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

Bernardo Lara
Alejandra Mizala
University of Chile

Andrea Repetto
Universidad Adolfo Ibáñez

In this article the authors 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. The authors analyze the effect of private voucher education on students who are forced to enroll at a different school to attend secondary education once graduated from primary schooling—structural switches. Moreover, the data set the authors use in this article contains information on previous academic achievement and thus allows them to identify differences in students' unobservable characteristics. Using a number of propensity-score-based econometric techniques and the changes-in-changes estimation method, the authors 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% of one standard deviation in test scores. In contrast, the literature on Chile based on cross-sectional data had previously found positive effects of about 15% to 20% of one standard deviation.

Keywords: *school choice, educational vouchers, school switches, student achievement, Chile*

One of the most important debates in educational policy relates to whether different forms of school choice should be extensively introduced. Proponents argue that choice generates competition, putting pressure on all schools to improve the quality of the education they provide. 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 introduce choice into the educational system. Although residential choice is the most prevalent form of school

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choice in the United States, private voucher schools, open enrollment programs, and charter and magnet schools coexist with traditional public schools. As of 2007, 16% of American students in Grades 1–12 were enrolled in chosen public schools. Outside the United States, Chile, Denmark, the 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 (Hanushek, Kain, & Rivkin, 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, Jacob, & Levitt, 2005, 2006; Hsieh & Urquiola, 2006).

In this article we study the effect of private voucher education on student performance using a new data set on Chilean students and a novel identification strategy. Specifically, the Chilean education system consists of 8 years of primary school and 4 years of secondary school. Most schools financed by the state voucher have primary or secondary education only. In fact, about 56% of eighth-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 to the secondary school performance of those who stayed in the public 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, that is, 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, allows us to account for this. Until recently only cross-sectional data had been available on Chilean students since national standardized tests are administered annually to a specified grade level that rotates every year among the 4th, 8th, and 10th grades. In 2004 and 2006, however, the test was administered to the same students in 8th and 10th grades. Although test scores are not comparable over time, the 2004 results can be used to identify differences in students' ability when analyzing the 2006 scores. In addition, 8th-grade scores reflect not only ability but also school-type effects if the hypothesis that school type matters is true. So controlling for pretreatment scores and limiting the sample to students 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 article we compare the 10th-grade performance of students who moved from a public to a private voucher school (treatment group) to the 10th-grade performance of students who stayed in the public school system (control group). That is, we estimate the effect of having attended 2 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. As a comparison, we also estimate test score differences on the full sample using the techniques of most of the previous literature on Chile's school choice system, that is, Heckman's correction for selection.

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, that is, a gain of 4% to 6% 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 CIC approach yields positive estimates of the same order of magnitude at the mean. Moreover, the results indicate that the effect on language scores is positive for students in the full distribution of outcomes, whereas the effect on math scores is concentrated at its upper end. However, the results are not statistically significant.

Heckman selection correction estimates on the full sample are large and significant only when we do not control for past test scores. We believe that even controlling for previous outcomes these results may be biased because prior test scores depend on the school type chosen for primary education. Still, 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 suggesting this is the case. These caveats must be kept in mind. Finally, and although based on different theoretical assumptions about the underlying behavior of the data, propensity-score-type and CIC estimators yield similar results. These estimated effects are much lower than those obtained by the previous literature on Chile based on cross-sectional data, which typically finds effects of about 0.15 to 0.2 standard deviations, but are in line with a number of articles on U.S. experiences that find small and many times ambiguous effects.

The article is organized as follows: The second section reviews the literature on school choice. The third section provides an overview of the Chilean educational system, and the fourth section explains our empirical strategy. The fifth section describes the data used in this study. The sixth and seventh sections present our results, extensions, and sensitivity analyses. Finally, the eighth section concludes.

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.

In the United States there are several small-scale voucher programs, mostly designed for low-income students, such as the Milwaukee Parental Choice Program, the Cleveland Scholarship and Tutoring Program, the Washington (D.C.) Opportunity Scholarship Program, and the New York City voucher experiment.[1] In general, this research finds relatively small achievement gains for students offered 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.

