3 at age 8 and for those enrolled in grade 4 at age 9 (the most prevalent grade levels), grade progression rates are higher by 8% in the treatment group and repetition rates lower by 7-8%. At age 9, 2% of children have never been enrolled and 1-2% have dropped out of school. As the second-to-last column shows, a high proportion of dropouts reenter school.

At age 10, for children in grades 4 or grades 5 (the most common grade levels), grade progression rates are higher and dropout rates lower for the treatment group. Repetition rates are similar across groups. Additionally, we observe a 9% higher rate of reentry from the dropout state for treatments (second-to-last column), although the total number of children in the dropout state is relatively small. At age 11, about one third of children have attained grade 6, the last year of primary school. About 23% of children in the control group drop out of school after completing primary school, as compared with 14% of treatment children. In both grades 5 and 6, participation in the program is associated with a greater probability of transiting to the next higher grade, a lower probability of repeating a grade, and a lower probability of dropping out.

Impacts on the Transition to Secondary School Although Mexican compulsory schooling laws require school attendance through grade 9, many children drop out of school during the transition from primary school to secondary school (See Figure 1). The large marginal increases in the PROGRESA benefit levels for children in secondary school grades are intended to encourage families to further their children's education beyond primary school. The estimated Markov transition matrices reveal substantial program impacts for children age 12-15. For example, at age 12, the grade progression rate is 11% higher for the treatment group than for the control group for the grade 6 to 7 transition and 9% higher for the grade 5 to 6 transition. The treatment drop-out rate is lower at all grade levels and the school reentry rate higher by 18%. A significant fraction of treatment group children are observed reentering school at grade 7, which is most likely a response to the large marginal increase in benefit levels.

Among 12-year-olds enrolled in grade 7, the grade repetition rate is somewhat higher in the treatment group and the grade progression rate lower, which suggests that some of the children who otherwise would have dropped out after completing primary school now remain in school because of the program, but fail to progress to the next higher grade. Similar patterns are observed in the age 13→age 14 transition matrix. By age 13, roughly a quarter of children have dropped out from school. The school reentry rate is 16% for controls and 33% for treatments, an increase of 17%. The drop-out rate is lower for the treatment group at all grades, especially at grade 6 where the differential is 29%.

Table 5(i) shows the estimated transition probabilities for the age 14→age 15 transition. By age 14, roughly 40% of children have dropped out of school; about 15% of these reenter, which does not differ much by treatment status. The grade progression rates are higher for the treated group for grades 5, 6 and 7. The rate of transiting from a grade to the drop-out state is lower for the treated group for all grades except the highest grade (grade 10), which may be due to the fact that there is no subsidy at grade 10. We do not find evidence, however, of children repeating grade 9 instead of moving on to grade 10 as might be expected if families tried to prolong receipt of the subsidy.

4.2.2. Are there spillover effects on children from ineligible families?

In addition to the educational subsidies, the PROGRESA program also gave additional resources to schools aimed at improving the quality of the schools in the PROGRESA communities.[31] These improvements were partly undertaken to prevent deterioration in quality that might result from induced enrollment increases with a fixed resource level. Because of the broader nature of these interventions, families who do not receive the subsidies because they do not satisfy the eligibility criteria might nonetheless be affected by the presence of the program in their community. For example, better equipped schools might attract greater numbers of ineligible children to school. On the other hand, increased enrollments might reduce school quality by increasing congestion. Also, higher school enrollments would be expected to coincide with a decline in the supply of child labor, which in turn could have additional effects by changing the labor market opportunities of children from ineligible families.

To examine the extent to which ineligible children are affected by the presence of PROGRESA, we reestimate the transition matrices using only the ineligible treatment and control groups. If these children are unaffected by the program, we expect the treatment and control transition matrices to be equal.[32] The test of equality of the matrices does not reject the null hypothesis that they are equal at conventional significance levels for every age. (P-values are reported in Table 6). Tests of equality of the individual columns of each matrix also show few instances of rejections. Thus the data are consistent with no spillover effects. If we look at the pattern of the estimated conditional probabilities, they suggest that if any spillover effects do exist, they are positive. At ages 8, 9 and 12, ineligible children residing in a treatment community have a lower grade retention rate in the most prevalent grade levels. At age 12, they also have

---

[31] Unfortunately, the available data do not permit a comparison of changes in direct indicators of school quality (e.g. class size, teacher/student ratios) in schools largely attended by children from treatment communities verses those attended largely by children from control communities.

[32] The estimated transition matrices are shown in Appenedix A, Table A.5(a)-(i), which is available upon request from the authors.

a lower probability of dropping out and a higher probability of reentering school conditional on having dropped out. However, overall these differences are not statistically significant.

4.2.3. Predicted Changes in the Educational Attainment Distribution After Longer Exposure to the Program

So far we have considered only the short-term impact of the program, but ultimately we are interested in the impacts for children who participate in the program over multiple years. With longer exposure we might expect year-by-year impacts to cumulate. Even if the program leads to modest changes in some of the elements of the transition matrices at each age, over time these impacts could generate substantial changes in the adult educational attainment distribution.

We next apply the methods described in section three to simulate the impact of longer-term program participation. Our first simulation assumes that a child participates continuously in the program for eight years, starting at age 6. We compare the predicted educational attainment distribution at age 14 with treatment to the predicted distribution for children who do not participate in the program over the same age range.

Table 7 gives the simulated pdf and cdf values for treatments and controls, where treatment now refers to being a member of a PROGRESA household for eight years, over ages 6-14. Figure 6 plots the corresponding histogram of educational attainment levels, which reveals substantial differences between the treatment and control groups. Our simulation predicts an average educational attainment of 7.83 for program participants at age 14, as compared to 7.15 years for controls. This implies a long-term average impact of the program of 0.68 additional years of education.[33] A comparison of the predicted cdf for treatments and controls shows that the program induces about 21% more children to attend some secondary school grades.[34]

---

[33] We also have conducted similar simulations using the 1998-99 data rather than the 1997-98 data. We focus on the 1997-98 data in the text, because we understand that for that year there was less anticipation among controls of soon being included in the program than in 1998-99 (in fact, they were incorporated for the 1999-2000 school year). Such anticipation would lead to higher enrollments at the secondary level for the controls, which indeed is the case for 1998-99 relative to 1997-98. Our simulation for 1998-99 indicates slightly smaller program effects - an average long-term impact of 0.30 additional years with 9% more children attending secondary school, as might be expected due to greater anticipation about being incorporated into the program. Using a structural model that explicitly allows for program announcement effects, Attanasio, Meghir and Santiago (2001) find these effects to be important.

[34] Schultz (2000a), as noted, has estimated the grade-by-grade effects of PROGRESA on enrollment rates. Using equal weight for each grade, he accumulates the grade-specific impacts across grades to get an implied longer-term impact estimate of 0.66 years (including an adjustment for preprogram group differences). When we instead weight his grade-specific estimates by the proportion of fourteen-year-old children in the control group who attained each of the grade levels, we obtain an estimated long-run program effect of 0.39, which is 57% smaller than the 0.68 impact estimate obtained by our simulation. One advantage of our simulation procedure

We next consider how program impacts vary with alternative lengths of exposure to the program. Table 8 reports the predicted average impact for fourteen year-olds who have participated for different numbers of years in the program, ranging from one to eight years. For example, a fourteen year-old who participated over the last 6 years would have started in the program at age eight. The average impact associated with five years of exposure is 0.63 as compared with 0.68 for eight years. This suggests that treatment in the three earliest ages (6,7,8) does not contribute much towards increasing average educational attainment at age 14. Most of the treatment impact occurs between ages 11-13.

4.2.4. Comparisons of Girls and Boys

As Table 1 shows, program benefit levels differ based on the gender and grade level of the child, with the greatest benefits going to female children in secondary school. In section 4.1, we showed that the enrollment impacts at the secondary level are larger for girls, but we also noted that there are channels in addition to enrollments through which the program may impact schooling attainment. Using the estimated transition matrices, we next examine whether the program has differential short-term and longer-term effects on girls and boys.[35] Through age 10, short-term program impacts by gender are similar, and, with few exceptions, both girls and boys in the program experience less grade repetition and better grade progression. At ages 11 and 12 , when dropping out starts to become empirically important, the program decreases drop-out rates of both girls and boys by roughly the same order of magnitude. For example, at age 13, the program decreases the probability of dropping out after completing primary school by 29% for both girls and boys. For girls, the program is effective in reducing dropping-out behavior in primary grades and in the first year of secondary school, but it has little impact on dropping out behavior in the second and third year of secondary school. For boys, there is a greater impact in reducing dropping-out rates at the higher secondary grade levels. If the long-term effects are estimated separately by gender using the procedure described above, we find that the increase for boys is 0.92 grades with 24% more attending secondary school and the increase for girls is 0.68 with 19% more attending secondary school. Thus, our simulation estimates a somewhat larger long-term impact for boys, even though benefit levels in secondary grades are slightly higher for girls.

---

over these calculations of the long-run effects is that it generates impacts on the entire treatment and control education distributions and not just on the means.

[35] The estimated transition matrices are included in an Appendix available on request from the authors. (Tables A.3(a)-(i) and Tables A.4(a)-(i))

4.3. Benefit-cost estimates

To assess whether the expected benefits of the program exceed the costs, we perform a benefit-cost calculation. First, we estimate the expected lifetime earnings with and without participation in the program, using the long-term impact estimate obtained above of 0.68 years additional years of secondary schooling and using estimates of returns to schooling and average worker wages that are drawn from the literature. We assume the rate of return to each year of secondary education is approximately 12% for both men and women (Schultz, 1988, Parker, 1999).[36] In addition, we assume a worker with the average number of years of education earns the average urban worker wage of 1300 pesos per month (Schultz, 2000b). Using these figures, the lifetime earnings for a person who does not participate in the program, begins working at age 15 and works until age 65 is

$$V_{T=0} = \sum_{a=15}^{65} \frac{\bar{y}}{(1+r)^a},$$

where $\bar{y} = 1300$ pesos/mo $\times$ 12 months and r is the discount rate, assumed to be either 3% or 5% in our calculations. The lifetime earnings for a person with 0.68 years additional years of education who works over the same time period and receives a rate of return equal to 12% for each additional year of education is given by

$$V_{T=0} = \sum_{a=15}^{65} \frac{\bar{y}(1+0.12 \times 0.68)}{(1+r)^a}.$$

The cost of the program is obtained by adding up the discounted values of the school subsidy amounts, given in Table 1, and the food subsidy benefit (fb), which is valued at 115 pesos per month.[37] Using an average family size of 5, we obtain a yearly food subsidy benefit per child equal to $(115/5) \times 12 = 276$ pesos, which we assume the child receives every year. Receipt of the school subsidy (s_a) begins at age 8, the earliest year the child can attain grade 3, for a total cost of

$$\text{cost}=\sum_{a=0}^{7} \frac{fb}{(1+r)^a} + \sum_{a=8}^{14} \frac{s_a + fb}{(1+r)^a}.$$

Comparing the program benefit in the form of higher lifetime earnings to the program cost

---

[36] These estimates are based on samples drawn from urban areas that are linked through migration to the rural PROGRESA communities in our sample. The authors find a rate of return equal to 5% for each year of primary schooling and 12% for each year of secondary schooling.

