EDUCATIONAL POLICY AND PERFORMANCE:

Evaluating the impact of targeted education programs in Ecuador

A thesis submitted by

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(Ecuador)

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To my wife and my kids

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Summary

The thesis has three main parts. The first part presents the experiences of conditional cash transfers (CCT) in Latin America and its impact on access to school. The literature review finds positive and significant effects of these programs on increasing school enrolment as well as on reducing child labor. However, very little is known about whether the impact of those programs comes from the transfer and/or from the condition. The Ecuadorian experience can be very illustrative in this debate. Because the *Bono de Desarrollo Humano* (BDH) is not a conditional cash transfer program but an unconditional cash transfer program the thesis provides fresh evidence on the importance of conditionality. By using a regression discontinuity design combined with a difference-in-difference approach, the research finds no statistically significant effect of the program on school enrolment. However, there are significant differences in consumption and education spending between beneficiaries and non-beneficiaries. Beneficiaries spend more on education than non-beneficiaries, but this difference is not reflected in the enrolment rates. The discrepancy between those results could be attributed to the lack of conditionality.

While there is no effect on enrolment, the increase in school spending could lead to improvements in students' cognitive achievements. In this regard, the second part evaluates the impact of the BDH on test scores. This part provides evidence of the impact of demand side interventions on students' cognitive achievement in Latin America. Two types of demand side interventions coexist in the region; conditional cash transfers and school vouchers. The little empirical evidence available shows no significant effects of CCT on test scores. On the other hand, the evidence for school vouchers is ambiguous. In Chile there are no significant effects, while in Colombia there are significant effects on test scores. As

a contribution, this thesis evaluates the impact of the Ecuador's *Bono de Desarrollo Humano* (BDH) on test scores. By using a regression discontinuity design, as well as a propensity score matching, the research finds no effect of the program on test scores. Once again, this suggests the importance of the conditionality aspect of programs such as the BDH.

The third part reviews the decentralization strategies applied in Latin America and its impact on education quality measured by students' test scores. Broadly, two types of decentralization are found. First, as in the cases of Chile and Argentine, administrative competencies were transferred to local governments. Second, experiences in Central America, where the decentralization process transferred administrative and pedagogical competencies to schools (school-based management), are analyzed. Lastly, this part evaluates the impact of a school-based management program on test scores in rural Ecuador. In this instance the novelty of the Ecuadorian case is that it represents the first school-based management program that explicitly includes objectives related to improving students' cognitive achievements. By combining a pipeline comparison design with propensity score matching the thesis finds evidence of significant positive effects of decentralization on students' test scores in the Hispanic system. However, the chapter also finds evidence of significant negative effects of the program in the Bilingual system probably due to mismatched curricula. While this chapter makes use of the most recent data on decentralization, the available data do not permit a rigorous evaluation and the results presented need to be interpreted in light of the data limitations.

Finally, the thesis emarks and reflects on educational policy in Ecuador. The main policy recommendation extracted from chapters 1 and 2 is the importance of transiting from a cash transfer to a conditional cash transfer program. The main policy recommendation from chapter three is the necessity of considering the program's impact evaluation design at the beginning of its implementation, rather than at the end.

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Introduction

Under the concept of development defined as the "expansion of capabilities of persons to lead the kind of lives they value and have reason to value" (Sen, 2001), education is a key component of a country's development strategy. Education has an intrinsic value, as a human right, as well as an instrumental value because increases in the quantity and quality of educational provision have been associated with a wide range of benefits, which include enhancing productivity, strengthening social participation and democracy, reducing poverty and income inequality, improving health and economic growth, and permitting active inclusion of the excluded. Despite recognition of the importance of education, access to schooling is still a problem in several developing countries. For this reason, the international community subscribed to the Millennium Development Goals where the target of reaching basic education for all by 2015 is included. In addition to the lack of access to education, school quality, measured by standardized tests, is also a concern especially amongst the poor in Latin America.

In the last few decades, education policy across the region has shifted from an emphasis on school enrolment to improving efficiency and quality of education. In this regard, education reform included, among others, components such as conditional cash transfer programs, decentralization, privatization and programs to enhance teachers' incentives.[1] However, problems remain in terms of getting children (especially the poor) to school as well as improving the quality of schooling. For this reason, the effectiveness of alternative interventions to improve educational performance is currently in debate. Many recent impact evaluation studies using new methods have provided additional and more reliable insight into the effectiveness of a variety of education programs. However, the number of studies with robust conclusions is still scarce, and

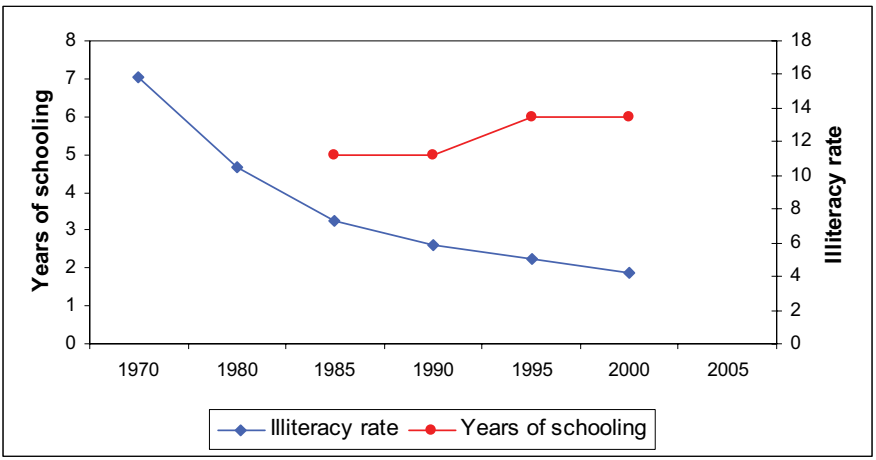
given the importance of evaluating the impact of social policy in general, and of education policy in particular, the task is still quite incomplete.

This introduction is structured as follows. The first part presents some indicators on education performance and education policy in Latin America. The second part introduces the country background and a description of education performance and education policy in Ecuador. The third part discusses some methodological aspects of the thesis and the last part outlines the thesis and highlights its main contributions.

Education performance in Latin America

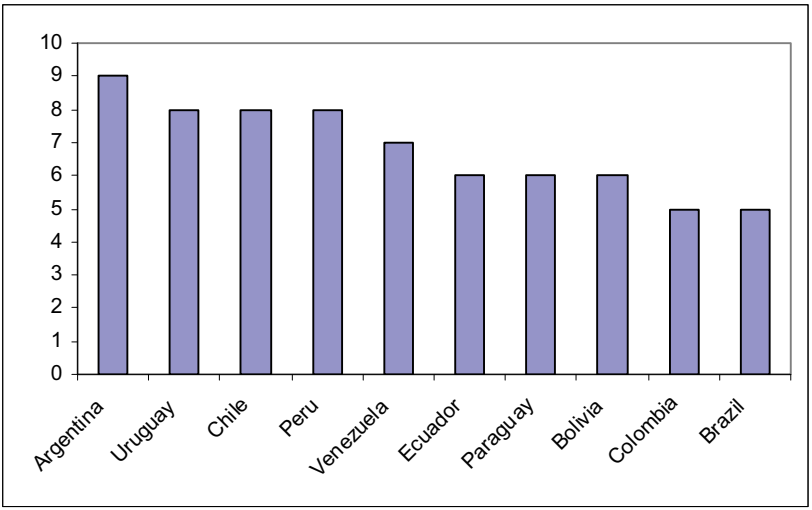
Latin America has improved its educational conditions considerably during the last few decades. As an example, the average years of schooling among those aged 24 and above increased from 5 to 6 years between 1985 and 2000. The illiteracy rate for those aged 15 and above reduced from 16% to 4% between 1970 and 2005. See Figure 1.

Figure 1
Years of schooling and illiteracy rates: Latin America and Caribbean



Source: World Bank. World Development Indicators, on line database.

Figure 2
Average years of schooling: several countries (2000)



Source: World Bank. World Development Indicators, on line database.

Table 1
Years of schooling: Ecuador

	2006
Ecuador	8.1
Area	
Urban	9.5
Rural	5.3
Sex	
Male	8.3
Female	7.9
Race	
Indigenous	4.3
Mestizo	8.4
White	8.7
Afro	7.0
Other	4.9
Consumption level	
Non-poor	9.5
Poor	5.3

Source: Encuesta de Condiciones de Vida, 2006. INEC.

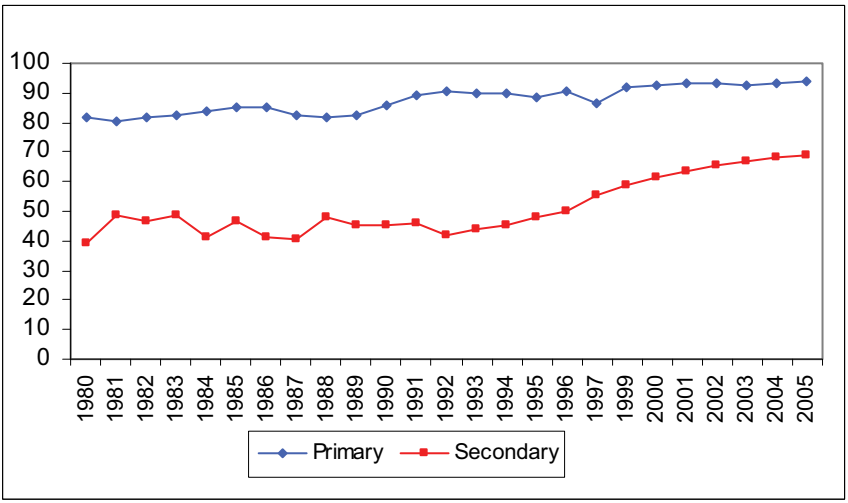
Despite these improvements, disparities remain across countries, as well as within each country. This tendency is illustrated in Figure 2, which shows the average years of schooling for some Latin American countries. Brazil and Colombia are the countries with the lowest level of schooling, while Argentina and Uruguay are the countries with the highest level. Ecuador lies amongst the countries with a low level of schooling.

Regarding disparities within each country, Table 1 introduces, as an example, the average years of schooling for those aged 24 and over in Ecuador. In this case, indigenous people, the inhabitants of rural areas and the poor have the lowest level of schooling.

These achievements and disparities are related to certain structural processes. During the age of import substitution industrialization (ISI), Latin America focused its education policy on school enrolment. As it is well known, the industrialization model was based on the expansion of internal demand for industrialized goods produced at a national level. During this period, one of the key means to achieve social mobility was education. In this regard, universal access to education was the main strategy to both incorporate the population into the development model as well as to increase internal demand. Consequently, the education system was designed to promote universal access to education. However, as already mentioned, the industrialization model did not include all social groups. Indigenous people, the inhabitants of the rural areas, and the poor of the cities were excluded from the model. This development strategy benefited the main social forces behind the model –the industrial entrepreneurs, the middle classes, and the industrial working class. In addition, the education system was in charge of providing the technicians that the industrialization process required. In this regard, technical education was also expanded. As a result, the region witnessed large gains in enrolment rates during the ISI period. Figure 3 demonstrates the net enrolment rate for primary and secondary education in Latin America over the last decades.

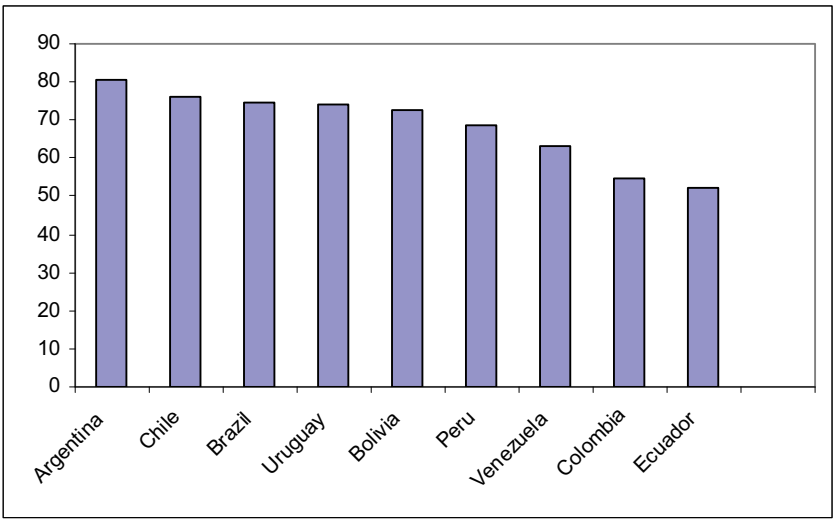
At the primary level, the region has almost achieved universal access to school. However, at the secondary level enrolment rates are still low in comparison to other regions of the world. The average net enrolment rate for secondary school is only around 60% (see Figure 3); and Ecuador has the lowest secondary enrolment rate across the region (see Figure 4).

Figure 3
Net enrolment rate in Latin America: primary and secondary education



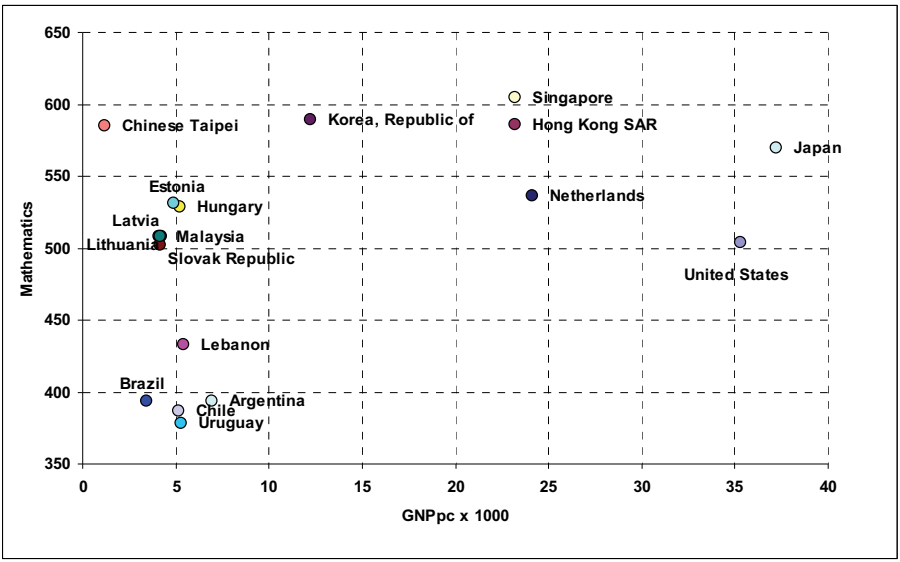
Source: World Bank. World Development Indicators, on line database.

Figure 4
Secondary enrolment rates for some Latin American countries (2005)



Source: World Bank. World Development Indicators, on line database.

Figure 5
Per-capita GDP (thousands of dollars of 2000) and score in math (2003)



Sources: World Bank. World Development Indicators, on line database; and International Association for the Evaluation of Educational Achievement (IEA), Trends in International Mathematics and Science Study (TIMSS), 2003.

With the crisis of the industrialization model, and the stabilization and adjustment policies, the education system started to focus on education quality. The globalization process, as well as the liberalization of Latin America economies during the 1990s, demanded increasing competitiveness in order to achieve active participation in the world market. Contrary to the neoclassical theory of international trade, the openness of the Latin American economies did not increase the demand for the abundant factor (unskilled labor force). In the Latin American context, openness led to an increase in the demand for highly qualified labor. In this regard, improving school quality became a priority in education policy.[2] As a consequence, education policy in this period focused on education quality. Despite the application of such policies during the 1990s, the quality of education in Latin America is still poor. Information from the International Association for the Evaluation of Educational Achievement (IEA) shows that Latin American countries obtained the lowest scores in both math and science. In Figure 5 the association between the

math test score and the per-capita GDP in constant dollars of 2000 is evident. Countries with similar, and even lower per-capita GDP, scored higher in math than the Latin American countries.

Education policy in Latin America

As previously mentioned, the main focus of education policymakers during the 1990s across the region was on improving students' cognitive achievements. In this regard, education policy in Latin America prioritized the following issues: a) decentralization of education, b) improvements of education in terms of equity and quality, c) teachers' incentives, d) the creation of systems of evaluation of students' achievements, and e) demand-side interventions.

Decentralization refers to transferring both administrative and pedagogical competencies from the central government to local governments or schools. Across the region one finds two types of education decentralization. First, decentralization is based on transferring responsibilities to local governments. Chile, Argentina, and Brazil are among the typical examples. The effect of these programs on education quality varies across countries and local governments. The main conclusion that one can extract from these experiences is that the impact of transferring administrative and/or pedagogical competencies to local governments depends on the level of technical, administrative and financial development of local governments. In this regard, the literature indicates that decentralization worked well when local governments had higher levels of technical, administrative and financial resources. Second, decentralization is also based on transferring competencies to schools. The most representative cases come from Central America. For instance, Nicaragua, El Salvador, Guatemala and Honduras have implemented school-based management programs. In these cases one finds positive and significant impacts on school enrolment in remote rural areas. However, no significant effects are found on students' cognitive achievements.

Improving education in terms of equity and quality has generally worked through targeting programs to poor schools in order to improve school infrastructure, to provide textbooks and other school materials, and to support teachers' training. An example of this kind of intervention was the P-900 program of Chile. This program had positive impacts on students' cognitive achievements.

Programs to influence teachers' incentives were implemented in some countries in order to improve pedagogical performance, to achieve teaching quality and to enhance student learning. Some countries in the region, such as Bolivia, Chile and Mexico, have established salary differentials, thereby rewarding teachers for working in rural areas, or have introduced salary structures that reward teachers for improved performance and student learning. El Salvador, Honduras, and Nicaragua devolved their authority to communities, thus granting professional autonomy to schools and teachers in the belief that the increased accountability would lead to higher teacher quality and student outcomes. Unfortunately, studies directed to evaluate the impact of these kinds of programs on school outcomes are not available in the region.

Another important policy in the region during the 1990s was the implementation of systems to evaluate learning. Chile, Honduras, Colombia, Dominican Republic, El Salvador, Brazil, Argentina, Mexico, Venezuela, Costa Rica, Uruguay, Bolivia, Ecuador and Nicaragua implemented evaluation systems during the 1990s. In addition, the region created the Latin American Lab of Evaluation of Education Quality *("Laboratorio Latinoamericano de Evaluación de Calidad de la Educación" (UNESCO/ OREALC))*. These systems provide useful data to carry out studies, which evaluate education policy and analyze the determinants of education quality.

Demand interventions took two paths across the region. On the one hand, some countries implemented conditional cash transfer programs directed to improve access to school among the poor. Mexico, Brazil, Nicaragua, Honduras, Costa Rica, Ecuador, Colombia and Chile are among the countries that have CCT programs in Latin America. The majority of studies evaluating the impact of these programs find significant and positive effects on school enrolment and on reducing child labor. On the other hand, school vouchers were implemented in some countries in order to improve both school access and students' cognitive achievements. In the region only Chile and Colombia have school voucher programs. Impact evaluation studies show ambiguous results. In the Chilean experience there is no significant effect on students' test scores, while in the Colombian case there are significant and positive effects.

In sum, results of education policy in Latin America are not consistent and are generally country specific. Decentralization of education,

based on transferring educational competencies to sub-national units, seems to work well where they have the administrative, financial and technical capabilities to assume educational competencies. On the other hand, school-based management strategies of decentralization seem to have a positive effect on improving school enrolment in Central America, but no significant effect on students' cognitive achievements. Focalized interventions directed to improve poor and/or lagged schools have positive effects on school quality. Conditional cash transfer programs seem to have significant effects on improving school enrolment across the region, however, little evidence is available regarding the impact of CCT programs on students' cognitive achievements. Finally, school vouchers present ambiguous results on education quality.

Education policies and performance in Ecuador[3]

Country background and education performance

Ecuador is a country characterized by high levels of poverty (around 61% using the criteria of unmet basic needs according to the 2001 population census), especially in rural areas (where the incidence is around 86%), as well as high inequality (the Gini coefficient of consumption was 0.46 according to the Living Conditions Survey 2006).

Educational indicators for Ecuador have continued to improve during the 1990s and into the first decade of the new millennium. However, in comparison to previous decades, the speed of educational improvement has slowed down and educational inequality has grown in many respects, except for the gender gap in education which has been almost closed. On average, the adult Ecuadorian population (over 24-years old) has completed 7.3 years of schooling, up from 6.7 years in 1990. Educational levels of the female population have risen much faster than that of males, such that by 2001 the gender gap in terms of years of schooling is limited: 7.5 years for males against 7.1 years for females. See Table 2.

This relatively favorable educational performance comes with a number of important qualifications.

First, the speed of educational performance slowed down significantly during the 1990s as compared to the 1970s and 1980s. Net primary school enrolment, for instance, increased by 20 percentage points during the 1980s (from 68.6 to 88.9%), but only by a meager 1 percentage point

during the 1990s. The same trend is observed for secondary and tertiary school enrolment rates. See Table 3.

Table 2
Ecuador: illiteracy rate and years of schooling of adult population (25 years-older) 1990—2001

	Illiteracy rate		Years of schooling	
	1990	**2001**	**1990**	**2001**
By Gender				
Men	9.5	7.7	7.1	7.5
Women	13.8	10.3	6.3	7.1
By Area				
Rural	20.8	15.5	4.0	4.9
Urban	6.1	5.3	8.3	8.7
By Ethnic Group				
Indigenous	n.a.	28.2	n.a.	3.3
Blacks	n.a.	11.6	n.a.	5.9
Other	n.a.	7.4	n.a.	7.6
National Average	**11.7**	**9.0**	**6.7**	**7.3**

Source: Population Census, 1990 and 2001. INEC.

Table 3
Ecuador: net enrolment rate by educational level 1990 and 2001

	NET ENROLMENT RATES					
	Primary		Secondary		Tertiary	
	1990	**2001**	**1990**	**2001**	**1990**	**2001**
By Gender						
Men	88.6	89.9	42.0	43.9	10.3	11.1
Women	89.2	90.4	44.1	45.4	11.3	12.6
By Area						
Rural	84.4	86.7	23.2	28.8	3.2	4.3
Urban	92.5	92.7	57.7	55.7	15.4	16.2
By Ethnic Group						
Indigenous	n.a.	86.2	n.a.	22.6	n.a.	2.4
Blacks	n.a.	84.4	n.a.	32.3	n.a.	4.5
Other	n.a.	89.8	n.a.	43.2	n.a.	10.9
National Average	**88.9**	**90.1**	**43.1**	**44.6**	**10.9**	**11.9**

Source: Population Census, 1990 and 2001. INEC

Second, the transition rates from primary to secondary education and from secondary to tertiary education are low and did not improve by any significant degree during the 1990s. According to the 2001 population census, net enrolment in primary education was 89%, while secondary education was just 45%. At the tertiary level net enrolment was 12%. See Table 3.

Third, except for the reduced gender gap, important disparities remain and according to several measures educational inequality has risen. The average level of schooling of the rural population is almost half that of the urban population (4.9 compared to 8.7 years) and this gap remained about the same during the 1990s. The education gap is even larger for the indigenous and black population as the average for these groups are estimated at, respectively, 3.3 and 4.5 years of schooling in 2001, well below the national average (7.3 years). (See Table 3).

Fourth, the quality of education is poor. The little information available on test scores shows that students are on average deficient in basic mathematical and language skills. The evidence in table 4 shows scores on a scale of 20 with 13 as a minimum pass score.

Table 4
Ecuador: tests scores for language and mathematics skills 1996 - 2000

	1996	1997	2000
Second grade			
Spanish language skills	10.43	8.24	9.45
Mathematics	9.33	7.21	8.48
Sixth grade			
Spanish language skills	11.15	9.31	9.78
Mathematics	7.17	4.86	6.03
Ninth grade			
Spanish language skills	12.86	11.17	11.70
Mathematics	7.29	5.35	6.01

Notes: Test scores are on a scale of 20 with 13 considered as the pass grade. Ninth grade refers to third year of secondary school.
Source: APRENDO. MEC

Students in 2nd, 6th and 9th grade clearly score substantially lower, particularly for mathematics skills. Between the short period of 1996 to 2000, where data is available, test scores appear to worsen. More disag-

gregated evidence shows that girls score slightly higher in language skills and boys in mathematics skills. Also, students in urban areas outperform those in rural areas, while those in the *Sierra* region do better than their peers in the *Costa* and private school enrollees perform better than those in public schools. For each category average scores are below the minimum.[4] The little comparable data on test scores in the region sketch a gloomy picture of the quality of Ecuador's education. Test scores for Ecuadorian students rank at the bottom for Latin America as Table 5 shows.

Table 5
Test scores of fourth graders primary school in select Latin American countries 1996

Country	Test scores (% of students scoring in each range)				Average score on 100 scale
	0-25	26-50	51-75	76-100	
Argentina	2.7	38.5	54.1	4.8	52.8
Bolivia	4.2	67.4	25.8	2.6	44.2
Costa Rica	0.7	33.9	49.6	15.7	57.5
Chile	2.7	39.8	47.5	9.9	53.6
Ecuador	13.6	59.7	25.2	1.5	41.2
Dominican Republic	2.2	60.7	34.9	2.2	46.8
Venezuela	0.8	30.7	49.8	18.7	59.1
Average of sample	4.1	47.2	40.2	8.5	50.8

Note: Test scores refer to internationally comparable tests taken in the indicated countries. Scores are taken from the average of language and mathematics skills. In Ecuador, the test was taken for a (representative) sample of 3,000 pupils in 159 primary schools across the country.
Source: UNESCO-OREALC. *Por qué, cómo y para qué medir la calidad de la educación*, Volumes I, II, and III. Paris: UNESCO.

Education policy

As in the rest of the region, the Ecuadorian education policy during the 1990s included the following aspects: the reform of basic education, the decentralization strategy, and demand-side interventions.

The reform of basic education

Following a national dialogue with broad participation of stakeholders from civil society and the education sector, consensus was reached re-

garding a major reform of the basic education system in 1996. The reform included the following points:

- A ten-grade basic education system should be introduced, integrating one year of pre-school education, six years of primary and three years of lower secondary education.
- Curriculum reform to improve teaching of Spanish language and mathematics, with greater emphasis on active learning and analytical skills and more attention to the education of social values, multicultural learning and environmental awareness.
- Strengthening of bilingual education for indigenous groups.

The basic education system should help extend the expected years of schooling beyond the traditional six years at the primary level, increase coverage of pre-primary schooling and push for greater gains in external efficiency. However, the introduction of the new system has been slow. By the end of 2006 the basic education system had not yet been introduced throughout the country. The Intercultural Bilingual Education system was created in 1998 and reached coverage of about 92,000 students in almost 2,000 schools by 2005, which corresponds to 5% of the total and about 10% of rural primary school enrolment in that year. There is no precise estimate of how much of the target group (indigenous families) is reached, but given the shares of the rural population which still speak indigenous languages this should be somewhere between two-thirds and three-quarters of the children in that category. Secondary bilingual education was available to 15,900 students in 2005 (about 1% of secondary school enrolment).

The decentralization of education

The most important experiences of decentralization during the 1990s include the creation of 120 Education Matrix centers (*Centros Educativos Matrices*, CEM), with each CEM being made up of 15 to 30 primary schools and kindergartens.[5] Parents and communities have an important say in the management of these school networks. Compared to the previous system where one administrative unit supervised up to 3,000 schools, the Autonomous School Networks program, known as *Redes Amigas*, promotes decentralized school management at the level of a network of 15 to 30 schools administered through a so-called Education Unit Center (EUC). After 1999, this was taken a step further, allowing

schools belonging to the same EUC to form autonomous school networks, manage their own budget and design their own teacher incentive systems. A more detailed description of this program is provided in chapter three.

As part of these programs, education policymakers implemented a system of academic achievement tests (APRENDO) in 1996. However, the system lacked its own budget and was executed only until 2000. In addition, the system faced resistance from the powerful teacher union (UNE), which felt the system would unduly evaluate teacher performance.

Demand-side interventions

In addition to the earlier-mentioned reforms, two social assistance programs have been introduced which have helped promote school enrolment. First, the school meal program (*Colación Escolar*) increased its coverage significantly after the 1999 economic crisis and now reaches about 1.4 million children at the primary school age or about 85% of the target group; this coverage is up from 45% in 1998 (Parandekar, Vos and Winkler, 2002). The majority of beneficiaries come from the poorest families, however, as analyzed in Vos et al. (2003), the near universal coverage of the program also implies significant leakage of benefits to the non-poor. While there is no impact evaluation currently available, the program has most likely had a positive influence on school enrolment, as well as helped keep children in school during the deep economic crisis of the late 1990s. Second, also as an upshot of the 1999 economic crisis, a (conditional) cash transfer program, *Beca Escolar*, was created in 2001 to enhance access of the (rural) poor to primary education. The program started with a pilot of 22,000 beneficiaries (children aged 6-14 years) and reached about 69,000 beneficiaries in primary school by September 2003, or 10-15% of the rural poor of the corresponding age. The targeting mechanism involves a combination of geographical targeting based on composite poverty characteristics (measured through the SELBEN information system) and individual means testing of potential beneficiaries in poor *parroquias* selected through the geographical targeting mechanism. Each beneficiary household receives a cash transfer of US$ 5 per month, per child subject to proof of school attendance.[6]

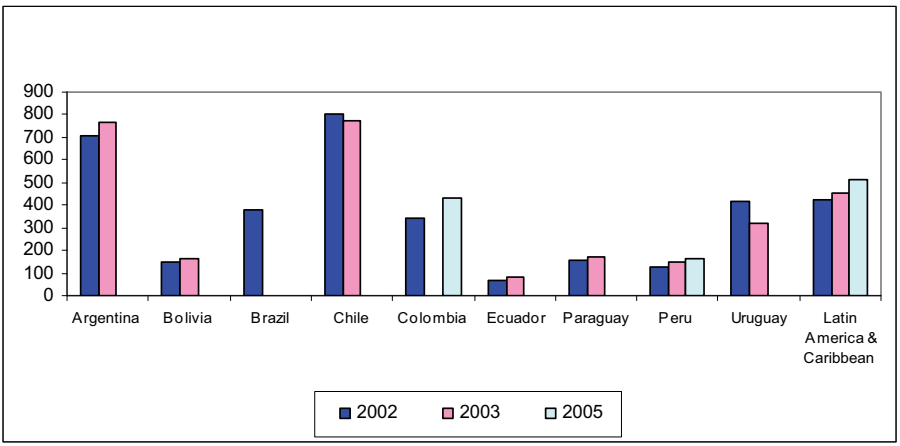
In 2003, a new (conditional) cash transfer program called the *Bono de Desarrollo Humano* was introduced, which is to gradually replace existing

cash transfer systems, including the *Beca Escolar* and the *Bono Solidario*. The conditionality consists of having either children attend primary school or mothers and young children access health centers.[7] This program will be explained in more detail in Chapter 1.

Trends in education expenditures in Ecuador

Social expenditure levels are low in Ecuador compared to other Latin American countries both as a share of GDP and on a per-capita basis (see Figure 6).

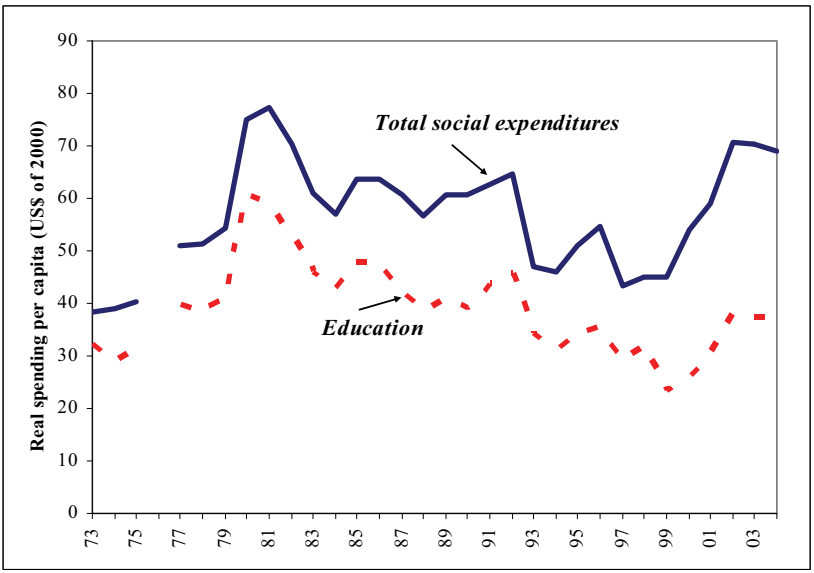
Figure 6
Social spending in Latin America: several years and countries
(US dollars per capita)



Source: World Bank. World Development Indicators, on line database.

In addition, real per capita social expenditure has fallen staggeringly since the early 1980s, and, although there has been a visible recovery since 2000, it currently stands below levels reached a quarter of a century ago (see Figure 7).

Figure 7
Social spending in Latin America (US dollars per capita)



Source: Vos et al. (2003) updated for 2001-3 from Ministry of Economy-UNICEF fiscal database. Public expenditures refer to central government budget only. Social expenditures include education, health, and social assistance (including cash transfer programs).

The decline in social expenditures has had the hardest effect on education and health spending. During the 1990s, the composition of social spending shifted in favor of targeted social protection programs (including the introduction of the cash transfer program *Bono Solidario*) and further against budgets for universal social services in education and health. Between 2001 and 2003, education and health budgets increased significantly, mainly due to various rounds of salary increases for teachers and medical personnel in public service. Figure 7 also shows the dramatic decline in real per capita public spending on education over the past decades. Despite the recovery in recent years, in 2003 real spending was forty percent less than that in 1980. The evidence suggests that has been no shift away from (more expensive) tertiary education to primary and secondary education, and the private sector coverage (23 percent) has not changed since 1996.

Methodological approaches

There are many methodologies that can be used to evaluate education programs and policy. Broadly, the three main methodologies commonly used are cost-benefit analysis, incidence analysis and impact evaluation studies.

Cost-benefit analysis is one type of economic evaluation. It is an attempt to do explicitly what the price mechanism does implicitly (Blaug, 1970). The main idea is to choose investment projects in order of their benefits per unit of costs. Under the cost-benefit analysis of public projects, benefits are computed based on the functioning of the private sector. However, when the benefit is not computable in monetary terms (because the kind of activity is not carried out in the private sector as in the case of defence programs or manpower programs), the cost-benefit analysis becomes a cost-effectiveness analysis. The main difference being that the former is concerned only with economic benefits, whereas the later takes account of all objectives, whether economic or not (Blaug, 1970). The cost-effectiveness analysis, in this regard, compares different interventions in terms of the cost per unit of effect on the outcome(s). Cost-benefit and cost-effectiveness analysis can be carried out before program execution (ex-ante) or after program execution (ex-post).