A related literature studies the performance of charter schools based on the mobility of students across establishments. Hanushek, Kain, Rivkin, and Branch (2007) analyze the Texas charter experiment based on a panel of students who move across different schools, including charter schools. The article finds that after an initial start-up period, charter schools' performance is statistically similar to that of public schools. The results also suggest that parents of children attending charter schools are more sensitive to quality in the switching decision. Using the same data set but a different identification approach, Booker, Gilpatric, Gronberg, and Jansen (2004) find a significantly positive impact on academic achievement. Studies that compare before and after intervention outcomes, however, might suffer from bias produced by Ashenfelter's dip, that is, the phenomenon that intradistrict 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 advantage of a randomized design, such as Angrist, Bettinger, Bloom, King, and Kremer (2002) and Angrist, Bettinger, and Kremer (2006) for Colombia and Kang (2007) for South Korea, show that students who attend private voucher schools experience a significant gain in test scores.[2] When vouchers are universal and have been in place for many years, however, rigorous empirical strategies are difficult to implement. Research on countries such as the Netherlands, Denmark, and Sweden has focused on the effect of competition on students' outcomes. The review by Barrera-Osorio and Patrinos (2009) suggests no effect of competition on educational outcomes in Denmark but a positive effect in the performance of Swedish public schools and of all schools in the Netherlands.

Evaluations of the Chilean voucher system have mainly focused on the relative effectiveness of private voucher vis-à-vis public schools.[3] Lacking randomized designs and panel data, researchers have addressed this question by comparing the achievement of students who attend public and private voucher schools to that of controls for their observed and—more tentatively—unobserved characteristics.

Most studies using cross-sectional individual-level data have found that students attending private voucher schools have higher educational outcomes than those from public schools; the estimated impact is typically between 0.15 and 0.2 standard deviations.

Mizala and Romaguera (2001) estimate the effects on 10th-grade test scores. Using ordinary least squares (OLS), the article finds an effect near 0.2 standard deviations. Sapelli and Vial (2002) also analyze 10th-grade data. The article uses the 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 who attend private schools that are solely financed by the voucher. Sapelli and Vial (2005) use data on 4th-grade test scores, characteristics of the educational market in the geographical area as instruments, and Heckman's methodology to find large effects of private voucher education, up to 60% of one standard deviation. The only article that finds no advantage of private voucher schools is McEwan (2001), which finds no consistent difference between public and nonreligious private voucher schools and a higher effectiveness of Catholic private voucher schools. These 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 articles hinges on the hypothesis that school choice depends on how densely concentrated are schooling alternatives in the neighborhoods. It is also assumed that these variables do not directly affect students' outcomes. The availability of schooling opportunities in the local area 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 market for older students is difficult to define as there are no restrictions on the location of the schools the students can attend. In other words, school density variables may reflect unobserved family and community characteristics that influence achievement invalidating the necessary exclusion restrictions.

Finally, Anand, Mizala, and Repetto (2009) use propensity score matching to compare the test scores of reduced-fee-paying low-income students in private voucher schools to those of similar students in public schools and in a free private voucher school. The results reveal that students in fee-charging private voucher schools score higher—a 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, in particular, if 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 nonrandom selection of students into different school types and for unobservables that simultaneously affect both the decision to attend a given school and students' performance. In the fourth section we describe our identification strategy and discuss its advantages and limitations.

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 management of public schools to municipal governments and the establishment of a voucher-type student-based subsidy paved the way for the private provision of publicly financed education. The voucher, which is of the same size for public and private voucher schools, is paid directly to schools on a per-student basis. It is intended to cover running costs and generate competition to attract and retain students. The monthly per student subsidy amounted to about $61.50 for primary schools and $73.30 for secondary schools in 2006.[4]

Three types of schools were established: public (municipal) schools, financed by the subsidy 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. 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 private schools willing to take the voucher were created. In 1985 there were 2,643 private voucher schools, 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% of the enrollment, whereas public sector enrollment dropped from 78.0% in 1981 to 47.7% in 2006. The 7.0% share of private non-voucher schools was practically unaffected by the system's transformation.

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 oversubscribed. Second, teachers' contracts in public schools are governed by a special legislation—the Teachers' Statute—which involves centralized collective bargaining as well as restrictions on teacher dismissal. Private schools' teachers come under the same Labor Code as other private sector workers in the country. Finally, there are differences in the ability to raise additional funding: Private voucher schools are allowed to charge tuition on top of the voucher up to a limit. 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.

The Chilean school system is divided into primary education (Grades 1–8) and secondary education (Grades 9–12). Since 2003 both primary and secondary education have been mandatory. Not all public and private voucher schools offer primary and secondary education, though. At the national level 76.4% of public schools and 52.0% of private voucher schools provide education up to eighth grade only; these rates are equal to 74.5% and 48.8% in urban areas, respectively. In terms of enrollment, in 2004 the fraction of students in public schools who had to switch schools at the end of eighth 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 eighth grade to continue their secondary education is essential to our identification strategy further described below. To better understand why many schools choose not to offer secondary grades, we conducted interviews with providers of private subsidized education. These interviews revealed that cost concerns are at the heart of the decision of whether to offer secondary education. Secondary education provision is more expensive: Because of regulatory requirements, it is necessary to hire specialized teachers for every subject area, whereas in primary education the same teacher covers all subjects. Thus, offering secondary grades requires a larger scale—a larger number of classrooms of the same grade to have each subject teacher teaching in different sections.[5] If this were not the case, teachers would be hired under part-time contracts, increasing their salary per hour. For this same reason secondary education management is more complex. Another explanation relies on the availability of land and infrastructure. Finally, prior to the 2003 compulsory secondary schooling law, the size of the secondary education market was smaller than the size of the primary education market.

