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Demand and Supply-Side Determinants of Conditional Cash Transfer Program Effectiveness: Improving the First-Generation Programs

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ABSTRACT

The *Programa Nacional de Becas Estudiantiles* is a conditional cash transfer program designed to promote long-term human capital accumulation among Argentine youth and to reduce poverty. The *Becas* program began in the late 1990s and is currently one of the priority social programs receiving additional support and protection since the 2001 economic emergency. This study follows a 1999 cohort of students from 24 schools for five years. Propensity score matching methods are used with a comparison group of students who were excluded due to quotas to estimate the average and marginal impacts of the *Becas* program. Multilevel methods are also used to estimate the contributions of *Becas* program management and school characteristics in explaining variation between schools in student outcomes. The results show significant effects of the *Becas* program in increasing attendance, reducing grade repetition and improving students' performance. The impacts were largest for students who received the scholarship for more than one year, suggesting a dynamic incentive may have been established by linking receipt of the scholarship in subsequent years to students' academic performance in the first year of participation. The multilevel analysis suggested that efforts to strengthen institutional capacity and conditions for learning in the schools and to promote better management and communication also contributed to student performance.

INTRODUCTION

Conditional cash transfer programs are currently an important economic and social policy tool being used to address problems of poverty, inequality and human capital development in both developing and developed countries. A primary objective of conditional cash transfer programs is to provide short-term assistance to families in poverty, while at the same time promoting investments in long-term human capital development through conditions on benefit receipt. Other important goals of these programs include income redistribution and the promotion of social inclusion. The coverage of these programs is vast in some countries; for example, Brazil's *Bolsa Familia* program that began in 2003 is expected to serve over 50 million poor persons.

Early evaluations of conditional cash transfer programs have reported some positive effects. The proxy means tests that are used in many of these programs to prioritize access to benefits appear to be effective in targeting the poor, and the electronic transfers of cash are more efficient (lower transaction costs) and less distortive (fewer errors of inclusion and price distortions) than in-kind transfers and general subsidies (Rawlings, 2004). Experimental evaluations of these programs have also reported positive impacts on school enrollment rates, consumption levels, immunization and nutrition monitoring, and reductions in child labor. In a number of countries, these programs are now being expanded, and "second-generation" models (or changes to the initial programs) are being considered or implemented.[1]

One of the ways in which thinking about conditional cash transfer programs is advancing is the greater attention that is now being given to important aspects of program implementation. A primary challenge to successful program implementation has been limited institutional capacity and resources for basic program management responsibilities, including registration of beneficiaries, regular and timely benefit transfers, monitoring of compliance with program conditions, recordkeeping, and the prevention of fraud and mismanagement of program funds (Tabor, 2002). In large countries like Argentina and Brazil, programs are administered at the municipal level, with local government, school and health authorities playing key roles in carrying out these responsibilities.

This research focuses on Argentina's *Programa Nacional de Becas Estudiantiles*, a conditional cash transfer program that targets youth ages 13-19 years, who are entering their 8th and 9th years of study in public schools and are at risk of leaving school before completing their education. The primary objectives of the program are to support the retention, promotion and graduation of students from the third cycle of the *Educación General Básica* (EGB)—equivalent to the high school years (i.e., grades 9-12)—thus increasing educational attainment and promoting human capital development among Argentine youth. The *Becas* program currently provides scholarships to approximately 350,000 economically vulnerable students each year (in the form of bi-annual payments to their families), conditional on the students' school attendance and annual grade progression.

[1] The *Bolsa Familia* program in Brazil merges several "first-generation" conditional cash transfer programs in that country, including *Bolsa Escola, Bolsa Alimentacao, Agente Joven*, Peti and others.

In this study, nonexperimental analyses—propensity score matching and marginal matching estimators—are used to assess the impact of Argentina's *Becas* program on students' educational attainment and performance in school. The impact of the treatment on the treated is estimated for two different groups: *Becas* program beneficiaries compared to eligible students who received no scholarship, and *Becas* program beneficiaries who received the scholarship for only one year compared to those who received the scholarship for two or more years (the marginal program impact). In addition, the analysis goes one step further to explore school-level factors, including characteristics of the schools and the students attending them and various aspects of the *Becas* program implementation, and their role in explaining the variation in *Becas* program impacts between schools. Thus, this research aims to make an important contribution to understanding both the demand- and supply-side driven effects of a large-scale conditional cash transfer program, and also offers insights into how the implementation of this and similar interventions for promoting human capital accumulation might matter for program outcomes.

This study finds significant impacts of the *Becas* program in increasing students' attendance, reducing grade repetition and improving students' performance in school. The impacts were largest for (and largely driven by) students who received the scholarship for more than one year. The *Becas* program appeared to establish important dynamic incentive effects by linking receipt of the scholarship in subsequent years to students' academic performance in the first year of participation. In addition, student performance was higher in schools with greater institutional capacity, better conditions for learning and superior management.

In the next section, additional background information on conditional cash transfer programs is provided and findings from recent evaluations are briefly described, followed by discussion of the motivation for a focus on Argentina's human capital development initiatives and the *Becas Estudiantiles* program. The methodology for this study and the samples and data used are described next, and then empirical analyses and study findings are presented. The paper concludes with recommendations for future human capital development programs of this type.

I. CONDITIONAL CASH TRANSFER PROGRAMS AND SCHOOLING INVESTMENTS

Conditional cash transfer programs were first initiated more than a decade ago, beginning with a pilot program in Mexico, and have since been widely implemented in Latin America and other countries.[2] The conditions that typically accompany cash transfers—requiring prenatal care, infant and children's health care, nutritional education, and minimum school attendance rates for children—are the key provisions that distinguish these programs' long-term objectives from a simple short-term income transfer program. In effect, they are intended to change the behavior of recipients, beginning a cycle of investments that will permanently change the health and well-being of poor families and break the intergenerational transmission of poverty. In this regard, they bear some similarity to U.S. welfare programs (i.e., Temporary Assistance to Needy Families) that require minimum levels of participation in work and/or training activities as a condition for receiving a monthly grant.

Conditional cash transfer programs are also described as a demand-side policy intervention, one that removes constraints to human capital development by reducing out-of-pocket expenditures (for schooling, health care, etc.) and opportunity costs (e.g., of the loss of children's labor income and time spent accessing services). Rawlings (2004) submits that the use of demand-side interventions to target assistance represents a "marked departure from traditional supply-side mechanisms such as general subsidies or investments in schools, health centers and other providers of social services." At the same time, Aedo (2004) argues that even a "pure" demand-side program will create pressures for supply-side investments or interventions. And there is general agreement in the development literature that the achievement of the long-term goals of conditional cash transfer programs is predicated on adequate supply-side investments (in schools, health care facilities, etc.) (Legovini and Regalia, 2001). Similar to this study of the Becas program, Handa (2002) was able to link household survey data to information on school infrastructure in rural Mozambique, and he found that dimensions of school quality and access (e.g., reduced travel time and costs) both affected primary school enrollment rates.

There have been a number of impact evaluations—experimental and nonexperimental—conducted of conditional cash transfer programs. The Progresa program in Mexico, an education, health and nutrition program introduced in rural communities (using random assignment), is among the best known and most intensively studied. Like the Becas program in Argentina, a primary objective of Progresa was to increase youth enrollment and attendance in school and reduce their early labor force participation. Dubois et al. (2003) also modeled the incentives generated by Progresa's cash transfers to change students' behavior and improve their performance in school (measured by success or failure to pass a grade). In their formal model, the static incentive comes from the reduced opportunity cost of attending school (due to the

[2] Some examples of these programs include *Progresa* and *Oportunidades* in Mexico; *Bolsa Familia, Bolsa Escola* and *Bolsa Alimentacao* in Brazil; *Red de Protección Social* in Nicaragua; *Program de Asignación Familiar* in Honduras; *Subsidio Familiar* in Venezuela; *Apoyo Familiar* in Colombia; *Program of Advancement Through Health and Education (PATH)* in Jamaica; Ecuador's *Bono de Desarrollo Humano*; Chile's *Subsidio Unitario Familiar*, Bolivia's *Beca Futuro*; *Programa Superémonos* in Costa Rica, *Fonds de Parrainage National* in Haiti, capitation grants in Gambia and Zimbabwe and matching-grants schemes in Ghana, Mauritius, and Turkey.

transfer payment), and the opportunity to receive future transfers if one stays in school produces the dynamic incentive effect. They demonstrate that if youth can choose their level of learning effort (i.e., it is not exogenously fixed), the cash transfer will increase students' effort and performance in school, as the value of completing an additional year of school will be higher for those in the program.

In their empirical analysis, Dubois et al. acknowledge that randomization in Progresa does not eliminate the problem of dynamic selection in modeling grade transitions and performance, although *average* program impact estimates should be unbiased by dynamic selection.[3] They find strong positive effects (treatment on the treated) of Progresa on school enrollment and continuation (associated with the transfers) for both primary and secondary school students, but they find significant improvements in school performance only for primary students. Skoufias and Parker (2001) use nonexperimental methods to analyze the Progresa data (e.g., differences-in differences) and similarly find significant increases in school attendance (and large reductions in workforce participation) for Progresa beneficiaries. Behrman et al. (2001) report that the impact of Progresa on grade progression is largest for youth between the ages of 12 and 15 years, with a peak at 14 years, the approximate initial target age of the *Becas* program. Other experimental and nonexperimental evaluations of these programs have likewise reported positive impacts on school enrollment and attendance rates (e.g., in Nicaragua and Colombia—see International Food Policy Research Institute, 2002 and Rawlings, 2004), although few others have examined these programs' effects on students' performance in school.

[3] Specifically, they note that although random assignment assures that unobserved factors influencing enrollment decisions are distributed independently of observed characteristics in the first year of the program, this will not be the case in the second year due to selection of students based on first-year educational outcomes (i.e., where treatment status affects students' learning effort and performance). For example, they expect that if the program has a positive effect on the propensity of continuing schooling, it will select individuals with lower unobserved characteristics (such as unobserved ability), on average, contributing to a downward bias in the probability of academic success and school continuation in subsequent years. See also Cameron and Heckman, 1998.