[37] The school subsidy was given for each of the nine months of the school year. In addition, we include in the cost calculation an annual subsidy given for school supplies. See the footnote to Table 1 for the school supply subsidy levels.

yields a benefit-cost ratio of 1.73 (2.69) for a real interest rate of 5% (3%).[38] However, this benefit ratio arguably understates the benefits of the program as it does not take into account increases in current consumption and improvements in health due to participation in the program as well as possible social welfare gains stemming from redistribution of income towards poorer households.

5. Conclusions

This paper uses a Markov schooling transition model to perform a disaggregated assessment of the impacts of the PROGRESA school subsidy program on the education of children in rural Mexico. This modeling approach permits a fuller analysis of program impacts than does a focus on enrollment rates alone, as in most previous studies. Indeed, consideration of impacts on repetition rates, drop-out rates and reentry rates in addition to enrollment rates leads to some different inferences regarding the impact of the program on schooling attainment. Comparisons of the estimated educational transition matrices for treatments and controls reveal that the program has a beneficial effect on the educational accumulation process, with statistical tests rejecting the hypothesis of zero program impact for most ages. Participation in the program is associated with higher enrollment rates, less grade repetition and better grade progression, lower dropout rates, and higher school reentry rates among dropouts. Particularly notable are the impacts on reducing dropout rates during the transition from primary to secondary school. Also notable are the impacts on grade progression that are observed even for younger children who do not yet receive educational subsidies under the program. These impacts were found to be independent of having older siblings receiving scholarships and are consistent with forward-looking behavior on the part of the parents. The program also appears to be effective in inducing children who dropped out prior to the initiation of the program to reenter school.

Comparisons of girls and boys in the control group show that in the absence of any intervention, girls tend to progress more rapidly than boys through the primary grades, with boys showing a greater tendency to lag behind in the number of grades completed. Because girls progress a little faster, lower school enrollments for girls at some ages need not imply less education. However, we find that girls are more likely than boys to drop out after completing primary school and female dropouts are less likely to reenter. This provides a rationale for the greater monetary transfers for girls in the secondary school grades.

When we examine program impacts by gender, we find that primary school impacts of the program are very similar by gender. At the secondary level, however, the program appears to be more effective in inducing boys to enroll in the second and third years of secondary school,

---

[38] If we omit the food subsidy benefit and base the calculcation only on the school subsidy benefit, we obtain a cost-benefit ratio of 2.54 for $r = 5\%$ and 3.83 for $r = 3\%$.

despite the fact that the benefit levels are slightly higher for girls. As a result, the program impact on schooling attainment is, if anything, greater for boys than for girls.

We find little support for the existence of strong spillover effects on children who do not receive school subsidies under the program but who reside in communities where PROGRESA is active. Our tests fail to reject the hypothesis that schooling transition patterns are the same among ineligible children in treatment and control communities. The absence of spillover effects suggests that the impact of the program came mainly through demand-side incentive effects rather than supply-side improvements in the schools, with the latter approximately balanced by congestion due to higher enrollments.

We also propose a simulation method for assessing the longer-run impact of the program, which is relevant because the policy change being considered is that of making PROGRESA a permanent program. Our method requires some auxiliary assumptions to be able to simulate the experiences of a synthetic cohort based on data from a cross-section of children of different ages. Empirical results show that longer-term participation would have a substantial effect on the age 14 educational attainment distribution. It would lead to about 21% more children enrolled in junior secondary school grades and about 0.7 more years of education on average for program participants, with somewhat larger long-term effects observed for boys.

Lastly, we undertake a benefit-cost analysis of the program. Under our most conservative assumptions, we obtain benefit-cost ratios of 1.7, which suggests that the program was justified in terms of the productivity gains even if distributional gains are not considered.

References

[1] Attanasio, Orazio, Meghir, Costas, and Ana Santiago (2001): "Education Choices in Mexico: Using a Structural Model and a Randomized Experiment to Evaluate PROGRESA," unpublished manuscript, University College London.

[2] Behrman, Jere and John Hoddinott (2001): "Program Evaluation with Unobserved Heterogeneity and Selective Implementation: The Mexican Progresa Impact on Child Nutrition," Philadelphia: University of Pennsylvania, mimeo.

[3] Behrman, Jere and Petra Todd (1999): "Randomness in the Experimental Samples of PROGRESA" International Food Policy Research Institute, Washington, D.C.

[4] Behrman, Jere and James Knowles (1999): "Household Income and Child Schooling in Vietnam" in *World Bank Economic Review* 13:2, 211-256.

[5] Cameron, Steve and James J. Heckman (1998): "Life Cycle Schooling and Dynamic Selection Bias: Models and Evidence for Five Cohorts of American Males" in *Journal of Political Economy*, Vol. 106, no. 2, p. 262-333.

[6] Coady, David (2000): "The Application of Social Cost-Benefit Analysis to the Evaluation of Progresa," International Food Policy Research Institute, Washington, D.C.

[7] Gertler, Paul (2000): "Final Report: The impact of PROGRESA on health." Report submitted to PROGRESA. International Food Policy Research Institute, Washington, DC.

[8] Gertler, Paul and Levy, Santiago and Jaime Sepulveda (2002): "Mexico's PROGRESA: Using a Poverty Alleviation Program as a Financial Incentive for Poor Families to Invest in Child Health," forthcoming in *The Lancet.*

[9] Gomez de Leon, Jose and Susan W. Parker (2000): "The Impact of Anti-Poverty Programs on Children's Time Use" PROGRESA, International Food Policy Research Institute, Washington, D.C.

[10] Handa, Sudhanshu and Marie-Carmen Huerta (2000): "An Impact Assessment of the Heath and Nutrition Component of Mexico's Anti-Poverty Program," unpublished manuscript, PROGRESA.

[11] Heckman, James J., Robert Lalonde and Jeffrey Smith (1999): "The Economics and Econometrics of Active Labor Market Programs" in *Handbook of Labor Economics*, *Volume III,* eds. O. Ashenfelter and D. Card (Elsevier: Amsterdam).

[12] Parker, Susan W. (1999): "Explaining Differences in Returns to Education in 39 Mexican Cities," unpublished manuscript, PROGRESA, Mexico City.

[13] Ravaillon, Martin and Quentin Wodon (2000): "Does Child Labor Displace Schooling? Evidence on Behavioral Responses to an Enrollment Subsidy" in *The Economic Journal*, 110, March, C158-C175.

[14] Sadoulet, Elisabeth, Finan, Frederico, and Alain de Janvry (2001): "How effective are educational subsidies for the rural poor? PROGRESA in Mexico," unpublished manuscript, Berkeley.

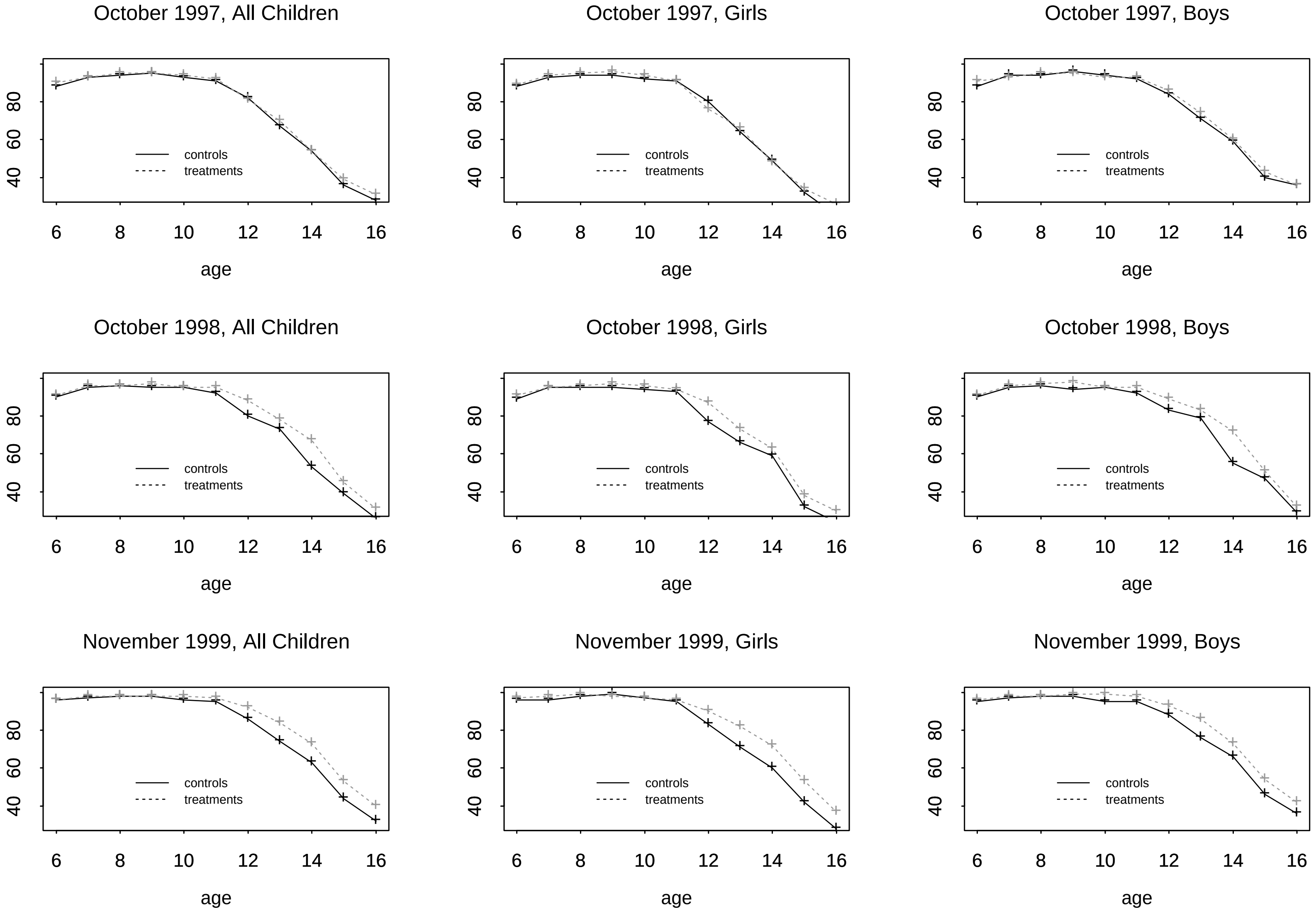
[15] Schultz, T. Paul (1988): "Education Investment and Returns," in *Handbook of Development Economics*, Vol. I, eds. H. Chenery and T. N. Srinivasan, Amsterdam: North Holland Publishing.