Private voucher and public schools represented similar shares of total enrollment in 2006: 44.0% and 47.7%, respectively. A small fraction, 27.6% of private voucher and 11.4% of public schools, provide secondary education. Furthermore, according to the data from the Ministry of Education, there are relevant observable differences in the experiences of students attending these two types of schools. Teachers at private voucher schools attend larger classrooms, 24.9 versus 21.9 students per teacher in public schools. Moreover, students enrolled in public schools belong to lower income households (monthly household's income of $413 vs. $672), they receive fewer financial resources at school ($82 vs. $92 per month), and relate to peers of lower socioeconomic backgrounds (9.1 years is the average maternal education at public schools vs. 10.7 years at private voucher schools).

Identification Strategy and Estimation Methodologies

The main methodological challenge we face is selection bias; that is, the assignment of students in schools is not random. To account for this, we propose an identification strategy based on a common phenomenon that characterizes the Chilean educational market: Most schools that are financed by the voucher provide either primary or secondary education only. In fact, in 2004, 56.4% of the students enrolled in 8th grade attended public or private voucher schools that did not offer secondary grades and 54.5% of those in urban areas. These students had to choose another school to continue studying.

We limit the analysis to the students who attended eighth grade in a public school that did not provide secondary grades and thus had to switch schools to continue their education. Our treatment group is thus composed of those 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 eighth grade—instead of any school that did not provide secondary education—improves the similarity between the treatment and control groups.

Another reason to limit the analysis to students attending public schools in 2004 is that, although 2004 test scores serve as control for student ability, the scores depend on the type of school the child attended up to eighth grade. That is, under the hypothesis that school type matters, the pretreatment correction might not be enough to account for unobserved ability.

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, the external validity of the results might be limited. Because the effects are identified from structural switches, it is an open question as to the extent that the estimates are relevant to students who attend schools that provide primary and secondary education. Moreover, our identification strategy does not include students attending elite public schools, also known as "emblematic schools," which select students but enroll them in 7th grade. If families who expect to obtain higher benefits from attending these schools are more likely to enroll, the 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 because of structural reasons at the end of eighth grade only.

Another concern relates to school dropouts. According to the Ministry of Education, 4.8% of students enrolled in secondary education dropped out in 2005. It is worth emphasizing that secondary education became mandatory in 2003, forcing all students to continue their education once they earned their primary school certificate.

Finally, although our interviews reveal that regulation-related cost concerns are at the heart of the decision of whether to offer secondary grades, this decision might still be endogenous to public school quality. In the interviews with private education providers we also inquired about their decision of establishing a new school. The interviews revealed they considered the demographic characteristics of the area, the economic profile of the families, the community's interests, the degree of bureaucracy of the local government, and the characteristics of existing schools. As a check, we estimated three models for 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 2003 and 2006. The second one is an OLS model for the number of private voucher schools that opened in the same period. The final one estimates the number of private voucher schools operating in 2006 in the local area. As controls we use the number of school-aged children in the municipal area, parental average education, mean household income, and the quality of public schools measured by the standardized SIMCE (Sistema de Medición de la Calidad de la Educación) test. The SIMCE test did not turn out to have a significant effect in any of our models.[6]

In what follows, we quickly review the estimators we use, their assumptions, and their properties. Our main results are based on two types of econometric techniques: propensity-score-based estimators and CIC estimators.

Propensity-Score-Based Methodologies

We use two propensity-score-based methodologies to identify the average treatment effect (ATE): propensity score weighting (PSW) and the combination of the latter with regression adjustment (double robust; DR). The approach relies on the usual assumptions used in matching: unconfoundedness and overlap (Imbens & Wooldridge, 2009). The first assumption states that treatment assignment is exogenous given the covariates or the propensity score. The second assumption states that individuals should have positive probabilities of being observed in both treatment and control groups, an assumption likely to be accomplished by the construction of a common support.