The incidence analysis has its origins in public finance. The main idea is to identify the beneficiaries of education programs and to evaluate whether these benefits are directed to the poor or not. In general terms, benefit incidence shows whom are benefiting from public services and describes how government spending affects the welfare of different groups of people or individual households (Demery, 2003). It is done by combining information about the unit costs of providing the public services with information on the use of these services.

An impact evaluation study, intends to isolate the effect of an intervention to see whether or not the program or policy is producing the expected results.[8] Impact studies can evaluate the effect of education policies at the macro or micro-level. In the first case, impact evaluation studies capture the effect of education programs or policies on macroeconomic or social variables. One of the main tools used for this type of analysis are the computable general equilibrium models based on the construction of social accounting matrixes.[9] These studies can evaluate the effect of education policies or programs before its implementation (ex-ante evaluation) or after (ex-post evaluation).

On the other hand, at micro-level, one can evaluate the effect of education programs on specific school outcomes at the household-level. In this case one can also made ex-ante evaluations or ex-post evaluations.[10] The main focus of this thesis is on ex-post evaluation of education programs at the micro-level.

In this regard, because expected education outcomes, at the household or individual-level, can be affected by other intervening factors (in addition to the program), it is necessary to create a counterfactual in order to isolate the program effect. A counterfactual indicates what the situation would have been had the program never been executed. The main problem is that the counterfactual is inherently unobserved. It is physically impossible to observe someone in two states at the same time (participating in a program and not participating). Thus, evaluation is essentially a problem of missing data.

Formally, let Y_{1i} be the result for unit i if it did receive the treatment. Y_{01} be the result for unit i if it did not receive the treatment. $T_i \in [0,1]$ is a dummy variable that takes the value of 1 if unit i receives the treatment and zero otherwise.

The observed result of unit i is: $Y_i = Y_{0i} + T_i(Y_{1i} - Y_{0i})$.

In addition, let be X a vector of control variables that influence program participation.

The individual effect is defined by:

$$I_i = Y_{1i} - Y_{0i} \mid T = 1$$

As already mentioned, because it is impossible to observe someone in two states at the same time (participating in a program and not participating), it is only possible to estimate the average effect. The average effect on the treated is the following expected value:

$$I = E(Y_1 - Y_0 \mid T = 1) = E(Y_1 \mid T = 1) - E(Y_0 \mid T = 1)$$

Where, $E(Y_0 \mid T = 1)$ is not directly observed and it is the counterfactual. This counterfactual is estimated by using an adequate comparison (or control) group, which is very similar to the group that receives the treatment. The counterfactual has the same initial conditions and it is influenced by the same environment as the treatment group, but does not receive the intervention. Broadly, there are two types of impact evaluation methodologies; experimental designs, and quasi-experimental stu-

dies. The basic idea of an experimental study is to compare two groups that have no systematic differences except that one group receives the treatment ("treatment group") and the other does not ("control group"). The simplest method is to take a sample of the population of potential beneficiaries and randomly divide the sample into "treatment" and "control" groups ("randomization"). Differences in the variables of interest across the two groups are unbiased estimates of the effect of the treatment. The experimental design is the most robust methodology to evaluate social programs because it allows one to correct the selection process by observables as well as by un-observables, leading to unbiased estimates of program impact. The main requirements for a good experiment are that the initial conditions be the same for the control and the treatment group, and that both groups have to face the same environment during program intervention, with the only difference being that the treatment group receives the benefit, while the control group does not. This is achieved if the initial randomization is correctly implemented. However, experimental designs can face some ethical and political problems, because program intervention is limited to the treatment group, and the control group does not receive the program benefits without any justification.

The second evaluation strategy consists of quasi-experimental designs. These methodologies are not as robust as the experimental designs, but can be very useful to evaluate social programs if one can adequately control the variables that intervene in the selection process (both observables and un-observables). Among the quasi-experimental methodologies most commonly used are the propensity score matching (PSM), the instrumental variables design (IV), and the regression discontinuity studies (RD). This thesis uses, as methodology strategies, regression discontinuity and propensity score matching. A regression discontinuity design is used in chapters 1 and 2; meanwhile a propensity score matching is used by chapter 2 and 3. One important point related to quasi-experimental studies is the importance of combining different methodologies in order to achieve robust conclusions. This thesis takes this into account by combining several methodologies in each chapter. A more detailed explanation of each methodology is introduced in every chapter.

Thesis contribution and outline

The contribution of this thesis is twofold. First, the thesis contributes to the field by evaluating the impact of major educational initiatives in Ecuador and proposing some policy recommendations. Second, in methodological terms, the thesis shows the sensitivity of quasi-experimental evaluation approaches, and highlights the necessity of combining different approaches, as well as the importance of having good quality data to obtain more credible policy conclusions.

The thesis is organized as follows. Chapter 1 presents the experiences of CCT in Latin America and its impact on access to school. The literature review finds positive and significant effects of these programs on increasing school enrolment as well as on reducing child labor. However, very little is known about whether the impact of those programs comes from the transfer and/or from the condition. The Ecuadorian experience can be very illustrative in this debate. Because the *Bono de Desarrollo Humano* (BDH) is not a conditional cash transfer program but an unconditional cash transfer program the chapter provides fresh evidence on the importance of conditionality. By using a regression discontinuity design combined with a difference-in-difference approach, the chapter finds no statistically significant effect of the program on school enrolment. However, there are significant differences in consumption and education spending between beneficiaries and non-beneficiaries. Beneficiaries spend more on education than non-beneficiaries, but this difference is not reflected in the enrolment rates. The discrepancy between those results could be attributed to the lack of conditionality.

While there is no effect on enrolment, the increase in school spending could lead to improvements in students' cognitive achievements. In this regard, Chapter 2 evaluates the impact of the BDH on test scores. This chapter provides evidence of the impact of demand side interventions on students' cognitive achievement in Latin America. Two types of demand side interventions coexist in the region; conditional cash transfers and school vouchers. The little empirical evidence available shows no significant effects of CCT on test scores. On the other hand, the evidence for school vouchers is ambiguous. In Chile there are no significant effects, while in Colombia there are significant effects on test scores. As a contribution, this chapter evaluates the impact of the Ecuador's *Bono de Desarrollo Humano* (BDH) on test scores. By using a regression discontinuity

design, as well as a propensity score matching, the chapter finds no effect of the program on test scores.

Chapter 3 reviews the decentralization strategies applied in Latin America and its impact on education quality measured by students' test scores. Broadly, two types of decentralization are found. First, as in the cases of Chile and Argentine, administrative competencies were transferred to local governments. Second, some experiences of Central America, where the decentralization process transferred administrative and pedagogical competencies to schools (school-based management), are analyzed. Lastly, this chapter evaluates the impact of a school-based management program on test scores in rural Ecuador. In this instance the novelty of the Ecuadorian case is that it represents the first school-based management program that explicitly includes objectives related to improving students' cognitive achievements. By combining a pipeline comparison design with propensity score matching the chapter finds evidence of significant positive effects of decentralization on students' test scores in the Hispanic system. However, the chapter also finds evidence of significant negative effects of the program in the Bilingual system probably due to mismatched curricula. While this chapter makes use of the most recent data on decentralization, the available data do not permit a rigorous evaluation and the results presented need to be interpreted in light of the data limitations.

Finally, the last chapter of the thesis contains concluding remarks and reflections on educational policy in Ecuador.

Notes

[1] A more detailed analysis of education reform in Latin America will be introduced below.

[2] It is important to highlight that the increase in the demand of highly qualified labor increased the wage gap between skilled and unskilled workers, which at the same time led to an increase in inequality across the region (Ganuza, Morley, Robinson and Vos, 2004).

[3] This part is based on Vos and Ponce (2004).

[4] The exceptions are for language skills of ninth graders in the Sierra (1996 only) and of students in private schools in sixth and ninth grade (1996 only). In all other cases scores are below the minimum.

[5] These reforms were implemented through programs (*EB/PRODEC* and *Redes Amigas*) supported by the World Bank and the IDB.

[6] The program never implemented mechanisms to enforce the complying of the conditionality.

[7] Specifically, the conditions for the target group are that:
- In education: the nuclear family has children in age 6-15, which are enrolled in school and are registered to attend at least 90% of classes;
- In health: the nuclear family has children in the age group of 0-6 years who should be receiving bi-monthly health controls at designated health centres;
- For families with children in both age groups, the conditions for education prevail in order to receive the cash transfer. However, as already said, the program has never implemented the mechanisms to enforce the complying of the conditions.

[8] For an introduction to impact evaluation see Moffitt (1991), Ravallion (2001), Baker (2000), Bourguignon and Pereira (2003), and Ravallion (2005).

[9] See Bourguignon and Pereira (2003) for a detailed review of some techniques to evaluate social policies at macro level.

[10] See Bourguignon, Ferreira and Leite (2002) for a review of ex-ante evaluation techniques.

1 The impact of (unconditional) cash transfers on school enrolment: Evidence from Ecuador*

ABSTRACT. Evaluations of conditional cash transfer programs in several Latin American countries indicate that such programs have substantial positive effects on school enrolment. It is unclear, however, whether it is the cash transfer itself, or the conditionality that matters most. This chapter presents fresh evidence from a cash transfer program in Ecuador. Unlike programs in other countries, the transfers are **un**conditional. Using a regression discontinuity design, we find a precisely estimated zero effect of eligibility on school enrolment. This suggests that the success of other programs should be attributed to the requirement that children attend school. In this regard, this chapter contributes to the current debate by highlighting the importance of conditionality to improve school enrolment through this type of program.

* This chapter is based on a paper co-written with Hessel Oosterbeek from the Amsterdam School of Economics and the Tinbergen Institute.

1.1 Conditional cash transfers and school enrolment in Latin America

Conditional cash transfer programs provide cash transfers to poor families conditional on the children of these families attending school and/or visiting health care centers. The attractiveness of these programs is the potential to combine short and long-term poverty reduction. The cash transfers reduce short-term poverty, while long-term poverty will be reduced if children of poor families acquire human capital.

A number of countries in Latin America have implemented conditional cash transfer programs to combat poverty. Countries that have adopted such programs include Brazil (in 1995), Mexico (1997), Honduras (1998), Nicaragua (2000), Costa Rica, Colombia (2001), Argentina, Uruguay, Chile and Jamaica. Rawlings and Rubio (2003) and Caldés et al. (2004) provide overviews of the various programs.

Some of these programs have been assessed through impact evaluation studies. These studies show substantial positive effects of conditional cash transfers on school enrolment. The programs in Mexico and Nicaragua have been evaluated using randomized field experiments. In Mexico enrolment rates at the secondary level increased from 67% to around 75% for girls and from 73% to around 78% for boys (Schultz 2004). In Nicaragua the program was targeted to pupils up to fourth grade in primary school. The program increased the enrolment rate for this group by 18 percentage points (Maluccio and Flores 2004).[1]

Other programs have been evaluated using non-experimental research designs. Duryea and Morrison (2004) used propensity score matching to evaluate the program in Costa Rica, and find a 5 to 9 percentage points increase in the probability of attending school. Attanasio et al. (2006) have evaluated the program in Colombia using propensity score matching in a difference-in-differences framework. They find an increase in school enrolment of 5 to 7 percentage points for 14 to 17 years old.

Given these successes of conditional cash transfer programs, one may ask whether the driving force is the cash transfer itself that enhances school enrolment, or the requirement that children attend school. If the cash transfers themselves are sufficient, resources can be saved by abandoning costly monitoring of school attendance. Moreover, such a finding shows the importance of liquidity constraints for school enrolment. On the other hand, if cash transfers do not matter, this indicates that liquidity constraints are not the source of low school attendance. Finally, if

families behave differently under conditional and unconditional cash transfer programs, this indicates that the government reduces families' welfare by making the cash transfers conditional. This is only justified if families behave sub-optimally.

De Brauw and Hoddinott (2007) attempt to disentangle the cash transfer from the school attendance requirement by exploiting the fact that some treated families in Mexico did not receive the forms needed to monitor the attendance of their children at school. They find that the absence of such forms reduced the likelihood of children attending school, suggesting that the requirement matters. Since the reason for not receiving forms is unknown, it is unclear whether the two types of families can be compared.

This chapter takes a different approach. We investigate the impact of the cash transfer program in Ecuador using a regression discontinuity design. Unlike the programs implemented in other countries, this program does not require children of treated families to attend school. We assume that if the program in Ecuador would have been a normal conditional cash transfer program, it would have produced effects similar to those in other Latin American countries. This implies that if we find that the unconditional cash transfers in Ecuador have effects of the same magnitude as the conditional cash transfers in other countries, we interpret this as the school attendance requirement having no effect. Likewise, if we find that unconditional cash transfers have no impact on school enrolment, then we conclude that all effects of conditional cash transfers should be attributed to the school attendance requirement.

At the start of the program in Ecuador some television programs mentioned the obligation of parents to send children to school in order to receive the transfer. The obligation was, however, never put into practice. Schady and Araujo (2007) use individuals' unawareness of the absence of the requirement to identify the effect of the requirement and find a positive effect. It is questionable, however, whether badly informed families are comparable to others.[2]

The remainder of the chapter is organized as follows. The next section describes the program in Ecuador in more detail and provides information about the specific context. Section 3 describes the empirical approach adopted in this paper. Section 4 describes the data. Section 5 presents and discusses the empirical results. Section 6 summarizes and concludes.

1.2 The Ecuadorian program

Ecuador is a lower-middle income country, characterized by high poverty levels and high inequality. During the last decades education levels have gone up. For example, between 1982 and 1990, enrolment increased from 68.6% to 88.9% for primary schools and from 29.5% to 43.1% for secondary schools. Moreover, the average number of years of schooling of the population aged 24 years or older increased from 6.7 to 7.3 between 1990 and 2001. Despite these improvements, the country faced a serious problem with school enrolment during the 1990s. In 2001 enrolment at primary and secondary levels stagnated around the values of 1990. This disappointing performance contrasts with aspirations. The 1990s was the decade of "Education for All", and Ecuador subscribed to several international declarations emphasizing the importance of education. In addition, at the end of the 1990s, the Ecuadorian government engaged in new programs aiming to improve access to primary education and school achievements. Paradoxically, educational inputs showed remarkable improvements during the same period. The pupil-teacher ratio for primary education declined from 30 in 1990 to 23 in 2001.

In 1998, the government in Ecuador launched a program called *Bono Solidario*. This program started as a safety net to compensate poor families for the elimination of gas and electricity subsidies. Initially the program used a self-targeting strategy directed at mothers with earnings below USD 40, people with disabilities and senior citizens. While the immediate political justification for this program was to compensate the poor for losses in their purchasing power caused by statutory increases in (heavily subsidized) petroleum and natural gas prices, the program quickly took on a life of its own, becoming the government's largest social expenditure outside of education, with total transfers equal to about one percent of the GDP (Vos et al., 2001). The transfer was modest, but non-trivial by Ecuadorian standards. At the time that the program started, mothers received about USD 15 per month, and senior citizens and people with disabilities received USD 7.50. On average, the share of *Bono* income in total household expenditures was 11 percent in 1999. During 2000, the program reached around 1.2 million beneficiary households, representing about 45 percent of Ecuadorian households.

Vos et al. (2001) evaluated *Bono Solidario* using propensity score matching. After finding important targeting errors in the program, they report

a positive impact of around 5 percentage points on school enrolment, although no significant impact was found on poverty indicators. Using an instrumental variables approach, León and Younger (2007) find that the program had very minor, yet significant positive effects on children's nutritional status. The instruments include an interaction of three program criteria (because one must meet all three criteria to receive the transfer payment): dummy variables for whether the household has monthly income less than $150, has no workers in the formal sector and has a mother of children younger than 18; a measure of the time that it takes to reach a bank branch, where the Bono is collected; and a dummy variable for households interviewed before April 1999, when the value of the Bono was increased.

At the end of the 1990s the government implemented another program called *Beca Escolar*. This program consisted of the transfer of USD 5 per child (up to two children per household), conditional on their enrolment in school and attendance of at least 90% of the school days. This program has never been evaluated.

In 2003, the two programs were reformulated and incorporated under a new program called *Bono de Desarrollo Humano* (BDH). The main objectives of the new program were to improve the targeting efficiency of the old programs, as well as the formation of human capital among poor families in Ecuador. The program has two components: education and health. The education component aims at children from the ages of 6 to 15 to enroll in school and attend at least 90% of the school days. The health component aims at children under 6-years old to attend health centers for medical check-ups. Unlike other programs in Latin America, up until 2006 the program had no mechanisms to verify attendance in school and in health care centers. Families are not taken off program rosters if their school-aged children are not enrolled in school or fail to attend classes regularly. Consequently, the program is best characterized as an unconditional cash transfer program instead of a conditional cash transfer program.

BDH uses an individual targeting strategy to select beneficiaries based on a poverty index (called *Selben* index). This index identifies potential beneficiaries of social programs by classifying families according to their unmet basic needs. The poverty index is computed using non-linear principal components analysis. Families pertaining to the poorest two quintiles (below 50.65 in the poverty index) receive the program. Cur-

rently, the program consists of a cash transfer of USD 15 per family per month. The annual budget of the program reached USD 190 million in 2004 (around 1% of GDP).

1.3 Empirical approach

As we had argued, a goal of the reform was to correct the program's targeting problems. In 2003 the Ecuadorian government decided to expand the coverage of the new program focusing on those from quintiles 1 and 2 in the Selben index. The government decided to evaluate the program's impact using a regression discontinuity design. The initial design of the program established two different amounts: USD 15 for families in the lowest quintile and USD 11.5 for in the second quintile.[3] The difference around the 40th percentile can be exploited to estimate the impact of the cash transfer per se, while the difference around the 20th percentile can be exploited to estimate the impact of different amounts of the cash transfer.

Once the research was designed and the baseline survey was conducted, the government decided to grant all families in the bottom two quintiles USD 15. Due to this, the design no longer permits evaluation of the impact of different amounts of the transfer. Instead it was decided to use a randomized design to evaluate the impact of those around the 20th percentile of the poverty index. Potential beneficiaries around this point were randomly assigned to treatment and control. Schady and Araujo (2005 and 2007) use this experimental design for their evaluation. We discuss their findings in more detail below where we compare them to our findings.

This chapter exploits the remaining of the original evaluation design, namely the discontinuity around the 40th percentile, in a regression discontinuity design.[4] In addition, the thesis combines an RD design with a difference in difference approach.[5]

The identifying assumption is that conditional on a flexible function of the poverty index and other observables, eligibility for treatment is random for families with a poverty index close to the 40th percentile. More formally, we will estimate equations of the following type using instrumental variables.

$$Y_{i,t} = X_{i,t-1}\beta + f(P_{i,t-1}) + \delta T_{i,t} + u_{i,t} \quad (1)$$

Where Y is school enrolment which takes a value of 1 if a child is enrolled and 0 otherwise, X is a vector of individual, household and community level characteristics, $f(P)$ is a flexible function (first, second and third degree polynomial) of the poverty index,[6] T is an indicator variable taking the value of 1 if the person receives the treatment and 0 otherwise, and u the error term. Subscript i indicates the child, t indicates the time period when the follow-up survey was conducted, t-1 refers to the baseline period.

In a standard regression discontinuity design one compares observations just below and just above the cutoff. We do this by restricting the analysis to observations that have their poverty index within a certain range around the cutoff. Widening this range increases the number of observations, but makes at the same time the treatment and control group more different. By presenting results for different ranges around the cutoff we examine the sensitivity of our results in this regard.

It turns out that not all families that receive the transfer meet the poverty index requirement. Likewise, not all families that meet this requirement received the transfer. This implies that the design is not a sharp regression discontinuity design but is instead a fuzzy design. There is not a deterministic relation between the poverty index and treatment but a probabilistic one. To address this we apply an instrumental variables approach where receipt of the cash transfer is instrumented by eligibility. This means that we will estimate a first stage equation in which the endogenous variable T in equation (1) is instrumented by the dummy variable eligibility (Z), which takes value 1 if the poverty index is below the cutoff and 0 otherwise.[7] The identifying assumption is then that

$$E(Z_{i,t} \cdot u_{i,t} \mid X_{i,t-1}, P_{i,t-1}) = 0 .$$

As already mentioned, one novel point in methodological terms is that since we have pre-intervention and post-intervention measures of outcomes, we can also combine the regression discontinuity design with a first difference approach. To this end, we estimate equations of the following form:

$$\Delta Y_{i,t} = X_{i,t-1}\lambda + f\,(P_{i,t-1}) + \gamma T_{i,t} + \Delta u_{i,t} \qquad (2)$$

Where ΔY is the change in school enrolment which takes a value of 1 if a child is enrolled at t and not enrolled at t-1, of 0 if the enrolment sta-

tus is the same at t and t-1, and of –1 if a child is enrolled at t-1 but not at t. Specification (2) allows changes of Y to be affected by X and $f(P)$[8].

In addition to equations (1) and (2) we will also present results from reduced form estimations. These equations have a similar specification as equations (1) and (2), except that T is replaced by Z. The reduced form equations recover the effect of the intention to treat (ITT).

1.4 Data

The sample for the evaluation of the BDH program in this chapter was drawn from the Selben rosters of four of the twenty-two provinces in the country: Carchi, Imbabura, Cotopaxi, and Tungurahua. All four provinces are in the sierra (or highlands) region of the country. All households that had previously received transfers from the Bono Solidario program were excluded because it was unclear how earlier transfers could have affected schooling decisions. To exploit the discontinuity in eligibility around the poverty index of 50.65, families with a poverty index between 47.65 and 53.65 were drawn. The data was collected by the Pontificia Universidad Católica del Ecuador, an independent firm that had no association with the program[9]. The sample design uses a two-stage procedure. Within the provinces, parishes were randomly drawn and, within these parishes, a random sample of households was taken. Finally, the sampling scheme selected only households with at least one child aged between 6 and 15 years old at the time of the baseline survey.[10]

The survey includes one record for each household member including their gender, age and relation to the head of the household. The survey also contains information on the level of schooling, the parents' level of schooling, marital status, and language spoken by all household members. For children aged between 5 to 17 years, the survey includes information on current enrolment (level and grade), causes in the case of no enrolment, and other variables related to the type of school the child attends, as well as some variables related to labor market status. Finally, the survey includes a complete module of household expenditures, which replicates the structure of the 1999 Ecuador LSMS; as well as an indicator that takes the value of 1 if the person belongs to a household that receives the cash transfer, and 0 otherwise.

Attrition is low and 96% of the households interviewed at the baseline were interviewed again in the follow-up survey. No significant dif-

ferences are found between households who were and were not interviewed. Attrition can introduce biases when correlated with treatment status (Angrist, 1997). A regression of an attrition indicator on treatment status has a coefficient of 0.0012 (s.e. 0.11), indicating that attrition will not bias our results.

The sample is restricted to children aged 7 to 16 years when they live in households that responded to the follow-up survey. This results in a sample of 2,384 children in 1,221 households.[11] Table 1.1 presents descriptive statistics for eligible and ineligible children/households. It does this separately for two samples. Columns 1-3 pertain to the full sample of children/households who have a poverty index at most three points different from the 40th percentile cutoff. Columns 4-6 are for the restricted sample of observations whose poverty index is at most one point different from the cutoff.

Table 1.1
Descriptive statistics by eligibility status: different discontinuity samples

Variable	*DS±3points*			*DS±1point*		
	Eligible (1)	Not eligible (2)	p-value (3)	Eligible (4)	Not eligible (5)	p-value (6)
School enrolment pre intervention	0.85	0.86	0.625	0.87	0.84	0.291
Child's age	11.91	12.00	0.498	11.82	12.31	0.009
Child is female	0.53	0.52	0.787	0.51	0.55	0.258
Log of per capita expenditures	2.92	3.07	0.000	2.94	3.01	0.034
Poverty index	49.42	51.88	0.000	50.17	51.11	0.000
Father's education	5.68	6.16	0.000	5.91	5.94	0.840
Mother's education	5.28	5.92	0.000	5.69	5.41	0.210
Head of household is male	0.85	0.87	0.307	0.84	0.83	0.562
Head of household is indigenous	0.09	0.06	0.002	0.08	0.04	0.025
Head of household can read and write	0.94	0.96	0.161	0.94	0.94	0.693
Household size	5.63	5.58	0.422	5.69	5.61	0.449
Number of children	1394	990		636	394	

Some of the variables listed in Table 1.1 serve as an input in the construction of the poverty index or are highly correlated with the poverty index. This is the case for head of household being indigenous, log of per capita expenditures and parents' education. It is therefore not surprising that we find significant differences for these variables between the groups below and above the cutoff. This suggests that treatment and control groups in this research design may be too different to compare. Recall, however, that the identifying assumption of the regression discontinuity design is that there are no systematic differences between treatment and control groups conditional on covariates (including a flexible function of the poverty index). Hence, differences in observed characteristics need not invalidate the research design.

When we restrict the sample to observations no more than one point from the cutoff, the eligible and non-eligible groups become more similar on most variables. This is evidenced by the *p*-values in column 6. For the poverty index, per capita consumption, and head of household being indigenous we still find (hardly surprising) significant differences. But these differences are reduced in size. The significant differences on parents' education have vanished. On the other hand, there appears now to be a significant difference in age between observations above and below the cutoff in the restricted sample. Omitting this variable as a control would bias the impact estimates upwards since older children are less likely to attend school.

The results in Table 1.1 show that enrolment rates in our sample are around 0.85. Any impact estimate should be regarded relative to current enrolment rates, since there are obvious ceiling effects.

1.5 Results

First stage

The first thing that we need to establish is the (first stage) effect of eligibility of the cash transfer on the actual receipt (treatment) of it. Out of a total of 537 families that were not eligible, 41 (8%) received the cash transfer. Out of 684 families that were eligible, 178 (26%) didn't receive the cash transfer. Hence for 18% of the families, eligibility and treatment-status do not coincide.[12]

Figure 1.1 plots the relation between the poverty index, eligibility and the probability of treatment. The discontinuity in the probability of

treatment at the eligibility cutoff is evident. Closely around the point where the poverty index equals 50.65, the probability of treatment drops by around 60 percentage points. Notice further that the relation between the actual receipt and the poverty index is almost flat at both sides of the cutoff. This indicates that the probability of treatment is independent of the poverty index conditional on the eligibility index.

Figure 1.1
First stage relation between poverty index and treatment

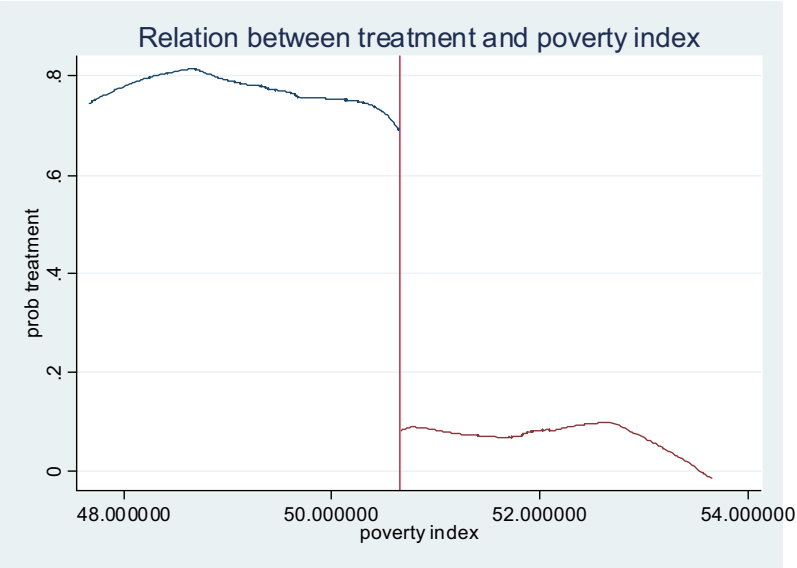


Table 1.2 shows these findings more formally for various specifications of the first stage relationship. The top panel contains the results for the full sample. Column (1) contains no control variables, column (2) adds controls for background characteristics (as those defined in Table 1.1), and column (3) adds a third degree polynomial of the poverty index. Even in this latter specification, the coefficient of eligibility status is not lower than 0.64, and is always significantly different from zero. Conclusions are the same throughout the three specifications showing the robustness of our results. The F-value for the instrument is never below

148. The flatness of the relation between treatment and poverty index at both sides of the cutoff is expressed by the low F-value for a joint test on the significance of the three poverty index terms. We cannot reject the hypothesis that conditional on other variables, the joint effect of these three terms equals zero.

Table 1.2
First stage results (program participation).

Variable	***(1)***	***(2)***	***(3)***
		Full Sample	
Eligibility status	0.694*	0.681*	0.648*
	(0.022)	(0.022)	(0.053)
R squared	0.469	0.510	0.511
F-value for instrument	1030.0*	955.8*	148.9*
F-value for poverty index terms			0.06
		Restricted Sample	
Eligibility status	0.680*	0.670*	0.635*
	(0.034)	(0.034)	(0.064)
R squared	0.436	0.513	0.513
F-value for instrument	404.3*	393.3*	97.3*
F-value poverty index terms			0.23
Controls	None	X	X, f(P)

Note: OLS estimates. Robust standard errors in brackets. * indicates significance at the 1% level. Number of observations equals 2384/1030 for full/restricted sample. Specification (1) contains no control variables, specification (2) adds controls for background characteristics (as those defined in table 1.1), and specification (3) adds a third degree polynomial of the poverty index.

The bottom panel of Table 1.2 reports results for the same first stage specifications when the sample is restricted to children in families that are no more than 1 point from the poverty rate cutoff. Point estimates are very similar to those for the full sample: effects are very significant, F-values for significance of the instrument are never below 97, and the poverty index (polynomial) has - conditional on eligibility status - no significant impact on treatment.

Reduced form

Table 1.3 shows the reduced form results for the full sample. We present results for different specifications corresponding to those in the previous tables. The top panel reports results for the levels specification, while the bottom panel reports results from specification in which the dependent variable is measured in first differences. In all specifications the point estimates are small and never significantly different from zero.

Table 1.3
Reduced form results for school enrolment (full sample)

Variable	***(1)***	***(2)***	***(3)***
		Levels	
Eligibility status	-0.009	0.002	0.013
	(0.017)	(0.015)	(0.035)
R squared	0.000	0.289	0.289
F-value poverty index terms			0.06
		First Differences	
Eligibility status	-0.002	-0.003	0.026
	(0.015)	(0.015)	(0.034)
R squared	0.000	0.123	0.124
F-value poverty index terms			1.12
Controls	None	X	X, f(P)

Note: OLS estimates. Robust standard errors in brackets. Number of observations equals 2384. Specification (1) contains no control variables, specification (2) adds controls for background characteristics (as those defined in table 1.1), and specification (3) adds a third degree polynomial of the poverty index.

Going from the first to the last columns we observe that adding more control variables makes the point estimate less negative or more positive. Differences between the point estimates in the different columns are, however, insignificant. For the results in the final columns we tested for the joint significance of the poverty index polynomial. We cannot reject that the joint effects of these three terms equals zero. On the basis of efficiency considerations, we should therefore prefer the results in column (2). The standard error on the impact estimate in that column is

substantially smaller than the standard error on the impact estimate in the final column.

Also for the restricted sample, none of the estimates in Table 1.4 differs significantly from zero. In this sample, adding more control variables makes the estimated impacts less positive or more negative. Like in the larger sample, we cannot reject the hypothesis that the joint effects of the three poverty index terms equals zero. Hence, for reasons of efficiency we prefer the results in the second column to those in the third.

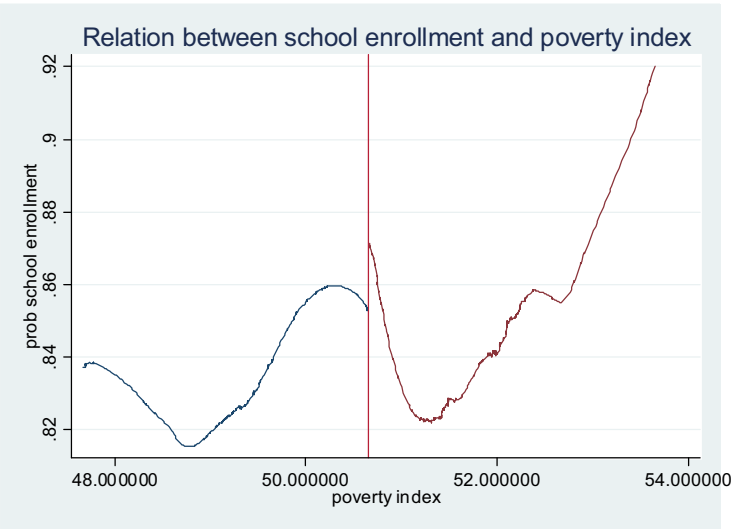
Table 1.4
Reduced form results for school enrolment (restricted sample)

Variable	*(1)*	*(2)*	*(3)*
		Levels	
Eligibility status	0.041	0.022	-0.048
	(0.027)	(0.025)	(0.047)
R squared	0.003	0.290	0.293
F-value poverty index terms			1.61
		First Differences	
Eligibility status	0.018	0.007	-0.023
	(0.023)	(0.024)	(0.046)
R squared	0.001	0.155	0.156
F-value poverty index terms			0.31
Controls	None	X	X, f(P)

Note: OLS estimates. Robust standard errors in brackets. Number of observations equals 1030. Specification (1) contains no control variables, specification (2) adds controls for background characteristics (as those defined in Table 1.1), and specification (3) adds a third degree polynomial of the poverty index.

Figure 1.2 illustrates the reduced form without any controls using data from the full sample. There appears to be no impact of eligibility status on school enrolment. This confirms the findings from Tables 1.3 and 1.4.

Figure 1.2
Reduced form relation (school enrolment and poverty index)



In the current application the reduced form results have a clear policy interpretation. These estimates show the effect on the group that the program was intended to serve (the effect of the intention to treat). Our preferred estimates for the full sample give point estimates equal to 0.002 and –0.003. These estimates are quite precisely measured (s.e. 0.015). An increase in school enrolment as small as 3 percentage points can therefore be excluded with 95% probability as impact estimate. Also, for the restricted sample the intention to treat effects are small. The point estimates equal 0.022 (s.e. 0.025) and 0.007 (s.e. 0.024)

Instrumental variable estimation

Tables 1.5 and 1.6 report the IV results for the full sample and the restricted sample, respectively. As already mentioned the instrument used is the assignment rule (1 for those below the cutoff and 0 otherwise). Point estimates are equal to the reduced form estimates (in Tables 1.3 and 1.4) divided by the first stage coefficient in the corresponding column (in Table 1.2). None of the impact estimates are significantly different from zero, implying that we cannot reject the hypothesis that receipt of the cash transfer has no impact on school enrolment.

