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Keywords: private schools, centralized school choice system, vouchers system
JEL codes: I22, I24, I28

The effectiveness of private schools in Chile: Evidence from centralized assignment lotteries

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I Introduction

Enrollment in private primary and secondary schools has grown steadily since the mid-nineties around the world. By 2019, in middle-income countries, 21% of primary (and 30% of secondary) education enrollment was in private institutions (The World Bank, 2023).[1] This push has been motivated by the need to increase enrollment rates quickly and commonly encouraged by the belief that the private sector is a more efficient service provider. However, it still remains largely unclear whether the private sector is capable of providing a public good—such as education—more effectively than the public sector. In this paper, we aim to answer this question for Chile, where a school voucher system fostering wide private-sector participation and school choice has been in place since 1981 and where currently more than a half of students are enrolled in private schools.

Chile's private voucher schools (PVSs)–institutions that are publicly funded but privately owned and managed—operate with far greater flexibility than traditional public schools (TPSs) in allocating inputs.[2] They own their capital, while TPSs do not (their capital belongs to the state). They can hire teachers under private-sector contracting laws, while public schools are subject to special legislation involving centralized collective bargaining and restrictions on teacher dismissal. They are allowed to charge tuition on top of the voucher provided by the government.[3] Finally, until recently, they could conduct arbitrary enrollment processes (Lara et al., 2011, Contreras et al., 2010). This flexibility, their advocates argue, allows PVSs to combine school inputs in a more efficient way and, thus, offer higher-quality education than public schools, directly improving their own students' outcomes and indirectly improving those of public schools students through competitive pressure.

The goal of this paper is to evaluate PVSs' effectiveness in improving academic outcomes. This is challenging because it is difficult to account for the role of omitted variables and selection in student outcomes given that students choose which schools to enroll in. To address these challenges, we leverage the introduction in 2016 of a centralized school choice system (CSCS), which mandates the use of lotteries to break ties in enrollment applications at oversubscribed schools. Through repeated iterations of the allocation algorithm, we can recover the deferred acceptance propensity score (DAPS), which represents the probability of PVS assignment. Since part of the DAPS is determined by randomization, its use represents an improvement over traditional propensity score matching (PSM) methods because, conditional on the DAPS, an offer of a PVS seat is as good as random (Abdulkadiroğlu et al., 2017). This allows us to compare the outcomes of students at schools of different types while conditioning on the probability of assignment and offer a credibly causal interpretation of the PVS attendance effect.

First, we focus on two cohorts of students who applied through the centralized school choice system to enter 1st grade in 2019 and 2020. From their respective application rounds, we record their propensity score (i.e., probability of being assigned to a PVS) and their assignment result (i.e., an indicator that equals one if they were assigned to a PVS and zero otherwise). Then, we compare the outcomes of TPS and PVS 4th graders (i.e., four years after school assignment by the CSCS), instrumenting PVS enrollment with the allocation by the algorithm while controlling for the propensity score and other relevant observables. We find that after four years of enrollment, the average differences in PVS and TPS student outcomes are precisely estimated zeros for both math and reading.

[1] In low-income countries, 13% (17%) of primary (secondary) school enrollment is private, although in populous outliers such as India and Pakistan—which have been subjects of academic interest (see, for instance, Bau (2022) and Muralidharan and Sundararaman (2015))—secondary private education accounts for 52% and 33% of enrollment, respectively.

[2] Fully private schools—those that are privately funded and managed—account for 10% of enrollment and are not studied in this paper mainly because they are excluded from the centralized school choice system (CSCS), which we leverage to overcome selection bias.

[3] Out-of-pocket fees are being gradually eliminated, as will be explained in Section III.

Second, we focus on the subset of students who, at the end of 8th grade, must switch schools due to their schools' grade configuration. These students must reapply through the CSCS to enter a new school in 9th grade; this gives us the opportunity to repeat the process described above, as students are reassigned to different schools. For some of these students, we observe their outcomes two years after reenrollment on their 10th-grade national standardized tests; for others, we observe their outcomes 4 years after reenrollment on their college entrance exams. In 10th grade, the average effect of PVS versus TPS attendance is 0.063σ in reading and 0.175σ in math. In 12th grade, the effect is 0.117σ in reading and 0.083σ in math. When we split the sample to examine the effects by gender and socioeconomic status (SES), we find the strongest effects for girls and nondisadvantaged students. In sum, PVSs appear to induce a causal improvement in academic outcomes for older students, albeit of a magnitude that is only a fraction of the observed differences.

Next, we explore possible mechanisms that might explain why the effects are null in 4th grade but sizable in 10th and 12th grades. We first consider whether families exert different levels of effort to support students' learning depending on the type of school to which they are allocated. We measure this using a question that asks parents how easy or difficult it is for them to support various aspects of schooling (e.g., technology, study habits, etc.). We find no differences between the responses of parents whose children attend PVSs and those attending TPSs. A second possibility is that some students who fail to gain admission to sought-after PVSs might decide to enroll in a fully private school. These schools charge high fees and do not receive any financial support from the government. We find evidence that this indeed occurs among high school students (but not primary school students), which induces negative selection into the group of students attending TPSs. This, in turn, implies that the sizable effects we observe might be biased away from zero, as the comparison group in TPSs ends up being composed of more students from poorer socioeconomic and academic background.

This paper contributes to the growing literature on the effectiveness of private schools, which has predominantly focused on charter schools in the United States using decentralized lotteries for identification. In contrast, this study leverages a centralized lottery system in Chile, a rising high-income country with one of the longest-standing nationwide school voucher programs. By doing so, it informs a broader policy debate on the expansion of private education, particularly in low- and middle-income countries, where private schooling has grown significantly in recent years (Baum et al., 2014). Given that Chilean PVSs operate similarly to publicly funded private schools in other contexts, such as *escuelas concertadas* in Spain, charter schools in the US, and grammar schools in the UK, these findings offer insights with potential relevance beyond the Chilean setting.

This study extends prior research on Chile's school voucher system by employing a novel identification strategy, leveraging a comprehensive dataset, and analyzing a policy environment where all students are eligible for vouchers. In contrast to the institutional features in studies on the US, which often rely on decentralized lotteries within specific districts, the centralized nature of Chile's system enhances the precision, external validity, and policy relevance of the findings. Furthermore, this research addresses key methodological limitations in previous studies by utilizing a centralized assignment mechanism, allowing a broader analysis beyond just applicants to PVSs. It also contributes to the literature on the use of the DAPS for identification and offers findings that contrast with those of prior studies reporting minimal or null effects of PVS enrollment in Chile. By adopting a more rigorous empirical approach, this study advances the understanding of private school management and its impact on student outcomes, complementing the international evidence on school choice and education policy.

The rest of this paper is organized as follows. Section II reviews the literature that has exploited both centralized and decentralized lotteries to estimate the effects of private schools in different contexts. In addition, it reviews the papers on the effectiveness of private education in Chile that use alternative identification strategies. Section III presents background information on both the school voucher system and the centralized school choice mechanism mandating allocation of seats by lottery. Section IV describes the data used in this paper. Section V presents summary statistics to describe the Chilean school environment. Section VI describes our empirical strategy and provides evidence supporting the identification assumption. Section VII discusses the results. Section VIII presents discusses mechanisms, and Section IX concludes.

II Related literature

This article contributes to a growing literature on private school effectiveness in general and specifically to the literature stream that uses admissions lotteries in different contexts to deal with endogeneity. In addition, it complements the findings of previous articles that have studied the case of Chile with different identification strategies. This mix of a novel identification strategy, a detailed dataset and a suitable context allows this study to extend the literature in the following directions.

First, it focuses on an upper middle income country school voucher system that has been in place for 40 years, making it one of the oldest in the world. This feature distinguishes this study from previous ones that use only particular places in the US that have recently allowed charter schools, for instance, Abdulkadiroğlu et al. (2011) in Boston, Angrist et al. (2013) in Massachusetts and Winters and Shanks (2021) in Newark. With the exception of the last one, these studies use decentralized lotteries as source of identification and use between 3 and 30 schools at a time.[4] The Chilean context, in which every student is eligible for a voucher to choose any school in any part of the country, increases the precision, external validity, and policy relevance of the findings.[5]

Second, this study solves two methodological limitations of previous studies that use decentralized lotteries.[6] On the one hand, they cannot examine impacts on students who do not apply (Chabrier et al., 2016) simply because the studies compare "one-shot" winners and losers among those who apply. The complexity of the Chilean centralized assignment mechanism induces variation in the probability of PVS assignment even for students who—while applying—do not rank these type of schools high on their application lists. Given the ubiquity of PVSs in the Chilean context and the fact that everyone is allowed to apply to them, this concern is alleviated.[7] On the other hand, previous studies with centralized and decentralized lotteries use records that might not be representative of the entire student population. The Chilean CSCS allocates students to both public schools and PVSs (fully private schools—which account for 10% of enrollment—are the only ones excluded from this enrollment process). Thus, concerns about

[4] Charter schools in the US are not entirely analogous to the Chilean PVSs. Charter schools operate as contractors to government agencies (called charter authorizers) that decide which charter schools open and whether they meet their contractual obligations and can continue receiving government funds (Harris and Larsen, 2023). Private voucher schools are essentially private firms that make their own entry and exit decisions and are subject to minimal oversight beyond curriculum requirements.

[5] Admittedly, however, given the staggered roll-out of the system, its match with the evaluations calendar and the need for nondegenerate probabilities of PVS assignment, in the best case, we use data from students attending between 1,700 and 2,900 schools, out of 11,000 operating in the country.

[6] Decentralized lotteries have been widely used to explore the effectiveness of charter schools in the US (Angrist et al., 2013, 2016) and occasionally to estimate the impact of attending sought-after public schools in contexts of choice (Cullen et al., 2006; Deming et al., 2014).