II. THE BECAS ESTUDIANTILES PROGRAM IN ARGENTINA

Argentina has undergone dramatic economic and social changes over the last three decades, paralleling global trends. Economic analyses suggest that Argentina's increased economic openness has led to structural economic change, reducing labor demand in the low-skilled, labor-intensive sectors and shifting it toward higher-skilled, technical sectors (Gasparini, 2003). These changes have in turn contributed to increasing returns to education (and widening wage gaps between those with a post-high school education and lower educational levels), while reducing employment among the low-skilled. Another resulting effect has been unprecedented increases in economic inequality, exacerbated by periods of economic volatility and crisis. Between 1974 and 2002, the Gini coefficient for household per capita income increased from 34.5 in 1974 to 53.8 in 2002, and the poverty rate increased from 5 percent to over 50 percent over the same period (Lee, 2000; Gasparini, 2003).[4] Prior to these changes, Argentina had laid claim to one of the most equal distributions of income in Latin America and a large middle class.

Resources from spells of substantial economic growth, particularly in the early 1990s, were not used effectively to combat these growing distributional problems, and labor market and education policies did not adequately address the problem of unskilled workers. The change in the share of aggregate labor for workers with less than a high school education had declined by one-third between 1974 and 2002, and unskilled workers experienced particularly large losses in both hourly wages and hours of work in the 1990s (Gasparini, 2003). At the same time, Gasparini's analysis using Mincer equations (to compute wage-education profiles) shows that returns to formal education were always positive over this period, including during economic downturns. As the poverty rate climbed nearly 10 percent over the 1990s, the lack of a concerted public policy response to the growing demand for a more highly educated workforce was garnering new attention.

Internal documents of the Inter-American Development Bank indicate that although nearly all Argentine children ages 6-13 years have access to primary school, only three-fourths of them complete nine years of basic education, and only half make it to the last year of high school.[5] Furthermore, there are large discrepancies in who completes high school by household income. As of 1999, only 27 percent of 19-20 year olds in the lowest quintile of household income completed high school, whereas the comparable number for those in the top income quintile was 83 percent.

The *Programa Nacional de Becas Estudiantiles* began in the late 1990s and was identified as one of the priority social programs meriting support and protection following the 2001 economic emergency in Argentina. The Argentine government and international lenders were concerned that the economic crisis would increase student dropout rates, given the out-of-pocket costs of children's school attendance—i.e., school supplies, clothing, transportation, etc.—that further

[4] The Gini coefficient measure of inequality ranges from zero (perfect equality) to one (perfect inequality). It is derived from Lorenz curve, which plots the cumulative percentage of recipient units against cumulative percentage of income received by these recipients (ranked from lowest to highest income).

[5] Loan proposal: Program for Social Protection and Mitigation of the Impact of the Crisis on the Poor, AR-0295.

strain tight family budgets in difficult economic times. Each annual *Becas* scholarship totals 400 pesos (approximately US $140), paid in two equal installments of 200 pesos each (during May and September).[6] Eligible students come from families with a monthly total income of less than 500 pesos and who do not receive any similar benefit from another organization. The current program goal is to reach approximately one quarter of indigent children ages 13-18 years. The cost of the 2003 Becas program (scholarships and operations) has been estimated at $46 million USD.[7]

Recent research by Carneiro and Heckman (2004) and others argues that human capital development policies should target the young. Carneiro and Heckman point to evidence, albeit limited, that the returns to investments in children at earlier ages are considerably higher than those aimed at the low- and unskilled adult population who are being displaced in the labor market by structural economic change. Hanushek, Leung, Yilmaz (2003) similarly argue that investments in individuals are most important at the primary and secondary education levels, due to the sizeable externalities generated through increases in aggregate educational levels (that improve the productivity of others).[8] In addition, Bedi and Edwards (2002) show that as aggregate educational attainment increases (and incomes rise), educational expansion follows and tends to be associated with a more equal distribution of income. Thus, public investments to promote increased educational attainment among poor youth in Argentina might also contribute importantly to reducing the growing inequalities in the distribution of income.

[6] This amount compares to an average monthly transfer of $55 in the Progresa program (or the equivalent of one quarter of the family's annual income).

[7] Argentina Plan de Emergencia Social, April 10, 2003.

[8] Hanushek, Leung and Yilmaz also maintain that in the absence of externalities, wage subsidies would dominate as a transfer mechanism to improve the distribution of income.

III. STUDY SAMPLES, DATA AND METHODOLOGY

A condition of international lenders' support for the priority social programs in Argentina was an evaluation of the programs' effectiveness in targeting benefits, delivering services, and improving health and educational outcomes for the poor. Since the loan and its requirements were negotiated following the 2001 economic crisis, the *Becas* program had been in operation for about four to five years at the time planning for the evaluation took place. Random assignment was not an option for the evaluation, as the program was expanded after the economic emergency to provide benefits to all eligible youth. However, a baseline survey that collected very detailed information on students and their families to determine applicants' eligibility for the program—*Encuesta de Los Aspirantes de Becas*—had been a standard application requirement since the initiation of the program in 1997.[9] This study takes advantage of the fact that in the early program years (prior to 2001), many more youth (and their families) completed baseline surveys and were determined eligible for the program than there were scholarships available.

A. Samples and data

In accord with the program objectives, the following student outcomes were of primary interest in the impact evaluation: student attendance (and retention) in school, absences, grade repetition, performance (grades) and school completion rates. In order to evaluate student outcomes for five years (through the expected year of graduation), a random stratified sample of cases from among the 8th grade eligible students in 1999 was selected to form the treatment and comparison groups. In 1999, there were approximately 300,000 applicants to the program; among the 265,000 students determined eligible to receive the scholarship in 1999, 149,000 were excluded due to quota restrictions that limited the number of Becas beneficiaries by school. If one assumes that the quotas were applied indiscriminately, then exclusion by quotas would function as an instrument to create equivalent treatment and comparison groups for the evaluation. Appendix A describes the criteria followed in the random stratified sample selection, and Table A.1 shows the distribution of treatment and comparison group members by schools (24) within provinces (8). The ratio of Becas beneficiaries (treatment group) to non-beneficiaries (comparison group) in the selected sample was approximately 2:1.

Following the selection of treatment and comparison group members (3490 students) from the baseline survey records, the Argentina-based evaluation team (from the *Universidad Nacional de Tres de Febrero—UNTREF, Centro de Investigaciones en Estadística Aplicada*—CINEA) went to the 24 schools to recover the administrative data (school records) for each student in the evaluation. The school records included the following information: student identification number, school year, most recent home address, continuation in school, left for another school, left the school registrar (e.g., discontinued studies), courses in school, completion of course material, course grades, absences, sanctions, and repetitions. The availability of the entirety of

[9] The *Becas* program was initially launched as a pilot (test) program in 1997 and then expanded coverage in subsequent years.

this information varied from school to school. Student registration information was complete for two-thirds of all students (from 29.9% to 98.6% complete across the provinces).[10]

A follow-up survey—*Encuesta para los Aspirantes 1999 y su Grupo Convi*viente—was also administered in the spring of 2004 (by UNTREF-CINEA) to collect data comparable to that obtained in the baseline survey and to gather supplemental information about the *Becas* students' experiences in the *Programa Nacional de Becas Estudiantiles*. A total of 2,586 students and their families completed the survey (see Table A.2 in Appendix A). The ratio of *Becas* beneficiaries (treatment group) to non-beneficiaries (comparison group) among the survey respondents was consistent with the full sample at approximately 2:1. The follow-up survey data include information on student demographics/family background (education, income, living conditions, labor force participation, etc.) and additional information about the students' behavior, receipt of the *Becas* scholarship and their perceptions of the effects of the program. Although the baseline and follow-up surveys were too extensive to list all of the measures collected, Table B.1 in Appendix B describes the primary measures from these surveys and the school records that were used in the empirical analyses.

A final source of data for the evaluation came from a survey administered to school officials in each of the 24 schools selected for the evaluation. School administrators provided basic information about the schools (grade levels, number of school days per year, school resources, educational priorities, etc.) and specific information about the implementation of the *Becas Estudiantiles* program in their school (e.g., ratings of different aspects of the program's functioning, changes made in implementing the program or as a result of the program, etc.). Administrative data from the Ministry of Education on all students in the study schools was also used to create several student population measures (e.g., overall grade average or performance, average distance to school, percentage of student households without a telephone, proportion of eligible families receiving *Becas* scholarships). The data from this survey that were used in the empirical analyses are described in Table B.2 in Appendix B.

Data from each of these four major sources—baseline survey, school records, follow-up survey and school administrator survey—were linked to facilitate econometric analyses of the program's outcomes and impact.[11] Prior to undertaking these analyses, however, the samples of treatment and comparison group members were compared (to test for statistical equivalence), and the youths' status as *Becas* beneficiaries or non-beneficiaries was verified with administrative data and information from the follow-up survey to check for contamination problems. In addition, the cases with missing follow-up survey data were compared to those with complete information to check for statistically significant differences between these groups. The details and results of these analyses are described in Appendix C.

[10] See the report by UNTREF-CINEA, December 31, 2003: *Evaluación del Impacto del Programa Nacional de Becas Estudiantiles*: 1º Informe de Avance.

[11] The assembly of these data was completed by UNTREF-CINEA and is thoroughly documented in two major reports: *Evaluación del Impacto del Programa Nacional de Becas Estudiantiles: 1º Informe de Avance* (December 31, 2003), and *Evaluación del Impacto del Programa Nacional de Becas Estudiantiles: 2º Informe de Avance* (February 15, 2004).

