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Nº 263 THE EFFECTIVENESS OF PRIVATE VOUCHER EDUCATION: EVIDENCE FROM STRUCTURAL SCHOOL SWITCHES

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The Effectiveness of Private Voucher Education: Evidence from Structural School Switches

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Abstract

In this paper we analyze the effect of private voucher education on student academic performance using new data on Chilean students and a novel identification strategy. Most schools in Chile provide either primary or secondary education. We analyze the effect of private voucher education on students that are forced to enroll at a different school to attend secondary education once graduated from primary schooling –structural switches. Moreover, the data set we use in this paper contains information on previous academic achievement and thus allows us to identify differences in students' unobservable characteristics. Using a number of propensity score based econometric techniques and changes-in-changes estimation methods we find that private voucher education leads to small, sometimes not statistically significant differences in academic performance. The estimated effect of private voucher education amounts to about 4 to 6 percent of one standard deviation in test scores. In contrast, the estimation methods used by the previous literature on Chile based on cross sectional data, i.e., that lacked information on prior test scores, leads to positive effects of about 14 percent of one standard deviation.

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I. Introduction

One of the most important debates in educational policy relates to whether different forms of school choice –charters schools, vouchers and others—should extensively be introduced (Hoxby, 2003). On the one hand, proponents argue that school choice generates competition, putting pressure on schools to improve the quality of the education provided by all types of schools. On the other hand, critics argue that choice produces sorting, isolating the most disadvantaged students into low performing schools. The vast theoretical and empirical literature presents a mixed picture of the impact of the different forms of school choice on student achievement.

Across countries, diverse initiatives have been undertaken in order to introduce choice into the educational system. Although residential choice is the most prevalent form of school choice in the United States, publicly and privately financed private voucher schools, open enrollment programs, charter schools and magnet schools coexist with traditional public schools.[1] As of 2007, 16% of American students in grades 1-12 were enrolled in chosen public schools.[2] Outside the United States, Chile, Denmark, Netherlands, South Korea and Sweden are countries with universal voucher programs. There are also countries with targeted voucher programs, some related to geographical areas (Cote d'Ivoire and the Czech Republic) or to specific populations (Colombia, Guatemala and Pakistan).

The evaluation of the performance of these diverse forms of school choice is a difficult task. Switching to a private voucher school might respond to the pursuit of higher school quality and peers or to shocks such as changes in family structure or employment opportunities

[1] Charter schools are public schools managed by a government appointed board with great administrative autonomy. Magnet schools are public schools that are allowed to attract students from outside zoned school boundaries by offering curriculum and pedagogical variety. They depend administratively on public school administration.
[2] Overall, 2% of 1st through 12th grade students attended charter schools in 2007. See Grady et al. (2010).

(Hanushek et al, 2004). Moreover, under universality of the voucher, treatment and control groups are extremely hard to build. In fact, selection bias may arise through two distinct channels. One is parental choice: families who take advantage of the voucher may have unobserved characteristics that are correlated with both, academic achievement and school type choice. The other is the manner in which schools select their students (Cullen et al, 2005 and 2006, and Hsieh and Urquiola, 2006).

In this paper we study the effect of private voucher education on student performance using a new data set on Chilean students that contains information on prior test scores and a novel identification strategy. Specifically, the Chilean education system consists of eight years of primary school and four years of secondary school. Most schools financed by the governmental voucher have primary or secondary education only. In fact, about 56 percent of 8^{th} grade enrollees must switch schools at the end of the academic year to attend secondary education. This exogenous change allows us to compare the secondary school performance of students who moved from a public to a private voucher, school with the secondary school performance of students that stayed in the public school system. In other words, the timing of structural switches is exogenous, allowing us to circumvent the phenomenon described as "Ashenfelter's dip" in the job training literature, i.e., selection on idiosyncratic temporary shocks (Ashenfelter, 1978).

Focusing on exogenous switches does not guarantee consistent estimators, however, because the assignment into different school types is not random. Limiting the analysis to students who attended primary education in a public school, together with the availability of previous test scores, allow us to account for this problem. Until recently only cross sectional data has been available on Chilean students, since national standardized achievement tests are administered annually to a specified grade level that rotates every year between the fourth, eighth and tenth grades. In 2004 and 2006, however, the test was administered to the same students – in

8th and 10th grades, respectively. Although test scores are not comparable over time, the results of the students in 2004 can be used to identify differences in students' ability when analyzing the 2006 results. In addition, primary school test scores reflect not only ability, they also reflect school type effects whenever the hypothesis that school type matters is true. So both controlling for pre-treatment achievement and limiting the sample to children attending the same types of schools in 8th grade, help to better account for selection bias.

Based on this identification strategy we estimate the effect of private voucher school education relative to public school education for those students who were forced to find a new school at the end of 8th grade. Specifically, in this paper we compare the 10th grade performance of students who moved from a public to a private voucher school (treatment group) with the 10th grade performance of students that stayed in the public school system (control group). That is, we estimate the effect of having attended two years of private voucher education after having attended a public school. In a sensitivity analysis, we also analyze the impact of structural moves from a private voucher school.

To estimate test score differences we use propensity score techniques and the changes-in-changes (CIC) approach developed by Athey and Imbens (2006) that allows for differences in the distributions of unobservables across treatment and control groups.

Propensity score based estimates are positive for math and language tests, and are statistically significant in most cases. The results point to a 2.4 to 3.0 test score gain, i.e., a gain of 4 to 6 percent of one standard deviation. Although significant in statistical terms, our findings point at a difference between private voucher education and public education that does not seem economically relevant. Validity tests, as those suggested by Imbens and Wooldridge (2009), indicate that the identification strategy is most likely appropriate.

The changes-in-changes approach yields positive estimates of the same order of magnitude at the mean. Moreover, the results indicate that the effect on language test scores is positive for students in the full distribution of results, whereas the effect on math test scores is concentrated at the upper end of the outcomes' distribution. However, the results are not statistically significant.

As a comparison and in order to better assess the effects of our methodological approach, we also estimate the effect of private voucher education using the techniques of most of the previous literature on Chile's school choice system; i.e., Heckman's correction for selection. This literature typically finds effects of about 0.15 to 0.2 standard deviations. In our full sample, we find an effect near 14% of one standard deviation when no correction for prior test scores is made, and an almost zero, not significant effect when we correct for previous test results. In other words, controlling for past achievement has a dramatic impact on the private school effect. Although we believe these results may be biased because prior test scores depend on the school type chosen for primary education, it is worth noting that all methods used lead to similar results: a very small effect of private voucher education on student achievement.

Summing up, our approach to selection bias relies primarily on the availability of pretreatment test scores, but also on a sample of students with potentially less selection on unobservables. This strategy, though, limits the generalizability of results as it might not be valid for children not undergoing a structural change. In addition, it does not guarantee that the selection problem has been fully dealt with despite validity tests suggest this is the case. These caveats must be kept in mind. Finally, and although based upon different theoretical assumptions about the underlying behavior of the data, propensity score type and changes-in-changes estimators yield similar results. These estimated effects are much lower than those obtained by

the previous literature on Chile based on cross sectional data, but in line with a number of papers on the United States' experiences that find small and many times ambiguous effects.

The paper is organized as follows: Section II reviews the previous literature on school choice. Section III provides a general overview of the Chilean educational system whereas Section IV explains our empirical strategy. Section V describes the data sources used in this study. Section VI presents our results and Section VII a number of extensions and sensitivity analyses. Finally, Section VIII concludes.

II. Previous Literature

In this section we focus on the empirical evidence on the differences in the academic achievement of students attending private voucher schools relative to those attending public schools. There is a closely related literature on the effect of competition on public schools, including the literature on sorting, that we do not review here.[3]

In the United States there are several small-scale voucher programs, mostly designed for low-income students.[4] Some are publicly funded, like the Milwaukee Parental Choice Program, the Cleveland Scholarship and Tutoring Program, and the Washington DC Opportunity Scholarship Program. Others are privately financed. From the public policy point of view, publicly-funded vouchers might be more interesting since they have the potential of being extended into larger school voucher programs. In general, the research done to date analyzing these experiences finds relatively small achievement gains for students offered educational

[3] Some studies analyze the impact of competition on public school students' achievement (see Hoxby, 2000 and 2003). Other papers study the effect of larger scale choice experiences, such as open enrollment within the Chicago Public School system, on students' outcomes, the degree of sorting and potential spillovers (see Cullen et al, 2005 and 2006).

[4] A review of the literature on the impact of private school vouchers can be found in Barrera-Osorio and Patrinos (2009), Belfield and Levin (2002), Hoxby (2003), Levin and Belfield (2003), McEwan (2004), Rouse and Barrow (2009), and Somers el al (2004).

vouchers. Some of these results are not statistically different from zero. In other words, the evidence suggests at most small improvements in the academic results of students who move to private schools thanks to the vouchers.[5]

There are many privately funded vouchers programs. However, only three of them have been evaluated using a randomized design where applicants were selected at random to receive a voucher: the New York City, the Dayton Ohio and the Washington DC experiences. Two evaluations – Mayer et al (2002) and Krueger and Zhu (2004)– report small but not statistically significant impact when all students are included, and significant positive effects when considering only African American students (Howell and Peterson, 2002 and Mayer et al., 2002). However, the positive impact on these students is not robust. In particular, Krueger and Zhu (2004) argue that the results are sensitive to decisions related to sample and race definition.

A related literature studies the performance of charter schools based on the observed mobility of students across establishments. Following Hanushek et al (2004) that estimates the costs and benefits of mobility in regular public schools, Hanushek et al (2007) analyze the Texas' charter experiment based on a panel of individual students that move across different schools, including charter schools. After controlling for student fixed effects to account for selection bias, Hanushek et al (2007) finds that after an initial start-up period, charter schools' performance is statistically similar to public schools' performance. The results also suggest that parents' of children attending charter schools seem more sensitive to quality in the decision of school switching. Using the same data set but a different set of indicator variables to account for school type and switches to and from charter schools, Booker et al (2004) find a significantly positive impact on students' academic achievement. Studies that compare before-and-after intervention

[5] See Rouse (1998), Witte et al (1995), Witte (1997) and Greene et al (1998) for the Milwaukee program, Metcalf (2001) and Belfield (2007) for the Cleveland case, and Wolf et al (2007 and 2008) for the DC program.

outcomes, however, might suffer from bias produced by "Ashenfelter's dip", i.e., the phenomenon that intra district choice is frequently assigned to school districts that have recently experienced a negative shock or that families may base their switching decision on the students' prior test scores. Then any gains in student achievement could be the result of mean reversion in performance rather than an effect of the treatment.

Outside the United States, the studies that have taken the advantage of a randomized design, like Angrist et al (2002 and 2006) for Colombia and Kang (2007) for South Korea, show that students that attend private voucher schools experience a significant gain in achievement test scores.[6,7] When vouchers are universal and have been in place for many years, however, more rigorous empirical strategies are difficult to implement. Research on countries like the Netherlands, Denmark and Sweden has focused on the effect of competition on students' outcomes. Barrera-Osorio and Patrinos (2009) review the empirical evidence on the effects of school choice on educational outcomes outside the United States and highlight that in the case of Denmark, studies do not find that competition improves educational achievement in public schools. In contrast in the case of Sweden higher competition has led to improvements in the performance of public school students, and in the Netherlands, competition has been beneficial for all students.