Table 1.5
IV results for school enrolment (full sample)

Variable	***(1)***	***(2)***	***(3)***
	Levels		
Actual beneficiaries	-0.013	0.003	0.019
	(0.025)	(0.022)	(0.056)
R squared	0.000	0.289	0.288
F-value poverty index terms			0.06
	First Differences		
Actual beneficiaries	-0.003	-0.004	0.043
	(0.022)	(0.022)	(0.055)
R squared	0.000	0.123	0.123
F-value poverty index terms			0.95
Controls	None	X	X, f(P)

Note: Robust standard errors in brackets. Number of observations equals 2384. Specification (1) contains no control variables, specification (2) adds controls for background characteristics (as those defined in Table 1.1), and specification (3) adds a third degree polynomial of the poverty index.

Table 1.6
IV results for school enrolment (restricted sample)

Variable	***(1)***	***(2)***	***(3)***
	Levels		
Actual beneficiaries	0.061	0.032	-0.076
	(0.040)	(0.037)	(0.076)
R squared	0.000	0.291	0.283
F-value poverty index terms			1.53
	First Differences		
Actual beneficiaries	0.026	0.011	-0.036
	(0.034)	(0.036)	(0.072)
R squared	0.000	0.155	0.155
F-value poverty index terms			0.30
Controls	None	X	X, f(P)

Note: Robust standard errors in brackets. Number of observations equals 1030. Specification (1) contains no control variables, specification (2) adds controls for background characteristics (as those defined in Table 1.1), and specification (3) adds a third degree polynomial of the poverty index.

For both samples we cannot reject that the joint effect of the three poverty index terms equals zero and therefore prefer the outcomes presented in the second columns. For the results obtained using the full sample this excludes – with 95% probability – that receipt of the cash transfer raises school enrolment by more than 4.6 (levels specification) and 3.9 (first difference specification) percentage points. For the restricted sample the respective figures are 10.4 and 8.2.

Our findings on the effect of the cash transfer can be compared with those reported by Schady and Araujo (2005). As previously mentioned, these authors use data from an experiment in which potential beneficiaries around the 20th percentile were randomly allocated to treatment and control. They report a significantly positive average effect, but this effect is concentrated among the poorest in their sample. For instance, they report a significant effect equal to 0.066 (s.e. 0.022) for children from families with below 20th percentile (median in their sample) per capita expenditures and an insignificant effect equal to 0.012 (s.e. 0.022) for children from families with above 20th percentile (median in their sample) per capita expenditures. The children in our sample come from families around the 40th percentile of per capita expenditures. Taken together, this clearly suggests that the effect of the cash transfer is heterogeneous and is larger for poorer families. Our estimate of a zero effect should thus not be interpreted as an estimate of the average effect of the program but rather as an estimate of the effect for children from families close to the 40th percentile cutoff. This result is policy relevant as it suggests that expanding the program to higher percentiles does not lead to an increase in enrollment, or in other words, at least from the perspective of school enrollment, the cutoff to qualify for program benefits may be lowered.

What did they do with the cash?

The results presented so far establish that unconditional cash transfers in Ecuador do not have a significant impact on school enrolment. In this subsection we address the question of how the families that received the cash transfer spent it. This is relevant in its own right. Is the transfer spent in a way that also (potentially) benefits the children in the families? Moreover, by looking at alternative outcomes we examine whether the research design applied in this paper is able to detect any impact.

Table 1.7 reports results for five separate spending categories for the full sample and the restricted sample, as well as for levels and changes in specifications.

Table 1.7
IV estimates of effects on log expenditures: both samples

Variable	*Food*	*School*	*Nonfood*	*Housing*	*PC*
Full sample					
Levels	0.057	0.213	-0.163	-0.110	0.008
	(0.093)	(0.196)	(0.163)	(0.087)	(0.083)
First Differences	0.143	-0.029	0.057	0.059	0.118
	(0.119)	(0.279)	(0.214)	(0.103)	(0.106)
Restricted sample					
Treatment status	0.245*	0.464	-0.184	-0.038	0.094
	(0.144)	(0.328)	(0.259)	(0.127)	(0.127)
First Differences	0.292	0.726*	0.239	0.181	0.287
	(0.184)	(0.449)	(0.346)	(0.160)	(0.164)

Note: Robust standard errors in brackets. * indicates significance at the 10% level. Number of observations equals 2384 for full sample and 1030 for restricted sample.

All effect estimates are obtained from specifications that also include the full set of background characteristics and a control for the poverty index. The results for the restricted sample reveal that receipt of the cash transfer leads to more food expenditures and more school related expenditures.[13] Food expenditures go up by 25 percentage points, school expenditures by 73 percentage points.[14] While the cash transfer does not increase school enrolment, it may better equip those who do attend. Part of the cash transfer is thus spent in a way that potentially raises children's human capital. One option is improving school attendance. Unfortunately, our data has no reliable information on attendance because during the fieldwork there were some strikes by the teachers' union at national level. Another option is improving students' cognitive achievements. In this regard it seems important to analyze possible effects of the program on test scores. The next chapter deals with this issue.

1.6 Summary and discussion

Various evaluation studies of cash transfer programs in Latin America that condition receipt of the transfer on children attending school, all find that such programs have substantial positive effects on school enrolment. The evidence comes both from studies that use data from randomized field experiments, as well as from studies that use non-experimental designs.

This chapter evaluates the effects of a cash transfers program in Ecuador where receipt of the transfers does not depend on children attending school. The design of the program includes a regression discontinuity. Families with a score on a poverty index equal to or below the 40th percentile are eligible for the transfers; families with a score above the 40th percentile on that index are not eligible. Although eligibility does not perfectly predict actual receipt of the cash transfer, there is a sharp drop in the probability of receiving treatment at the 40th percentile. We exploit this feature of the program's design to instrument receipt of treatment.

We find a rather precisely estimated zero effect of the cash transfer on school enrolment. Combined with the evidence from another study from Ecuador that looks at the effects for groups close to the 20th percentile, this suggests that the effects of the cash transfer on school enrolment is heterogeneous and increases with poverty. Our estimate of a zero effect should thus not be interpreted as an estimate of the average effect of the program but rather as an estimate of the effect for children from families close to the 40th percentile cutoff. The policy implication of our zero effect finding is therefore not that the program should be abandoned altogether, but rather that the eligibility threshold could be lowered.

This estimated impact of unconditional cash transfers in Ecuador contrasts with the estimates in previous studies of the impact of conditional cash transfers in other Latin American countries. A study by Maluccio and Flores (2004), which is based on a randomized experiment in Nicaragua, finds effects on enrolment equal to 26% for children from the 21% poorest families (extremely poor), equal to 12% for children from families from the next 24% poorest families (poor), and equal to 5% for children from families between the 45th and 66th percentile on the poverty scale. Our estimate of zero for families around the 40th percentile can probably best be compared to their estimates of 12% and 5%.

The differences in effects is suggestive evidence that not the cash transfer itself but the requirement to send children to school is the driving factor for the success of these programs.

De Brauw and Hoddinott (2007) and Schady and Araujo (2007) have used other approaches to disentangle the effects of the cash transfer per se and the requirement that children attend school. Both studies impose a conditional independence assumption that can be questioned. However, the two papers, also reach the same conclusion as we do, namely that conditionality is decisive.

The fact that families behave differently under conditional cash transfers than under unconditional cash transfers implies that families reach higher utility levels without the conditioning. The requirement that children should attend school is therefore only justified if there is a clear belief that families behave sub-optimally. This is the case in Ecuador, where poor families under-invest in education, because of credit constraint, and/or lack of information.

In addition, unconditional transfers are optimal for the people who make the decisions in the household. However, if those decision makers have different interests than their children, unconditional transfers may not be optimal for the child.

The main policy recommendation is the importance of creating mechanisms to enforce conditionality.

Notes

[1] The program in Honduras will also be evaluated through a randomized field experiment. Results are not yet available.

[2] Studies that take an entirely different – structural – approach to disentangle the effects of the cash transfer and of the conditionality include Attanasio et al. (2005), Bourguignon et al. (2003), De Janvry and Sadoulet (2006) and Todd and Wolpin (2003). All these studies conclude that the conditionality explains the bulk of the effects.

[3] The cutoff point between these quintiles on the poverty index was 42.87, while the cutoff point between the second and third quintiles was 50.65.

[4] Because program participation is not deterministically related to the score in the Selben index we have a fuzzy design. In this case we have to use an instrumental variable approach for identification. For more details see Sandra Black (1999), Angrist and Lavy (1999), Hahn, Todd and Van der Klaauw (1999), Van der Klaauw (2002), Jacob and Lefgren (2004), Chay, McEwan, and Urquiola. (2005).

[5] The research has a baseline and follow-up survey. Below, there are more specific details about the data.

[6] One potential pitfall of the IV approach is that it assumes that the relationship between the outcome variable and the variable that determines treatment is known. If one assumes the wrong functional form, estimates can be biased because of model misspecification. If, for example, the relationship is not linear around the cutoff, but the function is specified as linear, the estimated treatment effect may simply pick up any underlying non-linearity in the function (Jacob and Lefgren (2004), Chay, McEwan, and Urquiola (2005)). One way to deal with this problem is by using several functional forms of the control function. The robustness of estimates will be checked by including second and third order polynomials in the Selben index.

[7] Z is a good instrument because it is highly correlated to the probability of receiving the program (it is the assignment rule), and it is not correlated to the outcome variable (being just above and just below the cutoff does not have any correlation with the enrolment rate).

[8] In equation 2 we assume that the effect of the program is the same for new enrolment and for preventing drop out. New enrolees are children who were not enrolled in the base line, but enrolled in the second round. Drop outs are those who were enrolled in the base line but were not enrolled in the follow up. We did compute the effect of the program on new enrolment, as well as the effect on drop out. These estimates supported the assumption that the effect of the program does not differ for new entrants and drop outs. The problem with this specification is that the number of cases is reduced considerably, affecting the reliability of our estimates.

[9] Data used in this chapter as well as the used by Schady and Araujo (2005) were collected at the same time. As already mentioned, the impact evaluation of the program included both designs; the experimental and the regression discontinuity. The author of this thesis was the head of the Ecuadorian team in charge of evaluating the impact of the program.

[10] The baseline survey was conducted between June and August 2003, and the follow-up survey was carried out one and a half-year later between January and March 2005. The time interval between these two surveys may be considered adequate in terms of expecting results on school enrolment due to the program. While the new program started to operate in January 2003, the expansion of coverage to the four provinces of our sample took place after the baseline was taken.

The new beneficiaries in our four provinces of study started to receive the transfer in November 2003.

[11] Data on all key variables are available for all households in the sample, with the exception of parental education, which is missing in some cases.

[12] In comparison, in the (experimental) data analyzed by Schady and Araujo (2005) the comparable percentage equals 31%.

[13] School related expenditures include transportation, uniforms, tuition fees, textbooks and other school materials, and parents' contributions to school expenditures.

[14] This result is in the same line as the result found for those around the cutoff for the first quintile. Schady and Rosero (2007) find that households randomly assigned to receive the BDH transfers have a significantly higher food share in expenditures.

2 The impact of (un)conditional cash transfer programs on student's cognitive achievements: the case of the Bono de Desarrollo Humano of Ecuador

ABSTRACT. In chapter 1 we found significant impact of BDH on school spending. This could imply positive effects on students' cognitive achievements. In addition, there are theoretical reasons to believe that the program, as any transfer program, would have a positive impact on students' test scores. However, related empirical evidence is scarce. As a contribution, this chapter evaluates the impact of an unconditional cash transfer program, the Bono de Desarrollo Humano of Ecuador, on students' cognitive achievements. In methodological terms, the chapter relies on a variety of techniques, such as, regression discontinuity design and propensity score matching, to identify the effect of Ecuador's program on second grade cognitive achievement. Regardless of the empirical approach, there appears to be no significant impact of the program on test scores.

2.1 Demand-side interventions

Demand-side interventions played an important role in education policy in Latin America during the 1990s. Broadly, two types of programs were implemented in the region; conditional cash transfer programs and school vouchers.

Conditional cash transfer (CCT) programs started during the 1990s and have become an important component of social policy in most Latin America countries. The main idea of these programs is to provide money to poor families, which is conditional on investments in human capital such as sending children to school or taking them to health centers on a regular basis. Typically, two lines of human capital intervention are common in CCT programs: education, as well as health and nutrition. Regarding education, most CCT programs in Latin America focus on improving children's access to school. In fact, the majority of impact evaluation studies of CCT programs have found significant and positive impact on children's access to schools as well as on reducing child labor.

From a theoretical point of view, CCT programs may also be expected to exert an effect on students' cognitive achievements. On the one hand, there could be a positive impact because CCT programs may be associated with an increase in attendance rates (conditional on enrolment) and students with higher attendance rates are likely to have better test scores than students with lower attendance rates.[1] In addition, improved learning conditions can be found because of the potential impacts of the program on nutrition and consumption, as well as on reducing child labor.[2] On the other hand, negative effects of these programs on test scores may also be observed. The positive impact of these programs on school enrolment can increase the number of students, adding to school congestion, which can negatively affect students' achievements (Behrman, Sengupta and Todd, 2000). Notwithstanding these theoretical possibilities, the number of studies evaluating the effect of the CCT program on cognitive achievements is scarce. In addition, as mentioned in chapter one, the Ecuadorian experience is particular because the program has no mechanisms to enforce the complying of the condition. Therefore, the main contribution of this chapter is to evaluate the impact of the Ecuadorian un-conditional cash transfer program (Bono de Desarrollo Humano DBH) on students' cognitive achievements. The research has important political consequences and responds to the question: Is the program just increasing enrolment (among the extremely

poor), and increasing school spending (among the less poor) or is it also enhancing learning? In other words is it exerting an effect on enrolment and achievement?[3] The rest of the chapter is organized as follows. The next part presents some evidence on the impact of demand-side interventions on test scores in Latin America. The third part describes the Ecuadorian program. The fourth part presents the methodological approach used. The following part describes the data and shows some descriptive statistics. The next part shows the main results, and the last part concludes.

2.2 Experiences with conditional cash transfer and voucher programs in Latin America

The case of Mexico has been particularly fruitful in terms of research on the impact of CCT programs on several areas of social development. In particular, the impact of the program on students' cognitive achievements has also been evaluated. Behrman, Sengupta and Todd (2000), after almost a school year and a half of exposure to Progresa, found no significant impacts on test scores.

Regarding school voucher programs, there are only two important experiences in Latin America- Colombia and Chile. In the Colombian case, the government created a program directed to increase secondary school enrolment (Programa de Ampliación de Cobertura de la Educación Secundaria: PACES) in 1991 as an attempt to expand private provision of public services. PACES was targeted to low-income families living in neighborhoods classified as falling into the two lowest socioeconomic strata (out of 6 possible strata). To qualify for a voucher, applicants had to have been admitted to a participating secondary school, which had to be located in participating towns. The voucher was around US$ 190 in 1998, while enrolment and monthly fees for voucher applicants attending private schools averaged around US$ 340 in the same year. In the peak years of 1994 and 1995, roughly 90,000 students used vouchers. One advantage of this program was that, because the demand exceeded the supply, the Colombian government used a lottery system to allocate vouchers among participants, leading to a natural experiment. Angrist et al. (2002) used this identification strategy to evaluate the impact of the program on some school outcomes. Three years after the application, no significant differences in enrolment between "lottery winners and losers" was found, despite the fact that "lottery winners" had

completed an additional 0.1-years of school and were about 10 percentage points more likely than "losers" to have completed eighth grade, primarily because they repeated few grades. Regarding test scores, results show that, on average, "lottery winners" scored about 0.2 standard deviations higher than "losers". The effect of girls is larger and more precisely estimated than the effect on boys (Angrist, et al. 2002). In addition, Angrist, et al., (2004) have evaluated the long-term effect of PACES on test scores. Using the same natural experiment, they found that the program increased high school graduation rates by about 5-7 percentage points. By using administrative data from Colombia's centralized college entrance examinations, and after correcting for selection bias in the application for the exam between lottery winners and losers, they found a positive effect of the program of around 2 points in test scores.

Chile represents another experience with school vouchers. In 1981, the Chilean government created a nationwide voucher program with financial incentives for both public and private institutions. This initiative transferred fiscal schools from the Ministry of Education to municipalities and subsidized private schools that started to receive a per-student payment for every child attending their schools. Hsieh and Urquiola (2003) evaluated the impact of this program. By considering the fact that the program created a dynamic educational market with greater impact in communities with larger markets (in those where the demand for private schooling appears to have been greater), and the fact that the differential impact of the program is driven by community characteristics that are fixed over time, the impact of the program is measured by comparing the change in educational outcomes in urban and wealthier communities, to that in communities where private schooling increased by less (rural and poor communities). Results show that average test scores did not increase any faster in communities where the private sector made greater inroads and that average repetition and grade-for-age measures worsened in such areas. Results are robust for the introduction of a battery of controls for pre-existing and concurrent trends and even when instruments such as the initial population, urbanization rate, and degree of inequality, are used. However, one important conclusion of this paper is that the main effect of the program was an exodus of "middle class" students from the public to the private sector. The authors find that in communities where private schools grew more than public schools, there was a greater decline in socioeconomic status (measured by parental schooling

and income) of students of the latter relative to the community average. In addition, the paper shows that the loss of these students had a major effect on academic outcomes in the public sector (Hsieh and Urquiola, 2003).

2.3 The Ecuadorian program

The *"Bono Solidario"* program started in 1998 as a safety net to compensate poor families for the elimination of gas and electricity subsidies. Initially, the program used a self-targeting strategy directed at mothers with earnings below US$ 40, people with disabilities and senior citizens. While the immediate political justification for this program was to compensate the poor for losses in their real purchasing power caused by statutory increases in (heavily subsidized) petroleum and natural gas prices, the program quickly took on a life of its own, becoming the government's largest social expenditure outside of education, with total transfers equal to about one percent of GDP (Vos, León, and Brborich, 2001). By comparison, public education and health expenditures account for 2.5 and a bit less than 1 percent of GDP, respectively. The transfer was modest, but not trivial by Ecuadorian standards. At the time that the program started, mothers received 100,000 sucres per month, about US$15, and senior citizens and people with disabilities received 50,000. In April of 1999, those amounts were increased by 50%, mostly to account for high inflation. On average, the share of *Bono* income in total household expenditures was 11 percent in 1999. During 2000, the program reached around 1.2 million beneficiary households, representing about 45 percent of Ecuadorian households.

An impact evaluation of the Bono Solidario, conducted by Vos et al. (2001), who used a propensity score matching method, showed a positive impact of around 5 percentage points on school enrolment, although no significant impact was found on poverty indicators.[4] Another study, conducted by León and Younger (2007), who implemented an instrumental variable approach, shows that the program had very minor, yet significant and positive effects on children's nutritional status.

At the end of the 1990s, a CCT program called Beca Escolar, was also implemented. The program consisted of transferring US$ 5 per child (up to two children per household), conditional on the child's enrolment and 90% school attendance per month. No impact evaluation study of this program is available.

In 2003 the Bono Solidario was reformulated and became a CCT. The program was renamed Bono de Desarrollo Humano (BDH) and incorporated both the Bono Solidario and the Beca Escolar. The main objective of the new program has been to improve the formation of human capital among the poor of Ecuador. Education and health are the two components of the program. The education component requires children from the ages of 6 to 15 to enroll in school and to attend at least 90% of the school days in a month. The health component requires children under the age of six to attend health centers for bimonthly medical check-ups. Unlike other CCT programs in Latin America, the BDH has no mechanisms to verify conditionality. Consequently, households are not taken off program rosters if their school-aged children are not enrolled in school or fail to attend classes regularly. However, at the beginning of the program, some television programs were transmitted at a national level, advertising the obligation of parents to send children to school in order to receive the transfer. Those informational advertisements created some level of awareness regarding the obligation of parents to send their children to school and take them to the health centers in order to benefit from the program.

To select the beneficiaries, the program uses an individual targeting strategy based on a proxy means test computed by Selben (system of selection of beneficiaries of social programs). Selben identifies potential beneficiaries of social programs by classifying households according to an unmet basic needs index computed using non-linear principal components analysis.[5] Families pertaining to quintiles 1 and 2 receive the benefit (those are families that score less than 50.65 in the Selben index). Presently, around 90% of the beneficiaries of the program have the corresponding score in the Selben index. The difference is composed of families that did not have the Selben survey, but have received the program since its initial implementation. Currently, the program consists of a cash transfer of US$ 15 per month, per family. The annual budget of the program reached US$ 190 million in 2004 (around 1% of the GDP).

An impact evaluation conducted by Schady and Araujo (2005) used an experimental design. Families above and below the first Selben quintile threshold[6] were randomly assigned to treatment and control groups. After verifying the validity of the experiment, the paper uses a difference-in-difference strategy and concludes that the program has positive effects on enrolment (around 10 points) and negative effects on child

labor (about 17 percentage points). Finally, as was shown in chapter 1, we found no significant impact on school enrolment among those near the cutoff point in the Selben index (the less poor), but significant and positive effects on school spending.

2.4 Methodology

2.4.1 Empirical specifications

The objective of this chapter is to evaluate the impact of the BDH on students' cognitive achievement. As mentioned in the introduction, the main idea of an impact evaluation study is to isolate the effect of the program on students' test score. In this part we use a quasi-experimental approach based on the selection mechanisms to participate in the program. The main methodological approach consists of combining an education production function with a regression discontinuity design in order to get robust estimates of program impact. To start, we begin with the following equation:[7]

$$Y_i = X_i\beta + f(S_i) + \alpha T_i + u_i \quad (1)$$

Where Yi is the outcome variable (test score); Ti is an indicator variable that equals 1 if the person receives the BDH, and 0 otherwise; Xi is a vector of individual, household, school and teacher characteristics; *f*(Si) is a flexible function (first, second and third degree polynomial) of the score in the Selben index; and u_i is the error term. In this case, the parameter α represents the average treatment on the treated, after controlling for selection on observables. However, the problem with equation (1) is that program participation is not random, and Ti may be correlated with the error term as there may be some un-observable characteristics of individuals that may affect both program participation and test scores. In this case, OLS estimates of equation (1) will be biased and inconsistent. In addition, OLS estimates only refer to the conditional expected value of the outcome variable given program participation and individual, household, teacher and school characteristics. It does not, however, establish the causal effect of program participation (Verbeek, 2000). That is, the OLS estimate of α reflects the difference in the expected outcome of two people with the same observed characteristics, where one is participating in the program and the other is not. Because the program is targeted at the poor, it is possible that the coefficient α will be biased

downward and may be negative. This is an example of the "program placement effect" noted by Pitt, Rosenzweig, and Gibbons (1985) in which the program is endogenous to the dependent variable and geared toward the poor.

To tackle this problem, as already mentioned, this paper exploits the targeting strategy used by the program to create an exogenous source of identification. As stated, the program is conceded only to people scoring below 50.65 in the Selben index. This creates the proper conditions to use a regression discontinuity design (RD).[8] The main idea of the RD design is that assignment to the program depends on the value of an observed continuous variable relative to a given cutoff point. Furthermore, the corresponding propensity score (the probability of receiving the treatment) is a discontinuous function of this variable at the cutoff point. The fundamental assumption behind RD is that unobserved characteristics vary continuously (around the cutoff) with the observable characteristics used to determine treatment.[9] This assumption may not hold true if individuals can influence their position relative to the cutoff (Jacob and Lefgren, 2004). In our case, this concern is not a problem because families do not have any control over the calculation of the Selben index and they are unaware of the scoring procedure.

The idea behind the RD design is that comparing individuals within a very small interval around the cutoff point will be very similar to a randomization experiment at the cutoff. This is the case because they essentially have the same value of the Selben index (S). In this sense, one can expect that individuals just above and below the cutoff (S_0) are very similar, and thus have very comparable outcomes in the presence and absence of the program treatment. Bearing this in mind, a comparison between those above the cutoff (that do not receive the treatment) with those below (that receive the treatment) provides a potentially unbiased estimate of the treatment effect.

There are two kinds of RD designs in the literature: "sharp" and "fuzzy". Under the "sharp" design, individuals are assigned to treatment solely on the basis of an observed continuous variable called the selection or assignment variable: those who fall above the cutoff point do not receive the treatment ($Ti=0$ if $Si>S_0$), whereas those who fall below do ($Ti=1$ if $Si=<S_0$). In this case, T is deterministic and depends on the score in the Selben index, $Ti=f(Si)$. Where Si takes on a continuum of values and the point S_0, where the function f(S) is discontinuous, is as-

sumed to be known (50.65 in the score in the Selben index) (Hahn, Todd and Van der Klaauw, 2001). Under the "sharp" design, estimation using OLS will generate unbiased estimators because treatment is perfectly correlated to observable characteristics and is orthogonal to unobserved characteristics. One can thus identify the impact of the program by simply comparing individuals who belong to families scoring just below and just above the cutoff (Jacob and Lefgren, 2004). In this case, following the logic of the RD design, the next equation can be used:

$$Y_i^{RD} = X_i^{RD}\beta_{rd} + f\,(S_i^{RD}) + \alpha_{rd}T_i^{RD} + u_i \qquad (2)$$

Where the superscript RD indicates the regression discontinuity sample. To check for the robustness of estimates, several RD samples should be used.[10] Estimates in this case refer to the average causal effect of the treatment at the discontinuity point (Imbens and Lemieux, 2007).

One potential problem in the case of the BDH is that the assignment process does not depend exclusively on the score in the Selben index. Some individuals that receive the program may score above the cutoff, and others scoring below the cutoff may be excluded. This means that treatment assignment depends on the Selben index in a stochastic manner.[11] In the literature, this case is known as a "fuzzy" design. In addition to the Selben index, there are other variables that may influence the assignment to treatment. The important point to consider is that, even in this case, the propensity score function $\Pr(T=1|S)$ is known to have a discontinuity at S_0, similar to the sharp design. The fuzzy design can occur in cases of incorrect assignment relative to the cutoff point in a sharp design. In the fuzzy design, there is an error term: $Ti=E[Ti|Si]+ei$ (Hahn, Todd and Van der Klaauw, 2001). In this case, an instrumental variable (IV) strategy can be used to obtain unbiased estimates, where the instrument (Z) is the decision rule, which takes the value of 1 for those scoring below the cutoff and the value of 0 for those scoring above. This instrument satisfies the two basic conditions for being a "good" instrument. It is highly correlated to program participation (because it is the assignment rule) but not to students' cognitive achievement among those near the cutoff point (the RD samples).[12] The reduced form equation in this case yields:

$$Y_i^{RD} = X_i^{RD}\beta_{rd} + f\,(S_i^{RD}) + \alpha_{rd}Z_i^{RD} + u_i \qquad (3)$$

The 2SLS can be obtained by estimating:

$$Y_i^{RD} = X_i^{RD}\beta_{rd2} + f\,(S^{RD}{}_i) + \alpha_{rd2}\hat{T}_i^{RD} + e_i \tag{4}$$

Where $\hat{T}$ comes from the estimation of the first stage equation, which is defined by:

$$T_i^{RD} = X_i^{RD}\delta_1 + f\,(S_i^{RD}) + \delta_3 Z_i^{RD} + w_i \tag{5}$$

The result under IV gives local average treatment effect (LATE) estimates and refers to the impact of the program on those whose participation can be manipulated by the assignment rule (by the instrument), that is, those around the cutoff (Imbens and Lemieux, 2007).

One potential pitfall of the IV approach is that it assumes that the relationship between the outcome variable and the variable that determines treatment is known. If one assumes the wrong functional form, estimates can be biased because of model misspecification. If, for example, the relationship is not linear around the cutoff, but the function is specified as linear, the estimated treatment effect may simply pick up any underlying non-linearity in the function (Jacob and Lefgren (2004), Chay, McEwan, and Urquiola. (2005)). One way to deal with this problem is by using several functional forms of the control function. The robustness of estimates will be checked by including second and third order polynomials in the Selben index.

Additionally, following Hahn, Todd and Van der Klauw (1999 and 2001) another robust way of dealing with the problem of misspecification due to the use of the wrong functional form of the control function is by using non-parametric estimation techniques. In this case, the impact of the program, α, can be estimated by:

$$\alpha = \frac{Y^+ - Y^-}{T^+ - T^-} \tag{6}$$

Where $Y^+ \cong \lim_{S \to S^+{}_0} E[Y_i \mid S_i = S]$, and $Y^- \cong \lim_{S \to S^-_0} E[Y_i \mid S_i = S]$,

$$T^{+} \cong \lim_{S \to S^{+}_{0}} E[T_i \mid S_i = S] \quad \text{and} \quad T^{-} \cong \lim_{S \to S^{-}_{0}} E[T_i \mid S_i = S] \quad \text{, and} \quad .$$

The limits in equation (6) are estimated using non-parametric techniques.[13] The main idea of equation (6) is to compare the output of individuals nearly above the cutoff point (Y^{+}) with the output of individuals nearly below the cutoff point (Y^{-}), correcting for the probability of receiving treatment of those nearly above the cutoff point (T^{+}), and of those nearly below the cutoff point (T^{-}).

By not having to restrict the form of the control function, one can tackle the potential bias created by the misspecification of the functional form. It should be taken into account that non-parametric estimates require larger sample sizes than parametric estimates to obtain precise estimates (Mittelhammer et al., 2000). Moreover, non-parametric estimates tend to be less precise than parametric ones at given sample sizes. However, if the functional form of parametric estimates is not correct, then non-parametric estimates are more precise for a given sample size (Mittelhammer et al., 2000). In addition, regarding non-parametric regressions, the poor boundary performance of standard Kernel estimators is widely known. For this reason, this chapter will use local linear regression to estimate the limits of equation (6). This estimation technique has better boundary properties (Hahn, Todd, and Van der Klaauw, 1999; Han, Todd and Van der Klaauw, 2001; and Fan, 1992), as well as high asymptotic efficiency (Fan, 1992). Finally, one potential pitfall of non-parametric estimators is that they are sensitive to different bandwidths. Therefore, results of equation (6) will be presented with different bandwidths.

2.4.2 Reproducing the Selben index

As mentioned previously, the Ecuadorian government uses the Selben index to identify the poor in order to provide them with social programs. To do so, the Technical Secretariat of the Social Cabinet carries out surveys of the poor from both rural and urban areas. Once the surveys are finished, a data-base is created in order to compute the Selben index using the weights and variables reported in appendix A. The original Selben index was constructed in the LSMS of 1999 using non-linear principal components analysis and a combination of 27 variables. These variables can be classified into the following groups: infrastructure (6

variables), demographic characteristics of household members (9 variables), educational characteristics of household members (4 variables), and household assets (8 variables). The index is scaled from 0 to 100. As already mentioned, families scoring below 50.65 receive the benefit, while families scoring above 50.65 do not participate in the program. A key aspect of this chapter is the reproduction of the Selben index using our data in order to replicate the assignment process and use the RD strategy. The reproduction of the Selben index in our data was not perfect. While Selben is constructed using 27 variables, our dataset had information for only 20 variables. To replicate the index, this research worked with the same survey (LSMS of 1999) using only the 20 variables available in our data. With the same statistical procedure (non-linear principal components), the index can be re-estimated to obtain the new weights for the restricted variables and categories to create a quasi-Selben index. The different categories and variables used as well as their respective weights can also be seen in Appendix A. Once the new index was estimated, an OLS was run in the LSMS of 1999 to re-escalate the index in order to employ the same cutoff point used by Selben. Results of the regression show that the original Selben index can be computed based on the quasi-Selben index, with the following equation:

$$\underset{\substack{\\ (0.14312)}}{Selben = 9.159029} + \underset{(0.0032)}{0.925 * quasi\_selben} \tag{7}$$

Standard errors are in parenthesis. The R squared of the regression is 0.93.

Finally, with the new weights for the restricted variables and categories, the quasi-Selben index was computed in our data, and based on equation (7), the Selben index was also estimated. As already mentioned, the cutoff point in this case remains the same: 50.65.

2.4.3 Estimating the average treatment on the treated

As stated, the previously introduced RD design captures the effect of the program around the cutoff point. In this part, the research introduces another strategy to estimate the average treatment on the treated for the complete sample. It is well known, in the impact evaluation literature, that program impact may have heterogeneous results among beneficiaries.[14] In this regard results may be different for those far away from the cutoff point. To deal with this, the paper computes the average treat-

ment effect on the treated by using a propensity score matching strategy (PSM). One strong assumption when performing a PSM is the un-confoundedness assumption. The previous means that program participation is exogenous or un-confounded with potential outcomes conditional on a sufficiently rich set of covariates or pretreatment variables. This can be expressed formally as follows:

$$Y_i(0), Y_i(1) \perp T_i \mid X_i$$

Where Yi(0) is the potential outcome for controls, and Yi(1) is the potential outcome for treatment. T is an indicator variable for treatment status, and X is a vector of controls.

Under the un-confoundedness assumption the average treatment effect of the program for the treated can be estimated by comparing the outcomes for those in the treatment with those in the control group as follows:

$$ATT(x) \equiv E[Y(1) - Y(0) \mid X = x, T = 1]$$

If there are many covariates, it is recommended to use the propensity score (which is the conditional probability of receiving treatment given covariates). Rosenbaum and Rubin (1983, 1985) show that, under the assumption of un-confoundedness, adjusting solely for differences in the propensity score between treated and controls units removes all biases. The propensity score being:

$$p(x) \equiv \Pr(T = 1 \mid X = x) = E[T \mid X = x]$$

Which is assumed to be bounded away from zero and one:

$$0 < p(x) < 1$$

Following Ravallion (2005), the average treatment effect on the treated (ATT) when N participants in the program are matched to the nearest non-participant neighbor can be defined as follows:

$$ATT = (1/N)\sum_{i=1}^{N}(Y_i^1 - Y_i^0) \tag{8}$$

Where each participant (Y_i^1) is matched with the nearest non-participant (Y_i^0) based on the propensity score. It is important to note that a major source of bias while working with non-experimental studies

is the failure to satisfy the common support condition (Heckman et al. 1998). Imposing common support means that inferences on the impact of the program can be confined to "comparable people" in terms of their propensity scores. Formally, the previous means that:

$$Supp(X \mid P = 1) = Supp(X \mid P = 0)$$

This condition is imposed in our PSM estimation.

