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Towards more Equitable Distribution of Educational Opportunities: What is the Impact of the Preferential Subsidy Policy on the Achievement Level of the most Vulnerable Students in Chile?

Hacia una distribución más equitativa de las oportunidades educativas: ¿cuál es el impacto de la política de Subvención Preferencial en el desempeño académico de los alumnos más vulnerables en Chile?

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Abstract

In this paper we examine the effects of a school finance reform implemented in Chile in 2008. This reform changed the school funding formula from one based on student enrollment to one that gives between 50 and 60 percent more funding to each student from a lower socioeconomic background. This study presents evidence on the effects of this reform on student academic achievement in mathematics and language. The evidence relies on the use of comparative interrupted time series analysis to identify the impact of this policy. The analysis uses the changes in student test scores from before and after the implementation of the policy between participating, non-participating, and late-entry schools. The results indicate statistically significant differences in mathematics performance gains of 4th graders (0.18 SD) after 4 years of treatment compared to students without treatment, and 0.07 SD in language gains. The results also show a higher performance increase for vulnerable students compared to non-vulnerable ones. However, these advantages cannot be attributed to the program.

Keywords: educational policies, school finance, disadvantaged students

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Resumen

En este artículo se examinan los efectos de la reforma al sistema de financiamiento de las escuelas implementada en Chile en 2008. Esta reforma cambió la estructura de financiamiento de una fórmula basada en el número de alumnos que asisten a la escuela a una que entrega entre un 50 a 60 por ciento más de recursos a aquellos alumnos de menores niveles socioeconómicos. El estudio presenta evidencia sobre los efectos en el desempeño escolar en matemáticas y lenguaje basada en análisis de series interrumpidas de tiempo comparadas. Así, se usan los cambios en los resultados de pruebas estandarizadas antes y después de la implementación de la política, comparando escuelas que participaron del programa y escuelas que no participaron o que entraron tardíamente. Los resultados muestran diferencias estadísticamente significativas y positivas de aumento en el desempeño de matemáticas para alumnos de 4to grado (0,18 DE) después de cuatro años de tratamiento comparado con escuelas de control; también se observan diferencias de 0,07 DE en lenguaje. Los resultados también muestran un mayor aumento del nivel de desempeño para los alumnos que están calificados como vulnerables comparados con aquellos que no lo están. Estas últimas diferencias, sin embargo, no pueden atribuirse unívocamente al programa.

Palabras clave: políticas educativas, financiamiento de las escuelas, alumnos vulnerables

It is generally considered that education is one of the most important mechanisms to reduce poverty and achieve more equitable distribution of opportunities in our societies. For this reason, systematic educational inequalities, particularly those related to the social or economic status of the parents, represent a huge challenge for education to contribute effectively to that expectation.

Unfortunately, the evidence shows that reducing social inequalities is a complex task. Policies which seem to be promising options in theory —such as providing greater resources for more disadvantaged students— have often failed to produce the expected results. Indeed, evidence from studies that examine the positive effects of providing additional resources is rather ambiguous (see Hanushek, 2003; Wößmann, 2003), even when resources are focused on the poorest students or schools (see Benabou, Kramarz, & Prost, 2009; Lavy, 2012, among others). Some academics, such as Hanushek (2003), have argued that improvement policies based on resources and inputs have simply failed to improve school learning.

So, the question remains open on how much can be achieved by providing more resources to the most vulnerable students or how the school funding system can be organized to reduce learning gaps and provide social opportunities in a more equitable manner.

The objective of this study is to contribute to answering this question by studying the effects of the education reform implemented in Chile in 2008 through the Preferential School Subsidy (SEP by its Spanish acronym). This reform changed the funding mechanism from a formula heavily based on the number of students served by each school to one that considered additional resources depending on the number and concentration of students from lower socioeconomic levels. This study thus seeks to quantify the effect of this program on improving learning at the schools that participated in the new financing scheme. Although other studies have already addressed the quantification of these effects (see Valenzuela, 2013, for example), the idea is to extend the analysis using complementary methodological approaches and addressing different effects on more or less vulnerable students. Furthermore, considerations of cost-effectiveness of the policy are also raised.

The rest of the article is organized in the following manner. First, the preferential subsidy system is described; then there is an analysis of the literature; subsequently the methodology of analysis and the data are presented; and the results are presented and discussed; and finally, the main conclusions of the study are outlined.

Description of the preferential subsidy system

The preferential school subsidy system (SEP) is a national program that provides an additional grant per student to schools catering to those pupils classified as «priority» (approximately a third of students of a lower socioeconomic status) and that sign an equal opportunities agreement. The increase in the subsidy for the period considered in this study is close to 55% for priority students from pre-kindergarten to 4th grade (it is lower for higher grades) and depends on the level of concentration of poverty in schools (see Weinstein, Fuenzalida, & Muñoz, 2012). The equal opportunities agreement includes a series of commitments on the part of the school, such as preparing a four-year improvement plan with priority given to vulnerable students, non-selection, and accountability for the funds received, among others (see Elacqua, Mosqueira, & Santos, 2009; Weinstein et al., 2012, among others).

Origen of the SEP program

Before the government proposed this financial reform, there was already widespread concern about educational disadvantages of poorer students in Chile. This has been documented since the eighties, when the System of Measurement of the Education Quality (SIMCE by its Spanish acronym) and its predecessors were implemented. Because of this, after the country's return to democracy, various educational interventions were carried out in order to increase opportunities for students of a lower socioeconomic status (Cox, 2003).