[16] Schultz, T. Paul (2000a): "Progresa's Impact on School Enrollments from 1997/98 to 1998/99," International Food Policy Research Institute, Washington, D.C.

[17] Schultz, T. Paul (2000b): "School Subsidies for the Poor: Evaluating a Mexican Strategy for Reducing Poverty," forthcoming, *Journal of Development Economics.*

[18] Skoufias, Emmanuel and Susan W. Parker (2001): "Conditional Cash Transfers and Their Impact on Child Work and Schooling: Evidence from the PROGRESA Program in Mexico," *Economia* 2:1 (Fall, 2001), 45-96.

Figure 1: Percentage Enrolled by Age



October 1997, All Children

October 1997, Girls

October 1997, Boys

October 1998, All Children

October 1998, Girls

October 1998, Boys

November 1999, All Children

November 1999, Girls

November 1999, Boys

Figure 2: Average Treatment Impacts on Proportion Enrolled by Age

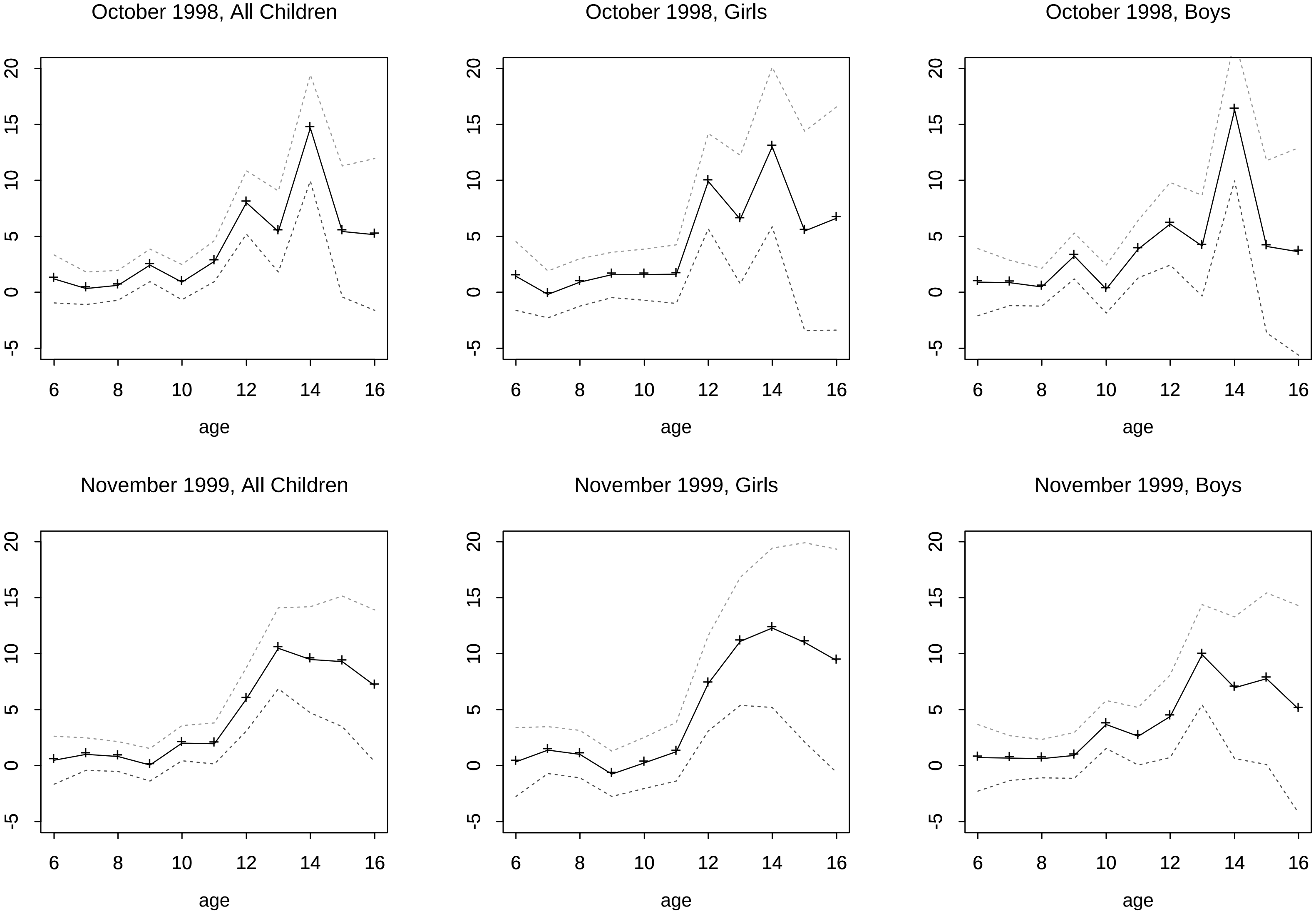
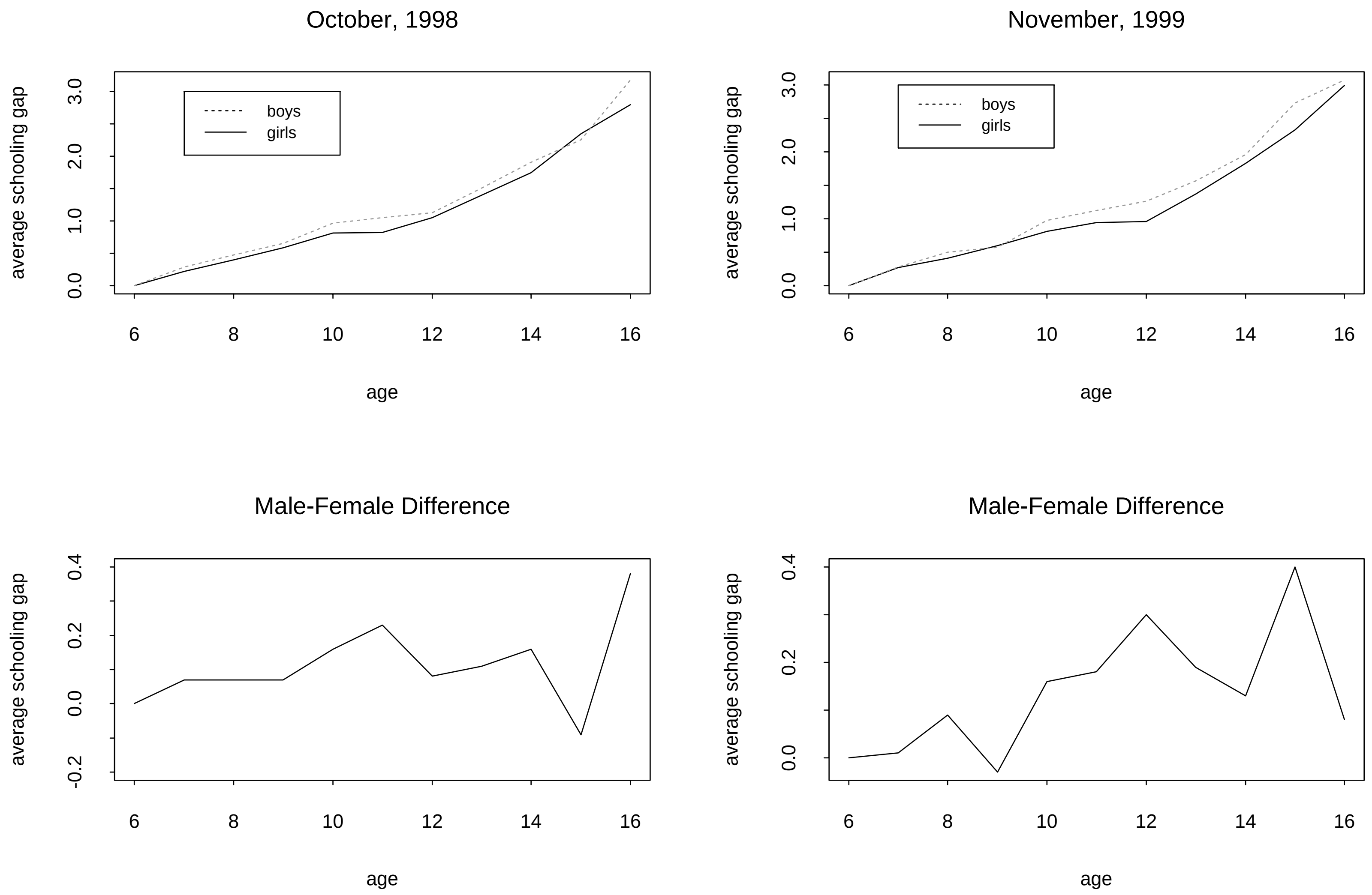


Figure 3: Comparison of Schooling Gap for Girls and Boys, Control Group
(Gap=Potential Grade Level-Actual Schooling Grade Level)