[7] Among the applicants in 2019, 85% included a PVS in their submitted lists of preferences. This alleviates to a great extent the concern that the local average treatment effects (LATEs) we recover overstate the potential effects for nonapplicants (Walters, 2018) precisely because virtually every student applies to a PVS.

sample selection due to incomplete lottery reports (Abdulkadiroğlu et al., 2011) or bias due to selective participation of schools in the study (Clark et al., 2015) or of schools in the seat allocation process (Winters and Shanks, 2021) are alleviated.

Third, this study adds to a growing literature that uses the DAPS embedded in centralized matching lotteries as a source of identification. Abdulkadiroğlu et al. (2017) introduced this methodological innovation and used it to study the effect of charter school attendance in Denver, where they find large effects in math, reading and writing. Morrill et al. (2022) use it to study the effect of magnet school attendance in Wake County, North Carolina; they conclude that magnet schools benefit student learning and engagement. Winters and Shanks (2021) study the effect of charter school attendance in Newark, New Jersey; they find positive effects in math and reading.

Fourth, we contrast the results of previous literature that find small or null effects of PVS enrollment on student outcomes in Chile using older data and identification strategies that raise different concerns. For instance, Hsieh and Urquiola (2006) use panel data at the municipality level; they include controls for preexisting and concurrent trends and instruments to deal with nonrandom entry of PVSs. They find no evidence of positive impacts on educational outcomes; they find increased sorting. Anand et al. (2009) use PSM to build treatment and control groups; such a strategy relies on the researcher assuming that selection can be accounted for only on the basis of observables, which might not hold if parents seeking private voucher enrollment advocate more to enroll their children in those schools and, at the same time, put more effort into their school progress. The authors find that students attending fee-charging PVSs perform 0.2σ higher than those attending public schools and no different from students attending free PVSs. Lara et al. (2011) exploit "structural switches" of students enrolled in schools in which the terminal grade is the 8th; doing so allows the researcher to break the selection channel only under the strong assumption that PVS enrollment is "as good as random" after previous academic performance is controlled for. They find small, sometimes not statistically significant, differences in academic performance between PVSs and TPSs. We extend this literature by using an empirical approach offering a more plausible interpretation of causal effects.

Finally, we contribute to the more general literature studying private schools in different contexts. Advocates of private management cite efficiency as a reason for encouraging it; this means that under certain conditions (e.g., strong government regulation, accountability mechanisms), students in private schools should outperform their TPS peers on different outcomes (Patrinos et al., 2009). Fryer (2014) tests this idea by randomly implementing a bundle of best practices from high-performing charter schools in low-performing, traditional public schools in Houston, Texas. He finds positive effects in math but little effect in reading. Bloom et al. (2015) study the management practices of schools across different countries nonexperimentally; they conclude that more autonomous schools have better managerial outcomes than TPSs, which are explained mainly by leadership and governance. Abdulkadiroğlu et al. (2016) take advantage of charter takeovers in Boston and New Orleans to study the effect of "passive" enrollment in charter schools. In both cases, the takeovers induce substantial gains in math and English language arts. Romero et al. (2020) study the outsourcing of Liberian schools; after one academic year, students in outsourced schools scored 0.18 standard deviations higher in English and math. Studying the Chilean system complements this evidence, as PVSs are fully private entities funded by state vouchers and have a high degree of freedom to implement their own educational practices, subject only to the national curriculum set by the government. The next section further explains how these schools work and how exactly they differ from TPSs.

III Institutional details

III.I School voucher system

In 1981, Chile implemented a universal school voucher system with the objective of increasing enrollment rates and educational quality by fostering competition among schools. The reform devolved the administration of public schools from the ministry of education (i.e., the central government) to municipalities (the smallest political-administrative division in the country), thereby decentralizing schools' management. Additionally, it allowed civil society organizations (NGOs, entrepreneurs, etc.) to become *sostenedores* and open schools wherever they decided to do so. All students became eligible for a voucher allowing them to choose to enroll in any school, either TPS or PVS. TPSs and PVSs would compete for enrollment to receive the voucher amount and fund their operations. This gave a strong push to the private subsidized sector, whose enrollment share rose from 15% in 1991 to 54% in 2023.

The basic features of the system remained unchanged until 2016, when a reform introduced three key changes.[8] First, it forbade out-of-pocket fees, which critics of the status quo argued contributed to socioeconomic segregation of the system (Valenzuela et al., 2014), as students from families who could pay similar fees largely ended up in the same schools. In practice, the government is gradually increasing the voucher amount over time to cover the difference between the current voucher and schools' sticker price, such that all schools will become free under the full regime around 2030 (and schools will experience no change in funding).[9] Second, it forbade for-profit schools in an aim to better align incentives with the provision of quality. With this change, previously for-profit schools must decide whether to become fully private—and give up government vouchers to remain for-profit—or change their legal status to "educational societies" and reinvest any surplus they obtain. Finally, it forbade schools from selecting students, which sought-after schools did to select the most advantaged (Contreras et al., 2010).[10] It did so by implementing a CSCS to allocate students to both types of schools.

Currently, PVSs and TPSs are similar in several aspects. For instance, none can turn away students, as the enrollment process is centralized, and both have to adhere to the same curriculum. Furthermore, the value of the voucher is the same for both school types; it varies only based on factors such as education level, provision of special programs, rural location, and students' "disadvantaged" status. Thus, conditional on school and student characteristics, TPSs and PVSs receive the same public funding.[11] The main differences between the two types of schools are that PVSs, similarly to firms in other private sectors of the economy, make their own entry and exit decisions, own their own capital, and make staffing decisions.[12] Furthermore, PVSs are free from the political pressures that TPSs face, as the latter are overseen by local authorities. In terms of educational practices, these differences could manifest in more flexibility on aspects rangings from the setting of school hours to the mode of instruction. More generally, these differences allow PVSs to operate more independently (or with fewer restrictions) than TPSs and be innovative and hence more effective in improving academic outcomes. Table 1 summarizes the similarities

[8] Another important change occurred in 2008, when disadvantaged students started receiving twice the value of the regular voucher.

[9] In our empirical analysis, we measure outcomes in 2022 and 2023, when some schools might still have charged copayments. As we mentioned, these tend to be low because the reform mandates that they fall over time.

[10] The combination of selection policies and copayment applied by voucher schools created a cream-skimming effect, leaving more disadvantaged students in public schools (Hsieh and Urquiola, 2006).

[11] Private funding could vary, however, when PVSs could charge out-of-pocket fees.

[12] This is true on the extensive and intensive margins. In PVSs Teachers can be fired (with compensation) if the school, like any other private firm, needs to do so. Similarly, the numbers of hours worked by any teacher can be adjusted flexibly, which is much more difficult in TPSs.

and differences. We also add fully private schools, as a reference.

Table 1: Differences between school types in Chile

	Traditional public	**Private voucher**	**Fully private**
Who owns school building?	Municipality	*Sostenedor*	*Sostenedor*
Who hires and pays teachers?	Municipality	*Sostenedor*	*Sostenedor*
Who sets curriculum?	Ministry Educ.	Ministry Educ.	Ministry Educ.
Can select students?	No	No	Yes
Charges out-of-pocket fees?	No	Yes*	Yes
Receives public funding	Yes	Yes	No

Note: *Sostenedor* is the term used in the Chilean system's nomenclature to refer to the person (natural or legal) in charge of the school (above the principal). $*$ Gradually phasing out as the governments increases the voucher amount (and sticker prices fall) until approximately 2030. These differences are observed from 2016.

III.II The Chilean centralized school choice system

The introduction of the CSCS organized the school enrollment process, which was previously uncoordinated and decentralized.[13] Under it, families had applied separately to schools, and these had—when their market power permitted—leverage to decide which students to admit, often on the basis of arbitrary processes such as interviews and admission exams aimed at selecting the most appealing students. The aim of the new mechanism was to introduce transparency and fairness to the process by mandating that all applicants to publicly funded schools (i.e., both TPSs and PVSs) go through the same web platform under publicly known rules.[14] Under the full regime since 2020, the CSCS has nationwide coverage and considers all fourteen levels of school education, ranging from prekindergarten to 12th grade.[15]

In practice, families access an online platform containing all the relevant information on schools such as number of vacancies, educational project, internal school regulations, school hours, and copayments (applicable in some PVSs).[16] They are allowed to apply to as many schools as they want and are not constrained on location, religion, or SES.[17] After families submit their rank-ordered lists of preferences for schools, the system runs the deferred acceptance mechanism algorithm, which aims to assign each student to her top preference provided that there are enough seats available (Abdulkadiroglu and Sönmez, 2003). If the number of applicants to a given school is lower than the number of available seats, all applicants are admitted, unless they can be reallocated in a more preferred school.[18] However, if the number of available seats is lower than the number of applicants, the system generates lists of students for each school taking into account three priority groups, quotas and a random number (drawn at the school level) to break ties.

[13] This section draws heavily upon Correa et al. (2021).

[14] Fully private schools are allowed to run independent admission processes. In addition, schools that run special teaching, education for adults and hospital-schools, which represent a minor share of total enrollment, are allowed to use their own application mechanisms.

[15] The adoption of this system occurred in a staggered manner across the 16 regions of the country over 4 years, starting with the smallest regions, followed by 4 and then 10 other regions during the second and third years, respectively, with the capital region of Santiago added in 2020.

[16] In strict terms, families apply not to a school but to a tuple of a school, school schedule and type of teaching. We use the term "school" hereafter for ease of exposition.

[17] Note that the fact that the list length is unconstrained rules out potential strategic behavior by families in choosing schools (Haeringer and Klijn, 2009). In other words, the incentives are such that families have nothing to gain from not reporting their preferences truthfully.