However, the idea of creating a more radical reform of school finance, which would provide additional resources for the most vulnerable students, officially began in 1999 when a group of academics proposed a subsidy system that would supply funds in accordance with the number of students per school and their socioeconomic status (González, Mizala, & Romaguera, 2002). A few years later, after a technical and political debate, the government decided to take control of the motion and, in 2005, the Ministry of Education coordinated with the Ministry of Treasury and the Budget Division to prepare a joint proposal. The preferential subsidy law, which made the SEP program official, was sent to Congress in early 2006 and was approved in early 2008. As an objective of this law, it was agreed to contribute to social equality, promote equal opportunities, and improve the quality of the Chilean education system.

Main components of the program

This reform raised the subsidy per economically disadvantaged student between 50% and 60%, initially covering students from pre-kindergarten to 4th grade. Later, the subsequent grades were included. All public and subsidized schools that receive contributions from the state and whose students are considered priority or vulnerable can opt for additional funds. However, these schools must first apply by submitting an improvement plan, accept higher levels of accountability, and guarantee there is no selection of their students.

To categorize students as priority, the government calculates a weighted score that considers family income, the education of the parents, and other socioeconomic characteristics. Those obtaining scores below the third decile can choose to receive these additional resources.

While the most visible and highlighted dimension of the law is the injection of more resources into schools, it also imposed new conditions to regulate the use of these resources, which were different from those governing the use of the existing subsidy. Schools that decide to be included in this program must sign an equal opportunities and academic excellence contract with the Ministry of Education. The contract imposes a number of new conditions. First, schools must prepare and implement a four-year plan to improve the quality and equity of the education provided by that institution, specifically focusing on students with poor academic performance. The Ministry of Education must approve this plan. Second, schools must justify their spending and report on the progress in implementing the agreed plans. Third, schools must ensure that they do not select students or expel students who enrolled between 1st and 6th grade. Schools also may not charge copayments to the families of students. Finally, the contract is signed for four years with the possibility of renewing it for the same period of time.

It should be noted that schools were given varying degrees of autonomy in the use of the resources. Participating schools are classified into three categories depending on their performance in national tests of learning and other complementary indicators. Those schools placed in the lowest category must prepare plans with the Ministry of Education and accept support from an external institution.

Consideration of additional conditions implied by participation in the program is important for two reasons. First, because the potential improvement may be related not only to the provision of additional resources, but also to the need to prepare improvement plans, receive external advice, or other required conditions. Second, because participation implies an administrative cost associated with the preparation of improvement plans and, in the case of private schools, limiting copayments and restriction of the selection process for priority students. This situation will probably lead to many subsidized schools delaying their entry into the program.

Literature review

Measuring the relationship between school resources and academic performance is a task that is considered critical for the development and assessment of educational policies and this has been the subject of a great many studies.

Empirical evidence on the relationship between spending and school performance has not been very conclusive (see the meta-analysis by Hanushek, 1986; Hanushek & Luque, 2003). Considering more recent literature, Wößmann (2003), using TIMSS data, finds no significant relationship between science and mathematics results and the differences between school resources. Häkkinen, Kirjavainen and Uusitalo (2003) use a discontinuity in education spending in Finland caused by a period of economic recession and also fail to find a relationship between the reduced spending and a change in academic performance. For these reasons, authors such as Hanushek (2003) have stated that policies based on resources or inputs have simply not contributed to the improvement in school performance, at least in the United States.

However, other studies have found a positive relationship. Levy (2002), for example, finds that an increase in education spending was related to an increase in academic performance and a reduction in dropout rates in high school or middle school in Israel. Card and Krueger (1996) found evidence in the United States of a positive relationship between higher education spending and higher future income of students. Based on this and other evidence, some authors argue that it is possible to make an impact on students through investments in resources, especially if these investments are focused on students with greater educational needs.

Resources focused on social disadvantaged students

However, the evidence on the impact of using greater resources focused on more disadvantaged students is also ambiguous. Leuven, Lindahl, Oosterbeek and Webbink (2007) analyze the case of subsidies focused on socially disadvantaged students in the Netherlands, which allowed increased spending on staff and computer equipment. The results suggest a nonexistent or negative effect on the performance of students. Benabou et al. (2009) study a French program that provided greater resources to schools in socially disadvantaged geographical areas and found no significant relationship between additional resources and the academic performance of students. In contrast, Lavy (2012) examined the effect of an experiment conducted in Israel which increased the subsidy per student provided given to the poorest pupils and included an increase in the time spent on key subjects. The results indicate that the higher spending and greater time were associated with improvements in performance.

In the United States the most important policy is the Elementary and Secondary Education Act (ESEA). Known as Title I, it was approved in 1965 as part of the War on Poverty devised by President Lyndon B. Johnson. It is considered to be the most important initiative to reduce social inequalities in education. Its aim was to close education gaps between students from different racial and socioeconomic groups. It included the provision of additional resources to schools that served a high proportion of socially disadvantaged students (see Cohen & Moffitt, 2009; Kosters & Mast, 2003).

Mullin and Summers (1983) conducted a review of 47 studies published between 1969 and 1980 and found that most studies showed evidence of a positive impact, that these impacts were higher in

early grades, and that no kind of intervention seemed to be consistently better than any other. Similarly, Borman and D'Agostino (1996) carried out a meta-analysis of 17 studies involving a total of 40 million students tested between 1966 and 1993. The results indicate a positive but moderate effect of policies (average size of effect 0.11 SD). They also found that the effects were larger for mathematics than language and greater in early grades (1st to 4th). Van Deer Klaauw (2008), on the other hand, assesses the impact of compensatory programs implemented in New York that were funded through Title I. The evidence is consistent with a rather negative impact that the authors attribute to misuse of resources. Matsudaira, Hosek and Walsh (2012) used discontinuous regression models to measure the impact of the program in a US district, but did not find any significant effect.