The sample verification analyses suggest that although there was some treatment group contamination, (i.e., 157 cases in the comparison group who reported receiving the *Becas* scholarship in at least one year), this problem was correctable and did not contribute to sample bias. However, there were statistically significant differences in the baseline characteristics of the treatment and comparison group members, which would not have been expected if the quotas had served as an effective instrument for randomly assigning eligible students to receive the *Becas* scholarship. Specifically, there were statistically significant differences between treatment and comparison group members in terms of their average family income per capita, number of household members, number of dependents under age 18, living conditions, overcrowding, work inside the home, and a measure of risk/vulnerability. In the case of each of these observed differences (shown in Table C.1), the treatment group members were a poorer, higher-risk group than the comparison group members. It is also important to point out, however, that there were no large differences between the two groups in their baseline schooling characteristics, i.e., age-grade difference, repetitions, number of times left school, and number of absences.

Ravela (2000) explains that the quota system used in the *Becas Estudiantiles* program was not a simple exclusion mechanism; rather, it was designed with the explicit objective to first enroll those applicants with higher indicators of need (i.e., those with higher scores on the index of need/risk). The higher levels of need among the treatment group members were also confirmed in multivariate analyses predicting treatment status.[12] These findings have important implications for the evaluation methodology used in this study.

B. Methodology

In the presence of these observable differences between the treatment and comparison group members (suggesting that poorer students were more likely to participate), econometric matching methods are used to adjust for potential bias due to nonrandom selection into the program and to estimate the effect of treatment on the treated. There is a large and growing literature on the use of econometric matching methods that explores and challenges Rosenbaum and Rubin's (1983) early finding that matching on the propensity score (i.e., the estimated probability of program participation) removes any bias associated with pre-treatment differences between the treatment and comparison groups, as long as there are no unobservable variables that influence selection into the program and the estimated outcomes (see, for example, Imbens, 1999, 2004; Mueser, Troske and Gorislavsky, 2003; Heckman and Navarro-Lorenzo, 2004 and Smith and Todd, 2004).

Some of these recent studies (e.g., Heckman and Navarro-Lorenzo and others in a February 2004 *Review of Economics and Statistics* symposium on the econometrics of matching) investigate the data requirements, conditions, and assumptions under which matching methods are more (or less) likely to reduce bias in the estimation of treatment effects. The findings of these studies generally imply that the *Becas* program evaluation is a good candidate for the application of the propensity score matching methodology. There are rich pre-treatment data available for use as the conditioning set (e.g., the index of need and other observable variables that were explicitly

[12] Ravela (2000) reports that the range of values on this index for all applicants in 1999 was 21-66; the range for the sample in this study is 23-58.

used to select *Becas* participants), and given (or in addition to) this set, there is also a "randomization device" that determines treatment status (i.e., the quotas that excluded more than one half of the eligible *Becas* applicants from the program).

At the same time, it is important to make clear that there are two stages or processes of selection in the *Becas* program that will require two different approaches to the matching estimation. In the first stage of initial selection into the program (in 1999), it is assumed that selection is based on the explicitly defined eligibility criteria (e.g., monthly family income less than 500 pesos) and the index of need/risk that was used to prioritize among applicants when filling the quotas. All treatment and comparison group members in this study met the *Becas* program basic eligibility criteria, and simple descriptive analyses confirmed considerable overlap between these groups in their need/risk scores.[13] Again, the assumption made is that there are no unobservable variables that influence selection into the program and program outcomes; that is, allocation to treatment status (other than prioritizing by the above criteria) was effectively random.

The second stage of selection determines the duration of participation or length of *Becas* scholarship receipt. Approximately one half of those youth who participated in the program received scholarship for 2 to 5 years.[14] Continued scholarship receipt was made conditional on students' regular school attendance and grade progression, and individuals knowledgeable about the *Becas* program administration confirmed that students' behavior and performance in school (e.g., records of attendance and grades) influenced the duration of their participation. Therefore, it was expected that among the participants (i.e., conditional on their selection into the program), selection into additional years of scholarship receipt would be based on a different set of characteristics, in particular, those associated with their performance in school.

Following Behrman, Cheng *Becas* and Todd (2004), a marginal matching estimator was employed to estimate the marginal treatment effect of an additional year of participation in the *Becas* program, using only data on program participants. As Behrman et al. demonstrate, because this estimator does not require assumptions about the process used in initially selecting students into the program (i.e., the first stage of selection described earlier), any selection on unobservable characteristics in the first stage should not bias the marginal impact estimates. At the same time, the marginal matching estimator does assume that any nonrandom selection into alternative program durations (the second stage of selection) is also based on *observable* characteristics.

A third methodological approach used in this study, multilevel modeling, takes advantage of the availability of both student- and school-level data that can be linked to analyze the variation in student outcomes between schools. Unconditional models estimated with these linked data show that there is, in fact, substantial (statistically significant) variation in student outcomes between

[13] The range of scores for nonbeneficiaries in the study sample on the index of need/risk was 31-45 (median=34), while the range for *Becas* beneficiaries was 23-58 (median= 38).

[14] The measure of the number of years the scholarship was received by students and their families was based on administrative records and the follow-up survey questions (that asked if the Becas scholarship was received in each year, 1999-2003). This measure thus automatically excludes the 904 cases with missing data in the follow-up survey. Administrative records showed that a total of 3416 annual scholarships were given to students in the study sample over this period.

schools: 23.9-30.5 percent of the total variation in student grade averages for the years 1999 to 2002 is between schools, and 17.4 percent of the total variation in the number of years students attended high school (third cycle of EGB) is between schools.[15] The presence of significant intra-class correlations in hierarchical data violates basic assumptions of the ordinary least squares regression, e.g., the assumption of independence of observations and that the number of independent observations is equal for all variables. Perhaps more important, however, ordinary least squares regression has been criticized for its neglect of hierarchical relationships and for fostering "an impoverished conceptualization" of relationships within and between multiple levels of social systems (Bryk and Raudenbush, 1992).

Multilevel methods are used in this study to model the relationships between school/*Becas* program characteristics (e.g., resources, educational priorities, program implementation and functioning, etc.) and student outcomes. A simple (random intercept) specification attempts to explain the variation in outcomes between schools with these school-level measures. A more complex (random coefficient) specification models the estimated effect of participation in the Becas program as a function of the school-level variables, i.e., to assess whether variation in the impact of the program across schools can be explained by these factors. These analyses are described in greater detail in the following section.

[15] As a general rule of thumb, Kreft (1996) defines a "high" intra-class correlation as one that is larger than $\rho = 0.25$, (i.e., more than 25 percent of the variation between groups or schools), although much smaller proportions of total variance at the group/school level may be statistically significant and warrant exploration.

IV. EMPIRICAL ANALYSES OF PROGRAM IMPACTS

A. Predicting treatment status and duration of scholarship receipt

As discussed above and shown in Appendices B and C, a rich set of pre-treatment (baseline) measures was available to use in predicting the probability of treatment or *Becas* scholarship receipt (i.e., the propensity scores). In addition, the choice of variables to use in predicting participation was informed by careful study and documentation of the application/intake criteria and decision processes for the *Becas* program (see, for example, Ravela, 2000). Based on this prior knowledge, it was expected that the two most important predictors (or determinants) of any program participation would be family income (per capita) at application and applicants' scores on the index of need/risk. Per capita income was measured categorically, with the lowest income rank=9 (0-15 pesos/month) and the highest income group=1 (more than 120 pesos/month). The models include two income variables: one indicating an income of less than 45 pesos per month, and the other the ranked ordinal measure.

Other variables included in the propensity score estimation of treatment status were: student age at application, sex and parent education level; number of household members, number of dependents under age 18, index of household living conditions, and index of overcrowding (persons per room); distance to school; number of student absences, grade repetition, age-grade difference and number of times student left school in the academic year prior to application; hours of work per day outside the home, number of days worked per week outside the home, hours per day in work in the home and number of days worked per week in the home; and province indicators. All of these variables were either observed prior to enrollment, are stable characteristics, or are deterministic with respect to time. In predicting the propensity to receive the Becas scholarship more than one year (for use in the marginal impact analysis), measures of students' school performance—grade averages and number of absences in the first year of participation—were added to the set of explanatory variables listed above to account for the conditions placed on youths' continued receipt of the scholarship (i.e., regular school attendance and academic advancement).

The results of the models predicting participation for the two different stages of selection, i.e., any treatment (including all treatment group and comparison group members), and receipt of *Becas* scholarship for 2 or more years (including only treatment group members), are shown in Table 1. The results from predicting any scholarship receipt (the first model) show that older children were significantly less likely to be *Becas* scholarship recipients, which is consistent with the goal of the program to intervene early in the third cycle of the EGB (i.e., at the time of entry to high school) and with subsequent findings that the duration of scholarship receipt was relatively short for many. Also consistent with the explicit selection criteria, those who scored higher on the index of need (i.e., suggesting greater need or higher risk of not completing school) were significantly *more* likely to receive the *Becas* scholarship; in terms of log-odds ratios, for each additional point higher on the need score, a student was 35.5 percent more likely to receive a scholarship. Those students from families with per capita incomes below 45 pesos per month were also more likely to participate (significant at α=0.06), although after controlling for the index of need and those with the lowest incomes, the relationship between income and participation reversed (i.e., students from families with lower per capita incomes were less likely

to participate). It should also be noted that students in Buenos Aires (the omitted province in the propensity score model) were more likely to be scholarship recipients than students from other provinces in the samples, while those who lived farther away from school were significantly less like to participate.

In general, the positive link between indicators of greater need and participation in the program is what program administrators like to see. In this model, the index of need and income measures together account for about two-thirds of the variation in the probability of initial scholarship receipt, with the index of need being the most influential predictor. In conjunction with the matching analysis, balancing tests were conducted to check the assumption that after conditioning on the above variables, the independence condition required to uphold matching was satisfied (i.e., no additional conditioning variables predict receipt of the scholarship). The results of these tests strongly support the use of matching with the propensity score model shown in Table 1, column 1.[16] The balancing test results are presented below along with the impact estimates.

The second model in Table 1, predicting receipt of *Becas* scholarship for two or more years, suggests that a very different set of criteria determined continuing program participation. The two most powerful predictors of additional years of scholarship receipt were students' grade averages in 1999 (i.e., their performance during their first year of program participation) and their age-grade difference in the year they entered the program (i.e., a larger difference, suggesting the student started late or was held back, was negatively related to the duration of participation). The only other statistically significant (positive) predictor of continuing program participation, other than the province indicators, was parent education level. In simple correlations, parent education level was significantly and negatively correlated with student absences from school.