Besides one-to-one matching, others types of matching are found in the literature (Ravallion, 2005). The five nearest neighbors and a Kernel matching will be used. In this case, in general terms, the estimator for the average treatment effect on the treated (ATT) can be defined by:

$$ATT = 1/N \sum_{i=1}^{N} (Y_i^1 - \sum_{j=1}^{C} W_{ij} Y_j^0) \tag{9}$$

Where W is the weight used in calculating the counterfactual for each participant, and C is the number of cases used to construct the counterfactual for each participant. Kernel estimates are computed using the Epanechnikov Kernel because it has the highest asymptotic efficiency among the alternative Kernel distributions (Mittelhammer et al., 2000).

However, conditioning only on the true propensity score rather than on the full set of covariates does not in general lead to an efficient estimator Hahn (1998). In this regard, Hirano, Imbens and Ridder (2003) propose an efficient estimator using the estimated propensity score matching. According to their approach, one may estimate a PSM by using a weighted ordinary least square of equation (1), where the weights are; 1 for the treated units, and $\hat{p}(X)/(1-\hat{p}(X))$ for controls.[15] In this case $\hat{p}$ is the estimated propensity score from the equation of program participation.

The main concern with the matching strategy is the lack of fulfillment of the un-confoundedness assumption. As already mentioned, this means that the selection process is not determined by un-observables. To test for the presence of un-observables in the selection process we rely on sample selection models.[16] In this regard, one way of testing for un-observables is by analyzing the partial correlation between the outcome variable of the principal equation and the residuals from the selection equation. In this case one can run the following model.

$$Y_i = \delta_0 + \delta_1 T_i + X_i \delta_2 + \delta_3 S_i + \delta_4 R_i + \upsilon_i \tag{10}$$

Where Yi, Ti, Xi and Si are the same as equation (1), and Ri are the generalized residuals from the probit model used to compute the propensity score for the matching.[17] Selection bias is determined if $\boldsymbol{\delta}_4$ is different from zero. A key issue in this test is the identification strategy used to estimate the model. Although differences in the functional form of the two equations (non-linear in the selection equation, and linear in the outcome equation) aids identification, it is a weak basis for identifying the model. In order to have a better identification one should incorporate variables that influence program participation but are not related to the outcome variable (test scores). One variable highly correlated with the participation in the program is the assignment rule[18]. As discussed above, families scoring below the threshold in the Selben index (50.65) receive the benefit, while families scoring above it do not. Additionally, it would be expected that the dummy for the assignment rule is not correlated with test scores. In any case, the validity of the instrument used for identifying the model will be assessed.

2.5 Data and descriptive statistics

Data were collected by the Latin America Faculty of Social Sciences (*Facultad Latinoamericana de Ciencias Sociales (FLACSO-Ecuador)*), which refer to the rural area and to the capital (Quito) of Ecuador.[19] Students from second grade of primary schools were interviewed. The survey was carried out from November 2004 to February 2005 and used three different instruments:[20] school, teacher, and household questionnaires. In addition, students from second grade were evaluated by using standardized tests on both mathematics and language. The fieldwork was very intensive. For every child the research team took information on its test scores (in both math and language), school and teacher characteristics, and household variables. The test scores, as well as school and teacher questionnaires, were filled out in the school, while the household questionnaire was filled out at the child's home.

The sample size is 2,588 children (1,469 for treatment, and 1,119 for control). A multi-stage cluster random sampling design was used. In the first stage, a school cluster was randomly selected. In the second stage, all the schools pertaining to the cluster were interviewed, and, finally, in the third stage, all the students from second grade were interviewed and

took the tests. The school questionnaire has information about the director of the school, school infrastructure, the number of teachers and its schooling level and experience, the number of students, the number of classrooms, books, computers, labs and other school inputs, location, participation in the school meal program, participation in programs directed to improve school quality, and some information about the last year the school underwent infrastructural improvements.

The teacher questionnaire was applied to the teacher in charge of mathematics and language. In this case, the survey obtained information about the teacher's schooling, experience, the type of contract (hired by the Ministry of Education or by the school), and the number of training courses attended during the last four years.

The household questionnaire starts with a register of every household member, their names, sex, age and relationship to the head of the household. Then, there is a module on household assets and infrastructure, as well as whether or not the household receives various social programs (including the BDH). On an individual level, the survey takes information on schooling level, parents' level of education, marital status and language spoken by all household members. In addition, employment status, labor conditions and incomes are taken among those aged 5 or more. For children between 5 to 17 years old, information on school enrolment, the type of school attended, education spending and attendance is available. Finally, the questionnaire has some questions regarding the child's use of time, recording the number of hours the child works, helps in housework, watches television and whether he or she receives parental help for homework.

Table 2.1 presents descriptive statistics of the main variables used in this paper for both beneficiaries and non-beneficiaries of the BDH. The table includes three groups of variables, which are child, and household variables, as well as school and teacher characteristics.

Regarding child and household variables, beneficiaries have lower scores (out of 20) for both mathematics and language in the second grade. In addition, as expected, beneficiaries score less in the Selben index than non-beneficiaries. No significant differences between beneficiaries and non-beneficiaries are found in terms of sex, the time spent by the child at work, and the sex of the head of the household. Beneficiaries are worse off than non-beneficiaries in terms of the head of household's level of schooling, whether or not they are illiterate. In addition, the

Table 2.1
Descriptive statistics for beneficiaries and non-beneficiaries

	Non-beneficiaries	Beneficiaries	Difference
Child and household characteristics			
Mathematics, second grade (out of 20)	9.3761	8.5102	0.8659*
	(0.1513)	(0.1349)	(0.2033)
Language, second grade (out of 20)	11.1463	10.2329	0.9133*
	(9.1411)	(0.1238)	(0.1879)
Score in Selben index	44.1959	36.5307	7.6652*
	(0.4288)	(0.3074)	(0.5141)
Dummy sex (1=female)	0.4930	0.4808	0.0121
	(0.0148)	(0.0129)	(0.0197)
Total of hours working on weekdays	8.1461	8.4907	-0.3445
	(0.4106)	(0.2993)	(0.4961)
Hours spent at homework (daily)	1.5717	1.4845	0.0872*
	(0.0245)	(0.0208)	(0.0319)
Hours that children study with parents (daily)	0.6374	0.5298	0.1075*
	(0.0217)	(0.0183)	(0.0282)
Hours that children watch TV. (daily)	1.2226	0.8614	0.3612*
	(0.0363)	(0.0286)	(0.0456)
Schooling of the head of household	6.2345	5.3304	0.9041*
	(0.1167)	(0.0934)	(0.1477)
Head of the household is indigenous	0.3575	0.5655	-0.2081*
	(0.0142)	(0.0129)	(0.0193)
Head of the household is illiterate	0.1283	0.1771	-0.0488*
	(0.0099)	(0.0099)	(0.0143)
Head of the household is female	0.1306	0.1284	0.0022
	(0.01001)	(0.0087)	(0.0133)
No. of persons aged less than 6 in the hh.	1.1377	1.4082	-0.2705*
	(0.0469)	(0.0432)	(0.0643)
No. of persons aged from 6 to 17 in the hh.	3.7702	4.3773	-0.6070*
	(0.0941)	(0.0865)	(0.1286)
No. of persons aged from 18 to 44 in the hh.	2.3728	2.6032	-0.2304*
	(0.0462)	(0.0505)	(0.0704)
No. of persons aged from 45 to 64 in the hh.	0.5333	0.6234	-0.090**
	(0.0310)	(0.0295)	(0.0433)
No. of persons aged more than 64 in the hh.	-0.1044	0.1419	-0.0375***
	(0.0129)	(0.0144)	(0.0200)
School characteristics			
% of children attending schools with one teacher	0.1368	0.1944	-0.0575*
	(0.0102)	(0.0103)	(0.0147)
% of children attending Hispanic schools	0.7096	0.5810	0.1286*
	(0.0135)	(0.0128)	(0.0188)
% of children from Quito	0.2474	0.1009	0.1465*
	(0.0128)	(0.0078)	(0.0143)
% of children attending schools with full-time principal	0.2360	0.1022	0.1337*
	(0.0126)	(0.0079)	(0.0142)
No. of learning guides per child	0.0589	0.0568	0.0022
	(0.0065)	(0.0052)	(0.0082)

% of children attending schools with computers	0.6947	0.5393	0.1554*
	(0.0136)	(0.0129)	(0.0189)
No. of books per pupil	1.5226	1.8059	-0.2832
	(0.1177)	(0.1032)	(0.1566)
% of children attending schools with access to internet	0.1088	0.0457	0.063*
	(0.0092)	(0.0054)	(0.0101)
Index of school infrastructure (out of five)	3.7202	3.5414	0.1788*
	(0.0252)	(0.0269)	(0.0379)
Teacher characteristics			
% of children with female teacher	0.6263	0.5777	0.0486*
	(0.0143)	(0.0128)	(0.0193)
Age of teacher (average)	37.5570	37.2374	0.3196
	(0.30001)	(0.2702)	(0.4053)
% of children with teacher with superior level	0.7667	0.6967	0.0699*
	(0.0125)	(0.0119)	(0.0175)
% of children with teacher hired by the Ministry	0.7921	0.7525	0.0396*
	(0.0120)	(0.0112)	(0.0166)
No. of training courses received by teachers (average)	6.6298	7.4055	-0.7757*
	(0.2063)	(0.2668)	(0.3543)
Number of *cases*	1119	1469	

Note: Standard errors in parentheses. * Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

treatment group has a higher percentage of households headed by indigenous, and bigger household size. This can be explained because the program targets the poorest people.

Regarding school characteristics, the percentage of beneficiaries attending multi-grade schools (with just one teacher) and schools pertaining to the indigenous system[21] is higher than among non-beneficiaries. Another indicator of school quality is related to whether or not the school has a full-time principal.[22] The percentage of children attending schools with a full-time principal is higher among non-beneficiaries than among beneficiaries. No significant differences are found among beneficiaries and non-beneficiaries regarding books and learning guides. However, significant differences in favor of non-beneficiaries are found in relation to the percentage of children that attend schools that have computer and Internet access. Similarly, significant differences are observed regarding the index of school infrastructure. This index is scaled from 0 to 5, and was computed using indicator variables that take the value of 1, if the characteristic is present and 0 otherwise. The following infrastructural characteristics were taken into account: teacher housing, potable water, electricity, bathrooms and space for children to play. Beneficiaries have a lower score in the index than non-beneficiaries.

In conclusion, based on the previous variables, non-beneficiaries attend better schools than beneficiaries.

Finally, regarding teacher characteristics, there are significant differences among beneficiaries and non-beneficiaries. The percentage of children taught by female teachers and by teachers with a superior level of education, as well as by teachers contracted by the Ministry of Education,[23] is higher among non-beneficiaries than beneficiaries. Therefore, it seems that non-beneficiaries attend schools with better teachers. However, one important point to make is that in the last four years, teachers of beneficiaries received more training courses than teachers of non-beneficiaries.

To summarize, from the descriptive statistics presented one finds differences in cognitive achievements between students who receive the BDH and those who do not. In addition, non-beneficiaries have a higher socioeconomic status and attend better schools, with better teachers, than beneficiaries. This is consistent with the targeting strategy of the program. In this regard, simple comparing between beneficiaries and non-beneficiaries, could lead to downward bias estimates of program impact.

2.6 Results

2.6.1 Local average treatment effect of the program

Table 2.2 presents estimates of Equation 1. Specification 1 includes child characteristics (sex, age and the score in the Selben index).[24] Specification 2 includes, in addition to the previous variables, household variables (indicator variables to determine whether the head of household is illiterate, indigenous, and female, and the number of individuals in the household for age groups).[25] Observed household and children characteristics reflect parents' ability to provide a supportive environment for their children. On an individual level, some characteristics that seem important are; sex, since parents or teachers may treat boys and girls differently, and age, since older students are more mature and more likely to score higher, and they can also have potential achievement problems. Because of credit market imperfections in the Ecuadorian context, assets variables, expressed through the Selben index, are included to take into account socioeconomic circumstances of the household. In addition, parent's characteristics, which can affect living standards and preferences

for children's education, are also included. Finally, household composition seems important because more children implies less time for parents to dedicate to every child. Specification 3 includes, in addition to the previous, school variables that are important determinants of cognitive achievements (indicator variables for urban, Hispanic school, multi-grade school, and whether the school has a full-time principal, computer, Internet, and the number of textbooks and learning guides per student), as well as teacher characteristics (age, sex, education level and training, and type of contract). Finally, specification 4 includes cantonal level fixed effects. In all cases, the program has a negative and significant association with mathematics and no significant relation to language. As already mentioned, this result only reflects the difference between beneficiaries and non-beneficiaries in test scores after correcting for the rest of the variables included across the different specifications. However, this coefficient should not be imbued with a causal interpretation. Table 2.3 explores the existence of some causal interpretation.

Table 2.2
OLS estimate of program impact (equation 1, complete sample)

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.5695**	-0.5127***	-0.7455*	-0.4121***
	(0.2967)	(0.2904)	(0.2683)	(0.2289)
R squared	0.0195	0.0277	0.1061	0.2745
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.2993	-0.2436	-0.3475	-0.1274
	(0.2485)	(0.2448)	(0.2256)	(0.2058)
R squared	0.0529	0.0591	0.1441	0.2434
No. of cases	2589	2589	2589	2589

Note: Standard errors are in parentheses and corrected for heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 2.3 presents estimates of equation 2. The specifications used are the same as before. Results are presented for three different RD samples: for those scoring from 49.6 to 51.6, from 48.6 to 52.6, and from 47.6 to 53.6 in the Selben index. In this case, no significant effects of the program on test scores are found for both mathematics and language.

Table 2.3
OLS estimate of program impact (equation 2, several RD samples)

RD sample from 49.6 to 51.6				
Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	0.0280	0.2774	0.4259	0.5506
	(0.8294)	(0.9295)	(0.9834)	(0.8771)
R squared	0.0192	0.0425	0.1849	0.5077
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.9505	-0.4389	-0.4501	1.0200
	(0.6362)	(0.6234)	(0.736)	(0.6984)
R squared	0.023	0.1474	0.2508	0.4768
No. of cases from above the cutoff		75		
No. of cases from below the cutoff		57		
RD sample from 48.6 to 52.6				
Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.0132	-0.0539	-0.4684	-0.2203
	(0.6659)	(0.6633)	(0.6691)	(0.6625)
R squared	0.0116	0.0406	0.1754	0.3749
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.3769	0.0174	-0.2182	-0.0858
	(0.5139)	(0.5203)	(0.5322)	(0.4906)
R squared	0.0078	0.0736	0.1839	0.3053
No. of cases from above the cutoff		141		
No. of cases from below the cutoff		112		
RD sample from 47.6 to 53.6				
Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.2179	-0.2152	-0.5894	-0.4558
	(0.4687)	(0.4795)	(0.4834)	(0.4681)
R squared	0.0114	0.0482	0.1706	0.3453
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	0.1215	0.1977	-0.1904	-0.1380
	(0.4617)	(0.4674)	(0.4754)	(0.4737)
R squared	0.004	0.0362	0.1398	0.2602
No. of cases from above the cutoff		202		
No. of cases from below the cutoff		186		

Note: Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Specifications in Table 2.3 include the score in the Selben index in a linear form. As a robustness check, specifications including different polynomial forms of the Selben index (quadratic and cubic) are presented in Appendix B. The conclusion remains that there is no significant impact

of the BDH on tests scores. In order to evaluate the validity of the RD strategy, Appendix C introduces, as an example, descriptive statistics for the RD[26] sample from 48.65 to 52.65. This table allows one to conclude that there are no other statistically significant differences between treatment and control group, except for the score in the Selben index. This confirms the key idea in the identification strategy of the RD design, that is, except for the discontinuity in the Selben index, there are no other discontinuities around the threshold.

Tables 2.4 and 2.5 present reduced form and IV estimates based on equation 3 and 4, respectively. The same specifications and the same RD samples are used as before. The estimates show no significant effects of BDH on test scores. To check for the robustness of the results, Appendix D presents estimates of equation 3 by incorporating different polynomial forms of the selection variable (quadratic and cubic). The effects remain unchanged.

Finally, Table 2.6 presents non-parametric estimates of equation 6. As discussed in the methodological section, non-parametric techniques are the most robust way to deal with estimations under the RD design. However, because of the sensitivity of these estimates to the bandwidth chosen, results are reported for several. Results for the bandwidth of 3 show a significant and positive effect of the program on mathematics for the different RD samples. However, this result is not robust and the coefficient becomes insignificant when different bandwidths are used.

As stated above, one potential problem of this research is that if the program has significant effects on school enrolment, the increase in the number of students would add to congestion and pressure on schools, and, therefore, could negatively affect students' achievements. In addition, if there are some students that take the test because they enrolled in school due to the program and they perform poorer on the test than students who would have taken the test in the absence of the program, then this selection will lead to a downward bias of the effect of the program on test scores (Behrman, Sengupta and Todd, 2000). This possibility is not a serious concern in our case. As discussed in Chapter 1, there is no significant impact of the program on school enrolment among children around the cutoff. Curiously, Chapter 1 finds positive effects of the program on food and education expenditures for the same children. However, these positive effects are not reflected in school enrolment or in students' cognitive achievements.

Table 2.4
Reduced form estimates of program impact
(equation 3, several RD samples)

RD sample from 49.6 to 51.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	-0.9174	-0.9171	-0.9785	-1.3536
	-1.375	-1.6089	-1.8463	-1.4882
R squared	0.0216	0.0438	0.1854	0.5089
Language	Specification1	Specification2	Specification3	Specification4
Z	-1.2154	-1.1714	-1.069	-0.6437
	-1.3527	-1.1764	-1.4903	-1.7312
R squared	0.0152	0.1491	0.2518	0.4676
No. of cases	132	132	132	132
RD sample from 48.6 to 52.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	0.1743	0.5012	0.078	-0.0583
	-1.097	-1.1773	-1.2476	-1.0818
R squared	0.0117	0.0413	0.1734	0.3745
Language	Specification1	Specification2	Specification3	Specification4
Z	-0.8799	-0.7363	-0.7081	-0.8912
	-0.8703	-0.8174	-0.93	-0.8456
R squared	0.0087	0.0755	0.1849	0.3076
No. of cases	253	253	253	253
RD sample from 47.6 to 53.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	0.4785	0.6339	0.2303	0.4607
	-0.9491	-0.9942	-0.9658	-0.8962
R squared	0.0116	0.0488	0.1675	0.3441
Language	Specification1	Specification2	Specification3	Specification4
Z	-1.3723	-1.2712	-1.4246	-1.2379
	-0.7459	-0.7342	-0.7438	-0.6783
R squared	0.0098	0.0408	0.1455	0.2645
No. of cases	387	387	387	387

Note: Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 2.5
IV estimates of program impact (equation 4, several RD samples)

RD sample from 49.6 to 51.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	6.9432	4.6821	5.6232	4.7828
	(13.7044)	(9.0337)	(11.656)	(6.2739)
Language	Specification1	Specification2	Specification3	Specification4
Z	9.1982	5.9803	6.1429	2.2747
	(18.2414)	(8.9301)	(11.287)	(6.6074)
No. of cases	132	132	132	132
RD sample from 48.6 to 52.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	-2.8228	-8.7649	-435.6350	1.1859
	(18.2619)	(26.806)	(343160)	(21.858)
Language	Specification1	Specification2	Specification3	Specification4
Z	14.2503	12.8740	3952.4500	18.1040
	(33.0376)	(29.149)	(3103782)	(53.4777)
No. of cases	253	253	253	253
RD sample from 47.6 to 53.6				
Mathematics	Specification1	Specification2	Specification3	Specification4
Z	-51.1550	-66.7090	7.3226	-58.4080
	(585.98)	(714.44)	(36.249)	(821.29)
Language	Specification1	Specification2	Specification3	Specification4
Z	102.8000	108.9900	-46.9330	157.0400
	(812.68)	(934.16)	(161.50)	(2159.58)
No. of cases	387	387	387	387

Note: Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 2.6
Non parametric estimates of program impact
(equation 6, several RD samples and bandwidths)

Mathematics				
RD sample	**Optimal bw.**	**bw (2)**	**bw (3)**	**bw(4)**
49.6 to 51.6	7.2078	7.8531	7.1838***	6.0429
	(7.9951)	(11.9629)	(4.2891)	(4.6129)
48.6 to 52.6	6.7008	7.5400	6.5143***	5.5483
	(6.7484)	(8.2002)	(3.6281)	(4.1867)
47.6 to 53.6	5.6475	6.1874	5.6085***	4.6684
	(4.9736)	(5.7389)	(3.1117)	(3.5139)
Language				
RD sample	**Optimal bw.**	**bw (2)**	**bw (3)**	**bw(4)**
49.6 to 51.6	-0.8520	-4.7720	1.9486	1.6980
	(3.7017)	(16.3838)	(3.4285)	(2.7188)
48.6 to 52.6	0.5648	-0.6420	2.0222	2.1941
	(3.0389)	(4.7759)	(3.0699)	(3.2522)
47.6 to 53.6	1.4288	1.3142	2.0907	1.6980
	(2.7908)	(3.7430)	(2.7109)	(2.7188)

Note: Asymptotic standard errors in parenthesis. * Significant at 1 percent level. ** Significant at 5 percent level. *** Significant at 10 percent level.

2.6.2 Average treatment effect on the treated

Appendix E includes estimates of the selection equation for program participation used in the propensity score matching. The model includes as regressors, the dummy that reproduces the assignment rule, some individual and household characteristics, and 32 dummies to include cantonal fixed effects.[27] As already mentioned, one of the main determinants of program participation is the assignment rule. In this case results show a significant and positive association between the assignment rule and program participation. The coefficient means that scoring below 50.65 in the Selben index increases the probability of receiving the program in 23 percentage points. In addition, those pertaining to households headed by indigenous have greater probability of participating in the program.

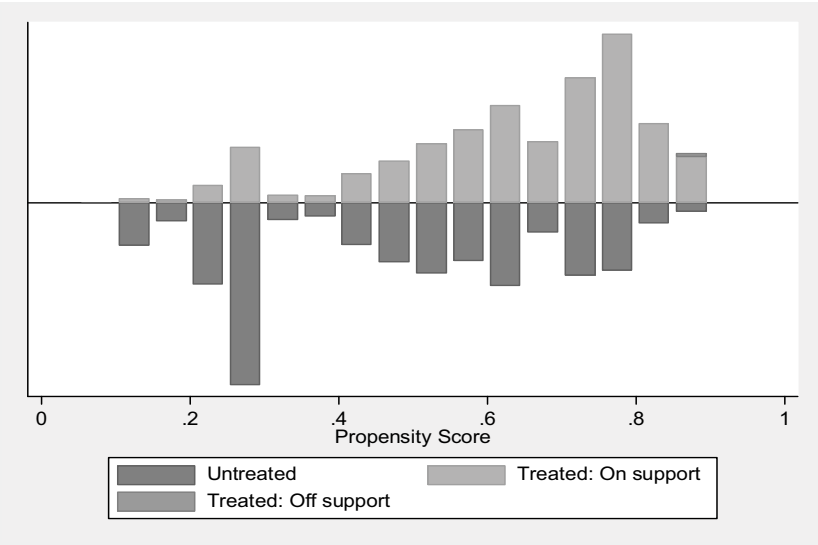
Table 2.7 introduces estimates using the PSM strategy according to equations 8 and 9. Results show no significant effects of the program on test scores on both language and mathematics. The result is robust and remains across the different types of matching.

Table 2.7
PSM estimates of program impact

ATT	One to one matching		Five nearest neighbors		Kernel matching	
	Math	Language	Math	Language	Math	Language
Treated	8.4979	10.2170	8.4986	10.2170	8.4979	10.2170
Controls	9.2568	10.1093	8.8464	10.1790	8.8697	10.2595
Difference	-0.7588	0.1077	-0.3477	0.0380	-0.3717	-0.0424
Standard error	(0.3681)	(0.3167)	(0.2400)	(0.2385)	(0.2805)	(0.2069)
Cases on common support	2,580	2,581	2,580	2,581	2,580	2,581

Note: * Significant at 1 percent level. ** Significant at 5 percent level. *** Significant at 10 percent level. Standard errors computed by bootstrap.

Figure 2.1
Distribution of propensity scores for treated and control



In order to verify the validity of the matching, Appendix F introduces the test of balancing variables after matching. The post-matching equality of means across the treatment and the control group suggests that on average the treated and the control groups are observationally identical. The similarities between control and treatment groups can be observed in the large regions of common support displayed in Figure 2.1.

This confirms that there is a lot of fuzziness in the assignment of the program. Appendix G introduces a cross tabulation between the assignment rule and the real treatment status. As an example, from the total that has to receive the program (scoring below the cutoff) only 65% in fact receives it, while 35% does not.

Table 2.8 presents results for weighted OLS estimates of equation 1. In this case the same four specifications as before are used. Results show no significant effects of the program on language. In the case of math, the result is significant but negative, although this result is not robust and it is significant only under specifications three and four.

Results presented in this part have to be carefully assessed. As mentioned in the methodological section, matching estimates are unbiased only if the un-confoundedness assumption is fulfilled. In order to evaluate this, Table 2.9 presents the results of estimates of equation 10.

Before proceeding, it seems important to evaluate the validity of the instrument used for identifying the model. As mentioned previously, the instrument is a dummy variable that reproduces the assignment rule (1 for those scoring below 50.65, and zero otherwise). According to the selection equation (Appendix E) the assignment rule has a significant impact on the probability of participating in the program. Scoring below 50.65 in the Selben index increases the probability of participating in the program by 23 percentage points.[28] Finally, in relation to the exclusion restriction, when the instrument is included in equation (1) it is not significant (see Appendix H). With this in mind, one can interpret the results of estimates of equation (10). As mentioned earlier, the un-confoundedness assumption is verified if the coefficient of the inverse Mills ratio is not significant. As displayed in Table 2.9, in this case, the coefficient is significant and positive, implying positive selection effects and consequently suggests that in the absence of the Mills ratio the estimated effect of the program could be biased upwards. The main point here is that the estimates reported indicate that the un-confoundedness assumption is not valid, and that it is difficult to recover the average treatment effect on the treated.

Table 2.8
PSM estimates of program impact using weighted OLS

	Specification 1	Specification 2	Specification 3	Specification 4
Mathematics				
T	-0.3623	-0.3664	-0.5388***	-0.4486***
	(0.3454)	(0.3419)	(0.3008)	(0.2550)
R squared	0.0047	`0.019	0.0855	0.2598
Language				
T	-0.0081	-0.0032	-0.1482	-0.0955
	(0.3053)	(0.2998)	(0.2447)	(0.2207)
R squared	0.0146	0.0336	0.147	0.2744
Number of cases	2589	2589	2589	2589

Note: * Significant at 1 percent level. ** Significant at 5 percent level. *** Significant at 10 percent level. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Table 2.9
Test for un-observables in the selection process: equation 10

	Specification 1	Specification 2	Specification 3	Specification 4
Mathematics				
Mills	1.5887**	1.1114	2.7899*	1.8561**
	(0.7676)	(0.8423)	(0.7456)	(0.7937)
T	-3.0710**	-2.2775**	-5.1387*	-3.5204*
	(1.2465)	(1.3603)	(1.1996)	(1.2826)
Language				
Mills	2.7538*	2.000*	2.7326*	3.4870*
	(0.7194)	(0.7673)	(0.6648)	(0.7962)
T	-4.6877*	-3.4370*	-4.7015*	-5.9164*
	(1.1525)	(1.2284)	(1.0821)	(1.299)
Number of cases	2587	2587	2587	2587

Note: * Significant at 1 percent level. ** Significant at 5 percent level. *** Significant at 10 percent level. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

2.7 Conclusions

Cash transfers play an important role in education policy throughout Latin America. Broadly, two kinds of cash transfer programs are found in the region; conditional cash transfers and school vouchers.

Conditional cash transfer programs aim to improve human capital, as well as reduce temporary poverty. In education, most CCT programs have a positive and significant impact on school enrolment. However, these programs do not incorporate explicit objectives related to improve students' cognitive achievements.

School vouchers have been applied only in Chile and Colombia with different impacts. In the Colombian case, a significant and positive impact on students' cognitive achievements is found, while in the Chilean case no impact is found.

From a theoretical point of view, there are important reasons to think that CCT programs will have a significant effect on students' cognitive achievements. However, the number of studies directed to evaluate the impact of CCT programs on students' cognitive achievements is scarce. As a contribution, this paper evaluates the Bono de Desarrollo Humano of Ecuador, on test scores. The Ecuadorian program is particular because it does not have mechanisms to verify the complying of the condition. In this regard it is an unconditional cash transfer. The main aim is to identify the effect of the program on learning as captured by cognitive achievement tests.

The various estimates presented in this paper suggest that there are no positive effects of the BDH on test scores. Specifically, estimates based on regression discontinuity design and for children around the cutoff reveal no positive effect of the program and matching estimates based on the entire sample also reveal no positive effects.

Chapter 1 reports that there are no significant effects of the BDH on school enrolment; however, there is an increase in educational spending. With this in mind, this chapter evaluated the possibility of positive effects of the BDH on students' cognitive achievement, and found no significant positive impacts. Both chapters analyze the impact of the program on those children around the cutoff. As mentioned in Chapter 1, these results could be attributed to the lack of conditionality in Ecuador's conditional cash transfer program.

From a policy perspective, it seems important to include explicit objectives related to enrolment and attendance as part of the conditions of the CCT programs. Without such requirements there do not appear to be positive effects on enrolment (at least for those close to the cutoff) or on cognitive achievement.

Notes

[1] As an example, Bedi and Marshall (1999 and 2003) show the importance of school attendance on test scores in Honduras.

[2] See Rawlings and Rubio (2003), Caldés, Coady, and Maluccio (2004) and Villatoro (2005) for reviews.

[3] The actual emphasis of Latin America education policy on schooling quality encourages the design of programs that affect both school enrolment and education quality.

[4] In addition the study finds a small but negative effect on the number of hours worked per week among the members of the household.

[5] The index is scaled from 0 to 100; 0 for the poorest and 100 for the richest. See the methodological part for more details on the construction of the Selben index.

[6] Those are the extremely poor.

[7] This is the typical education production function approach. For a review see Bowles, 1970; Hanushek, 1979; Behrman, 1999; Pritchett and Filmer, 1997; Todd and Wolpin, 2003.

[8] Some examples of studies using RD design can be found in Thistlethaite and Campbell (1960), Sandra Black (1999), Angrist and Lavy (1999), Hahn, Todd and Van der Klaauw (1999), Van der Klaauw (2002), Jacob and Lefgren (2004), Chay, McEwan, and Urquiola. (2005).

[9] The major identifying assumption for the RD strategy, in order to provide unbiased estimates of program impact, is that there are no other discontinuities around the cutoff (50.65). This is an exclusion restriction with respect to the discontinuity.

[10] Widening the bandwidths around the discontinuity increases the number of observations but at the same time increases the risk that the common trend assumption is violated. RD samples of ± 1, ± 2, ± 3, are used. These samples are relatively close to the discontinuity and include sufficient number of cases to obtain significant results.

[11] In some cases, political clientelism has distorted the allocation of the BDH benefits.

[12] One advantage of the RD design is that, when one is using instrumental variables, one can justify a Wald estimator even when the standard "exclusion assumption" that the instrument is uncorrelated with the error term is violated (Hahn, Todd and Van der Klaauw, 1999, and 2001).

[13] See Hahn, Todd and Van der Klaauw, (1999 and 2001) for more details.

[14] See Angrist (2004).

[15] Using this weight one obtains the average treatment on the treated. If one wants to get the average treatment effect for the population, the weights are $1/(\hat{P}(X)$ for treated units, and $1/(1-(\hat{P}(X))$ for the controls (see Hirano, Imbens and Ridder 2003 for details).

[16] For a review of models with sample selection bias see Vella (1998).

[17] This term is the inverse Mills ratio for the entire sample. See Vella (1998) for a review.

[18] The inclusion of an instrument in the probit selection equation gives rise to inconsistent estimates. (Bhattacharya and Vogt, 2007). For this reason, while we may include the instrument in the selection equation to test the un-confoundedness assumption, it should not be used in the probit models for matching. However, results remain the same whether we include or exclude the instrument in the probit model. Results of the probit without the instrument are available on request.

[19] I was the head of the team in charge of collecting the data. The data was the same used to evaluate the impact of a rural education program (Redes Amigas) on students' cognitive achievement. See chapter 3. For the current chapter we incorporate new cases from Quito in order to have some urban representation. The cognitive tests were designed by technicians from the Ministry of Education.

[20] As mentioned the new version of the Program (BDH) started in 2003. In this regard the short term effect of the program is evaluated.

[21] The Ecuadorian schooling system has two independent components: the Indigenous system, and the Hispanic system. Most of the indigenous people attend to indigenous schools, where Quichua and Spanish are taught. Multi-grade schools pertain, generally, to the poorer zones of the country.

[22] Full-time dedication means that the principal only takes care of administrative issues and has no teaching responsibilities.

[23] Teachers in Ecuador can be hired by the Ministry of Education (the majority), or by the community and the parents and teachers associations.

[24] Other child variables available in the data such as the time that child works, or the time that child watches TV, and whether she receives help from parents to study or not, were not included in the model because of endogeneity concerns. Results remain the same when those variables are included.

[25] The same groups as those presented in Table 2.1.

[26] Descriptive statistics for the other RD samples (from 49.65 to 51.65; and from 47.65 to 53.65) show a similar pattern as those reported here. Results are available under request.

[27] Results for the complete model (including the cantonal dummies) are available under request. See note 18.

[28] In addition, the pseudo R squared of the probit model for program participation increases from 0.11 to 0.13 by including the dummy of program assignment.

3 The impact of a school-based management program on students' cognitive achievement: a case study of Redes Amigas in rural Ecuador

ABSTRACT. Education decentralization plays an important role in Latin America. Most experiences of decentralization, based on transferring school administration to local communities, come from Central America, where the main objective was to improve school enrolment in remote rural areas. These experiences did not incorporate explicit objectives related to the improvement of the learning process. The Ecuadorian experience is novel because it represents a similar decentralization strategy but the main objective is to improve students' cognitive achievements. This chapter evaluates the impact of decentralization of rural schools in Ecuador on students' cognitive achievements. By using propensity score matching on a restricted sample of program applicants, the study finds evidence of significant and positive effect of *Redes Amigas* on students' test scores. The impact can be attributed to both the improvement of school inputs and changes in the school management structure. However, significant and negative impact on bilingual (Spanish and Quichua) schools is found, potentially, reflecting a problem of culturally inadequate curricula. Because of data limitations results are inconclusive and tentative.