The most popular of propensity score methodologies is propensity score matching. However, it is not clear how to estimate the standard errors in a way that takes into account clustering.[7] It is very likely that there is clustering at the school level, so we would like to estimate correctly the variance of treatment effects. Nevertheless, there is no method to correct for clustered errors in the context of propensity score matching. A popular option is bootstrapping at the cluster level; however, bootstrapping is not valid with matching.[8] Therefore, we focus on propensity-score-based estimators that do not have these problems.[9]

PSW weights the observations using the propensity score and the treatment status to balance the sample between treated and nontreated individuals based on the treatment probability. Specifically, we use the inverse probability weighting estimator proposed by Hirano, Imbens, and Ridder (2003). We perform bootstrapping at the cluster level to estimate standard errors. We perform bootstrapping before the construction of the propensity score and the common support, taking into account potential errors in these procedures.[10]

The propensity-score-weighted regression, introduced by Robins and Rotnitzky (1995) and Robins, Rotnitzky, and Zhao (1995), directly accounts for the correlation between covariates and outcomes.[11] In particular, we expect the pretreatment test score to have a strong and direct effect on the posttreatment test score. The method also has a double-robustness feature: The estimates are consistent if either the probability of treatment or the outcome regressions are incorrectly specified. Our implementation of the DR estimator follows the steps of Emsley, Luna, Pickles, and Dunn (2008). To estimate the standard errors we again use bootstrapping at the school level.

The unconfoundedness assumption is not directly testable. Nevertheless, Imbens (2004) and Imbens and Wooldridge (2009) describe how to assess its plausibility by estimating the impact of the treatment on a variable correlated with the outcome of interest but that is not affected by the treatment. A pretreatment outcome closely related to the posttreatment outcome, for example, eighth-grade test scores, can play this role. If the unconfoundedness assumption is true, then propensity score methods should estimate zero impact on eighth-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.

Let the 8th-grade score (Y_{8th}) be the dependent variable and let the treatment be the private voucher school status at 10th grade (D_{10th}). We use as covariates the 8th-grade (X_{8th}) observables and estimate this "false experiment" using the same procedures to verify whether $D_{10th} \perp Y_{8th} \mid X_{8th}$ holds. Nevertheless, it is worth emphasizing that finding a zero effect does not guarantee that unconfoundedness is true, but it indicates that it is very likely the case.

Changes-in-Changes Estimator

The second methodology we use is the CIC estimator introduced by Athey and Imbens (2006), which generalizes the difference-in-difference estimator under fewer assumptions for consistent estimates. An advantage of this method is that it allows for differences in the distributions of unobservables across treatment and control groups. It also allows for the estimation of the treatment on any quantile of the unobservables' distribution. The main idea is that the distribution of unobservables can be inferred from the pretreatment outcomes. Once the distributions of unobservables of each group are known, one can estimate how much of the observed effect is the result of differences in the pretreatment unobservables' distribution.

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 prior to the treatment will have the same rank after the treatment. This assumption allows the distribution of unobservables to differ across groups but not over time.

The CIC estimator can be estimated with and without control variables. Under the assumptions in Athey and Imbens (2006), the estimator is consistent and asymptotically normal. Again, we use bootstrapping at the school level to estimate clustered errors.

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

Data

The data used come from two sources. The first is the standardized test SIMCE, which is administered annually to a specified grade level that rotates every year between the 8th and 10th grades and, since 2005, every year to 4th grades. The rotation implies that except for the data we use, the SIMCE tests do not track students over time. We use 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 prior performance. Our data set cannot be analyzed as a panel, as SIMCE scores are not comparable across tests.[12]

The second data source is a questionnaire answered by the parents of students taking the SIMCE in 2004 and 2006. This data set provides information on characteristics of the students and their families. Although it is not mandatory for parents to answer the questionnaire, there is a high response rate for the key variables used in the analysis.

After excluding observations with incomplete information, modifications were made to target the population we are interested in studying. First, we consider only the students who were forced to switch schools at the end of eighth grade. Second, we exclude those in private nonvoucher schools because these schools typically serve the most elite families: They are not a realistic option for the average student given the high fees charged. Third, we consider only students in urban schools since in rural areas there is limited choice mainly as a result of geographic constraints.[13] Finally, near 12% of students who had to switch between the eighth and ninth grades switched again the following year. Unfortunately, we do not know the school type they attended in ninth grade, so we have excluded them from the analysis.[14]

Modifications were made to some of the variables. Specifically, parents reported the highest level of education that they had attended. We converted these into the 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.

The appendix displays the variables used along with their definition and data source. Table 1 summarizes the basic statistics for the sample used.[15] On average, those who switched to private schools score better than those who moved to public schools. They also have better resources: higher parental education and family income and parents with higher expectations about the education their children will attain. Moreover, a lower fraction of those who moved to private schools had repeated a grade and a higher fraction had attended preschool.