The fit of these two models predicting participation (and producing the propensity scores) is fairly good, as suggested by the goodness of fit measures from the logistic regressions, although the prediction of any participation appears more accurate. It is not surprising that the determination of additional years of scholarship receipt was a more selective process—one that apparently took into account (or rewards) student performance and compliance with the scholarship conditions, as suggested by those familiar with the program's implementation.

B. Matching analysis: average and marginal program impacts

The estimation of the propensity score, $P(X)$, reduces the matching problem to a single dimension. That is, the distribution of the propensity score (or probability of participation) is compared for treatment and comparison group members in making the matches. There are a number of different techniques that may be used in the matching procedure, e.g., one-to-one matching (nearest neighbor, within caliper), k-nearest neighbors, radius, kernel, local linear regression and Mahalanobis matching (see, for example, Mueser et al., 2003 and Heckman and Navarro-Lozano, 2004 for more information about these techniques). In this study, radius

[16] In the balancing tests, it was found that the propensity score estimation could be improved by adding the square of the index of need. This slightly modified version of the model was used in the subsequent impact estimations.

matching is used with a caliper of 0.05 to remove matches for which the difference in propensity scores exceeds this threshold. A common support is also imposed to preclude poor matches between treatment and comparison group members. Other specifications were tested, including local linear regression and nearest neighbor methods, but the results were not sensitive to the method used.[17] All standard errors for the impact estimates were computed using bootstrapping methods.

Figure 1 shows the distribution of the propensity scores in the treatment and comparison groups; in this case, the comparison group consists of those eligible students who did not receive the Becas scholarship in any year. The length of the bar associated with each level of the propensity score indicates the number of students with estimated probabilities of participation at a given level. It is clear from comparing the two groups that those in the treatment group have higher estimated propensities to participate, driven primarily (as discussed earlier) by their higher scores on the index of need. In fact, there are no comparison group members at the very highest level/bar (of the propensity score distribution). It is this uppermost segment of the treatment group that is excluded when a common support is imposed in the matching estimation of average program impacts.

As discussed earlier, a marginal matching estimator is also used to estimate the impact of one or more additional years of Becas scholarship receipt. The theoretical model for this estimation (and how it compares to the estimate of the average impact of treatment on the treated) is presented briefly in Appendix D; this method is thoroughly explicated in recent publications (see Behrman, Cheng and Todd and other articles in the February 2004 issue of the *Review of Economics and Statistics*).

Impacts on school attendance. Since a primary objective of the Programa Nacional de Becas Estudiantiles is to increase the number of poor students completing school (i.e., attending school a full 4-5 years from 8th or 9th grade to 12th grade), a key outcome measure used in the impact analysis is based on students' school attendance records and reported attendance in the follow-up survey. A measure was constructed that indicates for each year (1999-2003) if the students attended school all of the year, (with the null category indicating that they began school and then left that year or did not attend school at all in that year). From these data, the dependent variable—the number of years attended a full school year—was created (for the 2586 cases who have school record and follow-up data):

Did not attend a full year of school any year	Count=56	2.17%
Attended 1 full year of school	195	7.54
Attended 2 full years of school	244	9.44
Attended 3 full years of school	243	9.40
Attended 4 full years of school	334	12.92
Attended 5 full years of school	1514	58.85

[17] The matching estimation was performed using the Stata psmatch2 command. Information on downloading and using this routine can be found at: http://fmwww.bc.edu/RePEc/usug2001/psmatch.pdf.

Simple descriptive statistics (i.e., a chi-square test) showed that members of the treatment group were significantly more likely ($p<.0001$) to attend five full years of school than those in the comparison group, and a strong, statistically significant ($p<.0001$) and positive association between the number of years of Becas scholarship receipt and the number of years students attended a full school year was also confirmed. Of course, these simple descriptive statistics don't adjust for the observed differences between the treatment and comparison groups, and thus, the propensity score matching approach described above is used to estimate the Becas program impact on students' attendance. Average impacts were estimated for the treatment status of any participation and marginal impact estimates were produced for those receiving the Becas scholarship for two or more years (compared to those receiving the scholarship for just one year).

Table 2 summarizes the results of these econometric analyses. The first two rows in Table 2 present the average impact of treatment on the treated (for any program participation) and the marginal impact of treatment on the treated for those with two or more years of scholarship receipt, respectively. Both the matching and ordinary least squares (OLS) regression estimates are presented for comparison. The matching result in the first row, first column shows a statistically significant average impact of the Becas program, with receipt of the scholarship increasing the number of full school years attended by about one-fifth of a year. At the same time, the magnitude of this estimated impact is one half the size of the OLS estimate. As expected, the matching procedure dropped about 20 percent of the treatment group due to the lack of a common support (i.e., no close match in the comparison group for these cases), likely accounting for the discrepancy between the two estimates.

The marginal impact estimates shown in the second row suggest a fairly strong, statistically significant impact of the program on full school year attendance for those receiving the scholarship for two or more years compared to those receiving it only one year. These scholarship recipients (with a longer program duration) attend school almost one-half a year longer than students who participate in the *Becas* program for just one year. In this estimation (using only treatment group members), just three treatment group members are dropped from the estimation, and the matching and OLS estimates are nearly identical.

The final two rows of Table 2 show a similar pattern of results for the analysis predicting the *Becas* program's average and marginal impacts on high school graduation rates. Again, the matching estimation of average impacts discards about 20 percent of the treatment group, and once poor matches are excluded, no significant average impact on graduation rates is observed. However, the marginal impact estimations (matching and OLS) both show that those receiving the scholarship for two or more years are significantly more likely (about 8%) to complete high school than those receiving the scholarship only one year. In effect, the positive impact on youths' school attendance and high school completion appears to be primarily driven by students who are receiving the scholarship for more than one year.

Impacts on academic performance. As described above, an objective of conditional cash transfer programs like the *Becas* program is to change students' behavior, i.e., the dynamic incentive effect. That is, student are expected to be motivated by the opportunity to receive future transfers (or scholarships) if they stay in school, and in the case of the *Becas* program, if they keep up their grade averages, too. Dubois et al. (2003) explain that this presumes that students can choose to increase their level of learning effort. A series of questions on the follow-up survey

asked the *Becas* program participants if, *due to receipt of the Becas scholarship*, they: (1) put more effort into their studies; (2) tried not to be absent from school, (3) made an effort not be sanctioned, (4) felt more pressure to do well in school, and (5) felt more pressure to have better conduct in school.

Simple statistical (chi-square) tests were performed to determine if students' behavior was different for those receiving the scholarship for two or more years compared to those who did not continue in the program after their first year of participation. The results showed that those receiving the *Becas* scholarship for two or more years were significantly more likely to report that they had put more effort into their studies (65.2% vs. 60.6%, p=0.002) and tried not to be absent from school (64.6% vs. 60.1%, p=0.006). Although these are reported rather than observed changes in effort, information is available from the students' school records to assess whether their performance in school was affected by their receipt of the scholarship.

Two measures of student academic performance are available for each school year (from school records): students' average grades in their coursework and grade repetition.[18] Propensity score matching methods were again used to estimate the average impacts of the Becas program on these academic performance outcomes. The results of the analyses are shown in Table 3. As in the analysis of school attendance and high school completion, a large number of cases (up to one-third) were excluded from the average impact estimation due to a lack of common support (i.e., poor matches). The estimates of average program impacts produced by multilevel models are also reported in Table 3. These models include the same set of predictor variables that were used in the propensity score matching analyses, but as in the OLS estimation, they do not exclude any cases.

The findings presented in Table 3 suggest that participation in the *Becas* program contributed to significantly reduced grade repetitions and increased student grade averages in nearly every year, 1999-2002. The sizes of the estimated effects are fairly consistent from year to year, although they differ somewhat between the matching and multilevel models. The average impacts on *grade repetitions* produced in the matching analysis are nearly twice as large as those estimated by the multilevel models; the average impacts on *grade averages* are fairly comparable between the two methods of estimation. No average impacts on students' grades are found, however, for the years 2001 and 2002. This latter finding is not unexpected, given that about one half of all of the treatment group members received the scholarship in only one year (i.e., 1999 or 2000).

Table 4 presents the results of the marginal impact estimation (of receiving the *Becas* scholarship for more than one year) for these two outcomes (grade repetitions and grade averages). As in Table 3, results from both matching and multilevel models are reported. In each of the matching estimations, there were no more than two cases that were excluded from the analysis (i.e., off the common support), and in general, the sizes of the estimated effects are very similar for the two approaches to estimation.

[18] Within the cohort of students in this study (that began the Becas program in 1999), these academic performance data are available for fewer students each year, in part because some of these students leave school, but also in part due to information missing from the student records.

These results (in Table 4) also show that the *Becas* program reduced grade repetitions and contributed to significant increases in the scholarship recipients' grade averages. The probability of repeating a grade was reduced by about 0.05-0.08 in the years 1999, 2000 and 2001 (with no significant marginal impacts found in 2002). The marginal impact of the *Becas* program on students' grade averages was also not trivial. Looking across the years, grades appeared to increase by about 0.14-0.17 of a point with receipt of the scholarship for more than one year (with a median grade average of approximately 7.28 for students over this period).

In considering the significance of these findings, it is important to bear in mind that the program conditions required students to achieve good attendance and progress academically in order to continue in the scholarship program. One might argue that these improvements in student outcomes were driven primarily or solely by changing student profiles. At the same time, the selection process into additional years of scholarship receipt was modeled in the propensity score estimation (see again Table 1), and these same controls were entered into the multilevel models. And the size of the estimated average and marginal impacts on student grade averages for the years 1999, 2000 and 2001 were fairly close, despite these very different selective processes for initial participation and continuing participation.