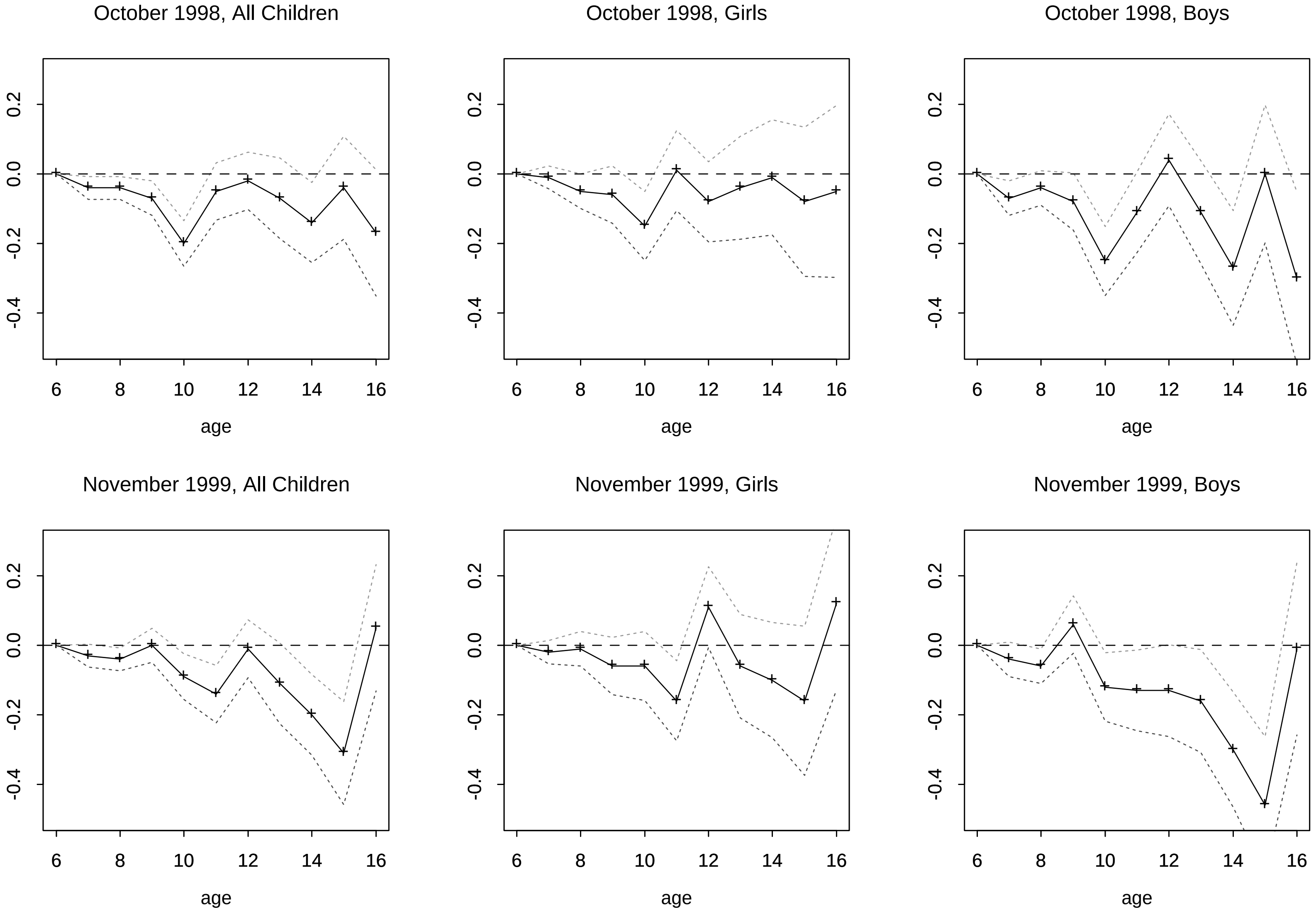
October, 1998

November, 1999

Male-Female Difference

Male-Female Difference

Figure 4: Average Treatment-Control Schooling Gap
(Gap=Potential Grade Level-Actual Schooling Grade Level)



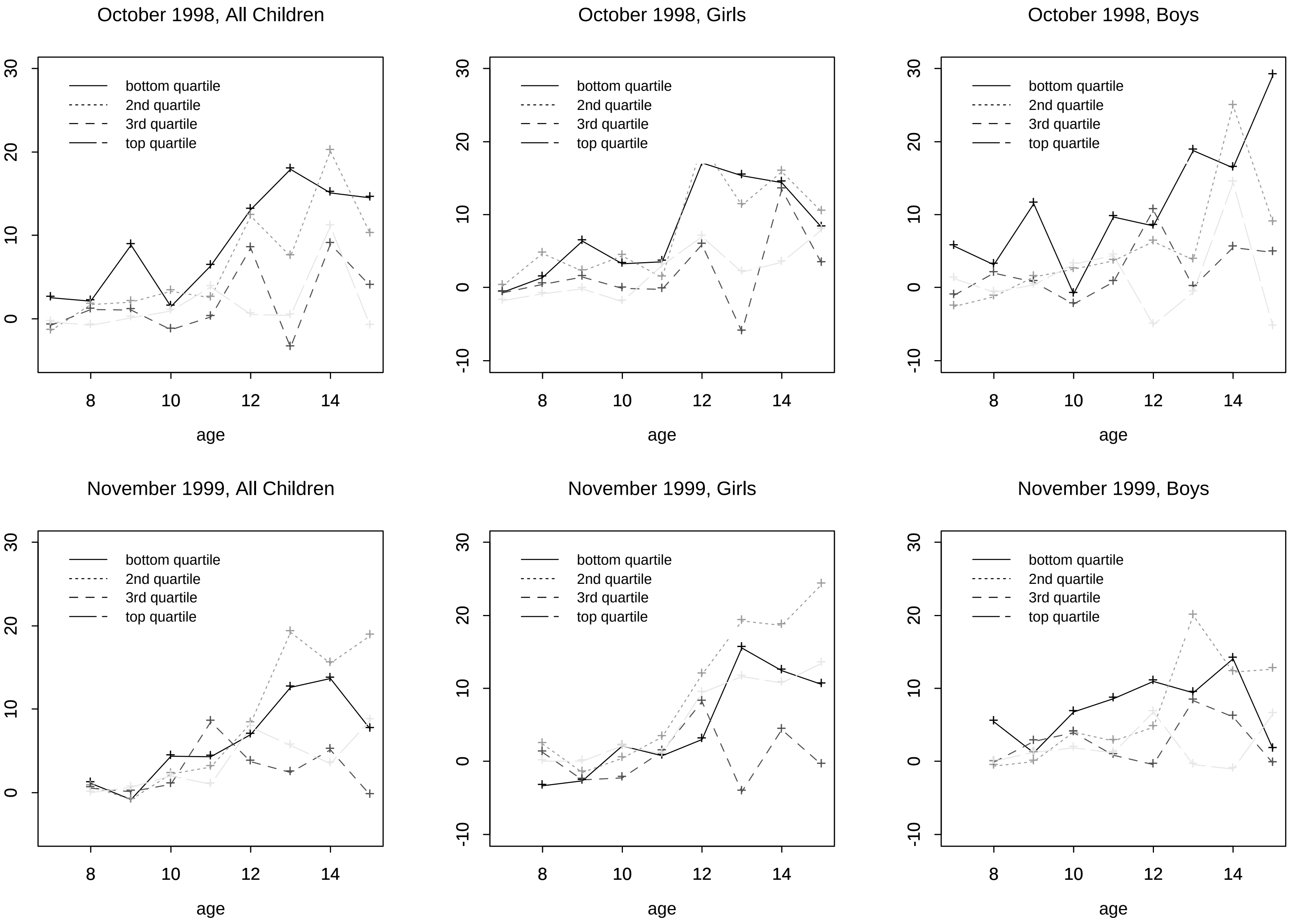
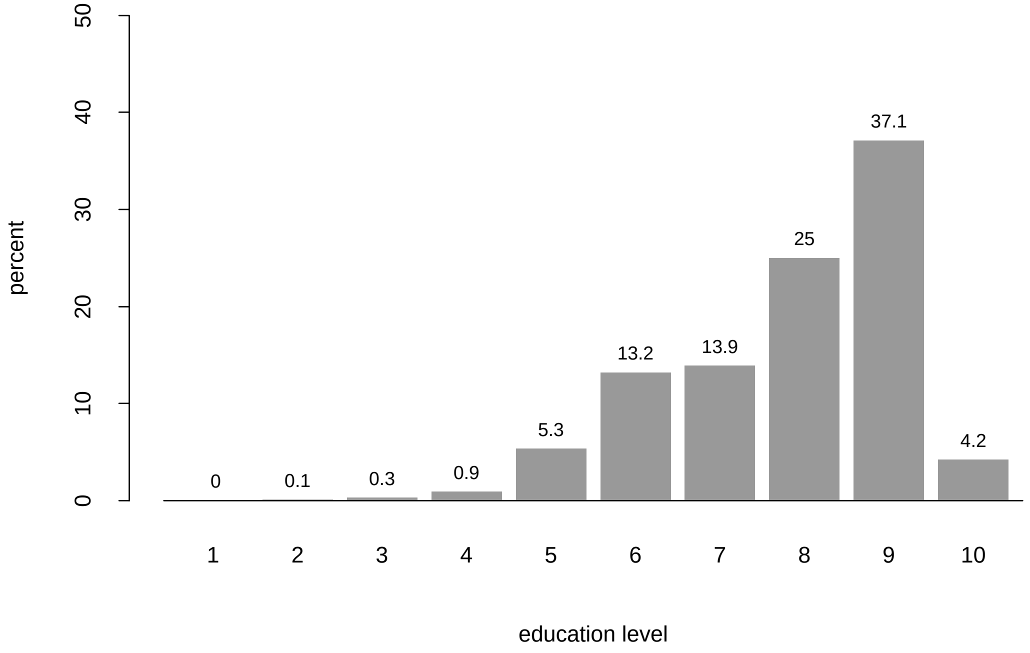


Figure 5: Average Treatment Impacts on Percentage Enrolled by Prob of Enrollment

Figure 6: Simulated Effects of Treatment on the Educational Attainment Distribution at Age 14