Table 1 also provides information on the characteristics of all children who took the language or math tests in both years; that is, it includes children attending private voucher schools in 2004 and 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 is income. In addition, parents have better expectations about their children's 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 drives these differences.

TABLE 1
Pretreatment Characteristics of Students Who 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				
M	71.20	83.53	–23.17	79.73
SD	48.54	52.50		50.96
# private schools in student's neighborhood				
M	32.08	45.48	–39.23	42.84
SD	29.70	36.61		35.65
Male				
M	0.45	0.49	–7.87	0.48
SD	0.50	0.50		0.50
Father's education				
M	9.42	9.92	–11.82	10.51
SD	3.48	3.53		3.81
Mother's education				
M	9.06	9.59	–13.30	10.18
SD	3.33	3.41		3.65
Expectations: Technical or professional institute				
M	0.33	0.35	–4.75	0.29
SD	0.47	0.48		0.45
Expectations: University				
M	0.40	0.44	–8.06	0.53
SD	0.49	0.50		0.50
Family income in pesos (divided by 1,000)				
M	189.15	225.53	–18.66	272.44
SD	175.50	201.18		270.36
Repeated grade				
M	0.119	0.094	7.64	0.101
SD	0.32	0.29		0.30
Attended preschool education				
M	0.930	0.950	–7.71	0.955
SD	0.26	0.22		0.21
Municipality's family income				
M	319.0	325.8	–5.27	337.1
SD	129.38	116.60		140.30
Municipality's number of school-aged children				
M	31466	43554	–35.30	41270
SD	28145.62	39424.20		38652.11
SIMCE 8th-grade math score				
M	249.21	252.06	–6.16	258.88
SD	44.78	44.41		47.72
SIMCE 10th-grade math score				
M	240.61	246.23	–9.18	253.54
SD	59.40	59.40		63.19
SIMCE 8th-grade language score				
M	249.87	252.52	–5.44	258.77
SD	47.14	47.26		49.32
SIMCE 10th-grade language score				
M	246.32	250.97	–9.35	256.18
SD	48.38	48.07		50.65
No. of observations	30,612	13,668		151,525

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

TABLE 2
Heckman Two-Step Estimator for the Full Sample

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

Note. Standard errors in parentheses. Variables used for identification are the number of public and of private voucher schools per square kilometer in the student's neighborhood and the percentage of the enrollment in the student's neighborhood attending a private voucher school.
***Significant at 1%.

Results

We estimate the effect of private voucher education on performance using alternative estimation methods. We start by using the methodological approach of 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.

Comparison With the Previous Literature

Our methodological approach based on 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 main results, and 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 articles.

Table 2 displays the results of this exercise. In the top panel we use the full 2006 sample and analyze it as a cross section using Heckman's two-step method. As in Sapelli and Vial (2002), our instruments relate to characteristics of the local educational market: the number of public and of private voucher schools per square kilometer in the student's neighborhood and the fraction of students in the neighborhood attending a private voucher school. All variables turn out statistically significant at conventional levels, except for the supply of public schools. The estimated effect amounts to 7.0 points in language and 8.4 points in math, both significant at 1%. 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 eighth-grade test score. We now find that the estimated effect of private voucher education drops dramatically and is no longer statistically significant at conventional levels.

These estimates suggest that the differences in results have to do with the methodological approach. We are able to replicate the previous results on Chile in our sample, but the identification strategy based on the characteristics of the geographical market is not robust to controlling for pretreatment test scores. Still, these results may not represent consistent estimates if the hypothesis that school type matters is true. For this reason, we now turn to the results based on our proposed identification strategy.

Propensity-Score-Based Methods

We first estimate the probability of attending a private voucher school within our subsample of students undergoing a structural switch, that is, 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 sociodemographic variables that characterize the municipality and the relative supply of private voucher schools in the students' neighborhood of residence. Second, we control for student- and family-level characteristics and pretreatment SIMCE test scores, all measured in 2004. The results are displayed in Table 3. They show that income, maternal education, and parental expectations on attending postsecondary education are correlated with the likelihood of attending private voucher schools as well as not having repeated a grade. Also, students are more likely to enroll in these schools whenever there is more supply 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.[16]

TABLE 3

Probability of Attending a Private Voucher School: Students Who 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-aged population in student's neighborhood	7.96E-07	7.39E-07
	(2.48E-06)	(2.48E-06)
Mean family 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. of observations	27,303	27,218
Pseudo R^2	.057	.057

Note. Standard errors in parentheses.