Considered together, the aforementioned studies provide mixed evidence on the effects of providing more resources to improve school performance, even when these resources are focused on socially disadvantaged students.

Evidence for the Chilean case

In the case of Chile, there is recent literature that has focused on identifying the effects on SIMCE learning indicators. This literature generally establishes a positive effect of the program, as in the case of Valenzuela, Villarroel and Villalobos (2013), as well as in the ongoing studies of Neilson (2013) and Mizala and Torche (2013), or the degree thesis of Carrasco (2014), on which this article is based. These authors, with the exception of the latter, have used fixed effects model methodologies at the school and year level, or on differences to quantify the effects of the preferential subsidy, and they conclude that there are significant effects of a magnitude of between 0.10 and 0.20, although in the case of Valenzuela (2013) the analysis is restricted to 2008-2010.

Although not the focus of this study, it is important to consider the effects of the program on dimensions that go beyond learning indicators. Irarrázaval et al. (2012), for example, found evidence of improved school practices, especially in the areas of curriculum management, and human and material resources. In contrast, Elacqua et al. (2013) warn of potential undesirable effects of increased accountability associated with this policy and regarding behavioral adjustments in their practices of improvement. In this regard, the authors state that schools with higher pressure associated with the quality indicators used by the program make greater use of short-term strategies to improve results at the expense of strategies to create long-term skills.

In that sense, the contribution of this study relates to the use of complementary methodologies to those used in the stated studies. Indeed, this study uses «interrupted time series», a methodology that allows the pre- and post-implementation trajectories of the policy to be modeled and control to be exercised, not only according to differences in level but also by differences in trajectory between schools that receive and do not receive a certain treatment. Potential noise in the measurement of a given year can thus be neutralized to achieve a more direct visualization of the effect of that policy. In addition, other methods of analysis are used to guarantee the robustness of inferences, such as trend analysis of explanatory factors for school performance. This article also introduces differentiated analyses in the case of students who are considered vulnerable and non-vulnerable in schools as a way to assess the intra-school convergence between these groups.

Data

This study considers a total of 9,308 institutions (4,255 municipal and 2,941 private subsidized schools) and uses information from the results of the System of Measurement of the Education Quality (SIMCE) from 2002 and from 2005 to 2011. The SIMCE databases provide information on the level of performance in language and mathematics in the 4th grade, and demographic information on students and the socioeconomic background of families (income and education level of parents, etc.).

This information is combined with information provided by the official records of the Preferential Subsidy Program regarding the years of participation in the program. In the first year in which the policy went into effect, more than 95% of municipal schools applied to the SEP program, while only half of subsidized private schools applied. Finally, information is also used from the administrative databases of Chilean schools on dependence, rurality, number of students, and other contextual information.

Methodology

The general model of analysis, the operationalization of the concept of *treatment* for this particular study and the definition of school groups *under treatment* and *under control* is shown below.

Basic analysis model

The study uses a method known as Comparative Interrupted Time Series (CITS), which compared the deviations of trend before and after the implementation of a program between a group under treatment and a control group.

In this study, the trajectories of schools that receive SEP resources (schools under treatment) before and after implementing the program are compared. Thus, the existence of breaks in the trajectory may reflect the existence of some factor causing the change. In addition, the comparison with the trend of the control group would rule out unobserved factors that may have influenced the trend of both the group under treatment and the control group. This type of strategies have been used in educational research (see Bloom, 1999; Dee & Jacob, 2011; Shadish, Cook, & Campbell, 2002; Wong, Cook, & Steiner, 2009). Formally:

$$A_{ist} = [a_0 + a_1 (t - t^*) + a_2 Post_t + a_3 (t - t^*) Post_t] +$$

$$+ [a_0' T_{st} + a_1' (t - t^*) T_{st} + a_2' T_{st} Post_t + a_3' (t - t^*) T_{st} Post_t] + e_{ist} \quad (1)$$

A_{ist} represents the academic performance of student i in the fourth grade, in the school s and in the period t. The year t^* represents the year of reference, 2007. $Post_t$ is a dichotomous variable that takes a value 1 for any year subsequent to 2007. T_{st} is a dichotomous variable that takes the value 1 starting from the year in which the school started receiving SEP resources. Finally, the coefficients a_i represent the levels and gradient of the trajectories before and after implementation of the policy. So, the effect of the SEP policy in any year t_p after t^* can be calculated as $a_2' + a_3' (t_p - t^*)$, where the value indicates whether the difference in the trajectories of the schools treated before and after the program was implemented differs from the trajectories for the control group.

Considering that the treatment is considered to be complete after four years of participation in the program, the null hypothesis is that there is no significant difference in performance between students in schools treated for four years and those that did not receive resources for that period or which received partial treatment (for example, during one or two years).

In order to improve the robustness of the results, the study considers additional controls which include the use of parametric and non-parametric models to determine the differences in trajectory between schools, as well as trend analysis of other factors that could explain the trend breaks, such as changes in the composition of the students or the size of the classes.

It should be noted that, although none of these approaches alone can guarantee a causal interpretation, the combination of these strategies and the consistency of the results strengthen the robustness of the inferences.

Operationalization of the concepts of "treatment", "schools under treatment" and "control schools"

This study considers all pupils in the same generation (4th grade) in each school as a unit of treatment. This means that even students who do not qualify as vulnerable, in a particular school, are included in the treatment group. This assumption makes sense because the program does not require the additional resources to be allocated to students specifically classified as vulnerable.