3.1 Introduction

This chapter evaluates the impact of decentralization of education on students' cognitive achievement in rural Ecuador. Decentralization plays an important role in education policy in Latin America. Broadly, two types of decentralization strategies have been applied across the region. First, Mexico and some South American countries implemented a transfer of some administrative functions from the central government to local governments. Second, some Central America countries implemented a transfer of administrative and pedagogical issues from the central government to schools. The Ecuadorian experience corresponds to the latter. Despite the importance of such decentralization processes, empirical evidence that evaluates the impact of decentralization on students' cognitive achievements is scarce. This chapter reviews the literature on this topic and evaluates the impact of a school-based management experience on test scores in rural Ecuador. The novelty of the Ecuadorian experience is that it represents the first example where improving students' learning is stated as an explicit objective. In contrast, most of the school-based strategies from Central America focused on improving school enrolment in remote rural areas. In methodological terms the chapter combines a pipeline design with propensity score matching. Program implementation in certain schools was delayed due to administrative issues, and this provides the possibility of constructing an adequate control group using those schools that had decided to participate but were unable to do so due to these administrative reasons (additional details are provided in the succeeding sections).

The chapter is organized as follows. The following part discusses the experiences of decentralization of education in Latin America. The third part presents the Ecuadorian experience. The fourth part introduces the methodological approach used. The following part gives some descriptive statistics and introduces the data used. The sixth part presents the main results, and the last part concludes.

3.2 Experiences of decentralization of education in Latin America

3.2.1 The debate over decentralization

Most Latin America countries started to decentralize their education systems during the 1990s. From a theoretical point of view, there are several arguments that may be advanced in favor of and against decentralization of education.

The main benefits of decentralization are related to the fact that people at the local level, or those who are closer to the classroom (teachers, parents and students in the case of education), have better information than the civil servants of the central government, and thus are better able to make decisions to improve schools (King and Özler, 2000). In this regard, it is assumed that decisions made by those who are closer to the school are better and more efficient[1] than decisions made by authorities from the central government, because the former can make use of information about local preferences (Di Gropello, 2006). Furthermore, decentralization, when it was thought to transfer schools to private and municipal sectors, was assumed to encourage local competition. In this case it is understood that local competition can improve school efficiency. In addition, decentralization is expected to provide local consumers with greater voice and increase accountability because local people can better control their schools (Winkler and Gershberg, 2000). This reform presumes a well-functioning democracy whereby all the externalities of education are tended to locally.

Among the proponents of decentralization in education, it is argued that the reform will have a direct impact on improving schools, local participation, as well as local competition and technical efficiency (first round effects). As a result of these first round effects, decentralization will have an indirect impact on the learning process leading to better cognitive achievements of students (second round effects). Despite these arguments, empirical evidence on the effects of decentralization is scarce.

Amongst the cons of decentralization, it is argued that if there are local elites that gain control over local decision-making, then the process can lead to the existence of non-democratic structures in the decision-making process, and social welfare may not improve (Winkler and Gershberg, 2000). In political terms, this could lead to a consolidation of local *caudillismos*. Furthermore, if externalities associated with education are

distributed beyond the confines of the locality, there is a strong argument for a high participation of central government in financing local education. Additionally, the different degree of technical development at the local level can influence the results of decentralization. In this regard, decentralization may worsen the provision of public goods if local governments lack administrative capacity (Galiani and Schargrodsky, 2001). It is assumed that local governments with greater administrative capacity can lead to better decentralization experiences than governments that lack technical capabilities. Finally, another argument against decentralization is that the agent[2] (schools), once given decision-making autonomy could use it opportunistically, putting its own interest before the national interest, thereby taking advantage of the fact that the principal (the Ministry of Education) will not observe the true effort of the agent. However, this *moral hazard* issue can be offset by the existence of a second principal through community participation (Di Gropello, 2006)

In any case, as can be observed, most of the theoretical debate is based on assumptions that should be empirically examined. One of the main problems in the debate on decentralization is the scarcity of empirical studies aimed at evaluating these assumptions, or in other words, the empirical analysis of the effects of decentralization.

3.2.2 Experiences with decentralization

As previously stated, in practical terms, there are two kinds of decentralization strategies in the region. First, a strategy based on transferring key administrative school decisions from the central government to local governments (municipal decentralization). Second, a strategy based on giving local communities the decision-making power on key aspects of the education process (school-based management strategy SBM).[3] Despite the focus on educational decentralization in Latin America, empirical evidence on its impacts on students' achievements is scarce. In this section a summary of the main experiences of decentralization in the region is presented. The summary focuses on impact evaluation studies.

Regarding decentralization experiences in Latin America, Chile's reform is the most representative case of municipal decentralization or privatization. Chile started its decentralization in the early 1980s by transferring schools from the central government to municipalities or private agents (privatization). In addition, the amount of resources granted to schools, by the central government, was proportional to the number of

students attending each school (Espínola, 1997). The central government kept the role of financing education, as well as determining the curriculum. On the other hand, municipal governments and private agents were in charge of contracting teachers, administering schools, training teachers and maintaining and constructing school buildings (Espínola, 1997). From the beginning, the process did not incorporate an impact evaluation strategy. As a result, there are no experimental studies of the impact of decentralization on education outcomes. However, a simple comparison of student achievement scores throughout the 1980s shows a decline in learning. In addition, the real per-student education spending also declined in the same period (Winkler and Gershberg, 2000). More recently, a study conducted by Hsieh and Urquiola (2003) found no significant effects of decentralization on school outcomes at the aggregate (municipal) level. Based on panel data from about 150 municipalities, the paper explored the effect of the reform on test scores, repetition rates, and years of schooling. In this case, the study exploits the fact that the privatization of schools would have a greater impact in communities with larger markets (where the demand for private schooling would be greater), and little impact in communities with reduced markets. As long as the differential impact is driven by community characteristics that are fixed over time, the impact of the program is measured by comparing the change in educational outcomes in urban and wealthier communities, to that in communities where private schooling increased to a lesser degree. As mentioned above, the paper finds no significant effects at the municipal level, although it finds a significant increase in the enrolment of better students in private schools. In fact, private schools attracted students from families with higher levels of income and schooling, leading to a fall in student-outcomes of public schools because the better students of public schools migrate to private schools.

During the 1990s, the Chilean government took additional steps toward school decentralization. Improving school inputs, promoting pedagogical innovations, and specific interventions aimed at the most disadvantaged schools were the central components of this phase of the decentralization. This process, again, did not incorporate an impact evaluation design, and, consequently no experimental evaluation studies are found.

On a descriptive level, during the 1990s, the real per capita education expenditures, including teachers' salaries increased. In addition, more

stable labor conditions for teachers were guaranteed. Finally, the evolution of test scores indicates a significant improvement during the period.

One specific intervention directed toward improving the quality of the most disadvantaged schools was the program called P-900. The program started in 1990 and was targeted at schools ranking below the regional average test score (for fourth grade in math and language). The intervention included four components: school infrastructure, instructional materials (special textbooks), teacher-training, and tutorial workshops for children with low performance. An impact evaluation study by Chay, McEwan and Urquiola (2005) finds a significant effect of the program on fourth grade test scores in math and language of around two points between 1988 and 1992. The study used a regression discontinuity design and exploited the fact that schools scoring below the average regional test score received the program, while schools scoring above did not. By comparing schools just below the average regional test score with those scoring just above, the study finds unbiased estimates of program impact.

Argentina is another example of municipal decentralization. The country decentralized the administration of the primary system during the 1960s and 1970s. As a consequence, school-administration was transferred to provincial governments. The administration of secondary schools was transferred between 1992 and 1994. The following actions were transferred to the provinces at the secondary school level; the administration of subsidies and the regulation of private schools, the determination of expenditures, the allocation of personnel and non-personnel budget, the appointment and dismissal of directors, teachers and staff, the salary decisions, the definition of the calendar year, and the opening or closure of schools. Schools can choose textbooks, teaching and evaluation methods, and to some degree the content, but in consultation with the provincial authority (Galiani and Schargrodsky, 2001). An impact evaluation of the decentralization of secondary schools, conducted by Galiani and Schargrodsky (2001), finds a positive and significant effect on public school students' test scores in both language and mathematics. The study considers, as a source of exogenous variation, the fact that the transfer of secondary schools to provincial governments was made between 1992 and 1994. School transfers were scheduled through the signature of bilateral agreements between the federal government and each province, and took place between February 1992 and

January 1994. This political experiment generates an exogenous variation in the jurisdiction of administration of secondary schools across time and space, and is used as an instrument to identify the causal effect of decentralization on students' cognitive achievements. On average, between 1994 and 1998, test outcomes of public schools improved 1.2 standard deviations from its distribution as a result of the decentralization process (Galiani and Schargrodsky, 2001). Another important conclusion of this study is that the impact of the program depends on province characteristics. The study finds that the impact was positive when schools were transferred to fiscally ordered provinces, but negative when provinces run significant fiscal deficits (Galiani and Schargrodsky, 2001). In this regard, the study concludes that the efficiency of the decentralization process depends on the level of technical development of the local governments.

Additional examples of decentralization based on transferring administrative issues to municipalities are found in Brazil, Mexico, Bolivia and Colombia.[4] Unfortunately, there are no impact evaluation studies of these decentralization experiences.

Regarding the second type of decentralization (SBM), some interesting experiences can be found especially in Central America. The first experience with a SBM program is EDUCO (*Educación con participación de la comunidad*) from El Salvador. The program started in 1991 and transferred school administration to community education associations (*Asociaciones comunales para la educación, ACEs*). ACEs are in charge of administering and managing schools, selecting, hiring and monitoring teachers, building and maintaining schools, while the Ministry of Education contracts them to deliver a given curriculum to a certain number of students. EDUCO schools can be established in rural areas and provide preschool and basic education (grades 1-9). In addition, there must be at least 28 students per grade in the community and no other education services. The main objective of the program was to expand educational access for children from remote rural areas. No specific objectives regarding students' cognitive achievements were established. However, an impact evaluation conducted by Jimenez and Sawada (1999) found significant and positive impacts of the program on language test scores[5] and on student attendance. The study used an education production function approach to evaluate the impact of EDUCO, where the outcome variables (test scores or days attended) were regressed on student, school

and community characteristics. To deal with the problem of endogeneity due to program selection, the study used instruments such as the proportion of EDUCO schools, and traditional schools at the municipal level. It is assumed that these percentages affect the likelihood that a student will attend an EDUCO school without directly affecting the education production function at the student level.

In 1993, the Nicaraguan government established, as a pilot project, management boards (*Consejos directivos*) in 24 secondary public schools[6] to ensure the participation of school-teachers, parents and students in making school decisions. Initially, the program was directed toward secondary schools, and transferred key management tasks from central authorities to the directive councils. The school councils were in charge of hiring and firing teachers and administrative staff, maintaining the school building, making and overseeing budget allocations, generating additional financial resources (students fees), overseeing teacher performance and making pedagogical choices (Di Gropello, 2006).

Unlike in El Salvador,[7] the goals of Nicaraguan reform were to increase the efficiency and effectiveness of education services (students' attendance, and students' test scores) (Di Gropello, 2006). By the end of 1995, the program covered around 100 secondary schools, and was extended to primary schools. At the primary school level the program took on two forms. One was for urban schools, which is similar to the secondary school model and another for rural schools (*Núcleos Educativos Rurales Autónomos* (NER)). The latter is a group of schools formed around a central school, which functions as a single school network. A central council administrates the NER. Its directive council is based in the central school, which is usually the largest in the group and the only school that has a director. At the end of 2005, there were over 200 single autonomous primary schools and 42 NERs consisting of two to four schools each (King and Özler, 2000). An impact evaluation conducted by King and Özler (2000) found a significant and positive impact of *de facto*[8] decentralization on students' test scores at the primary level both in mathematics and language. The study used a propensity score matching method, as well as an instrumental variable approach to evaluate the effect of both *de jure* and *de facto* decentralization on school outcomes. The instruments used were schools characteristics (enrolment and director's characteristics), and municipal level variables (mean levels of education and infrastructure and its variances). A potential problem of the study is

that the instruments used may not be particularly credible. It is hard to imagine that characteristics of the school and director are not related to student performance. Finally, the paper finds no significant effect of the program at the secondary school level (King and Özler, 2000).

Additional examples of school-based management experiences are found in Guatemala (PRONADE) and Honduras (PROHECO). In such cases the main objective, such as in El Salvador, is to expand school enrolment in isolated rural areas affected by conflict, poverty and natural disasters.[9] However, no impact evaluation studies of those experiences are available. In South America, one can also find some examples of school-based management experiences such as the case of Minas Gerais in Brazil.[10]

As the summary above suggests, the existing empirical studies display different levels of analytical rigor. While there are no experimental studies to evaluate the impact of decentralization, the few quasi-experimental studies that do exist suggest that the results of the effects of decentralization on school outcomes are context and country specific. Regarding decentralization based on the transfer of administrative issues from the central to the local governments, there is some evidence showing that the impact depends on the political, administrative and financial capacity of local governments. The decentralization process can be successful in those local governments with high levels of development, while it can be a disaster in those local governments with low levels.

In the case of privatization, no significant effects at the municipal level are found in the Chilean experience. Nevertheless, deterioration in the performance of public schools because of the migration of better students from public to private schools is found.

In the case of targeted interventions directed to improve the quality of schools with low academic performance, a significant and positive impact is found.

Finally, in the case of SBM programs, empirical evidence on its impact on students' cognitive achievement is not absolute. It should be emphasized that most of these programs, especially in Central America, were established to increase school enrolment in remote rural areas and do not have explicit learning objectives. Therefore, it should not be surprising to find no significant effects on test scores.

The importance of the decentralization of rural areas in Ecuador through *Redes Amigas* is that the program belongs to the second type of

decentralization reform in Latin America (SBM), but includes explicit learning objectives. In fact, unlike the Central America experiences of SBM, where the main objective was to improve school enrolment in remote rural areas, the main objective of *Redes Amigas* was to improve students' cognitive achievements.

3.3 Decentralization of education in Ecuador

PROMECEB-REDES AMIGAS started to operate in 1990. The program had two phases: from 1990 to 1999 (PROMECEB) and from 1999 to 2005 (REDES AMIGAS).The main objective of the program was to improve students' cognitive achievements in the rural areas of Ecuador through the decentralization of school management and community participation. To this end, schools were assigned to medium-size administrative units called *Centros Escolares Matrices (CEMs).* Each center supervised between 15 and 30 schools, compared to the previous institutional framework, where one administrative unit (*Dirección provincial*) supervised up to 3000 schools. In the second phase of this program, starting in 1999, schools in the same CEM were encouraged to organize themselves into autonomous school networks (*Redes Amigas*). These networks have received additional resources coming from the program to define their own teacher-training strategy, and budget for infrastructure and teaching materials.

To support the school network program, participating community boards received a substantial amount of training and advisory services from the central administration. The total budget of the program in its second phase was $50 million, of which $45 million came from the Inter American Development Bank, and $5 million from the Ecuadorian government. The program was supposed to end in 2002, but an extension was approved and the program finished at the end of 2004. From 2005 onwards, the program becomes a permanent activity of the Ministry of Education. In fact, an administration unit in charge of coordinating school networks (*Redes Amigas*) was created in the Ministry of Education.

Every network had, on average, 12 schools, 750 pupils, and 31 teachers. A directive council, made up of 4 teachers, 3 members from the parents' committee, and 1 person from the community was put in charge of decisions related to administrative and pedagogical issues. In the pedagogical realm, the directive council was advised by the pedagogical committee, which consisted of the deputy director of the network and the

corresponding director of each school. Each network had its own budget and therefore the capacity to hire teachers. In contrast to other schools in Ecuador, where the budget is administered and owned by the provincial directorate for education (*"Dirección Provincial de Educación"*), under *Redes Amigas* resources were transferred and administered by the network.

To participate in the program, schools have to meet the following requirements: a) be located in rural areas, b) be registered at the Ministry of Education (MOE), and c) apply to the program and sign an agreement with the MOE. Before signing the agreement with the MOE, it is compulsory to integrate the directive council as well as the pedagogical committee. In addition, the parents' committee and teachers must fill out a form to join the network.

Every network had two components in its budget: the budget that comes from the program that is distributed according to Table 3.1, and the budget from the government that is used to pay teachers' salaries and to purchase goods and services.

Table 3.1
Budget distribution of Redes Amigas

Activity	Percentage of the budget
Teachers training	15%
Teaching material	25%
Infrastructure	36%
Equipment	14%
Community participation	6%
Audit and consulting services	4%

Source: Redes Amigas

The central unit in charge of administrating the program offered assistance to school networks for five basic purposes: a) teacher-training, b) teaching materials, c) infrastructure, d) equipment, and e) community participation. At the end of the program it covered around 140,000 pupils, 2,200 schools, and 6,000 teachers, with a total of 187 networks. Of this total, 30% were Indigenous networks. The total coverage of the program represents 58% of all public school students in rural areas, with 40% of those students living in the poorest regions of Ecuador. The

program achieved universal coverage among the indigenous schools from the Costa and Amazonía.[11]

By comparing the number of pupils that attended the program and the program's annual budget (US$ 10 million), one can conclude that the per capita program spending was US$ 70 per year.

Finally, it is important to mention that the program had a strong opposition from the teachers' union of Ecuador (*Unión Nacional de Educadores*), especially because of budget transfer from the *Direcciones Provinciales* to the networks. They were against the decentralization strategy because under the *Redes Amigas* structure salaries as well as work conditions for teachers are negotiated at each school level instead of with the Ministry of Education at national level. Teachers' union felt this mechanism as affecting their negotiation power. On the other hand, parents as well as teachers and school directors (not pertaining to the teachers' union) were very enthusiastic about participating in the program. Teachers and school directors saw the program as an opportunity to improve school conditions, to obtain additional resources for school infrastructure and teaching materials, and to improve teachers' conditions, especially in terms of training. Communities saw the program as a way of participating in and monitoring the education process.[12]

3.4 Empirical specification

As mentioned in the introduction, the main idea of an impact evaluation study is to isolate the effect of the intervention. The application of experimental studies requires the design of a baseline and follow-up survey to be applied to randomly assigned groups to treatment and control. *Redes Amigas* did not incorporate any impact evaluation design during its implementation. In this regard it was not possible to have an experimental design. In addition, the program did not have any baseline survey. Within this data constrain, this chapter evaluates the impact of the program by combining two quasi-experimental approaches. The main idea of the methodological strategy is to correct for un-observables by using a pipeline comparison design, and to correct observables by using a propensity score matching. In what follows we develop the identification strategy in more depth.

Following the extensive literature on educational production functions,[13] where the outcome variable is regressed on various input variables intervening in the education process, one can start with the follow-

ing model for the *i*-th student in the *n*-th school and in the *m*-th community:

$$Y_{inm} = f(X_{inm}, C_{nm}, T_{inm}) \quad (1)$$

Where Y is the outcome variable, X is a vector of student and household characteristics, C is a vector of community and school level variables prior to the program intervention that can influence program participation. T is an indicator variable which takes the value of 1 if a school belongs to a school network and zero if a school belongs to a quasi-network[14]. The main determinants of program participation, in our particular case, are school and community variables. As mentioned, schools that decided to participate in the program had to organize themselves and conform to the directive council (with the participation of community members). It is possible that communities with better organization and participation, as well as schools with more motivated principals, teachers and parents have a higher probability of participating in the program. In this regard, the level of community organization and some specific school characteristics seem important to understand program participation. Unfortunately, information at the school and community level prior to the program intervention is not abundant. In order to gather information on community and school level variables before the execution of the program, this research uses data from the 1990 population census, and from the Ministry of Education.[15] Variables at the parochial level were constructed using data from the 1990 population census. In this case the following variables were matched with our data; poverty incidence, the average years of schooling for those over 24 years of age, and the percentage of illiteracy among those over 14 years of age. In addition, prior to the second phase of the program, some variables from the Ministry of Education (1994) were computed both at the parochial and school level. At the parochial level, the average ratio of students per teacher, the average ratio of students per classroom, and the average ratio of students per school-building were calculated, while at the school level the repetition rate and the students per teacher ratio were used. These variables can have an impact on the outcome variables, as well as on program participation and are included as vector C in equation (1).[16]

Furthermore, program intervention can have an impact on school outcomes through the improvement of school and teacher characteristics, as well as changes in the management structure. One way of isolat-

ing the effect of changes in the institutional set up of a school is by including in equation (1) a vector of school and teacher characteristics that are influenced by program intervention. In this case, the model is:

$$Y_{inm} = f(X_{inm}, C_{nm}, S_n, T_{inm}) \quad (2)$$

Where *X*, *C* and *T* are the same as in equation (1), and *S* is a vector of school and teacher characteristics that are influenced by program execution, the difference between results for equation (2) and equation (1) is the effect of changes in the management structure.

Equation 1 can be specified empirically by using a linear model as follows:

$$Y_{inm} = \alpha_0 + X_{inm}\alpha_1 + \alpha_2 T_{inm} + \alpha_3 C_{nm} + \varepsilon_i \quad (3)$$

Where the alphas are the parameters to be estimated, and the key parameter of interest is α_2, ε is an error term normally distributed with zero mean and constant variance. To simplify the notation, error terms associated with the school and community level variables are omitted. The same can be extended to equation (2).

$$Y_{inm} = \beta_0 + X_{inm}\beta_1 + S_n\beta_2 + \beta_3 T_{inm} + \beta_4 C_{nm} + \mu_i \quad (4)$$

In this case the parameter of interest is β_3. Equations (3) and (4) will be estimated using OLS.

There are two limitations of estimates using equations 3 and 4. First, there may be unobserved variables that simultaneously influence program participation and test scores, and have not been accounted for in the model. In this regard, the treatment variable may be correlated to the error term leading to biased and inefficient estimates. To tackle this source of bias the paper relies on a pipeline comparison design.[17] This approach relies on using schools that have successfully applied for the program, but have not yet received it, as a comparison group. These applicants have already indicated a preference toward participation in the program (Angrist, 1998). Therefore, the comparison group is composed of schools that decided to organize themselves as networks, have integrated both the directive council, and the pedagogical committee, and have subscribed to the agreement with the Ministry of Education of Ecuador to participate in the program. However, the program, mainly because of time and budget constraints and some administrative issues, has

not yet been executed. These networks are referred to as quasi-networks.[18]

Second, the analysis is not restricted to the region of "common support"[19] and, in addition, uses a particular specification of the function (linear). One alternative to estimate program impact that allows one to correct both limitations is propensity score matching (PSM). This also allows one to correct for the existence of non-linearities. As already indicated, this paper restricts the PSM to the sample of applicants. The main idea is that by using the pipeline strategy one corrects for un-observables in the selection process, while by using a PSM one corrects for observables.[20] One strong assumption when performing a PSM is the un-confoundedness assumption. This means that program participation is exogenous or un-confounded with potential outcomes conditional on a sufficiently rich set of covariates or pretreatment variables. This can be expressed formally as follows:

$$Y_i(0), Y_i(1) \perp T_i \mid X_i$$

Where *Yi(0)* is the potential outcome for controls, and *Yi(1)* is the potential outcome for treatment. T and X were already defined and refers to the treatment and control variables respectively.

Under the un-confoundedness assumption the average treatment effect of the program for the treated can be estimated by comparing the outcomes for those in the treatment with those in the control group as follows:

$$ATT(x) \equiv E[Y(1) - Y(0) \mid X = x, T = 1]$$

If there are many covariates, it is recommended to use the propensity score (which is the conditional probability of receiving treatment given covariates). Rosenbaum and Rubin (1983, 1985) have shown that, under the assumption of un-confoundedness, adjusting solely for differences in the propensity score between treated and controls units removes all biases. The propensity score being:

$$p(x) \equiv \Pr(T = 1 \mid X = x) = E[T \mid X = x]$$

Which is assumed to be bounded away from zero and one:

$$0 < p(x) < 1$$

The probability of participating in the program can be computed by using an econometric model where the dependent variable is a dichotomous variable that takes the value of 1 for program participants and zero otherwise. One option is to use a probit model, where the program participation variable is regressed against community and school characteristics previous to program implementation (vector *C*). As already mentioned, community and school variables previous to program intervention are considered the main determinants of program participation. Moreover, the selection equation will control for some household variables.

Formally, let Y_i^k be the outcome variable for individual i in state k. There are two possible states for the outcome; k=1 in the presence of the program, and k=0 in its absence. The average treatment effect on the treated (ATT) when N participants in the program are matched to the nearest non-participant neighbor can be defined as follows:

$$ATT = (1/N)\sum_{i=1}^{N}(Y_i^1 - Y_i^0) \tag{5}$$

Where each participant (Y_i^1) is matched with the nearest non-participant (Y_i^0) based on the propensity score. It is important to note that a major source of bias while working with non-experimental studies is the failure to satisfy the common support condition (Heckman et al. 1998). Imposing common support means that inferences on the impact of the program can be confined to "comparable people" in terms of their propensity scores. Formally, the previous means that:

$$Supp(X \mid P = 1) = Supp(X \mid P = 0)$$

This condition is imposed in our PSM estimation.

Besides one-to-one matching, others types of matching are found in the literature (Ravallion, 2005). The five nearest neighbors and a Kernel matching will be used. In this case, in general terms, the estimator for the average treatment effect on the treated (ATT) can be defined by:

$$ATT = 1/N\sum_{i=1}^{N}(Y_i^1 - \sum_{j=1}^{C}W_{ij}Y_j^0) \tag{6}$$

Where W is the weight used in calculating the counterfactual for each participant, and C in the number of cases used to construct the counter-

factual for each participant. Kernel estimates are computed using the Epanechnikov Kernel because it presents the highest asymptotic efficiency among the alternative Kernel distributions (Mittelhammer et al., 2000).

Recent developments in the matching literature show potential efficiency problems of PSM estimates when using bootstrap to compute standard errors. (Imbens, 2004; Hirano, Imbens and Ridder, 2003). Hirano, Imbens and Ridder (2003) propose another way of matching to obtain fully efficient estimates. According to them, one can use weighted OLS in the following equation:

$$Y_i = \gamma_0 + \gamma_1 T_i + X_i \gamma_2 + \varepsilon_i \tag{7}$$

Where Y, T and X are already defined, and the weights used are defined as 1 for the treated units, and $\hat{P}(X)/(1-\hat{P}(X))$ for controls.[21] In this case $\hat{P}$ is the estimated propensity score from the selection equation. Under this approach one can also estimate equation (7) incorporating school and teacher variables (S_i) affected by program intervention in order to isolate the effect of the program because of changes on school management.

As indicated above, PSM estimates will be biased if there are unobserved variables that jointly influence program participation and test scores. This research tries to control for un-observables by using a pipeline design. However, it is still possible that some un-observables influence the timing of program participation. As an example, more enthusiastic and organized communities and teachers can apply to participate in the program earlier than less organized and less enthusiastic communities. In this regard, quasi-networks could pertain to less enthusiastic and less organized communities and teachers and, for this reason, obtain lower scores. In this case, estimates from the pipeline design combined with the PSM will be biased upwards. One can test for un-observables by using a similar logic as the one used to estimate models with sample selection bias.[22] Consequently, one way of testing for un-observables is by analyzing the partial correlation between the outcome variable of the principal equation and the residuals from the selection equation. To do the previous, one can run the following model.

$$Y_i = \delta_0 + \delta_1 T_i + X_i \delta_2 + \delta_3 R_i + \upsilon_i \tag{8}$$

Where Y_i is the outcome variable, T_i is a dummy for program participation, X_i is a vector of control variables, and R_i are the generalized residuals from the probit model used to compute the propensity score in the PSM.[23] Selection bias is determined if δ_3 is different from zero.[24] A key issue in this test is the identification strategy used to estimate the model. Although differences in the functional form of the two equations (non-linear in the selection equation, and linear in the outcome equation) aids identification, it is a weak basis for identifying the model. As already mentioned, we did not find strong instruments in the data that could be used in this test. As a reference, we included pre-intervention variables in the selection equation, and excluded those from equation (8). In any case results of the test have to be taken carefully.

In methodological terms, one strong limitation of this study is the existence of only one survey. As mentioned, the evaluation study was conducted at the end of the program and no baseline survey was available. It would have been useful to have at least two surveys to construct panel data. If un-observables remain unchanged between the baseline and the follow-up survey, one can control for un-observables by using a difference-in-difference approach. In addition, because learning is a cumulative process, it would have been better to analyze the change in test scores (value added approach) as an outcome variable instead of only one point in time. This would be possible with panel data. However, as mentioned, the program did not have a baseline and it was only possible to incorporate information prior to the program, from the 1990 population census, and from administrative data from the Ministry of Education.

3.5 Data and descriptive statistics

Data were collected by the Latin America Faculty of Social Sciences (*Facultad Latinoamericana de Ciencias Sociales (FLACSO-Ecuador)*), and refer to the rural area of Ecuador.[25] Students from second and fourth grade in primary schools were interviewed. The survey was carried out from November 2004 through February of 2005, and used three different instruments: school, teacher, and household questionnaires. In addition, students from second and fourth grades were evaluated using standardized tests in both mathematics and language. Those tests were designed by a pedagogical team and evaluated the level of basic skills achieved by chil-

dren in both language and mathematics. The skills evaluated were the same as those used by the Ecuadorian System of Educational Achievements Measurement ("*Sistema Nacional de Medición de Logros Académicos SNMLA"*). Tests were different for the Hispanic and the bilingual schools.[26] For every child the research team gathered information on their test scores (in both math and language), characteristics of schools and teachers, and household variables. The test scores, as well as school and teacher questionnaires, were completed in the school, while the household questionnaire was filled out at the child's home. The research used a multi-stage cluster random sampling design, where, in the first stage, networks (and quasi-networks) were randomly selected. In the second stage, all the schools pertaining to the selected network were interviewed, and, finally, in the third stage, all the students from second and fourth grade were interviewed and took the tests. The sample was designed to have statistical representation for Hispanic and Indigenous networks as well. For this purpose, Indigenous networks were oversampled. Bilingual schools from the coast, as well as schools from the *Amazonia* (jungle) could not be included in the sample because the program achieved universal coverage and no controls were available. For this reason, the sample is representative in the Hispanic system for the Sierra and Costa regions, while in the bilingual system just for the Sierra.

Table 3.2
Sample size and distribution

	Second grade	Fourth grade
Hispanic		
Treatment	491	422
Control	435	448
Bilingual		
Treatment	206	167
Control	206	181
Total	1338	1218

The comparison group consisted of the total quasi-networks available at the time of the evaluation. The number of students sampled in bilingual and Hispanic schools, for treatment and comparison groups, as well

as for second- and fourth-grade is introduced in Table 3.2. The total sample size is 1,338 children from second-grade, and 1,218 children from fourth grade. In the Hispanic case, for second-grade, there are 491 children in the treatment group, and 435 in the comparison group. In the bilingual case, there are 206 children in the treatment group and 206 in the comparison group. In fourth grade, there are 422 and 448 children in the treatment and control group in the Hispanic case, as well as 167 and 181 in the treatment and comparison group in the bilingual case.

At the school level the sample size is of 147 schools (94 in the Hispanic system and 53 in the bilingual system). To have more comparable results of program impact, the sample design took into account the time of program intervention in order to avoid strong disparities among schools in the Hispanic and the Bilingual system. In this sense, the average number of years of program intervention is 6 for the Hispanic schools and 6 for the bilingual schools. The school questionnaire had information about the director of the school, school infrastructure, the number of teachers and its schooling level and experience, the number of students, the number of classrooms, books, computers, labs and other school inputs, location, and some information about the year of the last improvements (in terms of infrastructure) in the school. The teachers' questionnaire was applied to the person in charge of teaching mathematics and language. In this case the survey obtained information about teacher's schooling, experience, the type of contract (hired by the Ministry of Education or by the school), and the number of training courses attended during the last four years.

The household-questionnaire starts with a register of every household member, their names, sex, age and their relationship to the head of the household. Then, there is a module on household assets and infrastructure. On an individual level, the survey collects information on the schooling level, parents' level of education, marital status, and the language spoken by all household members. In addition, employment status, labor conditions and incomes are noted among persons aged 5 and over. For children between the ages of 5 and 17, information on school enrolment, the type of school attended, education spending, and attendance is available. Finally, the questionnaire has some questions about the time spent by the child in order to record the number of hours he/she works, helps in housework, watches television, and the degree of assistance received to complete homework assignments.

School, teacher, and household variables were matched to child variables. Table 3.3 introduces descriptive statistics for children from treatment and comparison groups of second grade in the Hispanic system. In addition, variables previous to program intervention are also included. Starting with child variables, students from the treatment group have better test scores than students from the comparison group for both math and language. In addition, children from the treatment group dedicate more hours to homework and to study with their parents, and to watch television than children from the comparison group. On the other hand, students from the comparison group work more hours on weekdays than those on the treatment group. Regarding household variables, the score in the Selben index[27] as well as the schooling of the household's head are higher for those in the treatment group. The control group has a higher percentage of households headed by indigenous. In general, the treatment group has better socioeconomic conditions than the control group. No significant differences are found in terms of household composition, except for the number of members from 6 to 17-years old. In this case the control group has more members than the treatment group.

In relation to school variables, as expected, children from the treatment group attend schools with better infrastructure and school inputs than children from the comparison group. The treatment group attends schools with more books, than children from the comparison group. Furthermore, the index of infrastructure[28] (out of five) is higher among children from the treatment group. Additionally, an important difference is that the percentage of children from the control group that attend to multi-grade schools is higher than those in the treatment group.