Simulated Distribution With Treatment



Simulated Distribution Without Treatment

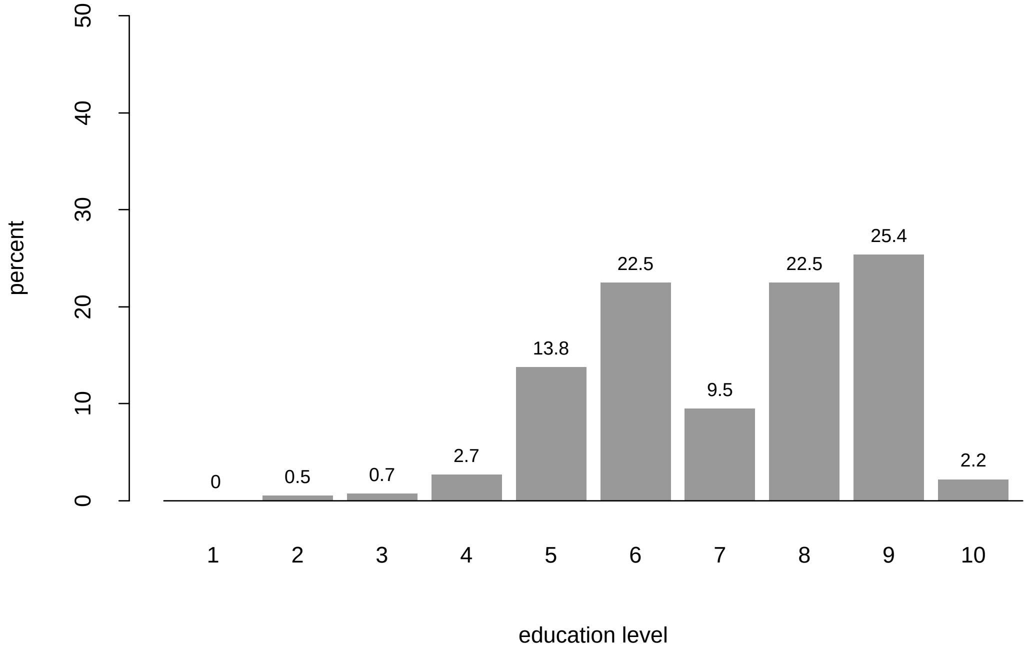


Table 1
Monthly Transfers for School Attendance under the PROGRESA Program

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) and Skoufias and Parker (2001). These grants correspond to the 1998-99 school year. In addition to these transfers, households receive a grant for school materials in the amount of 45 pesos per year for each child in primary school and 170 pesos per year for each child in secondary school. The maximum grant a household can receive is 695 pesos (Jan-June, 1999). The grant levels and grant maximums increase over time in nominal terms.

Table 2
Percentages Working for Pay and Average Monthly Earnings for Control Group Children by Age and Sex

age	**Percentage working for pay**		**Percentage of children who attend school among those who work for pay**		**Average Monthly Earnings in Pesos for Children who Work for Pay (# of observations in parentheses)**	
	Females	**Males**	**Females**	**Males**	**Females**	**Males**
6	0	0	*	*	*	*
7	0	0	*	*	*	*
8	1.15	1.61	100	100	130 (3)	1120 (1)
9	1.00	2.72	100	90	20 (1)	416 (3)
10	1.09	1.58	80	83	340 (2)	565 (4)
11	1.69	4.26	67	72	700 (2)	398 (12)
12	3.66	9.43	62	62	511 (10)	628 (28)
13	4.50	14.63	14	23	566 (15)	535 (43)
14	7.96	23.91	5	22	631 (27)	591 (78)
15	15.61	41.26	5	15	596 (25)	677 (128)
16	13.22	48.65	3	11	526 (25)	671 (112)

Table 3a
Program Impacts on Proportion Enrolled (All Children)
(T-statistics in parentheses)

Variable (all are indicator variables)	Oct. 1997	Oct. 1998	Nov. 1999
age 6	0.89	0.92	0.96
	(80.60)	(83.19)	(80.30)
age 7	0.94	0.97	0.97
	(85.16)	(90.87)	(89.92)
age 8	0.94	0.96	0.98
	(84.71)	(91.37)	(95.45)
age 9	0.95	0.96	0.98
	(80.19)	(87.42)	(91.09)
age 10	0.94	0.96	0.96
	(84.14)	(86.40)	(92.84)
age 11	0.92	0.93	0.95
	(80.72)	(84.56)	(84.95)
age 12	0.83	0.81	0.86
	(71.26)	(74.63)	(82.51)
age 13	0.69	0.74	0.74
	(55.40)	(65.17)	(67.66)
age 14	0.57	0.55	0.63
	(42.89)	(45.40)	(57.25)
age 15	0.39	0.43	0.44
	(26.52)	(33.46)	(36.85)
age 16	0.31	0.29	0.32
	(16.91)	(20.52)	(25.47)
age 17	0.26	*	0.23
	(8.06)		(16.83)
age 18	0.07	*	0.16
	(0.81)		(10.39)
treatment*age 6	0.02	0.00	0.00
	(1.09)	(0.16)	(0.32)
treatment*age 7	0.00	0.00	0.01
	(0.19)	(0.27)	(0.73)
treatment*age 8	0.01	0.01	0.01
	(0.64)	(0.55)	(0.62)
treatment*age 9	0.00	0.02	0.00
	(0.18)	(1.42)	(0.03)
treatment*age 10	0.00	0.0	0.02
	(0.27)	(0.35)	(1.52)
treatment*age 11	0.01	0.03	0.02
	(0.59)	(2.08)	(1.37)
treatment*age 12	-0.01	0.09	0.06
	(-0.52)	(6.27)	(4.48)
treatment*age 13	0.03	0.07	0.10
	(1.66)	(4.60)	(7.54)
treatment*age 14	-0.01	0.16	0.09
	(-0.78)	(10.68)	(6.67)
treatment*age 15	0.04	0.05	0.09
	(2.21)	(2.94)	(6.10)
treatment*age 16	0.02	0.06	0.07
	(0.98)	(3.10)	(4.40)
treatment*age 17	-0.01	*	0.04
	(-0.12)		(2.17)
treatment*age 18	0.04	*	0.01
	(0.36)		(0.35)
p-value from chi-square test that impacts are 0 for all ages	0.5652	<0.0001	<0.0001
p-value from chi-square test that impacts are 0 for ages 12 and older	0.3456	<0.0001	<0.0001

Table 3b
Program Impacts on Proportion Enrolled (Girls)
(T-statistics in parentheses)

Variable (all are indicator variables)	Oct. 1997	Oct. 1998	Nov. 1999
age 6	0.89	0.92	0.96
	(57.95)	(58.05)	(57.22)
age 7	0.94	0.97	0.96
	(58.21)	(64.49)	(62.91)
age 8	0.94	0.96	0.98
	(60.12)	(61.81)	(67.05)
age 9	0.95	0.96	0.99
	(56.99)	(61.80)	(65.68)
age 10	0.93	0.96	0.97
	(58.19)	(60.28)	(66.38)
age 11	0.92	0.94	0.95
	(56.96)	(59.54)	(59.26)
age 12	0.81	0.76	0.83
	(47.19)	(49.61)	(56.78)
age 13	0.65	0.69	0.71
	(36.60)	(41.76)	(46.69)
age 14	0.52	0.52	0.60
	(26.62)	(29.57)	(37.17)
age 15	0.34	0.36	0.42
	(16.23)	(18.89)	(24.36)
age 16	0.23	0.27	0.28
	(8.25)	(12.74)	(14.57)
age 17	0.26	*	0.24
	(5.54)		(11.76)
age 18	0	*	0.13
	(0.00)		(5.84)
treatment*age 6	0.01	0.00	0.00
	(0.35)	(0.17)	(0.14)
treatment*age 7	0.01	-0.01	0.01
	(0.62)	(-0.34)	(0.69)
treatment*age 8	0.01	0.01	0.01
	(0.41)	(0.43)	(0.54)
treatment*age 9	0.02	0.02	-0.01
	(0.75)	(0.75)	(-0.40)
treatment*age 10	0.02	0.01	0.00
	(1.16)	(0.33)	(0.13)
treatment*age 11	0.00	0.02	0.01
	(0.03)	(0.83)	(0.62)
treatment*age 12	-0.04	0.11	0.07
	(-1.73)	(5.79)	(3.88)
treatment*age 13	0.02	0.08	0.11
	(1.04)	(3.52)	(5.66)
treatment*age 14	-0.03	0.14	0.12
	(-1.29)	(6.43)	(5.99)
treatment*age 15	0.02	0.06	0.11
	(0.93)	(2.24)	(4.94)
treatment*age 16	0.05	0.08	0.09
	(1.61)	(2.92)	(3.84)
treatment*age 17	-0.06	*	0.03
	(-1.01)		(1.06)
treatment*age 18	0.12	*	0.04
	(0.73)		(1.32)
p-value from chi-square test that impacts are 0 for all ages	0.37	<0.0001	<0.0001
p-value from chi-square test that impacts are 0 for ages 12 and older	0.21	<0.0001	<0.0001

Table 3c
Program Impacts on Proportion Enrolled (Boys)
(T-statistics in parentheses)

Variable (all are indicator variables)	Oct. 1997	Oct. 1998	Nov. 1999
age 6	0.90	0.91	0.95
	(56.33)	(59.90)	(56.39)
age 7	0.94	0.96	0.97
	(62.43)	(64.32)	(64.30)
age 8	0.95	0.97	0.98
	(60.08)	(67.56)	(68.00)
age 9	0.96	0.96	0.98
	(56.72)	(62.12)	(63.04)
age 10	0.95	0.95	0.95
	(61.13)	(62.22)	(64.98)
age 11	0.92	0.93	0.95
	(57.52)	(60.33)	(60.92)
age 12	0.85	0.85	0.88
	(53.74)	(56.26)	(59.97)
age 13	0.73	0.79	0.76
	(41.96)	(50.64)	(49.06)
age 14	0.61	0.56	0.66
	(33.99)	(34.75)	(43.67)
age 15	0.42	0.50	0.46
	(21.25)	(28.44)	(27.72)
age 16	0.38	0.31	0.36
	(15.42)	(16.28)	(21.19)
age 17	0.27	*	0.23
	(5.89)		(12.05)
age 18	0.14	*	0.18
	(1.15)		(8.76)
treatment*age 6	0.02	0.00	0.01
	(1.16)	(0.05)	(0.32)
treatment*age 7	-0.01	0.01	0.01
	(-0.36)	(0.74)	(0.34)
treatment*age 8	0.01	0.01	0.01
	(0.36)	(0.38)	(0.34)
treatment*age 9	-0.01	0.02	0.01
	(-0.50)	(1.25)	(0.46)
treatment*age 10	-0.02	0.00	0.04
	(-0.76)	(0.16)	(2.00)
treatment*age 11	0.02	0.04	0.03
	(0.82)	(2.10)	(1.31)
treatment*age 12	0.02	0.06	0.04
	(0.94)	(3.05)	(2.36)
treatment*age 13	0.02	0.06	0.10
	(1.09)	(2.87)	(5.00)
treatment*age 14	-0.00	0.18	0.07
	(-0.26)	(8.66)	(3.55)
treatment*age 15	0.05	0.03	0.08
	(2.01)	(1.43)	(3.69)
treatment*age 16	0.00	0.03	0.05
	(0.03)	(1.46)	(2.32)
treatment*age 17	0.04	*	0.05
	(0.61)		(1.97)
treatment*age 18	-0.04	*	-0.02
	(-0.26)		(-0.75)
p-value from chi-square test that impacts are 0 for all ages	0.3023	<0.0001	<0.0001
p-value from chi-square test that impacts are 0 for ages 12 and older	0.5464	<0.0001	<0.0001