C. Balancing test results

As indicated earlier, the validity of the matching results rests on the assumption that there is no other variable that could be added to the conditioning set of the propensity score model that would improve estimation and no unobservable factors that influence selection into the program and student outcomes. Table 5 presents the results of balancing tests for key predictor variables in the propensity score models of any scholarship receipt (for the estimation of average impacts) and scholarship receipt for more than one year (for the estimation of marginal impacts).

For each variable and student outcome shown in the first column, after conditioning on the set of variables used in propensity score estimation, the means of the predictor variables for treatment and comparison group members (shown in bold in Table 5) are statistically equivalent. These results suggest that the propensity score model specifications are appropriate. In most cases, the percent reduction in bias is over 90 percent, with bias reduction as high as 99.4 percent. Thus, although there are no definitive tests to confirm that the propensity score or matching models have not left out important, unobserved factors determining participation at either stage, the balancing test results, the good fit of these models, and their consistent estimates are encouraging and suggest that it is unlikely that the positive impacts are driven *only* by selection effects.

D. Multilevel analysis with school-level variables

As discussed in the introduction, "second-generation" efforts to improve the design and effectiveness of conditional cash transfer programs are focusing on the implementation and management of programs, in addition to typical design considerations such as the size and targeting of transfers, the types of conditions imposed on recipients, and the frequency and duration of transfers. Average program impacts estimates obtained through experimental evaluations may be less useful if decisions made in implementation, or local resources available to support program development and their management, contribute importantly to variation in program impacts across communities or implementation sites.

Table B.2 in Appendix B describes a subset of variables from the school-level data that were collected for the Becas Estudiantiles evaluation and used in the analyses presented in this paper. These variables were selected in part because of the substantial variation across schools in these measures and also for their representation of important implementation and management factors or decisions, e.g., general school policies/management strategies and functioning, changes in strategies or institutional conditions due to the Becas program implementation, and school resources and population characteristics. The basic forms of the multilevel models that were estimated[19] to explain the variation in academic outcomes between schools (random intercept specification) and the variation in estimated impacts of the Becas program between schools (random intercept/random coefficient specification) are shown below:

$$Y_{ij} = \beta_{0j} + \beta_{1j}X_{1j} + ...+ \beta_{nj}X_{nj} + r_{ij} \quad \text{(level 1) student-level academic outcome submodel}$$

$$\beta_{0j} = \gamma_{00} + \gamma_{01}W_{1j} + ...+ \gamma_{0n}W_{nj} + u_{0j} \quad \text{(level 2) school-level random intercept submodel}$$

$$\beta_{1j} = \gamma_{10} \ , \ldots , \ \beta_{nj} = \gamma_{n0}$$

and the alternative random intercept/random coefficient specification:

$$\beta_{0j} = \gamma_{00} + \gamma_{01}W_{1j} + ... + \gamma_{0n}W_{nj} + u_{0j}$$

$$\beta_{1j} = \gamma_{10} + \gamma_{11}W_{1j} + ... + \gamma_{1n}W_{nj} + u_{1j} \quad \text{(level 2) school-level random coefficient submodel}$$

$$\ldots \beta_{2j} = \gamma_{10} \ , \ldots , \ \beta_{nj} = \gamma_{n0}$$

where X are student-level predictors; W are school-level variables; r and u and are student- and school-level error terms, respectively; and β and γ are estimated coefficients. In these analyses, the (dependent) academic outcome variable is student grade averages (performance) in school.

Table 6 presents a final subset of results from these estimations. The student-level predictors (X) and province indicators included in these models were the same set as used in the propensity score estimation (see Table 1) and are not shown in this table. The models estimated both the average and marginal impacts of the *Becas* program on student performance (grade averages) for four school years (1999-2002). The first line in each set of results shows the impact estimates from these models, which correspond closely to those presented in Tables 3 and 4 for both the average and marginal impact estimations (without school-level variables). Also included in each set of models are three school-level variables that were consistent and frequently statistically significant predictors of the variation in student outcomes between schools: the grade average of all enrolled students (a measure of peers'/student body academic achievement); a rating (1-10) of the effectiveness of communication and the execution of program procedures by school administrators; and a rating (1-4) of the contributions of the *Becas* program to institutional conditions for learning in the school (again, as reported by school administrators in surveys).

[19] The SAS proc mixed and glimmix macros were used in estimating these models.

Each of the models shown in Table 6 is a random intercept specification. Specifying the coefficient on the treatment indicator (the impact estimate) in these models as random and interacting it with school-level variables did not generate any statistically significant findings; in other words, it does not appear that these school-level factors explain any variation in *Becas program impacts* across the schools. However, the three school-level variables shown in the random intercept models in Table 6 do explain most of the variation in student *outcomes* between schools (i.e., see the row showing the percentage of variation between schools that is explained by the models).

The results show that the average level of (overall) student performance in the schools (a control variable) is positively related to better academic outcomes for students in the sample. A possible interpretation is that high-achieving peers (or possibly unobserved school factors associated with overall student body achievement) positively influence students' academic performance. More importantly for the purposes of this study, Table 6 also shows that more effective communication and management (execution of program procedures) in the schools is positively related to student performance. In addition, greater contributions of the *Becas* program to institutional conditions for learning in the schools are also positively associated with student performance (i.e., null contributions are negatively related to student grade averages). And as one would expect, these estimated effects are stronger in the estimation of marginal impacts, i.e., the school variables are more important predictors for students who stay in school and in the *Becas* program for a longer period of time.[20]

The school-level measures that were identified as statistically significant predictors of the variation in student performance between schools are fairly general constructs of institutional capacity and management at the school level. Interestingly, the more specific information about strategies implemented in the schools to improve student and parent participation, leadership and teaching approaches, education materials and technical assistance, etc. did not emerge as statistically significant predictors in these models. The small number of schools included in this study (n=24) limited the number of variables that could be included in the school-level submodels, although the statistics from the models estimated indicated that there was no more statistically significant variation between schools that remained to be explained. Thus, the implications of these results seem to suggest that effective communication and management and overall institutional conditions for learning are more important than any particular strategy for implementing change in educational approaches or school policies within the schools in this study.

[20] There were no statistically significant *average* impacts of the *Becas* program in academic years 2001 and 2002, and correspondingly, no statistically significant predictors of variation in student outcomes between schools in these years.

V. CONCLUSION

The *Programa Nacional de Becas Estudiantiles*, a conditional cash transfer program in Argentina, was designed to increase school attendance and educational attainment and to motivate better school performance among high-school aged youth who are poor and at risk of leaving school before completion of the third cycle of basic education (i.e., high school). This nonexperimental study of the Becas program found significant effects of the program in increasing attendance, reducing grade repetition and improving students' performance (average grades). These impacts were largest for (and largely driven by) students who received the scholarship for more than one year. It appears that the Becas program, as implemented, may have established important dynamic incentive effects by linking receipt of the scholarship in subsequent years to students' academic performance in the first year of participation. The models estimating selection into the program and into longer durations of scholarship receipt clearly showed that these processes were distinct for initial and continued participation. It is also important to reiterate that these positive impact estimates assume that selection is appropriately modeled and accounted for in the propensity score matching and multilevel estimation.

The primary measure used in this study to evaluate student performance—students' average grades as reported in their school records—clearly has limitations as a proxy measure for student achievement and/or human capital development. There are no standardized tests that are regularly or universally administered to high school students in Argentina to evaluate students' skill attainment/achievement. Furthermore, the evaluation of the Becas program's impact on the more ambitious goals of promoting long-term human capital development, income redistribution and social inclusion would require a much longer follow-up period, and ideally would include measures of the students' earnings/income following the high school years. Thus, while the results of this study suggest some promise for these types of conditional cash transfer programs in increasing years of schooling completed and improving students' grades in school for students who respond to the scholarship's incentives to perform better, no longer-term impacts of the program should be inferred from this study.

At the same time, there are some potentially useful policy recommendations for the *Becas* program and similar conditional cash transfer program initiatives that derive from this study. In terms of the *Becas* program implementation, it seems likely that if a larger number of students receiving the scholarship participated for more than one year, the program's impact could be appreciably augmented. In the ongoing *Becas* program, it would be worthwhile to gather more information about the students who do not receive the scholarship for multiple years. Do they fail to meet program conditions for continued participation, and if so, are there barriers to their academic success that could be addressed within the school or family? Do some *Becas* participants decline to receive the scholarship for social reasons (e.g., perception of a stigma) or because they *choose* to leave school (e.g., to take a job)?

The multilevel analysis suggested that efforts to strengthen institutional capacity and conditions for learning in the schools and to promote better management, communication and execution of program procedures also contributed to student performance (although no "best practices" or specific strategies were identified in this research). In general, this study confirms the findings of prior studies showing the promise of conditional cash transfer programs in increasing

educational attainment among poor youth, and also those studies suggesting that attention to and investments in both demand-side and supply-side factors that influence student attendance and performance are important.

