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THE EFFECTS OF SCHOOL INTEGRATION PROGRAMS: EVIDENCE FROM ADMISSION LOTTERIES

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The Effects of School Integration Programs: Evidence from Admission Lotteries *

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Abstract

An increasing number of countries have decided to adopt more inclusive educational practices for students with special educational needs (SEN). One of them is that students with SEN share the classrooms with regular students. In this context, School Integration Programs (SIP) are in charge of supporting these children. I use student assignment lotteries to estimate the effect of SIP on pupils with Transitory SEN achievement. Specifically, I compare different outcomes of TSEN students with similar preferences admitted to SIP schools through the randomized admissions lotteries with outcomes of TSEN students who were not accepted in SIP schools. I find SIP schools have a non-significant effect on the probability of changing school during the year, participating in the following admission process, distance to school, and attendance. However, for pupils with priority preference for SIP, the School Integration Programs positively impact take-up, besides the negative impact on approval probability. In contrast, for those with a secondary preference for SIP, the impact on take-up is negative but increases the probability of approbation. I suggest that school quality differences drive the effect.

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1. Introduction

Approximately 15% of the world's population experience some form of disability (World Bank, 2011)[1]. According to the II National Disability Study, 16.7% of the Chilean population aged two years and over has a disability. This group of people is more likely to experience adverse socioeconomic outcomes. In Chile, 30.8% of adults with disabilities have no formal education or have not completed primary education. While for the population without disabilities, it is only 12.4% (Senadis, 2015). There is a vast theoretical (Becker, 1994; Spence, 1973) and empirical evidence suggest a causal effect of schooling on earnings (Angrist and Keueger, 1991; Card, 2001; Oreopoulos, 2006). Therefore, increasing this group's access and permanence in secondary and tertiary education is essential.

Following this objective, an increasing number of countries have decided to adopt more inclusive educational practices for students with special educational needs (SEN). A student presents SEN when they need extra pedagogical help or additional resources to carry out their learning processes. Mainstreaming is one of the inclusive practices where children with SEN are placed in a general education classroom.

In general, the literature has shown positive effects in developing socio-emotional skills on regular students[2] (Hanushek et al., 2002; Ruijs and Peetsma, 2009. However, the effects on classmates' academic achievement are negative (Gottfried, 2014; Balestra et al., 2020) or non-significant (Ruijs, 2017a). Regarding the impact of mainstreaming on students with SEN, different disciplines suggest positive externalities on academic and social results (Peetsma et al., 2001; Meyer, 2001; Hanushek et al., 2002; Morgan et al., 2008; Schwartz et al., 2021).

In Chile, SEN students could assist in special education or regular schools. Some regular schools have an Integration Program which helps SEN students with their learning process with additional human and material sources. Nevertheless, parents could apply to both types of regular school. In general, regular schools without Integration Program were more likely to reject the entrance for different reasons, such as they are not prepared to educate SEN students. Since 2015, the Inclusion Law changed the admission process so students cannot be rejected from a school due to their socioeconomic status, academic performance, or having a special educational need, among other things. Moreover, if a school has over-demand lotteries assign students.

This study investigates the causal effect of the School Integration Program (abbreviated SIP) on educational outcomes of a particular subset of SEN students by exploiting school admission lotteries from "Sistema de Admisión Escolar" (abbreviated SAE).

My empirical analysis is based on detailed student-level panel data for students who attend 9th grade and were a part of SAE. I use the lottery data to avoid the critical issue of the non-random selection of students into schools. Then, to estimate SIP school impacts, I compare different outcomes of Transitory Special Educational Needs[3] (abbreviated TSEN) students admitted to these schools through the randomized

[1] This is the most up-date information about the magnitude of the population with disabilities worldwide. This amount is also shown on the World Bank website up-date on Oct 10, 2021.

[2] The term "regular students" refers to students without special educational needs.

[3] According to decree 170, the special educational needs can be of a permanent or transitory nature. The former are barriers that students experience throughout their schooling due to a disability, while the latter are non-permanent barriers that a

admissions lotteries with outcomes of TSEN students who were not accepted in SIP schools. To identify TSEN students, I use the information from the 2018 enrollments. It is essential to mention that students are recognized as SEN students only if they attend a school with a SIP.

SAE has several features that make it an excellent candidate for experimental analysis. First, I use administrative data, so it is easy to obtain information about the students at different times of their lives. Second, parents face almost no restrictions in choosing and declaring school preferences for their children. Expressly, the system does not limit the number of applications per student[4]. Third, it is one of the most extensive school choice programs in the world. Fourth, all students who want to change to schools administrated publicly or through vouchers have to participate in the SAE, independently of their personal qualities or status (such as Special Needs, Academic Excellency, or Disadvantage Student). So, I can explore the impact of choice within different groups of students.

A growing body of literature has used lotteries to identify causal effects. Some authors have focused on measuring the impact of entering the first-choice school, which best suits parents' preferences. In general, those students who enter their first option do not perceive benefits on test scores, attendance, or being retained (Cullen et al., 2006; Hastings et al., 2006b). However, some groups could experience significant gains. For example, Cullen et al. (2006) show that white females chose academically-focused schools. So when randomized into those schools, they expended more effort on academics and experienced significant gains in test scores. Also, non-academic benefits have been observed, such as reducing the crime rate (Deming, 2011) and increasing university attainment (Deming et al., 2014). Therefore, school choice can be a fundamental factor in intergenerational mobility. On the other hand, lotteries have been used to evaluate the causal effect of charter (Clark et al., 2015) and Montessori (Ruijs, 2017b) schools, showing non-significant impacts on student achievement.

Overall I find that School Integration Programs may have heterogeneous effects by the type of revealed preferences. For students with *priority preference for SIP*, the Integration Program has a positive effect on take-up (about 16 percentage points), besides the negative impact on approval probability about 9 percentage points. In contrast, for students with *secondary preference for SIP*, the impact on take-up is negative (about -8 percentage points) but increases the probability of approbation about 14 percentage points. It seems that the effects are driven by the school quality related to parents' preferences. Students with *priority preference for SIP* were more likely to choose high-quality SIP schools, while students with *secondary preference for SIP* low-quality schools. There are not significant effects on other academic outcomes such as attendance, drop-out and distance to school.

When evaluating these findings, it is important to have in mind several weaknesses associated with the data that limit the ability to generalize our results to other contexts. The most important limitation of our data set is that I identify SEN students using administrative data of 2018. Consequently, as they need extraordinary help for a certain period of their schooling, there is a certain probability that they have overcome their learning barriers and do not need support during 2019[5]. Second, the analysis is limited

student requires at some point in their life. Therefore, they only need extraordinary help and support for a certain schooling period.

[4]However, the system requires at least two school applications. There are two exceptions where the restriction is not fulfilled: when the student applies to a rural school or is enrolled in a school that offers continuity of studies and belongs to the SAE.

[5]Figure A5 shows the probability of TSEN diagnosis renewal. The average likelihood (excluding 2020) of renewal conditional

to educational attainment in a very short term (same year) and aggregated outcomes (such as approval, attendance, or drop-out). I have no information about standardized test scores because 2020 SIMCE was canceled[6]. Also, high school graduation, college attendance, or labor market outcomes cannot be observed because the cohort is still in high school. In addition to this, educational achievements after 2019 are contaminated by the effects of the pandemic.

This thesis contributes to the existing literature in two main areas. First, it provides new evidence on the effect of attending the preferred school on educational achievement in a developing country. All investigated school choice lottery programs are conducted in developed countries. Since resources, culture, and institutional frameworks are different between developed and developing countries, the validity of these past results should be interpreted with caution in our context. Second, I evaluate the effect of the school integration program on the educational outcomes of students with special needs. This effect is even less explored in the literature due to the lack of comparable information within students with special educational needs. Moreover, this work contributes to guiding inclusion education policy and answering whether programs work.

The remainder of this paper is structured as follows. Section 2 establishes how this study relates to the existing literature on school choice and inclusive education. Section 3 describes the Chilean Educational System, the School Integration Program, and the admission process. Section 4 introduces the data set. Section 5 explains the empirical strategy. Section 6 presents the results. Section 7 and 8 discuss possible mechanisms and heterogeneous effect. Section 9 show some robustness checks. Finally, Section 10 concludes.

2. Related Literature

This study is related to three big strands of literature: first, the effect of mainstreaming; second, the importance of school choice; and third, centralized assignment as impact evaluation. The methodologies and results of the most important studies in each area are presented below.

First, the effects of mainstreaming have on educational processes can be measured from two different perspectives: the SEN mainstreamed students' and the mainstreamed students' peers. There is not much economic evidence on the effects of mainstreaming on students with SEN, mainly due to the lack of comparable information within this subset of students. However, Hanushek et al. (2002) use an individual fixed-effect model to estimate the effects of moving into or out of special education programs[7]. They found positive effects of special education programs on mathematics achievements of SEN students, specifically for learning disabled (LD) or emotionally disturbed students. Also, the results are robust to a series of specification tests. Morgan et al. (2008) used propensity score matching techniques to analyze data from the Early Childhood Longitudinal Study, Kindergarten Class of 1998–99 in the US. They report that special education services had a negative or a statistically non-significant impact on children's learning and behavior for students with LD or languages impairments. Schwartz et al. (2021) estimate the impact of special programs comparing

on keeping in the same school is 74%. While conditional to being in a school with SIP is around 69%. Finally, conditional to being in the school system is 65%.

[6] In Chile, SIMCE tests provide information on the learning standards achieved by the country's students who are in the assessed levels (4th-grade, 6th-grade, 8th-grade, and 10th-grade).

[7] Special education programs are a plan that allows students with disabilities to participate in the regular classroom. These programs are similar to School Integration Programs in Chile.

the same students before and after being classified with LD. They find that academic results improve for LD students after being classified. However, there are no effects on attendance. Also, the authors show that the impact is different by gender (impacts are larger for girls) or grade of classification (larger effects for elementary school classification).

Regarding the effect of mainstreaming on students without SEN, Hanushek et al. (2002) show that having a classmate with SEN improves the academic performance of regular students. Specifically, the estimated parameters indicate that a ten percentage point increase in the percentage of students classified as disabled increases achievement by roughly 0.016 standard deviations. On the other hand, Ruijs (2017b) used three different approaches: student fixed models, school fixed effects models, and neighborhood variation with Netherlands data. He documented that SEN students do not have a statistically significant effect on the academic achievement of their classmates. Even more, distinguishing between different types of SEN does not change the results. Also, Contreras et al. (2020) provide evidence of the effect of mainstreaming on the academic achievement of their peers without SEN but in a developing country (Chile). To do this, they evaluate the policy change implemented in 2010 using a fixed-effect model. They documented that before the policy change, having a peer with SEN had a negative effect on the academic performance of regular students. However, after the policy change, negative results in standardized tests decreased or became slightly positive[8].

Additionally, Gottfried (2014) also study the peer effects of classmates with disabilities but on students' non-cognitive outcomes. He used longitudinal data from the Early Childhood Longitudinal Study in the US (as Morgan et al., 2008) and a fixed-effect model. He shows that students with more classmates with SEN have higher externalizing and internalizing behavioral problems. Also, the results differ based on disability category. Finally, Balestra et al. (2020) is the first comprehensive evaluation of the effect of mainstreaming in education on the life of young adults. The Switz School System has organized secondary and primary schools separately, so students are reshuffled when transitioning between levels occurs. The authors exploit the variation in classroom composition from this quasi-random assignment to classes. They find that one additional SEN student in a class of 20 reduces test scores by 2.5% of a standard deviation. Nevertheless, these negative spillovers do not occur if students with SEN are less than 15-20% of students in a classroom.

Also, the effect of mainstreaming has been studied in other disciplines. Ruijs and Peetsma (2009) made a literature review of the impact of inclusion in students with SEN. They suggest that the effect of inclusive education on academic achievement seems to be slightly positive. However, a few studies found negative effects (Rogers and Thiery, 2003). At the same time, many studies found neutral (Dessemontet et al., 2011; Cole et al., 2004[9]) or positive effects (Peetsma et al., 2001; Jepma, 2003; Lindsay, 2007[10]). Regarding

[8]The authors argue that the additional resources and recognition seems to explain the difference with the literature that founds negative effects.

[9]Dessemontet et al. (2011) compared students with Intellectual Disabilities (ID) included in general education classrooms with ID in special schools. They find that students in mainstreaming classrooms improved literacy skills than children attending special schools. However, there are no differences in mathematics. Cole et al. (2004) compared students with learning disabilities or mild mental disabilities in inclusive and non-inclusive primary school classes in Indiana (US). The analysis showed no differences between the children in inclusive and non-inclusive classes.

[10]Peetsma et al. (2001) compare the development of matched pairs of primary-aged pupils in mainstream and special education over periods of 2 and 4 years. They found that, on average, children in regular education achieved somewhat better on mathematics than children in special schools for learning and behavioural difficulties. Jepma (2003) also found that students with learning and behavioural difficulties or mild mental retardation in regular education made more progress in language and maths. Lindsay (2007) examined publications between 2000-2005 concludes that inclusive education generally showed positive

socio-emotional effects, some authors have found positive effects of inclusive and special education (Casey et al., 2006), but others negative effects (Cole et al., 2004; Myklebust, 2007).

Second, this thesis is also related to the importance of school choice. Due to self-selection bias, estimating a causal relationship between school choice and student outcomes has been difficult. One of the first methodological attempts to address these concerns was the use of instrumental variables (Evans and Schwab, 1995; Sander, 1996; Neal, 1997). However, the effects found when using this empirical strategy are mixed and partially explained by the instruments' validity.

The newer and most popular approach nowadays is to exploit random lotteries. Hastings et al. (2009), using a mixed-logit discrete demand model with lottery data, found that parents value proximity to the school highly. However, the importance of a school's mean test score increases with a student's income and own academic ability. Also, the authors show that those who place the highest weight on academic characteristics when choosing a school benefit the most academically when admitted to their first-choice school, although the average student does not improve their academic outcomes when winning a school lottery. However, among white females, in particular, were significant gains in test scores resulting from winning the lottery (Hastings et al., 2006a). There is suggestive evidence that this may result from both the choices they made and personal effort. Cullen et al. (2006) exploit randomized lotteries that determine high school admission in the Chicago Public Schools. They compared lottery winners with lottery losers and did not find benefits across many traditional academic measures. However, lottery winners experience improvements on a subset of nontraditional outcome measures, such as self-reported disciplinary incidents and arrest rates. In later work (Cullen and Jacob, 2007), the authors examine effects for various demographic subgroups and students whose application behavior suggests a strong academic preference but finds non-significant impact.

The effects of winning lotteries on other outcomes have also been studied. For example, Deming (2011) estimates the causal effect of winning the lottery to attend the first-choice school on criminal activity using high school admission in the Chicago Public Schools. He shows that seven years after random assignment winning the lottery reduces adult crime by about 50%. This effect is concentrated among African American males and youth who have the highest risk for criminal involvement. He also shows modest improvements on absences and suspensions. However, there is no noticeable impact on test scores for any youth in the sample. On the other hand, Hastings et al. (2006b) show that lottery losers -who do not get their first-choice- are significantly more likely to vote in the ensuing school board election than lottery winners. This is consistent with the hypothesis that losing the school choice lottery caused parents to vote against the incumbent school board chair, causing it to lose the election. Deming et al. (2014) also use Charlotte-Mecklenburg Public School Choice lottery data. They find a significant overall increase in college attainment among lottery winners who attend their first-choice school. This result is strongly predicted by gains on several measures of school quality, and the effect is concentrated among girls.

Third, this thesis uses centralized assignment to estimate the causal effect of School Integration Programs. In this line, Clark et al. (2015) estimate charter school impacts, comparing test scores of students admitted to charter schools through the randomized admissions lotteries with outcomes of applicants who were not admitted. They find that the effects of charter middle schools on student achievement were negative but not statistically significant. However, the impacts change across schools and students; for more disadvantaged

but small effects.

schools and students, the impact is more positive while for more advantaged is more negative. Also, Ruijs (2017a) exploits admission lotteries. She investigates the causal effects of Dutch Montessori secondary education by comparing students winning admission lotteries to students who lose the lottery and therefore attend another school. Montessori education has no statistically significant effect on students' academic achievement, thus, provides an alternative way to attain similar outcomes.

Despite the similarity of the problem, some differences could make the recently mentioned approach impossible to replicate. First, according to Public Charter School data[11], charter schools were approximately 4.2% of the public school in the 2006-2007 school year (when the study was conducted). Charter students made up 2.4% of U.S. public school enrollment in those years and are highly demanded. Also, Ruijs (2017a) uses only two Montessori schools. For that reason, it is easy to find students who were not admitted to the analysis school by the lottery. In my study, some students could lose the lottery for a SIP school but win the lottery at another SIP school. Second, the focus of the paper is all the types of students. However, in this thesis, the focus is on TSEN students, which is a small group. So, it could be challenging to have enough controls and treaties by schools.

On the other hand, Abdulkadiroğlu et al. (2017) theoretically develop empirical strategies that fully exploit random assignment. The authors argue that all features of student preferences and school priorities can shape the probability of assignment to each school. Conditional on preferences and priorities, however, centralized assignments are independent of potential outcomes. Therefore, as in other stratified randomized research designs, conditioning on the propensity score eliminates selection bias arising from the association between conditioning variables and potential outcomes (Rosenbaum and Rubin, 1983). The authors emphasize that this strategy is more efficient than focusing on first-ranked schools and identifying a more representative average causal effect. Despite the benefits of this approach, the implementation is highly complex (uses simulations). I opted for a more straightforward approach that also tries to overcome the problems mentioned by the authors. My empirical approach compares students assigned to a school with SIP with those who are not, conditional on them ranking similar preferences for SIP. Then, my sample is balanced in terms of priorities. This could be interpreted as a matching on preferences and priorities.