Table 4
Overall Average Effect of Program on Probability of Repeating a Grade, Dropping Out, and Reentering School
(standard error in parentheses)

Age (in 1997)	**Prob Repeating Among Those Enrolled in School**			**Prob Dropout Among Those Enrolled in School**			**Prob Reenter Among Those Dropped Out of School**		
	T	C	Diff	T	C	Diff	T	C	Diff
6	36	41	-4.59	0.8	1.6	-0.8	*	*	*
			(2.45)			(0.56)			
7	27	34	-7.1	1.0	1.0	0.0	100.0	100.0	0.0
			(2.17)			(0.47)			(0.00)
8	27	32	-5.4	0.3	0.7	-0.4	100.0	96.0	0.0
			(2.15)			(0.36)			(5.81)
9	24	30	-6.5	1.0	1.4	-0.4	97.2	94.7	2.5
			(2.16)			(0.55)			(5.81)
10	24	25	-0.8	1.6	2.9	-1.4	94.4	87.5	6.9
			(2.01)			(0.73)			(7.76)
11	20	25	-5.0	6.3	12.2	-5.9	65.5	45.8	19.7
			(2.00)			(1.46)			(12.02)
12	30	34	-3.7	10.4	16.8	-6.4	44.5	29.7	14.8
			(2.31)			(1.85)			(6.65)
13	35	40	-5.1	12.2	22.7	-10.5	34.1	16.9	17.2
			(2.56)			(2.41)			(4.33)
14	49	47	1.9	23.3	34.9	-11.6	16.9	15.5	1.33
			(2.75)			(3.28)			(3.23)
15	58	62	-4.1	31.3	37.7	-6.4	14.2	10.8	3.4
			(2.92)			(4.43)			(2.62)

Table 5(a)
Eligible Treatment Transition Matrix
Age 6 -> Age 7

	Grade (G)		
	2	**1**	**NE**
P(3 | G)	0.14	0.06	0.03
P(2 | G)	0.86	0.68	0.40
P(1 | G)	...	0.25	0.37
P(Drop | G)	...	0.01	...
P(NE | NE)	...	...	0.21
No. obs	221	824	115
P(G)	0.19	0.71	0.10

Eligible Control Transition Matrix
Age 6 -> Age 7

	Grade (G)		
	2	**1**	**NE**
P(3 | G)	0.13	0.08	0.03
P(2 | G)	0.85	0.57	0.39
P(1 | G)	...	0.33	0.41
P(Drop | G)	0.02	0.02	...
P(NE | NE)	...	...	0.16
No. obs	129	499	87.0
P(G)	0.18	0.70	0.12

Treatment-Control Differences
Age 6 -> Age 7

	Grade (G)		
	2	**1**	**NE**
P(3 | G)	0.00	-.02	-.01
P(2 | G)	0.01	0.11	0.01
P(1 | G)	...	-.08	-.05
P(Drop | G)	-.02	-.01	...
P(NE | NE)	...	...	0.05
No. obs	350	1323	202
P(G)	0.19	0.71	0.11
p-value	0.18	0.00	0.79

Table 5(b)
Eligible Treatment Transition Matrix
Age 7 -> Age 8

	Grade (G)				
	3	**2**	**1**	**Drop**	**NE**
P(4 | G)	0.28	0.11	0.05	...	0.12
P(3 | G)	0.71	0.66	0.21	0.60	0.20
P(2 | G)	...	0.23	0.62	0.40	0.37
P(1 | G)	...	...	0.10	...	0.17
P(Drop | G)	0.01	0.00	0.03	...	...
P(NE | NE)	...	...	...	...	0.14
No. obs	128	775	252	10.0	65.0
P(G)	0.10	0.63	0.20	0.01	0.05

Eligible Control Transition Matrix
Age 7 -> Age 8

	Grade (G)				
	3	**2**	**1**	**Drop**	**NE**
P(4 | G)	0.17	0.10	0.05	1.00	0.09
P(3 | G)	0.83	0.59	0.22	...	0.19
P(2 | G)	...	0.30	0.58	...	0.25
P(1 | G)	...	...	0.14	...	0.15
P(Drop | G)	...	0.01	0.02	...	...
P(NE | NE)	...	...	...	...	0.32
No. obs	70.0	468	165	1.00	53.0
P(G)	0.09	0.62	0.22	0.00	0.07

Treatment-Control Differences
Age 7 -> Age 8

	Grade (G)				
	3	**2**	**1**	**Drop**	**NE**
P(4 | G)	0.11	0.01	-.00	-1.0	0.03
P(3 | G)	-.12	0.07	-.01	0.60	0.01
P(2 | G)	...	-.07	0.04	0.40	0.12
P(1 | G)	...	...	-.04	...	0.02
P(Drop | G)	0.01	-.00	0.01	...	...
P(NE | NE)	...	...	...	...	-.18
No. obs	198	1243	417	11.0	118
P(G)	0.10	0.63	0.21	0.01	0.06
p-value	0.16	0.03	0.71	0.00	0.19

Table 5(c)
Eligible Treatment Transition Matrix
Age 8 -> Age 9

	Grade (G)					
	4	**3**	**2**	**1**	**Drop**	**NE**
P(5 | G)	0.19	0.06	0.03	...	...	...
P(4 | G)	0.81	0.70	0.24	0.26	0.77	0.12
P(3 | G)	...	0.23	0.54	0.23	0.08	0.14
P(2 | G)	...	...	0.19	0.46	0.15	0.21
P(1 | G)	...	...	...	0.05	...	0.17
P(Drop | G)	...	0.00	0.00	...	...	...
P(NE | NE)	...	...	...	...	...	0.36
No. obs	110	618	435	65.0	13.0	42.0
P(G)	0.09	0.48	0.34	0.05	0.03	.

Eligible Control Transition Matrix
Age 8 -> Age 9

	Grade (G)					
	4	**3**	**2**	**1**	**Drop**	**NE**
P(5 | G)	0.13	0.07	0.03	...	...	...
P(4 | G)	0.88	0.62	0.16	0.28	0.43	0.06
P(3 | G)	...	0.30	0.60	0.17	0.43	0.31
P(2 | G)	...	...	0.21	0.37	...	0.03
P(1 | G)	...	...	...	0.15	...	0.11
P(Drop | G)	...	0.00	0.01	0.02	0.14	...
P(NE | NE)	...	...	...	...	...	0.49
No. obs	64.0	328	257	46.0	7.00	35.0
P(G)	0.09	0.45	0.35	0.06	0.05	.

Treatment-Control Differences
Age 8 -> Age 9

	Grade (G)					
	4	**3**	**2**	**1**	**Drop**	**NE**
P(5 | G)	0.07	-.01	0.00	...	...	...
P(4 | G)	-.07	0.08	0.08	-.02	0.34	0.06
P(3 | G)	...	-.07	-.06	0.06	-.35	-.17
P(2 | G)	...	...	-.02	0.09	0.15	0.19
P(1 | G)	...	...	...	-.11	...	0.05
P(Drop | G)	...	0.00	-.01	-.02	-.14	...
P(NE | NE)	...	...	...	...	...	-.13
No. obs	174	946	692	111	20.0	77.0
P(G)	0.09	0.47	0.34	0.05	0.04	.
p-value	0.26	0.08	0.07	0.22	0.09	0.05

Table 5(d)

Eligible Treatment Transition Matrix

Age 9 -> Age 10

	Grade (G)						
	5	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(6 | G)	0.26	0.06	0.02	...	...	0.05	...
P(5 | G)	0.74	0.72	0.19	0.15	...	0.53	...
P(4 | G)	...	0.22	0.60	0.24	0.47	0.21	0.26
P(3 | G)	...	...	0.19	0.44	0.20	0.16	0.15
P(2 | G)	...	...	...	0.14	0.20	...	0.15
P(1 | G)	...	...	...	...	0.13	...	0.07
P(Drop | G)	...	0.01	0.01	0.03	...	0.05	...
P(NE | NE)	...	...	...	...	...	...	0.37
No. obs	72.0	524	372	177	15.0	19.0	27.0
P(G)	0.06	0.43	0.31	0.15	0.01	0.02	0.02

Eligible Control Transition Matrix

Age 9 -> Age 10

	Grade (G)						
	5	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(6 | G)	0.17	0.06	0.01	...	...	...	...
P(5 | G)	0.83	0.64	0.17	0.10	...	0.25	...
P(4 | G)	...	0.30	0.59	0.17	0.12	0.50	0.22
P(3 | G)	...	...	0.21	0.51	0.18	0.13	0.11
P(2 | G)	...	...	...	0.20	0.47	...	0.22
P(1 | G)	...	...	...	...	0.12	...	0.06
P(Drop | G)	...	0.00	0.02	0.02	0.12	0.13	...
P(NE | NE)	...	...	...	...	...	...	0.39
No. obs	48.0	266	223	92.0	17.0	8.00	18.0
P(G)	0.07	0.40	0.33	0.14	0.03	0.01	0.03

Treatment-Control Differences

Age 9 -> Age 10

	Grade (G)						
	5	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(6 | G)	0.10	-.01	0.01	...	...	0.05	...
P(5 | G)	-.10	0.08	0.02	0.05	...	0.28	...
P(4 | G)	...	-.08	0.01	0.06	0.35	-.29	0.04
P(3 | G)	...	...	-.03	-.07	0.02	0.03	0.04
P(2 | G)	...	...	...	-.05	-.27	...	-.07
P(1 | G)	...	...	...	...	0.02	...	0.02
P(Drop | G)	...	0.00	-.01	0.01	-.12	-.07	...
P(NE | NE)	...	...	...	...	...	...	-.02
No. obs	120	790	595	269	32.0	27.0	45.0
p-value	0.21	0.08	0.50	0.36	0.14	0.49	0.97