[6] The PACES program in Colombia was aimed to provide low-income students access to secondary private education. Since the program was oversubscribed, lotteries were performed to select voucher students. In South Korea students who finish elementary school are randomly assigned to public or private subsidized middle schools in their residential districts. The aim of the program is to generate homogeneity across schools within districts (Barrera-Osorio and Patrinos, 2009).

[7] Lavy (2009) uses differences in differences and a regression discontinuity design to evaluate school choice among public schools in Tel Aviv, Israel, to find large effects on students' outcomes. The paper also finds positive effects on behavioral outcomes, such as social acclimation in school and school violence.

Evaluations of the Chilean voucher system have mainly focused on the relative effectiveness of private voucher *vis-à-vis* public schools.[8] Lacking randomized designs and panel data, researchers have addressed this question by comparing the achievement of students who attend public and private voucher schools with controls for their observed and –more tentatively– unobserved characteristics.

Given that data on socioeconomic characteristics was only available at the school level until 1997, early studies of relative effectiveness across school sectors used aggregate school averages. In general, all of the studies conclude that the socioeconomic characteristics of families are statistically significant in order to explain student performance in the different types of school. Nonetheless, when the performance of public and private voucher schools is compared, the studies arrive at different conclusions.[9]

Availability of individual-level data since 1997 induced a new generation of studies that includes controls for students' resources and that attempts to account for selection. Most studies using cross-sectional individual-level data found that students attending private voucher schools have higher educational outcomes than those from public schools; the estimated impact of these studies is typically between 0.15 and 0.2 standard deviations, although there is large variability in the estimated impact. Table 1 summarizes these cross-sectional findings.

Mizala and Romaguera (2001) estimate the effects on 10^{th} grade test scores. Using OLS, the paper finds an effect of about 0.2 standard deviations. Sapelli and Vial (2002) also analyze

[8] A second line of research has attempted to identify the effect of school competition on students' achievement. Hsieh and Urquiola (2006) find that private voucher schools "cream skim" students from more advantaged families, while relegating disadvantaged ones to the public sector. On the other hand, Gallego (2002, 2006) and Auguste and Valenzuela (2003) find that greater competition significantly raises test scores. There also exists a related literature that analyzes public and private school enrollment practices in response to vouchers (Elacqua, 2006). Other papers study whether private school networks have an academic advantage over public schools, once student characteristics, selectivity and peer attributes are controlled for (Elacqua et al, 2008). Finally, other papers analyze the socioeconomic stratification of achievement in the Chilean voucher system (Mizala and Torche, 2009).

[9] See McEwan and Carnoy (2000), Mizala and Romaguera (2000), Tokman (2002).

10th grade data. The paper uses variables associated to characteristics of the educational market in the geographical area as instruments and Heckman's two stage correction for selection bias. It finds a treatment on the treated effect of 0.15 standard deviations that is largely heterogeneous, reaching levels of 0.5 standard deviations for low income students that attend private schools that are solely financed by the voucher. Sapelli and Vial (2005) use data on 4th grade tests scores. The paper also uses characteristics of the educational market in the geographical area as instrumental variables and Heckman (1979) methodology. The study finds large effects of private voucher education, up to 60% of one standard deviation. The only paper that finds no advantage of private voucher schools is McEwan (2001) which finds no consistent difference between public and non-religious private voucher schools, and a higher effectiveness of Catholic private voucher schools. McEwan (2001) estimates are also based on Heckman's approach and an identification strategy based on the characteristics of the local educational market.

The research design of these papers hinge on the hypothesis that school choice depends on how densely concentrated are schooling alternatives in the neighborhoods or on average unit prices also defined by geographical area. At the same time, it is assumed that school densities and unit prices do not directly affect students' outcomes. The availability of schooling opportunities in each student's neighborhood might not, however, represent a valid instrument as families choose their area of residence and schools choose their location. Moreover, the geographical area that delimits the educational market for older students might be difficult to define as there are no restrictions on the location of the schools the students can attend. Thus 10th grade students typically travel across the town or city to attend school. In other words, school density variables may reflect unobserved family and community characteristics that influence achievement invalidating the necessary exclusion restrictions.

Finally, Anand et al (2009) use propensity score matching to compare the tests scores of reduced-fee paying, low-income students in private voucher schools to those of similar students in public schools and in free private voucher schools in Chile's Metropolitan Region. The results reveal that students in fee-charging private voucher schools score higher - a test score gain of 0.2 standard deviations-, than students in public schools. The provision of scholarships to identify treatment and control groups has a number of limitations though, in particular, whenever scholarship assignment is based on unobserved ability or if the scholarship itself influences parental and student motivation.

At the heart of this debate is whether the data available and estimation strategies are enough to control for non-random selection of students into different school types and for unobservables that simultaneously impact both, the decision to attend a given school and student's performance. In what follows we describe our identification strategy and discuss its advantages and limitations. In addition, in section VI below we review again the previous literature on Chile in order to identify, in our data set, the sources of differences in results. In particular, we review the role of the identification strategies used in most of the literature and of the lack of information on students' earlier achievement measures.

III. The Chilean School System

In the early 1980s, a military regime undertook sweeping reforms in many Chilean markets. The educational system was not an exception: a decentralization process transferred the administration of public schools to municipal governments, and the establishment of a voucher-type student-based subsidy paved the way for private sector participation as a provider of

publicly financed education.[10] The voucher, which is of the same size for both public and private voucher schools, is paid directly to schools on a per-student basis. The voucher is intended to cover running costs and generate competition between schools to attract and retain students, thus promoting more efficient and better quality education services. The monthly per student subsidy amounted to approximately $61.5 for primary schools and $73.3 for secondary schools in 2006.[11]

Three types of schools were established: public (municipal) schools, financed by the student-based subsidy granted by the State and run by municipalities; private voucher schools, financed by the subsidy and run by the private sector, and private schools that do not receive vouchers, financed by the tuition paid by parents and run by the private sector. There are no restrictions on the location of the schools the students can attend. Except for time constraints and other costs, students can travel to any part of a town or city to attend the school of their choice.

After the reform, a large number of new private schools willing to take the voucher were created. In 1985 there were 2,643 private voucher schools in Chile, a number that grew to almost 5,000 by 2006. As a result, a massive migration from the public sector occurred. By 2006 private voucher schools reached 44.0 percent of the enrollment, at the expense of the public sector, whose enrollment had dropped from 78 percent in 1981 to 47.7 percent in 2006 (Table 2). Enrollment in private non-voucher schools –about 7 percent of total enrollment— was practically unaffected by the system's transformation.[12]

Non-voucher private schools are generally for profit, whereas private voucher schools can be either for profit or non-for-profit. For-profit schools operate like private firms, generating

[10] Before the reform, there existed private-subsidized schools that were for free and funded by the government, but that received a lump-sum subsidy that was substantially smaller than the per-student spending in the public sector (Aedo, 2000).
[11] At an exchange rate equal to 530 Chilean pesos per US dollar.
[12] A small portion of the school population attends schools run by educational corporations linked to business organizations.

returns for their owners. Elacqua (2006) estimates that about 70 percent of private voucher schools are for profits.

There are important differences in the regulation faced by private voucher and public schools. First, private schools that accept vouchers are allowed to select their students. On the contrary, public schools are required to admit all students interested in enrolling, unless they are over-subscribed.

Second, the regulation of teacher contracts also differs, including the capacity to directly hire and dismiss teachers, which private voucher schools have but public schools do not. As a matter of fact, teachers' contracts in public schools are governed by a special legislation -the Teachers' Statute- that involves centralized collective-bargaining as well as restrictions on teacher dismissal. Private schools, both subsidized and non-subsidized by the voucher, operate as private firms, and their teachers come under the same Labor Code of other private sector workers in the country.

Finally, there are differences in the ability to raise alternative sources of financing. Initially schools that accept vouchers were not allowed to charge tuition to supplement the subsidy, but this restriction was eased in 1993. As of today, about 50 percent of private voucher schools charge tuition. Public schools are allowed to charge fees only at the secondary level, although in practice few of them do. In addition, public schools can receive subsidies from the municipalities if the voucher is not enough to cover the entire budget; in fact, a number of municipalities transfer a significant amount of money to the schools. For this reason, authors such as Sapelli and Vial (2002) claim that some public schools face a soft budget constraint. Still, many municipalities transfer no funds at all.

The K-12 Chilean school system is divided into primary education (from kindergarten to 8^{th} grade) and secondary education (from 9^{th} to 12^{th} grade). Since 2003 both primary and

secondary level education are mandatory. Almost all private non-voucher schools offer primary and secondary education. However, this is not the case for public and private voucher schools: at the national level 76.4 percent of public schools and 52.0 percent of private voucher schools provide education up to 8th grade only; these rates are equal to 74.5 percent and 48.8 percent in urban areas, respectively.[13] In terms of enrollment, in the year 2004, the fraction of students from public schools who had to switch schools at the end of the 8th grade reached 74% both at the national level and in urban areas.

The fact that many students have to switch schools at the end of 8th grade to continue their secondary education is essential to our identification strategy further described below. In order to gain a better understanding of the reasons why many schools choose not to offer secondary grades, we conducted an interview with the Executive Director of CONACEP (*Corporación Nacional de Colegios Particulares de Chile*), an association of private voucher schools established in 1983 that represents over 800 schools and that enrolls more than 600 thousand students --about a third of private voucher school enrollment. We also conducted interviews with other providers of private voucher education. The interviews revealed that cost concerns are at the heart of the decision of whether to provide primary education only. The provision of secondary education is more expensive given that, due to regulatory requirements, it is necessary to hire specialized teachers for every subject area, whereas in primary education the same generalist teacher covers all subjects in the curriculum (except for arts and physical education). Thus, having secondary grades requires a larger scale, i.e., a larger number of classrooms of the same grade within the school in order to have each subject teacher teaching in different

[13] In addition, according to official data from the Ministry of Education, in 2004 in the Chilean urban sector there were 4,409 schools offering primary education (2,886 offered primary education only), and 2,654 schools offering secondary education (710 offered secondary education only).

sections.[14] If this were not the case, teachers would be hired under part time contracts which increase their salary per hour. For this same reason secondary education management is more complex than primary education. Another explanation why schools do not offer secondary education relies on the availability of land and infrastructure.

Table 2 provides some detail on the characteristics of private voucher and public schools for year 2006. First, they represent similar shares of total enrollment. In addition, a small fraction of schools provide secondary education. The differences in enrollment levels and shares correlate with the observed differences in class sizes across school types, with teachers at private voucher schools attending a larger number of students. Nevertheless, students enrolled in public schools belong to lower income households, receive fewer financial resources at school, and relate to peers of lower socio-economic backgrounds. These statistics show that there are relevant observable differences in the educational experiences of students attending different types of schools.