In conclusion, previous literature suggests some benefits to attending schools more suited to what families want. Also, admission lotteries are ideal for identifying causal effects for school choice. However, no literature has investigated the impact of school choice on students with disabilities. Choosing a school for children with disabilities requires, in addition to incorporating typical variables such as distance, academic and non-academic results, considering the capacity of schools to provide tools for children to have adequate learning. A good proxy for this could be to have an integration program. Therefore, a better understanding of the effects of school integration programs on satisfaction and academic outcomes can help parents in their school choice problem. In addition, it could provide valuable information for policymakers to improve inclusive practices.

[11] To see more information see https://data.publiccharters.org/digest/charter-school-data-digest/how-many-charter-schools-and-students-are-there/ and https://nces.ed.gov/fastfacts/display.asp?id=84

3. Context

The Chilean educational system consists of preschool education, primary education (8 years), secondary education (4 years), and tertiary education[12]. The administration of the educational establishments can be exclusively public, voucher, and private schools. The first two receive state funding. Public schools, including Municipal (33% of enrollment) and Local Education Services (4.3% of enrollment), do not charge families. While voucher schools, which include subsidized (52.4% of enrollment) and Delegated Administration[13] (1.2% of enrollment) are funded by states and families.

Apart from the classification by funding type, the system provides regular and special education. Regular or mainstream education serves students with and without SEN, while special education (5%) of enrollment in 2019) focuses only on students with SEN. In general, students with SEN who attend regular schools do so through School Integration Programs (SIP). According to the Chilean Ministry of Education (MINEDUC) data in 2019, students with SEN represented 12,3% of the enrollment of public and voucher regular schools. That means that 57,5% of students in public and voucher regular schools had at least one classmate with SEN[14].

The SIPs is an educational strategy with an inclusive approach, voluntarily implemented among public and voucher schools. If a school decides to implement SIP, they receive a special state grant for each enrolled student with SEN. Nevertheless, they are responsible for diagnosing and identifying the student's SEN. Then the school has to provide additional human[15] and material resources to support and provide more opportunities for student learning. There are multiple SEN diagnoses, but the SIP classifies them into Permanent Special Educational Needs (PSEN) and Transitory Special Educational Needs (TSEN). For example, hearing disabilities, vision disabilities, intellectual disability, autism, multiple disabilities, and deaf-blindness are considered PSEN. On the other hand, Attention Deficit Disorder[16], Specific Language Disorder, Specific Learning Disorder, and Borderline Intellectual Functioning are considered TSEN[17]. Figure A1 shows the different types of diagnoses using the 2018 enrollment data. Most TSEN has Specific Learning Disorder (36.6%), while most PSEN has Intellectual Disabilities (73.3%).

Due to the PSEN students needing more help and more resources, the grant amount differs between PSEN and TSEN. According to Contreras et al. (2020), the PSEN grant is about US$ 320 per month, and the TSEN grant is US$ 275 per month. Meanwhile, the regular grant is about US$100. The grant's objective is to be spent on tools related to the program, for example, the provision of materials or the hiring of educational assistants. In 2019, 53.6% of the regular schools that received state funding implemented this program[18].

[12] In Chile, primary and secondary are compulsory.

[13] Delegated Administration School (Decree-Law 3166) are 70 vocational schools administrated by business associations.

[14] Both metrics are calculated using enrollment data and seems to have increased over the years. In 2014, students with SEN represented 8.4% of the enrollment of public and voucher regular schools. That year, 41% of students in regular schools had at least one classmate with SEN.

[15] Schools with SIP must have regular teachers, special education teachers, educational assistants, and speech therapists who contributed to the child's learning process.

[16] Also called deficit hyperactivity disorder.

[17] Table A2 categorizes and explains the different diagnoses.

[18] Table A3 shows how many schools have SIP by type of funding.

In this study, I focus on public and voucher schools because they were affected by the Inclusion Law (Law 20,845) promulgated in 2015; thus, they have to admit their students using a centralized admission system. And both types of schools together enrolled more than 90.9% of all primary and secondary education students in Chile.

The Inclusion Law also aims to eliminate arbitrary discrimination of students in their application process. So, the law requires schools that receive government funding (partial or complete) to use a centralized admissions system called "*Sistema de Admisión Escolar*" (SAE). The system consists of a website[19] where parents submit their child's applications to public and voucher schools. Also, the website provides information about the schools, such as if it has an integration program, available seats per grade, and SIMCE results. Unlike other centralized school admissions systems, the SAE allows students to apply to as many schools as they want[20].

The system assigns students to schools using a Deferred Acceptance (DA) algorithm, introduced by Gale and Shapley (1962), considering parents' preferences, quotas, and priority criteria (this is explained below). Then, it is possible to distinguish two cases: i) when the number of applicants to the school-grade is less than the number of seats in this school-grade, in which case all applicants are accepted, and ii) when the number of applicants exceeds the number of available positions (i.e., over-demand), and so randomized lotteries are used to determine who gets the spot.

The law defines a set of quotas and priority groups that are used to order students and takes considerable importance in over-demand schools. There are three quotas, which must fill in the following order. First, the *Special Need Quota* prioritizes students with permanent special educational needs and reserved at most two seats per classroom per school. This quota is processed before any other priority group or quota if the school has a SIP. Second, the *Academic Excellence Quota* prioritizes students with high academic performance[21] and assigns between 30% and 85% of the total seats depending on the school (Correa et al., 2019). This quota is applicable only in high-performance schools. Third, the *Disadvantaged Quota* prioritizes the most vulnerable students according to the Social Registry of Homes. Disadvantaged students have 15% of the seats reserved.

It is important to mention that the Permanent Special Needs quota process is different. In general, it consists of parents visiting the school, submitting papers that validate the students' diagnosis, and sometimes getting an interview with the coordinator in charge. Based on this information, the schools rank the students using different criteria such as order of arrival, randomization, disability infrastructure match, etc. Therefore, the lottery number is not valid, and there is no exogenous variation to exploit for PSEN students. So, I will not use it in my analysis.

Additionally, there are three priority groups for students that are not part of the quotas. First, *Sibling Priority* consists of students with a sibling already enrolled or admitted at the school. Second, *Working Parent Priority*, which consists of students that have a parent working at the school. Third, *Returning Student Priority*, who was enrolled at the school in the past and were not expelled from it. Being part of a priority group increases the chance to get acceptance. For example, in an over-demand school, a student

[19]Visit the official site for more information `https://admision.mineduc.cl/vitrina-vue/`

[20]For example, in the Chicago Public School choice programs, the student must reside within the school district

[21]Students are considered high academic performance if it comes from the top 20% of their school's grades ranking.

with a sibling currently studying at the school has more chance to win the lottery than a returning student. A random lottery number determines the admission within priority groups.

One of the systems' primary goals is the joint allocation of siblings to the same school (Correa et al., 2019). The idea is to reduce the travel time of families with multiple children. For that reason, the *Sibling Priority* is processed before the other priority criteria. Also, the families are allowed to report whether they want to prioritize the joint assignment of their children, which changes the order of preference if an older sibling gets acceptance in a school, putting this one as the more preferred option. I will ignore the fact that joint assignments could change the schools' ranking because I am interested in parents' revelated preferences and whether a student is assigned to a SIP school through lotteries. Therefore, changing the order of preference could be considered part of the lottery process (quasi-random). Also, few students have this type of application (less than 8%).

The SAE has been implemented gradually since 2016 (admission 2017), and over the years, more regions were added, as shown in Figure A2. It is important to mention that SAE has two rounds: main round and complementary round. The main round applications usually are from August to September. This round gives an assignment using the algorithm already explained. Then there is a deadline for parents to accept, decline, or decide to wait for the waiting list (beginning of November). After the waiting list runs, students can decline or accept the assignment. Sometimes, students cannot be assigned to any of their preferences, so they are left without an assignment and invited to participate in the supplementary period (mid-November). The complementary round does not have a waiting list, and students who do not obtain a place in one of their preferences are assigned to the closest school with places. However, there are occasions when no neighboring school has a place, and the student is left without an assignment. Then comes a stage that is not part of the SAE called regulation (January), where it looks for a school for those who do not have an assignment.

For this study, I considered the admissions process of 2018 (as Correa et al., 2019). That means students who started the academic year in March 2019 (hereafter Admission 2018) because of the health measures due to the covid-19 pandemic, the following admission processes could not be carried out normally. This primarily affected the students with SEN since it was not possible to carry out the re-evaluations of their diagnoses in many cases. In addition, the classes stopped being in person and the schools had to adjust their evaluation system drastically. Therefore, the results of these years are biased by the effects of the pandemic.

4. Data

This section describes the data and methodology that are used to answer my main questions. I begin by describing the different data sources used to create a detailed student-level panel. Then explain how I use lotteries to estimate the effects of attending a particular school.

This study uses four data sources. First, I used the SAE admission dataset to identify students competing for a seat in an over-demanded school and finally were assigned (or not) randomly through the algorithm. This data source also contains students' characteristics such as gender, home address[22], siblings at the

[22]Parents have to report their address to the system because if the algorithm cannot give them one of their preferences in

school they are applying to, considered high academic performance or from disadvantaged environments. Second, I link the SAE data to Ministry of Educations' administrative databases. This record contains detailed information on whether students display PSEN or TSEN[23]. It is important to mention that being recognized as a SEN student implies that the student attended a school with SIP during that particular year.

This dataset also includes school characteristics like address, type of education, and information about having SIP. Third, I use data from the Agency for the Quality of Education to characterize the quality of schools. Specifically, standardized test results (SIMCE), personal and social development indicators at the school level, and socio-economic indicators. Fourth, I link the students of the sample with their academic performance in 2018 and 2019. This administrative information also came from MINEDUC and includes information such as school attendance and promotion status at the end of the school year. Here, I define the key variables used in the analysis.

Complier.—A primary measure of satisfaction with the SAE system is the assignation take-up. There are two approaches to identify this: on the one hand, seeing the enrollments in April 2019, and on the other, using the school where the student finishes the school year from the database of academic performance. According to the first approach, over 76% of the sample students take it. Using the second approach, 73% enrolls and attend the SAE school. The first approach shows very short-term satisfaction. For that reason, the second approach is used. Also, most interest outcomes came from Academic Performance data at the end of the academic year.

School Attendance.—From Academic Performance data, it is possible to obtain the annual attendance of students. In this variable, 0 means that the student never went to school, and 100 never missed a school day. In Chile, the minimum required to approve the academic year is 85% of attendance. However, the annual average attendance at school is 90.2%, which does not vary much according to the type of administration.

Promotion Status.— Also, from the Academic Performance data, I obtain the final grade status. At the end of the year, students are classified as pass, fail, or retired. Chilean educational system considered that students approved the school year if they have at least 85% of attendance, and one of the following conditions holds: (i) the student passed all the subjects, (ii) the student fails one subject and has at least 4.5 (of 7.0) as GPA, or (iii) the student fails two subjects but has a GPA more or equal to 5.0 (of 7.0).

Admission 2020.—Another measure of satisfaction with the SAE system is participating in the following admissions process. The SAE 2019 admissions data makes it possible to identify which students are trying to change schools again (in the subset of public schools or vouchers). On average, 15 % participate in the admission process at the end of 2019.

Distance to School.—SAE data and MINEDUC administrative dataset make it possible to calculate the distance to school from home in 2018 and 2019. However, the addresses provided by the parents could not be georeferenced correctly. To eliminate this concern, the observations that are over the 99th percentile are

the complementary stage, the system will accept them in the nearest school with available seats. The data set they do not contain the real georeference but one with a random error.

[23]The Ministry of Educations' administrative databases have included information about students diagnosed with SEN since 2011. However, since 2013 it has been possible to differentiate between TSEN or PSEN.

eliminated. Then, on average, the students are assigned 5 kilometers from their homes.

Student Characteristics.—It is possible to get students' characteristics information from 2018 School Enrollment data such as gender (also reported in SAE dataset), age, and type of SEN. As I mentioned in Section 3, TSEN students (which are the focus of this study) are grouped into four diagnoses: Attention Deficit Disorder, Specific Language Disorder, Specific Learning Difficulties and Borderline Intellectual Functioning[24].

School Quality.—Using data from Agency for the Quality of Education I collect a series of quality proxies. First, to evaluate the school academic performance I use 10th Grade math and reading test score from 2018 SIMCE. Second, to evaluate non-academic performance I use School Environment Index and Academic Self-esteem and Motivation Index. Both came from the Personal and Social Development Indicators database obtained from complementary questionnaires to the 10th Grade SIMCE. The scale is from 0 up to 100 (where 100 is the maximum). The national average for the School Environment Index is 74.5, while for Academic Self-Esteem and Motivation Index is 75.6. I will use these variables to test the balance in observable characteristics between treaties and controls.

School Characteristics.— Also, from the Agency for the Quality of Education data, it is possible to get socio-economic information of schools. The Agency classified schools into five groups: low income (group 1), medium low income (group 2), medium-income (group 3), medium high income (group 4), and high income (group 5). I define low income as groups 1 and 2, medium-income groups 3 and 4, and high income as 5. Additionally, Ministry of Education's administrative databases include information about SIP agreement, tuition fees, type of school day (morning, afternoon or full day), type of administration (public or voucher), type of education (vocational or academic), and SEP classification (autonomous, emerging or recovery)[25]. This variables are considered for the balance in observable characteristics.

SAE Characteristics.—As I mentioned before, the SAE algorithm uses quotas and priorities to deliver the final allocation. Within the SAE variables, it is possible to find dummy variables for being a former student, having a sibling at school, or a parent who works at school. In addition, from the database of applications, it is possible to determine the number of applications made by the student (strictly speaking, the student's attorney-in-fact), in which preference is accepted, and the percentage of applications to schools with a SIP agreement.

The following section explains the methodology I used to select the samples to answer my research question. Also, it shows some summary statistics.

[24] Also Section 3 explain in more detail every group.

[25] The purpose of the Preferential School Grant (*Subvención Escolar Preferencial* in Spanish, SEP from here) is to contribute to equal opportunities by improving the equity and quality of education. Schools with SEP receive additional resources for each student from disadvantaged environments. According to the Ministry of Education, 80% of schools that meet the requirements are SEP. Schools with SEP are classified into three groups based on the results obtained in the last three measurements of the SIMCE Test of 4th Basic of Language, Mathematics, Natural Sciences and Social Sciences (which represents 70%) and other complementary indicators as retention, approval, and integration rate (which represents 30%). In the first place, the autonomous (High Performance) is characterized by the fact that they have consistently shown good educational results. Second, Emerging (Medium or Medium Low Performance) has not consistently shown good educational results. Finally, In Recovery (Insufficient Performance) has repeatedly shown poor educational results.

4.1. *Study Design*

In 2019, more than 2,637,000 students[26] were enrolled in public or voucher schools in grades 1-12. Around 63.8% of the students belong to another Region than the Metropolitan[27]. Only 274,990 students participated in the main round and 46,698 in the complementary round of SAE admission of 2018. It is possible to note that 26.9320 of the complementary round came from the main round, and 19.778 were new students. For this third year of implementation, the system offered 32,197 options in the main round, belonging to 12 levels (K-12) and 6,421 schools.

I considered students who only participate in the main round because the students who participate in the complementary round could be potentially different. However, some students participate in both rounds; thus, the valid lottery comes from the complementary process. For simplicity, these students were removed from the sample. As a robustness exercise, I generated a second sample that contains the complete information of the process (both rounds). In this sample, for students who participated in both rounds, I considered the complementary round assignment but the main round preferences because they have fewer restrictions and can better reveal their preferences.

To answer my research questions, I follow one specific cohort of the SAE - those entering to 9th grade in 2019 - and observe their different educational outcomes. I follow this cohort for three reasons: first, 9th grade is the secondary entrance level, so there are more observations. Second, it is possible to obtain baseline characteristics. Third, students are already in a more advanced stage of development, so it is more likely that it has already been detected if they have some special educational needs. There are a total of 78,437 students in the 9th-grade cohort in the main round. Meanwhile, the complementary round has 9,661 students. Since the main goal is to understand the importance of school choice, students who apply only to one school or apply to rural schools are dropped from the observed sample. The system only allows making a single application under two scenarios: the student applies to a rural school or is enrolled in a school that offers continuity of studies and belongs to the SAE. So eliminating both groups avoid the selection of the limited offer or the self-selection.

In addition, the sample is restricted in two additional ways. First, the analysis focuses on students whose admission to SIP school was determined solely by the lottery number. Since those listing a school without over-demand first have guaranteed admission, there is no exogenous variation to exploit. Therefore, I dropped these observations (similar to Hastings et al., 2005; 2006a; Abdulkadiroğlu et al., 2017). Second, for the analysis to be valid, comparing a group of similar students is necessary. For that reason, I eliminate those students who are only applying to schools with SIP or only applying to schools without SIP as their preferences seem to indicate that they are potentially different.

[26]This number does not consider Early Childhood Education and Adult Education.

[27]According to the last census, the Metropolitan Region concentrates 40.5% of the Chilean population. But as I mentioned before, SAE admission was not working in the Metropolitan Region until admission 2020. So, I will not consider it for this study.

Table 1: Summary Statistics Naive Approach

	Full Sample	Type Assigned of School			
		Without SIP	With SIP	Difference	p-value
	(1)	(2)	(3)	(2)-(3)	(4)
Students' characteristics					
Female (%)	0.44	0.45	0.43	0.02	0.330
Age	13.50	13.48	13.52	-0.04	0.164
Specific Learning Difficulties (%)	0.49	0.48	0.49	-0.01	0.494
Attention Deficit Disorder (%)	0.16	0.16	0.17	-0.00	0.778
IQ in Borderline (%)	0.35	0.36	0.34	0.02	0.349
Attendance in 2018 (%)	92.70	93.10	92.44	0.65	0.028
GPA in 2018	5.45	5.44	5.45	-0.01	0.748
Approved 2018 (%)	0.98	0.99	0.98	0.01	0.228
Fail 2018 (%)	0.01	0.01	0.01	-0.00	0.666
Drop-out 2018 (%)	0.00	0.00	0.00	-0,00	0.034
Distance to school	8.96	8.52	9.24	-0.73	0.604
School attended in 2018					
Low Income (%)	0.70	0.73	0.68	0.05	0.002
Medium Income (%)	0.29	0.26	0.31	0.05	0.004
Reading Test Score (%)	233.61	233.90	233.42	0.48	0.495
Reading Math Score (%)	242.20	242,26	242,16	0.09	0.905
Number of Disadvantaged	278,70	268,43	285,24	-16.81	0.011
Students' characteristics at time of application					
Siblings Priority (%)	0.09	0.09	0.09	-0.01	0.543
Working Parent Priority (%)	0.00	0.00	0.00	0.00	0.364
Returning Student Priority (%)	0.03	0.02	0.03	-0.01	0.288
N. Applications	4.84	4.60	5.00	-0.40	0.000
SIP Applications (%)	0.56	0.50	0.59	-0.09	0.000
Number of observations	3,164	1,232	1,932		

Notes: Column 1 reports the media of the full sample. Column 2 reports the media for students assigned to schools without SIP. In contrast, column 3 reports those assigned to a school with SIP. Columns 4 and 5 report the difference between the media of both groups and the p-value. The first eleven rows describe students' characteristics. The following five rows show socio-economic and quality measures of the school attended in 2018. The last five rows show students' characteristics at the time of application.