*Significant at 10%. **Significant at 5%. ***Significant at 1%.

TABLE 4

Students Attending Public Schools in 2004: Average Treatment Effect Estimates Using Ordinary Least Squares (OLS), Propensity-Score-Based Methods, and Validity Tests

	Estimated effect		Validity test	
Methodology	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)

Note. Standard errors in parentheses.
*Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table 4 presents the ATE estimates and the validity tests. Using PSW we obtain positive effects that are statistically significant at 1% for language and at 5% for math. Similar results are obtained using DR, with statistical significance ranging between 1% and 10%. The estimated effect amounts to 2.36 to 3.03 points on the SIMCE test, that is, 4% to 6% of one standard deviation, an effect much smaller than the one found by the previous literature. Given the extent of the overlap in our propensity scores and to complement our results, we also report the OLS estimates. Although somewhat smaller than our DR and PSW estimated effects, the results are quite similar and are also statistically significant.

The results of the validity tests are presented in the right-hand panel of Table 4.[17] These tests find no effect of the 10th-grade treatment on 8th-grade scores. The results suggest that there are no relevant unobservable variables in the pretreatment 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 posttreatment period.

CIC Estimators

Table 5 presents the results using CIC estimation methods without and with controls for covariates.[18] The effect is lower than the one obtained using propensity score methods, always below 2 points. In all cases the effect is not statistically significant.

We also infer the treatment effect on the entire distribution of outcomes. Figures 1a and 1b present the results after correcting for covariates. Figure 1a shows that all students above the 34th percentile experience positive effects in math. For the language test, the positive effects are evenly distributed along the distribution. That is, not only the average student but also most students along the distribution of outcomes benefit from private voucher school education. However, none of these estimates are statistically significant.

TABLE 5

Students Attending Public Schools in 2004: Changes-in-Changes (CIC) Results

	Estimated effect	
Methodology	Language	Math
CIC without covariates	1.873	1.845
	(2.173)	(2.994)
CIC with covariates	1.207	1.511
	(1.782)	(2.699)

Note. Standard errors in parentheses.

Summing up, if our identification strategy based on the subsample of students who were forced to switch schools is valid, then the estimators suggest that there are small differences in the performance of students attending private voucher and public schools.

Sensitivity Analyses and Extensions

In this section we provide a number of extensions and sensitivity analyses to gauge the robustness of our identification strategy.

We start by reestimating the effect of private voucher education now limiting the subsample to those students who attended eighth grade in a

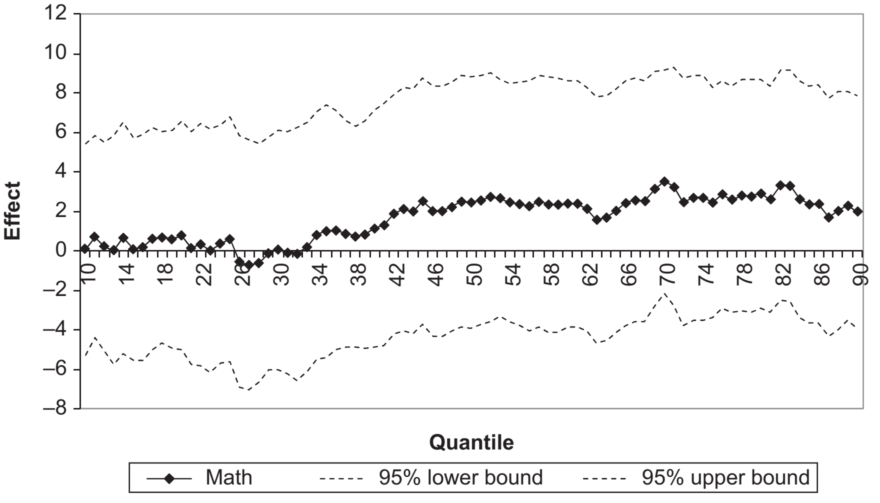

FIGURE 1A. *Effect along the distribution of test scores on students who finish 8th grade at a public school; changes-in-changes approach, with covariates; math.*