[18] The number of available seats is calculated as the total number of seats for each grade, minus the number of students advancing towards that grade, minus the number of students held back.

The algorithm runs in multiple consecutive iterations, each of which we can better explain by describing it in two stages.

In the first stage, the system generates lists of applicants ordered by the system's priorities and within them by their random number, in the following order[19]: 1) students with siblings already enrolled in the school, 2) disadvantaged students up to the system quota of a 15% enrollment share, 3) students whose parents work at the school, 4) former students of the school, and 5) the remaning students with no priority status.[20] Importantly, a student will have as many random numbers as schools to which she applies and will be placed on the same number of lists.

In the second stage of the process, students are assigned to schools on the basis of the ordered lists according to the number of available seats. Rejected students have their first choice erased and are assigned their second choice as the first one for the following iteration. In this next iteration, new lists are generated with the schools that still have seats and including the students rejected in the first iteration who declared as second choice a school that still has seats. Thus, the algorithm continues until there are no more rejected applications.

Once the algorithm allocates seats, the results are made public, and families can either accept or reject the school to which they were assigned. Families who refuse to accept their assigned school, who were not assigned (for instance, because they applied to very few schools), or who did not apply can reapply in a complementary process with the same features as the regular one. Since the vast majority of seats are allocated in the regular process, we use data only from it.

As a way of illustrating the process, in the 2019 application cycle (for enrollment in 2020), there were 483,070 applicants in the regular stage. The average number of preferences reported by these applicants was 3.48, and the median applicant submitted three preferences. They were allocated to one of 4,984 schools. Fifty-two percent of applicants accepted the allocation given by the algorithm, while 13% accepted it but were placed on a waiting list for a more preferred choice. Only 7.2% rejected the initial allocation, and the rest either did not respond or were not allocated. This latter group had the option of re-entering the complementary process under the same rules but with a reduced number of school alternatives. In 2019, 87,604 students applied in the complementary process.

IV Data

We use data from the CSCS to obtain the DAPS, administrative records at student and school levels, national standardized tests scores (SIMCE), and college entrance exams (PAES). We provide further details on each data source below.

Centralized school choice system records. Available for the years between 2016 and 2022, the CSCS records contain students' rank-ordered list of preferences for schools, indicator variables for the system priorities for students (disadvantaged status,[21] having a sibling in the school, having a parent working in the school, being a former student of the school), tie-breaker lottery numbers, the result of the admissions

[19] In a parallel stage, separate lists are generated for high-achieving schools, early specialization schools, and special needs schools (PIE, for the acronymum in Spanish). In the first case, a list is generated for the 30% of seats reserved for applicants from the top 20% of the performance distribution in their previous school. For the other two cases, schools are free to use their own criteria to fill all their available seats. While we use these applications to replicate the admissions process (otherwise we would not be able to replicate the actual assignments, as the application outcomes are interdependent), we drop the students in these schools from our estimations.

[20] Note that, in contrast to several other centralized school assignment systems (for instance, Mexico's and Ghana's), the Chilean algorithm does not consider distance to the school or previous academic performance.

[21] The law defines disadvantaged ("priority" in the Chilean nomenclature) students as those "who face socioeconomic

process (i.e., what school each student was assigned to) and schools' number of available seats.[22] These data allow us to replicate multiple times the DA assignment of students to schools using different random numbers to calculate one propensity score for each student the year she applies through the CSCS. Appendix B explains how we obtain the propensity scores.

Administrative records. These contain various observed characteristics for years spanning 2016 to 2023. At the student level, these data contain age, gender, socioeconomic level, school grade, and annual grades. At the school level, they contain an identifier for whether it is in a rural or urban area, administrative dependency (i.e., whether it is a TPS or PVS), number of students, number of teachers, years of experience of teachers, and their number of teaching hours.

Primary- and secondary-school academic outcomes. We use standardized tests scores from the official Chilean testing system (SIMCE, for its acronym in Spanish), which all students in the corresponding grade, regardless of school type, must take in early October of each year. Every year, the government tests different grades; Appendix Table 12 details what grade–year combinations we use. This dataset also contains self-reported parent education, income level, and other subjective questions. Importantly, the questionnaire that parents must answer asks them how easy or difficult it is for them to support students in a range of school-related activities; this allows us to proxy for the level of effort parents put into their children's education. Given the intersection between the roll-out of the system and the SIMCE evaluation calendar, we use two cohorts of 4th graders (2022 and 2023) and two cohorts of 10th graders (2022 and 2023 as well). For the 10th graders, we also observe their past outcomes from 4th grade (2016 and 2017). These tests are low stakes in the sense that they are anonymous (individual results are never released to the public) and have no consequence for students. However school averages are publicly announced as a way to inform parents about schools' quality and to allocate a small funding incentive for the better-performing schools; in this sense, they can be thought of as high-stakes for schools.

College entrance exams. We use Prueba de Acceso a la Educación Superior (PAES) test scores for both math and reading from December of each year. These tests are not mandatory and are administered subject to a fee, but since it is waived for poor students, in practice, 72% of the students graduating each year take it. Different programs differently weight each test—along with high school grades—to rank students and determine admissions. These tests consist of multiple-choice questions and are taken over two consecutive days. We use the tests from 2021, 2022 and 2023. Importantly, for the cohorts who took it in 2021 and 2023, we also observe their past SIMCE outcomes from 8th grade (2017 and 2019, respectively), while for the 2022 cohort, we use their 6th-grade (2016) outcomes.

V Descriptive Statistics

Figure 1 depicts the distribution of tests scores in reading and math for the two exams for which we have data. We standardize the tests for each year, subject, and level to have mean equal to zero and

obstacles to succeeding academically." The ministry of development identifies such students as those who meet at least one of three criteria. The first is enrollment in the Chile Solidario Social Protection System, Ethical Family Income (IEF) program or the Security and Opportunities System. These three initiatives are all means-tested programs through which the ministry of development targets poverty alleviation resources. The second criterion is belonging to a household in the poorest third of the Social Registry of Households, a registry of the near universe of households in Chile. The third criterion is belonging to section A of the public health insurance scheme (Fonasa), which targets the poorest families in Chile by providing free access to health care. The set of these three criteria seeks to capture socioeconomic obstacles to school achievement.

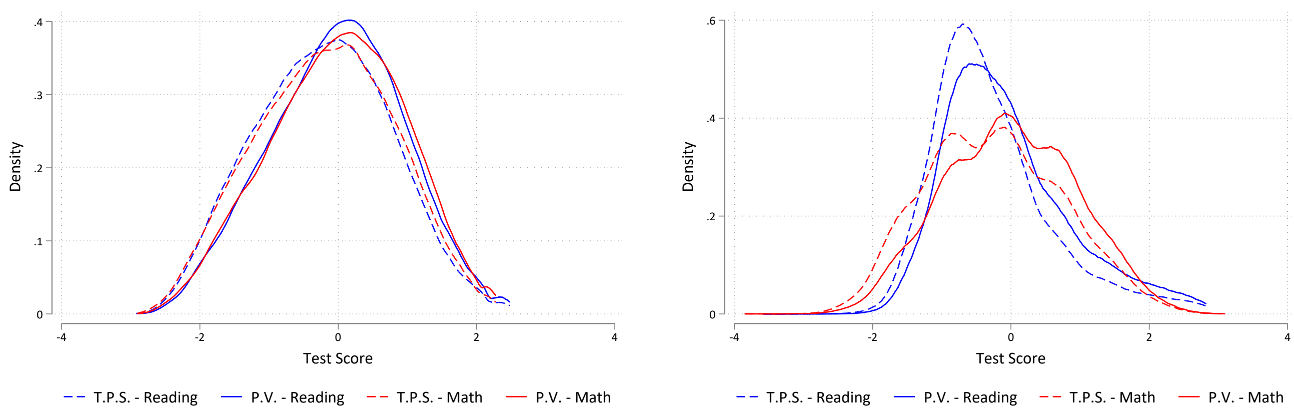
[22] In Chile, the school year takes place between March and December of each year. Therefore, students seeking to enroll in school year (coinciding with calendar year) t apply during year $t-1$.

standard deviation equal to one. Panels (a) and (b) show that both in the 8th-grade and college entrance exams, students in PVSs (shown by the continuous lines) outperform their peers in TPSs (shown by the dashed lines). The average for PVS students is .203σ higher in math (and .185 σ higher in reading) than that for TPS students in the 8th-grade tests scores (Panel (a)). These differences appear even higher in college-entrance exams (Panel (b)). These patterns are often highlighted in the public debate discussions as a proof that PVSs provide better education quality than TPSs, although the patterns certainly might reflect a wide array of factors that correlate with PVS enrollment, such as family inputs.

Figure 1: Student-level distribution of tests scores, by school administrative dependency and subject

(a) 8th grade (SIMCE)

(b) 12th grade (PAES)



Note: Student-level kernel distributions of tests scores. Panels (a) uses SIMCE outcomes of all students in 8th grade in 2023 (N=183,504). Panel (b) uses PAES (college entrance exam) outcomes of all students who took the test in 2023 (N=207,920). Dashed lines correspond to students in TPS and continuous lines to students in PVSs. Tests scores are standardized to have mean equal to zero and variance equal to one in each grade and year.

Table 2 aims to illustrate the differences and similarities between PVSs and TPSs on a wide range of school- and student-level observed characteristics. Panel (a) presents school-level characteristics for the universe of Chilean schools functioning during 2022. We observe that PVSs have higher student–teacher ratios than TPSs. This is due mainly to the fact that PVSs tend to have more students than TPSs (note that the average number of teachers is only slightly higher in TPSs).[23] In turn, this can be linked to the fact that among TPSs, a high share are located in rural areas, whereas only a minor share of PVSs are rural. Regarding teachers, a higher share of hours is taught by women in PVSs than in TPSs. In addition, PVS teachers are less experienced than those in TPSs, which can be linked to the rigidities in public-sector contracting laws.