The focus of the study is to understand the importance for TSEN to get a SIP school. It would be interesting to identify this effect on PSEN students. Nevertheless, this is not possible because the algorithm has a particular process for them explained in Section 3, and it is not possible to use the exogenous variation of the SAE: For that reason students without TSEN are dropped. At the end the final sample has 3.164 students. Using the results of SAE, it is possible to classify the students into two groups: those who were assigned to schools without SIP (1,232 students) and those who were assigned to schools without SIP (1,932 students). Table 1 shows the balance test in observable characteristics for both groups. There are some significant differences, one of them is drop-out in 2018. Among students who were assigned to school without SIP, none dropped out of the school. However, a small proportion of children assigned to schools with SIP did it. Also, there are differences in the socio-economic level. Those who were assigned to schools with SIP came from schools with lower socio-economic levels but with a lower amount of disadvantaged students. This group, on average, makes more applications and prefers more SIP schools.

In conclusion, there are some significant differences between the groups. Additionally, this is a naive approach because it does not consider the ranking of students' preferences. In this study case, the preferences ranking gives information about unobservable characteristics such as the TSEN severity or how SEN is the student for their parents. To address this concern is possible to compare students with similar preferences for SIP schools. The idea is to estimate the effect of SIP exposure in homogeneous groups. Therefore two samples were generated from the declared preferences of students (preference lists) who have similar top two choices, which are the most likely to get the assign and the most preferred for the parents.

Sample 1 or *Priority Preference SIP* selects students who apply to SIP school as the first choice and without SIP as the second. Then, the treaties are those admitted to a SIP school and the control group who do not, regardless of the preference of the admitted school. For example, if a student has a SIP school as first choice and No SIP school as the second choice and gets accepted to a SIP school which is their fourth choice, It will be considered part of the treatment group. There are 469 students in the treatment group and 211 in the control group. One disadvantage of this methodology is that Almost 83% of the treatment students were accepted to their first or second choice. Therefore, It is hard to distinguish how much treatment effect is driven by SIP exposure or first preference. However, the control group seems to be the better counterfactual. Sample 2 or *Secondary Preference SIP* contains the subset of TSEN students with the opposite preferences. So, it selects students who apply to school without SIP as the first choice and SIP as the second. Then, the treaties are also those admitted to SIP schools and controls who do not. There are 360 students in the treatment group and 498 students in the control one.

Table A4 of **Appendix A** compare sample 1 and 2 in observable characteristic. This table shows some differences between both samples. Specifically, sample 2 has more students from low-income schools (at the 5% level) and made fewer applications (at the 1% level). The difference in the percentage of females, students with Attention Deficit Disorder, and the distance to school are statistically significant at the 10% level. These differences must be taken into account when comparing results between samples.

5. Empirical Strategy

In theory, lottery-induced randomization provides a simple solution to the problem of endogenous sorting of students (Cullen et al., 2006). Since the school that students were assigned is randomly (based on their preferences), TSEN students with similar preferences who enter a school with a SIP agreement and those who do not, on average, should be identical in terms of unobservable and observable characteristics. Moreover, the sample selection criteria help to hold this affirmation. Consequently, using OLS provides a consistent estimate of the impact of being accepted into a SIP school and winning the lottery.

I estimate the "intent-to-treat" (ITT) effects of the SIP exposure on TSEN students, which are essentially differences between treatment and control group means. Formally, I estimate ITT effects on an outcome (y) using OLS of the form

$$y_{ic} = \alpha + \beta^{ITT} Treat_{ic} + \gamma \mathbf{X}_{ic} + \zeta_c + \varepsilon_{ic} \tag{1}$$

where y_{icr} is the outcome of interest for student i and county c. The observed outcomes are: take-up, change school during 9th grade, participate in SAE admission 2020, distance to school, change in distance to school in relation to the previous year, attendance, approve 9th grade, fail 9th grade, and drop out 9th grade. Although most interest results are binary, I am interested in the marginal effect; therefore, imposing a specific distribution to the errors using binary models does not significantly benefit. For that reason, my main estimations use OLS[28]. $Treat_{icr}$ is an indicator variable for being randomly assigned to a school with SIP. $\mathbf{X}_{icr}$ is a vector of baseline covariates which includes gender, age, dummy for sibling priority, number of applications and percentage of SIP applications. All of them, except gender, are the significant differences found in the balance in observables. ζ_c is county fixed effect. In addition, I cluster the standard errors by school because randomization occurred at school level.The estimates of β^{ITT} in (1) identify the causal impact of *being offered admission to* a school with SIP.

Since not all families choose to attend the lottery school, these ITT estimates understate the causal effect of being exposed to an integration program. I estimate the impact of *attending* a school with SIP, i.e., "treatment on the treated" (TOT) using admission to SIP school through the lotteries as an instrumental variable for ever attending a school with SIP (Angrist et al., 1996; Clark et al., 2010; Chetty et al., 2016; Ruijs, 2017a). Therefore I estimate a two-stage least squares model, where the first stage is a regression of a binary variable indicating whether the student attended a SIP school (TakeTreat) on treatment status (Treat) and of all other covariates included in the model (1). Formally, the first stage is

$$TakeTreat_{ic} = \alpha_1 + \beta_1 Treat_{ic} + \gamma_1 \mathbf{X}_{ic} + \zeta_{c1} + \varepsilon_{ic1} \tag{2}$$

where $Treat_{ic}$ is an indicator variable for being randomly assigned to a school with SIP. While, $TakeTreat_{ic}$ is an indicator variable which is 1 if the treatment was received and 0 otherwise.

The second stage is a regression of the outcome variable of interest on predicted SIP school attendance and of all other covariates included in model (1). Formally,

$$y_{ic} = \alpha_2 + \beta_2^{TOT} \widehat{TakeTreat}_{ic} + \gamma_2 \mathbf{X}_{ic} + \zeta_{c2} + \varepsilon_{ic2} \tag{3}$$

where β_2^{TOT} provides the estimate of the effect of SIP school attendance on the outcome variable. Two restrictions must be hold to interpret this coefficient as a causal effects. First, the *relevance restriction*, i.e., the admission to a SIP school is highly predictive of whether a student attends a SIP school. **Appendix C** shows first stage results confirming that the lottery is indeed a relevant instrument. Second, the *exclusion restriction* means that the instrument should be uncorreleated with the error term. In other worlds, the Admission to a SIP school (instrument) only affects the outcomes of interest through SIP school attendance. This implies, for example, that disappointment about losing the lottery should not lead to a decrease in motivation for school (Ruijs, 2017a).

The following section reports estimates of (1) and (3) for various outcomes y_i. I begin by analyzing if the baseline characteristics are balanced between the treatment and control groups. I then turn to impacts on satisfaction with the SAE, such as taking the assignation and participating in the following SAE, and academic achievement, such as attendance and passing the grade.

[28] As a robustness check, in Appendix E I estimate specification (1) using a binary model. The conclusions do not change.

6. Results

6.1. *Balance Tests*

A balance in baseline characteristics between the treatment and control group would be expected when there is a random assignment. Table 2 reports summary statistics and balance tests for some baseline covariates for treatment and control groups using sample 1 and 2. The first two columns of Table 2 present the summary statistics for TSEN students with priority preference for SIP (sample 1). Column 1 presents the mean of the control group, while columns 2 shows the difference with the treatment group. According to column 2, students who were assigned to schools with SIP were slightly older. However, there are no statistical differences (at the 5% level) in other students' or schools' characteristics. Additionally, the treatment group, on average, had more siblings priority, a fewer number of applications, and a higher percentage of applications to SIP schools. Since there are significant differences in the baseline, my preferred identification strategy will control those differences. In a randomized experiment, controlling for baseline values of covariates that could explain the variation in the outcome does not affect the expected value of an estimator. Still, it can reduce its variance (Duflo et al., 2006).

The last two columns of Table 2 uses TSEN students with secondary preference for SIP. Students who were assigned to schools with SIP are statistically equal to those assigned to schools without SIP in all characteristics, except the siblings' priority and percentage of applications to SIP schools. The treatment group has a lower probability of having siblings priority and more applications to SIP. Imbalance in sibling priority is expected as parents generally prefer to have all their children in the same school (for simplicity or more information already tested).

In conclusion, as expected, given that the admissions lotteries were random, treatment and control group students exhibited few statistically significant differences in baseline characteristics. For sample 1, four of the 26 characteristics were statistically significant differences at the 5% level. While sample 2 has only two.

6.2. *The impact of SIP schools*

6.2.1. *Satisfaction with SAE Assignation*

Table 3 show the estimates effect of SIP treatment on different students' satisfaction outcomes. Panel A uses students with *priority preference for SIP* (Sample 1), while Panel B uses students with *secondary preference for SIP* (Sample 2). All the specifications consider baseline covariates (gender, age, siblings priority, number of applications, and percentage of SIP applications), county fixed effects, and standard errors are clustered at the school level. Except for column 1, the even columns show the ITT effects, while the odd columns show the TOT effects.

Column 1 reports estimates of the specification in (1) with an indicator for taking up the SAE assignation as the dependent variable y_i. The media of the control group is 69% for this outcome using sample 1 and 80%

Table 2: Summary Statistics and Balance Test for Children in Samples 1 and 2

	Priority Preference For SIP (Sample 1)		Secondary Preference For SIP (Sample 2)	
	Mean Control Group (1)	Balancing Test (2)	Mean Control Group (3)	Balancing Test (4)
Student Characteristics				
Female (%)	0,4076	-0,0003	0,4558	-0,0003
Age 2018	13,3981	0,1456**	13,4779	0,0443
Specific Learning Difficulties (%)	0,4123	0,0696*	0,4940	-0,0412
Attention Deficit Disorder (%)	0,1943	-0,0216	0,1526	-0,0221
IQ in Borderline (%)	0,3934	-0,0479	0,3534	0,0633*
Attendance in 2018	93,0664	-0,3030	93,3514	-0,2570
GPA in 2018	5,4536	-0,0130	5,4476	0,0177
Approved 2018 (%)	0,9716	0,0178*	0,9940	-0,0134*
Fail 2018 (%)	0,028	-0,0178*	0,0060	0,0134*
Distance to school	6,9808	-1,3813	6,3581	0,7370
School attended in 2018				
Low Income (%)	0,7014	0,0171	0,7851	-0,0546*
Medium Income (%)	0,2938	-0,0167	0,2149	0,0463
High Income (%)	0,0047	-0,0047	0,0000	0,0000
Reading test score	235,4739	-2,9546*	232,1687	2,0027
Math test score	244,7156	-3,2662*	240,0221	2,4751*
Academic Motivation Index	75,3855	-0,0853	75,5961	-0,2351
School Environment Index	76,5855	-0,4357	76,9125	-0,6523*
Citizen Participation Index	79,0948	-0,2434	79,5977	-0,6594
Healthy Habits Index	72,3359	-0,2552	73,1093	-0,4639
SAE Admission				
High Performance (%)	0,0711	-0,0199	0,0562	0,0160
Disadvantaged (%)	0,6303	0,0648*	0,6988	-0,0432
Siblings priority (%)	0,0379	0,0900***	0,1365	-0,0671***
Working parent priority (%)	0,0000	0,0021	0,0060	-0,0060
Returning student priority (%)	0,0190	0,0130	0,0281	0,0052
Number of applications	4,5640	-0,5618***	3,8494	0,0534
Applications to SIP (%)	0,5551	0,0299**	0,5636	0,0272***
Number of observations	211	680	498	858

Notes: Columns 1 and 3 report the means for TSEN students who were assigned to a School without SIP. Columns 2 and 4 report separate regression coefficients and standard deviations of the variables indicated in each row on an indicator variable equalling 0 if the students were assigned to a school without SIP and equalling 1 if the student were assigned to a school with SIP. The first ten rows describe students' characteristics. GPA is on scale from 1 to 7. The following five rows show socio-economic and quality measures of the school attended in 2018. Every school index are on a scale from 0 to 100. The last five rows show students' characteristics at the time of application.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

using sample 2. It is important to remember that the control group was also offered a treatment, i.e., being assigned to school without SIP. Among students with *priority preference for SIP*, being offered admission to a school with SIP increases the probability of acceptance of the assignation by 15.8 percentage points. In contrast, for students with *secondary preference for SIP* being offered admission to a school with SIP decreases the probability of acceptance of the assignation by 7.9 percentage points. This opposite effect is not surprising because students were assigned a less preferred choice.

Figure A3 of **Appendix A** shows the non-compliers type of school attended during 2019. In both control and treatment groups, a large percentage of non-compliers ended up attending schools with SIP. Surprisingly, it is often a school that was not in their declared preferences or a least preferred choice in their application list. One possible explanation is that families know well their top preferences. So after they are assigned, they research/learn about the other choices and change their preferences. Another possible explanation, is that during the regularization period, they try to find a school they like more, but only their least preferred choices have available seats.

Table 3: The Impact of being offered a place in a SIP School in Satisfaction Outcomes

	Take-up	Change School		Part. in next SAE		Distance to School		Change in Distance	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)									
SIP School	0,158***	0,009	0,011	0,020	0,024	-1,738	-2,104	0,335	0,407
	(0,041)	(0,015)	(0,017)	(0,022)	(0,025)	(1,361)	(1,555)	(1,479)	(1,687)
Mean Control Group	0.69	0.02	0.03	0.07	0.09	8.17	7.89	1.25	1.46
Number of Observations	660	660	680	660	680	658	677	654	673
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)									
SIP School	-0,079**	-0,005	-0,007	0,021	0,030	-0,794	-1,110	-0,829	-1,158
	(0,034)	(0,014)	(0,018)	(0,020)	(0,027)	(0,938)	(1,242)	(0,961)	(1,268)
Number of Observations	824	824	858	824	858	811	843	808	840
Mean Control Group	0.80	0.04	0.05	0.04	0.04	8.90	8.93	2.58	2.53
Method	OLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS

Notes: Columns 1, 2, 4, 6, and 8 report ITT estimates from OLS regressions of an outcome on indicators for being assigned to a school with SIP. . Columns 3, 5, 7, and 9 report TOT estimates using a 2SLS specification, instrumenting for attending a SIP school with the treatment assignment indicators. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard errors, reported in parentheses, are clustered by schools. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in column 1 is an indicator for the student taking up a lottery assignation. For columns 2 and 3 is change the school during the year. For columns 4 and 5, the dependent variable is participating in the following admission process, while columns 6 and 7 are the distance to school. Finally, for columns 8 and 9 is the change in distance to the school in relation to the previous year.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Concerning other satisfaction metrics there is no statistically significant difference on changing schools during the 9th-grade or participation in the following SAE process between schools with SIP and without it for both samples. Additionally, columns 6 and 8 estimate the ITT effect on the distance to school and the change in distance relative to 2018. Columns 7 and 9 estimates the TOT; there are no statistically significant effects on either of those outcomes. So, students assigned to SIP schools stay in the same school (at least two years) in the same proportion as those assigned to a school without SIP. Also, students assigned to a SIP school do not have to travel more than students assigned to a school without a SIP. After the SAE, the students leave 8-9 km from their school on average. However, both groups have to travel more than the past year (about 1.5-2.5 km extra).

In conclusion, Table 3 shows evidence that the School Integration Program affects students' satisfaction in the same way as schools without this program. However, there are statistically significant effects on the assignation take-up, and the direction of the effects (positive or negative) depend on the preferences families disclose in their applications.

6.2.2. Academic Measures

Table 4 presents estimates of SIP treatment effects on students' academic outcomes. Panel A uses students with *priority preference for SIP* (Sample 1). In contrast, Panel B uses students with *secondary preference for SIP* (Sample 2). All the specifications consider baseline covariates (gender, age, siblings priority, number of applications, and percentage of SIP applications), county fixed effects, and standard errors are clustered at the school level. Except for column 1, the even columns show the ITT effects, while the odd columns TOT effects.

Column 1 (ITT) shows the estimates of the specification in (1) with attendance as the dependent variable. The media of the control group using sample 1 is 87.91%, and using sample 2 is 86.90%, which is expected given that Chile's school attendance levels are generally high. Being offered admission to a SIP school does not have a statistically significant effect on this measure, independent of the sample or the type of estimator (ITT or TOT). This finding is interesting because it rejects the hypothesis that receiving extra tools motivates students declared with TSEN in 2018 to attend more classes.

Columns 3 and 4 have the approval of 9th-grade as dependent variables. Column 3 shows that the media of the control group for students with *priority preference for SIP* is 80%, while for students with *secondary preferences for SIP* is 53% which is relatively low. Being offered a place in a SIP school has a different impact depending on the preferences. For students with *priority preference for SIP* it decreases the probability of passing the academic year by 8.8 percentage points (column 3). This effect is significant at the 10% level. However, the TOT estimate is 10.6 percentage points (column 4) significant at the 5% level. In contrast, for students with *secondary preferences for SIP* being assigned to a school with SIP increases the passing probability by 13.7 percentage points (column 3). Moreover attending increases the probability by 19.3 percentage points (column 4). As expected, the opposite occurs when the outcome variable is failing 9th grade (columns 5 and 6), and there is no significant effect on drop-out (columns 7 and 8).