Table 5(e)
Eligible Treatment Transition Matrix
Age 10 -> Age 11

	Grade (G)							
	6	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(7 | G)	0.31	0.04	0.01	...	...	...	0.09	...
P(6 | G)	0.67	0.75	0.20	0.15	...	...	0.23	...
P(5 | G)	...	0.20	0.62	0.18	0.15	...	0.23	...
P(4 | G)	...	...	0.16	0.44	0.20	0.50	0.14	0.11
P(3 | G)	...	...	...	0.20	0.45	0.25	0.14	0.18
P(2 | G)	...	...	...	...	0.18	0.25	0.09	0.18
P(1 | G)	...	...	...	...	...	...	...	0.04
P(Drop | G)	0.02	0.01	0.01	0.03	0.02	...	0.09	...
P(NE | NE)	...	...	...	...	...	...	...	0.50
No. obs	90.0	438	392	172	55.0	4.00	22.0	28.0
P(G)	0.07	0.36	0.33	0.14	0.05	0.00	0.02	0.02

Eligible Control Transition Matrix
Age 10 -> Age 11

	Grade (G)							
	6	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(7 | G)	0.32	0.10	0.02	...	...	...	0.06	...
P(6 | G)	0.64	0.68	0.15	0.08	...	...	0.18	...
P(5 | G)	...	0.19	0.62	0.19	0.09	...	0.35	...
P(4 | G)	...	...	0.20	0.49	0.11	0.67	0.12	0.04
P(3 | G)	...	...	...	0.20	0.51	...	0.12	0.17
P(2 | G)	...	...	...	...	0.19	0.33	...	0.09
P(1 | G)	...	...	...	...	...	...	...	...
P(Drop | G)	0.04	0.03	0.01	0.05	0.11	...	0.18	...
P(NE | NE)	...	...	...	...	...	...	...	0.70
No. obs	50.0	280	230	102	47.0	3.00	17.0	23.0
P(G)	0.07	0.37	0.31	0.14	0.06	0.00	0.02	0.03

Treatment-Control Differences
Age 10 -> Age 11

	Grade (G)							
	6	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(7 | G)	-.01	-.06	-.00	...	...	...	0.03	...
P(6 | G)	0.03	0.07	0.04	0.07	...	...	0.05	...
P(5 | G)	...	0.01	0.00	-.01	0.06	...	-.13	...
P(4 | G)	...	...	-.04	-.05	0.09	-.17	0.02	0.06
P(3 | G)	...	...	...	0.00	-.06	0.25	0.02	0.00
P(2 | G)	...	...	...	...	-.01	-.08	0.09	0.09
P(1 | G)	...	...	...	...	...	...	...	0.04
P(Drop | G)	-.02	-.01	0.00	-.02	-.09	...	-.09	...
P(NE | NE)	...	...	...	...	...	...	...	-.20
No. obs	140	718	622	274	102	7.00	39.0	51.0
p-value	0.82	0.00	0.51	0.44	0.22	0.65	0.82	0.55

Table 5(f)
Eligible Treatment Transition Matrix
Age 11 -> Age 12

	Grade (G)								
	7	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(8 | G)	0.24	0.05	0.01	...	...	...	...	0.09	...
P(7 | G)	0.72	0.66	0.21	0.13	...	...	...	0.26	...
P(6 | G)	...	0.15	0.65	0.19	0.13	...	...	0.04	...
P(5 | G)	...	...	0.12	0.47	0.17	0.15	...	0.04	...
P(4 | G)	...	...	...	0.18	0.48	0.30	...	0.11	0.05
P(3 | G)	...	...	...	...	0.18	0.30	1.00	...	0.05
P(2 | G)	...	...	...	...	...	0.25	...	0.04	0.33
P(1 | G)	...	...	...	...	...	...	...	...	...
P(Drop | G)	0.04	0.14	0.01	0.03	0.04	...	...	0.41	...
P(NE | NE)	...	...	...	...	...	...	...	...	0.57
No. obs	50.0	369	370	198	77.0	20.0	1.00	46.0	21.0
P(G)	0.04	0.32	0.32	0.17	0.07	0.02	0.00	0.04	0.02

Eligible Control Transition Matrix
Age 11 -> Age 12

	Grade (G)								
	7	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(8 | G)	0.17	0.04	0.00	...	...	...	...	...	...
P(7 | G)	0.62	0.54	0.15	0.07	...	...	...	0.14	...
P(6 | G)	...	0.19	0.63	0.18	0.09	...	...	0.14	...
P(5 | G)	...	...	0.19	0.51	0.20	...	...	0.09	...
P(4 | G)	...	...	...	0.18	0.50	0.33	0.33	0.05	...
P(3 | G)	...	...	...	...	0.17	0.33	0.33	...	0.13
P(2 | G)	...	...	...	...	...	0.11	0.33	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...
P(Drop | G)	0.21	0.23	0.03	0.06	0.04	0.22	...	0.59	...
P(NE | NE)	...	...	...	...	...	...	...	...	0.87
No. obs	29.0	246	216	124	46.0	9.00	3.00	22.0	15.0
P(G)	0.04	0.35	0.30	0.17	0.06	0.01	0.00	0.03	0.02

Treatment-Control Differences
Age 11 -> Age 12

	Grade (G)								
	7	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(8 | G)	0.07	0.01	0.01	...	...	...	...	0.09	...
P(7 | G)	0.10	0.12	0.06	0.06	...	...	...	0.12	...
P(6 | G)	...	-.04	0.02	0.01	0.04	...	...	-.09	...
P(5 | G)	...	...	-.07	-.03	-.03	0.15	...	-.05	...
P(4 | G)	...	...	...	-.00	-.02	-.03	-.33	0.06	0.05
P(3 | G)	...	...	...	...	0.01	-.03	0.67	...	-.09
P(2 | G)	...	...	...	...	...	0.14	-.33	0.04	0.33
P(1 | G)	...	...	...	...	...	...	...	...	...
P(Drop | G)	-.17	-.09	-.02	-.03	-.00	-.22	...	-.18	...
P(NE | NE)	...	...	...	...	...	...	...	...	-.30
No. obs	79.0	615	586	322	123	29.0	4.00	68.0	36.0
p-value	0.06	0.01	0.04	0.32	0.96	0.17	0.51	0.25	0.06

Table 5(g)

Eligible Treatment Transition Matrix

Age 12 -> Age 13

	Grade (G)									
	8	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(9 | G)	0.25	0.06	0.01	...	...	...	...	...	0.01	...
P(8 | G)	0.75	0.62	0.12	0.06	...	...	...	...	0.09	...
P(7 | G)	...	0.24	0.58	0.16	0.10	...	...	...	0.25	...
P(6 | G)	...	...	0.11	0.58	0.17	0.15	...	...	0.04	...
P(5 | G)	...	...	...	0.16	0.51	0.15	...	...	0.01	...
P(4 | G)	...	...	...	...	0.17	0.44	0.20	...	0.02	0.13
P(3 | G)	...	...	...	...	...	0.18	0.40	1.00	0.01	...
P(2 | G)	...	...	...	...	...	...	0.13	...	0.01	0.09
P(1 | G)	...	...	...	...	...	...	...	...	...	...
P(Drop | G)	...	0.08	0.19	0.03	0.03	0.09	0.27	...	0.57	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	0.78
No. obs	36.0	234	312	179	86.0	34.0	15.0	1.00	150	23.0
P(G)	0.03	0.22	0.29	0.17	0.08	0.03	0.01	0.00	0.14	0.02

Eligible Control Transition Matrix

Age 12 -> Age 13

	Grade (G)									
	8	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(9 | G)	0.26	0.06	...	...	...	...	...	...	...	...
P(8 | G)	0.74	0.65	0.11	0.07	...	...	...	...	0.03	...
P(7 | G)	...	0.16	0.46	0.14	0.11	...	...	...	0.12	...
P(6 | G)	...	...	0.17	0.50	0.10	0.04	...	...	0.01	...
P(5 | G)	...	...	...	0.18	0.39	0.15	...	...	0.01	...
P(4 | G)	...	...	...	...	0.33	0.35	0.10	...	0.03	0.10
P(3 | G)	...	...	...	...	...	0.19	0.30	...	0.04	0.05
P(2 | G)	...	...	...	...	...	...	0.30	...	...	0.05
P(1 | G)	...	...	...	...	...	...	...	...	...	0.05
P(Drop | G)	...	0.14	0.26	0.12	0.07	0.27	0.30	1.00	0.75	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	0.75
No. obs	34.0	139	190	121	61.0	26.0	10.0	1.00	69.0	20.0
P(G)	0.05	0.21	0.28	0.18	0.09	0.04	0.01	0.00	0.10	0.03

Treatment-Control Differences

Age 12 -> Age 13

	Grade (G)									
	8	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(9 | G)	-.01	0.00	0.01	...	...	...	...	...	0.01	...
P(8 | G)	0.01	-.03	0.01	-.00	...	...	...	...	0.06	...
P(7 | G)	...	0.09	0.11	0.02	-.01	...	...	...	0.13	...
P(6 | G)	...	...	-.06	0.09	0.08	0.11	...	...	0.03	...
P(5 | G)	...	...	...	-.02	0.12	-.01	...	...	-.00	...
P(4 | G)	...	...	...	...	-.15	0.10	0.10	...	-.01	0.03
P(3 | G)	...	...	...	...	...	-.02	0.10	1.00	-.04	-.05
P(2 | G)	...	...	...	...	...	...	-.17	...	0.01	0.04
P(1 | G)	...	...	...	...	...	...	...	...	...	-.05
P(Drop | G)	...	-.06	-.07	-.08	-.03	-.18	-.03	-1.0	-.18	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	0.03
No. obs	70.0	373	502	300	147	60.0	25.0	2.00	219	43.0
p-value	0.89	0.10	0.02	0.07	0.15	0.29	0.71	0.16	0.08	0.63