Panel (b) shows the characteristics of students taking the SIMCE tests in 4th grade. Those in PVSs perform approximately .2σ higher than those in TPSs in reading and math. The student body is balanced in terms of gender, but in TPSs, a slightly higher share of students self-identify as part of an ethnic group, and 18% of students in TPSs are foreigners, while only 8% in PVSs are. When parents are asked whether they think that their students will reach college, a higher share of them answer affirmatively in PVSs. Regarding the ability to support students in various tasks such as dealing with technology (e.g., installing an app), organizing times and places to study, setting strategies and habits, and help with school (homework and exams preparation), the differences between the two types of schools in the shares of parents who support

[23]Note also that the dispersion in school size among PVSs is enormous because PVSs tailor educational projects to schools that can be very small, such as arts or Montessori schools.

students is not very large.[24] Regarding variables that capture socioeconomic background, more meaningful differences emerge: 28% of TPS students (versus only 17% in PVSs) fall within the lowest income bracket (out of 5); 56% of PVS students (versus 37% of those in TPSs) have a father with college education; and 61% of PVS students (versus 74% in TPSs) are disadvantaged, which broadly implies that they belong to a household with income in the bottom third of the income distribution.

Panel (c) contains the characteristics of students taking the SIMCE tests in 10th grade. The observed differences in tests scores in favor of PVS students widen with respect to those in 4th grade. The student body composition does not change much, although apparently a smaller share of TPS students are foreigners in 10th grade than in 4th grade. The strength of parental support with activities related to school weakens in 10th grade, particularly for students in TPSs. Finally, panel (d) contains the characteristics of students taking the college entrance exams. Once again, the observed differences in tests scores are approximately $.2\sigma$ for both reading and math. Grades (labeled "GPA" in the table)—which in Chile range between 1 and 7—are somewhat better among PVS students, although these might not necessarily reflect objective learning. Interestingly, a higher share of girls than boys take the tests in both TPSs and PVSs.

VI Empirical strategy

Our goal is to estimate the effect of PVS attendance on students' academic outcomes vis-à-vis their outcomes under TPS attendance. The key challenge we face is that students attending PVSs and TPSs might differ in unobserved ways that we cannot account for. In other words, students might self-select into different types of schools, thus confounding the effect of their attending a given type of school with the effect of variables that determine enrollment. To overcome this challenge, we exploit the quasi-random assignment embedded in the CSCS. In particular, we instrument PVS *enrollment* with PVS *assignment* by the system while controlling for the student's probability of being assigned to a PVS. Doing so allows us to compare PVS students to TPS students with an equal probability of being offered a PVS seat. Abdulkadiroğlu et al. (2017) show that controlling for the propensity score obtained within the DA assignment mechanism allows the researcher to draw causal conclusions from comparisons between treated and untreated units.

We obtain the probability of a student's being offered a PVS seat by running the DA algorithm 10,000 times for each admission year with different random tie-breaking number draws while holding student preferences, system priorities and school vacancies constant. In each of these simulations, we store the number of times a student is allocated to a PVS; then, we divide this number by 10,000 to obtain the DAPS, which captures her probability of being assigned to any PVS in each admission process. Figure 2 in Appendix A shows a histogram of our estimated DAPS for the cohorts entering 1st grade in 2019 and 2020 and for the cohorts forced to switch schools (due to grade configuration) into 9th grade in 2021 and 2022, by assignment status. Reassuringly, for both groups of students, the DAPS distribution is skewed to the right for the students who are offered a PVS seat and to the left for those who are not. The overlap of both distributions allows us to compare the outcomes of assigned and unassigned applicants

[24] More precisely, in 2022 and 2023, parents were asked "How easy or difficult is it for you to support your son or daughter in the following school aspects?: (i) Helping him/her solve problems with technology (for example, connecting to the Internet, installing an application, downloading or sending files, etc.); (ii) Helping him/her organize his/her day with times and places to study; (iii) Helping him/her with study habits and strategies; (iv) Supporting him/her in his/her school activities (for example, with his/her homework, studying for tests, etc.)." Valid response options were "easy," "difficult," and "I don't know how to do it." We report the share who find it easy.

Table 2: Descriptive statistics – 2022

Panel A: Schools				
	PVS		TPS	
	Mean	SD	Mean	SD
Student/teacher ratio	18.7	8.17	10.51	5.52
Number of teachers	17.63	19.84	19.26	18.12
N students per school	347.72	445.71	248.99	300.15
Teaching hours female (1)	.82	.2	.72	.24
Teacher years experience	13.6	6.34	15.24	5.47
Rural(2)	.13	.34	.51	.5
Number of Observations	5672	5672	5193	5193
Panel B: Students - 4th grade SIMCE				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized Math	.02	.96	-.22	.98
Standarized Lang	.03	.98	-.18	.99
Male students	.49	.5	.5	.5
Any ethnic group (3)	.13	.34	.16	.37
Foreign students	.08	.27	.18	.38
Expects college (4)	.86	.34	.79	.41
P.A. Technology (5)	.81	.39	.74	.44
P.A. Organize studying (5)	.85	.36	.85	.36
P.A. Study habits and strategies (5)	.81	.39	.82	.39
P.A. School activities (5)	.88	.33	.86	.34
Lowest income bracket (6)	.17	.38	.28	.45
Highest income bracket (6)	.07	.25	.02	.14
Father went to college	.56	.5	.37	.48
Disadvantaged(7)	.61	.49	.74	.44
Number of Observations	104312	104312	66651	66651
Panel C: Students - 10th grade SIMCE				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized math	.05	.96	-.26	.89
Standarized lang	.07	.97	-.17	.97
Male students	.49	.5	.5	.5
Any ethnic group (3)	.13	.33	.17	.38
Foreign students	.08	.27	.14	.35
Expects college (4)	.86	.35	.76	.43
P.A. Technology (5)	.57	.5	.48	.5
P.A. Organize studying (5)	.78	.41	.74	.44
P.A. Study habits and strategies (5)	.73	.44	.69	.46
P.A. School activities (5)	.73	.44	.69	.46
Lowest income bracket (6)	.3	.46	.5	.5
Highest income bracket (6)	.04	.19	.01	.11
Father went to college	.49	.5	.31	.46
Disadvantaged(7)	.59	.49	.72	.45
Number of Observations	85470	85470	52588	52588
Panel D: Students - College entrance exam PAES				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized Math	-.06	.89	-.25	.83
Standarized Lang	-.01	.94	-.25	.98
GPA(8)	5.85	.76	5.76	.95
Male students	.45	.5	.45	.5
Number of observations	158468	158468	94183	94183

Notes: (1) Share of teaching hours taught by female teachers. (2) Share of rural schools. (3) Share of students who consider themselves as belonging to an ethnic group. (4) Share of students whose parents expect them to obtain a bachelor's degree or higher level of education. (5) PA (parental assistance) questions refer to the share of students whose parents report finding it easy to help them in each topic. (6) Household income in a typical month used in brackets: [0 to 300,000 CLP] = 1, [300,001 to 600,000 CLP] = 2, [600,001 to 1,200,000 CLP] = 3, [1,200,001 to 1,800,000 CLP] = 4, > 1,800,001 CLP = 5 (CLP ≈ USD 900). (7) Disadvantaged denotes a student in a household with income in the bottom third of the income distribution. (8) GPA in Chile ranges between 1 (minimum) and 7 (maximum). Observations from academic year 2022. Table 2 in the Appendix presents descriptive statistics for year 2023.

with similar propensity scores; we exclude students with a degenerate DAPS (i.e., equal to either 0 or 1) from our estimation sample because, for instance, for two students with a DAPS equal to zero, neither could be observed being offered a PVS seat.[25] Appendix B documents the details on how we replicate the DA mechanism and produce the propensity score.

Once we obtain the DAPS, we estimate the following two-stage least squares (2SLS) system with three different samples: 4th graders, 10th graders, and 12th graders:

$$\text{Test Score}_{it} = \alpha + \beta \mathbb{X}_{it} + \rho \widehat{\text{Private Voucher}}_{it} + \phi \hat{p}_{it} + \varepsilon_{it} \quad (1)$$

$$\text{Private Voucher}_{it} = \mu + \gamma \mathbb{X}_{it} + \pi \text{Allocated}_{ig\tilde{t}} + \psi \hat{p}_{i\tilde{t}} + \nu_{it} \quad (2)$$

where Test Score$_{it}$ corresponds to the reading or mathematics test score in SIMCE (the primary and secondary standardized tests) or PAES (the college entrance exam) of student i in year t. Private Voucher$_{it}$ is an indicator that equals one if student i is enrolled in a PVS in year t and zero otherwise; Allocated$_{i\tilde{t}}$ is an indicator that equals one if student i was allocated by the DA algorithm to a PVS in baseline year $\tilde{t}$.[26] $\mathbb{X}_{it}$ is a vector of observed characteristics containing gender, father's education, number of people in the household, and a five-category income variable; it is included to increase precision. $\hat{p}_{i\tilde{t}}$ is the DA propensity score for student i during the admission process of year $\tilde{t}$.

Our empirical strategy corresponds to 2SLS augmented by the DA propensity score. The "Allocated" instrument is relevant, in theory, as it increases the probability of a student's being enrolled in the follow-up year (in which we measure outcomes), and it satisfies the exclusion restriction in that it does not impact academic achievement through any means other than enrollment. In the next section, we show empirically that this first stage is valid. Under the assumption that after conditioning on a rich set of covariates any remaining variation in tests scores is random, we interpret ρ in Equation 1 as the causal effect of PVS relative to TPS attendance. Notwithstanding this, note that the exclusion restriction could still be violated if families failing to gain access to PVSs respond in differential ways to their allocation outcome. We explore to what extent this concern might invalidate our results in Section VII.