In sum, the results suggest that being offered a place in a SIP school has a significant effect on passing courses for students with TSEN. However, the direction of the effect depends of the type of preferences.

Table 4: The Impact of being offered a place in a SIP School in Academic Outcomes

	Attendance		Approve		Fail		Drop-out	
	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)								
SIP School	-0,832	-0,992	-0,088*	-0,106**	0,086**	0,104**	0,011	0,014
	(1,918)	(2,152)	(0,046)	(0,053)	(0,039)	(0,044)	(0,018)	(0,021)
Number of Observations	647	668	660	680	660	680	660	680
Mean Control Group	87.91	85.99	0.80	0.76	0.14	0.15	0.03	0.04
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)								
SIP School	0,348	0,482	0,137***	0,193***	-0,141***	-0,198***	-0,006	-0,008
	(1,571)	(2,054)	(0,030)	(0,039)	(0,028)	(0,037)	(0,014)	(0,019)
Number of Observations	811	846	824	858	824	858	824	858
Mean Control Group	86.68	86.90	0.73	0.73	0.22	0.21	0.04	0.04
Method	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS

Notes: Columns 1, 3, 5, and 7 report ITT estimates from OLS regressions of an outcome on indicators for being assigned to a school with SIP. Columns 2, 4, 6, and 8 report TOT estimates using a 2SLS specification, instrumenting for attending a SIP school with the treatment assignment indicators. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard errors, reported in parentheses, are clustered by schools. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in columns 1 and 2 is attendance. For columns 3 and 4 is an indicator variable of approbation. For columns 5 and 6, an indicator variable of failing while columns 7 and 8 for drop-out.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

The key implication of Tables 3 and 4 for our analysis of SIP exposure effects is that the students with *priority preferences for SIP* and students with *secondary preferences for SIP* are affected differently in some outcomes. Regarding changing school during the year, participating in the following admission process, distance to school, attendance, and drop-out, on average, the school integration programs have no statistically significant effect for both groups. However, for students with *priority preferences for SIP*, on average, the effect on the probability of assist to a SIP school is positive (15.8 percentual points), but the impact on grade passing is negative (-8.8 percentual points). In contrast, for children with *secondary preferences for SIP*, the impact on take-up is negative (-7.9 percentual points) but increases the probability of grade passing (13.7 percentual points). The TOT effects are more extensive in absolute value for both types of students.

The results could be driven by differences in some characteristics related to the school and parental choices. A possible hypothesis is that students with *priority preference for SIP* are assigned to more distant schools or are assigned to a school with higher academic demand, affecting their academic achievement. Another explanation is that parents made selective compensation. That is, parents with *priority preference for SIP* have a greater understanding of their child's needs. Therefore, if they are not assigned to a school with SIP, they might substitute the tools SIP schools give with alternative supporting activities. In contrast, parents *secondary preference for SIP* do not commit this substitution. The following section examines some of these possible mechanisms.

7. Discussion

Sections 6.2.1 and 6.2.2 indicate that for students with *priority preference for SIP*, the School Integration Programs have a positive impact on take-up, beside the negative impact on approval probability. In contrast, for students with a *secondary preference for SIP*, the impact on take-up is negative but increases the probability of approbation. In this section, I discuss different channels which could explain the results.

7.1. *The impact of SIP schools on failing*

It is possible to distinguish at least two reasons for failing the school year. First, students could fail if their attendance rate is less than 85 % of the school days or because of grades[29]. It is important to understand the reason behind the failure to shed light on the mechanism behind the results. Table 5 shows that the negative effect on failure for students with a *priority preference for SIP* assigned to a school with an integration program is driven by grades. This is interesting because it establishes that students do not pass due to performance problems related to the schools' quality, not because they stop attending school. On the other hand, panel B shows that students with a *secondary preference for SIP* being assigned to a school with SIP decrease the probability of failing by attendance and grades. However, the impact is more extensive on failing by grades.

[29]Failing by grades occurs if a student fails a subject and has a GPA lower than 4.5 or if he fails two subjects and has a GPA lower than 5.0.

Table 5: Reason of Failure

	ITT (1)	TOT (2)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)		
Fail	0,086**	0,104**
	(0,039)	(0,044)
Fail by Attendance	-0,005	-0,006
	(0,019)	(0,022)
Fail by Grades	0,090***	0,109***
	(0,034)	(0,038)
Number of Observations	660	680
PANEL B:SECONDARY PREFERENCE FOR SIP (SAMPLE 2)		
Fail	-0,141***	-0,198***
	(0,028)	(0,037)
Fail by Attendance	-0,047***	-0,066***
	(0,016)	(0,021)
Fail by Grades	-0,094***	-0,133***
	(0,025)	(0,033)
Number of Observations	824	858

Notes: Column 1 reports ITT estimates from OLS regressions of an outcome on indicators for being assigned to a school with SIP. In contrast, column 2 reports TOT estimates using a 2SLS specification, instrumenting for attending a SIP school with the treatment assignment indicators. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard errors, reported in parentheses, are clustered by schools. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. For each panel, the dependent variable in row 1 is an indicator variable of failing. For row 2 is an indicator variable of failing by attendance, while row 3 is failing by grades.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Distance to School

So far, there is evidence that the effect on failure is driven by final grades (GPA). One hypothesis is that the distance to school could affect their performance. For example, if students in SIP spend more time traveling to school, the study hour after school could be affected by time and quality reduction (less motivation, more tiredness). However, this conjecture is not supported by the results. The previous section shows that there are no significant differences in the average distance to school between students who were offered schools with and without SIP for both types of students (see Columns 6 and 7 of Table 3).

School Quality

Another hypothesis is that schools' quality drives the differential effect. So, parents with *priority preference for SIP* applied to high-performing SIP schools, which are more difficult or more academically demanding. In contrast, parents with *secondary preference for SIP* applied to low-performing SIP schools. Table A5 presents some quality measures (SIMCE score and personal development indices) comparing treatment and control schools. The results suggest that the schools with and without SIP in sample 1 are statistically similar in many respects. However, on average, schools with SIP have a lower score in mathematics (5 points), science (3 points), and school environment index (0.73 points). In contrast, the schools with SIP in sample 2 have considerably lower quality levels than schools without SIP. In fact, schools with SIP, on average, are 20 points lower in math and around 10 points lower in reading.

On the other hand, Table A6 compare school without SIP for both samples (i.e., schools of the control group) and also compare school with SIP (i.e., schools of treaties). Column (2) shows that schools without SIP between samples have no statistical differences in academic scores, but schools from sample 2 have a better school environment and citizen participation index. In contrast, schools with SIP are statistically different. Students with *secondary preferences for SIP* were assigned to schools with lower academic and development indices than students with *priority preferences.* To sum up, parents with *priority preference for SIP* apply to better SIP schools than parents with *secondary preferences for SIP.* But both apply to similar schools without SIP (at least academically). Therefore, it appears that the SIP schools from sample 1 are the elite of schools with SIP, which creates incentives to keep their quality status. Thus, even if facilities and tools are given to children with special educational needs, the high exigence increases the probability of failure. In contrast, the schools with SIP in sample 2 also provide tools but are less demanding, positively impacting approval.[30]. This suggests that the effects on passing probability are driven by school quality differences, which is a proxy of academic demand.

Another possibility is that, although schools without SIP in both samples have similar academic quality, those in Sample 1 do not know how to assess TSEN students and help them with their grades at the end of the year. Whereas schools without SIP in sample 2 do not. A naive way to test this implication is to identify bunching around 4.5 and 5.0. However, Figure A4 shows little evidence of clustering for both types of schools. Therefore, the results suggest that this is not the mechanism behind the results (at least the main channel).

Diagnostic Renewal

Another channel to explore is related to diagnostic renewal. As explained before, students were considered TSEN during 2018. However, if they assist a SIP school during 2019, the school must reevaluate the diagnosis. Therefore, being assigned to a school with SIP does not imply that the diagnosis is renewed[31]. There are different reasons for not renewing the diagnosis. For example, the student has already exceeded his educational need, or the school has more children with TSEN than the subsidy limit, so those who receive it need more help.

[30]Table A7 consider that some school are more academic demanding than other, so it includes as a control variable the mean of the dependent variable in the same school-type of teaching-grade in the previous year. This makes it possible to compare students with similar previous rates. The effects are robust to these control variables.

[31]Figure A5 shows the probability of renewal of the diagnosis per year in three ways: conditional on keeping in the system, being in a school with SIP, and keeping in the same school. It is essential to mention that at the beginning of the academic year of 2020, the covid-19 pandemic began. Therefore, some schools could not adequately carry out the renewal process, opting to renew the diagnosis without evaluation. This could be explaining the spike.

Table A8 splits the treatment group in two: those who keep TSEN diagnosis during 2019 and those who do not. Then, I compare both groups with the control group, which by default do not keep their diagnosis[32]. Given that the renewal of the diagnosis is correlated with unobservables that affect the outcome of interest, the results presented in Table A8 cannot be interpreted as causal. Panel A highlights that the negative effect on approval is correlated with the not being renewed diagnosis. This makes sense if the renewal of the diagnosis leads to the delivery of tools that help them in learning, and the children whose school does not renew their TSEN status need this help and did not receive it. On the other hand, for children in panel B (secondary preference for SIP) for whom the diagnosis is not renewed, the effect of being in school with SIP on approval continues to be positive and significant. However, it is less than for those who renew the diagnosis.

Complementing this result with what was found regarding the quality of schools, a possible hypothesis is that in elite schools, there is an insignificant spillover effect of the tools delivered to children who have a renewed diagnosis with those who do not renew the diagnosis.

To sum up, this section provides evidence that the school quality might drive the effects. The most important finding is that students with *priority preferences for SIP* apply to higher academic quality SIP schools. In contrast, students with *secondary preferences for SIP* apply to low academic quality SIP schools. Therefore, higher academic difficulty drives the negative effect for the students in sample 1.

7.2. *The impact of SIP schools on take-up*

Why are parents applying and going to a school which decreases their children's' passing probability? There are two possible explanations developed in this subsection.

One explanation is that informational problems exist. That is, the parents rank school without knowing the effects on the passing probability of their child. Then, the take-up effect is driven only by the preferences' ranking. So, the positive effect for students with priority preference for SIP is because a important percent (about 83%) of treaties get their first choice. However, being assigned to a SIP for students with secondary preferences for SIP means that they are not getting their first choice. Therefore, the effect is negative.

An alternative explanation, which might be more realistic, is that the parents experimented. That means parents are trying to optimize his decisions while improving his information simultaneously (as a classic multi-armed bandit problem described by Robbins (1952)). Parents have a trade-off between exploration (trying out each school to find the best one) and exploitation (playing the choice believed to give the best payoff). In this case, parents know that elite SIP schools have a lower approbation rate; nevertheless, the benefit of going to those schools could be higher than the cost. So, they try with this option hoping they will be successful, but if it does not work, they change their child to another school[33]. Approval is not the only success metric. Another essential metric is the quality of match with the school regarding values, environment, and belonging. However, I do not have the information to explore this hypothesis in depth.

[32]To be considered as a student with Special Education Needs, the student must go to a school with SIP.

[33]To validate this hypothesis, Figure A6 shows the effect of being assigned to a school with SIP on monthly attendance. However, the evidence of exploration is not clear because the negative impact on attendance during November and December could be explained by the "Estallido Social" (massive demonstrations and riots that led to some schools having to close).

8. Heterogeneous Effects

There may be differential effects of the School Integration Program for different groups of students. To measure this, I tested specifications (1) and (3) to understand variability in three different student characteristics: gender, TSEN diagnoses, and administration type. First, it could be that girls do better in schools with SIP, as girls are generally more capable of working independently than boys (Ruijs, 2017a). Second, the results could differ according to the type of TSEN the student was diagnosed with because they face different learning barriers. Third, there may be differential effects by type of administration (public or bond) given the socio-economic group that attends and the management capacities[34]. Tables from **Appendix D** show these heterogeneous effects.

First, Table D1 shows that girls with priority preference for SIP have a higher treatment take-up: about 26.1 percentage points (at the 1% level), while boys just 10.1 percentage points (at the 10% level). The negative effect on take-up for students with secondary preferences for SIP is only valid for girls (at the 10% level). The null hypothesis of equal effects of the School Integration Program by gender cannot be rejected for other satisfaction outcomes. On the other hand, Table D2 shows the impact on academic outcomes. For attendance and drop-out, there are also no differential effects by gender. However, the negative impact on approval for TSEN students with priority preferences for SIP is driven by boys, which is in line with the hypothesis that changes in the environment are more costly for boys. But, these negative effects do not appear in boys with secondary preferences for SIP. However, the positive impact is higher for girls. This differential pattern of results has been found in many studies in the school setting (e.g.,Kling et al., 2007; Hastings et al., Hastings et al.; Angrist et al., 2009).

Second, Tables D3 and D4 show the effect of SIP on students' satisfaction and academic outcomes by type of TSEN. An interesting finding is that the positive effect on failing 9th-grade for students with *priority preference for SIP* is driven by students with Attention Deficit Disorder (ADD). Also, being assigned to a school with SIP does not increase their take-up and decreases their attendance. On the other hand, the effect of SIP on students with ADD and *secondary preference for SIP* in approving 9th-grade is zero. This could suggest that students with ADD need other tools or practices for their learning process. According to the literature, some of the good practices are seating ADD children in front seats, providing frequent breaks between learning (Purdie et al., 2002), and using assistive technology that helps them with organization and better study habits (Black and Hattingh, 2020).

Third, Tables D5 and D6 show the estimates of run specification 1 for public and voucher school separately. The results using sample 1 should be taken with caution because only 10% of the students in the control group were assigned to a public school. However, there is no important difference between both types of administration.

In conclusion, It seems School integration Programs have different effects on the probability of passing for boys versus girls. On average, the impact is non significant or positive for girls, while negative or positive (but smaller in absolute value) for boys. Also, the results show that the effect of School Integration Programs

[34] According to Castillo et al. (2011), public schools target all social strata, while voucher schools are more segmented. In addition, there would be no significant differences in school management, but public schools are more efficient in obtaining additional monetary resources.

differ by the type of TSEN. It seems that inclusion programs are less beneficial for children with ADD in academic terms (measured in their probability of passing on to the next grade).

9. Robustness Check

This study uses random assignments from admissions lotteries to estimate the effects of integration programs. However, some methodological decisions were made (restrict the sample) to compare students with similar characteristics before the assignment. In this section, I explore the impact of SIP using other samples. I considered four criteria: (1) the first or the second choice is a school with SIP (called sample 3); (2) students who have been denied a SIP school (called sample 4); (3) students with at least a half of their applications are a school with SIP (called sample 5); (4) students with at least one of their applications is a school with SIP[35] (called sample 6).

Appendix B shows the balances in observable characteristics within the different samples. In general, the control and treatment groups are very similar in most aspects. However, there is a trade-off between the size of the sample and the similarity of the students. This is not surprising since making the sample selection criteria more flexible makes it more likely that there is more heterogeneity among students.

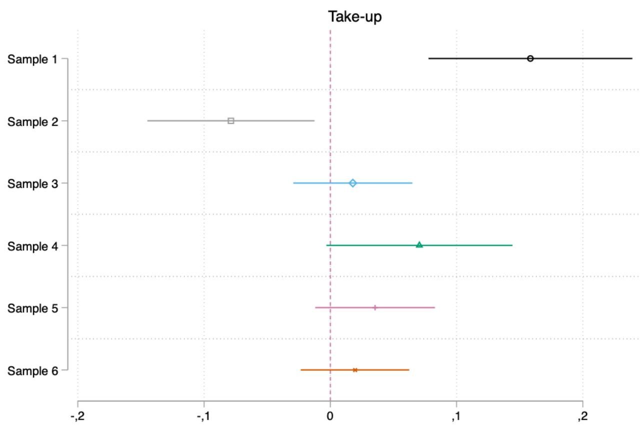
Figure 1 shows the impact of being offered a place in a SIP school on satisfaction outcomes using samples 1 to 6, while figure 2 shows the effects on academic outcomes. Every sample has a different color, and the ITT coefficient is represented with a circle and TOT with a square. The results suggest no significant differences in satisfaction outcomes between schools with SIP and without it, using other samples. However, the coefficient of assignation take-up and participation in the Next SAE is always positive. In contrast, the distance to school has a negative coefficient. Only using sample 6, there is a statistically significant effect (at the 5% level), which means students assigned to SIP school goes to schools closer to their homes. On the other hand, there are no significant effects on Attendance or drop-out. Regarding approval, being offered a place in a SIP school has a positive coefficient, although there is only significant at the 10% level in samples 5 and 6. As expected, the failure coefficient is negative but only significant on samples 5 and 6.

In sum, the non-significant differences found in section 6.2.1 and 6.2.2 are robust using other samples. However, the significant effect on take-up is not robust using other samples, which could be explained because the selection criteria do not focus on obtaining the first choice. Regarding approval, the results are more similar to those of students with secondary preferences for SIP. Schools' quality could explain this. Table B3 shows that schools with and without SIP of samples 4 to 6 follow the same quality pattern to SIP schools of sample 2. Schools with SIP have a lower quality than those from sample 1, but schools without SIP are statistically similar to those from sample 1.

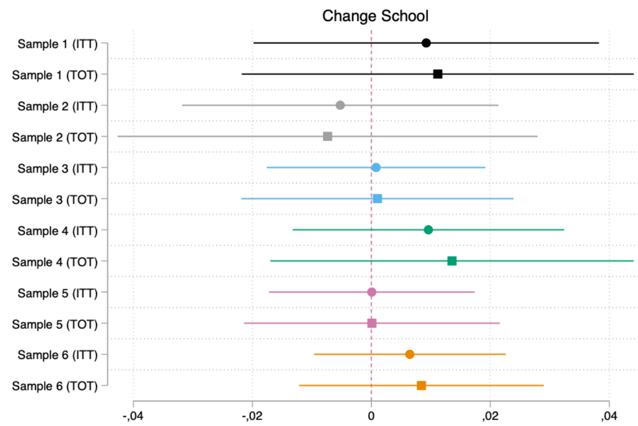
[35] In the initial sample, I drop students who only apply to SIP school or that, on the contrary, only applies to schools without SIP.