IV. Identification Strategy and Estimation Methodologies

The main methodological challenge we face is dealing with selection bias. That is, we need to recognize that the observed assignment into different schools is not random. In order to account for this estimation difficulty, we propose an identification strategy based on a common phenomenon that characterizes the Chilean educational market: the fact that most schools in Chile that are financed by the voucher provide either primary or secondary education only. In fact, in 2004, 56.4 percent of the students enrolled in 8th grade attended public or private voucher

[14] As a matter of fact, the number of classrooms per school increases threefold in the transition from primary to secondary education, with almost no changes in classroom size. That is, in primary education, schools have on average about 1.6 sections in each grade and almost 33 students per section. Meanwhile, secondary schools have on average 4.5 sections in each grade and about 36 students per section.

schools that did not provide secondary level education and 54.5 percent of those living in urban areas. These students had then to choose another school to continue studying.

Until 2003, secondary schooling was not compulsory: students had to complete their education only up to 8th grade. This law on compulsory schooling thus reduced the size of the secondary education market relative to the size of the primary education market. Also, cost concerns led schools to narrow down to attending students in primary education only. The official secondary education curriculum requires that students are taught by teachers specialized in each subject. In contrast, for most of primary level education, a single teacher serves as the instructor for all subjects.

Our estimation strategy limits the analysis to the subsample of students that attended 8th grade in a public school that did not provide secondary education, and thus had to switch to another school in order to continue their education. Our treatment group is then composed of the students who moved to a private voucher school whereas our control group includes those who moved to another public school. Limiting the analysis to students who attended a public school in 8th grade –instead of any school that did not provide secondary education-- improves the similarity between the treatment and control groups. In section VI we also provide the estimation results of limiting the analysis to students that were forced to switch schools but had attended a private voucher school in 8th grade, using the same setup for building treatment and control groups.[15]

Another reason to limit the analysis to students attending public schools in 2004 is that, although 2004 test scores allow us to control for student ability, these test results depend on the type of school the child attended up to 8th grade. In other words, under the hypothesis that school

[15] The strategy of separating the samples depending on the type of school attended in 8th grade and estimating the effects on both subsamples is an alternative approach to including all students that had to switch schools and controlling for school fixed effects.

type matters, the correction for 8th grade scores might not be enough to account for unobservable ability. So, limiting the analysis to this particular group of students allows for controlling for 8th grade test scores that reflect pre-treatment achievement of students in control and treatment groups that had previously attended the same type of school.

Our identification strategy is based on a number of assumptions, and thus has advantages and shortcomings. The most important advantage is that structural switches are expected and thus are not correlated with temporary unobserved shocks, taking care of potential mean reversion in test scores. That is, our results are based on a sample with potentially less selection on unobservables.

Our strategy has limitations, though. In particular, some issues concern the external validity of the results. Because the effects are identified from structural school switches, it is an open question as to the extent that the estimates are relevant to students that attend schools that provide both, primary and secondary education. Moreover, our identification strategy leaves out from the analysis the students attending elite public schools, also known as "emblematic schools", which select students but enroll them earlier, in 7th grade. If families who expect to receive higher benefits from attending these schools are more likely to enroll, our estimated effect may overstate the expected benefit to the average student. So the evaluation provides a consistent estimate of the benefit of the population that switches due to structural reasons at the end of 8th grade only.

Another potential concern relates to the fact that some children might choose not to attend secondary education or to drop out. According to the Ministry of Education, a small fraction of students enrolled in secondary education in 2005 --4.8 percent-- dropped out in that same year. It is worth emphasizing that secondary schooling became mandatory starting in 2003, forcing all students to continue their education once they earned the primary school certificate.

Finally, although our interviews reveal that the main reason why some schools choose not to offer secondary grades are related to cost concerns induced by the regulation, it is still possible that private voucher schools choose to locate where public secondary schools are not a good option. That is, the decision to offer secondary schooling might be endogenous to public school quality. In the interviews with private voucher education providers we also inquired about the main variables they consider at the moment of establishing a new private voucher schools in a neighborhood. The interviews revealed that the primary reasons considered are the demographic characteristics of the area (population in school age), economic profile of the families, communities' interests, the degree of bureaucracy of the local government, and the characteristics of existing schools. As a check, we estimated three models related to the supply of private voucher schools in any given municipality. The first is a probit model for the probability that at least one new private voucher school opened between years 2003 and 2006 in the local area. The second one is an OLS model for the number of new private voucher schools that opened in that same period. The final model is an OLS estimate of the number of private voucher schools operating in 2006 in the local area. The explanatory variables we chose are the number of school-aged children in the municipal area, the average education of mothers and fathers, the average household income, and the average public school SIMCE. The quality of public school supply measured by SIMCE tests scores did not turn out to have a significant effect in any of our models.[16]

In what follows, we quickly review the estimators we use, their assumptions and properties. We use two types of econometric techniques: propensity score based estimators and changes-in-changes estimators.

[16] The estimation results are available upon request.

a. Propensity score based methodologies

We use two propensity score based methodologies to identify the average treatment effect (ATE): propensity score weighting and the combination of the latter with regression adjustment (double-robust). The approach relies on the usual assumptions used in matching: unconfoundedness and overlap (Imbens and Wooldridge, 2009).

Assumption 1. Unconfoundedness

$$D_i \perp (Y_{i0}, Y_{i1}) \mid X_i$$

where X_i represents observable variables.

Assumption 2. Overlap

$$0 < pr(D_i = 1 \mid X_i = x) < 1, \text{ for all } x.$$

The first assumption, also known as conditional independence, states that treatment assignment is exogenous given the covariates or the propensity score.[17] The assumption means that participation in the treatment program does not depend on the outcome after controlling for differences in observed variables, such as socioeconomic status and performance in pre-treatment tests. It is a very controversial assumption (Imbens and Wooldridge, 2009), but still very popular, especially since Dehejia and Wahba (2002) showed good results in comparing experimental data and matching results in the evaluation of a training program. The second assumption states that individuals should have positive probabilities of being observed in both treatment and control groups. This is less controversial and is likely to be accomplished by the construction of a common support.

The most popular of propensity score methodologies is propensity score matching. It consists of estimating the effect using as the counterfactual the observation with the closest

[17] Rosenbaum and Rubin (1983) showed the equivalence in the information contained in the vector of covariates and the propensity score in the unconfoundedness assumption.

propensity score value, allowing for the construction of control and treatment groups that are very similar in the probability of being treated. However, it is not clear how to estimate the standard errors in a way that takes into account clustering.[18] In the case analyzed in this paper, it is very likely that there is clustering at the school level, so we would like to estimate correctly the variance of treatment effects. As far as we know, there is no method to correct for clustered errors in the context of propensity score matching.[19] A popular option for taking into account clustered errors is bootstrapping at the cluster level; however, bootstrapping is not valid with matching.[20] Therefore, we focus on other propensity score based estimators that do not have these problems.[21]

Another propensity score based estimator of the average treatment effect is propensity score weighting. This method weights the observations using the propensity score and the treatment status. The idea is to balance the sample between treated and nontreated individuals based on the probability of treatment (Imbens and Wooldridge, 2009). Specifically, we use the inverse probability weighting (IPW) estimator proposed by Hirano, Imbens and Ridder (2003). Over this estimator we perform bootstrapping at the cluster level in order to estimate standard errors and confidence intervals. The bootstrapping procedure is performed before the construction of the propensity score and of the common support, taking into account potential errors in these procedures.[22]

The other propensity score based method considered in this paper allows us to directly account for the correlation between covariates and outcomes. In particular, we expect the pre-treatment test score to have a very strong and direct effect on the post-treatment test score. The

[18] This concern also relates to modeling the selection process based on Heckman (1979) type methods.
[19] Abadie and Imbens (2002) assume a diagonal conditional variance matrix for their matching variance estimation.
[20] Abadie and Imbens (2008) state that with matching the standard conditions of bootstrapping are not satisfied, and that the variance estimated using bootstrap diverges from the true variance.
[21] Despite the described problems, we estimated one-to-one propensity score matching to find a very similar ATE. Results are available upon request.
[22] Imbens (2004) argues that for regression and propensity score methods -- excluding propensity score matching-- bootstrapping is likely to lead to valid standard errors and confidence intervals.

method is the propensity score weighted regression, introduced by Robins and Rotnizky (1995), Robins et al (1995), and Robins and Ritov (1997).[23] This method also has a double-robustness feature: it provides consistent estimators if either the probability of treatment or the outcome regressions are incorrectly specified, implying some safeguard against model misspecification. Our implementation of the double-robust (DR) estimator follows the steps suggested by Emsley et al. (2008). To estimate the standard errors we again use bootstrapping at the school level, taking into account that errors might be correlated within schools.

The unconfoundedness assumption is a key assumption underlying these methods, but it is not directly testable. However, Imbens (2004) and Imbens and Wooldridge (2009) describe how to assess the plausibility of the assumption. A possibility consists on estimating the impact of the treatment on a variable correlated with the outcome of interest but that is not affected by the treatment. The authors suggest that a pre-treatment outcome closely related to the post-treatment outcome could play this role. The ideal variable is the pre-treatment value of the variable of interest. Fortunately, the 8th grade test score fulfills this role within our subsample of students that attended a public school in 8th grade. If the treatment had not been applied and the unconfoundedness assumption is valid, then propensity score methods should estimate zero impact on 8th grade outcomes. Otherwise, it would indicate that there are unobservables that affect the outcome that would trick the researcher into stating that there is a treatment effect when there is not.

In our case, we define the score at 8th grade (Y_{8th}) as the dependent variable and as treatment the private voucher school status at 10th grade (D_{10th}). We use as covariates the set of variables of 8th grade (X_{8th}) and estimate this "false experiment" using the same procedures (PSW

[23] Further details on this method can be found in Imbens (2004) and Imbens and Wooldridge (2009). A generalization of this method is described in Wooldridge (2007).

and double-robust). In this manner, we verify whether $D_{10th} \perp (Y_{8th,0}, Y_{8th,1}) \mid X_{8th}$ holds. Nevertheless, it is worth emphasizing that finding no effect does not guarantee that $D_{10th} \perp (Y_{10th,0}, Y_{10th,1}) \mid X_{8th}$ is true, which is our main assumption, but it indicates that it is very likely the case.

b. Changes-in-Changes Estimator (CIC)

The alternative methodology implemented in this paper is the changes-in-changes estimator of the average effect of the treatment introduced by Athey and Imbens (2006). This estimator is a generalization of the difference-in-difference estimator, which needs fewer assumptions in order to obtain consistent estimations.

An important advantage of this methodology is that it allows for differences in the distributions of unobservables across treatment and control groups. Moreover, the estimator allows for the estimation of the treatment on any quantile of the unobservables distribution.

The main idea behind the estimator is that the distribution of unobservables of each group can be inferred from the pre-treatment outcomes. Once the distributions of unobservables of both groups are known, one can estimate how much of the observed effect is due to pre-treatment unobservables distribution differences.