Regarding teacher characteristics, some significant differences are observed between treatment and comparison groups. Children in the treatment group are taught predominantly for female and younger teachers than those in the comparison group. In addition, the percentage of children attending classes with teachers contracted by the Central Ministry is higher for the comparison group. As previously mentioned, one of the key elements of *Redes Amigas* was that economic resources were transferred to the networks and teachers are hired and fired by the network. No significant differences are found in terms of teacher-training or in teachers' academic level. Finally, all variables previous to intervention show significant differences between the two groups. Control group has

Table 3.3
Descriptive statistics for control and treatment: second grade (Hispanic system)

Variable	Second grade			
	Treatment	Control	Difference	
Child variables				
Math (out of 20)	11.9556	7.4327	4.5229	*
			(0.3042)	
Language (out of 20)	12.8209	9.4747	3.3462	*
			(0.2916)	
Dummy female=1	0.4889	0.4821	0.0068	
			(0.0320)	
Number of hours worked on weekdays	4.9339	6.8940	-1.9601	*
			(0.5316)	
Hours dedicated to homework (daily)	1.6614	1.5062	0.1552	*
			(0.0516)	
Hours that children study with parents (daily)	0.8691	0.4853	0.3838	*
			(0.0476)	
Hours dedicated to watch TV. (daily)	1.5164	0.8530	0.6634	*
			(0.0716)	
Household variables				
Score in Selben index	46.7527	37.5201	9.2326	*
			(0.6801)	
Schooling of the household's head	6.5983	5.5063	1.0920	*
			(0.2225)	
Household's head is indigenous (%)	0.1030	0.3494	-0.2464	*
			(0.0255)	
Household's head is illiterate (%)	0.0991	0.1284	-0.0293	
			(0.0203)	
Household's head is female (%)	0.1474	0.1052	0.0422	**
			(0.0213)	
Number of members younger than 6 in the hh.	0.8591	0.9517	-0.0926	
			(0.0611)	
Number of members from 6 to 17 in the hh.	2.6690	2.9308	-0.2618	**
			(0.0883)	
Number of members from 18 to 44 in the hh.	1.8812	1.9224	-0.0412	
			(0.0588)	
Number of members from 45 to 64 in the hh.	0.3963	0.3731	0.0232	
			(0.0428)	
Number of members older than 64 in the hh.	0.1066	0.1341	-0.0275	
			(0.0260)	

(Continued)

higher levels of poverty incidence, illiteracy rates, and students per teacher at the parochial level.

Table 3.4 shows the same descriptive statistics for fourth grade in the Hispanic system. Results are similar to those found in second grade.

Table 3.3 *(Continuation)*

Variable	Second grade			
	Treatment	Control	Difference	
School variables				
% of children attending multi-grade schools	0.1649	0.2620	-0.0971	*
			(0.0260)	
% of children attending schools with full time director	0.0523	0.0628	-0.0105	
			(0.0149)	
Number of learning guides per child	0.0590	0.1366	-0.0776	*
			(0.0190)	
% of children attending schools with at least one computer	0.7706	0.7316	0.0390	
			(0.0276)	
Number of books per student	3.3459	2.4758	0.8701	**
			(0.3420)	
% of children attending schools with access to internet	0.0000	0.0628	-0.0628	*
			(0.0109)	
Index of school infrastructure (out of five)	4.0523	3.6058	0.4465	*
			(0.0485)	
Teacher variables				
% of children with female teacher	0.8692	0.6142	0.2550	*
			(0.0267)	
Average age of teachers	34.2535	40.9580	-6.7045	*
			(0.5940)	
% of children with teachers with superior academic level	0.7786	0.7987	-0.0201	
			(0.0261)	
% of children with teachers contracted by the Ministry	0.7545	0.9098	-0.1553	*
			(0.0235)	
Average number of training courses received by teachers (last four years)	8.3179	8.7861	-0.4682	
			(0.5279)	
Variables previous to intervention				
Poverty incidence	69.5771	81.0860	-11.5089	*
			(0.9308)	
Ratio student per teacher (parochial level)	12.8306	16.4648	-3.6342	*
			(0.2880)	
Ratio student per classroom (parochial level)	22.1266	21.8079	0.3187	
			(0.2796)	
Ratio student per school building (parochial level)	88.6374	79.0331	9.6043	*
			(2.5925)	
Illiteracy rate (parochial level)	13.8246	18.6785	-4.8539	*
			(0.3855)	
Years of schooling (parochial level)	5.3452	3.9758	1.3694	
			(0.0754)	
Percentage of people with superior education level	0.0752	0.0325	0.0427	*
			(0.0022)	
Repetition rate (school level)	0.0179	0.0256	-0.0077	**
			(0.0022)	
Ratio student per teacher (school level)	29.7346	25.8327	3.9019	*
			(0.6783)	

Note for Tables 3.3-3.6: Standard errors are in parenthesis. *Significant at 1%. ** Significant at 5%. *** Significant at 10%.

Table 3.4
Descriptive statistics for control and treatment: fourth grade (Hispanic system)

Variable	Fourth grade			
	Treatment	Control	Difference	
Child variables				
Math (out of 20)	9.7249	7.2570	2.4679	*
			(0.2620)	
Language (out of 20)	13.7762	11.9739	1.8023	*
			(0.2860)	
Dummy female=1	0.5221	0.4598	0.0623	**
			(0.0328)	
Number of hours worked on weekdays	5.3165	7.0916	-1.7751	*
			(0.4847)	
Hours dedicated to homework (daily)	1.7513	1.5638	0.1875	*
			(0.0556)	
Hours that children study with parents (daily)	0.6567	0.4278	0.2289	*
			(0.0453)	
Hours dedicated to watch TV (daily)	1.5705	0.9379	0.6326	*
			.0765	
Household variables				
Score in Selben index	46.3854	37.9890	8.3964	*
			(0.6942)	
Schooling of the household's head	6.1563	5.4878	0.6685	*
			(0.2339)	
Household's head is indigenous (%)	0.1007	0.3279	-0.2272	*
			(0.0264)	
Household's head is illiterate (%)	0.1384	0.1428	-0.0044	
			(0.0229)	
Household's head is female (%)	0.1662	0.1106	0.0556	**
			(0.0225)	
Number of members younger than 6 in the hh.	0.6736	0.8172	-0.1436	**
			(0.0581)	
Number of members from 6 to 17 in the hh.	2.8158	3.1445	-0.3287	**
			(0.0878)	
Number of members from 18 to 44 in the hh.	1.9090	1.9196	-0.0106	
			(0.0661)	
Number of members from 45 to 64 in the hh.	0.4335	0.5100	-0.0765	
			(0.0482)	
Number of members older than 64 in the hh.	0.0885	0.0963	-0.0078	
			(0.0214)	

(Continued)

Table 3.4 *(Continuation)*

Variable	Fourth grade			
	Treatment	Control	Difference	
School variables				
% of children attending multi-grade schools	0.1561	0.1726	-0.0165	
			(0.0244)	
% of children attending schools with full time director	0.0372	0.0522	-0.0150	
			(0.0137)	
Number of learning guides per child	0.0598	0.1307	-0.0709	*
			(0.0196)	
% of children attending schools with at least one computer	0.7132	0.7891	-0.0759	*
			(0.0282)	
Number of books per student	3.0525	3.1087	-0.0562	
			(0.3012)	
% of children attending schools with internet access	0.0000	0.0522	-0.0522	*
			(0.0107)	
Index of school infrastructure (out of five)	3.9976	3.7040	0.2936	*
			(0.0470)	
Teacher variables				
% of children with female teacher	0.5804	0.6385	-0.0581	***
			(0.0320)	
Average age of teachers	39.5244	40.8895	-1.3651	**
			(0.6155)	
% of children with teachers with superior academic level	0.8275	0.8493	-0.0218	
			(0.0242)	
% of children with teachers contracted by the Ministry	0.8484	0.8995	-0.0511	**
			(0.0216)	
Average number of training courses received by teachers (last four years)	8.6247	7.7991	0.8256	
			(0.3730)	
Variables previous to intervention				
Poverty incidence	69.3247	78.9492	-9.6245	*
			(0.9974)	
Ratio student per teacher (parochial level)	13.0975	16.2871	-3.1896	
			(0.2942)	
Ratio student per classroom (parochial level)	22.3800	22.3388	0.0412	
			(0.2869)	
Ratio student per school building (parochial level)	85.9899	82.2709	3.7190	
			(2.6345)	
Illiteracy rate (parochial level)	13.7579	17.8593	-4.1014	*
			(0.4084)	
Years of schooling (parochial level)	5.3110	4.1316	1.1794	*
			(0.0803)	
Percentage of people with superior education level	0.0740	0.0353	0.0387	
			(0.0022)	
Repetition rate (school level)	0.0199	0.0287	-0.0088	*
			(0.0023)	
Ratio student per teacher (school level)	29.7452	26.3407	3.4045	*
			(0.6465)	

Table 3.5
Descriptive statistics for control and treatment: second grade (bilingual system)

Variable	Second grade			
	Treatment	Control	Difference	
Child variables				
Math (out of 20)	5.8309	8.6895	-2.8586	*
			(0.4435)	
Language (out of 20)	7.6291	11.2780	-3.6489	*
			(0.3909)	
Dummy female=1	0.5258	0.4729	0.0529	
			(0.0455)	
Number of hours worked on weekdays	8.8685	27.4440	-18.5755	*
			(1.6589)	
Hours dedicated to homework (daily)	1.4830	1.4034	0.0796	
			(0.0725)	
Hours that children study with parents (daily)	0.3497	0.2768	0.0729	**
			(0.0216)	
Hours dedicated to watch TV. (daily)	0.2417	0.7996	-0.5579	*
			(0.0907)	
Household variables				
Score in Selben index	27.4680	34.3170	-6.8490	*
			(0.9967)	
Schooling of the household's head	2.3238	4.4053	-2.0815	*
			(0.3536)	
Household's head is indigenous (%)	0.9526	0.9407	0.0119	
			(0.0208)	
Household's head is illiterate (%)	0.4312	0.2771	0.1541	*
			(0.0433)	
Household's head is female (%)	0.1327	0.1962	-0.0635	***
			(0.0343)	
Number of members younger than 6 in the hh.	1.1830	0.9134	0.2697	**
			(0.0907)	
Number of members from 6 to 17 in the hh.	3.0751	3.1552	-0.0801	
			(0.1157)	
Number of members from 18 to 44 in the hh.	1.8967	1.6895	0.2072	**
			(0.0982)	
Number of members from 45 to 64 in the hh.	0.5258	0.5054	0.0204	
			(0.0717)	
Number of members older than 64 in the hh.	0.1971	0.1263	0.0708	
			(0.0488)	

(Continued)

Table 3.5 *(Continuation)*

Variable	Second grade			
	Treatment	Control	Difference	
School variables				
% of children attending multi-grade schools	0.2112	0.1588	0.0524	***
			(0.0351)	
% of children attending schools with full-time director	0.0000	0.0252	-0.0252	**
			(0.0107)	
Number of learning guides per child	0.0852	0.0214	0.0638	*
			(0.0154)	
% of children attending schools that have at least one computer	0.2159	0.1335	0.0824	**
			(0.0340)	
Number of books per student	1.5249	1.4303	0.0946	
			(0.3131)	
% of children attending schools that have access to internet	0.0000	0.0000	0.0000	
			0	
Index of school infrastructure (out of five)	3.2582	3.4945	-0.2363	
			(0.0988)	
Teacher variables				
% of children with female teacher	0.4272	0.4332	-0.0060	
			(0.0452)	
Average age of teachers	32.7511	30.8808	1.8703	**
			(0.7063)	
% of children with teachers with superior academic level	0.7464	0.7256	0.0208	
			(0.0403)	
% of children with teachers contracted by the Ministry	0.5023	0.5234	-0.0211	
			(0.0456)	
Average number of training courses received by teachers (last four years)	9.1220	7.7111	1.4109	
			(1.4113)	
Variables previous to intervention				
Poverty incidence	92.3483	90.2179	2.1304	**
			(0.8030)	
Ratio student per teacher (parochial level)	16.8101	20.1332	-3.3231	*
			(0.6149)	
Ratio student per classroom (parochial level)	26.0079	25.0079	0.9999	**
			(0.46079)	
Ratio student per school building (parochial level)	98.3741	81.1411	17.2330	*
			(3.8508)	

(Continued)

***Table 3.5** (Continuation)*

Variable	Second grade			
	Treatment	Control	Difference	
Illiteracy rate (parochial level)	40.4559	41.5311	-1.0752	
			(1.5464)	
Years of schooling (parochial level)	3.4575	2.7664	0.6911	*
			(0.1875)	
Percentage of people with superior education level	0.0739	0.0612	0.0128	*
			(0.0048)	
Repetition rate (school level)	0.0025	0.0007	0.0018	**
			(0.0007)	
Ratio student per teacher (school level)	24.6347	24.6316	0.0031	
			(0.8529)	

Table 3.5 presents descriptive statistics for second grade in the bilingual system. In this case, students from the comparison group have higher test scores than those of the treatment group in both mathematics and language. In addition, children from the comparison group work considerably more hours on weekdays, and dedicate fewer hours to study with parents than those in the treatment group. Regarding household variables, children from the comparison group have better socioeconomic conditions that those from the treatment group. The comparison group has a higher score in the Selben index, and the head of the household has a higher level of education than the treatment group. No significant differences are found in the percentage of households headed by indigenous peoples. One interesting point in the indigenous case is that most of school conditions are not statistically different between comparison and treatment groups. There are no differences in the infrastructure index, the number of books, or access to Internet. However, there are better conditions for the comparison group in terms of multi-grade schools, learning-guides per student, and full time director. No significant differences are found in terms of teacher variables. Finally, the comparison group has a lower poverty incidence, but the treatment group has better schooling levels at the parochial level.

Similar results are observed in fourth-grade. See Table 3.6.

Table 3.6
Descriptive statistics for control and treatment: fourth grade (bilingual system)

Variable	Fourth grade			
	Treatment	Control	Difference	
Child variables				
Math (out of 20)	6.4137	9.2731	-2.8594	*
			(0.3595)	
Language (out of 20)	7.1666	9.8981	-2.7315	*
			(0.3757)	
Dummy female=1	0.5057	0.4444	0.0613	
			(0.0508)	
Number of hours worked on weekdays	9.9684	33.3310	-23.3626	*
			(2.3870)	
Hours dedicated to homework (daily)	1.4425	1.4876	-0.0451	
			(0.0748)	
Hours that children study with parents (daily)	0.3060	0.2530	0.0530	
			(0.0508)	
Hours dedicated to watch TV (daily)	0.2442	0.5787	-0.3345	*
			(0.0854)	
Household variables				
Score in Selben index	28.5939	34.7311	-6.1372	*
			(1.1305)	
Schooling of the household's head	3.2941	4.4272	-1.1331	**
			(0.4582)	
Household's head is indigenous (%)	0.9298	0.9209	0.0089	
			(0.0270)	
Household's head is illiterate (%)	0.3567	0.3411	0.0156	
			(0.0489)	
Household's head is female (%)	0.1111	0.2046	-0.0935	**
			(0.0376)	
Number of members younger than 6 in the hh.	1.0517	0.7870	0.2647	**
			(0.0943)	
Number of members from 6 to 17 in the hh.	3.4425	3.1064	0.3361	**
			(0.1332)	
Number of members from 18 to 44 in the hh.	1.9195	1.8888	0.0307	
			(0.1128)	
Number of members from 45 to 64 in the hh.	0.5747	0.5046	0.0701	
			(0.0788)	
Number of members older than 64 in the hh.	0.1264	0.0879	0.0385	
			(0.0367)	

(Continued)

Table 3.6 *(Continuation)*

Variable	Fourth grade			
	Treatment	Control	Difference	
School variables				
% of children attending multi-grade schools	0.2241	0.1435	0.0806	**
			(0.0389)	
% of children attending schools with full-time director	0.0000	0.0324	-0.0324	**
			(0.0134)	
Number of learning guides per child	0.0617	0.0244	0.0373	*
			(0.0130)	
% of children attending schools with at least one computer	0.2298	0.1481	0.0817	**
			(0.0394)	
Number of books per student	1.5634	1.5041	0.0593	
			(0.3921)	
% of children attending schools with Internet access	0.0000	0.0000	0.0000	
			0	
Index of school infrastructure (out of five)	3.1954	3.5370	-0.3416	*
			(0.1086)	
Teacher variables				
% of children with female teacher	0.3045	0.2592	0.0453	
			(0.0457)	
Average age of teachers	33.1436	32.4675	0.6761	
			(0.8053)	
% of children with teachers with superior academic level	0.8965	0.7824	0.1141	
			(0.0376)	
% of children with teachers contracted by the Ministry	0.6551	0.6712	-0.0161	
			(0.0482)	
Average number of training courses received by teachers (last four years)	6.4425	6.5972	-0.1547	
			(0.5974)	
Variables previous to intervention				
Poverty incidence	93.0994	90.8220	2.2774	**
			(0.9022)	
Ratio student per teacher (parochial level)	18.2533	20.9606	-2.7073	*
			(0.7116)	
Ratio student per classroom (parochial level)	25.0303	25.1932	-0.1629	
			(0.4567)	
Ratio student per school building (parochial level)	99.4929	75.4402	24.0527	*
			(4.0879)	
Illiteracy rate (parochial level)	43.2205	42.7989	0.4216	
			(1.8088)	
Years of schooling (parochial level)	3.2825	2.5806	0.7019	*
			(0.2069)	
Percentage of people with superior education level	0.0706	0.0572	0.0134	**
			(0.0052)	
Repetition rate (school level)	0.0015	0.0004	0.0011	**
			(0.0006)	
Ratio student per teacher (school level)	26.7809	25.3750	1.4059	
			(1.0821)	

In sum, from this descriptive analysis one can conclude that, in the Hispanic case, schools that finally received program intervention are composed of students with better socioeconomic background than schools that did not receive treatment. In the bilingual case, it is the opposite. Schools that receive the intervention are composed of students with worse socioeconomic conditions than schools that do not receive treatment. If the pipeline design worked appropriately one would not expect such differences. In addition, the large differences in observable characteristics amongst the treatment and control groups raises doubts about the ability of the pipeline comparison design to control for differences in unobservable characteristics. This issue will be evaluated later on in the text.

Finally, in the Hispanic system, treatment schools have better infrastructural and learning conditions that those in the comparison group, while in the bilingual system no significant differences are observed between treatment and comparison schools. This could mean differences in program application between the Hispanic and the bilingual system. In this regard, it seems important to highlight that the application of the program in the bilingual system started at the end of the 1990s. For this reason, the pedagogical materials and school inputs used by the program were the same as those developed under the Hispanic system. Although the first experiences of bilingual schools started in the early 1980s, the pedagogical materials used by the program did not reflect this important experience. Additionally, in the bilingual case the Ministry explicitly promoted the participation in the program of the poorer schools. In the Hispanic case, meanwhile, there was more self-selection, and participation in the program depended more on the community, teachers and parents' willingness.

3.6 Results

3.6.1 OLS estimation

The first step to evaluate the program impact, as indicated in the methodological section, was to run an education production function. In this regard, OLS estimates of equations 3 and 4 are introduced in Tables from 3.7 to 3.10. In those tables the following specifications were used. Specification 1 includes only the treatment variable to see the simple difference in test scores between treatment and comparison groups. Speci-

fication 2 includes, in addition, child[29] and household variables. Observed household and children characteristics reflect parents' ability to provide a supportive environment for their children. At the individual level, some characteristics that seem important are; sex, since parents or teachers may treat boys and girls differently, and age, since older students are more mature and more likely to score higher, and they can also have potential achievement problems. Because of credit market imperfections in the Ecuadorian context, assets variables, expressed through the Selben index, are included to take into account socioeconomic circumstances of the household. In addition, parent's characteristics, which can affect living standards and preferences for children's education, are also included (ethnicity, and illiteracy). Finally, household composition seems important because more children implies less time for parents to dedicate to every child. Specification 3 includes, in addition, community and school level characteristics previous to program intervention. As already mentioned, community and school level characteristics were important to determine program participation. Poverty and illiteracy levels give us an idea of the socioeconomic level of the community. In addition, student per teacher, student per school building, and student per classroom, were computed at parochial level and included to proportionate an idea of the schooling context of the community. Finally, some variables at school level are also included, such as the repetition rate and the student per classroom ratio.[30] Specification 4 includes, in addition, some school and teacher characteristics that are influenced by program execution and are expected to affect test scores. The following variables were included in this case; the school infrastructure index, the number of learning-guides per student, the number of textbooks per students, a dummy variable for schools with full-time principal, and the number of training courses received by teachers in the last four years. Those variables try to capture the different components of program execution. As stated above, specification 3 represents the overall effect of the program, while specification 4 estimates the effect of changes in school management after controlling for improvements in school inputs.

Table 3.7
OLS estimates of program impact on test scores: equations 3 and 4 second grade (Hispanic system)

	Mathematics, second grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	4.4423*	0.6766	4.1638*	0.7115	3.9356*	0.7712	3.5757*	0.7562
Age			1.0425	1.3001	1.4305	1.3237	1.3588	1.3251
Age squared			-0.0519433	0.0701	-0.0674	0.0714	-0.0670	0.0721
Dummy (1=female)			0.1220	0.3165	0.17450	0.3035	0.1857	0.3050
Selben index			0.0224	0.0238	0.0163	0.0240	0.0310	0.0201
Schooling of the head of hh.			-0.0739	0.0513	-0.0716	0.0475	-0.0796	0.0489
Head of hh indigenous			-1.0153***	0.5618	-1.2779**	0.6017	-1.4140**	0.5740
Head of hh is female			0.2980	0.5773	0.2719	0.5512	0.2798	0.5416
Hh members aged <6			-0.004789	0.1801	-0.0900	0.1758	-0.0371	0.1705
Hh members aged 6–17			-0.1752834	0.1465	-0.1968	0.1381	-0.1644	0.1369
Hh members aged 18–44			0.1823***	0.1864	0.1642	0.1757	0.1019	0.1687
Hh members aged 45–65			-0.4040645	0.2185	-0.3191	0.2060	-0.3315	0.2078
Hh members aged >65			0.1560846	0.3764	-0.0162	0.3781	-0.0058	0.3715
Dummy for region (Costa=1)			-0.0368644	0.7902	-1.4881	0.9248	-1.1934	0.9398
Poverty incidence (parrochial level, 1990)					-0.0656**	0.0292	-0.0630**	0.0269
Illiteracy rate (parrochial level, 1990)					-0.00465	0.0567	0.0049	0.0502
Student per teacher (parrochial level, 1994)					0.00865	0.0753	-0.0353	0.0732
Student per school building (parroch. level)					-0.0351*	0.0125	-0.0340**	0.0113
Student per classroom (parrochial level, 1994)					0.2719**	0.1330	0.2753**	0.1320
Repetition rate (school level, 1994)					-18.074**	8.1353	-18.234**	9.0326
Student per teacher (school level, 1994)					0.0109	0.0313	0.0144	0.0300
School infrastructure index							-0.0106	0.3658
Number of learnig guides per student							1.5003	1.0537
Number of textbooks per student							0.03941	0.0373
Dummy (full time principal = 1)							0.0610	2.2251
Number of training courses for teachers							-0.0614**	0.0244
Constant	7.4965*	0.4532	2.5207	0.687	3.3234	6.8149	3.773575	6.6725
Number of cases	926		926		926		926	
R squared	0.1792		0.1971		0.2385		0.2541	

Language, second grade

	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	3.0061*	0.6860	2.1808*	0.6843	2.0289*	0.6526	2.1475*	0.6705
Age			1.2556	0.9534	1.5144***	0.9258	1.776**	0.8722
Age squared			-0.0692	0.0542	-0.0818	0.0524	-0.0962**	0.0489
Dummy (1=female)			-0.0589	0.2997	-0.0711	0.2865	-0.1011	0.2967
Selben index			0.0738*	0.0263	0.0675*	0.0245	0.0577**	0.0236
Schooling of the head of hh.			-0.0099	0.0561	-0.0091	0.0544	-0.00771	0.0548
Head of hh is indigenous			-1.8825*	0.6336	-1.4854**	0.6368	-1.368**	0.6555
Head of hh is female			-0.1506	0.4984	-0.1758	0.4771	-0.2384	0.4823
Hh members aged <6			0.1923	0.1736	0.1100	0.1750	0.1068	0.1778
Hh members aged 6–17			-0.0036	0.1121	-0.0282	0.1163	-0.0397	0.1157
Hh members aged 18–44			-0.0892	0.2036	-0.0786	0.2073	-0.075	0.2066
Hh members aged 45–65			-0.0896	0.2600	-0.0495	0.2588	-0.0363	0.2583
Hh members aged >65			-0.3103	0.4447	-0.2853	0.4201	-0.2720	0.4135
Dummy for region (Costa=1)			-0.3569	0.7044	-1.535***	0.8467	-1.6172**	0.8241
Poverty incidence (parrochial level, 1990)					-0.0147	0.0181	-0.0212	0.0210
Illiteracy rate (parrochial level, 1990)					-0.113**	0.0456	-0.0998**	0.0500
Student per teacher (parrochial level, 1994)					0.0399	0.0871	0.0763	0.1000
Student per school building (parroch. level)					-0.0217**	0.0092	-0.0207**	0.0095
Student per classroom (parrochial level, 1994)					0.1697	0.1183	0.1403	0.1196
Repetition rate (school level, 1994)					-14.786**	6.8832	-16.882**	7.2197
Student per teacher (school level, 1994)					0.020	0.0267	0.0197	0.0254
School infrastructure index							0.2547	0.4704
Number of learnig guides per student							-0.3612	0.5362
Number of textbooks per student							0.0430	0.0343
Dummy (full time principal = 1)							1.2230***	0.7380
Number of training courses for teachers							0.0333	0.0242
Constant	9.8068*	0.4540	2.1964	4.2114	1.9074	4.5973	0.0465	5.0873
Number of cases	926		926		926		926	
R squared	0.1013		0.1638		0.204		0.2133	

Note: *Significant at 1%, ** Significant at 5%, and *** significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations

Table 3.8
OLS estimates of program impact on test scores: equation 3 and 4 fourth-grade (Hispanic system)

	Mathematics, fourth grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	2.3588*	0.5487	2.168*	0.5325	2.177*	0.5462	2.201*	0.5023
Age			1.108	1.2863	0.8467	1.2787	0.8994	1.2834
Age squared			-0.0529	0.0567	-0.040	0.0563	-0.0424	0.0570
Dummy (1=female)			0.5360	0.3008	0.584**	0.2977	0.5737**	0.2864
Selben index			-0.0007	0.0241	-0.002	0.0249	0.0032	0.0232
Schooling of the head of hh.			-0.0278	0.0451	-0.041	0.0480	-0.0149	0.0451
Head of hh is indigenous			-1.2218**	0.5861	-1.602*	0.5181	-1.327*	0.5004
Head of hh is female			0.2491	0.4802	0.3050	0.4549	0.4108	0.4645
Hh members aged <6			0.0529	0.1803	0.0168	0.1728	0.0496	0.1634
Hh members aged 6–17			0.0447	0.1405	0.0261	0.1326	0.0273	0.1295
Hh members aged 18–44			0.2106	0.1312	0.211	0.1313	0.1723	0.1348
Hh members aged 45–65			-0.0292	0.2009	-0.0533	0.2082	0.0304	0.2015
Hh members aged >65			0.8115**	0.3566	0.717**	0.3546	0.8129**	0.3406
Dummy for region (Costa=1)			0.1146	0.5849	-0.0741	0.5625	0.0402*	0.5169
Poverty incidence (parrochial level, 1990)					-0.0586	0.0204	-0.0548*	0.0209
Illiteracy rate (parrochial level, 1990)					0.0830	0.0590	0.1022***	0.0632
Student per teacher (parrochial level, 1994)					0.1421	0.0981	0.132***	0.0791
Student per school building (parroch. level)					-0.0024	0.0081	0.0029	0.0074
Student per classroom (parrochial level, 1994)					-0.1127	0.1060	-0.1395	0.0928
Repetition rate (school level, 1994)					-7.986	8.0373	-9.511	7.8926
Student per teacher (school level, 1994)					0.0424	0.0328	0.050	0.0313
School infrastructure index							0.268	0.3850
Number of learnig guides per student							2.815*	0.8247
Number of textbooks per student							0.113*	0.0525
Dummy (full time principal = 1)							1.1559***	0.7021
Number of training courses for teachers							-0.0237	0.0385
Constant	7.401*	0.3419	1.238	7.1008	5.590	6.9270	2.861	6.9043
Number of cases	870		870		870		870	
R squared	0.0809		0.1064		0.1483		0.1959	

Language, fourth grade

	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	1.536**	0.7927	0.7602	0.6903	1.1123**	0.5360	1.139**	0.4372
Age			2.287**	1.1401	1.957***	1.0954	1.982**	1.0390
Age squared			-0.0976**	0.0500	-0.084***	0.0482	-0.085***	0.0455
Dummy (1=female)			0.2657	0.2780	0.3252	0.2594	0.3326	0.2512
Selben index			0.1243*	0.0218	0.0851*	0.0186	0.084*	0.0174
Schooling of the head of hh.			0.0385	0.0484	-0.0114	0.0427	0.0084	0.0401
Head of hh is indigenous			-1.502**	0.6218	-0.6617	0.4603	-0.4943	0.4765
Head of hh is female			-0.3459	0.4126	-0.4530	0.4149	-0.4759	0.4238
Hh members aged <6			0.0535	0.2155	-0.1291	0.1759	-0.118	0.1706
Hh members aged 6–17			-0.0058	0.0996	-0.111	0.1089	-0.1526	0.1110
Hh members aged 18–44			0.0619	0.1502	0.104	0.1367	0.0386	0.1380
Hh members aged 45–65			-0.1732	0.2109	-0.121	0.1939	-0.1003	0.2010
Hh members aged >65			0.6202	0.3784	0.4769	0.3776	0.4974	0.3863
Dummy for region (Costa=1)			-1.255***	0.6879	-2.037*	0.6441	-1.996*	0.5498
Poverty incidence (parrochial level, 1990)					-0.058*	0.0236	-0.0578*	0.0199
Illiteracy rate (parrochial level, 1990)					-0.1367*	0.0393	-0.1273*	0.0418
Student per teacher (parrochial level, 1994)					0.233*	0.0467	0.2691*	0.0579
Student per school building (parroch. level)					-0.0044	0.0074	-0.0006	0.0065
Student per classroom (parrochial level, 1994)					-0.0472	0.0985	-0.1076	0.0851
Repetition rate (school level, 1994)					-0.1758	8.0768	-4.4564	7.4929
Student per teacher (school level, 1994)					-0.0204	0.0356	-0.021	0.0313
School infrastructure index							0.1248	0.3313
Number of learnig guides per student							1.147**	0.5417
Number of textbooks per student							0.193**	0.0757
Dummy (full time principal = 1)							0.5116	0.6286
Number of training courses for teachers							-0.051	0.0369
Constant	12.281+	0.4164	-5.361	6.6613	3.9555	6.6544	3.554	6.6805
Number of cases	870		870		870		870	
R squared	0.0312		0.2002		0.2875		0.3272	

Note: *Significant at 1%, ** Significant at 5% *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Table 3.9
OLS estimates of program impact on test scores: equation 3 and 4 second grade (bilingual system)

	Mathematics, second grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-2.5388**	1.190	-1.7109	1.2581	-3.2841*	1.2009	-3.444*	1.2703
Age			1.3418	0.8363	0.6592	0.7046	0.6339	0.7317
Age squared			-0.0496	0.0391	-0.0201	0.0345	-0.0152	0.0361
Dummy (1=female)			-0.5408	0.4585	-0.8814**	0.4314	-0.7419	0.4049
Selben index			0.1179*	0.0384	0.0252	0.0344	0.0204	0.0315
Schooling of the head of hh.			-0.0361	0.0595	0.0133	0.0611	0.0335	0.0555
Head of hh is indigenous			-1.8241**	0.9230	-1.8436**	0.7558	-1.7161**	0.8329
Head of hh is female			-0.9111	0.7032	-0.6577	0.5423	-0.3815	0.5445
Hh members aged <6			0.3485	0.2814	-0.0035	0.2420	0.0844	0.2212
Hh members aged 6–17			0.0007	0.1674	0.0217	0.1761	-0.0644	0.1715
Hh members aged 18–44			-0.2177	0.2565	-0.1842	0.2312	-0.2529	0.2197
Hh members aged 45–65			0.3179	0.2973	0.2138	0.3018	0.2595	0.3163
Hh members aged >65			-0.5930**	0.3015	-0.4133	0.3210	-0.3722	0.3524
Poverty incidence (parrochial level, 1990)					0.0231	0.1182	0.0952	0.1225
Illiteracy rate (parrochial level, 1990)					-0.0514	0.0956	-0.1366	0.0912
Student per teacher (parrochial level, 1994)					-0.2155	0.1950	-0.1305	0.1592
Student per school building (parrochial level)					-0.0134	0.0189	-0.0159	0.0203
Student per classroom (parrochial level, 1994)					0.2187**	0.0955	0.1044	0.0888
Repetition rate (school level, 1994)					162.261*	35.9733	133.20*	39.0924
Student per teacher (school level, 1994)					-0.0828	0.0727	-0.0151	0.0706
School infrastructure index							-0.3485	0.4342
Number of learnig guides per student							-1.5013	2.0665
Number of textbooks per student							0.0385	0.0693
Dummy (full time principal = 1)							-7.9001*	2.0463
Number of training courses for teachers							0.0820*	0.0199
Constant	8.3446*	0.7113	-1.6533	5.317902	7.4120	8.2398	4.908	7.7323
Number of cases	412		412		412		412	
R squared	0.0677		0.1387		0.3132		0.391	

Language, second grade

	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-3.2912*	1.0544	-2.957**	1.1555	-3.259**	1.3342	-3.838*	1.3734
Age			0.4756	0.6048	0.5037	0.5114	0.5455	0.5169
Age squared			-0.0150	0.0273	-0.0139	0.0243	-0.0158	0.0243
Dummy (1=female)			-0.6372	0.4802	-0.9071**	0.4169	-0.9502**	0.4032
Selben index			0.0586	0.0398	0.0105	0.0366	0.0205	0.0373
Schooling of the head of hh.			-0.0478	0.0563	0.0075	0.0536	-0.0085	0.0540
Head of hh is indigenous			-1.595***	0.9170	-0.2879	0.9863	-0.3796	1.0179
Head of hh is female			0.8544	0.6097	0.5981	0.6341	0.5441	0.6428
Hh members aged <6			0.2790	0.2845	0.1560	0.2068	0.1745	0.2067
Hh members aged 6–17			-0.0755	0.1754	-0.0721	0.1690	-0.0987	0.1704
Hh members aged 18–44			-0.1662	0.2141	-0.1878	0.2123	-0.1695	0.1989
Hh members aged 45–65			0.0249	0.2424	-0.0059	0.2396	0.0291	0.2324
Hh members aged >65			-0.0446	0.2990	-0.0241	0.3416	-0.1982	0.3426
Poverty incidence (parrochial level, 1990)					0.1525	0.1038	0.2074***	0.1080
Illiteracy rate (parrochial level, 1990)					-0.2339*	0.0835	-0.2382**	0.0902
Student per teacher (parrochial level, 1994)					0.1240	0.1721	0.0810	0.1685
Student per school building (parrochial level)					-0.0117	0.0214	-0.0077	0.0210
Student per classroom (parrochial level, 1994)					-0.0422	0.0760	-0.0425	0.1003
Repetition rate (school level, 1994)					62.805**	24.8776	60.60***	34.6953
Student per teacher (school level, 1994)					0.0378	0.0555	0.0632	0.0625
School infrastructure index							0.0460	0.4022
Number of learnig guides per student							3.038***	1.6673
Number of textbooks per student							0.0641	0.0630
Dummy (full time principal = 1)							-3.016***	1.7195
Number of training courses for teachers							-0.0034	0.0177
Constant	10.8009	0.6542	7.8100***	4.6316	2.555	6.9345	-2.813	7.7685
Number of cases	412		412		412		412	
R squared	0.1325		0.1679		0.3293		0.3533	