Figure 1: Satisfaction Outcomes

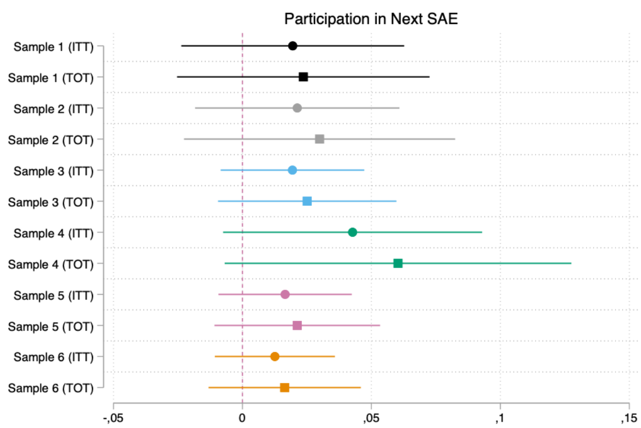
(a) Take-up



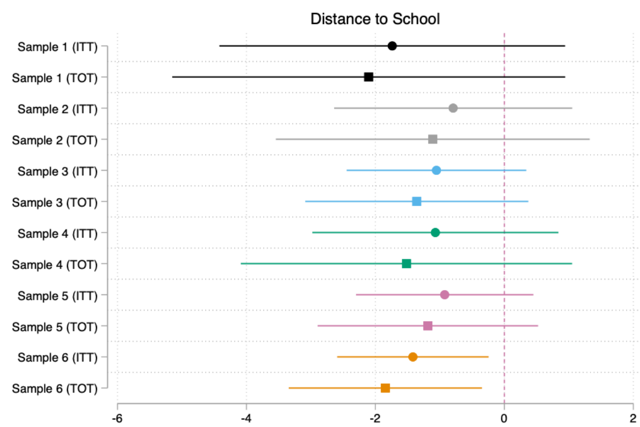
(b) Change School



(c) Part. next SAE



(d) Distance to School



(e) Change in Distance

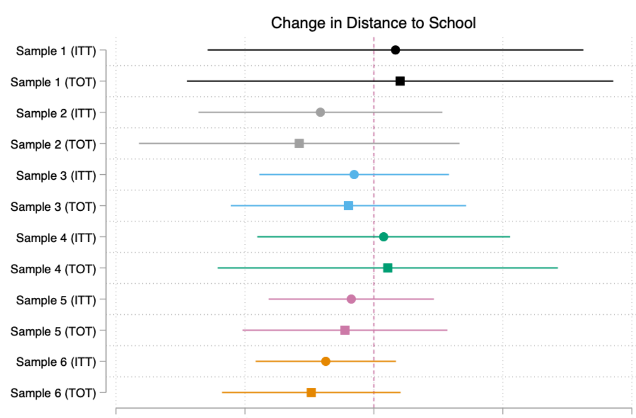
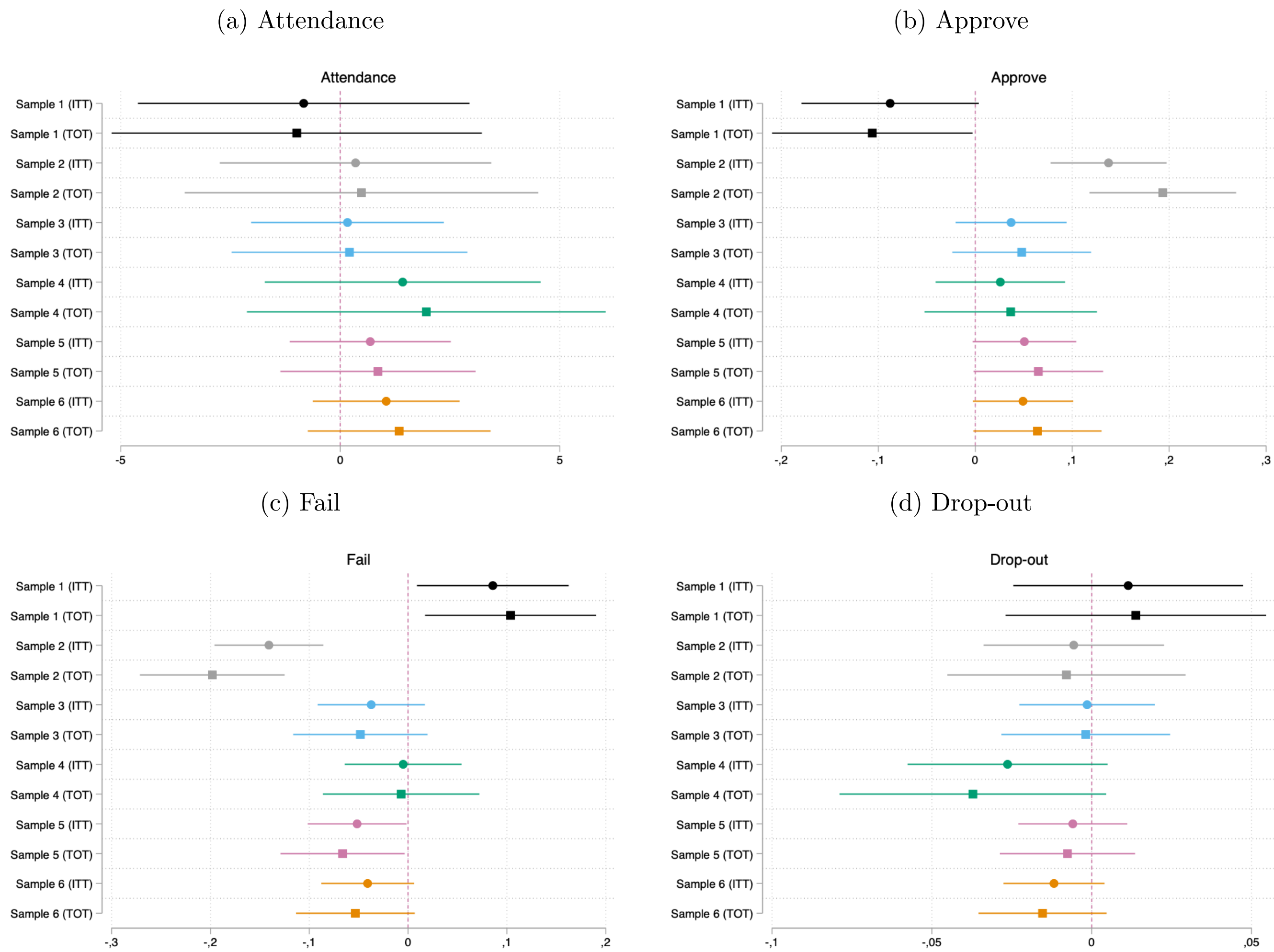


Figure 2: Academic Outcomes



(a) Attendance (b) Approve (c) Fail (d) Drop-out

Appendix E explores four others robustness check. First, I re-estimate specification (1) using a logit for binary outcomes (take-up, change school, participate in the following SAE, approve, fail, and drop-out). Second, I check the robustness of the TOT effects excluding of the sample controls who are non-compliers and ended up attending to SIP schools. Third, re-estimates specification 1 and 3 using both round of SAE. Fourth, including the satisfaction and academic outcomes, 9 different variables were tested in the analyses shown above. At a 5% significance level, there still a high possibility that one variable will turn out significant by chance that is I will commit type I error[36]. Therefore, I re-estimates the adjusted p-values using different methods for multiple testing. Most of the results are robust.

10. Conclusion

This study investigates the causal effect of School Integration Programs by exploiting school admission lotteries for students with similar preferences. The results show evidence that a school's participation in the SIP programs affects short-term students' outcomes. However, the effects depend on the preferences

[36]The probability of at least one significant result is 0.37. This came from $P(\text{at least one significant result}) = 1 - P(\text{no significant results}) = 1 - (1 - 0.05)^9 = 0.37$

families disclose in their applications. For students with what I defined as *priority preferences for SIP*, the effect on grade passing is negative. While for students with *secondary preferences for SIP* the effect is positive. However, there is no statistically significant effect of School Integration Programs on attendance or short-term satisfaction outcomes. Most notably, there is no significant effect on the distance to school, which indicates students assigned to schools with integration programs do not have to travel more to get this type of education.

Moreover, I provide suggestive evidence that the school quality might drive the effects. Students with *priority preferences for SIP* apply to higher academic quality SIP schools while students with *secondary preferences for SIP* apply to low academic quality SIP schools. Even though the students are in schools that understand their educational needs and have the tools to help them in both cases, the academic difficulty is different.

I have shown evidence that parents are self-selecting into different school qualities. The differences could be related to motivation, how parents value the education, among other unobservable characteristics. It would be interesting to test whether these differences are the mechanisms driving the previous results in future research. It is conceivable that the differences in academic outcomes in either direction are driven by selective parental compensation. Parents with a *priority preference for SIP* have a greater understanding of their child's needs. Therefore, if they are not assigned to a school with SIP, they might substitute the tools SIP schools give with alternative supporting activities (such as private tutoring) or invest more time themselves. In contrast, parents with *secondary preferences for SIP* do not have a finished compression of their child's needs; therefore, they do not commit this substitution.

Also, further investigations could explore the effect of School Integration Programs on ADD students in more detail. I show evidence that students with ADD do not benefit from SIP; indeed, they could be negatively affected. However, the focus of this thesis was not specifically ADD, and some methodological restrictions at the moment to select the sample could be affecting the composition of students with ADD. For example, this type of student could be self-selecting and apply to schools that do not have over-demand or apply only to SIP schools. If that is the case, I am not considering them, and the results lose external validity.

As this study's results are based on 680 to 858 lottery participating students, the results might suffer from a lack of power. Moreover, the analyses are based on non-metropolitan schools; one might worry that the results cannot be generalized to other metropolitan schools. The metropolitan area has better access to professionals, technology, and material resources. On the other hand, my sample considers students who had TSEN in the previous year of the evaluation. Therefore, some students might overcome their necessity and self-select to specific programs biasing my results (the direction of the bias is not clear). However, admission lotteries help with this problem. All sample students apply to schools with and without SIP, but ultimately, the lotteries define which one they attend.

Another issue is that the results are based on short-term aggregate results. Therefore, there are two major limitations. First, the long-term effects (high school graduation, access to tertiary education, or salaries) cannot be observed. These results are even more interesting because even though integration programs seek to provide tools to cope and overcome their educational need, it is not a quick process, and changing schools could have a disruptive effect (Alexander et al., 1996; Hanushek et al., 2001), then the benefits of SIP could be long-term. Second, the analyzed outcomes show aggregated student performance information (for example, passed or failed a course). Other studies have shown that students could benefit more in specific subjects such as mathematics (Hanushek et al., 2002). These limitations arise because the studied cohort is very young, and the pandemic affected the evaluation system, making the performance of standardized tests impossible.

Limitations notwithstanding, this is the first study to investigate the causal effects of School Integration Programs. Overall, the results of this study indicate that School Integration Programs have heterogeneous impacts on TSEN students' academic achievement and depend on the parents' preferences, which is correlated with school quality. This is very useful information for parents, students, and policymakers that are evaluating School Integration Programs. In particular, because my finds show that typical claims such as students have to travel more to school with SIP, or the program does not affect the students' achievement appears to be false.

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12. **Appendix A**

12.1. *Tables*

Table A1: Priority Criteria

First priority	Those applicants who have a sibling in the establishment at the time of applying.
Second priority	15% Priority Students by school grade. This criteria is applied as long as the percentage of priority students by grade in the school is less than 15%.
Third priority	Children of an adult employed by the school.
Fourth priority	Applicants who wish to return to the same school, only if they were not expelled.

Source: "Sistema de Admisión Escolar" website.

Table A2: SEN Classification

PSEN	Deafness and Hearing loss	Hearing loss greater than 40 decibels.
	Blindness and vision impairment	Vision alteration that causes limitations in the reception, integration, and handling of visual information.
	Intellectual Disability	Intellectual performance is significantly below the average, which occurs concurrently with limitations in adaptive behavior, manifested in practical, social, and conceptual skills and, begins before the age of 18.
	Autism Spectrum Disorder	Qualitative alteration of a set of capacities related to social interaction, communication, and mental flexibility.
	Dysphasia	Severe and permanent alteration of all the components of the mechanism of acquisition of the linguistic system.
	Multiple Disabilities and Deaf-blindness	Presence of a combination of physical, medical, educational, and social-emotional needs.
TSEN	Attention Deficit Disorder	Generalized behavior with a clear presence of attention deficit, impulsivity and/or hyperactivity.
	Specific Language Disorder	A significant limitation in the level of oral language development manifested by late-onset or slow development.
	Specific Learning Disorder	Severe Difficulty in learning to read, write and/or learn mathematics.
	Borderline Intellectual Functioning	A score between 70 and 79, inclusive, in a psychometric assessment test of IQ.

Source: "Manual de apoyo a la Inclusión Escolar en el marco de la Reforma Educacional"

Table A3: School with SIP

		Type of funding									
		Public Schools				Voucher Schools				Total	
		Municipal		LES		Subsidized		DAS			
		N	%	N	%	N	%	N	%	N	%
School Integration Program	No	1,199	24.6%	63	27.0%	3,673	65.6%	70	100.0%	5,005	46.4%
	Yes	3,680	75.4%	170	73.0%	1,926	34.4%	0	0.0%	5,776	53.6%
	Total	4,879	100.0%	233	100.0%	5,599	100.0%	70	100.0%	10,781	100.0%

Notes: This table shows the number and percentage of schools with School Integration Program by type of funding. Most public schools (75.3%) have an Integration Program, while only 34% of voucher schools have implemented this program.
Source: Statistics Unit, Study Center, Planning and Budget Division, Ministry of Education.

Table A4: Summary Statistics (Sample 1 and 2)

	Media Sample 1 (1)	Media Sample 2 (2)	Difference (2)-(1)	p-value
Student Characteristic				
Female (%)	0.41	0.46	-0.05	0.057
Age	13,50	13.50	0.00	0.957
Specific Learning Difficulties (%)	0.46	0.48	-0.02	0.523
Attention Deficit Disorder (%)	0.18	0.14	0.04	0.055
Borderline IQ (%)	0.36	0.38	-0.02	0.428
Attendance 2018 (%)	92.86	93.24	-0.39	0.269
GPA in 2018	5.44	5.46	-0.01	0.657
Approved in 2018 (%)	0.98	0.99	-0.00	0.448
Fail in 2018 (%)	0.02	0.01	0.00	0.448
Distance to School	7.15	9.94	-2.79	0.067
School attended in 2018 Characteristics				
Low Income (%)	0.71	0.76	-0.05	0.029
Medium Income (%)	0.28	0.23	-0.05	0.032
High Income (%)	0.00	0.00	0.00	0.261
Reading test score	233.44	233.00	0.44	0.663
Math test score	242.47	241.06	1.41	0.186
Students' characteristics at time of application				
Siblings priority (%)	0.10	0.11	0.01	0.594
Working parent priority (%)	0.00	0.00	0.00	0.439
Returning student priority (%)	0.03	0.03	0.00	0.785
Number of applications	4.18	3.87	0.30	0.001
SIP applications (%)	0.58	0.58	0.00	0.929
Number of Observations	680	858		

Notes: The first column corresponds to the media of sample 1, which includes students with SIPs' school as first preference and without SIP as second preference. The second column corresponds to the media of sample 2, which means students who apply to a school without SIP as first preference and with SIP as second preference. The third column compares the difference between both groups, and the fourth column shows the p-value. The first fifteen rows contain students and school attended in 2018 characteristics. However, the last five rows include variables related to the SAE process, such as students' characteristics at the time of application.