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TABLES & FIGURES

Table 1: Determinants of Participation in the *Becas Estudiantiles* Program

Predictors (*=statistically significant at α<0.05, standard errors in parentheses below coefficients)	**Any scholarship receipt** (n=2585)	**Beneficiary 2 or more years** (n=1341)
Intercept	**-4.8570*** (1.1904)	-3.0873 (1.5653)
Age	**-0.1630*** (0.0806)	-0.1328 (0.0967)
Sex (male=1)	0.1194 (0.1032)	-0.2084 (0.1219)
Family income per capita	**-0.2357*** (0.0557)	0.0078 (0.0691)
Income per capita < 45 pesos/month	**0.3200 (0.1725)**	0.1158 (0.1996)
Index of need/risk	**0.3036*** (0.0311)	0.0508 (0.0299)
Parent highest education level	0.0755 (0.0406)	**0.1109*** (0.0511)
Number in household	-0.0189 (0.0471)	-0.0218 (0.0526)
Number of dependents	-0.0337 (0.0546)	0.0173 (0.0552)
Living conditions index	-0.1114 (0.0901)	0.1757 (0.0870)
Family members per room	0.0380 (0.0587)	0.0315 (0.0530)
Distance to school	**-0.1635*** (0.0691)	0.0101 (0.0765)
Daily hours of work outside the home	-0.0413 (0.1047)	-0.1846 (0.1114)
Number of days worked inside the home	-0.0293 (0.0496)	0.0901 (0.0548)
Hours per day worked inside the home	-0.0630 (0.0971)	-0.1369 (0.1019)
Grade average in 1999	n.a.	**0.4750*** (0.1048)
Number of student absences (in 1999 for model 2)	0.0158 (0.0145)	-0.0039 (0.0098)
Number of grade repetitions	-0.1024 (0.0653)	-0.0320 (0.0801)
Age-grade difference	-0.0555 (0.1074)	**-0.2801*** (0.1303)
Number of times student left school	-0.2374 (0.2275)	-0.3091 (0.3039)
Salta	**-1.8755*** (0.2167)	**-1.099*** (0.2667)
Entre Ríos	-0.2304 (0.3678)	-0.3407 (0.3931)
Mendoza	**-1.4448*** (0.2014)	**-0.9637*** (0.2908)
Santa Fe	0.0210 (0.2077)	**-0.5223*** (0.2230)
Tucumán	**-0.4748*** (0.1789)	0.0747 (0.1971)
Corrientes	-0.1523 (0.2385)	0.0128 (0.2309)
Córdoba	**-1.3436*** (0.2384)	**-1.6601*** (0.3171)
Percent concordant/discordant	78.9/20.9	69.2/30.5

Table 2: Econometric matching analysis results: Impacts of Programa Nacional de Becas Estudiantiles on number of years attended full school year and graduation

Number of years attended full school year	Matching estimates *(standard errors in parentheses)*	OLS estimates *(standard errors in parentheses)*
Average impact: treatment group members (n=1916) vs. comparison group members (n=670)	0.216[a*] (0.078)	0.441* (0.064)
Marginal impact: treatment group members with 2 or more years of Becas scholarship receipt (n=830) vs. treatment group members reporting only 1 year of Becas scholarship receipt (n=893)	0.481[b]* (0.075)	0.482* (0.059)
Completed third cycle of Education General Basica (high school completion)		
Average impact: Treatment group members (n=1916) vs. comparison group members (n=670)	0.054[a] (0.035)	0.112* (0.021)
Marginal impact: Treatment group members with 2 or more years of Becas scholarship receipt (n=830) vs. treatment group members reporting only 1 year of Becas scholarship receipt (n=893)	0.084[c]* (0.024)	0.087* (0.022)

*Indicates statistically significant at $\alpha<0.05$.

[a] 395 treatment group members are not on the common support

[b] 3 treatment group members are not on the common support

[c] 2 treatment group members are not on the common support

† All standard errors reported in the table for the matching analysis are estimated through bootstrapping and are bias-corrected.

Table 3: Analyses of average impacts of Programa Nacional de Becas Estudiantiles On grade repetition and grade averages (student performance)

Outcome Estimation method	**Becas beneficiaries vs. nonbeneficiaries**							
	n [a]	**1999**	**n**	**2000**	**n**	**2001**	**n**	**2002**
Grade repetition: differences in probability (standard errors)								
Matching *n and cases excluded*	*1331*[b] / *387*[c]	-0.108* (0.032)	*1082* / *301*	-0.139* (0.054)	*768* / *237*	-0.154* (0.054)	*637* / *194*	-0.122* (0.054)
Multilevel estimation	*1718*[d]	-0.057* (0.023)	*1383*	-0.078* (0.031)	*1005*	-0.094* (0.035)	*831*	-0.045 (0.038)
Grade average: average differences (standard errors)								
Matching *n and cases excluded*	*1459* / *412*	0.156* (0.047)	*1194* / *323*	0.187* (0.058)	*890* / *278*	-0.061 (0.096)	*700* / *209*	-0.150 (0.094)
Multilevel estimation	*1871*	0.127* (0.036)	*1517*	0.159* (0.044)	*1168*	0.058 (0.056)	*909*	-0.035 (0.070)

*Statistically significant at $\alpha < 0.05$

[a] Number in subsample (treatment + comparison group members)

[b] Total number of cases in matching estimation (Stata ps2match)

[c] Number of cases excluded from matching due to failure of common support

[d] Estimates from estimation of multilevel models of program impacts (SAS proc mixed)

Table 4: Analyses of marginal impacts of Programa Nacional de Becas Estudiantiles

Outcome Estimation method	**Beneficiaries ≥ two years vs. beneficiaries one year**							
	n [a]	**1999**	**n**	**2000**	**n**	**2001**	**n**	**2002**
Grade repetition: differences in probability (standard errors)								
Matching *n and cases excluded*	*1257*[b] *2*[c]	-0.053* (0.022)	*1047* *2*	-0.072* (0.030)	*788* *2*	-0.079* (0.035)	*662* *2*	-0.053 (0.036)
Multilevel estimation	*1259*[d]	-0.039 (0.020)	*1049*	-0.052* (0.025)	*790*	-0.064* (0.027)	*664*	-0.034 (0.029)
Grade average: average differences (standard errors)								
Matching *n and cases excluded*	*1340* *1*	0.161* (0.045)	*1141* *2*	0.144* (0.060)	*907* *2*	0.169* (0.053)	*711* *2*	0.145* (0.068)
Multilevel estimation	1341	0.135* (0.032)	1143	0.143* (0.037)	909	0.173* (0.045)	713	0.173* (0.053)

*Statistically significant at $\alpha < 0.05$

[a] Number in subsample (treatment + comparison group members)

[b] Total number of cases in matching estimation (Stata ps2match)

[c] Number of cases excluded from matching due to failure of common support

[d] Estimates from estimation of multilevel models of program impacts (SAS proc mixed)

Table 5: Balancing test results

A. Results from average impact estimation					
Conditioning variable		Mean of variable		*Test results: Percent reduction in bias*	
Outcome	*Sample*	*Treatment*	*Comparison*		
Index of need/risk	Unmatched	38.56	34.88	% bias	85.8
School years completed	Matched	**36.62**	**36.75**	% reduction	96.3
Index of need/risk	Unmatched	38.19	34.45	% bias	88.4
Grade average 1999[a]	Matched	**35.81**	**35.83**	% reduction	99.4
Index of need/risk	Unmatched	38.13	34.54	% bias	83.5
Grade repetition 1999[b]	Matched	**35.71**	**35.91**	% reduction	94.3
Income per capita	Unmatched	6.26	5.69	% bias	31.3
Grade average 1999[c]	Matched	**5.85**	**5.84**	% reduction	98.0
Number in household	Unmatched	5.46	4.62	% bias	46.5
Grade average 1999	Matched	**5.03**	**4.85**	% reduction	77.6
Number of dependents	Unmatched	4.51	3.32	% bias	65.8
Grade average 1999	Matched	**3.88**	**3.77**	% reduction	90.2
Living conditions index	Unmatched	2.03	1.83	% bias	29.6
Grade average 1999	Matched	**1.90**	**1.89**	% reduction	94.1
Index of overcrowding	Unmatched	2.06	1.43	% bias	48.7
Grade average 1999	Matched	**1.56**	**1.67**	% reduction	82.3
B. Results from marginal impact estimation					
Age-grade difference	Unmatched	1.21	1.39	% bias	-26.0
Grade average 1999	Matched	**1.21**	**1.17**	% reduction	79.4
Parent education level	Unmatched	5.49	5.10	% bias	26.7
Grade average 1999	Matched	**5.49**	**5.45**	% reduction	89.2
Distance from school	Unmatched	2.10	1.97	% bias	14.5
Grade average 1999	Matched	**2.10**	**2.08**	% reduction	89.9
Grade repetitions (in past)	Unmatched	1.47	1.73	% bias	-26.2
Grade average 1999	Matched	**1.47**	**1.43**	% reduction	85.5

[a] Corresponding reductions in bias for years 2000, 2001 and 2002, respectively: 97.5%, 90.1%, 91.7%

[b] Corresponding reductions in bias for years 2000, 2001 and 2002, respectively: 93.3%, 89.2%, 92.8%

[c] Results were typically similar for outcomes in other years for other conditioning variables as well.

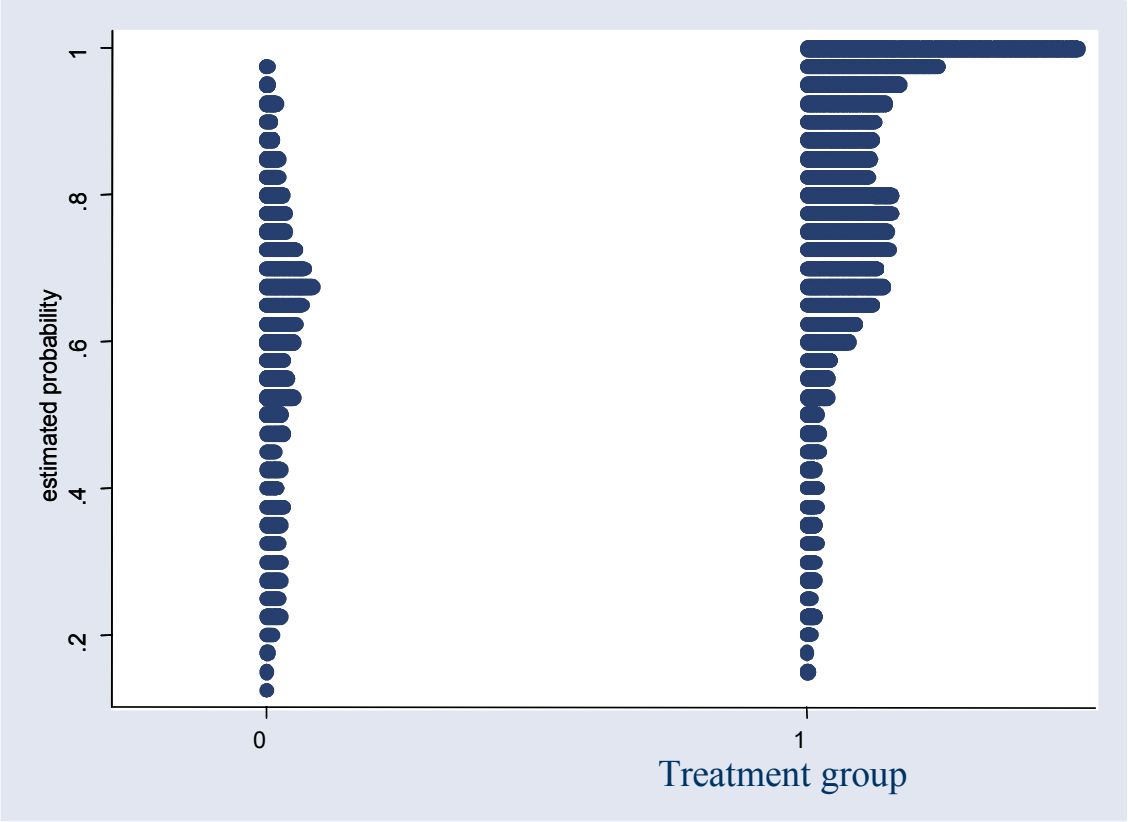
†Table 6: Multilevel models of student grade averages (performance) with school-level predictors