VI.I Assessing the validity of the DAPS

The DAPS allows us to rule out the influence of unobserved determinants of PVS enrollment on academic achievement. To lend credibility to our claim that its use can indeed yield a causal interpretation of the estimated differences between PVS students and TPS students with an equal probability of PVS assignment, we conduct the following balance test. We run a regression trying to predict PVS assignment as a function of a set of observed characteristics fixed at the moment of application to the CSCS. We do so first for students applying to enter 1st grade (whose outcomes we observe when they are in 4th grade) and then for students applying to enter 9th grade (whose outcomes we observe when they are in 10th grade). The results are shown in Table 3.

In Column (1), we include all applicants to 1st grade; the estimates show that fathers' education strongly predicts PVS assignment. In addition, students self-identifying as part of an ethnic group, foreigners and socioeconomically disadvantaged students are less likely to be allocated to PVSs, ceteris paribus. In Column (2), we use the subset of students for whom we obtain an estimate of the DAPS, excluding those with a degenerate risk (i.e., a DAPS equal to zero or one); with this smaller sample (which we call

[25]Similarly, for two students with DAPS equal to one, neither could be observed not being offered a seat.

[26]The baseline year for 4th graders is the year when they apply to enter 1st grade; for 10th and 12th graders, it is the year when they apply to enter 9th grade in a different school due to grade configuration. Refer to the calendar in Appendix Table 12 for a visual clarification.

Table 3: Probability of PVS assignment as a function of applicant characteristics

	4th grade		10th grade	
	(1)	(2)	(3)	(4)
	All applicants	Compliers	All applicants	Compliers
Male	0.00122	0.0157***	-0.0150***	0.00528
	[0.00452]	[0.00591]	[0.00427]	[0.00557]
Father went to college	0.0764***	0.0181***	0.0664***	0.0296***
	[0.00479]	[0.00623]	[0.00495]	[0.00616]
Ethnicity	-0.0385***	-0.0126	-0.00624	-0.0164**
	[0.00599]	[0.00820]	[0.00543]	[0.00734]
Foreign	-0.157***	-0.00514	-0.0268*	-0.0278
	[0.0128]	[0.0151]	[0.0142]	[0.0186]
$[300,001 - 600,000]$ CLP	0.0348***	0.00592	0.0738***	0.0209***
	[0.00631]	[0.00874]	[0.00469]	[0.00616]
$[600,001 - 1,200,000]$ CLP	0.0918***	0.0265***	0.103***	0.0376***
	[0.00708]	[0.00945]	[0.00786]	[0.00948]
$[1,200,001 - 1,800,000]$ CLP	0.137***	0.0412***	0.130***	0.0298
	[0.0114]	[0.0143]	[0.0223]	[0.0246]
$>$ 1,800,001 CLP	0.165***	0.0598***	0.0383	-0.0259
	[0.0150]	[0.0182]	[0.0370]	[0.0451]
Disadvantaged	-0.0258***	-0.0121*	-0.0336***	-0.0122*
	[0.00505]	[0.00637]	[0.00502]	[0.00624]
D.A.P.S.		0.984***		1.016***
		[0.00894]		[0.00848]
Number of observations	44394	14588	51796	17268

Note: Regressions using the assignment indicator as dependent variable and the reported set of variables as covariates observed at baseline. Standard errors in brackets *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. All applicants in Column (1) correspond to students entering 1st grade through the CSCS in 2019 or 2020. All applicants in Column (3) correspond to students who must switch schools (due to grade configuration) by applying through the CSCS to enter a new school in 9th grade. In both cases, the compliers in Columns (2) and (4) are the subset of them who have a DAPS $\in (0,1)$ and have nonmissing observed characteristics. These samples are used for the estimation of Equations 1 and 2.

"compliers"), the estimates shrink notably and, in some cases, lose significance. The analysis in Columns (3) and (4) for the students forced to switch after 8th grade is analogous. Observed characteristics predict PVS assignment, but their predictive power—particularly that of socioeconomic variables—disappears when we use the sample of compliers and after we control for the DAPS. In other words, this subset of students along with the DAPS can be used to recover the causal impact of PVS attendance.[27]

VII Results

We estimate the system of Equations 1 and 2 in three different samples. First, we measure the outcomes in 4th grade for students who entered school in 1st grade. Second, we measure the outcomes

[27]Appendix Table 14 shows that the sample of compliers is somewhat different from the whole population on key characteristics, such as reading tests scores, but not different on others, such as gender and ethnicity. This is relevant for external validity and potential policy lessons that can be drawn from our estimation sample to the universe of Chilean students.

of 10th graders forced to switch schools between 8th and 9th grade due to grade configuration. Finally, we measure the outcomes of 12th graders who were also forced to switch schools between 8th and 9th grade. The estimates for these three samples have different interpretations, as we measure the differences between students in different types of schools after a different number of years: The 4th graders are treated over 4 years, the 10th graders are treated over 2 years, and the 12th graders are treated over 4 years. We show the results below for each sample.

VII.I Fourth-grade outcomes

Table 4 shows the results of estimating the system of Equations 1 and 2 with the sample of 4th-grade students. We control for gender and indicator variables for whether the student's father has a college education, whether the student self-identifies as part of an ethnic group, whether the student is a foreigner, and whether the student is disadvantaged; we also include a 5-category household income variable. We cluster the standard errors at the school level in all specifications. Column (1) shows the results of the first stage; the probability of a student's being enrolled in a PVS in 4th grade after receiving an offer to enter a PVS in 1st grade increases by almost 43 percent, once we control for the DAPS. In Column (2), we report the intent-to-treat parameter using the centralized PVS assignment (and controlling for the DAPS). We find a very small and insignificant effect; this means that being awarded a PVS seat does not impact SIMCE outcomes.[28] More importantly, when we instrument PVS attendance with the assignment variable, the resulting 2SLS coefficients are again small and insignificant (Columns (3) and (4)); for the reading test, we obtain a nonsignificant negative 0.004σ effect, and for math, we find a nonsignificant 0.026σ effect. In other words, PVS attendance, relative to TPS attendance, does not have any effect on academic outcomes after four years of enrollment.

Table 4: Effect of PVS attendance – 4th-grade sample

	First Stage	Reduced Form	2SLS	
	(1)	(2)	(3)	(4)
		Reading	Reading	Math
P.V. Allocated	0.427***	-0.00198		
	[0.0129]	[0.0243]		
P.V. Enrolled			-0.00464	0.0266
			[0.0569]	[0.0608]
Obs	14588	14588	14588	14756
N° of schools	2833	2833	2833	2845

Note: Estimates of Equations 1 and 2. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who applied through the CSCS to enter 1st grade in 2019 or 2020 and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level.

[28] We report the first-stage and reduced-form results for the sample used to estimate the effects in the reading exam; for the math exam, they are virtually equal.

VII.II Tenth-grade outcomes

We perform a similar exercise by comparing the outcomes of PVS and TPS students in 10th grade. In this case, we leverage that students in 8th grade in schools that do not offer 9th grade must switch by applying through the CSCS. In this case, the interpretation of our estimates changes, as the effects we measure correspond to outcomes two years after (not 4 years after, as with the 4th-grade students) being enrolled in a PVS versus a TPS. Table 5 shows the results. According to Column (1), receiving an offer for a 9th-grade seat at a PVS increases the student's likelihood of being enrolled in a PVS in 10th grade by 64.5%, once we control for the DAPS (i.e., the probability of being offered a seat).[29] Column (2) reports the intent-to-treat effect, according to which receiving an offer increases tests scores by 0.074σ in reading. More importantly, Columns (3) and (4) report the 2SLS coefficients; we find that after two years of PVS "treatment," students perform 0.115σ and 0.219σ better than their TPS peers in reading and math, respectively. Furthermore, for this subset of students, we can control for the lagged tests scores observed while they were in 4th grade (six years before we measure their outcomes in 10th grade). The results do not change significantly, which is not surprising given the argument that all pretreatment observed characteristics are balanced across students due to the inclusion of the DAPS. The results remain positive, although the point estimates drop, particularly for the reading exam. Overall, we interpret them as evidence that PVSs do induce arguably causal positive effects on academic outcomes, albeit of only a fraction of the size of those observed in the simple population mean differences.

Table 5: Effect of PVS attendance – 10th-grade sample

	First Stage	Reduced Form	2SLS		2SLS V.A.	
	(1)	(2)	(3)	(4)	(5)	(6)
		Reading	Reading	Math	Reading	Math
P.V. Allocated	0.645***	0.0740**				
	[0.0133]	[0.0288]				
P.V. Enrolled			0.115**	0.219***	0.0631*	0.175***
			[0.0446]	[0.0445]	[0.0344]	[0.0368]
Obs	17288	17288	17288	17461	17149	17409
N° of schools	1847	1847	1847	1854	1843	1854
V.A.	No	No	No	No	Yes	Yes

Note: Estimates of Equations 1 and 2. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who because of grade configuration had to switch schools to enter 9th grade in 2021 or 2022 by applying through the CSCS and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level. Columns (5) and (6) include lagged tests scores as covariates, which correspond to those observed 6 years earlier, when the students were in 4th grade.