Table A5: School Quality

	Priority Preference for SIP (Sample 1)		Secondary Preference for SIP (Sample 2)	
	Mean w/o SIP (1)	Dif. w/SIP (2)	Mean w/o SIP (3)	Dif. w/SIP (4)
Math test score	254.99	-5,21**	257.76	-20,03***
Reading test score	244.33	-1,60	246.60	-9,71***
Science test score	237.32	-3,28**	238.21	-11,45***
Academic Motivation Index	74.94	-0,19	74.69	-0,43*
School Environment Index	77.27	-0,73**	77.99	-2,78***
Citizen Participation Index	71.22	-0,55	72.23	-2,39***
Healthy Habits Index	78.53	-0,07	78.7	-1,17***

Notes: This table compares the school quality of schools with and without SIP. Columns 1 and 2 used sample 1, which is students with *priority preferences for SIP*, while columns 3 and 4 use sample 2, which is students with *secondary preferences for SIP*. Columns 1 and 3 show the mean of assigned schools without SIP, while columns 2 and 4 the difference with assigned schools with SIP. The table shows that schools with SIP from sample 1 are statistically equal to schools without SIP in many aspects, such as reading test scores, academic motivation index, citizen participation, and healthy habits index. However, schools with SIP from sample 2 have lower quality for all the aspects considered.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A6: School Quality Sample 1 vs Sample 2

	w/o SIP		w/SIP	
	Mean Sample 1 (1)	Sample 2 (2)	Mean Sample 1 (3)	Sample 2 (4)
Math test score	254,99	2,77	249,78	-12,05***
Reading test score	244,33	2,28	242,73	-5,84***
Science test score	237,32	0,89	234,04	-7,28***
Academic Motivation Index	74,94	-0,24	74,74	-0,48**
School Environment Index	77,27	0,73**	76,53	-1,32***
Citizen Participation Index	71,22	1,01***	70,67	-0,83**
Healthy Habits Index	78,53	0,17	78,45	-0,92***

Notes: This table compares the school quality of schools between samples. Columns 1 and 2 compares assigned schools without SIP. While Columns 3 and 4 compare assigned schools with SIP. Column 1 shows the mean of schools without SIP from sample 1, while column 2 shows the difference with schools without SIP from sample 2. Column 3 shows the mean of schools with SIP from sample 1, while column 2 shows the difference with schools with SIP from sample 2. The table indicates that schools without SIP are statistically equal for most of the characteristics between samples, excluding school environment and citizen participation index. However, schools with SIP from sample 2 have lower quality than schools with SIP from sample 1 for all the aspects considered.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A7: The impact of SIP school in Academic Outcomes Adding Controls

	ITT (1)	TOT (2)	ITT (3)	TOT (4)
PANEL A: PRIORITY PREFERENCE FOR SIP				
Attendance[a]	-0,832	-0,992	-0,859	-1,030
	(1,918)	(2,152)	(1,938)	(2,184)
Approve	-0,088*	-0,106**	-0,106**	-0,128**
	(0,046)	(0,053)	(0,045)	(0,051)
Fail	0,086**	0,104**	0,094**	0,113**
	(0,039)	(0,044)	(0,041)	(0,045)
Drop-out	0,011	0,014	0,012	0,014
	(0,018)	(0,021)	(0,019)	(0,022)
Number of Observations	660	680	630	651
Extra Controls			✓	✓
PANEL B: SECONDARY PREFERENCE FOR SIP				
Attendance[b]	0,348	0,482	1,017	1,402
	(1,571)	(2,054)	(1,524)	(1,969)
Approve	0,137***	0,193***	0,159***	0,220***
	(0,030)	(0,039)	(0,030)	(0,038)
Fail	-0,141***	-0,198***	-0,153***	-0,213***
	(0,028)	(0,037)	(0,029)	(0,038)
Drop-out	-0,006	-0,008	-0,006	-0,008
	(0,014)	(0,019)	(0,015)	(0,019)
Number of Observations	824	858	784	819
Extra Controls			✓	✓

Notes: Odd columns report ITT estimates, while even columns report TOT. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects, and the standard errors are clustered at the school level. Columns (3) and (4) include as control the mean of the dependent variable in the same school, grade (9th grade), and type of teaching (academic or different types of vocational) but in the previous year. The number of observations changes because some schools had not this information. Some school has this information for other types of teaching, but not for the one of interest.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

[a] The number of observations for attendance is different because students whose final status is "Drop-out" have a missing value. Therefore, the number of observations for Columns (1) and (2) are 647 and 668, respectively.

[b] The number of observations for attendance is different because students whose final status is "Drop-out" have a missing value. Therefore, the number of observations for Columns (1) and (2) are 811 and 846, respectively.

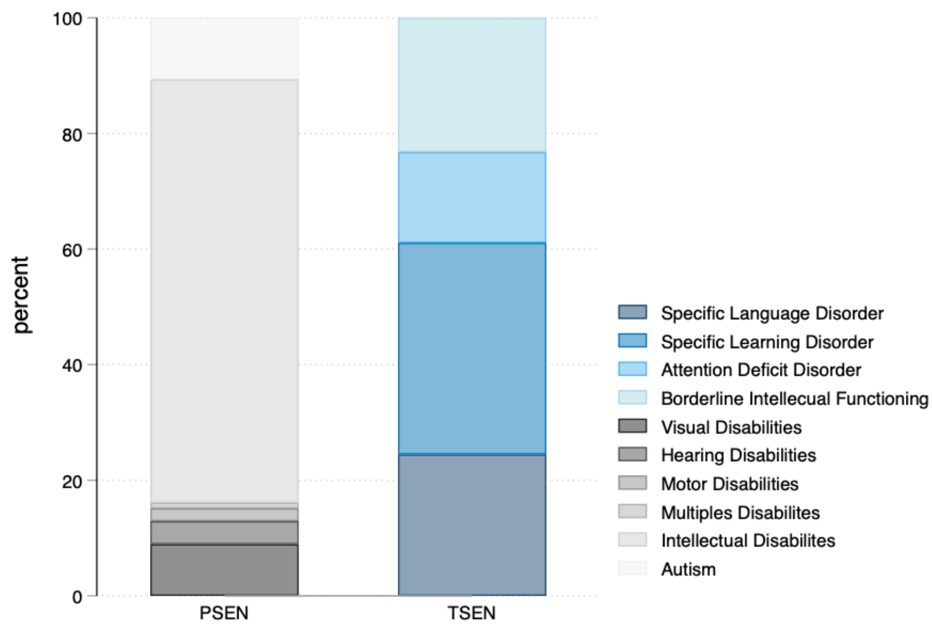
Table A8: Effects for Renewal of Diagnosis

	All Sample	Renovation	
		Yes	No
PANEL A: PRIORITY PREFERENCE FOR SIP			
Complier	0.158***	0.215***	0.142***
	(0.041)	(0.048)	(0.047)
Change School	0.009	-0.012	0.015
	(0.015)	(0.017)	(0.016)
Part. Next SAE	0.020	0.013	0.019
	(0.022)	(0.028)	(0.026)
Dist. To School	-1.738	-3.587**	-0.952
	(1.361)	(1.688)	(1.565)
Change Distance	0.335	-0.218	0.0428
	(1.479)	(1.872)	(1.588)
Attendance	-0.832	2.506	-3.794*
	(1.918)	(2.273)	(2.108)
Approve	-0.088*	-0.039	-0.120**
	(0.046)	(0.064)	(0.046)
Fail	0.086**	0.058	0.107***
	(0.039)	(0.059)	(0.038)
Drop-out	0.011	-0.015	0.038*
	(0.018)	(0.021)	(0.021)
Number of Observations	660	376	460
PANEL B: SECONDARY PREFERENCE FOR SIP			
Complier	-0.074	0.011	-0.142**
	(0.052)	(0.052)	(0.066)
Change School	-0.004	-0.009	0.005
	(0.014)	(0.017)	(0.019)
Part. Next SAE	0.015	-0.005	0.026
	(0.020)	(0.023)	(0.028)
Dist. to School	-0.773	0.261	-1.068
	(0.977)	(1.460)	(1.134)
Change Distance	-0.721	0.486	-1.353
	(0.955)	(0.991)	(1.437)
Attendance	0.348	-0.721	0.490
	(1.574)	(2.334)	(1.702)
Approve	0.145***	0.166***	0.131***
	(0.032)	(0.042)	(0.037)
Fail	-0.139***	-0.176***	-0.119***
	(0.030)	(0.037)	(0.037)
Drop-out	-0.005	0.010	-0.013
	(0.015)	(0.022)	(0.015)
Number of Observations	811	629	649

Notes: Columns 1 estimates specification (1) for satisfaction and academic outcomes. Column 2 shows correlations between being assigned to a school with SIP and the outcomes of interest for students whose diagnosis was renewed by the school. While column 3 those for those whose diagnosis was not renewed by the school. As I mentioned, the results showed in this table are correlations because the renewal of the diagnosis is not random among the students.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

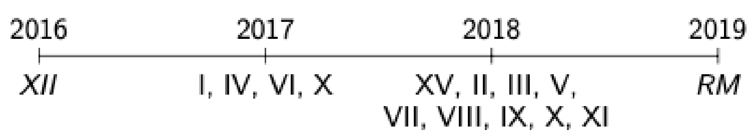
12.2. *Figures*

Figure A1: Special Education Needs Diagnosis



Notes: The figure shows the different diagnoses for students with PSEN and TSEN using the diagnoses obtained after enrollment for the year 2018. The majority of children with PSEN have intellectual disabilities, while the majority of diagnoses of TSEN are specific learning disorders.

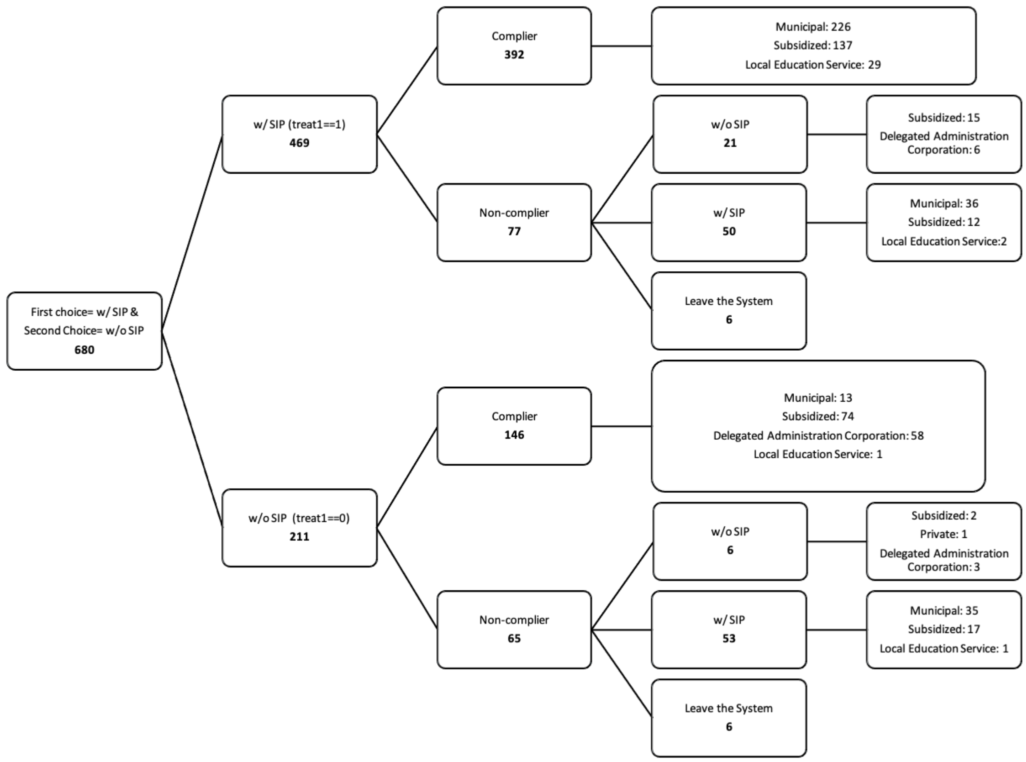
Figure A2: SAE implementation by Region



Notes: The figure shows the regions that have joined the new school admission system (SAE) by year. In 2016 the system was implemented only in the 12th region (Región de Magallanes y Antártica Chilena). Since 2019, all public and voucher schools have implemented the SAE for their admission process.

Figure A3: Type of School Attended

(a) Priority Preference for SIP



(b) Secondary Preference for SIP

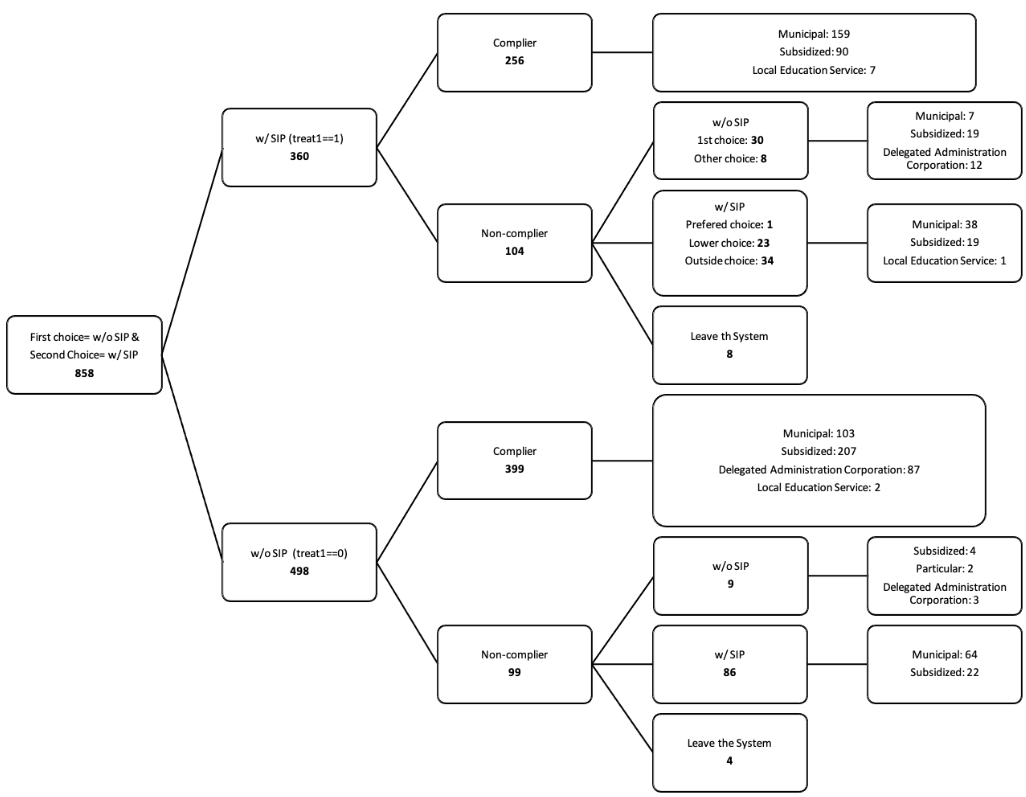
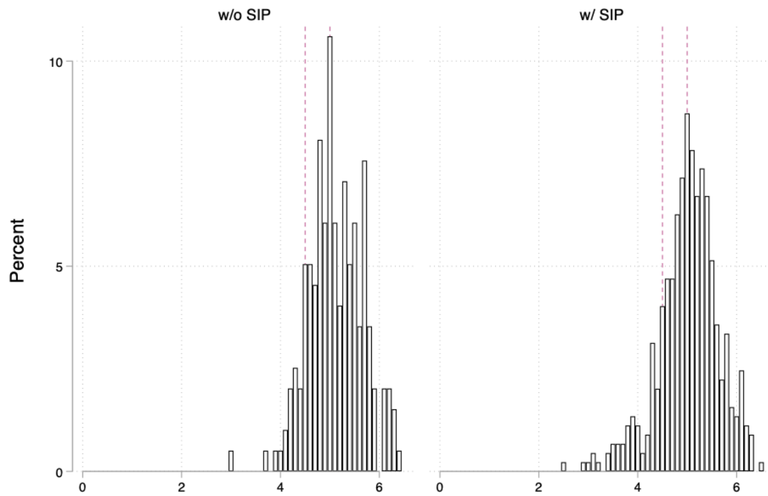
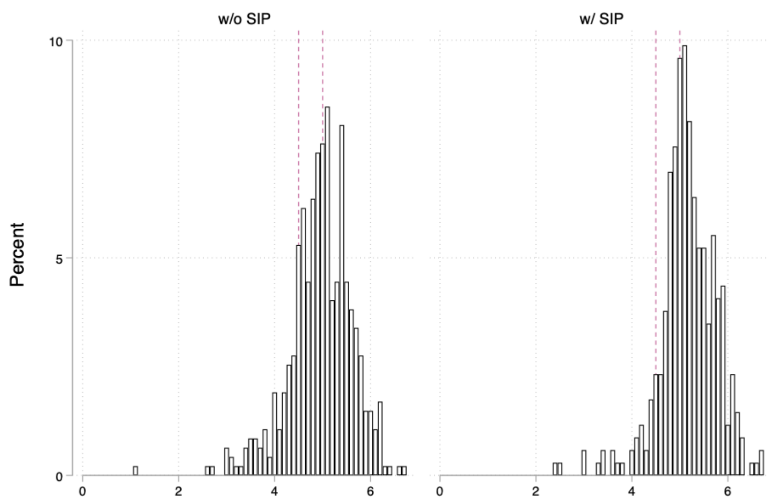


Figure A4: GPA Distribution

(a) Priority Preference for SIP

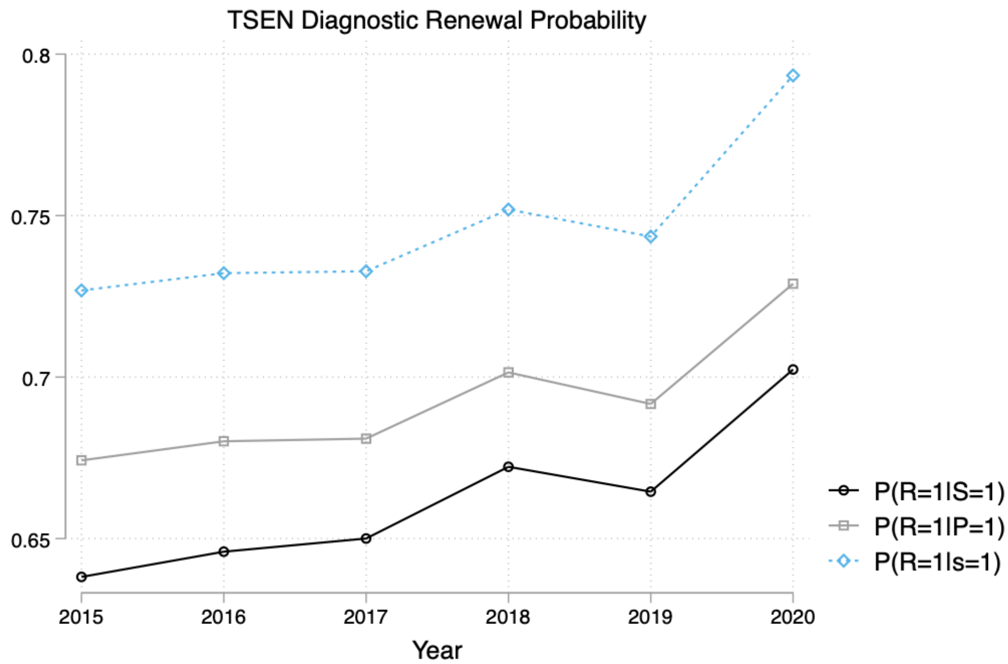


(b) Secondary Preference for SIP



Notes: The figure shows the distribution of GPA separated by schools without SIP (left) and with SIP (right). Panel A the GPA of students with *priority preferences for SIP*, while Panel B considers the GPA of students with *secondary preferences for SIP*.