More formally, the method compares group quantiles that had the same unobservable effects in the period before the treatment, assuming that quantiles that have the same rank of unobservables $U_i=u$ at time $t=0$, will have the same rank at $t=1$. This assumption allows the distribution of unobservables to differ across groups, but not over time. More specifically,

according to Athey and Imbens (2006), the assumptions required to identify the treatment effect in a data set with the same individuals across time are the following[24]:

Assumption 1. Model

The outcome of an individual in the absence of intervention satisfies the relation $Y^N=h(U,T)$.

Assumption 2. Strict Monotonicity

The production function h(u,t), where $h: \mathbb{U} \times \{0, 1\} \rightarrow \mathbb{R}$, *is strictly increasing in u for* $d \in \{0, 1\}$.

Assumption 3. Rank Similarity within Groups

$U_{i0} \mid D_i \overset{d}{\sim} U_{i1} \mid D_i$.

Assumption 4. Support

$\mathbb{U}_1 \subseteq \mathbb{U}_0$

Assumption 3 requires that the rank within groups does not vary in arbitrary ways. It allows for a non-observable at time t of the form

$$U_{it} = e_i + v_{it}$$

with e_i a time invariant individual specific unobserved component (fixed effect), and v_{it} an idiosyncratic error term with the same distribution in both periods. Assumption 4 is made in order to identify the treatment on the treated effect. However, in order to obtain the ATE, we also need $\mathbb{U}_1 \supseteq \mathbb{U}_0$. Both are accomplished by the construction of a common support for non-observables.

Athey and Imbens (2006) also propose a CIC estimator with controls for covariates. The procedure is done in two stages. After defining the vector of group dummy variables (indicating treated and controls) and the covariates X, the first step consists of estimating through OLS (without a constant) the equation:

[24] Some of these assumptions are also discussed, in an educational context, in Angrist et al (2006).

$$Y_{it} = D_i'\delta_t + X_{it}'\beta_t + \varepsilon_{it}$$

and of constructing the residuals in the following way:

$$\hat{Y}_i = Y_i - X_i'\hat{\beta} = D_i'\hat{\delta} + \hat{\varepsilon}_i$$

Then, the second step consists of applying the CIC estimator to the residuals obtained in the last equation, which means that the estimation is based upon the part of the variation in the dependent variable that observables do not explain. If the assumptions are correct, this provides us with a consistent and asymptotically normal estimator. Again, we use bootstrapping at the school level to estimate clustered errors.

Finally, it is worth emphasizing that the CIC approach is built in order to accomplish the validity test of the unconfoundedness assumption described above, as it compares the quantiles that exactly fulfill $D_{10th} \perp (Y_{8th,0}, Y_{8th,1}) \mid X_{8th}$, since they have the exact same unobservable effect in 8th grade.

V. Data

The empirical data used in this study come primarily from two sources. The first source is a standardized test called the SIMCE (*Sistema de Medición de la Calidad de la Educación*, Education's Quality Measurement System), which is administered annually throughout Chile to a specified grade level that rotates every year between the 8th and 10th grades, and starting in 2005, every year to 4th grades. This rotation implies that, except for the case of the data used in this paper, the SIMCE tests do not track students over time.

This paper uses the 2006 SIMCE data which was administered to 10th graders. Because the 2004 SIMCE test was administered to 8th graders, for the first time we also have data on student previous performance, allowing us to better control for previously unobserved

characteristics, and to form new treatment and control groups. Specifically, we take advantage of the fact that the majority of Chilean students mandatorily changes school at the end of the 8th grade, the last year of primary school, given the way that the educational system is organized. However, it is important to note that our data set cannot be analyzed as a panel, as SIMCE scores are not comparable across tests.[25]

One limitation of working with data on older students --instead of 4th graders, for instance-- is that ability based selection is easier among students in higher grades, since at this point in life, schools have more information on the students' academic abilities as they can track their records.

The second data source is a questionnaire that is answered by the parents of students that participated in the SIMCE in 2004 and 2006. This questionnaire provides information on the socio-economic characteristics of each student, such as their family income and parents' education. Although it is not mandatory for parents to complete the questionnaire, there is a high response rate for most of the key variables used in this analysis.[26]

Once these data sources were combined into a comprehensive database and after excluding those with incomplete information, several modifications were made to target the population that we are interested in studying. First, according to our identification strategy we only consider the subsample of students who were forced to switch schools after they finished their primary education. Second, we exclude students in private non-voucher schools from the analysis because these schools typically serve only the most elite families in Chile. Private non-voucher schools are not a realistic educational option for the average student in Chile because the typical fee charged at private non-voucher schools is over three times the per-student voucher

[25] In Appendix 2 we describe our matching procedure across data sets.
[26] To perform our analysis, we only use the observations that have complete information; no data is imputed for missing observations.

paid by the state and much higher than the cost of attending a fee-charging private voucher school. Finally, we only consider students that attend urban schools, since in rural areas students have limited school choice as a result of geographic and other constraints.[27]

In addition, about 12 percent of students that had to switch school between the 8th and 9th grades declare to have switched again between the 9th and 10th grades. Unfortunately, we do not know which school type they attended in 9th grade. Given this data limitation, we have excluded these students from the analysis.[28]

Modifications were also made to some of the variables in the database in order to make them compatible with our analysis. For example, on the parental questionnaire, parents reported the highest level of education that they had attended. These levels were converted into the corresponding number of years they had been in formal education: the maximum time a parent could spend in basic education is 8 years, high school is 12 years, professional school or technical institute is 16 years, college is 17 years, a master's degree is 19 years, and a doctoral degree is 22 years.

Appendix Table A1 provides a complete list of variables used in this paper, along with their definition and data source. Table 3 summarizes the basic statistics for the sample used in this study, i.e., students that completed 8th grade in a public school and had to switch schools to pursue secondary education.[29] On average, without controlling for student or school-level characteristics, those who switched to private voucher schools tend to score better than students who moved to public schools. They also have better resources: parents with higher education

[27] Currently, 63 rural municipalities out of a total of 345 municipalities do not have private voucher schools. However they are ubiquitous in urban areas.

[28] Since this sample decision might induce a sample selection bias as the choice to switch again might be correlated with achievement, we also estimated our models including these students to find similar estimated effects. Results are available from the authors upon request.

[29] Tables A2 and A3 in the Appendix present the basic statistics for the sample of students who completed 8th grade in a private voucher school and had to switch schools, and for all students who had to switch school after 8th grade, respectively.

levels, higher family income, and parents with higher expectations about the education level their sons and daughters will attain. Moreover, a lower percentage of students who moved to private vouchers schools had repeated a grade and a higher percentage had preschool education than those who switched to public schools.

Table 3 also provides information on the characteristics of all children who took the language or math tests in both years. In addition to those characterized in the first two columns, the last column includes children attending private voucher schools in 2004, elite public schools in 2006 and all those who switched voluntarily from a public school at the end of 2004. Parental education in the full sample is higher, as also is income. In addition, parents have better expectations with respect to college education in the full sample. Moreover, students performed better on average in all four tests. The inclusion of students attending elite public schools and private voucher schools drive these differences. At the same time, it is worth emphasizing that the sample we investigate is not only better from a methodological standpoint, but it is also more interesting from a public policy point of view.

VI. Results

We estimate the effect of private voucher education on student performance using alternative estimation methods. As a comparison, we start by estimating the effect of private voucher education using the methodological approach used in most of the previous literature on Chile's educational market. We then analyze the results using our identification strategy and both, the propensity score methods and the CIC approach. The estimators presented in this section are based on different assumptions on the manner students select themselves into school types. Propensity score-type estimators assume selection on observables, whereas the CIC

approach allows for observables and unobservables but assumes that the within group distribution of unobservables remains constant over time.

a. A comparison with the previous literature

The methodological approach we propose based upon structural school switches, yields an estimated effect of private voucher education that is much smaller than the one found in the previous literature on Chile. Before we present and discuss our results, and in order to assess whether these differences are related to the sample chosen, the level of schooling, the time period or the methodology, we estimate in our data set the effect of private voucher education using the identification approach used in most of these previous papers.

Table 4 displays the results of this exercise. In the top panel we use the full 2006 sample and analyze it as a cross section using the Heckman (1979) two step method for dealing with selection bias. Our instruments relate to characteristics of the local educational market, i.e., the number of public and of private voucher schools per square kilometer in the student's neighborhood, and the fraction of the enrollment on the neighborhood that attends a private voucher school.[30] All these variables turn out statistically significant at conventional levels, except for the public supply of schools. The estimated effect of private voucher education amounts to 7.0 points in the language test and 8.4 points in the math test, both significant at a 1%. Using the relevant sample standard deviations, these effects represent a test score gain of 13% and 14% of one standard deviation, respectively.

In the second panel we repeat this exercise, but now include as a regressor the 8^{th} grade test score. We now find that the estimated effect of private voucher education drops sharply and is no longer statistically significant at conventional levels.

[30] These are the instruments used by Sapelli and Vial (2002).

These results suggest that the differences in our results relative to those of the previous literature have to do with the methodological approach. Our sample replicates the previous results on Chile, but the identification strategy based upon the characteristics of the geographical market is not robust to controlling for pretreatment test scores. In addition, the correction for previous test scores should not be enough to account for selection bias whenever the hypothesis that the school type does matter for academic performance is true. In other words, the bottom panel results may not provide consistent estimates of the treatment effect. For this reason, we now turn to the results based upon our proposed identification strategy.

b. Propensity score based methods

We first estimate the probability of attending a private voucher school within our subsample of students undergoing a structural switch; i.e., the propensity score. In the model we include two types of variables. First, to control for the decision of schools to provide secondary education and for the availability of private voucher education in the local area, we include mean socio-demographic variables that characterize the municipality and the relative supply of private voucher schools in the students' neighborhood of residence. Secondly, we include student and family level characteristics and pre-treatment SIMCE test scores, all measured in 2004, as controls. The results are displayed in Table 5. They show that income, maternal education and parental expectations on attending post secondary education are correlated with a higher likelihood of attending private voucher schools. On the contrary, students who have repeated a grade are less likely to attend private voucher schools. Also, students are more likely to enroll in private voucher schools whenever there are more private voucher schools in the neighborhood. Finally, the estimation results show no significant statistical correlation between the probability of treatment and the students' past score in the standardized tests.

Figures 1a and 1b show the densities of the estimated propensity scores by both school type and subject test. These densities display a very similar mode; i.e., the difference in the mode of the treatment probability density is only 5 percentage points. The densities also display a common support.[31]

Table 6 presents ATE estimates using propensity scores based methods and validity tests. Using propensity score weighting we obtain positive effects that are statistically significant at a 1 percent for language and at 5 percent for math tests, respectively. Similar results are obtained using propensity score weighted regressions (double robust), with statistical significance ranging between 1 percent and 10 percent. The estimated effect of private voucher education amounts to 2.36 to 3.03 points in the standardized achievement test, i.e., 4 to 6 percent of one standard deviation in test scores, an effect much smaller than the one found in the previous literature on Chile.

Given the extent of the overlap in our propensity scores and as a complement of our results, the Table also reports the OLS estimates. Although a bit smaller than our double-robust and PSW estimated effects, the results are quite similar and also statistically significant.