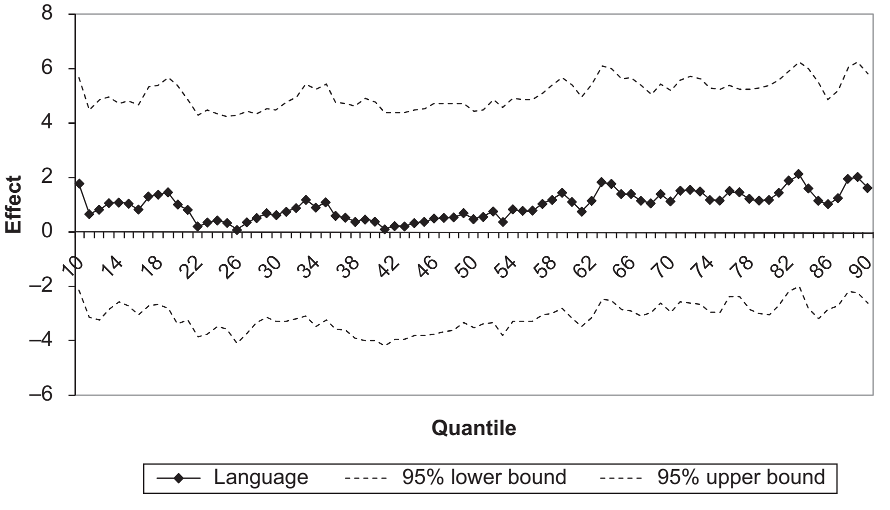

FIGURE 1B. *Effect along the distribution of test scores on students who finish 8th grade at a public school; changes-in-changes approach, with covariates; language*

private voucher school who provided no secondary education and thus had to switch to another school at the end of eighth grade. Our new control group is now composed of those who moved to a public school, whereas our treatment group includes those who moved to another private voucher school.

Table 6 presents the results. The estimated effects on language test scores are negative in most cases, whereas the effects on math test scores are positive. These small estimates, however, are never statistically significant except for the math effect using OLS.[19]

The right-hand panel of Table 6 presents the validity tests. We find a significant effect of the treatment on eighth-grade scores, with a high likelihood that the transition from private voucher to public schools is endogenous. That is, the unconfoundedness assumption is unlikely to hold in this setup. Perhaps these students opted mainly for high-achievement public schools. These schools face excess demand and thus can select high-ability students.

Our second exercise analyzes voluntary switches. The focus on structural switches allows

TABLE 6

Students Attending Private Voucher Schools in 2004: Average Treatment Effect Estimates Using All Methods and Validity Tests

	Average treatment effect		Validity test	
Methodology	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)		

Note. OLS = ordinary least squares; CIC = changes-in-changes. Standard errors in parentheses.
*Significant at 10%. ***Significant at 1%.

TABLE 7

Students Attending Public Schools in 2004 Who Switched Voluntarily: Average Treatment Effect Estimates Using All Methods and Validity Tests

	Average treatment effect		Validity test	
Methodology	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)		

Note. OLS = ordinary least squares; CIC = changes-in-changes. Standard errors in parentheses.
***Significant at 1%.

us to circumvent the possibility of mean reversion, a phenomenon we indirectly analyze here. 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 school. In this case, we might find a smaller or even a negative effect of the treatment.

Table 7 presents our results and validity tests. OLS and propensity-score-based estimated effects are smaller than in our baseline 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 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 8th grade that reverses to the mean in 10th grade. Contrary to these results, the CIC estimators are larger than those obtained using our sample of

TABLE 8

Students Attending Public Schools in 2004 Who Had to Switch Schools: Average Treatment Effect Estimates Following Hirano and Imbens (2001)

	Average treatment effect	
Methodology	Language	Math
Double robust	2.472*** (0.841)	2.624* (1.358)

Note. Standard errors in parentheses.
*Significant at 10%. ***Significant at 1%.

structural switchers, but again they are not statistically significant.

Our third robustness exercise analyzes alternative controls. The SIMCE parental questionnaire data set contains a large number of questions on household characteristics. In using the DR estimator, Hirano and Imbens (2001) suggest a decision rule to decide which controls to include based on the *t*-statistic. In this exercise we follow their approach. Table 8 shows that our results are robust to the choice of controls based on this rule.[20]

Our final extension estimates the amount of selection on unobservables relative to the selection on observables required to attribute the full estimated private voucher effect to selection bias based on the method suggested by Altonji, Elder, and Taber (2005). In general terms, the approach is based on the assumption that the part of an outcome that is related to observables has the same relationship with the endogenous treatment as the part related to unobservables.

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 test scores are positive but small.

Discussion and Concluding Remarks

In this article we revisit the school choice debate using new data on Chile and a new identification strategy. We start by replicating the methods used by the previous literature 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 test performance.

We then examine the differences in the 10th-grade test scores of students who moved from a public to a private voucher school with the 10th-grade scores of students who stayed in the public school system. With these groups at hand, we estimate test score differences using propensity score techniques and the CIC method. 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. The CIC estimates suggest that these positive effects apply to the full distribution of results in the case of language and to most of the distribution in the case of math, that is, they are not concentrated within a given group of students. However, they are not statistically significant. In sum, although based on different theoretical assumptions about the underlying behavior of the data, propensity score type and CIC estimators yield similar results.