VII.III Twelfth-grade outcomes

We repeat the analysis using data from three other cohorts of students who were forced to switch schools (also because of grade configuration at the end of 8th grade) in either 2018, 2019, or 2020. For

[29] This first-stage result is stronger than the one for the 4th-grade estimation as a result of the shorter time lapse between CSCS allocations and the moment we measure outcomes.

these students, we observe their college entrance exams results (in 2021, 2022, and 2023) after they had been enrolled in their new schools for four years. Table 6 shows the results. According to the first-stage estimate in Column (1), PVS assignment in 9th grade increases the probability of a student's being enrolled in a PVS in 12th grade by 54%. According to Column (2), receiving an offer for a 9th grade PVS seat improves reading tests scores relative to being in a TPS, when we control for other covariates and the DAPS. Columns (3) and (4) report the 2SLS coefficients; we find that after four years of PVS "treatment," students perform 0.164σ and 0.146σ better than their TPS peers in reading and math, respectively. For these students, we can control for the lagged tests scores observed while they were in 8th grade (four years before we measure their outcomes in 10th grade, and during the last year in their previous school).[30] As in the case of the 10th-grade outcomes, the results remain qualitatively the same, although the point estimates drop to 0.117σ for reading and 0.083σ for math. The main conclusion that PVSs exert a causal improvement on tests scores remains valid for college entrance exams.

Table 6: Effect of PVS attendance – 12th-grade sample

	First Stage	Reduced Form	2SLS		2SLS V.A.	
	(1)	(2)	(3)	(4)	(5)	(6)
		Reading	Reading	Math	Reading	Math
P.V. Allocated	0.547***	0.0897***				
	[0.0138]	[0.0261]				
P.V. Enrolled			0.164***	0.146***	0.117***	0.0838**
			[0.0480]	[0.0387]	[0.0316]	[0.0366]
Obs	18718	18718	18718	18448	18513	18288
N° of schools	1999	1999	1999	1985	1993	1980
Value Added	No	No	No	No	Yes	Yes

Note: Estimates of Equations 1 and 2. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who, because of grade configuration, had to switch schools to enter 9th grade in 2018, 2019 or 2020 by applying through the CSCS and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level. Columns (5) and (6) include lagged tests scores. For the cohort who took PAES 2021 and 2023, we use 8th-grade SIMCE scores from 2017 and 2019, respectively; for the one that took PAES 2022, we use the 2016 6th-grade SIMCE.

VII.IV Heterogeneity

Next, we conduct an heterogeneity analysis to explore whether the results are driven by a particular group of students. Table 7 shows the results from our estimating the second stage in Equation 1 with different samples. In all cases, for boys, girls, disadvantaged and nondisadvantaged students, the point estimates are very close to zero and insignificant. In other words, the main conclusion that PVS enrollment does not improve academic outcomes in 4th grade remains valid for all these subgroups.

We next explore the heterogeneity in the effects on 10th graders in Table 8, where we report the second-stage results from Equation 7, including lagged tests scores as covariates in all specifications. When we use the samples of male students only and disadvantaged students only (Columns (1) and (3),

[30] The exception is the cohort who took the college entrance exam in 2022, for whom we observe their 6th-grade outcomes. See the calendar of evaluations in Table 12 for further clarification.

Table 7: Heterogeneity in effect of PVS attendance – 4th-grade sample

	Reading				Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Female	Disadv.	Non-Disadv.	Male	Female	Disadv.	Non-Disadv.
P.V. Enrolled	0.0127	-0.0230	-0.00762	-0.00811	0.0560	-0.00479	0.101	-0.0973
	[0.0766]	[0.0780]	[0.0698]	[0.0866]	[0.0766]	[0.0846]	[0.0750]	[0.0877]
Obs	7419	7169	8842	5746	7517	7239	8956	5800
N° of schools	2218	2152	2427	1915	2231	2160	2439	1922
V.A.	No	No	No	No	No	No	No	No

Note: Estimates of Equation 2 for different subsamples in each column. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who applied through the CSCS to enter 1st grade in 2019 or 2020 and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level.

respectively) on the reading exam, we cannot rule out the effects being zero; admittedly, however, this might be due to lack of power. When we use the samples of female and nondisadvantaged students only, we find that the positive and significant results that we find in the full sample are mostly driven by these groups, with the point estimates here equal to 0.083σ and 0.092σ, respectively. For the math exam, it is also true that female and nondisadvantaged students drive most of the result in the full sample, as for them the point estimates equal 0.185σ and 0.217σ (Columns (6) and (8)), respectively. However, in contrast to what we find for reading, we still find significant and sizable results for male and disadvantaged students. In sum, PVSs appear particularly effective at improving math tests scores for female and nondisadvantaged 10th graders.

Table 8: Heterogeneity in effect of PVS attendance – 10th-grade sample

	Reading				Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Female	Disadv.	Non-Disadv.	Male	Female	Disadv.	Non-Disadv.
P.V. Enrolled	0.0402	0.0833**	0.0489	0.0925*	0.159***	0.185***	0.153***	0.217***
	[0.0500]	[0.0381]	[0.0397]	[0.0483]	[0.0481]	[0.0425]	[0.0400]	[0.0501]
Obs	8021	9108	11787	5342	8134	9254	11968	5420
N° of schools	1376	1499	1640	1235	1386	1509	1652	1243
V.A.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates of Equation 2 for different subsamples in each column. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who applied through the CSCS to enter 1st grade in 2019 or 2020 and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level. All estimations include lagged tests scores as covariates, which correspond to those observed 6 years earlier, when the students were in 4th grade.

Finally, we explore the heterogeneity in the effects on 12th graders in Table 9. For the reading exam, we confirm the pattern that female and nondisadvantaged students perform much better than their TPS peers, whereas the difference for male and disadvantaged students relative to their TPS peers is positive and significant but of a smaller magnitude. For the math exam, this is also true on the gender dimension; female PVS students perform 0.093σ better than girls in TPSs. For male students, however, we cannot rule out that the effect is zero. Finally, for the math exam, we find that the effect in the full sample is driven mainly by disadvantaged students, who perform 0.082σ better than their TPS peers. Overall, these results confirm the positive causal impact of PVSs on college entrance (i.e., high-stakes) exams.

Table 9: Heterogeneity in effect of PVS attendance – 12th-grade sample

	Reading				Math			
	(1) Male	(2) Female	(3) Disadv.	(4) Non-Disadv.	(5) Male	(6) Female	(7) Disadv.	(8) Non-Disadv.
P.V. Enrolled	0.0771*	0.146***	0.0985**	0.143***	0.0558	0.0938***	0.0828**	0.0582
	[0.0417]	[0.0395]	[0.0386]	[0.0427]	[0.0353]	[0.0336]	[0.0322]	[0.0385]
Obs	8052	10461	11031	7482	8008	10366	10917	7457
N° of schools	1511	1616	1708	1500	1502	1610	1704	1495
V.A.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates of Equation 2 for different subsamples in each column. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Sample of students who applied through the CSCS to enter 1st grade in 2019 or 2020 and have a nondegenerate DAPS. Covariates include indicator variables for female students, students whose fathers have a college degree, foreign students, socioeconomically disadvantaged students, and a 5-category variable for household income level. All estimations include lagged tests scores as covariates, which correspond to those observed 4 years earlier, when the students were in 8th grade.

VIII Mechanisms

VIII.I Do parents change their behavior depending on their children's enrollment?

One plausible threat to identification is the possibility that TPS students' parents might react by exerting extra effort to compensate for the failure to secure a sought-after PVS seat. This is an example of a behavioral response from parents, one that might be quantitatively relevant if the parents of TPS students have a lower opportunity cost for their effort, which is reasonable given their lower SES on average (Albornoz et al., 2018). If this is the case, our estimates would be biased, as we cannot rule out that everything else (e.g., family inputs) remains constant after enrollment even though enrollment (conditional on the DAPS and covariates) itself can be plausibly regarded as random.

To alleviate this concern, we use a set of questions that parents of students taking the SIMCE each year must answer regarding (among other things) how they support their children's schooling. Parents are asked the following: "How easy or difficult is it for you to support your son or daughter in the following school aspects?: (i) Helping him/her solve problems with technology (for example, connecting to the Internet, installing an application, downloading or sending files, etc.); (ii) Helping him/her organize his/her day with times and places to study; (iii) Helping him/her with study habits and strategies; (iv) Supporting him/her in his/her school activities (for example, with his/her homework, studying for tests, etc.)." Valid response options are "easy," "difficult," and "I don't know how to do it." Also, in 2022 parents were also asked about the frequency with which they set times for studying, watching TV, playing, and sleeping. Possible answers to these questions were: "never", "sometimes this month", "sometimes this week", "always".

We run the 2SLS system of Equations 1 and 2 using as dependent variable an indicator that equals one if parents answer that they find it easy to support their student on each dimension of school aspects and zero otherwise. For the case of setting times, we use an indicator that equals one if parents set times at least sometimes during the month, and zero if they never set times. Table 10 shows the estimates for the 4th-grade sample; we find nonsignificant and very small point estimates for the differences in parental inputs between students enrolled in PVSs and those enrolled in TPSs across all four measures.[31] In other words, the null effects we find in 4th grade cannot be explained by the possibility that parents react to an allocation to TPS by exerting additional.[32]

Table 10: Do parents change their behavior upon their children enrolling in a PVS? – 4th-grade sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Technology	Organization	Habits	Activities	Study time	TV time	Play time	Sleep time
P.V. Enrolled	0.0144	-0.0229	-0.0133	-0.0151	0.00507	0.0361	0.0132	0.0184**
	[0.0200]	[0.0209]	[0.0224]	[0.0182]	[0.0104]	[0.0244]	[0.0187]	[0.00885]
Obs	14298	14246	14190	14233	5295	5273	5279	5279
Clusters	2816	2816	2810	2812	1400	1399	1400	1399

Note: Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. "Private Voucher" corresponds to the second-stage regression coefficient from a 2SLS system in which the dependent variable is each of the ones specified in the columns.

VIII.II Do students not offered a PVS seat enroll in fully private schools?