We now proceed to estimate the validity tests proposed by Imbens and Wooldridge (2009). We first estimate the propensity score without controlling for the pre-treatment test score. The results of the probit estimation are shown in Table A4 in the appendix. We then use the estimated propensity score to assess the effect of the treatment on 8th grade test scores, using the same weighting techniques as before (IPW, double-robust and OLS). The results are presented in

[31] We constructed the common support by dropping the observations of the control (treatment) group that had propensity scores below (above) the minimum (maximum) propensity score of the treatment (control) group. Only 7 observations were dropped according to this criterion. In addition, the weighting estimators might give too much weight to some observations. The individual with the highest weight represented only 0.05 percent of the sample, so no additional observations were dropped when estimating the IPW. However, a cutoff of a 5 percent weight was used in the bootstrap procedure. That is, all resampling observations with weights above 5 percent of the iteration samples were dropped from the bootstrap estimations.

the right hand side panel of Table 6. These tests find no effect of the 10th grade treatment on 8th grade scores. These results suggest that there are no relevant unobservable variables in the pre-treatment period and thus unconfoundedness is accomplished in 8th grade. These results in turn imply that it is highly likely that our strategy accounts for unobservables in the post-treatment period.

c. CIC estimators

Table 7 presents the results obtained using CIC estimation methods without and with controls for covariates, respectively, as proposed by Athey and Imbens (2006).[32] The estimated effect of private voucher education is slightly lower than the one obtained using propensity scores based econometric technique, ranging from 1.21 to 1.87 points. However, the estimated effects turn out to be not statistically significant in all cases.

The CIC methodology allows us also to infer the effect of the treatment on the entire distribution of students. Figures 2a and 2b present the results after correcting for covariates. Figure 2a shows that all students above the 34th percentile of the distribution of math test scores experience positive effects of attending private voucher education. For language test scores, the positive effects are evenly distributed along the distribution of results. That is, according to the CIC estimates, not only the average student is benefited by private voucher school education but also most students along the distribution of outcomes. However, none of these estimates are statistically significant.

Summing up, if our identification strategy based on the analysis of the subsample of students who were forced to switch schools is valid, then the estimators suggest that there are

[32] The covariates used to estimate CIC with controls are parental education, family income dummies, number of household members, student's gender, whether the child repeated a grade, whether the child attended preschool, and the number of books at home. We also include school aged population and mean income in the municipality.

small differences in the performance of students attending private voucher and public schools that tend to favor the former.

VII. Sensitivity analyses and extensions

In this section we provide a number of extensions and sensitivity analyses in order to gauge the robustness of our identification strategy. We start by estimating the effect of private voucher education limiting our sample to those students that attended private voucher education in 8th grade and that had to switch schools to continue their education. In our second exercise we estimate our model including those students that voluntarily switched schools. In the third analysis we reestimate our main results using Hirano and Imbens (2004) method for choosing covariates. Finally, we provide an estimate of the relative importance of selection on unobservables, following the approach of Altonji et al. (2005).

a. Structural switches from private subsidized schools

As an extension of our identification strategy we re-estimate the effect of private voucher education now limiting the subsample to those students who attended 8th grade in a private voucher school that provided no secondary education, and thus had to switch to another school in order to continue their education. Then our new control group is composed of the students who moved to a public school whereas our treatment group includes those who moved to another private voucher school.

Table 8 presents the results for propensity score based methods and the CIC approach. In the case of language SIMCE test scores, the estimated effect is in most cases negative, ranging

from -1.25 to 0.61 points. In the case of math SIMCE test scores, the effect is positive ranging from 2.03 to 3.63 points in the test. However, none of the estimated results are statistically significant, except for the math effect using OLS.[33]

The right hand panel of Table 8 presents the validity tests proposed by Imbens and Wooldridge (2009). The statistics suggest a significant effect of the treatment on 8th grade scores, with a high likelihood that the transition from private voucher to public schools is an endogenous phenomenon. In other words, the unconfoundedness assumption is unlikely to hold in this setup. This means that students who moved from a private voucher schools to a public school may be different in unobservable ways from those who moved to another private voucher school. For instance, one possible reason for this result is that the students who finished primary school in a private voucher school, and switched to a public school, opted mainly for a high achievement public school. These schools have excess demand and thus can select high-ability students.

b. Voluntary switches

Our focus on structural switches allows us to circumvent the possibility of mean-reversion in the data. We indirectly analyze the extent of this problem by reestimating our model for the subsample of students who voluntarily switched from a public school at the end of the 8th grade. Those who switched despite their schools provided secondary education may be experiencing a negative shock to test scores. If there is mean reversion, then we might find a larger positive effect of private voucher education. Alternatively, voluntary switchers may be experiencing a positive test score shock that allows them to gain admission to a private voucher school. In this case, we might find a smaller or even a negative effect of the treatment.

[33] The descriptive statistics of these students and the model that estimates the probability of attending a private voucher school are presented in Tables A2 and A5 of the Appendix, respectively.

Table 9 provides our results using all estimation methods. The Table also provides the validity tests. OLS and propensity score based estimated effects are smaller than our baseline estimates and are not statistically significant. These results suggest that voluntary switchers may have experienced a positive shock in academic performance by the end of primary education. The tests suggested by Imbens and Wooldridge (2009) validate the hypothesis that these switches are not exogenous and that the results are subject to selection bias. Alternatively, these negative coefficients in the validity tests jointly with the zero estimated effect might be interpreted as evidence of a negative shock in 8^{th} grade that reverses to the mean in 10^{th} grade.

Contrary to these results, the changes-in-changes estimators are larger than those obtained using our sample of structural switchers. Again, these do not turn out to be statistically significant.

c. Alternative controls

The SIMCE parental questionnaire data set contains a large number of questions on household characteristics. Hirano and Imbens (2001) use the double-robust estimator that combines weighting with the propensity score and regression. They also have a large number of potential covariates in their data set. They suggest a decision rule to decide on which controls to include based on the t-statistic. In this exercise we follow their approach. Table 10 shows that our results are robust to the choice of controls based on this decision rule.[34]

d. Relative importance of selection on unobservables

[34] As an alternative check, we also use a different set of controls in all propensity score methodologies, obtaining similar results. This alternative specification also includes school characteristics (school size and per pupil resources), the reasons parents listed for choosing the school the students attended in 8^{th} grade and controls for the motivation and involvement of the parents in the education of their children (participation in PTA meetings, and number of times they talk to the teachers). Results are available upon request.

Our final extension estimates the amount of selection on unobservables relative to selection on observables required to attribute the full estimated private voucher effect to selection bias based upon the method suggested by Altonji et al. (2005). In general terms, the approach is based on the assumption that the part of an outcome --tests scores in our application-- that is related to observables has the same relationship with the endogenous treatment --private voucher school attendance-- as the part related to unobservables.

Altonji et al. (2005) apply their method to the effect of Catholic school education in the United States. They find that in order to attribute the full effect of Catholic school attendance to selection bias, the selection on unobservables would need to be 3.55 times stronger than the selection on observables when the outcome is high school graduation, and 1.43 times stronger when the outcome is enrollment in a 4-year college. The ratio falls to 0.08 and 1.04 when 12^{th} grade reading and math tests scores are considered.

Our estimates for this ratio are quite similar to those of Altonji et al. (2005). For the language SIMCE, the implied ratio is 0.617, whereas for the math SIMCE the implied ratio is 0.744. These ratios jointly with the point estimates provided above suggest that the effects of private voucher education on tests scores are positive but small.

VIII. Discussion and Concluding Remarks

In this paper we revisit the school choice debate using new data on Chilean students and a new identification strategy. Specifically, we start by replicating the methodology used by the previous literature on Chile, taking advantage of the availability of past test scores to control for prior achievement. Using this method, we find no effect of private education on student performance.

Then we examine the differences in the 10^{th} grade standardized test performance of students who moved from a public to a private voucher school (treatment group) with the 10^{th} grade standardized test performance of students that stayed in the public school system (control group).

With these groups at hand, we estimate test score differences using propensity score techniques and the changes-in-changes approach developed by Athey and Imbens (2006). Propensity score techniques lead to positive and many times statistically significant effects for both math and language tests. Moreover, validity tests for the unconfoundedness assumption are passed. In addition, the changes-in-changes approach suggests that these positive effects apply to the full distribution of results in the case of language scores and to the upper part of the distribution in the case of math scores, i.e., they are not concentrated within a particular group of students. However, these latter estimates are not statistically significant. In sum, although based upon different theoretical assumptions about the underlying behavior of the data, propensity score type and changes-in-changes estimators yield similar results. The Heckman correction method using past test scores also leads to similar results.

The statistical significance of our results contrasts with their economic relevance as the private voucher education effect we find is never larger than 6 percent of one standard deviation. Different hypothesis might explain why the estimated effect is small. One possibility is that competition does put pressure on both types of schools, leading them to achieve similar academic results.

Alternatively, private voucher schools might not be motivated enough to provide better academic results. In fact, the current Chilean school system regulation is lax, allowing schools to survive even if the academic performance of their students is poor. To continue participating in the voucher system, private schools have to meet minimal standards, and there is no supervision

on how public resources are spent. Consequently, very few schools close in Chile. As a matter of fact, an educational system reform recently passed into law a regulation that will put higher academic pressure on schools financed by public resources.

A complementary reason might be that private and public schools do compete, but in dimensions other than academic achievement. A possibility is that parents care about peer socioeconomic makeup in itself, regardless of achievement (Elacqua et al., 2006, Hsieh and Urquiola, 2006). Alternatively, parents do care about school achievement and are able to assess average school performance; but given the strong correlation between socioeconomic status and students' performance, they cannot assess the value added by the school (Mizala et al., 2007). This is consistent with the scant evidence of students switching to more effective schools (Mizala and Urquiola, 2008).

Another hypothesis is that the treatment we measure does not allow for enough exposure of students to private voucher education. Recall that the evaluation is done after only two years of private education. Thus, our results could be reinterpreted as a gain of 4 to 6 percent of one standard deviation in a two year period, which is in line with the estimated 1.5 to 2.3 percent of a standard deviation gain per year in math test scores that Rouse (1998) reported in the case of students selected for the Milwaukee Parental Choice Program.

In addition, the results are representative of students who have attended public schools up to the 8^{th} grade – at 14 years of age on average. Perhaps, at this stage it is too late to generate significant changes in the academic achievement of students.

Furthermore, school switching might be disruptive, at least in the short run, since students need to adapt to a new environment (Hanushek et al, 2004). Possibly, switching to a private school does enhance students' achievement, but the assimilation takes time. An interesting question is whether these effects vary by school type.

Finally and as a consequence of our non-experimental approach, our results may be explained by potential biases introduced by our identification strategy if it is not fully able to control for non-random selection of students into different school types and for unobservables that affect school choice and performance at the same time.

Although this paper has dealt with the case of Chile, we believe the methods and identification strategy are a useful approach to analyze a wide variety of school choice experiences performed in other countries.