In a second stage, a differential analysis is carried out to estimate the effects of the program separately for vulnerable and non-vulnerable students. In any case, the results of this analysis corroborate the supposition that the positive effects of the program are not exclusively restricted to students who are classified as vulnerable.

Definition of schools under treatment

It is important to point out that the analysis takes into account not only participation in the program, but also the time during which the institution has been participating. In this regard, a school is considered to be receiving *complete treatment* if it has participated for a period of four years. That is, whether the cohort of students assessed in 2011 has been in the program since 1st grade (in this case, since 2008).

So, for the purpose of this study, the group of schools considered to be *under treatment* would be comprised of schools that received *complete treatment*. In other words, the schools that entered the program in 2008 and, therefore, whose students received the benefits of the program from 1st to 4th grade.

Definition of control schools

It is not easy to find a group of schools that have not been affected by the SEP program and which is suitable as a comparison group. Conceptually, an ideal comparison group would consist of schools that had the same level and trend as schools under treatment in the absence of the SEP program.[1]

Given this difficulty, the study compares the trajectories of schools under treatment with the trajectories of various control groups. The first comparison group consists of schools that could have participated in the program but did not apply during the first four years (2008-2011). As almost all municipal schools applied during the first year of implementation, the candidate schools for the control group are mostly private subsidized schools (more than half chose not to participate in the first year).

The first concern that arises regarding the validity of this approach is the use of private schools as a group of comparison for a mixture of both private and municipal institutions. Indeed, private and municipal schools could differ in certain dimensions that undermine the validity of the estimates. To address this problem, the results are presented separately to carry out a comparison solely between subsidized private schools and another that also includes municipal schools.

A second concern refers to the endogeneity of participation. The reasons for not participating could be due to structural factors that also affect school performance and, therefore, make comparisons inadequate with schools not participating in the program. However, there is a significant group of private schools that simply entered the program late. There is anecdotal evidence that at least some of the schools that entered late had delayed their entry to evaluate the costs of participating in the program (e.g. the administrative burden or ministerial interference). So, after observing the situation of other private schools and evaluating the evolution of ministerial supervision, they decided to participate. In this case, these are decisions that probably have less to do with the performance of schools and, therefore, would not restrict them from being candidates for control groups. For this reason, the decision was taken to make additional comparisons between schools that entered the program in the first year and schools that entered in the third or fourth year.

It should be noted that prior trajectories of the group of schools who entered the program in the first year and those that entered late are extremely similar, not only in terms of the trends, but also the level. Although this similarity is not a requirement for the comparison, it does endorse the validity of making a comparison between these groups of schools. If the trajectories were similar during the period prior to the intervention, there is good reason to believe that, in the absence of other structural changes, the trajectories should have remained similar for both the treated and untreated schools.

Although addressing the problem of endogeneity of the decision is difficult, the use of the aforementioned strategies helps mitigate possible bias in the estimation of the effects of the program.

Evaluation of other factors that could affect the change in trajectories

It is important to consider that the validity of comparing schools under treatment and control schools could be limited if factors outside the program that affect school performance changed a) after the start

[1] Strictly speaking, this assumption is not a requirement of validity for the inferences. The only requirement is that the effects of factors external to the program on *trajectory changes* must be similar for the control group and treatment group. In other words, if there was some unobserved factor explaining trend changes, this would affect the comparison groups similarly.

of the program, b) mostly in one of the groups (treatment or control), and c) by a sufficient magnitude to explain the differences identified. Indeed, if, for example, the educational level of parents improved by much in the schools under treatment just after they entered the program, the improvement in performance could be due more to this change in educational level than to the program.

Even though it is very difficult to confirm these aspects directly, the idea is to provide indirect evidence that can clarify the existence of such situations. Following Wong et al. (2009), the analysis involves the same models presented above (CITS), but using the socioeconomic status of the students and the size of the classroom as a dependent variable (using different indicators). Thus, the *effect of treatment* in this case provides evidence on whether there was a significant variation in these explanatory variables after the start of the program that could explain the differences in performance.

Results

The results show significant differences in school performance in both mathematics and language. In mathematics, the differences attributable to participation in the program are approximately 0.18 SD after four years of participation, compared with the control groups. In the case of language, the differences are smaller (approximately 0.07 SD) and their significance depends on the specification of the models.

Graphic analysis of trajectories

Generally speaking, the trends appear to be relatively stable and linear for the first six years of trajectory experienced before implementation of the program. The level of SIMCE performance in the control schools tends to be higher, although the trends are similar. After implementation of the program, even though both groups improve their performance level, schools under treatment do so at a higher rate. This difference is higher for mathematics than for language.

The comparison between subsidized schools that participated in the program for four years and those that participated for one year is particularly illustrative. For example, Figure 1 shows the difference in trajectories in mathematics between students in subsidized private schools that participated in the SEP program for four years (i.e. since 2008) and those that entered later (2011) and only had one year of treatment. In this case, as stated above, the schools under treatment and the control schools have very similar trajectories prior to the program, both in terms of level and trend.