Table 5(h)
Eligible Treatment Transition Matrix
Age 13 -> Age 14

	Grade (G)										
	9	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(10 | G)	0.35	0.06	0.02	...	...	...	...	...	...	0.01	...
P(9 | G)	0.59	0.77	0.16	0.08	...	...	...	...	...	0.05	...
P(8 | G)	...	0.14	0.54	0.14	0.10	...	...	...	...	0.04	...
P(7 | G)	...	...	0.14	0.47	0.07	0.06	...	...	...	0.20	...
P(6 | G)	...	...	...	0.11	0.60	0.19	0.25	...	...	0.01	...
P(5 | G)	...	...	...	...	0.12	0.42	0.20	0.20	...	0.02	...
P(4 | G)	...	...	...	...	...	0.17	0.40	0.20	...	...	0.11
P(3 | G)	...	...	...	...	...	...	0.05	0.20	...	0.00	0.04
P(2 | G)	...	...	...	...	...	...	...	0.40	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...
P(Drop | G)	0.06	0.02	0.14	0.20	0.12	0.17	0.10	...	...	0.67	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	0.86
No. obs	34.0	146	213	168	84.0	36.0	20.0	5.00	...	228	28.0
P(G)	0.04	0.15	0.22	0.17	0.09	0.04	0.02	0.01	0.00	0.24	0.03

Eligible Control Transition Matrix
Age 13 -> Age 14

	Grade (G)										
	9	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(10 | G)	0.29	0.05	...	...	...	...	...	...	...	...	...
P(9 | G)	0.59	0.69	0.13	0.05	...	...	...	...	...	0.01	...
P(8 | G)	...	0.20	0.56	0.08	0.07	...	...	...	...	0.05	...
P(7 | G)	...	...	0.08	0.27	0.07	0.13	...	...	...	0.05	...
P(6 | G)	...	...	...	0.11	0.51	0.22	0.18	...	...	0.04	...
P(5 | G)	...	...	...	...	0.21	0.41	0.36	...	...	0.01	...
P(4 | G)	...	...	...	...	...	...	0.27	0.33	...	0.01	0.10
P(3 | G)	...	...	...	...	...	...	0.09	...	...	...	...
P(2 | G)	...	...	...	...	...	...	...	0.67	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...
P(Drop | G)	0.12	0.05	0.24	0.49	0.14	0.25	0.09	...	...	0.84	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	0.90
No. obs	17.0	95.0	102	92.0	57.0	32.0	11.0	3.00	...	153	10.0
P(G)	0.03	0.17	0.18	0.16	0.10	0.06	0.02	0.01	0.00	0.27	0.02

Treatment-Control Differences
Age 13 -> Age 14

	Grade (G)										
	9	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(10 | G)	0.06	0.01	0.02	...	...	...	...	...	...	0.01	...
P(9 | G)	...	0.08	0.03	0.03	...	...	...	...	...	0.04	...
P(8 | G)	...	-.06	-.02	0.06	0.03	...	...	...	...	-.01	...
P(7 | G)	...	...	0.06	0.20	0.00	-.07	...	...	...	0.15	...
P(6 | G)	...	...	...	0.00	0.09	-.02	0.07	...	...	-.03	...
P(5 | G)	...	...	...	...	-.09	0.01	-.16	0.20	...	0.02	...
P(4 | G)	...	...	...	...	...	0.17	0.13	-.13	...	-.01	0.01
P(3 | G)	...	...	...	...	...	...	-.04	0.20	...	0.00	0.04
P(2 | G)	...	...	...	...	...	...	...	-.27	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...
P(Drop | G)	-.06	-.03	-.09	-.29	-.02	-.08	0.01	...	...	-.17	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	-.04
No. obs	51.0	241	315	260	141	68.0	31.0	8.00	...	381	38.0
p-value	0.73	0.33	0.08	0.00	0.62	0.14	0.84	0.66	.	0.00	0.83

Table 5(i)
Eligible Treatment Transition Matrix
Age 14 -> Age 15

	Grade (G)											
	10	**9**	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(11 | G)	...	0.03	...	...	...	...	...	...	...	...	...	...
P(10 | G)	0.53	0.28	0.13	0.04	...	...	...	...	...	...	0.03	...
P(9 | G)	...	0.24	0.68	0.22	0.08	...	...	...	...	...	0.03	...
P(8 | G)	...	...	0.18	0.43	0.07	0.08	...	...	...	...	0.02	...
P(7 | G)	...	...	...	0.13	0.40	0.08	0.25	...	...	...	0.08	...
P(6 | G)	...	...	...	...	0.11	0.58	0.25	...	...	...	0.01	...
P(5 | G)	...	...	...	...	...	0.15	0.06	0.33	...	...	0.00	...
P(4 | G)	...	...	...	...	...	...	0.13	0.33	...	...	...	0.03
P(3 | G)	...	...	...	...	...	...	...	...	...	...	0.00	...
P(2 | G)	...	...	...	...	...	...	...	...	...	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...	...
P(Drop | G)	0.47	0.46	0.02	0.17	0.34	0.12	0.31	0.33	...	...	0.83	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	...	0.97
No. obs	15.0	109	130	113	83.0	26.0	16.0	6.00	...	...	343	38.0
P(G)	0.02	0.13	0.15	0.13	0.10	0.03	0.02	0.01	0.00	0.00	0.40	0.04

Eligible Control Transition Matrix
Age 14 -> Age 15

	Grade (G)											
	10	**9**	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(11 | G)	0.27	0.06	...	...	...	...	...	...	...	...	...	...
P(10 | G)	0.36	0.23	0.08	0.02	...	...	...	...	...	...	0.02	...
P(9 | G)	...	0.25	0.71	0.18	0.09	...	...	...	...	...	0.02	...
P(8 | G)	...	...	0.17	0.38	0.08	0.04	...	...	...	...	0.02	...
P(7 | G)	...	...	...	0.06	0.25	0.12	...	...	...	...	0.06	...
P(6 | G)	...	...	...	...	0.08	0.38	0.08	...	...	...	0.02	...
P(5 | G)	...	...	...	...	...	0.08	0.23	...	...	...	...	...
P(4 | G)	...	...	...	...	...	...	...	...	...	...	...	0.05
P(3 | G)	...	...	...	...	...	...	...	0.50	...	...	...	...
P(2 | G)	...	...	...	...	...	...	...	...	...	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...	0.05
P(Drop | G)	0.36	0.46	0.05	0.35	0.51	0.38	0.69	0.50	1.00	...	0.85	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	...	0.90
No. obs	11.0	69.0	78.0	65.0	53.0	26.0	13.0	2.00	1.00	...	204	20.0
P(G)	0.02	0.13	0.15	0.12	0.10	0.05	0.02	0.00	0.00	0.00	0.38	0.04

Treatment-Control Differences
Age 14 -> Age 15

	Grade (G)											
	10	**9**	**8**	**7**	**6**	**5**	**4**	**3**	**2**	**1**	**Drop**	**NE**
P(11 | G)	-.27	-.03	...	...	...	...	...	...	...	...	...	...
P(10 | G)	0.17	0.04	0.05	0.03	...	...	...	...	...	...	0.00	...
P(9 | G)	...	-.01	-.03	0.04	-.01	...	...	...	...	...	0.01	...
P(8 | G)	...	...	0.01	0.05	-.00	0.04	...	...	...	...	0.00	...
P(7 | G)	...	...	...	0.07	0.15	-.04	0.25	...	...	...	0.02	...
P(6 | G)	...	...	...	...	0.03	0.19	0.17	...	...	...	-.01	...
P(5 | G)	...	...	...	...	...	0.08	-.17	0.33	...	...	0.00	...
P(4 | G)	...	...	...	...	...	...	0.13	0.33	...	...	...	-.02
P(3 | G)	...	...	...	...	...	...	...	-.50	...	...	0.00	...
P(2 | G)	...	...	...	...	...	...	...	...	...	...	...	...
P(1 | G)	...	...	...	...	...	...	...	...	...	...	...	-.05
P(Drop | G)	0.10	-.01	-.04	-.19	-.17	-.27	-.38	-.17	-1.0	...	-.02	...
P(NE | NE)	...	...	...	...	...	...	...	...	...	...	...	0.07
No. obs	26.0	178	208	178	136	52.0	29.0	8.00	1.00	...	547	58.0
p-value	0.10	0.73	0.32	0.05	0.29	0.20	0.05	0.22	1.00	.	0.75	0.33

Table 6

P-values from Pearson Chi-Squared Tests of Equality of Schooling Transition Matrices

Hypothesis Tested	age 6	age 7	age 8	age 9	age 10	age 11	age 12	age 13	age 14
Eligible Treatment and Eligible Control Transition Matrices are Equal (Treatment has no impact)	0.0007	0.0071	0.0051	0.3027	0.2541	0.0055	0.0105	< 0.0001	0.0671
For Girls, Eligible Treatment and Eligible Control Transition Matrices are Equal (Treatment has no impact for Girls)	0.3604	0.7606	0.0421	0.2022	0.4437	0.0803	0.1763	0.0877	0.1371
For Boys, Eligible Treatment and Eligible Control Transition Matrices are Equal (Treatment has no impact for Boys)	0.0004	0.0462	0.0972	0.3812	0.3040	0.0942	0.1793	0.0073	0.4285
Non-eligible Treatment and Non-eligible Control Transition Matrices are Equal (iNo spillover effects)	0.2833	0.3682	0.2069	0.3064	0.3008	0.6452	0.6576	0.5269	0.9515
Eligible Control Matrix for Girls Equal to and Eligible Control Matrix for Boys (No gender difference in educational progression patterns)	0.1842	0.1766	0.3455	0.7161	0.3643	0.2835	0.1949	0.1278	0.5170

Table 7
Simulated Education Distribution at Age 14 for Treatment and Control Children After Exposure to Treatment for 8 Years, Age 6-14

grade	treatment %	control %	treatment cdf	control cdf
1	0.00	0.03	0.00	0.03
2	0.13	0.45	0.13	0.48
3	0.32	0.74	0.45	1.22
4	0.85	2.72	1.3	3.94
5	5.32	13.82	6.62	17.76
6	13.17	22.50	19.79	40.26
7	13.85	9.50	33.64	49.76
8	24.96	22.50	58.6	72.26
9	37.12	25.36	95.72	97.62
10	4.21	2.23	100	100

Table 8
Predicted Age 14 Average Impact After Varying Lengths of Exposure

Exposure (years)	Average Impact on Years of Education
1	0.27
2	0.40
3	0.55
4	0.57
5	0.63
6	0.65
7	0.67
8	0.68