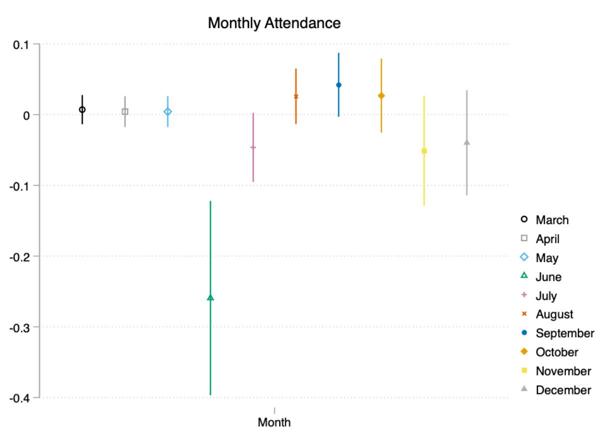
Figure A5: TSEN Diagnostic Renewal Probability



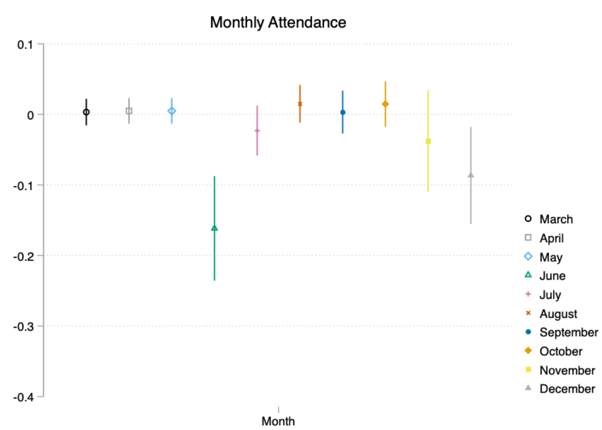
Notes: The figure shows the probability of renewal of the diagnosis for students with TSEN in the following year. The back line shows the probability of renewal conditional on staying in the system ($P(R = 1|S = 1)$). The grey line shows the probability of renewal conditional to being in a school with SIP the following year ($P(R = 1|P = 1)$). Finally, the light blue line is conditional on staying in the same school ($P(R = 1|s = 1)$).

Figure A6: Monthly Attendance

(a) Priority Preference for SIP



(b) Secondary Preference for SIP



Notes: The figure shows the ITT estimates on monthy attendance. Panel A used monthly attendance of students with *priority preferences for SIP*, while Panel B considers monthly attendance of students with *secondary preferences for SIP*. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications and percentage of SIP applications. Also, add county fixed effects. Standard error, reported in parenthesis, are clustered by the school level.

13. Appendix B

This section provides information and results using other samples. Also, I show the results using samples 1, 2, and 3 as a benchmark.

It is important to mention that the samples were created after restricting the sample, as is explained in section 4.1. That is, considering as initial sample the full sample of Table 1. Then, I only consider the students on the main round and exclude students who participate in both processes (as sample 1, 2, and 3). The criteria for sample selection are explained below.

- **Sample 1:** includes TSEN students who have as first choice a school with SIP and second choice without SIP.
- **Sample 2:** includes TSEN students who have as first choice a school without SIP and second choice with SIP.
- **Sample 3:** includes TSEN students who have as first choice or second choice a school with SIP, but not both.
- **Sample 4:** includes TSEN students who have been denied a SIP school.
- **Sample 5:** includes TSEN students who at least a half of their applications are schools with SIP. I like this approach the least since it weights all choices equally, which is counter-intuitive because the top preferences are more important.
- **Sample 6:** include TSEN students who at least one of their applications is a school with SIP (Similar to the *naive approach* in Section 4.1, but restricted to the main sample).

Table B1 shows balancing tests, testing whether there are differences in observable characteristics between TSEN students who won the lottery, therefore are assigned to a School with SIP and TSEN students who do not and are assigned to a School without SIP. Columns (1), (4), and (7) report the means and standard deviations for TSEN students who were assigned to a School without SIP. Columns (2), (5), and (8) report separate regression coefficients and standard deviations of the variables indicated in each row on an indicator variable equalling 0 if the students were assigned to a school without SIP and equalling 1 if the student were assigned to a school with SIP. These regressions do not include controls, county fixed effect, or cluster. While, columns (3), (6), and (9) report similar regression coefficients but include county fixed effects, and the standard errors are clustered by school level.

The balancing tests show that using sample 4, without county fixed effect or cluster, has no significant differences between both groups for nearly all characteristics. The exceptions are a socio-economic group (at the 10% level), percentage of high-performance students (at the 10% level), and percentage of applications to a school with SIP (at the 1% level). While using sample 5 without county fixed effect or cluster, socio-economic group (at the 5% level), healthy habits index (at the 5% level), the number of applications (at the 10% level), and percentage of application to SIP (at the 1% level) have significant differences between control and treaties. Finally, using sample 6 without county fixed effect or cluster, there are more significant differences. Such as age (at the 10% level), attendance (at the 10% level), socio-economic group (at the 1% level), citizen participation index (at the 10% level), healthy habits index (at the 1% level), and percentage of application to SIP (at the 1% level). Thus, there is a trade-off between the number of observations and the similarity between the control and treatment groups. Adding county fixed effects and clustering standard errors by schools, almost all the differences between groups disappear, except age and percentage of applications to SIP (see columns (3), (6), and (9)). Therefore, it is important to control by these differences to estimate the effect of being assigned to a SIP school.

Tables B2 and B3 compares school within and between samples. In general, schools with SIP are statistically different from schools without SIP in all academic quality measures. Moreover, schools with SIP from samples 4, 5, and 6 have lower quality than those from sample 1. This support the idea that parents with *priority preference for SIP* apply to high-quality SIP school ("elite SIP Schools").

Table B1: Descriptive Statistics and Covariate Balance

	Sample 4			Sample 5			Sample 6		
	Control Group Mean	without FE and cluster	with FE and cluster	Control Group Mean	without FE and cluster	with FE and cluster	Control Group Mean	without FE and cluster	with FE and cluster
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Student Characteristics									
Female (%)	0,448	-0,009	-0,084*	0,448	-0,032	-0,053	0,437	-0,020	-0,054
	(0,028)	(0,036)	(0,048)	(0,018)	(0,023)	(0,043)	(0,015)	(0,020)	(0,038)
Age 2018	13,411	0,084	0,134**	13,472	0,047	0,063*	13,474	0,049*	0,058*
	(0,035)	(0,051)	(0,058)	(0,026)	(0,034)	(0,037)	(0,021)	(0,029)	(0,032)
Specific Learning Difficulties (%)	0,448	0,040	0,047	0,463	0,028	-0,014	0,477	0,015	-0,018
	(0,028)	(0,036)	(0,039)	(0,019)	(0,023)	(0,026)	(0,015)	(0,020)	(0,021)
Attention Deficit Disorder (%)	0,196	-0,006	-0,005	0,164	-0,006	-0,015	0,161	0,001	-0,008
	(0,022)	(0,029)	(0,030)	(0,014)	(0,017)	(0,020)	(0,011)	(0,015)	(0,017)
IQ in Borderline (%)	0,356	-0,034	-0,042	0,373	-0,022	0,030	0,363	-0,016	0,026
	(0,027)	(0,034)	(0,038)	(0,018)	(0,022)	(0,025)	(0,014)	(0,019)	(0,021)
Attendance in 2018 (%)	92,798	-0,795	-0,830	93,222	-0,467	-0,217	93,217	-0,511*	-0,188
	(0,366)	(0,567)	(0,668)	(0,234)	(0,339)	(0,345)	(0,192)	(0,286)	(0,310)
GPA in 2018	5,455	-0,026	-0,064*	5,427	0,006	-0,027	5,437	0,004	-0,019
	(0,026)	(0,034)	(0,034)	(0,017)	(0,022)	(0,024)	(0,013)	(0,018)	(0,022)
Approved 2018 (%)	0,979	-0,002	-0,010	0,986	-0,004	-0,006	0,987	-0,002	-0,003
	(0,008)	(0,011)	(0,012)	(0,004)	(0,006)	(0,006)	(0,003)	(0,005)	(0,005)
Fail 2018 (%)	0,021	0,002	0,010	0,014	0,003	0,005	0,013	0,002	0,003
	(0,008)	(0,011)	(0,012)	(0,004)	(0,006)	(0,006)	(0,003)	(0,005)	(0,005)
Distance to school	6,625	-0,787	-0,997	6,680	-0,910*	-0,493	6,404	-0,620	-0,538
	(0,688)	(0,860)	(0,933)	(0,451)	(0,550)	(0,647)	(0,337)	(0,460)	(0,524)
School attended in 2018									
Low Income (%)	0,684	-0,064*	-0,039	0,733	-0,042**	-0,003	0,746	-0,049***	-0,011
	(0,026)	(0,035)	(0,039)	(0,016)	(0,021)	(0,024)	(0,013)	(0,018)	(0,022)
Medium Income (%)	0,310	0,062*	0,034	0,262	0,038*	-0,001	0,249	0,046***	0,008
	(0,026)	(0,034)	(0,037)	(0,016)	(0,021)	(0,023)	(0,013)	(0,018)	(0,021)
High Income (%)	0,003	-0,003	-0,004	0,001	-0,001	-0,002	0,001	-0,001	-0,001
	(0,003)	(0,003)	(0,004)	(0,001)	(0,001)	(0,002)	(0,001)	(0,001)	(0,002)
Reading test score	235,437	-1,223	-1,329	234,000	-0,512	-0,235	233,760	-0,378	-0,001
	(1,102)	(1,387)	(1,448)	(0,725)	(0,888)	(1,021)	(0,573)	(0,755)	(0,888)
Math test score	244,877	-1,210	-1,216	241,989	-0,079	-0,230	241,961	0,056	0,103
	(1,239)	(1,602)	(1,593)	(0,805)	(0,998)	(1,109)	(0,630)	(0,835)	(0,950)

Continued on next page

Table B1 – *Continued from previous page*

	Sample 4			Sample 5			Sample 6		
	Control Group Mean	without FE and cluster	with FE and cluster	Control Group Mean	without FE and cluster	with FE and cluster	Control Group Mean	without FE and cluster	with FE and cluster
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Academic Motivation Index	75,565	-0,305	-0,297	75,445	-0,120	-0,152	75,492	-0,105	0,022
	(0,264)	(0,336)	(0,379)	(0,172)	(0,218)	(0,267)	(0,142)	(0,186)	(0,234)
School Environment Index	76,643	-0,453	-0,564	76,549	-0,278	-0,185	76,572	-0,303	-0,067
	(0,328)	(0,428)	(0,459)	(0,213)	(0,269)	(0,288)	(0,173)	(0,229)	(0,248)
Citizen Participation Index	79,115	-0,661	-0,642	79,105	-0,337	-0,335	79,249	-0,388*	-0,156
	(0,324)	(0,419)	(0,479)	(0,227)	(0,274)	(0,311)	(0,184)	(0,234)	(0,259)
Healthy Habits Index	72,415	-0,672	-0,342	72,677	-0,646**	-0,354	72,823	-0,689***	-0,163
	(0,364)	(0,476)	(0,519)	(0,244)	(0,307)	(0,342)	(0,201)	(0,264)	(0,309)
High Performance (%)	0,074	-0,028*	-0,029	0,062	-0,010	-0,007	0,058	-0,000	0,004
	(0,014)	(0,017)	(0,021)	(0,009)	(0,011)	(0,012)	(0,007)	(0,009)	(0,011)
SAE Admission									
Disadvantaged (%)	0,607	0,019	0,057	0,666	-0,000	0,019	0,676	-0,008	0,022
	(0,027)	(0,035)	(0,040)	(0,018)	(0,022)	(0,023)	(0,014)	(0,019)	(0,020)
Siblings priority (%)	0,040	-0,012	-0,018	0,091	0,008	0,009	0,089	0,009	-0,001
	(0,011)	(0,013)	(0,015)	(0,011)	(0,014)	(0,017)	(0,008)	(0,012)	(0,014)
Working parent priority (%)				0,004	-0,003	-0,005	0,003	-0,001	-0,002
				(0,002)	(0,002)	(0,003)	(0,002)	(0,002)	(0,003)
Returning student priority (%)	0,021	0,004	0,003	0,018	0,011	0,015	0,021	0,008	0,008
	(0,008)	(0,011)	(0,016)	(0,005)	(0,007)	(0,010)	(0,004)	(0,006)	(0,009)
Number of applications	4,963	0,201	0,238	4,290	0,176*	0,097	4,330	0,095	0,004
	(0,133)	(0,158)	(0,182)	(0,076)	(0,092)	(0,107)	(0,059)	(0,076)	(0,091)
Applications to SIP (%)	0,567	0,136***	0,116***	0,627	0,050***	0,037***	0,514	0,121***	0,083***
	(0,010)	(0,010)	(0,011)	(0,004)	(0,005)	(0,005)	(0,005)	(0,006)	(0,007)

Notes: Columns (1), (4), and (7) report the means and standard deviations for TSEN students who were assigned to a School without SIP. Columns (2), (5), and (8) report separate regression coefficients and standard deviations of the variables indicated in each row on an indicator variable equalling 0 if the students were assigned to a school without SIP and equalling 1 if the student were assigned to a school with SIP. These regressions do not include controls, county fixed effect, or cluster. Columns (3), (6), and (9) report similar regression coefficients but include county fixed effects, and the standard errors are clustered by school level. Every school index are on a scale from 0 to 100. However, GPA is on scale from 1 to 7.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table B2: School Quality Other Samples

	Sample 3			Sample 4			Sample 5			Sample 6		
	w/o SIP (1)	w/SIP (2)	p-value (3)	w/o SIP (4)	w/SIP (5)	p-value (6)	w/o SIP (7)	w/SIP (8)	p-value (9)	w/o SIP	w/SIP	p-value
Math test score	256.93	244.57	0.000	254.09	242.16	0.000	256.52	244.63	0.000	257.53	244.99	0.000
Reading test score	245.93	240.21	0.000	244.40	238.04	0.000	246.01	239.51	0.000	246.17	239.77	0.000
Science test score	237.94	230.89	0.000	237.38	228.84	0.000	238.08	230.49	0.000	238.12	230.7	0.000
Academic Motivation Index	74.77	74.54	0.175	74.77	73.73	0.000	74.84	74.29	0.000	74.89	74.4	0.000
School Environment Index	77.78	75.96	0.000	77.02	75.01	0.000	77.78	75.80	0.000	77.71	75.85	0.000
Citizen Participation Index	71.93	70.31	0.000	70.93	69.16	0.000	71.82	69.94	0.000	71.87	70.07	0.000
Healthy Habits Index	78.65	78.05	0.004	78.32	76.99	0.000	78.70	77.81	0.000	78.81	77.88	0.000
Number of Observations	709	824		326	461		724	1,293		709	824	

Notes: This table compares the school quality of schools with and without SIP. Columns 1 to 3 used sample 3, columns 4 to 6 used sample 4, columns 7 to 9 used sample 5, and finally columns 10 to 12 used sample 6. Columns 1, 4, 7, and 10 show the mean of schools without SIP, while columns 2, 5, 8, and 11 the mean of schools with SIP. Columns 3, 6, 9, and 12 show the p-value of the difference. The table suggest that schools with SIP have lower quality than schools without SIP for all the aspects considered.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table B3: School Quality Using Other Samples

	w/SIP				w/o SIP			
	Mean Sample 1	Sample 4	Sample 5	Sample 6	Mean Sample 1	Sample 4	Sample 5	Sample 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Math score	249,78	-6,93***	-6,37***	-7,01***	254,99	-2,55	2,61	3,13
Reading score	242,73	-4,61***	-4,10***	-4,33***	244,33	0,22	2,50*	2,27*
Science score	234,04	-4,87***	-4,58***	-4,88***	237,32	0,19	1,22	0,98
AM Index	74,74	-0,95***	-0,55***	-0,50***	74,94	-0,46	-0,12	-0,06
SE Index	76,53	-1,50***	-0,98***	-1.00***	77,27	-0,71	0,71**	0,55*
CP Index	70,67	-1,4***	-0,94***	-0,88***	71,22	-0,83*	0,77**	0,80**
HH Index	78,45	-1,39***	-0,84***	-0,83***	78,53	-0,58	0,27	0,36

Notes: This table compares the school quality of schools between samples. Columns 1 to 4 compares assigned schools with SIP. While Columns 5 to 8 compare assigned schools without SIP. Column 1 shows the mean of schools without SIP from sample 1. Column 2 to 4 shows the difference between schools with SIP from sample 1 with schools with SIP from sample 4, 5, and 6, respectively. Column 5 shows the mean of schools without SIP from sample 1. Column 6 to 8 shows the difference between school without SIP from sample 1 without schools with SIP from sample 4, 5, and 6, respectively. The table indicates that schools without SIP are statistically equal for most of the characteristics between samples. However, schools with SIP from sample 1 have higher quality than those from other samples.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

14. **Appendix C**

Table C1 reports the first stage coefficients belonging to the IV regressions in Tables 3 and 4. For all variables, the first stage coefficients are large (0.824 and 0.839) and significant at the 1% level. On the other hand, the F-statistics are high, ranging from 1141.35 to 1237.5, above the rule of thumb of minimum of 10. Also, Cragg-Donald Wald F statistic are reported which reject the null hypothesis of weak instruments.