Note: *Significant at 1%, ** Significant at 5% *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Table 3.10
OLS estimates of program impact on test scores: equation 3 and 4 fourth grade (bilingual system)

	Mathematics, fourth grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-3.012*	0.7845755	-3.064*	0.7334	-4.338*	1.2221	-5.385*	1.6532
Age			-0.3729**	0.1979	-0.3296	0.2578	-0.228	0.2833
Age squared			0.0081**	0.0037	0.0066	0.0051	0.0043	0.0055
Dummy (1=female)			-0.3336	0.4253	-0.2809	0.4261	-0.0262	0.4209
Selben index			0.0350	0.0279	0.038	0.0265	0.027	0.0265
Schooling of the head of hh.			-0.065	0.0479	-0.0742	0.0507	-0.0787	0.0504
Head of hh is indigenous			-1.260***	0.7266	-1.004	0.6441	-1.012	0.6418
Head of hh is female			0.684	0.6155	0.6494	0.5897	0.700	0.5868
Hh members aged <6			-0.1253	0.2865	-0.098	0.2950	-0.135	0.2913
Hh members aged 6–17			0.291***	0.1551	0.1819	0.1403	0.184	0.1359
Hh members aged 18–44			0.511**	0.2402	0.4929**	0.2319	0.388***	0.2281
Hh members aged 45–65			0.320	0.2700	0.2690	0.2699	0.071	0.2526
Hh members aged >65			0.1850	0.4871	0.1147	0.4248	-0.0860	0.4488
Poverty incidence (parrochial level, 1990)					0.1297	0.1165	0.207***	0.1225
Illiteracy rate (parrochial level, 1990)					0.055	0.0969	0.053	0.1031
Student per teacher (parrochial level, 1994)					-0.2156	0.1622	-0.2932	0.2006
Student per school building (parrochial level)					0.0077	0.0189	0.0139	0.0216
Student per classroom (parrochial level, 1994)					-0.200**	0.0976	-0.282***	0.1615
Repetition rate (school level, 1994)					-10.898	23.5797	-24.13	35.8086
Student per teacher (school level, 1994)					0.077	0.0598	0.123**	0.0603
School infrastructure index							-0.233	0.4906
Number of learnig guides per student							3.196	2.6006
Number of textbooks per student							0.096	0.0746
Dummy (full time principal = 1)							-5.566**	2.3035
Number of training courses for teachers							0.0083	0.0524
Constant	9.425*	0.5286598	11.056	2.158252	3.7867	8.8415	-0.658	8.1155
Number of cases	348		348		348		348	
R squared	0.1584		0.2059		0.2487		0.3002	

Language, fourth grade

	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-2.68*	0.7699793	-2.222*	0.7727	-2.657	1.6423	-3.023	2.1799
Age			-0.0166	0.2571	0.1301	0.2927	0.084	0.2905
Age squared			0.0038	0.0045	0.0005	0.0053	0.0008	0.0053
Dummy (1=female)			-0.3735	0.4481	-0.4675	0.4588	-0.428	0.4359
Selben index			0.076**	0.0335	0.059***	0.0316	0.0469	0.0302
Schooling of the head of hh.			0.0215	0.0451	0.0321	0.0413	0.022	0.0392
Head of hh is indigenous			0.4207	0.5187	0.4604	0.5754	0.3688	0.5461
Head of hh is female			0.4261	0.4873	0.4842	0.4658	0.4581	0.4383
Hh members aged <6			-0.0836	0.2407	-0.086	0.2250	-0.1463	0.2068
Hh members aged 6–17			0.111	0.1512	-0.0047	0.1247	-0.015	0.1262
Hh members aged 18–44			0.235	0.2233	0.2149	0.2127	0.1638	0.2111
Hh members aged 45–65			0.295	0.2588	0.1958	0.2526	0.0934	0.2523
Hh members aged >65			0.3725	0.5997	0.195	0.6368	0.0524	0.5960
Poverty incidence (parrochial level, 1990)					0.127	0.1151	0.183	0.1299
Illiteracy rate (parrochial level, 1990)					-0.078	0.1175	-0.0998	0.1278
Student per teacher (parrochial level, 1994)					-0.0938	0.1948	-0.116	0.2379
Student per school building (parrochial level)					-0.0062	0.0243	-0.0103	0.0273
Student per classroom (parrochial level, 1994)					0.0636	0.1384	0.097	0.1952
Repetition rate (school level, 1994)					23.08	26.9811	33.41	37.8907
Student per teacher (school level, 1994)					0.061	0.0459	0.091**	0.0416
School infrastructure index							0.1975	0.4238
Number of learnig guides per student							5.442**	2.6102
Number of textbooks per student							0.0465	0.0635
Dummy (full time principal = 1)							-0.5618	1.9792
Number of training courses for teachers							-0.1187**	0.0499
Constant	9.80*	0.4662342	5.569***	3.2487	-3.728	8.3031	-7.485	7.8747
Number of cases	348		348		348		348	
R squared	0.1202		0.1703		0.2133		0.2775	

Note: *Significant at 1%, ** Significant at 5% *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Table 3.11
Probit model to estimate the equation for program participation

Dependent variables: T (1=Redes Amigas)	Hispanic				Indigenous			
	Second grade		Fourth grade		Second grade		Fourth grade	
	Coeficient	*Std. Error*	*Coeficient*	*Std. Error*	*Coeficient*	*Std. Error*	*Coeficient*	*Std. Error*
Age	-0.1956	0.4465	-0.9869***	0.5352	-1.340*	0.4191	-0.0732	0.1422
Age squared	0.020	0.0248	0.0516	0.0244	0.0577*	0.0188	0.0002	0.0026
Dummy (1=female)	0.0502	0.0873	0.226**	0.0890	0.0976	0.1053	0.1127	0.1431
Selben index	0.0680*	0.0103	0.0652*	0.0120	-0.0821*	0.0152	-0.0825*	0.0169
Schooling of the head of hh.	0.0076	0.0227	-0.0232	0.0234	-0.0606**	0.0251	0.0421	0.0359
Head of hh is indigenous	-0.2405	0.2613	-0.4146	0.3619	-0.4621	0.5499	0.6412**	0.3031
Head of hh is female	0.2926**	0.1368	0.21401	0.1811	0.1209	0.2593	-0.527**	0.2803
Hh members aged <6	0.2006*	0.0579	0.0991	0.0694	-0.0755	0.0759	0.0957	0.0856
Hh members aged 6–17	-0.0358	0.0305	0.0030	0.0565	-0.0802	0.0570	-0.0806	0.0549
Hh members aged 18–44	-0.0183	0.0597	0.0054	0.0374	0.0959	0.0881	-0.1219***	0.0739
Hh members aged 45–65	0.1456***	0.0821	-0.0924	0.1012	-0.0851	0.0823	-0.1363	0.1455
Hh members aged >65	0.2065	0.1561	0.1225	0.1721	0.1592	0.1194	0.3167	0.2028
Dummy for region (Costa=1)	1.4335*	0.4768	1.1693**	0.5360				
Poverty incidence (parrochial level, 1990)	-0.0146	0.0188	-0.0023	0.0190	0.076	0.0731	0.0933	0.1202
Illiteracy rate (parrochial level, 1990)	0.0229	0.0327	0.0188	0.0322	0.2448***	0.1489	0.5419*	0.1160
Student per teacher (parrochial level, 1994)	-0.2507*	0.0779	-0.2592*	0.0760	-0.7254***	0.4475	-1.049*	0.3207
Student per school building (parrochial level)	-0.0086	0.0072	-0.0100	0.0071	-0.00007	0.0188	0.035*	0.0103
Student per classroom (parrochial level, 1994)	0.1154	0.0848	0.0918	0.0917	-0.0892	0.1642	-0.8068*	0.2645
Repetition rate (school level, 1994)	0.9100	5.0944	-2.660	5.6295	-12.617	48.5049	-45.390	62.2298
Student per teacher (school level, 1994)	0.0278***	0.0161	0.027***	0.0168	0.0091	0.0219	0.0266	0.0142
Constant	-1.725	2.5100	3.042	3.5565	9.552**	4.2813	8.617***	4.4763
Number of cases	928		870		412		348	
Pseudo R squared	0.4393		0.3871		0.5012		0.7593	

Note: *Significant at 1%, ** Significant at 5% *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

As displayed in Table 3.7, the OLS estimates show a statistically significant impact of the program on second grade mathematics and language for the Hispanic system. The result remains significant through the four different specifications used, showing a positive effect of the intervention through two channels: improving school inputs, and changing the school management structure. However, the effect of school inputs seems weak. In mathematics, none of the school input variables is sig-

nificant; suggesting that the overall impact of the program is mainly due to changes in school management. In language, on the other hand, the dummy for full-time principal has a significant and positive association with test scores.

Table 3.8 displays results for the Hispanic system in fourth grade. In this case, again, one finds significant and positive effects of the program on both mathematics and language. In addition, in both cases the number of learning-guides and the number of textbooks per student, as well as the dummy for full-time principal has a significant and positive association with test scores. Conspicuously, in the bilingual case, the effect of program intervention is significant but negative for both mathematics and language in second grade. The result remains through the four specifications used. However, in terms of school inputs, the number of learning-guides per student, and the number of training courses for teachers are positively associated with test scores in language and math respectively suggesting that improving these items could lead to improvements in students' achievements. Contrary to the Hispanic case, under the bilingual system, having a full-time principal is negatively associated with test scores. This is an unexpected result. It is hypothesized that having a full-time principal can improve school management and lead to improvements in the learning process; however, this does not appear to be happening in bilingual schools. It is likely that improving the management structure leads to additional school inputs, especially textbooks. A potential problem is that textbooks and other inputs used by bilingual schools were developed under the Hispanic system and may not be suited to the needs of students in the bilingual system.[31]

Similar results are found for fourth-grade. See Tables 3.9 and 3.10.

3.6.2 PSM estimates

The next part introduces the results of the PSM estimates. As mentioned earlier, estimates under PSM use several types of matching (nearest neighbor, five nearest neighbors and Kernel), and are restricted to the region of common support. To begin, the results of the selection equation are introduced. As already mentioned, the selection equation is a probit model that analyzes the probability of program participation by incorporating variables related to community and school characteristics previous to program intervention, as well as control variables at the household level, plus geographic controls (a dummy variable for region).

Results for the Hispanic as well as the bilingual case are displayed in Table 11. In the Hispanic case there is a significant and positive association between the score in the Selben index and program participation. Higher values in the Selben index (wealthier households) are associated with a higher probability of participating in the program. Schools pertaining to the *Costa* have higher probability of participating in the program than those of the *Sierra*. The student per teacher ratio, which shows the size of the student population as well as the endowment of teachers at parochial level has a significant and negative relation to program participation. This means that communities with higher levels of concentration of schooling age population have lower probability of participating in the program. Finally, the student per teacher ratio at school level, which reflects the congestion of schools, shows a significant and positive relation to program participation. Schools with higher classroom congestion have more probability of participating in the program. Results are similar for fourth and second grade. Regarding the bilingual system, Table 11 shows a negative relation between program participation and the Selben index, as well as with the schooling of the household head. This means that households with a lower socioeconomic status and a lower schooling level of its head have a higher probability of participating in the program. At the parochial level, the illiteracy rate has a positive association with program participation. It also suggests that communities with higher illiteracy rates have a higher probability of participating in the program. The number of students per teacher at parochial level is negatively associated with program participation. Again, this means that communities with higher levels of concentration of schooling age population have lower probability of participating in the program.

Results of PSM estimates are presented separately for second- and fourth-grade in the Hispanic as well as the bilingual system. See Tables 3.12 and 3.13.

Table 3.12
PSM estimates of program impact in second grade.
Several types of matching. Restricted to the region of common support.

Hispanic National	**One to one matching**		**Five nearest neighbors**		**Kernel matching**	
ATT	*Math*	*Language*	*Math*	*Language*	*Math*	*Language*
Treated	11.8308	12.8589	11.8308	12.8589	11.8308	12.8589
Controls	7.6200	9.6804	6.9632	9.7203	6.9633	9.7804
Difference	4.2108*	3.1784*	4.8676*	3.1385*	4.8675*	3.0784*
Standard error	0.5182	0.6443	0.4942	0.5803	0.4674	0.5763
Cases on common support	914	917	914	917	914	917
Bilingual	**One to one matching**		**Five nearest neighbors**		**Kernel matching**	
ATT	*Math*	*Language*	*Math*	*Language*	*Math*	*Language*
Treated	4.6666	7.5097	4.6666	7.5208	4.6660	7.5208
Controls	8.0625	10.9029	7.8541	10.2625	7.9895	10.3461
Difference	-3.3958*	-3.3932*	-3.1875*	-2.7416*	-3.3228*	-2.8252*
Standard error	0.8557	0.8149	0.7185	0.8940	0.9692	0.7266
Cases on common support	302	412	302	412	302	412

Note: *Significant at 1%. ** Significant at 5%. *** Significant at 10%. Standard errors in parenthesis and computed by bootstrapping.

In the Hispanic case, a positive and significant impact of the program is found in both mathematics and language for second and fourth grade. Results are robust through the different types of matching used. The program has an impact of around 5 points, and 3 points (out of 20) in second grade for math and language respectively. In fourth grade the impact is around 2 points for math and 1 point for language. In the bilingual system (of *Sierra*) for second grade, the program has a negative impact on both mathematics and language. The result is robust through the different types of matching. In fourth-grade the effect of the program is also negative for mathematics, and no significant results are found for language. Appendix A introduces the results for the OLS estimates of equation (3) and (4) for the Hispanic and bilingual system respectively, but restricting the sample only to the region of common sup-

port. Results are the same as those found under the OLS estimates for the complete sample.

Table 3.13
PSM estimates of program impact in fourth grade.
Several types of matching. Restricted to the region of common support.

Hispanic National	**One to one matching**		**Five nearest neighbors**		**Kernel matching**	
ATT	*Math*	*Language*	*Math*	*Language*	*Math*	*Language*
Treated	9.6910	13.7513	9.6910	13.7513	9.6910	13.7513
Controls	7.9109	13.1675	7.4209	12.8157	7.3822	12.8959
Difference	1.7801*	0.5837	2.2701*	0.9356***	2.3088*	0.8553**
Standard error	0.4953	0.4856	0.4751	0.4766	0.3655	0.3404
Cases on common support	830	830	830	830	830	830
Bilingual	**One to one matching**		**Five nearest neighbors**		**Kernel matching**	
ATT	*Math*	*Language*	*Math*	*Language*	*Math*	*Language*
Treated	6.0780	7.1257	6.0780	7.0992	6.0158	7.0158
Controls	9.1418	5.0239	8.1546	5.4141	8.8898	5.4573
Difference	-3.0638**	2.1017***	-2.0765***	1.6851	-2.8740**	1.5585
Standard error	1.2961	1.3337	1.1024	1.0660	1.123	1.2456
Cases on common support	322	348	322	348	307	307

Note: *Significant at 1%. ** Significant at 5%. *** Significant at 10%. Standard errors in parenthesis and computed by bootstrapping.

Appendix C introduces the results for the bias corrected matching developed by Abadie and Imbens (2002). The advantage of this technique is that it combines the bias reduction from the matching –produced by comparing units with similar values of the covariates-, with the bias reduction from the regression. In addition, the technique uses matching with replacement, which allows one to improve the quality of matching (Abadie and Imbens, 2002). Results are similar to those of the normal matching showing a positive and significant impact of the program in mathematics and language in the Hispanic system. Results for

the bilingual system become positive or insignificant. Results for the weighting match in the Hispanic and the bilingual case are introduced in Tables 3.14 and 3.15 respectively.

Table 3.14
Weighted OLS estimates of program impact (equations 7 and 7.1)
Hispanic system

	Hispanic							
	Second grade				Fourth grade			
	Specif.1	*Specif.2*	*Specif.3*	*Specif.4*	*Specif.1*	*Specif.2*	*Specif.3*	*Specif.4*
Mathematics								
T	4.8441*	5.2614*	4.7367*	3.7437*	2.1604*	2.3458*	2.3537*	2.2859*
	(0.9463)	(0.9424)	(0.6856)	(0.6917)	(0.5353)	(0.4617)	(0.4593)	(0.4319)
R squared	0.2235	0.2800	0.3647	0.3969	0.0779	0.1380	0.1662	0.2127
Language								
T	3.3962*	3.1687*	2.7314*	2.2281*	0.8637*	0.9014	1.1403**	1.0915*
	(0.8004)	(0.7299)	(0.6960)	(0.8015)	(0.9861)	(0.7592)	(0.4456)	(0.3985)
R squared	0.1350	0.2226	0.2546	0.2904	0.0107	0.1595	0.2798	0.3338
Number of cases	927	927	927	927	870	870	870	870

Note: *Significant at 1%, ** Significant at 5%. *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Table 3.15
Weighted OLS estimates of program impact (equations 7 and 7.1)
Bilingual system

	Bilingual							
	Second grade				Fourth grade			
	Specif.1	*Specif.2*	*Specif.3*	*Specif.4*	*Specif.1*	*Specif.2*	*Specif.3*	*Specif.4*
Mathematics								
T	-2.2885**	-2.0798***	-3.6080*	-3.7292*	-1.1992	-1.3756***	-1.8852**	-2.0751**
	(1.1024)	(1.1987)	(0.9407)	(0.8434)	(0.7470)	(0.7768)	(0.7745)	(0.8884)
R squared	0.0527	0.1160	0.3468	0.4359	0.0508	0.2909	0.4170	0.5017
Language								
T	-3.5314*	-3.3192*	-3.8099*	-4.3181*	2.5177**	1.860***	1.2420	1.1772
	(1.1474)	(1.1426)	(1.1364)	(1.1228)	(0.9220)	(1.0751)	(0.8145)	(0.7725)
R squared	0.1352	0.1609	0.3556	0.4064	0.1177	0.3823	0.5769	0.6498
Number of cases	412	412	412	412	348	348	348	348

Note: *Significant at 1%, ** Significant at 5%. *** Significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Results are similar to those under the simple matching. Under the weighting match, the same four specifications as in the simple OLS are used, and results show a significant and positive impact of the program on test score for both language and mathematics in the Hispanic case. In second grade the effect of the program on mathematics is around 4.7 points under specification (3), while under specification (4) it is only around 3.7 points. In the case of language the effect to the program is around 2.7 points under specification (3) and 2.2 points under specification (4). In fourth grade, the effect of the program on math is around 2.4 points under specification (3) and 2.3 under specification (4); while the effect of the program on language is around 1.1 points under specification (3) and 1 point under specification (4). These results suggest that the effect of the program works through two channels of intervention: improving school infrastructure (around 80% for math in second grade) as well as through changes in school management (around 20% of the effect for math in second grade). In the bilingual case the effect of the program is significant and negative for both mathematics and language in second grade. For fourth grade the program has a negative effect on math, but no significant effect on language.

An explanation for the negative result in the bilingual system could be that, as already mentioned, in the bilingual case, the Ministry of Education explicitly promoted the participation of the poorest schools and despite the use of PSM and restricting comparisons to applicants and participants the evaluation design may not have successfully eliminated pre-program differences between participants and non-participants. One additional explanation, as already mentioned, refers to the hypothesis of culturally inadequate curricula (Glewwe et al. 2007). The program provided Indigenous and Hispanic schools with the same school textbooks and other inputs. The indigenous children have a different worldview and cultural values than the children form the Hispanic system. The mechanical transposition of school materials developed for other cultural context could produce negative effects on the learning process.

Figure 3.1
Distribution of propensity scores for treatment and control groups
Hispanic system

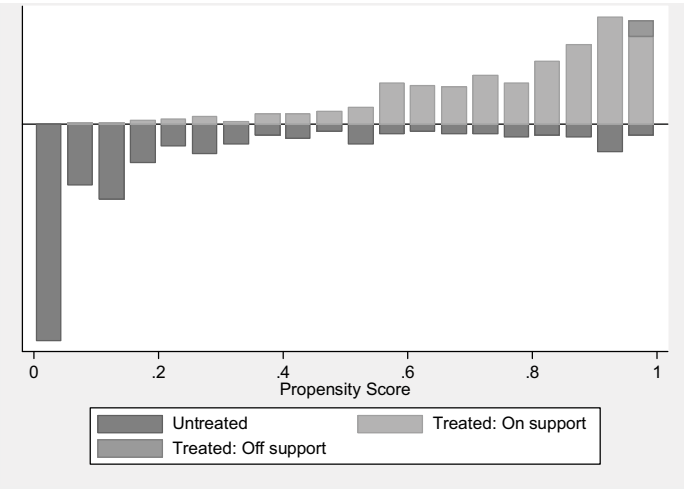
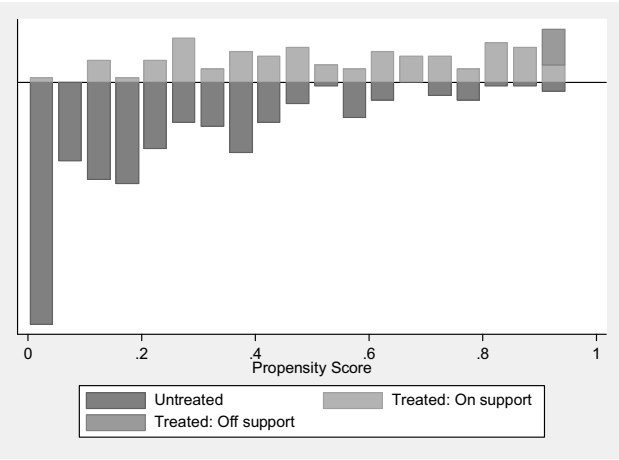


Figure 3.2
Distribution of propensity scores for treatment and control groups
bilingual system



In order to evaluate the quality of the matching, Appendix B introduces the test for balancing variables before and after matching across the treatment and control groups. Results are introduced separately for second grade in the Hispanic and the bilingual system.[32] While post-matching tests support the null hypothesis of equality of means (between treatment and control groups) of most of the variables included in the selection equation, there are some important variables that determine program participation, such as the score in the Selben index, the dummy for region, and the ratio student per classroom that remain significantly different between treatment and control groups. The post-matching differences between the treatment and control groups suggest that given the available data it is difficult to obtain an unbiased estimate of the effect of the decentralization program on test scores. The same conclusion can be obtained when one analyses the regions of common support between treatment and control groups (see Figures 3.1 and 3.2). One interesting point in this part is that according to this evidence the matching for the bilingual case performs better than the matching for the Hispanic schools.

As mentioned in the methodological section, a potential bias of PSM estimates could come from the presence of un-observables in the selection process. Results of estimates of equation (8) to test for un-observables are presented in table 16. As mentioned, the idea of the test is to compute the generalized residuals from the probit model used to estimate program participation, and then incorporate them, as a regressor, in a OLS regression where the dependent variable is the outcome variable of interest (the respective test score). Two specifications were used. Specification 1 includes individual and household characteristics, and specification 2 includes, in addition, school variables that were influenced by the program.[33] In all cases the coefficient of the generalized residuals is statistically insignificant, implying that, possibly, un-observables do not exert an influence on program participation.[34]

Table 3.16
Test for un-observables in the selection process: equation 8

	Second grade				Fourth grade			
	Mathematics		*Language*		*Mathematics*		*Language*	
	Specif.1	*Specif.2*	*Specif.1*	*Specif.2*	*Specif.1*	*Specif.2*	*Specif.1*	*Specif.2*
Hispanic								
Mills ratio	0.0211	-0.3880	0.3344	0.6521	0.3892	0.2755	0.9043	0.8425
	(0.8165)	(0.6828)	(0.7622)	(0.9017)	(0.6603)	(0.6309)	(0.7490)	(0.7223)
T	4.1375*	4.4224*	1.7648	1.3985	1.6744***	1.8377***	-0.3883	-0.3116
	(1.0500)	(0.9411)	(1.2923)	(1.4877)	(0.9898)	(0.9617)	(1.0340)	(1.0516)
R squared	0.1971	0.2169	0.1644	0.1764	0.1073	0.1541	0.2051	0.2517
Number of cases	926	926	927	927	870	870	870	870
Bilingual								
Mills ratio	-2.0465	-1.7047	-0.4850	-1.0037	0.1657	0.1445	1.5901	1.7911
	(1.3707)	(1.3236)	(1.2929)	(1.1940)	(0.9065)	(1.0310)	(1.0430)	(1.1471)
T	0.2004	-0.4048	-2.5044	-1.9021	-3.4828*	-3.6948*	-3.0590*	-3.3154*
	(1.7702)	(1.4765)	(1.5816)	(1.4392)	(0.7848)	(0.7271)	(0.9008)	(0.8206)
R squared	0.1659	0.2537	0.1696	0.2382	0.2269	0.2478	0.194	0.2358
Number of cases	412	412	412	412	326	326	326	326

Note: *Significant at 1%, ** Significant at 5%, and *** significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

3.7 Conclusions

Decentralization of education is an important component of educational policies in Latin America. Most Latin American countries started decentralizing their education systems during the 1990s. Broadly, two types of decentralization reforms are found in the region. First, decentralization based on transferring school administration from the central government to local governments or private associations. Second, decentralization based on transferring school administration to local communities with parents' participation. Despite the importance of decentralization in Latin America, evidence on the impact of these policies on school outcomes is scarce. The existing evidence shows that when decentralization is based on transferring school administration to local governments, its impact on school outcomes depends on the level of development of local

governments. The process appears to be successful when local governments have adequate technical, financial and political capability.

Regarding the second case of decentralization based on transferring school administration to local communities with parents' participation, most of the experiences come from Central America, where the main objective was to improve school enrolment in remote rural areas.

Ecuador, like most Latin America countries, started its decentralization process at the beginning of the 1990s. The model of decentralization applied was based on transferring administrative, budget, and pedagogical responsibilities to schools. One novel point in the Ecuadorian experience of educational decentralization was that the program explicitly intended to improve students' cognitive achievements. The program was expected to improve learning conditions through two channels; by improving school inputs, and by changing the school management structure. In this chapter, an impact evaluation of this program was conducted.

The original design of the program, like most decentralization experiences in Latin America, did not include an impact evaluation design. Consequently, it was not possible to carry out an experimental study, or to have a baseline survey. Administrators of the program decided to evaluate its impacts once it had almost finished. Fortunately, a group of schools that decided to participate in the program, but had not received the treatment was available to create a comparison group. In this regard, by restricting the sample to program applicants (pipeline design) this paper attempted to control for un-observables in the selection process for program participation. In addition, the paper tried to control for observables by using a propensity score matching. Despite methodological efforts to construct an appropriate control group, post-matching statistical tests suggested that the treatment and control group still differ, raising the possibility that the positive effect of decentralization in Hispanic schools and the negative effect in bilingual schools may well be attributed to positive and negative selection, respectively. Overall, given the currently available data it would be premature to draw conclusions about the effect of the decentralization program on test scores. However, decentralization has increased parental and community participation in the educational process. Therefore, decentralization could play an important role in improving school monitoring through parents and the community. A greater parental role may bring parents closer to the educational

process and increase the acceptability and value of educational investments.

Notes

[1] Efficiency can de defined in two different ways; technical as well as social efficiency. Technical efficiency refers to produce a higher output for similar costs or the same output for lower costs. Social efficiency refers to choices that reflect more closely consumers' preferences. (Di Gropello, 2006).

[2] This argument comes from the principal-agent literature. See Di Gropello (2006) for a review.

[3] Di Gropello (2006) finds three decentralization models of education in Latin America: The "Sub-national government model" applied in Argentina, Mexico, Chile and Brazil, where the education service delivery was transferred to the municipal level. The "Sub-national shared responsibility model", applied in Colombia and Bolivia, where the main responsibilities in education were transferred to the departmental and municipal level. Finally, the "School autonomization model", applied in Nicaragua, Honduras, El Salvador, and Guatemala, where school administration was transferred to local communities.

[4] See Espínola V. (1997), Gershberg A. (1999), and Winkler and Gershberg (2000) for a review.

[5] Although the effect of the program is not robust, it is sensitive to the specification of the participation equation. The main conclusion of the paper is that the program has not lessened child learning (Jimenez and Sawada, 1999).

[6] Taken from a total of around 400 public secondary schools.

[7] In addition to El Salvador, the cases of Guatemala, and Honduras also defined as the main objective of decentralization to improve school enrollment in remote rural areas.

[8] *De facto* decentralization was computed by using an index that measures the level of participation of parents on key school decisions. It does not refer to program participation, which is named *de jure* decentralization by the authors. See, King and Özler 2000 for more details.

[9] See Emanuela Di Gropello (2006) for a review of those programs.

[10] See Guedes, at, al. (1997) for a review of the experience of Minas Gerais.

[11] Ecuador has three geographic regions (*Costa, Sierra and Amazonía),* and two different education systems, the Hispanic system, where the official language is Spanish and most students are *mestizos*, and the indigenous system where Spanish and Quichua are taught, and most students are indigenous.

[12] An evaluation study of the program, using qualitative data, show that teachers and communities of schools in the program were more enthusiastic and more involved in the education process (Flacso, 2005). I was the head of the team in charge of this study.

[13] For a review of the education production function approach, see Bowles, 1970; Hanushek, 1979; Behrman, 1999; Pritchett and Filmer, 1997; Todd and Wolpin, 2003.

[14] Quasi-networks are composed of schools that have successfully applied for the program, but, because of some administrative issues, have not yet received it.

[15] The Ecuadorian Ministry of Education takes schools and teachers information every year. Data have been available in electronic means since 1994.

[16] Those variables could not be used as instruments under an instrumental variable approach. Although they are highly correlated with program participation, they do not satisfy the "exclusion restriction" because they are also correlated with test scores.

[17] Examples of studies using a pipeline comparison design are: Angrist, 1998; Chase, 2002; and, Galasso and Ravallion, 2004.

[18] Most of the quasi-networks did not receive treatment due to administrative reasons. For example, an incorrectly filled admission form, or the composition of the pedagogical committee was not adequate.

[19] The region of common support refers to individuals with similar characteristics regarding the variables that influence program participation.

[20] Like all experiences of decentralization in Latin America, the initial design of the program did not incorporate an impact evaluation part. In this sense, an experimental design was not possible. The evaluation study only was contracted at the end of the program as a requirement of the IDB. Fortunately, the availability of quasi-networks was an opportunity to use a pipeline comparison design.

[21] Using this weight one obtains the average treatment on the treated. If one wants to get the average treatment effect for the population, the weights are $1/(\hat{P}(X)$ for treated units, and $1/(1-(\hat{P}(X))$ for the controls (See, Hirano, Imbens and Ridder 2003 for details).

[22] For a review of models with sample selection bias see Vella (1998).

[23] This term is the inverse Mills ratio for the entire sample. See Vella (1998) for a review.

[24] Jalan and Ravallion (1999) use this test.

[25] I was the head of the team in charge of designing the survey as well as taking the data.

[26] Tests were different in order to capture cultural differences between children from Hispanic and bilingual schools. Tests were probed and validated with the technical support of the Ministry of Education.

[27] The Selben index is a multivariate index computed using nonlinear principal components, and it is a combination of 20 variables representing basically household assets, and household socio-demographic characteristics. The index is scaled from 0 to 100 and is used to target social programs in Ecuador. Values close to 0 stand for extremely poor, while values close to 100 stand for wealthy.

[28] The school infrastructure index is scaled from 0 to 5, and was computed using indicator variables that take the value of 1 if the characteristic is present. The following characteristics were taken into account: teacher housing, potable water, electricity, bathrooms, and space for children to play.

[29] Some child variables such as time dedicated to work, time watching TV, and whether parents help to children with homework, were not included because of endogeneity concerns. However, results are not different when those variables are included.

[30] It was not possible to incorporate current school and community fixed effects because of the sample design. As mentioned, the sampling unit was the network and inside the network information was taken for all the schools pertaining to the network. Networks superpose to communities, so it is impossible to find in one community schools for treatment and control group. In the same way, it is impossible to find in one school students from treatment and control group.

[31] Glewwe et al. (2007) using an experimental design found no significant effect of a textbooks program in Kenya. One of the reasons for the null impact is that the textbooks were written in English and the poorer students could not use them. According to the authors, this is an example of culturally inadequate curricula.

[32] Results are similar for fourth grade and are available under request.

[33] Individual and household variables, as well as school characteristics are the same introduced under the OLS estimates of equations (3) and (4).

[34] As already mentioned, to facilitate the identification of equation 8, I included community level variables previous to program intervention in the selection equation, and excluded them from the output equation. Although these variables are not good instruments (because they also affect test scores); the fact that those variables are lagged several years can be helpful. In this case the endogeneity affects only the small sample properties of the OLS estimates but not its asymptotic distribution (Verbeek, 2000).

4 Concluding remarks and policy reflections

This thesis analyzed educational policy in Latin America during the 1990s. In this period, these policies prioritized the following issues: a) decentralization of education, b) improvements of education in terms of equity and quality, c) teachers' incentives, d) the creation of systems of evaluation of students' achievements, and e) demand-side interventions. The thesis focuses on the impact of these reforms on education outcomes especially on demand-side interventions and on decentralization of education.

Demand-side interventions took on two forms in the region. Conditional cash transfers programs, and school vouchers. Mexico, Brazil, Nicaragua, Honduras, Costa Rica, Ecuador, Colombia and Chile are among the countries that have CCT programs in the region. The majority of studies that evaluate the impact of these programs find significant and positive effects on school enrolment and on reducing child labor. However, it is not clear whether the impact of these programs comes from the transfer and/or from the condition. The Ecuadorian experience can be very illustrative in this debate. Because the *Bono de Desarrollo Humano* is not a conditional cash transfer program but an unconditional cash transfer program, it provides fresh evidence on the importance of conditionality. By using a regression discontinuity design combined with a difference-in-difference approach, the thesis finds statistically insignificant effects of the program on school enrolment among those around the 40th percentile in the poverty index. The lack of an enrolment effect may be attributed to the lack of conditionality. However, the chapter does find significant differences in consumption and education spending between beneficiaries and non-beneficiaries. Beneficiaries spend more on education than non-beneficiaries, but this difference is not expressed in enrolment rates probably due to the lack of conditionality.