School-level predictors (n=24) (standard errors in parentheses)	**Grade average 1999**	**Grade average 2000**	**Grade average 2001**	**Grade average 2002**
Average impact estimation *Number of student-level cases*	*n=1765*	*n=1437*	*n=1116*	*n=867*
Becas scholarship recipient (impact estimate)	0.119* (0.037)	0.139* (0.046)	0.037 (0.058)	0.041 (0.073)
Grade average (all students)	0.580* (0.168)	0.367* (0.146)	0.232 (0.223)	0.043 (0.299)
Rating: Becas program management/execution (1=lowest to 10=highest)	0.021 (0.021)	0.049* (0.018)	0.038 (0.028)	0.069 (0.037)
Rating: Becas program contributions to conditions for learning (1=much to 4=nothing)	0.008 (0.031)	-0.071* (0.026)	-0.061 (0.042)	-0.083 (0.056)
% of total variation in grade averages between schools	23.9%	30.2%	27.9%	30.5%
% of variation between schools explained by model	93.5%	98.4%	92.9%	90.0%
Marginal impact estimation *Number of student-level cases*	*n=1269*	*n=1087*	*n=874*	*n=684*
Becas scholarship recipient for 2 or more years (impact estimate)	0.145* (0.033)	0.161* (0.038)	0.178* (0.045)	0.178* (0.054)
Grade average (all students)	0.629* (0.170)	0.407* (0.140)	0.334 (0.194)	0.164 (0.271)
Rating: Becas program management/execution (1=lowest to 10=highest)	0.026 (0.021)	0.510* (0.017)	0.062* (0.024)	0.055 (0.034)
Rating: Becas program contributions to conditions for learning (1=much to 4=nothing)	0.008 (0.033)	-0.108* (0.027)	-0.089* (0.038)	-0.110* (0.052)
% of total variation in grade averages between schools	23.3%	28.9%	28.4%	29.9%
% of variation between schools explained by model	94.2%	99.7%	97.1%	93.3%

*=statistically significant at $\alpha<0.05$

† Student-level variables included in these models were the same set as the determinants of participation (beneficiary) model shown in Table 1. Three of these student-level variables were statistically significant in all of the multilevel models estimated: sex (negative effect for males), parent highest education level (positive effect) and number of grade repetitions (negative effect).

Figure 1: Distribution of propensity scores (for estimation of any scholarship receipt) by treatment status



APPENDIXES

APPENDIX A: STUDY SAMPLES

The following criteria were used in the random stratified sample selection for the Becas treatment and comparison groups:

a. Geographical area: 8 provinces were selected (from among 22 provinces) to represent the diverse geographical areas of Argentina

b. A total of 24 schools were selected from within these eight provinces

   i. The total number of *Becas* beneficiaries and eligible students who were not *Becas* beneficiaries had to be large enough to permit random sample selection within the schools (a minimum of approximately 200 beneficiaries/200 eligible non-beneficiaries)

   ii. Within the provinces, at least one district with a high ratio of program participation to demand for the benefit was chosen, and one district with a low participation/demand ratio (suggesting higher levels of need) was selected

   iii. Approximately equal numbers of "cabecera" (head) and "non-cabecera" schools were selected

The schools selected were generally clustered within the provinces to make data collection cost-effective. *Becas* beneficiaries and non-beneficiaries were randomly selected for the treatment and comparison groups from within the 24 schools.

Table A.1: Distribution of treatment and comparison group members by schools and provinces

Province	School	Treatment group	Control group	Total
Salta	506	111	97	208
	594	109	88	197
	Total	220	185	405
Ciudad de Buenos Aires	128	161	93	254
	134	98	65	163
	355	128	66	194
	361	87	74	161
	Total	474	298	772
Entre Ríos	961	51	20	71
Mendoza	356	93	33	126
	366	75	42	117
	908	84	169	253
	Total	252	244	496
Santa Fe	350	78	22	100
	392	57	9	66
	400	100	63	163
	1737	119	29	148
	2153	85	47	132
	Total	439	170	609
Tucumán	308	106	73	179
	778	111	24	135
	779	181	39	220
	Total	398	136	534
Corrientes	320	73	39	112
	325	61	13	74
	1116	141	35	176
	Total	275	87	362
Córdoba	220	49	32	81
	1238	67	31	98
	1255	40	22	62
	Total	156	85	241

Table A.2: Follow-up survey responses— Encuesta para los Aspirantes 1999 y su Grupo Conviviente

Province/geographical area	Respondents with complete surveys	No or incomplete response
Córdoba	191	49
Corrientes	329	33
Mendoza	323	173
Paraná	61	10
Ciudad de Buenos Aires	457	306
Rosario	280	81
Salta	288	117
Santa Fe	170	78
Tucumán	493	41
Total and %	**2592 (74.5%)**	**888 (25.5%)**

APPENDIX B: VARIABLES USED IN THE EMPIRICAL ANALYSES

Table B.1: Baseline Survey, School Records and Follow-up Survey Measures

Variable	Range	Variable definition/construction
Outcome measures		
Number of years attended full school year	0-5	From follow-up survey question asking (for each year, 1999-2003) if students attended school all of the year (or began and left that year or did not attend school at all)
Number of absences	0-89	From student records in school registrar (1999-2003)
Grade repetition	0-1	Binary variables indicating if the student repeated a grade, from student records in school registrar (1999-2003)
Completion of third-cycle EGB (high school)	0-1	Binary variable indicating if the student completed the third-cycle of the EGB (high school)
Subject material performance (grades)	1-10	Students' performance in coursework, quantified from 10 (excellent) to 1 (poor), by subject (1-17) and year, from student records in school registrar
Average subject material (grade) performance	1-10	Students' average performance in coursework (across 17 subjects) from student records in school registrar by year
Labor force participation	1-5	Categorical measure of number of hours worked outside the home per week (follow-up survey)
Control variables		
Age	13-17	Students' age in years (baseline)
Sex	0-1	Male=1, Female=0 student (baseline)
Family income per capita Low income indicator (=1 if income per capita < 45)	0-9	Total monthly income divided by number in household, 9 categories: 0-15 (=9), 15-39, 30-44, 45-59, 60-74, 75-89, 90-104, 105-120, greater than 120 pesos per month (=1) (baseline and follow-up)
Number in household	0-13	Number of persons living in household (baseline)
Number of dependents	0-8	Number of household members age 19 or less, or with a sickness/incapacity that makes them dependent (baseline)
Living conditions index	0-6	Average of values of four intermediate indicators: construction material, floors, bathrooms and appearance or condition (baseline)
Household members per room	0-6	Number of household members divided by number of rooms in the home (baseline)
Household members' average number of years of schooling	1-5	Average years of education of all household members: 1=12 or more; 2=12<9; 3=9<6; 4=6<3; 5=less than 3 years (baseline)
Parent education level	0-10	Years of education of head of household (baseline)
Distance to school	1-4	Distance from student's home to central school—less than 1 km, 1-5 km, 6-10 km, or more than 10 km (baseline)
Number of student absences	0-89	Number of student absences from school administrative records (in year before application and 1999-2003)
Number of times students left school	1-3	Number of times students left school from school administrative data in year before application (baseline) and from school records by year
Number of grade repetitions	1-5	Number of times students repeated a grade from school administrative data in year before application (baseline) and grade repetition from school records by year
Age-grade difference	1-5	Difference between students' age and the age in grade year from school administrative data (baseline)
Daily hours of work outside the home	1-8	Hours per day worked outside the home (0-2, 3-4, 5-6, 7 or more) by student (baseline)
Days worked outside of home	1-5	Number of days per week worked outside of the home (less than 1, 1-2.5, 2.5-4, 4-5.5, more than 5.5) by student (baseline)
Number of days worked inside the home	1-4	Time worked in the home (occasionally, 2-3 days per week, 4-5 days per week, all days) by student (baseline)

Variable	Range	Variable definition/construction
Hours per day worked inside the home	1-5	Hours per day worked inside the home (0-2, 3-4, 5-6, 7 or more) by student (baseline)
Index of need/risk (higher score=higher need/risk)	23-58	Constructed using the 20 measures described in the table below and/or above (computed by UNTREF)

2. Measures used to construct the index of precariousness/risk

1	Family income per capita (see above)
2	Number dependents (see above)
3	Occupation of the head of household
4	Head of household is pregnant
5	Type of home (e.g., homeless, hotel, single-family home, ranch, etc.)
6	Tenancy (own with/without mortgage, rent, rooming with others, etc.)
7	Living conditions index (see above)
8	Household members per room (see above)
9	Distance to school (see above)
10	(Student) average number of hours worked per day outside the home
11	Number of tasks performed inside the home (e.g., caring for siblings)
12	Average years of education of all household members (see above)
13	Age-grade difference of student (see above)
14	Student illness or disability (weighted indicator)
15	Student is head of household (weighted indicator)
16	Student is pregnant (weighted indicator)
17	Student is a parent (weighted indicator)
18	Student lives alone (weighted indicator)
19	Student lives in institution (weighted indicator)
20	Student lives with employer (weighted indicator)
Maximum index value possible=79	Minimum index value=17

Note: Indicators (dummy variables) are also used in the statistical models to control for differences across provinces.