The statistical significance of our results contrasts with their economic relevance: The effect we find is never larger than 6% of one standard deviation. A different hypothesis might explain why this effect is small. One is that competition puts 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. The current Chilean school system regulation is lax, allowing schools to survive even if their academic performance is poor. To continue receiving the voucher, private schools have to meet minimal standards with no supervision on how the resources are spent. Very few schools close. In 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 schools do compete but in dimensions other than academic achievement. Perhaps parents care about peer socioeconomic makeup in itself, regardless of achievement (Elacqua, Schneider, & Buckley, 2006; Hsieh & Urquiola, 2006). Alternatively, parents do care about achievement and are able to assess average performance, but given the strong correlation between socioeconomic status and students' results, they cannot assess the value added by the school (Mizala, Romaguera, & Urquiola, 2007). This is consistent with the scant evidence of students switching to more effective schools (Mizala & Urquiola, 2008).

Another hypothesis is that the treatment we measure does not allow for enough exposure of students to private education. Recall that the evaluation is done after only 2 years of private education. Thus, our results could be reinterpreted as a gain of 4% to 6% of one standard deviation in a 2-year period, which is in line with the estimated 1.5% to 2.3% 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.

Also, our sample represents students who have attended public schools up to the eighth grade. Perhaps at this age it is too late to obtain significant changes in academic achievement.

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

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

Appendix

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

(continued)

Appendix (continued)

Name of variable	Description	Source
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 2004–2006
No. of books in the student's home	Number of books in the student's house	Parental questionnaire 2004–2006
No. of people in the student's home	Number of people in the student's home	Parental questionnaire 2004–2006
School type	School type (0 if public, 1 if private voucher)	SIMCE database

Note. SINIM = Sistema Nacional de Información Municipal.

Notes

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

2. 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 is to generate homogeneity across schools within districts.

3. A second line of research has attempted to identify the effect of school competition on students' achievement; see Hsieh and Urquiola (2006), Gallego (2002, 2006), and Auguste and Valenzuela (2003). A related literature analyzes public and private school enrollment practices in response to vouchers (Elacqua, Schneider, & Buckley, 2006). Finally, Mizala and Torche (in press) analyze the socioeconomic stratification of achievement in the Chilean voucher system.

4. At an exchange rate equal to 530 Chilean pesos per U.S. dollar.

5. The number of classrooms per school increases threefold in the transition from primary to secondary education, with almost no changes in classroom size. Primary education schools have on average 1.6 sections in each grade and almost 33 students per class. Secondary schools have on average 4.5 sections in each grade and about 36 students per section.

6. The estimation results are available on request.

7. This concern also relates to modeling the selection process based on Heckman's method.

8. 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.

9. Despite the described problems, we estimated one-to-one propensity score matching to find a very similar average treatment effect. Results are available on request.

10. 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.

11. Further details on this method can be found in Imbens (2004) and Imbens and Wooldridge (2009).

12. In a companion paper, we describe our matching procedure across data sets. See Lara, Mizala, and Repetto (2010).

13. Currently, 63 rural municipalities out of a total of 345 municipalities do not have private voucher schools. However, these schools are ubiquitous in urban areas.

14. 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 on request.

15. Lara et al. (2010, Tables A2 and A3) present the statistics for the sample of students who completed eighth grade in a private voucher school and had to switch schools and for all students who had to switch school after eighth grade.

16. Lara et al. (2010, Figures 1a and 1b) show the densities of the estimated propensity scores by school type and subject test. The densities display a very

similar mode with a difference of only 5 percentage points. They also display a common support.

17. The respective propensity score model estimation can be found in Lara et al. (2010, Table A4 in the appendix).

18. The covariates used are parental education, family income dummies, the 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.

19. 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 of Lara et al. (2010).

20. As an alternative check, we also used 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 eighth 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 on request.

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Authors

BERNARDO LARA is a young research associate at the Center for Applied Economics of the Department of Industrial Engineering at the University of Chile, and a PhD student in Economics of Education at Stanford University. República 701, Santiago, Chile; blara@ing.uchile.cl.

ALEJANDRA MIZALA is professor of economics at the Center for Applied Economics of the Department of Industrial Engineering and Academic Director of the Center for Advanced Research in Education at the University of Chile, República 701, Santiago, Chile; amizala@dii.uchile.cl.

ANDREA REPETTO is professor of economics in the School of Government at Universidad Adolfo Ibáñez, Diagonal Las Torres 2640, Penalolen, Santiago, Chile; andrea.repetto@uai.cl.