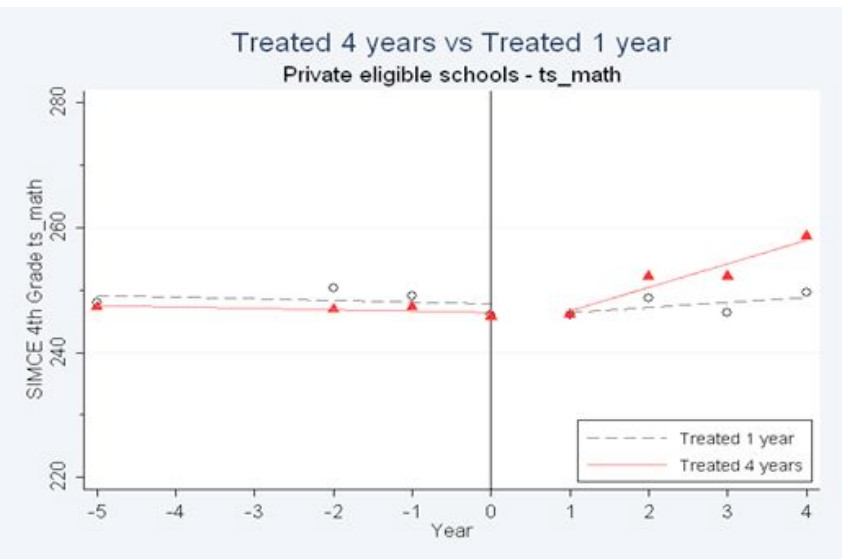

Figure 1. Difference in trajectory between students in schools that participated in the SEP for 4 years vs. 1 year (eligible private schools). NB: The year 0 corresponds to 2007, prior to the implementation of the SEP program.

Regression analysis

As a second approach, the differences between the treatment and control groups were estimated four years after the policy was first implemented, that is, after the first cohort who started the program in 1st grade graduated.

The most significant results are shown in Table 1 and Table 2. The parameters of the linear model of the trajectory difference after implementation of the policy are shown; in this case, the constant and gradient of the curve that best fit the trend subsequent to the start of the program in 2008. They also include the estimate of differences four years after the program began, both in terms of the SIMCE score and in terms of standard deviations.

The results show systematic and positive differences in changes of trajectory between treated schools and control schools, before and after the program was started. The differences vary from 0.04 to 0.09 SD for language and between 0.13 and 0.34 SD for mathematics, depending on the comparison groups.

Table 1
Estimated effects of SEP program in 4th grade: mathematics scores

		Differential effect compared with the control group							
		Without covariates				With covariates			
		Level	Gradient	Total effect after 4 years		Level	Gradient	Total effect after 4 years	
Schools under treatment for 4 years vs.				In SIMCE points	In SD			In SIMCE points	In SD
Without treatment	Muni. and elig. priv.	-5.71**	3.91**	9.92**	0.198	-5.17**	3.36**	8.27**	0.165
	Eligible private	-4.14**	2.91**	7.51**	0.150	-3.36**	2.50**	6.63**	0.133
Treated for only 1 year	Muni. and elig. priv.	-2.73	4.97**	17.10**	0.342	-3.05	4.05**	13.10**	0.262
	Eligible private	-5.47**	4.04**	10.70**	0.214	-0.73	2.53**	9.41**	0.188
Treated for 1 or 2 years	Muni. and elig. priv..	-4.27**	4.05**	11.93**	0.239	-4.97**	3.85**	10.43**	0.209
	Eligible private	-2.46	2.95**	9.35**	0.187	-3.33+	2.69**	7.45**	0.149

** p < 0.01, * p < 5%, + p < 0.10.

Table 2
Estimated effects of SEP program in 4th grade: language scores

		Differential effect compared with the control group							
		Without covariates				With covariates			
		Level	Gradient	Total effect after 4 years		Level	Gradient	Total effect after 4 years	
Schools under treatment for 4 years vs.				In SIMCE points	In SD			In SIMCE points	In SD
Without treatment	Muni. and elig. priv.	-2.75**	1.85**	4.64**	0.093	-2.26**	1.30**	2.93*	0.059
	Eligible private	-1.36+	1.20**	3.44**	0.069	-0.58	0.88*	2.93*	0.059
Treated for only 1 year	Muni. and elig. priv.	-2.32	2.51**	7.72*	0.154	-2.21	1.71*	4.65	0.093
	Eligible private	-0.79	1.64	5.78	0.116	-0.48	0.98	3.43	0.069
Treated for 1 or 2 years	Muni. and elig. priv.	-2.06+	1.55**	4.14+	0.083	-2.26+	1.23*	2.67	0.053
	Eligible private	-0.19	0.99	3.77	0.075	-0.79	0.69	1.97	0.039

** p < 0.01, * p < 5%, + p < 0.10.

The results for both subjects indicate a positive average effect for schools under treatment and are robust for the different specifications, both parametric and nonparametric.[2] Note that the effect is consistently higher for the models without control variables (0.03 higher on average for both subjects).

Analysis of changes in composition in participating or control schools

As stated previously, to improve the robustness of the results it is useful to review possible changes in the composition of students (in terms of socioeconomic status, for example) or the school structure (class size) in order to dismiss these variables as factors that could explain the differences in performance.

As done by Wong et al. (2009) and Dee and Jacob (2011), the composition of schools is examined in terms of factors that could affect results. For that purpose a change of indicators for the size of the class and socioeconomic level is explored. To illustrate the trends in these variables, the trajectories for the education of the mother in eligible private schools are shown in Figure 2. The figures for the other control variables are shown in Appendix A (Figure A1).