Students offered a PVS seat differ from those who are not. The use of the DAPS allows us to to account for these differences. However, the existence of fully private schools in Chile (which do not participate in the centralized admissions process) provides an outside option to students who do not receive a PVS offer. In particular, this outside option is more concrete for better-off families, as fully private schools fund their operations exclusively on the basis of out-of-pocket fees. Hence, if it is true that students failing to gain access to a PVS opt for fully private schools, the students observed in TPSs in our estimation sample would be negatively selected. As a consequence, our comparisons regarding the follow-up years (when we measure outcomes, either 2 or 4 years after applications through the CSCS) would be biased away from zero, because the comparison group in TPSs are those presumably with lower socioeconomic background and academic performance.[33]

To explore this possibility, we run a set of regressions using as dependent variable an indicator that takes value 1 if a student is enrolled in a fully private school in the follow-up year (when we measure outcomes) and zero otherwise. The explanatory variables are an indicator for allocation to a TPS, the DAPS, and the same covariates used in our main estimations. Table 11 shows the results. In Column (1), we use the sample of students who applied to enter 1st grade (the base year) in 2019 and 2020 and are observed in any school—including fully private ones—in 4th grade (the follow-up year) in 2022 and 2023, respectively. We find that receiving an offer for a TPS makes it marginally less likely that students opt to enroll in a fully private school. This means that, at least in 4th grade, there is little evidence that families opt out when they do not receive a PVS offer.

[31] Notice that we do find a positive effect on setting time to sleep in column (8), although it is very unlikely that this effect alone could drive the main result.

[32] Table 15 reports the same exercise using the 10th-grade sample. The results on the questions we can compare are also null (behavior regarding setting times is not considered in the 10th grade sample). Note that we cannot conduct this exercise using the sample that takes the college entrance exams, as these questions are part of the SIMCE tests.

[33] Another possibility that might explain the sizable effects might be that differences in quality between TP and PV schools to which students apply with a non-degenerate probability of admission are large. For instance, if oversubscribed PVSs are particularly good (similar to what happens in studies in the US using admission to sought-after charter schools with decentralized lotteries (Chabrier et al., 2016)). While there are differences between schools in our estimation sample, those differences are analogous to those observed in the universe of schools in Table 2.

Table 11: Do students not offered a PVS seat enroll in fully private schools?

	Dep var: Enrolled in fully private school		
	(1)	(2)	(3)
	4th grade	10th grade	12th grade
TPS Allocated	-0.00865***	0.108***	0.0671***
	[0.00291]	[0.0214]	[0.0133]
Obs	15016	22474	24924
N° of schools	2860	2204	2372
Mean Dep Var	0.0148	0.0781	0.0713

Note: The dependent variable is an indicator that takes value 1 if a student is enrolled in a fully private school in the follow-up year and zero otherwise. "PV Allocated" equals 1 for students offered a PVS seat in the year they apply through the CSCS. Sample includes all applicants with a DAPS $\in (0,1)$ who are observed in the follow-up year. Specifications control only for the DAPS. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

In Column (2), we repeat the analysis with the sample of students who applied to enter 9th grade in 2019 and 2020. In this case, we do find more meaningful evidence of selection, as students offered a TPS spot (i.e., failing to gain access to a PVS) are 10.8% more likely to migrate to the fully private sector. This implies that the students observed in TPS in our estimation sample are negatively selected and, therefore, when we estimate the performance differences from PVS students, the results could be biased away from zero (or they are an upper bound on the true effect). In Column (3), we repeat the analysis for the students who applied in 2019, 2020 and 2021 to enter 9th grade and whose outcomes we observe in the college entrance exams. In this case, we again observe some mild evidence of selection.

The degrees of selection are also consistent with the size of point estimates observed across different grades and have important implications for our conclusions. In 4th grade, the lack of selection implies that the null effects found in our estimations in Table 4 reflect truly null causal effects of PVS attendance. In 10th grade, though, where we find the greatest effects of PVS attendance (Table 5) we also find that selection is the highest. In other words, at least part of the effect of attending a PVS we found can be explained by the migration of students failing to gain admission to PVSs towards fully private schools, which left in the TPSs a group of negatively selected students, and not to causal improvements in academic performance. The same is true to a lessen degree in 12th grade, where nor selection nor treatment effects are very high.

IX Conclusion

The growing popularity of school choice and private provision in education systems around the world is driven by the idea that parents "know best" for their children, as well as by the belief that private organizations can combine inputs more efficiently than public institutions, which typically face rigidities and red tape. The Chilean school system has long allowed students to choose where they enroll and has given private schools the freedom to hire and dismiss teachers (or make investment decisions) at their discretion. Given this, the Chilean system offers a unique opportunity to test whether this mix of policies gives private schools an advantage over TPSs. In this paper, we focus on one particular institution, "private voucher schools," which are privately owned and managed, but publicly funded through vouchers that schools receive when students choose to attend them.

Evaluating the effectiveness of PVSs is challenging because of the difficulty of accounting for unobserved differences among families who enroll in different types of schools and the inputs they provide for their children. In this paper, we overcome this challenge by leveraging the CSCS in Chile, which randomizes the allocation of seats at oversubscribed schools. We use a rich dataset on primary- and high-school standardized tests, college entrance exams, and family inputs. Our results suggest that in the post-COVID period, Chilean PVSs induce causal academic improvements only for older students. However, we cannot rule out the possibility that this result is due to the fact that students who fail to access a PVS through the CSCS enroll in a fully private (fee-charging) school, thereby inducing negative selection in the control group of TPS students.

Although our study overcomes several methodological concerns in the literature on the effectiveness of private education, the results should be taken cautiously, mainly due to the following limitation. One might still be concerned about the general equilibrium effects of PVSs and their impact on the education system's overall quality. More precisely, while our study can recover causal impacts of PVS attendance under plausible assumptions, it could still be the case that the null differences we find between PVSs and TPSs are due to public schools improving their effectiveness in response to PVSs' competitive pressure. This is the distinction that Chen and Harris (2023) make between "participant effects" (i.e., effects over those that enroll in a private school relative to a similar comparison group) and "competitive effects" (i.e., how the availability of private schools affects the outcomes of students who remain in nearby TPSs). Given the universal nature of the Chilean vouchers system and the long-standing presence of PVSs, we would need a model and stronger assumptions to distinguish between participant and competitive effects. We believe this may be a promising avenue for future research.

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Appendix A: Additional figures and tables

Table 12: Calendar of evaluations

Year	Grade										
2016	.	.	.	.	1	2	3	4	5	6	7
2017	.	.	.	1	2	3	4	5	6	7	8
2018	.	.	1	2	3	4	5	6	7	8	9
2019	.	1	2	3	4	5	6	7	8	9	10
2020	1	2	3	4	5	6	7	8	9	10	11
2021	2	3	4	5	6	7	8	9	10	11	12
2022	3	4	5	6	7	8	9	10	11	12	.
2023	4	5	6	7	8	9	10	11	12	.	.

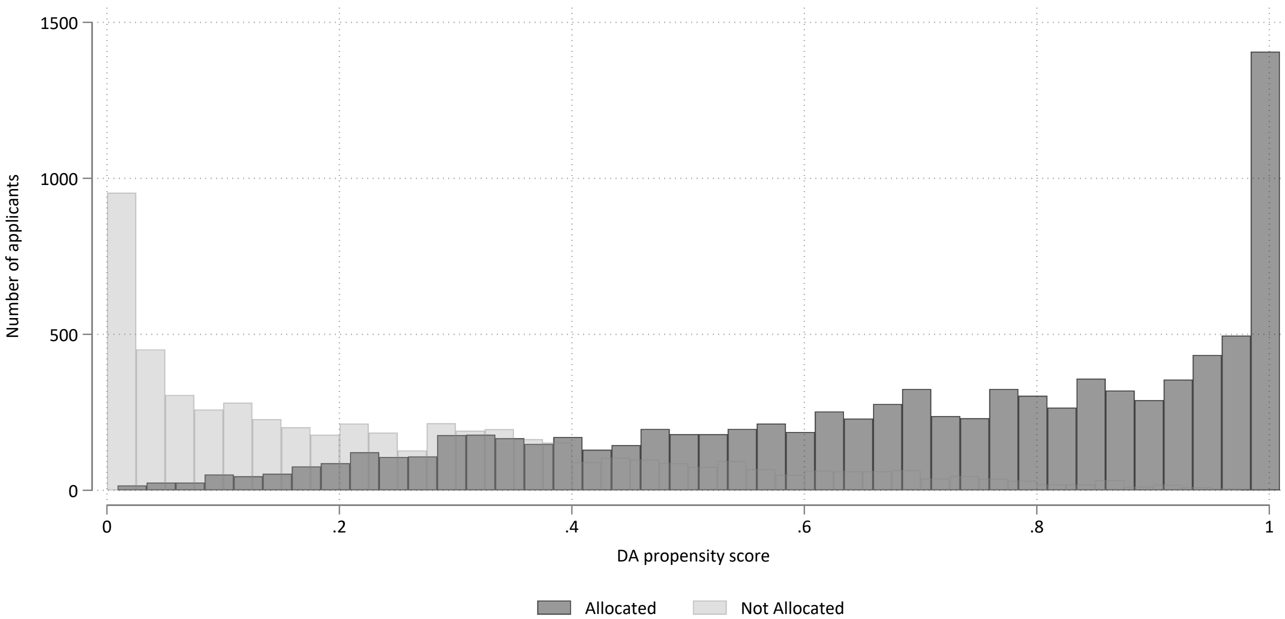
Note: Green squares indicate availability of SIMCE scores. Light-blue squares indicate availability of PAES college entrance exam scores. Starting in 2020 (below the dashed line), the CSCS was fully in place; this means that all students aiming to enroll in school year 2020 had to apply through the CSCS in 2019.