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Table 1
Cross-Sectional Literature on the Relative Effectiveness of Chile's Private Voucher Schools

Author	Goal	Database/Grade	Method	Identification Strategy/Instrument	Result (fraction of one st. dev.)[a] Math	Language	Other
Mizala and Romaguera (2001)	Estimate educational production functions for Chile using student level data	SIMCE 1998 10th grade	OLS		0.235 (0.04)	0.247 (0.04)	
McEwan (2001)[b]	Relative effectiveness of Catholic private voucher-schools	SIMCE 1997 8th grade	Heckman 2-step correction	Number of schools of each type per square km in the municipality (linear and squared terms)	**Relative to Public DAEM:** Catholic Voucher: -0.109 (0.191); Protestant voucher: 0.503 (0.548); Non-religious voucher: -0.262 (0.149). **Relative to Public Corporation**: Catholic Voucher: 0.103 (0.185); Protestant voucher: 0.882 (0.590); Non-religious voucher: -0.035 (0.121).	**Relative to Public DAEM:** Catholic Voucher: -0.055 (0.136); Protestant voucher: 1.309 (0.614); Non-religious voucher: -0.122 (0.108). **Relative to Public Corporation**: Catholic Voucher: 0.145 (0.133); Protestant voucher: 1.652 (0.673); Non-religious voucher: 0.098 (0.090).	Distinguishes 6 school categories:public DAEM, public corporation, catholic voucher, protestant voucher, non-religiuos voucher, private non-voucher.
Sapelli and Vial (2002)	Estimate average treatment effect (ATE) and treated on the treated (TT) for families choosing private voucher education	SIMCE 1998 10th grade	Heckman 2-step correction	Number of public and private voucher schools per square kilometer, and the fraction of private voucher enrollment in total enrollment (private voucher plus public), both by geographical area (province or electoral district)		ATE: 0.043 (0.01); TT: 0.145 (0.03)	Authors also provide estimates by income group and by the size of per student public transfers above the voucher. Results are similar across income groups. **TT of up to 0.5 sd are found for schools funded by the voucher only.**
Sapelli and Vial (2005)	Estimate treatment on the treated effects of private voucher education	SIMCE 2002 4th grade	Heckman 2-step correction	Ratio of the average fee charged by schools in 2000 to the SIMCE test score in 1999 by geographical area and by school type	TT: 0.592 (0.106)	TT:0.598 (0.113)	Includes only those schools that are mainly financed by the voucher.
Anand, Mizala and Repetto (2009)[c]	Estimate the effect of private voucher education on low income children	SIMCE 2002 4th grade	Propensity Score Matching	Provision of scholarships to low income children to attend private voucher schools that charge fees.		**Fee charging private voucher relative to free private voucher schools**: 0.057 (0.072) **Fee charging private voucher realtive to public schools**: 0.216 (0.058)	Outcomes of reduced fee paying, low income students in fee charging private voucher schools are compared to similar students in free private and public schools.

a. Standard errors are in parentheses.

b. McEwan (2001) allows for two types of public schools. Public DAEM and public corporations are both financed by the voucher and owned by the municipalities. But public corporations enjoy some autonomy from the municipality's bureaucracy. About 80% of public schools and 60% of public school enrollment in the 1997 data set is accounted for DAEM schools.

c. One-to-one with replacement estimates. Similar results are obteined using nearest-neighbor, kernel and local-linear regression estimates.

Table 2
Private Voucher vs Public Schools' Characteristics in 2006

	Public	Private Voucher
Number of schools	5,971	4,897
Number of schools that provide secondary education	682	1,350
% of schools that provide secondary education	11.4%	27.6%
Total enrollment	1,539,465	1,417,992
Secondary school enrollment	454,013	457,938
Total enrollment share	47.7%	44.0%
Secondary school enrollment share	43.6%	43.9%
Students per teacher	21.9	24.9
Students per teacher in secondary education	25.9	27.9
Monthly resources per student (dollars)	82	92
Mothers' years of education	9.1	10.7
Fathers' years of education	9.3	11.0
Monthly household's income (dollars)	413	672

Sources: Ministry of Education, 2006 SIMCE data base and Central Bank of Chile.
The 2006 average exchange rate of 530 pesos per dollar was used.

Table 3
Pretreatment characteristics of students that completed 8th grade in a public school and had to switch schools

	10th grade at Public School	10th grade at Private School	t-stat	Full SIMCE Sample[a]
# schools in student's neighborhood	71.20	83.53	-23.17	79.73
	(48.54)	(52.50)		(50.96)
# private schools in student's neighborhood	32.08	45.48	-39.23	42.84
	(29.70)	(36.61)		(35.65)
Male	0.45	0.49	-7.87	0.48
	(0.50)	(0.50)		(0.50)
Father's education in 2004	9.42	9.92	-11.82	10.51
	(3.48)	(3.53)		(3.81)
Mother's education in 2004	9.06	9.59	-13.30	10.18
	(3.33)	(3.41)		(3.65)
Expectations: technical or professional institute	0.33	0.35	-4.75	0.29
	(0.47)	(0.48)		(0.45)
Expectations: university	0.40	0.44	-8.06	0.53
	(0.49)	(0.50)		(0.50)
Family income in pesos (divided by 1,000)	189.15	225.53	-18.66	272.44
	(175.50)	(201.18)		(270.36)
Repeated grade	0.119	0.094	7.64	0.101
	(0.32)	(0.29)		(0.30)
Attended preschool education	0.930	0.950	-7.71	0.955
	(0.26)	(0.22)		(0.21)
Municipality's mean family income	319.0	325.8	-5.27	337.1
	(129.38)	(116.60)		(140.30)
Municipality's number of school age children	31466	43554	-35.30	41270
	(28145.62)	(39424.20)		(38652.11)
SIMCE 8th grade math score	249.21	252.06	-6.16	258.88
	(44.78)	(44.41)		(47.72)
SIMCE 10th grade math score	240.61	246.23	-9.18	253.54
	(59.40)	(59.40)		(63.19)
SIMCE 8th grade language score	249.87	252.52	-5.44	258.77
	(47.14)	(47.26)		(49.32)
SIMCE 10th grade language score	246.32	250.97	-9.35	256.18
	(48.38)	(48.07)		(50.65)
Nº observations	30,612	13,668		151,525

a. Child took at least both language tests or both math tests.

Table 4
Heckman-two step estimator for the full sample

Methodology	ATE			
	Language		Math	
Full sample, without controlling for past test scores	7.033	***	8.398	***
	(1.354)		(1.654)	
Full sample, controlling for past test scores	1.650		0.351	
	(1.005)		(1.196)	

Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Note: Variables used for identification are the number public and of private voucher schools per squared kilometer in the student's neighborhood, and the percentage of the enrollment in the student's neighborhood that attends a private voucher school.

Table 5
Probability of attending a private voucher school
Students that completed 8th grade in a public school in 2004

	Math		Language	
# schools in student's neighborhood	-0.011	***	-0.011	***
	(0.003)		(0.003)	
# private voucher schools in student's neighborhood	0.022	***	0.022	***
	(0.005)		(0.005)	
School age population in student's neighborhood	7.96E-07		7.39E-07	
	(2.48E-06)		(2.48E-06)	
Mean income in student's neighborhood	-1.54E-04		-1.48E-04	
	(3.52E-04)		(3.53E-04)	
Male	0.090		0.090	
	(0.056)		(0.057)	
Father's education	7.33E-05		-1.54E-04	
	(0.003)		(0.003)	
Mother's education	0.007	*	0.007	**
	(0.004)		(0.004)	
Expectations: technical or professional institute	0.105	***	0.111	***
	(0.028)		(0.028)	
Expectations: university	0.035		0.040	
	(0.040)		(0.040)	
Repeated grade	-0.104	***	-0.103	***
	(0.034)		(0.035)	
Attended preschool education	0.054		0.056	
	(0.048)		(0.048)	
Family income between 100,000 and 200,000 pesos	0.128	***	0.128	***
	(0.028)		(0.028)	
Family income between 201,000 and 300,000 pesos	0.225	***	0.221	***
	(0.038)		(0.038)	
Family income between 301,000 and 400,000 pesos	0.297	***	0.296	***
	(0.053)		(0.054)	
Family income between 401,000 and 500,000 pesos	0.320	***	0.320	***
	(0.062)		(0.062)	
Family income between 501,000 and 600,000 pesos	0.406	***	0.398	***
	(0.071)		(0.071)	
Family income between 601,000 and 800,000 pesos	0.454	***	0.448	***
	(0.078)		(0.078)	
Family income between 801,000 and 1,000,000 pesos	0.501	***	0.504	***
	(0.116)		(0.117)	
Family income between 1,001,000 and 1,200,000 pesos	0.506	***	0.504	***
	(0.158)		(0.158)	
Family income between 1,201,000 and 1,400,000 pesos	0.500	***	0.499	**
	(0.202)		(0.202)	
Family income between 1,401,000 and 1,600,000 pesos	0.445	*	0.394	
	(0.259)		(0.255)	
Family income between 1,601,000 and 1,800,000 pesos	0.790	**	0.788	**
	(0.335)		(0.334)	
Family income over 1,801,000 pesos	0.645	***	0.643	***
	(0.247)		(0.247)	
SIMCE 8th grade math score	1.31E-04			
	(4.87E-04)			
SIMCE 8th grade language score			8.45E-05	
			(4.20E-04)	
Constant	-0.871	***	-0.863	***
	(0.188)		(0.177)	
Nº observations	27,303		27,218	
Pseudo R^2	0.057		0.057	
Standard errors in parentheses.				
*** significant at 1%, ** significant at 5%, * significant at 10%.				

Table 6
Students attending public schools in 2004
ATE estimates using OLS and propensity scores based methods, and validity tests

Methodology	Estimated effect				Validity test			
	Language		Math		Language		Math	
OLS	1.952	**	2.348	*	0.245		0.340	
	(0.814)		(1.349)		(0.800)		(0.799)	
Pscore Weighting	2.752	***	3.029	**	0.697		0.731	
	(0.836)		(1.459)		(0.816)		(0.837)	
Double-robust	2.362	***	2.644	*	0.428		0.553	
	(0.837)		(1.449)		(0.799)		(0.792)	
Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.								

Table 7
Students attending public schools in 2004
Changes-in-Changes results

Methodology	Estimated effect			
	Language		Math	
CIC without covariates	1.873		1.845	
	(2.173)		(2.994)	
CIC with covariates	1.207		1.511	
	(1.782)		(2.699)	
Standard errors in parentheses.				