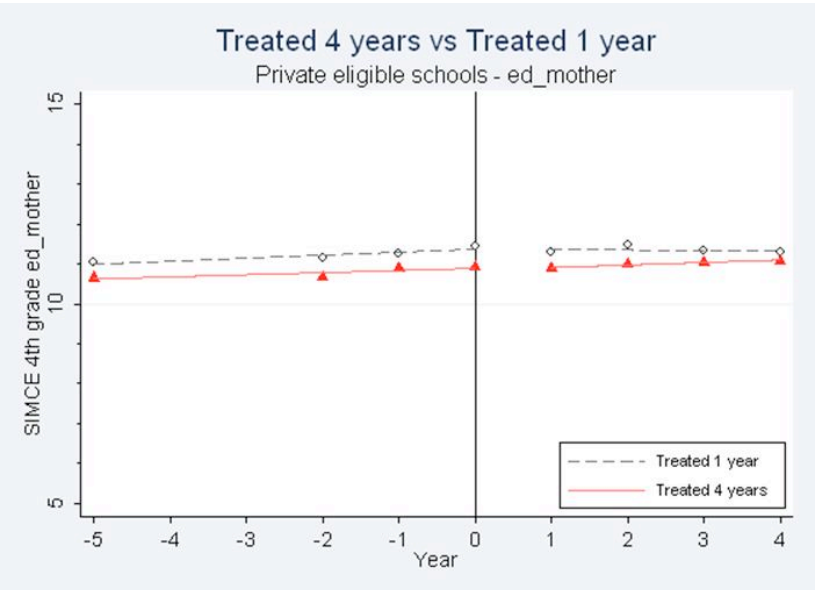

Figure 2. Changes in composition for the educational level of the mother (eligible private schools). NB: The year 0 corresponds to 2007, prior to the implementation of the SEP program.

In Figure 2 similar trends can be observed both before and after the implementation of the program. In the case of the other variables, there is a greater increase in the size of the classes for the treated schools. In this particular case, however, this increase, if it affects academic performance, would do so against schools under treatment and would, therefore, support the inferences drawn. In the case of the socioeconomic indicators, no major differences can be observed in the trends, except the concentration of vulnerable students, which seems to be slightly increased in the case of the control schools. This would undermine the results of the control schools and thus weaken the inferences. However, considering the sizes of the differences, it would be hard to explain the differences measured as an effect of the program.[3] In any case, this would justify the use of controls.

[2] The results of non-parametric models are not included in this article, but may be requested.

[3] In terms of the magnitude of these changes (see Table A1, Appendix A), the majority are not significant when the trajectories of the schools under treatment are compared with the control schools. However, the presence of some significant differences justifies the use of these variables as controls for the estimate.

Differential effects due to vulnerability of the students

One of the main aims of the program was to reduce the gaps associated with socioeconomic factors. This would probably be achieved if schools that received the subsidy improved their performance, since the treated schools cater specifically to the most disadvantaged students. However, it may be that these improvements do not reduce the learning gaps if, for example, the most vulnerable students in these schools improve less than less vulnerable students. For this reason, it is interesting to compare the trajectories of both groups of students.

In this regard, there are three questions that one would like to answer to obtain a more complete picture of what happens in schools and as a result of the program: a) if vulnerable students in schools under treatment improve more than vulnerable students in control schools; b) if these students are improving more than non-vulnerable students in schools under treatment; and c) if the difference in improvement between vulnerable and non-vulnerable students is higher in treated schools than in control schools.

The first question is relevant because if the answer is affirmative, one could infer that the policy is benefiting vulnerable students, because treated vulnerable students are improving more. The second question is whether vulnerable students are getting closer to non-vulnerable students within the universe of treated schools. This question is relevant because if the answer is negative, the program would be benefitting non-vulnerable students more, which could eventually increase performance gaps, even if everyone were improving. Finally, the answer to the third question indicates whether the difference in change between vulnerable and non-vulnerable students in treated schools is greater than the difference in the control schools. In other words, whether the policy is particularly beneficial to vulnerable students. It could be the case that vulnerable students are improving comparatively less in both schools under treatment and control schools. Ideally, however, one would expect that the differences between vulnerable and non-vulnerable students would favor vulnerable students and that the difference would be even higher in schools under treatment.

By way of illustration, the graph showing the trajectories for both vulnerable and non-vulnerable students in treated and control schools is shown below (Figure 3). It can be seen that vulnerable pupils in both treated and control schools have a lower performance than non-vulnerable students. After implementation of the policy, differences arise in the trajectories of vulnerable pupils in control schools and in schools under treatment, and between non-vulnerable students in both groups of schools. After the policy is implemented, however, students in schools under treatment tend to improve more than their counterparts in the control schools. This applies to both vulnerable and non-vulnerable students. In schools under treatment, vulnerable students tend to increase slightly more than non-vulnerable students. This pattern is repeated for all comparison groups.[4]

[4] The graphs are not reproduced here, but may be requested.

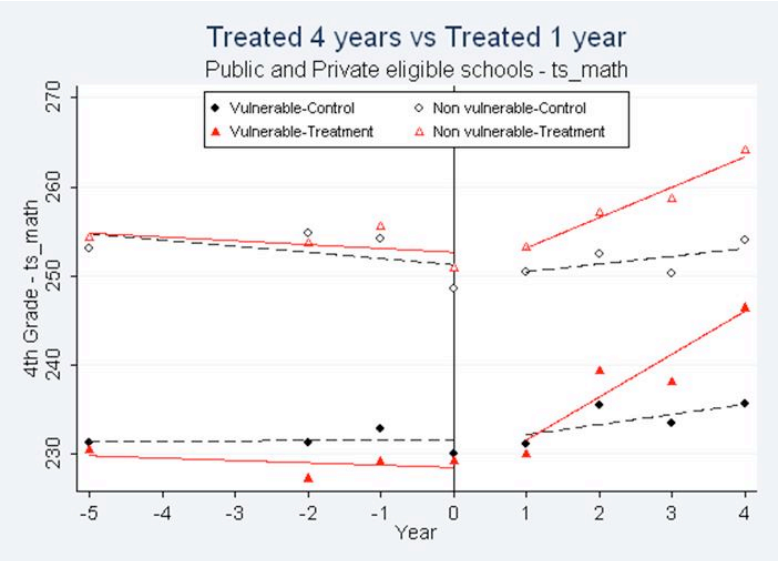

Figure 3. Comparison of trajectories of vulnerable and non-vulnerable students in schools under treatment and control schools (eligible private schools). NB: The year 0 corresponds to 2007, prior to the implementation of the SEP program.