Table 13: Descriptive statistics – 2023

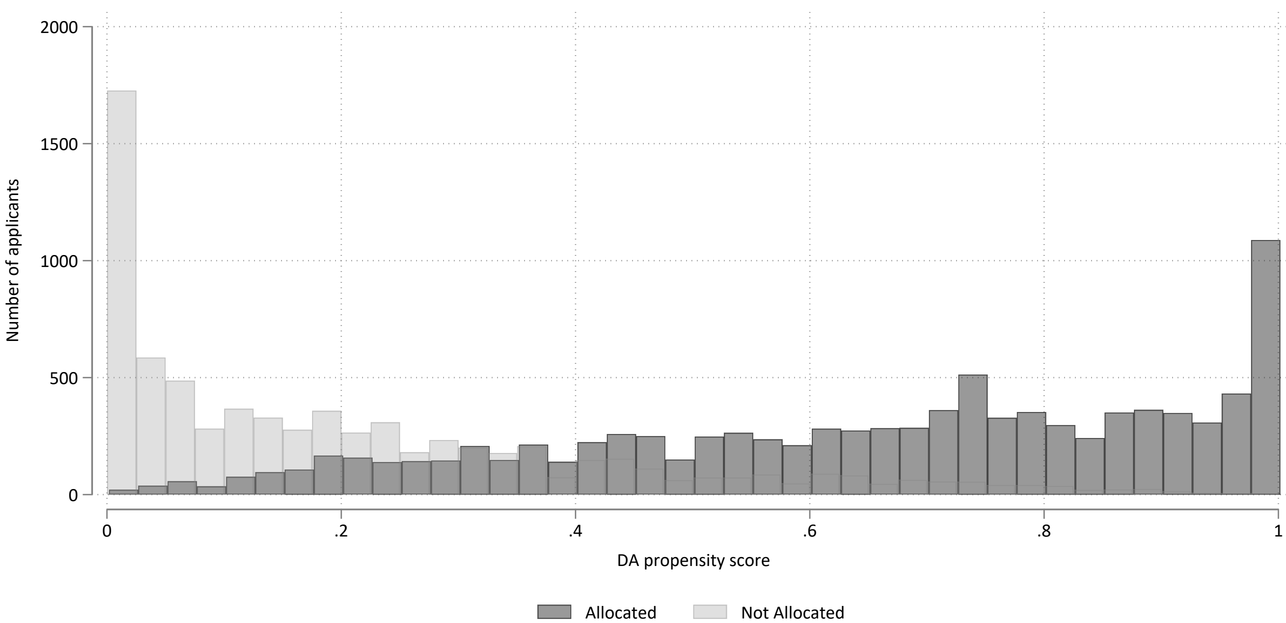
Panel A: Schools				
	PVS		TPS	
	Mean	SD	Mean	SD
Student/teacher ratio	18.31	7.95	10.25	5.44
Number of teachers	17.79	20.05	19.66	18.44
N students per school	345.71	443.58	248.03	296.46
Teaching hours female (1)	.82	.2	.72	.24
Teacher years experience	13.87	6.25	15.21	5.27
Rural(2)	.13	.34	.51	.5
Number of Observations	5672	5672	5188	5188
Panel B: Students - 4th grade SIMCE				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized Math	0	.96	-.2	.98
Standarized Lang	.01	.98	-.17	1
Male students	.49	.5	.51	.5
Any ethnic group (3)	.14	.34	.16	.36
Foreign students	.09	.29	.19	.39
Expects college (4)	.88	.33	.81	.39
P.A. Technology (5)	.83	.38	.77	.42
P.A. Organize studying (5)	.84	.37	.84	.37
P.A. Study habits and strategies (5)	.8	.4	.81	.39
P.A. School activities (5)	.86	.34	.86	.35
Lowest income bracket (6)	.14	.35	.24	.43
Highest income bracket (6)	.09	.28	.03	.16
Father went to college	.58	.49	.39	.49
Disadvantaged(7)	.64	.48	.77	.42
Number of Observations	106293	106293	64807	64807
Panel C: Students - 10th grade SIMCE				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized math	.05	.95	-.29	.9
Standarized lang	.07	.96	-.2	.98
Male students	.49	.5	.5	.5
Any ethnic group (3)	.13	.34	.17	.38
Foreign students	.09	.28	.15	.35
Expects college (4)	.86	.35	.77	.42
P.A. Technology (5)	.62	.48	.54	.5
P.A. Organize studying (5)	.79	.4	.76	.43
P.A. Study habits and strategies (5)	.74	.44	.71	.45
P.A. School activities (5)	.73	.44	.69	.46
Lowest income bracket (6)	.25	.43	.44	.5
Highest income bracket (6)	.04	.2	.01	.12
Father went to college	.51	.5	.33	.47
Disadvantaged(7)	.63	.48	.75	.43
Number of Observations	90346	90346	55654	55654
Panel D: Students - College entrance exam PAES				
	PVS		TPS	
	Mean	SD	Mean	SD
Standarized Math	-.04	.91	-.28	.85
Standarized Lang	-.01	.94	-.26	.97
GPA(8)	5.9	.67	5.81	.84
Male students	.46	.5	.45	.5
Number of observations	164259	164259	97258	97258

Notes: (1) Share of teaching hours taught by female teachers. (2) Share of rural schools. (3) Share of students who consider themselves as belonging to an ethnic group. (4) Share of students whose parents expect them to obtain a bachelor's degree or higher level of education. (5) PA (parental assistance) questions refer to the share of students whose parents report finding it easy to help them in each topic. (6) Household income in a typical month used in brackets: [0 to 300,000 CLP] = 1, [300,001 to 600,000 CLP] = 2, [600,001 to 1,200,000 CLP] = 3, [1,200,001 to 1,800,000 CLP] = 4, > 1,800,001 CLP = 5; CLP ≈ USD 900. (7) Disadvantaged refers to a student in a household with income in the bottom third of the income distribution. (8) GPA in Chile ranges between 1 (minimum) and 7 (maximum). Observations from academic year 2023.

Figure 2: DA propensity score, by assignment status



(a) 4th grade



(b) 10th and 12th grades

Note: DAPS used in the estimations of 4th-grade (Panel (a)) and 10th- and 12th-grade outcomes (Panel (b)). The DAPS shown in Panel (a) is calculated in the admission process when students apply to enter 1st grade. The DAPS shown in Panel (b) is calculated in the admission process when students apply to enter 9th grade after being forced to switch from a school that ends in 8th grade.

Table 14: Comparison between lottery compliers and rest of population

	4th grade			10th grade		
	Compliers	Rest of population	p-value	Compliers	Rest of population	p-value
Standarized Math	-.082	-.048	0	-.079	-.15	0
Standarized Lang	-.066	-.012	0	-.049	-.082	0
Male	.503	.509	.191	.507	.469	0
Father went to college	.496	.528	0	.443	.33	0
Ethnicity	.144	.155	.001	.145	.174	0
Foreign	.235	.04	0	.315	.023	0
Parents expectations	.862	.872	0	.831	.761	0
P.A. Technology	.794	.85	0	.567	.515	0
P.A. Organize studying	.844	.848	.185	.774	.772	.435
P.A. Study habits and strategies	.811	.81	.822	.722	.715	.057
P.A. School activities	.866	.877	0	.715	.699	0
Income $<$ 300,000 CLP	.172	.16	0	.228	.43	0
Income $>$ 1,800,001 CLP	.05	.034	0	.02	.004	0
Prioritized	.685	.606	0	.67	.688	0
Propensity Score	.668	.53	0	.471	.466	.147
Allocated	.644	.623	0	.407	.57	0
Private Voucher	.538	.654	0	.479	.584	0
N	14588	373734	.	17268	346208	.

Note: Compliers are those students with a propensity score strictly between zero and one used in the main estimation. The rest of the population is all students in the corresponding grade whose standardized tests scores and observed characteristics are nonmissing.

Table 15: Do parents change their behavior upon their children enrolling in a PVS?

	(1)	(2)	(3)	(4)
	Technology	Organization	Habits	Activities
P.V. Enrolled	-0.00267	0.00356	0.00398	0.0105
	[0.0158]	[0.0143]	[0.0146]	[0.0154]
Obs	16920	16843	16709	16848
Clusters	1843	1840	1837	1839

Note: "PV Enrolled" corresponds to the second-stage regression coefficient from a 2SLS system in which the dependent variable is each of those specified in the columns. Standard errors clustered at the school level in brackets. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. 10th-grade student sample.

Appendix B: Obtaining propensity scores with the Chilean deferred acceptance algorithm

We explain here the relevant characteristics of the Chilean DA mechanism used to calculate the DAPS. The full details of the implementation are in Correa et al. (2021).

Obtaining the real assignment requires, for each student–school application pair, a lottery number unique within the school. This number is used as a rank in the case of oversubscribed schools to randomly break ties among students within the same priority class (who are considered equivalent by the mechanism). This amounts to generating a sequence of distinct random numbers to students applying to each school. In addition, to improve the chances that siblings are assigned together, the lottery numbers are forced to be consecutive within each family group.

In practice, the lottery numbers are drawn with a pseudo random number generator (PRNG), started from an unpredictable seed (earthquake data) calculated between the end of the application process and the assignment phase and which then becomes public information. The lottery numbers are generated by assigning a rank order to each family group by means of the Fisher–Yates shuffle algorithm and then ordering within groups by the same procedure.

We started with the publicly available, anonymized application data and code for the admission process. The code was then modified to execute, in parallel, independent full assignment instances starting from the same application data, but using a different seed for each corresponding PRNG. Every student–school application was provided with a global counter that would be increased on completion of each assignment instance if the student ended up being assigned to that school in the instance (the counters were made atomic to prevent race conditions, due to the parallel nature of the execution). Finally, after all the instances finished their execution, the DAPS for each application was calculated as its accumulated counter value divided by the numbers of instances run.