Table B.2: School Level Variables and Descriptive Information

Ratings of program/school (1=lowest—10=highest/best)	Mean	Standard deviation	Minimum	Maximum
General program functioning	6.95	1.73	4.0	10.0
Communication/execution of program procedures	6.23	1.90	2.0	10.0
Technical assistance	7.55	2.28	3.0	10.0
Educational materials	7.24	2.59	1.0	10.0
Ratings of program contributions	**1=Much**	**2=Some**	**3=Little**	**4=Nothing**
Contribution to institutional conditions for learning	29%	46	12.5	12.5
Contribution to student retention	25%	46	25	4
Changes made through program implementation	**1=better**	**2=no change**	**3=worse**	
Role of teacher in class	54.3%	45.7	0.0	
Participation of students	63.1%	36.9	0.0	
Leadership in school	60.7%	39.3	0.0	
Participation of parents	34.0%	62.8	3.2	
School strategies/policies adopted since 1999			**(% of schools)**	
Address income/poverty problems			70.8%	
Educational priorities of institution oriented toward lower income students			87.5%	
Integration of Becas students			83.3%	
Changes in the work of teachers			37.5%	
Changes in the hours/activities of students			41.7%	
School population characteristics	**Mean (or %)**	**Standard deviation**	**Minimum**	**Maximum**
Number of days of classes held in year	142.4	40.4	40	177
Grade average of all students in school	7.32	0.38	6.64	8.23
Annual amount of scholarship considered adequate for beneficiaries ($)	606	155	400	1000
Percent of students eligible for scholarship who get it	44.2%	5.9	34.0	55.8
Percent of students more than 5 kilometers from school	23.1%	15.7	0	73.5
Percent of students with no telephone at home	67.4%	12.3	37.9	96.3

APPENDIX C: SAMPLE VERIFICATION AND TESTS FOR STATISTICAL EQUIVALENCE

Before undertaking statistical comparisons of treatment and comparison group members, administrative data and follow-up survey reports were used to check for treatment group contamination (i.e., the possibility that some comparison group members may have received the Becas scholarship). Although no problems were observed in the administrative data, 62 follow-up survey respondents (in the comparison group) reported receiving the *Becas* scholarship in 1999; 58 said they received the scholarship in 2000; 37 reported receiving it in 2001, and 44 comparison group members said they received the scholarship in 2002. In all, there were 157 separate cases in the comparison group who reported receiving the *Becas* scholarship in at least one year (for a total of 201 scholarships).

Comparisons of the baseline characteristics of treatment and comparison group members (both with and without the 157-case sample adjustment) showed that the sample adjustment had a negligible effect on the comparability of the two groups. It was also clear, however, that there are some significant differences in the baseline characteristics of the treatment and comparison group members; in other words, they are not statistically equivalent (as expected if the quotas had randomly assigned eligible students to receive the *Becas* scholarship or to not receive it.) In particular, there are statistically significant differences between the treatment and comparison group members in terms of their family income per capita, number of household members, number of dependents under age 18, living conditions, overcrowding, work inside the home, and a measure of risk/vulnerability (see Table C.1).

An additional comparison was made to determine if the treatment comparison group members for which follow-up survey data were available differed from those who did not respond to the survey. Follow-up survey responses were missing from 581 treatment group members and 323 comparison group members (see Table C.2). In general, the differences between those with and without follow-up survey data are small, although some of these differences are statistically significant. These results suggest that impact analyses using the follow-up survey data should control for the differences between these two groups.

Finally, it is also important to note that there are some discrepancies between students who are identified as *Becas* beneficiaries in a given year and these students/families' reports that they did not receive the cash benefit in that same year. Among the treatment group, 259 reported not receiving the scholarship in 1999; 285 said they did not receive the scholarship in 2000; 175 indicated that they did not receive the benefit in 2001, and 21 treatment group members said they did not receive the *Becas i*n 2002. It is speculated that some of these reports of non-receipt simply reflect administrative delays in the administration of the *Becas* program and the delivery of the subsidies, (i.e., the subsidy is in fact received, but at a later time.)

Table C.1: Comparison of BASELINE CHARACTERISTICS OF treatment and comparison group MEMBERS *(with sample adjustment)*†

Treatment group N=2497 **Comparison group N=993**					
Variable	Mean-treatment group	Mean-comparison group	Std. Dev. (*unequal variance)	Std. Dev.-comparison	P-value
Age	14.12	14.14	1.06	1.06	0.701
Gender (% female)	53.1%	51.2%			0.309
Income per capita (9=lowest income, 1=highest)	**6.28**	**5.53**	**1.83**	**1.83**	**<0.0001**
Number of household members	**5.47**	**4.88**	**1.94***	**1.68**	**<0.0001**
Number of dependents (age 19 or under)	**4.61**	**3.46**	**2.13***	**1.60**	**<0.0001**
Living conditions index	**2.05**	**1.86**	**0.83***	**0.64**	**<0.0001**
Index of overcrowding	**2.19**	**1.48**	**1.68***	**0.97**	**<0.0001**
Parent education level	5.28	5.14	1.45*	1.65	0.033
Distance to school center	2.05	2.00	0.87	0.84	0.169
Number of times left school	1.06	1.06	0.25	0.24	0.976
Absences from school	1.76	1.39	4.80*	3.52	0.033
Grade repetitions	1.64	1.68	1.02	1.03	0.384
Age-grade difference	1.35	1.34	0.73*	0.68	0.797
Index of weekly work	1.06	1.05	0.36*	0.33	0.263
Index of work outside the home	1.10	1.06	0.64*	0.52	0.126
Index of work inside the home	2.26	2.14	1.39	1.37	0.016
Daily hours in home tasks	**1.79**	**1.71**	**0.74***	**0.69**	**0.002**
Index of precariousness	**38.88**	**34.93**	**5.41***	**3.00**	**<0.0001**

†Cases in the original comparison group who reported in the follow-up survey that they received the Becas in at least one year (1999-2003) were reassigned to the treatment group (i.e., n=157 cases changed treatment status).

Variables shown in bold indicate statistically significant differences between treatment and comparison group members (at $\alpha<0.01$).

Table C.2: Comparison of cases with and without missing follow-up survey data

Total number of cases with missing follow-up data: 904					
Treatment group: 581 (24.8%) **Comparison group: 323 (28.1%)**					
Variable	Mean	Mean-missing	Std. Dev. (*unequal var.)	Std. Dev.-missing	P-value
Age	**14.10**	**14.21**	**1.05**	**1.07**	**0.006**
Gender (% female)	52.1%	53.8%			0.386
Income per capita	**6.16**	**5.81**	**1.77***	**2.08**	**<0.0001**
Number of household members	**5.38**	**4.97**	**1.86**	**1.96**	**<0.0001**
Number of dependents (age 19 or under)	**4.34**	**4.11**	**2.02***	**2.17**	**0.006**
Living conditions index	1.99	2.01	0.76*	0.95	0.483
Index of overcrowding	1.95	2.10	1.53	1.61	0.016
Distance to school center	2.02	2.08	0.85*	0.90	0.072
Number of times left school	1.06	1.08	0.24*	0.29	0.061
Absences from school	**1.66**	**1.04**	**4.51***	**3.67**	**<0.0001**
Grade repetitions	1.65	1.73	1.02	1.04	0.035
Age-grade difference	1.35	1.35	0.71	0.71	0.913
Index of weekly work	1.05	1.07	0.34*	0.40	0.198
Index of work outside the home	1.08	1.11	0.58*	0.67	0.204
Index of work inside the home	2.21	2.28	1.38	1.40	0.206
Daily hours in home tasks	1.80	1.82	0.72	0.74	0.999
Index of precariousness	**37.61**	**38.18**	**5.07***	**5.42**	**0.005**
Becas beneficiary in 1999, 2000, 2001 or 2002	68.02%	64.27%			0.039

APPENDIX D: AVERAGE AND MARGINAL MATCHING MODELS

The matching analysis of average program impacts compares the school outcomes of students who received the *Becas* scholarship (the treated) with those of students who applied but did not participate in the program. The matching estimator is consistent for estimation of the treatment on the treated, which is commonly expressed as:

$$E(Y_1 - Y_0 \mid D = 1),$$

where Y_1 is student outcome in the presence of treatment, Y_0 is the outcome in its absence, and D = 1 indicates treatment (scholarship receipt). This estimation relies on the mean independence assumption that, conditional on observable characteristics X, the expected outcome in the absence of treatment does not depend on one's treatment status, i.e.,

$$E(Y_0 \mid D = 1, X) = E(Y_0 \mid D = 0, X) \text{ for } X \varepsilon S,$$

where S is defined as the overlapping support among the treatment and comparison groups, i.e. $S = Supp(X \mid D=1) \cap Supp(X \mid D=0)$. The average treatment effect (Δ_{ATE}), simply written, is:

$$\Delta_{ATE} = 1/n \sum [E(Y_1 \mid D = 1, X) - E(Y_0 \mid D = 0, X)]$$

Propensity score matching reduces the matching process to a one-dimensional comparison of treated and untreated students with similar propensity scores; as long as the mean independence assumption holds for matching on X, it will also be valid for matching on the propensity score P(X).

The estimation of the marginal treatment effect (Δ_{MTE}) of increasing the number of years of Becas scholarship receipt, from ℓ_1 to ℓ_2 years, uses only treatment group members (i.e., D=1 for all cases in the estimation). This may be simply written as:

$$\Delta_{MTE} = 1/n \sum [E(Y_1(\ell_2) \mid X) - E(Y_0(\ell_1) \mid X)],$$

where $\ell_1, \ell_2 > 0$. Δ_{MTE} is the effect of increasing the duration of scholarship receipt from ℓ_1 to ℓ_2 years for the subset of students who participated at least the length of ℓ_1.

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