To address the questions listed above statistically, two groups of trajectory parameters were included for both schools under treatment and control schools in order to describe trends both for vulnerable and non-vulnerable students. Based on this, the differences between the types of students in the case of questions nº 1 and nº 2 were compared, and for the case of question nº 3 differences in differences were compared. For the purpose of improving the robustness of the analysis, and only for question nº 2, fixed effects at the school level were included. The use of fixed effects highlights variations within schools and allows to control for the fact that the most vulnerable students are not randomly distributed, but are concentrated in certain schools.

In the case of mathematics, the results are the following. Regarding the first question, vulnerable students in treated schools improve their performance more than similar students in control schools (an average of 0.17 SD). The differences are significant in all models analyzed. Considering only treated schools, for the purposes of question nº 2, vulnerable students improve more than non-vulnerable students (an average of 0.10 SD). The results are significant in all models and consistent when a fixed effect is introduced at the school level. Finally, regarding Question nº 3, the results depend on the definition of control and treatment groups and, in most cases, the differences are not significant.

To summarize, vulnerable students in schools under treatment improved more than students in control schools. Furthermore, within schools under treatment, vulnerable students improve more than non-vulnerable students.

However, the differential increase between vulnerable and non-vulnerable students is not significantly higher in schools under treatment compared with control schools. This occurs because vulnerable students in control schools also improve more than non-vulnerable students and at a similar or slightly lower rate. Thus, the evidence of a positive effect of the program on vulnerable students depends on the choice of the comparison group. However, in this regard it could be argued that achieving a differential effect that favors vulnerable students when a large increase has already been achieved for all students (in the case of the treated schools) is more difficult than in the case of control schools, where the average increase is significantly lower. In any case, further investigation is required in this respect.

So, the evidence suggests a positive effect of the program on reducing learning gaps between vulnerable and non-vulnerable students for at least two reasons. First, students in schools under treatment improve more than students in control schools. Second, because the effects are greater for vulnerable students than for non-vulnerable students. However, the differential increase is similar in schools under treatment and control schools and, therefore, cannot be clearly attributed to the program.

The summary of the results is shown in the following table:

Table 3
Differential effects between vulnerable and non-vulnerable students (units of SD)

	Number of years in SEP (treatment vs. control)					
	4 years vs. 1 or 2 years		4 years vs. 1 year		4 years vs. 0 years	
Differences between:	Sub priv.	Eligible	Sub priv.	Eligible	Sub priv.	Eligible
Vulnerable vs. non-vuln. (control schools)	-0.010	0.075	0.110+	-0.061	0.150**	0.144**
Vulnerable. vs. non-vuln. (schools under treatment)	0.103**	0.103**	0.103**	0.103**	0.103**	0.103**
Vulnerable under treatment vs. those in control schools	0.255*	0.193**	0.151*	0.305**	0.091*	0.083
Non-vuln. students in schools under treatment vs. those in control schools	0.142*	0.166**	0.158**	0.142*	0.139**	0.123**
Differences in differences for vulnerable students	0.113	0.027	-0.007	0.163*	-0.048	-0.040

NB: the columns indicate two types of school universes: just subsidized private schools and all eligible schools (municipal and subsidized private schools).

Cost-effectiveness considerations

According to the previous analysis, participation in the program is associated with an increase of approximately 0.2 SD in mathematics. Since this increase is considered high in the literature (see Mosteller, 1995, for example), it would be an encouraging result to improve the performance of schools. However, for the purpose of guiding educational policies, it is important to consider not only the magnitude of the impact, but also its costs.

A rigorous analysis of cost-effectiveness transcends the scope of this study, as it would include careful identification of the direct and indirect costs involved (see Levin & McEwan, 2001, for example). However, a simple analysis can be done to illustrate the «cost-effectiveness» of the program. One way to express this cost-effectiveness is by using the quotient between the magnitude of the effect and the proportion of increased cost per student associated with the program. Considering that the additional investment per vulnerable student is about 60% of the base investment (about 750 dollars per student per year[5]), and considering that approximately 30% of the students in participating schools received this additional subsidy, the average increase per student, considering all the students in the generation, would be 18%. Since the entire intervention lasted four years, the total cost of the program per student would be about 540 dollars. Taking the average effect of 0.20 SD, one can estimate a cost-effectiveness ratio of 0.37, indicating that an increase of 1,000 dollars would be associated with an increase of 0.37 SD in mathematics. This would be relatively low compared to an average cost-effectiveness of 0.15[6] for policies to reduce class sizes.

[5] NB: The values are expressed in 2010 US dollars, adjusted by purchasing power parity (PPP).
[6] For the estimate of this ratio, see Carrasco (2014).

Discussion and conclusions

The reform of the school funding system carried out through the SEP program represents an important effort to reduce educational inequalities and help even out social opportunities. The overall impact of this policy is still unknown, both because more time is required to understand its effects and because it is expected that these effects should go beyond reducing gaps in mathematics and language.

In that respect, this study represents an effort to measure these impacts at the same time as understanding them better within schools, differentiating depending on the vulnerability of the students. In a broad sense, this study indicates that the policy has been successful both in improving school performance and in reducing socioeconomic gaps, particularly in mathematics.

The differences between participating schools for four years and comparable schools that did not participate or which only participated for one or two years is important in terms of the effect of the policy compared to other similar policies (see the case of Title I mentioned earlier). In this regard, this type of policy is promising in reducing learning gaps for other developing countries or industrialized countries with large differences, because it shows that the provision of additional resources to schools with more vulnerable students, accompanied by accountability mechanisms and support for schools, can lead to significant improvements.