Table 8
Students attending private voucher schools in 2004
ATE estimates using all methods and validity tests

Methodology	ATE			Validity test		
	Language	Math		Language		Math	
OLS	-0.286	3.613	*	-1.593		-2.412	*
	(1.201)	(1.892)		(1.166)		(1.265)	
Pscore Weighting	-1.252	2.033		-2.278	*	-3.647	***
	(1.207)	(1.787)		(1.208)		(1.309)	
Double-robust	-0.766	3.165		-1.944		-3.647	***
	(1.191)	(1.953)		(1.199)		(1.309)	
CIC without covariates	0.608	3.492					
	(3.051)	(4.738)					
CIC with covariates	-0.296	3.632					
	(2.569)	(3.870)					

Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 9
Students attending public schools in 2004 and that switched voluntarily
ATE estimates using all methods and validity tests

Methodology	ATE			Validity test		
	Language	Math		Language		Math	
OLS	0.198	1.624		-8.510	***	-9.038	***
	(1.399)	(2.004)		(2.256)		(2.552)	
Pscore Weighting	0.236	0.443		-8.279	***	-9.056	***
	(1.929)	(2.846)		(2.807)		(3.129)	
Double-robust	0.009	0.989		-8.791	***	-9.532	***
	(1.526)	(2.222)		(2.908)		(3.181)	
CIC without covariates	3.066	4.959					
	(6.127)	(8.190)					
CIC with covariates	1.573	3.462					
	(3.694)	(5.105)					

Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 10
Students attending public schools in 2004 and that have to switch schools
ATE double robust estimates following Hirano and Imbens (2001)

Methodology	ATE			
	Language		Math	
Double-robust	2.472	***	2.624	*
	(0.841)		(1.358)	

Standard errors in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Figure 1a
Propensity scores by school type, math

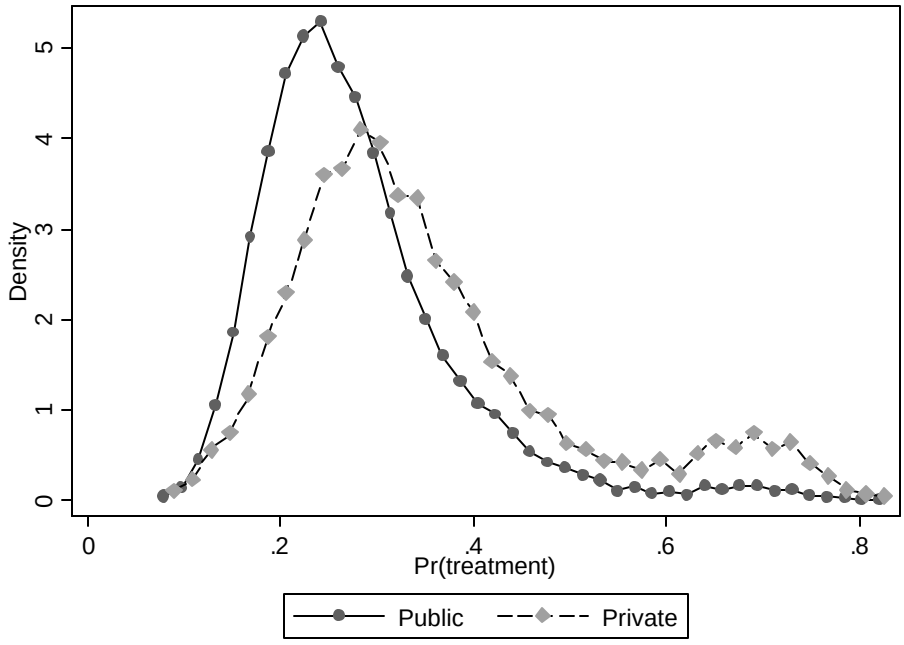


Figure 1b
Propensity scores by school type, language

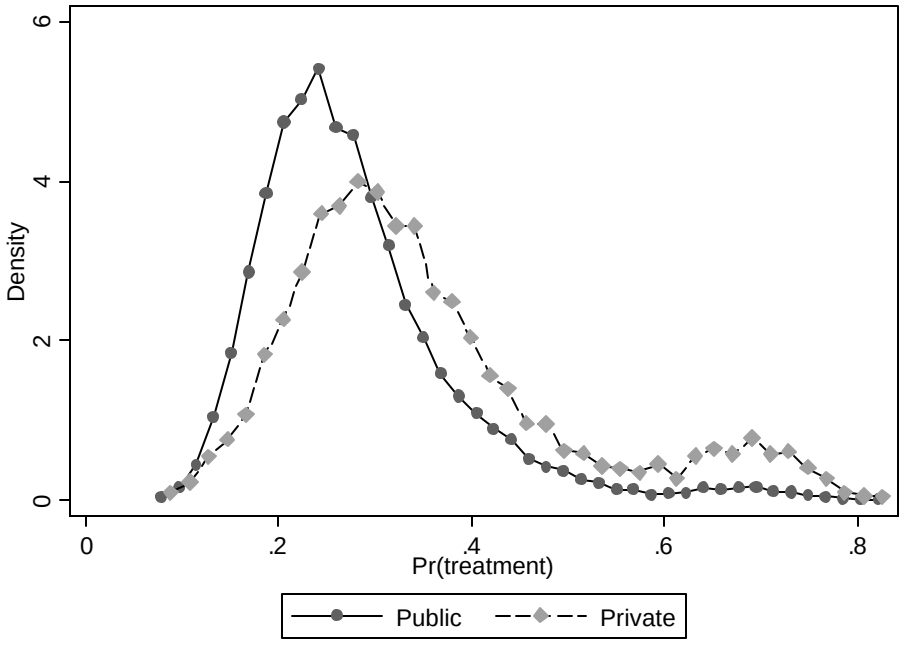


Figure 2a
Effect on students who finish 8th grade at a public school
CIC with covariates along the distribution of test scores, Math

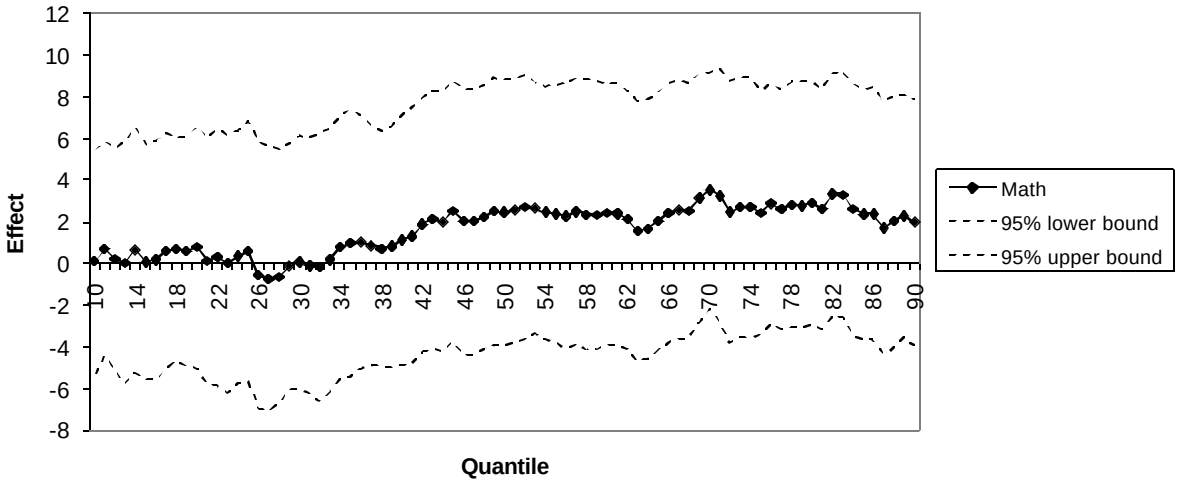
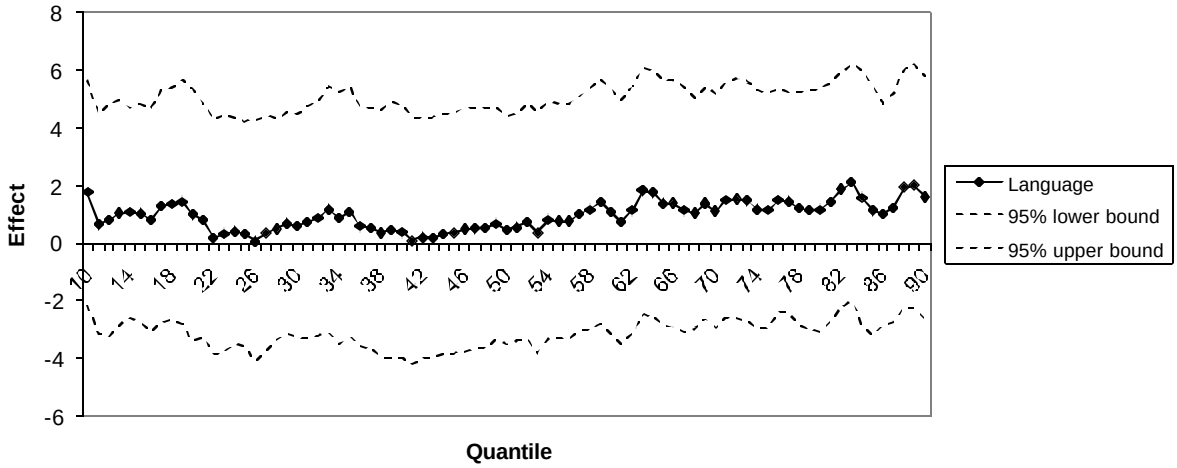


Figure 2b
Effect on students who finish 8th grade at a public school
CIC with covariates along the distribution of test scores, Language



APPENDIX 1

Table A1
Variables used in the analysis

Name of Variable	Description	Source
Student characteristics		
SIMCE 8th grade math score	Student's score on the math section of the SIMCE 2004	SIMCE database 2004
SIMCE 8th grade language score	Student's score on the language section of the SIMCE 2004	SIMCE database 2004
SIMCE 10th grade math score	Student's score on the math section of the SIMCE 2006	SIMCE database 2006
SIMCE 10th grade language score	Student's score on the language section of the SIMCE 2006	SIMCE database 2006
# schools in student's neighborhood	Number of schools in the student's neighborhood of residence 2004	Parental questionnaire 2004 and Ministry of Education
# private schools in student's neighborhood	Number of private schools in the student's neighborhood of residence 2004	Parental questionnaire 2004 and Ministry of Education
Male	Dummy: 1 if the student is male, 0 if female	Parental questionnaires
Father's education 2004	Number of years of education for the student's father	Parental questionnaires
Mother's education 2004	Number of years of education for the student's mother	Parental questionnaires
Expectations: university	Dummy: 1 if the parents expect student to attend college, 0 if not	Parental questionnaires
Expectations: technical or professional institute	Dummy: 1 if the parents expect student to attend a technical or professional institute, 0 if not	Parental questionnaires
Income dummies	Family income dummies	Parental questionnaire 2004
Repeated grade	Dummy: 1 if the student has repeated a grade, 0 if not	Parental questionnaire 2004
Preschool	Dummy: 1 if the student attended preschool, 0 if not	Parental questionnaire 2004
Population at school age in student's neighborhood	Population that is between 5 and 18 years old in the student's neighborhood of residence 2004	SINIM
Mean income in student's neighborhood	Mean income of families in the student's neighborhood of residence 2004	Parental questionnaire 2004/06
Nº of books in the students home	Number of books in the student's house	Parental questionnaire 2004/06
Nº of people in the students home	Number of people in the student's home	Parental questionnaire 2004/06
School type	School type (0 if public, 1 if private voucher)	SIMCE database

Table A2

Descriptive statistics of students that completed 8th grade in a private voucher school and had to switch schools