If the increase in education spending is not reflected in enrolment, it may be possible to find positive impacts on students' cognitive achievements. In this regard, Chapter 2 evaluates the effect of the BDH on test scores. Across the region there is scarce evidence on the impact of CCT programs on students' cognitive achievements. Except for the Mexican experience, no other CCT program has been evaluated to analyze its impact on learning. On the other hand, the empirical evidence on the impact of school vouchers on test scores is not conclusive. The Chilean experience shows no significant impacts; meanwhile the Colombian experience shows significant and positive impacts. As a contribution this thesis evaluates the impact of an unconditional cash transfer program on test scores in Ecuador. By using a regression discontinuity design the thesis finds no significant effects of the program on test scores among those around the 40^{th} percentile in the poverty index.

From these two chapters one can suggest some policy recommendations for the Ecuadorian program. First, it is important to transit from a cash transfer to a conditional cash transfer program. As was shown in Chapter 1, the available literature on this topic shows that most of the impact of CCT programs is produced by the conditionality. In this regard it is necessary to create adequate conditions to enforce conditionality both in terms of school enrolment and attendance. Second, like the majority of CCT programs in Latin America, it may be more efficient to have a different amount of transfers depending on the poverty level (a bigger amount for those in quintile 1 according to the Selben index), the age of the children (smaller amount for the youngest), and ethnicity (a bigger amount for indigenous and afro-Ecuadorians). Third, once the conditionality has been implemented, an increase in school enrolment and attendance would be expected. Therefore, it seems important to combine the conditional cash transfer with some supply-side interventions in order to avoid possible school congestion. Finally, it is imperative to highlight the significance of thinking about impact evaluation at the beginning of the program intervention. In this sense, the experience of the Bono de Desarrollo Humano and its impact evaluation design represents an example of good practice in social policy.

In methodological terms these two chapters highlight the importance of having good data to draw robust conclusions about the effects of an educational program. Methodologically speaking, the novelty of this research was to combine different quasi-experimental designs in order to

ensure robust estimates. Chapter 1 combined a difference-in-difference with a regression discontinuity design, while Chapter 2 combined a regression discontinuity design with a propensity score matching.

Regarding decentralization, two models of education decentralization are found across the region. First, a model based on transferring competencies to local governments. Chile, Argentina, and Brazil are among the typical examples. The effect of these programs on education quality varies across countries and local governments. The main conclusion that one can extract from these experiences is that the impact of transferring administrative and/or pedagogical competencies to local governments depends on the level of technical, administrative and financial development of local governments. In this regard, one finds in the literature that decentralization works quite well when local governments have higher levels of technical, administrative and financial resources. Second, there is a model based on transferring competencies to schools. The most representative cases of this type of decentralization come from Central America. Nicaragua, El Salvador, Guatemala and Honduras where they have implemented school-based management programs. In these cases one finds positive and significant impacts of these experiences on school enrolment in remote rural areas. This thesis analyzes the impact of education decentralization on students' cognitive achievements in rural Ecuador. The Ecuadorian experience is a school-based management type. The novel point of this study is that improving learning was included as an explicit objective of the program. The thesis found some evidence of positive effects of decentralization on test scores in the Hispanic system. However, due to data limitations, the result is not robust. On the other hand, the thesis finds significant and negative effects of the program on the bilingual schools, which may be due to deficiencies in program execution and/or culturally inadequate curricula.

From this chapter some policy recommendations can be suggested. First, as already mentioned, it is important that social programs consider evaluation design at the beginning of program implementation, rather than at the end. In the specific case of *Redes Amigas*, due to the lack of a baseline survey it was not possible to draw robust conclusions about the impact of the program on test scores. Research was based on a single cross-section of data and, in methodological terms, the thesis combined a pipeline design with propensity score matching. Despite attempts at creating an appropriate control group, it is likely that this was not suc-

cessful. The lack of a baseline survey represents an example of bad practice in terms of evaluation of social policies. Second, decentralization of education in Ecuador increased parental and community participation. In fact, to participate in the program an important level of participation of parents and communities was required. By bringing parents closer to schools the program improves the control mechanisms of parents on teachers and principals, and increases parents' valuation of education. This could have important effects on school enrolment and attendance. Third, there is some evidence of the positive effects of the program on test scores in the Hispanic schools, but a negative effect on the bilingual schools. One hypothetical explanation for the negative effect in the bilingual case is that indigenous students have a different worldview and different cultural values than *mestizos*. One of the limitations of the program is that school inputs, especially textbooks, were developed for the Hispanic system and mechanically applied to the bilingual schools. Hence, it seems important to have specific school inputs for every type of school. Having generalized school inputs and the same curricular structure for the Hispanic and the bilingual schools can create a problem of culturally inadequate curricula.

Finally, Ecuador has low levels of educational expenditure as compared to other Latin America countries. While it is important to increase the level of education spending in the country it is also important to prioritize investments. The country still has many needs, including investments in school infrastructure, school inputs for the poor such as uniforms, textbooks and other school materials, teacher-training, implementing a system of teacher incentives, continuation of the application of standardized tests to evaluate the quality of learning and to strength the system of evaluation of students' cognitive achievements.

To prioritize such investments, the country needs more research in order to better understand what the main determinants are for both school enrolment and students' cognitive achievements. In this regard, impact evaluation studies or cost-benefit analysis of the main educational interventions is still needed. While this thesis evaluated two important educational initiatives, the research agenda is ample and still incomplete.

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Appendices

Chapter 2

Appendix A

Variables, categories and weights to construct the Selben index

		Using 20 variables	Using 27 variables
		Weights	Weights
1	**Geographic area**		
	Rural disperse-Country	0.0000000	0.0000000
	Rural block- Coast	1.7868746	1.8280949
	Rural block- Highlands	1.8031189	1.8280949
	Urban-Coast	3.1513970	3.2713278
	Urban-Highlands	3.9311241	3.8165491
2	**Floor**		
	Others	0.0000000	0.0000000
	Soil	0.0000000	0.0000000
	Cane	0.0000000	0.0000000
	Plank	1.6406758	1.5073765
	Cement	2.7777778	2.6138550
	Tile	5.0519818	4.9550994
	Parquet	5.0519818	4.9550994
3	**Electricity**		
	None	0.0000000	0.0000000
	Candle	0.4385965	0.4490058
	Private power plant	2.9564652	2.9345734
	Public company	6.2215724	6.1577935
4	**Shower availability**	n.a	
	None		0.0000000
	Shared		1.3113652
	Excusive		2.3537323
5	**Toilets**		
	None	0.0000000	0.0000000
	Latrine	1.0883691	0.9300834
	Toilet and blind well	1.4294997	1.2828736
	Toilet and septic well	2.4691358	2.3091725
	Toilet and sewage systems	4.2560104	4.1372675

		Using 20 variables	Using 27 variables
		Weights	Weights
6	**Type of cooking fuel**		
	Others	0.0000000	0.0000000
	Firewood	0.0000000	0.0000000
	Electricity	3.8661468	3.8325850
	Gas	3.8661468	3.8325850
7	**Land availability**	n.a.	
	No		0.0000000
	Rented		0.7509527
	Own		2.4321901
8	**Persons per bedroom**		
	More than 4 persons	0.0000000	0.0000000
	Between 3 and 4 persons	1.2345679	1.1866581
	Up to 2 persons	3.6874594	3.6241180
9	**Number of children aged under six living at home**		
	Four or more children	0.0000000	0.0000000
	2-3 children	2.5666017	3.1270045
	One child	3.7037037	4.2655548
	Don't have any children	6.3515270	6.8473380
10	**Members in working age that don't receive an income**		
	10 or more	0.0000000	0.0000000
	7-9 members	0.2111761	0.6093650
	5-6 members	0.9096816	1.3470173
	3-4 members	1.9818064	2.4695318
	1-2 members	3.9961014	4.4098781
	All members receive incomes	6.0103964	6.3822963
11	**Head of the home spoken language**		
	Indigenous language	0.0000000	0.0000000
	Only Spanish	0.2111761	2.4855677
	Other languages	3.2488629	3.2392559
	Spanish and other languages	5.0032489	4.9711353
12	**Head of the home education level**		
	None	0.0000000	0.0000000
	Alphabetization Center	0.5360624	0.5131495
	Basic education- adults	1.3482781	1.3149455
	Elementary School	1.7868746	1.6998076
	High school	3.8174139	3.7203335
	Superior-not university	5.2144250	5.0673509
	Superior-university	5.4743340	5.3720334
	Postgraduate	6.5951917	6.4945478
13	**Spouse education level**		
	None	0.0000000	0.0000000
	Alphabetization Center	0.6172840	0.5291854
	Basic education- adults	0.1461988	0.2245029
	Elementary School	1.8518519	1.7639513
	High school	4.1260559	4.0089801
	Superior-not university	5.1332034	5.0513149
	Superior-university	5.6042885	5.5484285
	Postgraduate	6.7089019	6.8152662
	Doesn't have a spouse	1.7706303	1.9082745

		Using 20 variables	Using 27 variables
		Weights	Weights
14	**Is the head of the home affiliated to any insurance**		
	Not affiliated	0.0000000	0.0000000
	Affiliated	3.4275504	3.3996151
15	**Has the household some credit**	n.a.	
	No		0.0000000
	Yes		2.5891056
16	**Kitchen or kitchenette availability**		
	No	0.0000000	0.0000000
	One	5.1494477	5.0513149
	2 o more	6.4814815	6.4304041
17	**Color TV availability**		
	No	0.0000000	0.0000000
	One	2.5990903	2.5176395
	2 o more	4.4834308	4.4579859
18	**Refrigerator availability**		
	No	0.0000000	0.0000000
	One	3.1676413	3.1270045
	2 o more	4.1260559	4.1051956
19	**Telephone**	n.a.	
	No		0.0000000
	One		2.5218561
	2 or more		3.5081820
20	**Car availability**		
	No	0.0000000	0.0000000
	One	4.3372320	4.2655548
	2 o more	5.7179987	5.6606799
21	**Stereo availability**		
	No	0.0000000	0.0000000
	One	3.0214425	2.9826812
	2 o more	5.1332034	5.0673509
22	**VHS availability**		
	No	0.0000000	0.0000000
	One	4.0935673	4.0410520
	2 o more	6.0103964	5.9493265
23	**Children aged between 6-15 years who don't go to school**		
	At least one doesn't go	0.0000000	0.0000000
	All go	0.3573749	0.4008980
	There are no children at home	3.5412606	3.5439384
24	**Type of school children attended**		
	They don't go to school	0.0000000	0.0000000
	All go to a public school	0.0000000	0.0000000
	At least one goes to a public school	0.0000000	0.0000000
	All go to a private school	0.6335283	0.7055805
	There are no children at home	3.3950617	3.3515074

		Using 20 variables	Using 27 variables
		Weights	**Weights**
25	**Number of children that have died**	n.a.	
	4 or more		0.1120825
	Three		0.0000000
	Two		0.6724950
	One		1.9614436
	All are alive		4.8195472
	No children at home		7.0275723
26	**Is the last child still alive**	n.a.	
	No		0.0000000
	Yes		5.9515804
	No children at home		10.6926698
27	**Number of disabled persons at home**	n.a.	
	Two or more		0.0000000
	One		0.7509527
	None		1.5019054

Appendix B

OLS estimates of equation 2.
Several RD samples, and polynomial forms of the selection variable (Selben)

QUADRATIC

RD sample from 49.6 to 51.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	0.0798	0.2997	0.4594	0.6160
	(0.8429)	(0.9462)	(0.9984)	(0.9071)
R squared	0.0216	0.0432	0.1862	0.5093
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.9655	-0.4593	-0.4637	-1.0429
	(0.6468)	(0.6299)	(0.7473)	(0.7210)
R squared	0.0232	0.1482	0.2511	0.477

RD sample from 48.6 to 52.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.0326	-0.0729	-0.5089	-0.2427
	(0.6690)	(0.6678)	(0.6694)	(0.6659)
R squared	0.0141	0.0434	0.1834	0.379
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.3918	-0.0032	-0.2395	-0.1050
	(0.5137)	(0.5199)	(0.5321)	(0.4898)
R squared	0.0097	0.0779	0.1868	0.3093

RD sample from 47.6 to 53.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.2146	-0.2132	-0.5901	-0.4520
	(0.4699)	(0.4811)	(0.4856)	(0.4656)
R squared	0.0123	0.0488	0.1715	0.3457
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	0.1226	0.1987	-0.1918	-0.1324
	(0.4639)	(0.4699)	(0.4772)	(0.4699)
R squared	0.0044	0.0367	0.1403	0.2613

CUBIC

RD sample from 49.6 to 51.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	0.0748	0.2999	0.4596	0.6158
	(0.8509)	(0.9462)	(0.9984)	(0.9073)
R squared	0.0237	0.0432	0.1863	0.5093
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.9582	-0.4595	-0.4639	-1.0434
	(0.6348)	(0.6299)	(0.7473)	(0.7966)
R squared	0.0286	0.1482	0.2511	0.477

RD sample from 48.6 to 52.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.0540	-0.1061	-0.5340	-0.2419
	(0.6708)	(0.6695)	(0.6748)	(0.6664)
R squared	0.0161	0.0466	0.1843	0.379
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.3714	0.0088	-0.2215	-0.0974
	(0.5157)	(0.5231)	(0.5366)	(0.4941)
R squared	0.0121	0.0784	0.1873	0.3097

RD sample from 47.6 to 53.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.2315	-0.2311	-0.6100	-0.4606
	(0.4626)	(0.4724)	(0.4813)	(0.4558)
R squared	0.0167	0.0535	0.1745	0.3499
Language	Specification 1	Specification 2	Specification 3	Specification 4
T	0.1352	0.2104	-0.1728	-0.1265
	(0.4674)	(0.4734)	(0.4810)	(0.4750)
R squared	0.0087	0.04	0.1444	0.2638

Note: Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix C

Descriptive statistics for the RD sample from 48.65 to 52.65

	Non bene-ficiaries	Bene-ficiaries	Difference
Child and Household Characteristics			
Mathematics, second grade (out of 20)	8.6366	8.7317	-0.0951
	(0.4304)	(0.4380)	(0.6175)
Language, second grade (out of 20)	10.9854	10.5480	0.4374
	(0.3622)	(0.4019)	(0.5401)
Score in Selben index	50.8564	50.5328	0.3236**
	(0.0968)	(0.1034)	(0.1419)
Dummy sex (1=female)	0.5547	0.5431	0.0116
	(0.0426)	(0.0464)	(0.0630)
Total of hours working on weekdays	5.5659	5.4625	0.1034
	(0.6070)	(0.5395)	(0.8256)
Hours spent at homework (daily)	1.6518	1.6300	0.0218
	(0.0760)	(0.0841)	(0.1132)
Hours that children study with parents (daily)	0.8421	0.6728	0.1693
	(0.0740)	(0.0805)	(0.1093)
Hours that children watch TV (daily)	1.6970	1.6290	0.0680
	(0.1015)	(0.0994)	(0.1433)
Schooling of the head of household	7.3795	6.6030	0.7765
	(0.3036)	(0.2597)	(0.4074)
Head of the household is indigenous	0.1752	0.2500	-0.0748
	(0.0325)	(0.0403)	(0.0513)
Head of the household is illiterate	0.0802	0.0603	0.0199
	(0.0233)	(0.0222)	(0.0325)
Head of the household is female	0.0802	0.0603	0.0199
	(0.0233)	(0.0222)	(0.0325)
Number of persons aged less than 6 in the hh.	0.8905	0.5517	0.3387**
	(0.0846)	(0.0748)	(0.1148)
Number of persons aged form 6 to 17 in the hh.	3.0580	3.1637	-0.1057
	(0.2011)	(0.2037)	(0.2879)
Number of persons aged form 18 to 44 in the hh.	2.0510	1.9910	0.0600
	(0.1037)	(0.1028)	(0.1472)
Number of persons aged form 45 to 64 in the hh.	0.3138	0.4396	-0.1258
	(0.0583)	(0.0716)	(0.0915)
Number of persons aged more than 64 in the hh.	0.0583	0.1034	-0.0451
	(0.0248)	(0.0374)	(0.0438)

School Characteristics			
Percentage of children attending schools with one teacher	0.0291 (0.0144)	0.0172 (0.0121)	0.0119 (0.0192)
Percentage of children attending Hispanic schools	0.8978 (0.0259)	0.8448 (0.0337)	0.0530 (0.0419)
Percentage of children from Quito	0.4087 (0.0421)	0.2844 (0.042)	0.1243 (0.0599)
Percentage of children attending schools with full-time principal	0.3211 (0.04)	0.1896 (0.0365)	0.1315 (0.055)
Number of learning guides per child	0.0221 (0.0059)	0.0394 (0.0182)	-0.0173 (0.0179)
Percentage of children attending schools with computers	0.8248 (0.0325)	0.8189 (0.0359)	0.0059 (0.0484)
Number of books per pupil	2.0030 (0.6585)	1.3716 (0.2591)	0.6314 (0.7545)
Percentage of children attending schools with access to internet	0.1605 (0.0314)	0.1293 (0.0312)	0.0312 (0.0447)
Index of school infrastructure (out of five)	3.9270 (0.0396)	3.8790 (0.0672)	0.0480 (0.0753)
Teacher Characteristics			
Percentage of children with female teacher	0.8029 (0.0341)	0.7500 (0.0403)	0.0529 (0.0524)
Age of teacher (average)	40.5985 (0.9497)	42.3700 (1.018)	-1.7715 (1.394)
Percentage of children with teacher with superior level	0.7664 (0.0362)	0.7241 (0.0416)	0.0423 (0.055)
Percentage of children with teacher hired by the Ministry	0.8394 (0.0314)	0.9224 (0.0249)	-0.0830 (0.0412)
Number of training courses received by teachers (average)	5.6934 (0.3345)	6.8879 (0.7777)	-1.1945 (0.8025)
Number of cases	137	116	

Note: *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix D

Reduced form estimates of equation 3.
Several RD samples, and polynomial forms of the selection variable (Selben)

QUADRATIC

RD sample from 49.6 to 51.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.9753	-0.9420	-1.0219	-1.4515
	(1.3871)	(1.6188)	(1.8595)	(1.4941)
R squared	0.0242	0.0445	0.1867	0.5105
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-1.2175	-1.1543	-1.0592	-0.6518
	(1.3552)	(1.1839)	(1.5004)	(1.7244)
R squared	0.0152	0.1495	0.2518	0.4676
Number of cases	132	132	132	132

RD sample from 48.6 to 52.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	0.2603	0.6322	0.2457	0.0685
	(1.1391)	(1.2338)	(1.2616)	(1.1061)
R squared	0.0143	0.0443	0.1812	0.3786
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.8242	-0.6110	-0.6263	-0.7894
	(0.8954)	(0.8599)	(0.9484)	(0.8627)
R squared	0.0101	0.0791	0.1873	0.311
Number of cases	253	253	253	253

RD sample from 47.6 to 53.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	0.4560	0.6121	0.1986	0.4890
	(0.9459)	(0.9950)	(0.9704)	(0.9071)
R squared	0.0124	0.0493	0.1684	0.3446
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-1.3898	-1.2942	-1.4512	-1.2042
	(0.7370)	(0.7248)	(0.7365)	(0.6687)
R squared	0.0104	0.0415	0.1463	0.2653
Number of cases	387	387	387	387

CUBIC

RD sample from 49.6 to 51.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.9799	-0.9449	-1.0254	-1.4552
	(1.3883)	(1.6197)	(1.8597)	(1.4936)
R squared	0.0243	0.0446	0.1868	0.5105
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-1.2174	-1.1529	-1.0585	-0.6514
	(1.3553)	(1.1837)	(1.5006)	(1.7244)
R squared	0.0152	0.1495	0.2518	0.4676
Number of cases	132	132	132	132

RD sample from 48.6 to 52.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.5506	-0.2283	-0.1818	0.1884
	(1.3417)	(1.6249)	(1.6948)	(1.5354)
R squared	0.0165	0.0466	0.1818	0.3786
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.4561	-0.6089	-0.5691	-0.9518
	(1.4264)	(1.4227)	(1.5482)	(1.5564)
R squared	0.0107	0.0791	0.1873	0.3111
Number of cases	253	253	253	253

RD sample from 47.6 to 53.6

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
Z	-0.6799	-0.4903	-0.8722	-0.6979
	(1.2027)	(1.3001)	(1.3864)	(1.1889)
R squared	0.0169	0.0533	0.1721	0.3487
Language	Specification 1	Specification 2	Specification 3	Specification 4
Z	-1.1150	-1.1178	-1.1963	-1.0491
	(1.0981)	(1.1268)	(1.2540)	(1.1153)
R squared	0.0107	0.0416	0.1466	0.2654
Number of cases	387	387	387	387

Note: Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations. *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix E

Selection equation for determinants of program participation
Probit model

Dep. Var: Bonus	dF/dx	Robust Std. Err.
Assignment rule (1 if score in selben <= 50.65)	0.2311*	0.0303
Age	-0.0047	0.0082
Sex	-0.0112	0.0206
Head of hh is indigenous	0.0741**	0.0315
Head of hh is illiteracy	-0.0086	0.0312
Head of hh is female	0.0148	0.0324
Number of members of hh aged less than 4	-0.0063	0.0075
Number of members of hh aged from 5 to 17	0.0023	0.0044
Number of members of hh aged from 18 to 44	0.0084	0.0075
Number of members of hh aged from 44 to 64	-0.0065	0.0105
Number of members of hh aged more than 64	0.0143	0.0215
Obs. P	0.5667	
Pred. P	0.5721	

Note: *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level. Standard errors corrected by heteroskedasticity.

Appendix F
Test of balancing variables after matching

Variable	Mean sample	% reduct treated	t-test control	%bias	|bias|	t	p>|t|
[3] Assignment rule	Unmatched	0.86376	0.59786	62.8	16.21	0	
	Matched	0.86311	0.88706	-5.7	91	-1.96	0.05
Age	Unmatched	8.7384	8.6175	8.2	2.07	0.038	
	Matched	8.7392	8.8453	-7.2	12.3	-1.86	0.063
Sex (1=female)	Unmatched	0.48093	0.49062	-1.9	-0.49	0.625	
	Matched	0.48186	0.49144	-1.9	1.1	-0.52	0.604
Household head is indigenous	Unmatched	0.56335	0.35478	42.8	10.76	0	
	Matched	0.56194	0.56331	-0.3	99.3	-0.07	0.941
Household head is illiterate	Unmatched	0.17711	0.12601	14.3	3.57	0	
	Matched	0.17659	0.19233	-4.4	69.2	-1.1	0.273
Household head is female	Unmatched	0.12807	0.13226	-1.2	-0.31	0.753	
	Matched	0.12799	0.13689	-2.6	-112.1	-0.71	0.478
Hh members aged <6	Unmatched	1.4128	1.1367	17	4.26	0	
	Matched	1.4127	1.3936	1.2	93.1	0.31	0.753
Hh members aged 6–17	Unmatched	4.3883	3.7721	18.9	4.74	0	
	Matched	4.373	4.2505	3.8	80.1	0.95	0.341
Hh members aged 18–44	Unmatched	2.611	2.3789	13.1	3.26	0.001	
	Matched	2.6051	2.4565	8.4	36	2.13	0.033
Hh members aged 45–64	Unmatched	0.62398	0.53351	8.2	2.07	0.039	
	Matched	0.62423	0.60233	2	75.8	0.54	0.592
Hh members aged >64	Unmatched	0.14237	0.10277	7.9	1.97	0.049	
	Matched	0.13347	0.13689	-0.7	91.4	-0.18	0.857

Appendix G

Cross tabulation between assignment rule and real treatment status

Treatment status	Assignment rule (Selben index)		
	More than 50.65	*Less than 50.65*	*Total*
Non-beneficiaries	450	673	1,123
% in row	40.07	59.93	100
% in col	69.12	34.62	43.28
Beneficiaries	201	1,271	1,472
% in row	13.65	86.35	100
% in col	30.88	65.38	56.72
Total	651	1,944	2,595
% in row	25.09	74.91	100
% in col	100	100	100

Appendix H

OLS estimates of equation (1), including the instrument (T_hat)

Mathematics	**Specification 1**	**Specification 2**	**Specification 3**	**Specification 4**
T_hat	0.6431	0.6285	-0.3321	-0.1738
	(0.4653)	(0.4658)	(0.3891)	(0.3422)
R squared	0.0181	0.0267	0.1019	0.2733
Language	**Specification 1**	**Specification 2**	**Specification 3**	**Specification 4**
T_hat	0.3338	0.3155	-0.7269	-0.5871
	(0.4678)	(0.4620)	(0.3985)	(0.3435)
R squared	0.0524	0.0589	0.1446	0.2443
Number of cases	2589	2589	2589	2589

Note: *Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level. Standard errors corrected by heteroskedasticity.

Chapter 3

Appendix A

Results of OLS estimates of equation (3) and (4). Only for the region of common support.

Hispanic system

Second grade	**Mathematics, second grade**							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	4.3343*	0.6710	4.1548*	0.7098	4.0199*	0.7436	3.6543*	0.7276
Number of cases	914		914		914		914	
R squared	0.1728		0.1906		0.2341		0.2483	
	Language, second grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	3.0287*	0.6847	2.1598*	0.6911	1.9898*	0.6619	2.124*	0.6788
Number of cases	913		913		913		913	
R squared	0.1032		0.1638		0.2015		0.2101	

Fourth grade	**Mathematics, fourth grade**							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	2.2893*	0.5685	2.1570*	0.5351	2.1882*	0.5467	2.2112*	0.5022
Number of cases	830		830		830		830	
R squared	0.0757		0.1036		0.1477		0.1976	
	Language, fourth grade							
	Specification 1		**Specification 2**		**Specification 3**		**Specification 4**	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	1.47***	0.8163	0.7522	0.6891	1.1324**	0.5383	1.1698*	0.4387
Number of cases	830		830		830		830	
R squared	0.0284		0.1906		0.2793		0.3171	

Note: *Significant at 1%, ** Significant at 5%, and *** significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Bilingual system

Second grade	Mathematics, second grade							
	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-3.6779**	1.5274	-2.9772*	1.4790	-3.0324**	1.2042	-3.8848**	1.1672
Number of cases	302		302		302		302	
R squared	0.1214		0.2244		0.3741		0.4401	
	Language, second grade							
	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-3.2801*	1.4927	-2.821***	1.5029	-3.0250**	1.3504	-4.3717*	1.2023
Number of cases	302		302		302		302	
R squared	0.1165		0.1738		0.2926		0.3695	

Fourth grade	Mathematics, fourth grade							
	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-3.3474*	0.8037	-3.4456*	0.7246	-3.1097*	0.9176	-4.4839*	1.0002
Number of cases	322		322		322		322	
R squared	0.1826		0.2278		0.2794		0.3421	
	Language, fourth grade							
	Specification 1		Specification 2		Specification 3		Specification 4	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
T (1 = Redes Amigas)	-2.7073*	0.8611	-2.2436**	0.8761	-0.8977	1.1322	-1.7069	1.2248
Number of cases	322		322		322		322	
R squared	0.1158		0.1734		0.2667		0.3406	

Note: *Significant at 1%, ** Significant at 5%, and *** significant at 10%. Standard errors are in parenthesis and corrected by heteroskedasticity and within-peer correlations.

Appendix B
Test for balancing variables, before and after matching

Hispanic system

Variable	Sample	Mean		%bias	% redu bias	t	p>t
		Treated	Control				
Score in Selben index	Unmatched	46.842	38.813	77.8	11.8	0	
	Matched	46.511	43.153	32.5	58.2	5.08	0
Schooling of the household's head	Unmatched	6.609	5.4138	34.6	5.22	0	
	Matched	6.5324	5.6159	26.5	23.3	4.1	0
Household's head is indigenous	Unmatched	0.10183	0.31264	-53.8	-8.27	0	
	Matched	0.10438	0.09603	2.1	96	0.43	0.667
Household's head is female	Unmatched	0.14868	0.10345	13.6	2.06	0.039	
	Matched	0.14823	0.15031	-0.6	95.4	-0.09	0.928
Hh members aged <6	Unmatched	0.85743	0.90345	-4.9	-0.74	0.461	
	Matched	0.86013	0.93111	-7.5	-54.3	-1.14	0.254
Hh members aged 6-17	Unmatched	2.6568	2.9126	-18.5	-2.81	0.005	
	Matched	2.6649	3.0063	-24.6	-33.4	-3.89	0
Hh members aged 18-44	Unmatched	1.8859	1.9241	-4.1	-0.62	0.532	
	Matched	1.8873	1.7912	10.3	-151.5	1.66	0.097
Hh members aged 45-64	Unmatched	0.39511	0.38391	1.7	0.25	0.8	
	Matched	0.39875	0.41962	-3.1	-86.3	-0.5	0.616
Hh members aged >64	Unmatched	0.10794	0.13793	-7.3	-1.12	0.263	
	Matched	0.10647	0.08977	4.1	44.3	0.68	0.498
Dummy for region (Costa=1)		0.45621	0.18391	61	9.19	0	
		0.45303	0.56159	-24.3	60.1	-3.38	0.001
Poverty incidence	Unmatched	69.491	80.231	-74.3	-11.13	0	
	Matched	69.897	63.432	44.7	39.8	6.53	0
Illiteracy rate (parochial level)	Unmatched	13.808	18.824	-80.7	-12.45	0	
	Matched	13.893	12.478	22.8	71.8	4.44	0
Ratio student per teacher (paroch. level)	Unmatched	12.835	16.648	-82.5	-12.67	0	
	Matched	12.924	12.823	2.2	97.3	0.45	0.65
Ratio student per school building (parochial level	Unmatched	88.529	83.636	12.4	1.89	0.059	
	Matched	88.308	103.61	-38.7	-212.8	-7.13	0
Ratio student per classroom (paroch. level)	Unmatched	22.135	22.389	-6	-0.92	0.359	
	Matched	22.179	23.438	-29.9	-394.9	-4.5	0
Repetition rate (school level)	Unmatched	0.01816	0.02551	-21.2	-3.19	0.001	
	Matched	0.01801	0.02375	-16.5	21.9	-2.51	0.012
Ratio student per teacher (school level)	Unmatched	29.722	25.826	38.1	5.7	0	
	Matched	29.909	29.216	6.8	82.2	0.96	0.337

Bilingual system

Variable	Sample	Mean		%bias	% redu bias	t	p>t
		Treated	Control				
Score in Selben index	Unmatched	27.531	32.937	-54.5	-5.53	0	
	Matched	28.316	26.703	16.3	70.2	1.18	0.238
Schooling of the household's head	Unmatched	2.3252	4.4029	-53.6	-5.44	0	
	Matched	2.5521	1.9792	14.8	72.4	1.19	0.234
Household's head is indigenous	Unmatched	0.96117	0.96117	0	0	1	
	Matched	0.97917	0.98958	-5.4	.	-0.58	0.563
Household's head is female	Unmatched	0.13592	0.15534	-5.5	-0.56	0.577	
	Matched	0.14583	0.08333	17.7	-221.9	1.36	0.176
Hh members aged <6	Unmatched	1.1893	0.98058	20.9	2.12	0.034	
	Matched	0.96875	0.92708	4.2	80	0.31	0.756
Hh members aged 6-17	Unmatched	3.1019	3.2282	-9.9	-1	0.317	
	Matched	3.1771	3.1563	1.6	83.5	0.11	0.913
Hh members aged 18-44	Unmatched	1.9126	1.733	16.7	1.7	0.09	
	Matched	1.9167	1.8542	5.8	65.2	0.42	0.673
Hh members aged 45-64	Unmatched	0.52427	0.54854	-3	-0.31	0.758	
	Matched	0.55208	0.53125	2.6	14.2	0.18	0.856
Hh members aged >64	Unmatched	0.19417	0.08738	20.4	2.07	0.039	
	Matched	0.125	0.16667	-7.9	61	-0.73	0.467
Poverty incidence		92.494	91.635	9.8	1	0.318	
		93.687	95.15	-16.8	-70.3	-1.89	0.06
Illiteracy rate (parochial level)	Unmatched	40.932	44.402	-20.6	-2.1	0.037	
	Matched	46.574	48.706	-12.7	38.6	-1.09	0.277
Ratio student per teacher (paroch. level)	Unmatched	16.951	21.318	-65.9	-6.69	0	
	Matched	21.281	22.282	-15.1	77.1	-1.18	0.241
Ratio student per school building (parochial level)	Unmatched	97.535	72.542	62.1	6.31	0	
	Matched	77.844	70.14	19.2	69.2	1.6	0.11
Ratio student per classroom (paroch. level)	Unmatched	26.051	25.607	8.5	0.86	0.39	
	Matched	25.887	26.423	-10.3	-21	-1.1	0.271
Repetition rate (school level)	Unmatched	0.00259	0.00078	24.2	2.45	0.015	
	Matched	0.00238	0.00083	20.6	14.6	1.46	0.146
Ratio student per teacher (school level)	Unmatched	24.656	24.384	3.1	0.31	0.754	
	Matched	26.734	25.275	16.6	-435.9	1.06	0.288

Appendix C
Bias adjusted matching

Hispanic

	Second grade			Fourth grade		
	One to one	*Five nearest*	*Ten nearest*	*One to one*	*Five nearest*	*Ten nearest*
Math						
ATT	4.6681*	4.7952*	4.6319*	2.4243*	2.1511*	1.9468*
	(0.4932)	(0.4087)	(0.3884)	(0.5228)	(0.4200)	(0.3784)
Language						
ATT	1.8583*	1.8525*	1.8569*	0.7161	0.7625	1.0059*
	(0.4359)	(0.3535)	(0.3340)	(0.5206)	(0.3638)	(0.3243)
Cases	927	927	927	870	870	870

Indigenous

	Second grade			Fourth grade		
	One to one	*Five nearest*	*Ten nearest*	*One to one*	*Five nearest*	*Ten nearest*
Math						
ATT	7.5548*	3.8543*	3.3708*	-1.0009**	0.2538	-0.2688
	(0.5332)	(0.4505)	(0.4040)	(0.4441)	(0.4136)	(0.4149)
Language						
ATT	-0.2564	-5.4606*	-5.7938*	3.3094*	4.370*	4.1953*
	(0.5179)	(0.4267)	(0.4108)	(0.4501)	(0.4414)	(0.4391)
Cases	412	412	412	348	348	348

Note: *Significant at 1%, ** Significant at 5%, and *** significant at 10%. Standard errors are in parenthesis.

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