It should be noted that these effects could be greater as both the government and schools achieve greater maturity in the functioning of the program and its benefits. Indeed, initial implementation faced practical setbacks in schools and among advocates, as well as in the government. It is probable that the initial difficulties particularly affected those schools that joined in 2008 compared with those that entered the program later. This would point to an underestimation of the detected effects due to lower initial effectiveness of the program associated with limitations of the implementation and, therefore, could be auspicious for a potentially greater impact of the program.

Despite the positive elements identified in this study and others like it, it is clear that replication or extension of the program would benefit from further research, for example, to help determine how important the resource component is compared to the generation component in a four-year plan, the accountability component or external support. It also represents a challenge since even a large increase in resources, like that associated with this program, is clearly insufficient to achieve a more ambitious goal of evening out educational opportunities for socioeconomic levels. Finally, it also remains to go deeper into the overall effects of this program, both positive and negative, which include not only cognitive components but also other dimensions of student development, such as capacity building and construction of educational cultures within schools.

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Appendix A

Table A1
Estimated differences for the control variables after four years of implementation of the SEP program

		Treated for 4 years vs.			
		Never treated		Treated for 1 or 2 years	
		Muni + priv.	Priv.	Muni. + priv.	Priv.
Sample size (for all years)		1,309,416	606,476	1,067,756	371,818
Class size					
	F_test_Ho	2.96	0.03	2.23	6.61 *
	Total_effect	-0.67	0.077	1.11	2.17 *
	sd	9.95	8.99	10.1	9.63
	Effect size	-0.07	0.01	0.11	0.23 *
NSE					
	F_test_Ho	25.5	20.2	14.2	3.61
	Total_effect	0.096 **	0.084 **	0.11 **	0.061
	sd	0.77	0.75	0.7	0.69
	Effect size	0.12 **	0.11 **	0.16 **	0.09
Education of mother					
	F_test_Ho	7.42	2.93	12.3	4.55
	Total_effect	0.17 **	0.11 +	0.41 **	0.27 *
	sd	3.33	3.03	3.22	3.06
	Effect size	0.05 **	0.04 +	0.13 **	0.09 *
% vulnerable students					
	F_test_Ho	3.95	0.0	6.65	2.15
	Total_effect	-1.34 *	0.0	-4.22 **	-2.62
	sd	25.7	19.9	24.4	21.7
	Effect size	-0.05 *	0.00	-0.17 **	-0.12

** p < 0.01, * p < 0.05, + p < 0.1

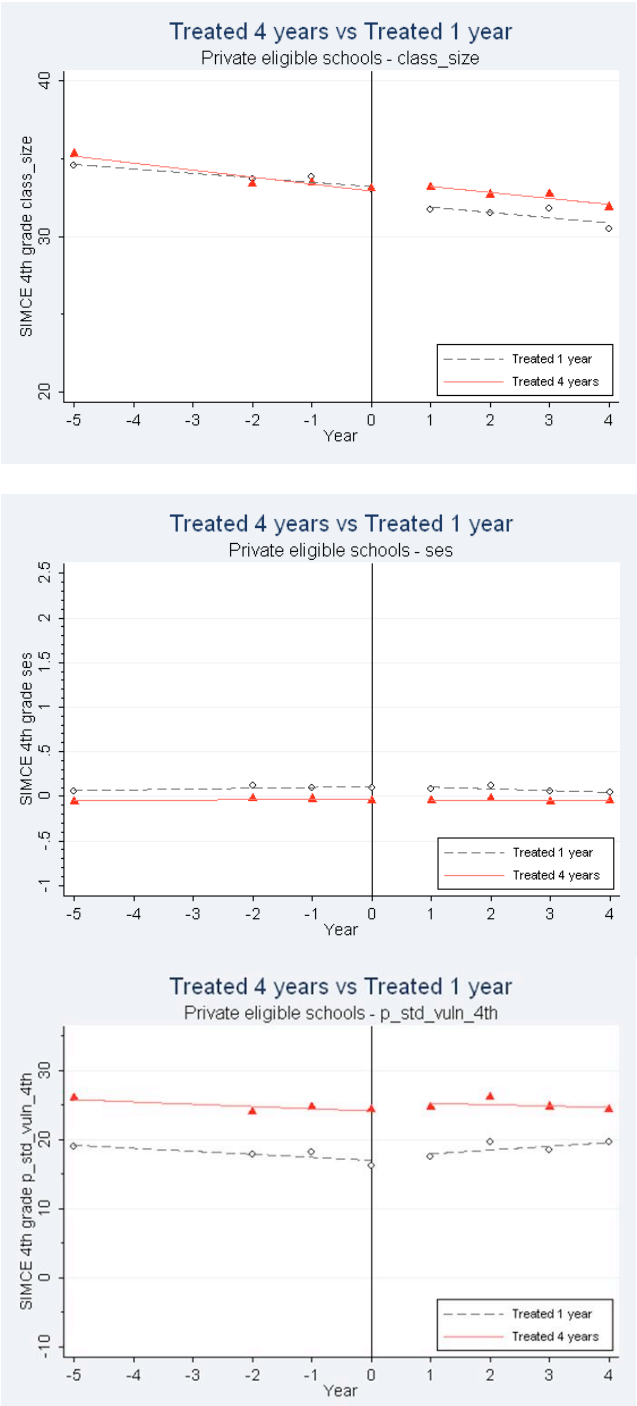

Figure A1. Interrupted time series for control variables: graphic representation.