	10th grade at Public School	10th grade at Private School	t-stat
# schools in student's neighborhood	76.683	83.913	-8.60
	(49.86)	(52.00)	
# private schools in student's neighborhood	40.968	50.217	-16.16
	(32.91)	(36.06)	
Male	0.459	0.476	-2.12
	(0.50)	(0.50)	
Father's education	10.363	10.923	-8.31
	(3.51)	(3.56)	
Mother's education	10.158	10.668	-7.84
	(3.46)	(3.42)	
Expectations: technical or professional institute	0.275	0.309	-4.51
	(0.45)	(0.46)	
Expectations: university	0.555	0.563	-1.01
	(0.50)	(0.50)	
Family income (divided by 1,000)	234.388	284.839	-13.61
	(203.35)	(241.91)	
Repeated grade	0.084	0.069	3.67
	(0.28)	(0.25)	
Attended preschool education	0.962	0.973	-3.87
	(0.19)	(0.16)	
Municipality's mean family income	317.440	322.121	-2.60
	(123.63)	(107.46)	
Municipality's number of school age children	36,695	46,447	-15.92
	(31,462)	(40,632)	
SIMCE 8th grade math score	263.925	263.623	0.41
	(48.39)	(46.06)	
SIMCE 10th grade math score	257.119	260.270	-3.22
	(63.48)	(60.56)	
SIMCE 8th grade language score	263.207	263.550	-0.45
	(49.14)	(47.35)	
SIMCE 10th grade language score	259.957	260.897	-1.21
	(49.96)	(48.55)	
Nº observations	6,547	10,257	

Had enough information: Individual has both language scores or both math scores

Table A3
Descriptive statistics of all students that completed 8th grade and had to switch schools

	10th grade at Public School	10th grade at Private Voucher School	t-stat
# schools in student's neighborhood	72.184	83.847	-26.94
	(48.975)	(52.489)	
# private schools in student's neighborhood	33.622	47.727	-49.50
	(30.627)	(36.743)	
Male	0.455	0.486	-7.67
	(0.498)	(0.500)	
Father's education	9.583	10.338	-21.80
	(3.500)	(3.577)	
Mother's education	9.242	10.030	-23.84
	(3.374)	(3.456)	
Expectations: technical or professional institute	0.316	0.332	-3.85
	(0.465)	(0.471)	
Expectations: university	0.426	0.493	-15.69
	(0.495)	(0.500)	
Income (divided by 1,000)	197.135	251.005	-31.83
	(181.543)	(221.558)	
Repeated grade	0.113	0.083	11.70
	(0.316)	(0.276)	
Attended preschool education	0.935	0.960	-12.52
	(0.246)	(0.197)	
SIMCE 8th grade math score	251.813	257.021	-13.70
	(45.782)	(45.486)	
SIMCE 10th grade math score	243.509	252.243	-17.43
	(60.476)	(60.295)	
SIMCE 8th grade language score	252.220	257.250	-12.67
	(47.767)	(47.610)	
SIMCE 10th grade language score	248.718	255.221	-16.07
	(48.942)	(48.520)	
Nº observations	37,171	23,934	

Table A4

Probability of attending a private voucher school for validity tests (students that completed 8th grade in a public school in 2004)		
	Math	
# schools in student's neighborhood	-0.011	***
	(0.001)	
# private schools in student's neighborhood	0.022	***
	(0.002)	
Male	0.089	***
	(0.025)	
Father's education	-2.42E-04	
	(0.003)	
Mother's education	0.007	***
	(0.003)	
Expectations: technical or professional institute	0.110	***
	(0.024)	
Expectations: university	0.041	
	(0.028)	
Repeated grade	-0.105	***
	(0.030)	
Attended preschool education	0.055	
	(0.041)	
Population at school age in student's neighborhood	7.57E-07	
	(9.46E-07)	
Mean income in student's neighborhood	-1.49E-04	
	(1.82E-04)	
Family income between 100,000 and 200,000 pesos	0.127	***
	(0.023)	
Family income between 201,000 and 300,000 pesos	0.221	***
	(0.031)	
Family income between 301,000 and 400,000 pesos	0.295	***
	(0.039)	
Family income between 401,000 and 500,000 pesos	0.320	***
	(0.053)	
Family income between 501,000 and 600,000 pesos	0.402	***
	(0.060)	
Family income between 601,000 and 800,000 pesos	0.447	***
	(0.069)	
Family income between 801,000 and 1,000,000 pesos	0.511	***
	(0.100)	
Family income between 1,001,000 and 1,200,000 pesos	0.505	***
	(0.154)	
Family income between 1,201,000 and 1,400,000 pesos	0.500	***
	(0.187)	
Family income between 1,401,000 and 1,600,000 pesos	0.395	
	(0.245)	
Family income between 1,601,000 and 1,800,000 pesos	0.788	**
	(0.339)	
Family income is more than 1,801,000 pesos	0.643	***
	(0.236)	
Constant	-0.842	***
	(0.076)	
Nº observations	27,258	
Pseudo R^2	0.057	
Standard errors in parentheses.		
*** significant at 1%, ** significant at 5%, * significant at 10%		

Table A5
Probability of attending a private voucher school
(Students that completed 8th grade in a private voucher school in 2004)

	Math		Language	
# schools in student's neighborhood	-0.010	***	-0.010	***
	(0.003)	***	(0.003)	
# private schools in student's neighborhood	0.017		0.017	***
	(0.006)		(0.006)	
Male	0.046		0.029	
	(0.082)		(0.085)	
Father's education 8th grade	8.90E-03	*	8.57E-03	*
	(0.005)		(0.005)	
Mother's education 8th grade	0.010	*	0.010	*
	(0.006)		(0.006)	
Expectations: technical or professional institute	0.082	*	0.087	*
	(0.049)		(0.048)	
Expectations: university	-0.021		-0.022	
	(0.072)		(0.072)	
Repeated grade	-0.098	*	-0.093	*
	(0.055)		(0.055)	
Attended preschool education	0.015		0.019	
	(0.089)		(0.088)	
Population at school age in student's neighborhood	1.54E-06		1.42E-06	
	(2.58E-06)		(2.57E-06)	
Mean income in student's neighborhood	-1.09E-05		2.39E-05	
	(4.81E-04)		(4.82E-04)	
Family income between 100,000 and 200,000 pesos	0.135	***	0.137	***
	(0.047)		(0.047)	
Family income between 201,000 and 300,000 pesos	0.280	***	0.275	***
	(0.059)		(0.058)	
Family income between 301,000 and 400,000 pesos	0.461	***	0.456	***
	(0.070)		(0.070)	
Family income between 401,000 and 500,000 pesos	0.350	***	0.347	***
	(0.088)		(0.089)	
Family income between 501,000 and 600,000 pesos	0.390	***	0.383	***
	(0.088)		(0.089)	
Family income between 601,000 and 800,000 pesos	0.516	***	0.508	***
	(0.109)		(0.110)	
Family income between 801,000 and 1,000,000 pesos	0.675	***	0.670	***
	(0.141)		(0.145)	
Family income between 1,001,000 and 1,200,000 pesos	0.604	***	0.594	***
	(0.224)		(0.224)	
Family income between 1,201,000 and 1,400,000 pesos	0.928	***	0.877	***
	(0.317)		(0.321)	
Family income between 1,401,000 and 1,600,000 pesos	0.539	*	0.535	*
	(0.290)		(0.288)	
Family income between 1,601,000 and 1,800,000 pesos	0.950	**	0.943	**
	(0.450)		(0.448)	
Family income is more than 1,801,000 pesos	0.909	***	0.908	***
	(0.283)		(0.282)	
SIMCE 8th grade math score	-8.40E-04			
	(7.35E-04)			
SIMCE 8th grade language score			-5.19E-04	
			(6.13E-04)	
Constant	-0.046		-0.133	
	(0.275)		(0.257)	
Nº observations	9,681		9,633	
Pseudo R^2	0.046		0.045	
Standard errors in parentheses.				
*** significant at 1%, ** significant at 5%, * significant at 10%				

Appendix 2. Construction of the Data Set

As explained in the main text, the SIMCE tests are not gathered as a longitudinal study. That is, the 2004 and 2006 data sets can be understood as repeated cross-sections.

After dropping children living in rural areas, children attending private non-voucher schools and those with no information on 8th grade SIMCE scores, the 2004 data set has 215,719 students. Many of them could not be matched, so we are left with 162,763 students. The reasons for attrition are varied and cannot be identified in the data. Desertion and repetition might both explain why some children report scores in 2004 but not in 2006. Secondly, some children may have not taken any of the tests due to sickness or other similar reasons. In addition, due to lack of representativeness, the Ministry of Education does not report the tests scores of the schools with high rates of absenteeism in the days the tests were taken. In addition, immigration in and out of Chile might explain why some children may have taken the tests in only one year. Finally, typing errors might also explain the inability to find a common student identification number across years. At the end, and after dropping students not enrolled at public or private voucher schools in 2006, we have 151,270 students for which we have at least both math test scores or both language test scores.

In addition, some students did not have the parental questionnaire for one of the years. Moreover, although there is a relatively high response rate, many relevant variables are not reported by all parents. Note that this problem is absent in the changes-in-changes estimates without controls. The most complete control variable is family income. As a check of the relevance of the lack of information, we repeated our full analysis controlling only for income in all stages and methods. Our results are quite robust to limiting the analysis in this manner. The results are available upon request from the authors.

The Table below describes the main statistics of the sample of students with information on both language tests scores or on both math tests scores. We also provide statistical information on the sample of students with incomplete SIMCE test scores. Children with incomplete information tend to come from families with lower socioeconomic backgrounds. Their performance in the 8th grade SIMCE is also lower.

Appendix 2. Descriptive statistics of students by their information availability (8th grade at PS and MUN)

	SIMCE full information[a]	SIMCE incomplete information[b]	t-stat
# schools in student's neighborhood	79.751	80.182	-1.65
	(50.93)	(50.52)	
# private schools in student's neighborhood	42.843	43.441	-3.29
	(35.62)	(34.88)	
Male	0.476	0.537	-25.42
	(0.50)	(0.50)	
Father's education 2004	10.507	9.342	52.83
	(3.81)	(3.72)	
Mother's education 2004	10.175	8.888	61.92
	(3.65)	(3.55)	
Expectations: technical or professional institute	0.289	0.327	-16.27
	(0.45)	(0.47)	
Expectations: university	0.529	0.306	88.70
	(0.50)	(0.46)	
Family income 2004 (divided by 1,000)	272.269	214.157	43.70
	(270.11)	(234.21)	
Repeated grade	0.101	0.297	-110.67
	(0.30)	(0.46)	
Attended preschool education	0.955	0.934	18.84
	(0.21)	(0.25)	
Municipality's mean family income	336.996	302.453	51.91
	(140.33)	(132.78)	
Municipality's number of school age children	41278	42234	-4.85
	(38651.20)	(37627.90)	
SIMCE 8th grade math score	258.868	228.734	133.41
	(47.71)	(43.28)	
SIMCE 8th grade language score	258.761	226.218	136.06
	(49.31)	(48.55)	
Nº observations	151,270	60,378	
a. Individual has both language scores or both math scores			
b. Individual has either math or language scores at 8th grade, but does not have the same test score at 8th and 10th grade. Also, individual is in the 2004 data set, but not in the 2006.			

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