Table C1: First Stages Coefficients for Satisfaction Outcomes

	Change School TakeTreat (1)	Part in next SAE TakeTreat (2)	Distance to School TakeTreat (3)	Change in Distance TakeTreat (4)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)				
Treat	0,826***	0,826***	0,826***	0,824***
	(0,024)	(0,024)	(0,024)	(0,024)
Number of Observations	680	680	677	673
F-statistic	1187,33	1187,33	1187,36	1141,35
Cragg-Donald Wald F-statistic	762,48	762,48	760,38	739,01
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)				
Treat	0,711***	0,711***	0,715***	0,716***
	(0,031)	(0,031)	(0,030)	(0,031)
Number of Observations	858	858	843	840
F-statistic	529.69	529.69	554.35	543.22
Cragg-Donald Wald F-statistic	886.17	886.17	887.17	888.65

Notes: Each column reports the first stage of IV regressions with treatment assignation as an instrument for attending a School with SIP. Panel A shows the first stages for satisfaction outcomes using sample 1 (i.e., students applying to SIP as first choice and w/o SIP as the second choice). In contrast, Panel B shows the first stage using sample 2 (i.e., students applying to w/o SIP as first choice and w/SIP as the second choice). Controls include gender, student age, sibling priority, number of applications, and percentage of SIP applications. All regressions include county fixed effects, and the standard errors, reported in parenthesis, are clustered at the school level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table C2: First Stages Coefficients for Academic Outcomes

	Attendance TakeTreat (1)	Approve TakeTreat (2)	Fail TakeTreat (3)	Drop-out TakeTreat (4)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)				
Treat	0,839***	0,826***	0,826***	0,826***
	(0,024)	(0,024)	(0,024)	(0,024)
Number of Observations	668	680	680	680
F-statistic	1237,5	1187,33	1187,33	1187,33
Cragg-Donald Wald F-statistic	810,66	762,48	762,48	762,48
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)				
Treat	0,723***	0,711***	0,711***	0,711***
	(0,031)	(0,031)	(0,031)	(0,031)
Number of Observations	846	858	858	858
F-statistic	546.7	529.69	529.69	529.69
Cragg-Donald Walds F-statistic	955.01	886.17	886.17	886.17

Notes: Each column reports the first stage of IV regressions with treatment assignation as an instrument for attending a School with SIP. Panel A shows the first stages for satisfaction outcomes using sample 1 (i.e., students applying to SIP as first choice and w/o SIP as the second choice). In contrast, Panel B shows the first stage using sample 2 (i.e., students applying to w/o SIP as first choice and w/SIP as the second choice). Controls include gender, student age, sibling priority, number of applications, and percentage of SIP applications. All regressions include county fixed effects, and the standard errors, reported in parenthesis, are clustered at the school level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

15. **Appendix D**

There may be differential effects of the School Integration Program for different groups of students. To measure this, in section, I tested specifications (1) and (3) to understand variability in three different student characteristics: gender, TSEN diagnoses, and administration type.

15.1. *Gender*

First, it could be that girls do better in schools with SIP, as girls are generally more capable of working independently than boys (Ruijs, 2017a).

Table D1 shows that girls with priority preference for SIP have a higher treatment take-up: about 26.1 percentage points (at the 1% level), while boys just 10.1 percentage points (at the 10% level). The negative effect on take-up for students with secondary preferences for SIP is only valid for girls (at the 10% level). The null hypothesis of equal effects of the School Integration Program by gender cannot be rejected for other satisfaction outcomes. On the other hand, Table D2 shows the impact on academic outcomes. For attendance and drop-out, there are also no differential effects by gender. However, the negative impact on approval for TSEN students with priority preferences for SIP is driven by boys, which is in line with the hypothesis that changes in the environment are more costly for boys. But, these negative effects do not appear in boys with secondary preferences for SIP. However, the positive impact is higher for girls. This differential pattern of results has been found in many studies in the school setting (e.g.,Kling et al., 2007; Hastings et al., Hastings et al.; Angrist et al., 2009).

Table D1: Heterogeneous Effects by Gender on Satisfaction Outcomes

	Take-up		Change School		Part. in next SAE		Distance to School		Change in Distance	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)										
Treat	0,101*	0,261***	0,011	0,015	0,012	0,036	-1,673	-0,324	1,067	1,008
	(0,058)	(0,078)	(0,021)	(0,024)	(0,030)	(0,044)	(1,742)	(1,873)	(2,567)	(2,196)
Number of Observations	386	257	386	257	386	257	384	256	382	254
Mean Control group	0.73	0.64	0.02	0.02	0.08	0.06	8.56	7.59	1.15	1.39
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)										
Treat	-0,069	-0,085*	-0,012	0,009	0,006	0,031	-0,339	-0,505	-1,093	0,649
	(0,051)	(0,049)	(0,017)	(0,029)	(0,021)	(0,042)	(1,212)	(1,544)	(1,311)	(1,344)
Number of Observations	438	364	438	364	438	364	428	360	425	360
Mean Control Group	0.78	0.83	0.04	0.05	0.03	0.05	8.94	8.85	3.64	1.35

Notes: All columns report ITT estimates on satisfaction outcomes. The odd columns estimate the impact of School Integration Programs for boys, while even columns for girls. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county and region fixed effects. Standard errors, reported in parenthesis, are clustered by the school level. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in columns 1 and 2 is an indicator for the student taking up a lottery assignation. For columns 3 and 4 is change the school during the year. For columns 5 and 6, the dependent variable is participating in the following admission process, while columns 7 and 8 are the distance to school. Finally, for columns 9 and 10 is the change in distance to the school in relation to the previous year.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D2: Heterogeneous Effects by Gender on Academic Outcomes

	Attendance		Approve		Fail		Drop-out	
	Male (1)	Female (2)	Male (3)	Female (4)	Male (5)	Female (6)	Male (7)	Female (8)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)								
Treat	-3,636	5,129	-0,169***	0,047	0,141**	0,004	0,025	-0,024
	(2,652)	(3,797)	(0,060)	(0,073)	(0,054)	(0,053)	(0,025)	(0,036)
Number of Observations	378	251	386	257	386	257	386	257
Mean Control Group	88.93	86.42	.79	.81	.15	.12	.03	.03
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)								
Treat	0,999	0,615	0,138***	0,152***	-0,162***	-0,121***	-0,001	-0,019
	(2,636)	(2,039)	(0,046)	(0,045)	(0,042)	(0,043)	(0,025)	(0,013)
Number of Observations	432	358	438	364	438	364	438	364
Mean Control Group	85.56	88.02	0.68	0.78	0.25	0.18	0.06	0.03

Notes: All columns report ITT estimates on academic outcomes. The odd columns estimate the impact of School Integration Programs for boys, while even columns for girls. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county and region fixed effects. Standard errors, reported in parenthesis, are clustered by the school level. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in columns 1 and 2 is attendance. For columns 3 and 4 is an indicator variable of approbation. For columns 5 and 6, an indicator variable of failing while columns 7 and 8 for drop-out.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

15.2. *Type of diagnosis*

Second, the results could differ according to the type of TSEN the student was diagnosed with because they face different learning barriers.

Table D3: Heterogeneous Effects by Type of TSEN on Satisfaction Outcomes

	Take-up			Change School			Part. in next SAE			Dist. to School			Change in Distance		
	SLD (1)	ADD (2)	IQB (3)	SLD (4)	ADD (5)	IQB (6)	SLD (7)	ADD (8)	IQB (9)	SLD (10)	ADD (11)	IQB (12)	SLD (13)	ADD (14)	IQB (15)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)															
Treat	0,154**	0,138	0,213***	0,013	-0,031	-0,008	0,084**	0,026	-0,020	-1,948	1,059	-2,132	-0,003	2,853	1,502
	(0,073)	(0,148)	(0,075)	(0,028)	(0,046)	(0,030)	(0,033)	(0,088)	(0,043)	(1,899)	(1,838)	(2,252)	(2,317)	(2,679)	(2,381)
N. of Observations	288	108	220	288	108	220	288	108	220	287	108	219	284	107	219
Mean Control Group	0.76	0.66	0.64	0.03	0.02	0.01	0.03	0.10	0.10	7.59	7.23	9.24	0.58	-0.14	2.62
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)															
Treat	-0,049	-0,158	-0,101	-0,008	0,024	0,003	0,032	-0,050	0,038	-0,695	-1,815	-1,654	-0,559	-1,933	-2,299
	(0,047)	(0,124)	(0,063)	(0,023)	(0,049)	(0,013)	(0,026)	(0,057)	(0,037)	(1,501)	(1,976)	(1,703)	(1,440)	(1,911)	(2,118)
N. of Observations	382	97	295	382	97	295	382	97	295	375	95	289	375	94	287
Mean Control Group	0.76	0.66	0.64	0.04	0.04	0.05	0.03	0.08	0.04	8.97	6.69	9.75	1.92	3.90	2.95

Notes: All columns reports ITT estimates on satisfaction outcomes. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications and percentage of SIP applications. Also, add county and region fixed effects. Standard errors, reported in parenthesis, are clustered by the school level. SLD=Specific Learning Disorder; ADD= Attention Deficit Disorder; IQB=Borderline Intellectual Functioning.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table D4: Heterogeneous Effects by Type of TSEN on Academic Outcomes

	Attendance			Approve			Fail			Drop-out		
	SLD	ADD	IQB	SLD	ADD	IQB	SLD	ADD	IQB	SLD	ADD	IQB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)												
Treat	-1,969	-6,559*	3,653	0,007	-0,235*	-0,070	-0,041	0,290***	0,103	0,024	0,049	-0,025
	(2,807)	(3,860)	(3,806)	(0,058)	(0,122)	(0,077)	(0,051)	(0,094)	(0,069)	(0,027)	(0,039)	(0,034)
N. of Observations	284	104	215	288	108	220	288	108	220	288	108	220
Mean Control Group	88.37	91.82	85.59	0.80	0.83	0.78	0.15	0.10	0.14	0.03	0.00	0.05
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)												
Treat	-0,810	5,381	2,350	0,123**	0,166	0,167***	-0,124***	-0,195*	-0,156***	0,008	-0,034	-0,027
	(2,025)	(3,571)	(3,601)	(0,048)	(0,105)	(0,053)	(0,044)	(0,114)	(0,046)	(0,020)	(0,033)	(0,034)
N. of Observations	374	96	292	382	97	295	382	97	295	382	97	295
Mean Control Group	88.44	86.30	84.40	0.76	0.71	0.69	0.2	0.24	0.24	0.03	0.05	0.06

Notes: All columns reports ITT estimates on academic outcomes. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications and percentage of SIP applications. Also, add county and region fixed effects. Standard errors, reported in parenthesis, are clustered by the school level. SLD=Specific Learning Disorder; ADD= Attention Deficit Disorder; IQB=Borderline Intellectual Functioning.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

15.3. *Type of Administration*

Third, there may be differential effects by type of administration (public or voucher) given the socio-economic group that attends and the management capacities. According to Castillo et al. (2011), public schools target all social strata, while voucher schools are more segmented. In addition, there would be no significant differences in school management, but public schools are more efficient in obtaining additional monetary resources.

Table D5: Heterogeneous Effects by Type of Administration on Satisfaction Outcomes

	Take-up		Change School		Part. in next SAE		Distance to School		Change in Distance	
	Voucher (1)	Public (2)	Voucher (3)	Public (4)	Voucher (5)	Public (6)	Voucher (7)	Public (8)	Voucher (9)	Public (10)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)										
Treat	0,180***	0,332***	0,039	0,046	0,048	0,050	-2,195	0,503	1,423	-1,354
	(0,060)	(0,119)	(0,026)	(0,064)	(0,045)	(0,057)	(1,443)	(0,679)	(1,884)	(1,791)
Number of Observations	333	311	333	311	333	311	333	308	328	308
Mean Control group	0.69	0.67	0.03	0.00	0.07	0.05	8.58	4.44	1.47	-0.72
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)										
Treat	-0,096*	-0,010	-0,003	0,026	-0,025	0,024	-2,250	-1,331	-2,824*	-0,235
	(0,052)	(0,063)	(0,027)	(0,036)	(0,031)	(0,020)	(1,425)	(2,045)	(1,634)	(0,921)
Number of Observations	476	324	476	324	476	324	468	317	468	314
Mean Control Group	0.79	0.84	0.06	0.01	0.05	0.01	9.64	6.68	3.42	0.07

Notes: All columns reports ITT estimates. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications and percentage of SIP applications. Also, add county and region fixed effects. Standard error, reported in parenthesis, are clustered by the school level. The results using sample 1 should be interpreted with caution because there are only 21 observations in the treatment group considering public schools.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D6: Heterogeneous Effects by Type of Administration on Academic Outcomes

	Attendance		Approve		Fail		Drop-out	
	Voucher (1)	Public (2)	Voucher (3)	Public (4)	Voucher (5)	Public (6)	Voucher (7)	Public (8)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)								
Treat	-1,012	-0,512	0,000	-0,011	0,006	-0,062	0,021	0,025
	(3,242)	(5,042)	(0,063)	(0,172)	(0,051)	(0,170)	(0,033)	(0,040)
Number of Observations	324	307	333	311	333	311	333	311
Mean Control group	87.55	91.05	.8	.81	.13	.19	.04	0
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)								
Treat	2,300	-1,543	0,150***	0,208***	-0,150***	-0,168**	-0,011	-0,014
	(1,975)	(3,769)	(0,043)	(0,076)	(0,040)	(0,074)	(0,020)	(0,031)
Number of Observations	470	317	476	324	476	324	476	324
Mean Control Group	85.99	88.77	.73	.73	.22	.22	.05	.03

Notes: All columns reports ITT estimates. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications and percentage of SIP applications. Also, add county and region fixed effects. Standard error, reported in parenthesis, are clustered by the school level. The results using sample 1 should be interpreted with caution because there are only 21 observations in the treatment group considering public schools.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

16. Appendix E

This section explores four other robustness check. First, I compare OLS's estimators with those obtained using a logit model. Second, I check the robustness of the TOT effects excluding of the sample controls who are non-compliers and ended up attending to SIP schools. Third, I re-estimates specification 1 and 3 using both round of SAE. Fourth, Nine different variables were tested in the analyses above.

16.1. *Estimation with Binary Model*

The focus of this study is to estimate the causal effect of School Integration Programs. Therefore, I am interested in the marginal effect, and imposing a specific distribution to the errors using binary models does not significantly benefit. Table D7 and D8 compares the estimator of being assigned to a school with SIP (ITT) using OLS and logit[37] on different binary outcome. Most coefficients do not change much in value and significance level. Except when the outcome of interest is failing the grade, it becomes significant at 10% level.

[37] A logit model was selected (instead of probit) based on Akaike's selection criteria.

Table D7: Satisfaction Outcomes

	Take-up		Change School		Part. Next SAE	
	OLS	Logit	OLS	Logit	OLS	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: PRIORITY PREFERENCES FOR SIP						
Treat	0,147***	0,134***	0,001	-0,000	-0,000	-0,001
	(0,039)	(0,033)	(0,014)	(0,016)	(0,022)	(0,022)
Number of Observations	680	680	680	680	680	612
PANEL B: SECONDARY PREFERENCES FOR SIP						
Treat	-0,084***	-0,083***	-0,011	-0,011	0,023	0,023
	(0,029)	(0,028)	(0,014)	(0,015)	(0,017)	(0,016)
Number of Observations	858	858	858	858	858	858

Notes: All columns report ITT estimates of the effect of being assigned to a school with SIP on binary satisfaction outcomes. Even columns estimate using OLS while the even one's logit model. Columns (1) and (2) have a dependent variable take the assignation. Columns (3) and (4) change the school during the year. Finally, columns (5) and (6) participate in the following admission process. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. However, the specifications do not include county fixed effects because the logit model has problems iterating with fixed effects when it has many dummies. Standard error, reported in parenthesis, are clustered by the school level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D8: Academic Outcomes

	Aprove		Fail		Part. Drop-out	
	OLS	Logit	OLS	Logit	OLS	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: PRIORITY PREFERENCES FOR SIP						
Treat	-0,049	-0,052	0,075**	0,080*	-0,006	-0,005
	(0,044)	(0,046)	(0,037)	(0,041)	(0,015)	(0,016)
Number of Observations	680	680	680	680	680	680
PANEL B: SECONDARY PREFERENCES FOR SIP						
Treat	0,124***	0,126***	-0,116***	-0,122***	-0,021*	-0,021
	(0,028)	(0,030)	(0,027)	(0,030)	(0,011)	(0,014)
Number of Observations	858	858	858	858	858	858

All columns report ITT estimates of the effect of being assigned to a school with SIP on binary academic outcomes. Even columns estimate using OLS while the even one's logit model. Columns (1) and (2) have a dependent variable, an indicator variable that is 1 if the student approves. Columns (3) and (4) an indicator variable that is 1 if the student fails, while columns (5) and (6) for drop-out. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. However, the specifications do not include county fixed effects because the logit model has problems iterating with fixed effects when it has many dummies. Standard error, reported in parenthesis, are clustered by the school level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

16.2. *Treatment on the Treated*

As is explained in Figure A3 some TSEN assigned to the treatment schools, ended up attending other school. Also, this happened with some child of the control group. To estimates the "treatment on the treated" using admission to SIP school through the lotteries as an instrumental variable for ever attending a school with SIP (as Angrist et al., 1996; Clark et al., 2010; Chetty et al., 2016; Ruijs, 2017a). Therefore I estimate a two-stage least squares model, where the first stage is a regression of a binary variable indicating whether the student attended a SIP school (TakeTreat) on treatment status (Treat) and of all other covariates included in the model (1). Where TakeTreat is an indicator variable which is 1 if the treatment was received and 0 otherwise (more details in section 5).

However, some children in the control group do not attend the assigned school. Therefore, I re-estimate the TOT effects excluding this group, i.e., TakeTreat* is an indicator variable that is 1 if the treatment was received, missing value if someone of control does not assist their assigned school, and 0 otherwise. Table D9 and D10 show compares the results using both methodologies. The estimated effects using TakeTreat* in some cases give slightly larger coefficients, but the main conclusions keep. Attending a school with SIP has non-significant effects on satisfaction outcomes (estimated by TOT), attendance, and drop-out. However, the effect on the probability of approbation is negative for students with priority preferences for SIP while positive for students with secondary preferences for SIP.

Table D9: Satisfaction Outcomes

	Treatment on the Treated			
	Change School	Part. Next SAE	Distance to School	Change
	(1)	(2)	(3)	(4)
PANEL A: PRIORITY PREFERENCES FOR SIP				
Treat	0,011	0,024	-2,104	0,407
	(0,017)	(0,025)	(1,555)	(1,687)
Mean Control Group	0.03	0.09	7.89	1.46
Number of Observation	680	680	677	673
Treat*	0,008	0,046*	-1,516	2,462
	(0,022)	(0,025)	(1,849)	(2,014)
Mean Control Group*	0.03	0.08	7.57	0.72
Number of Observations*	615	615	613	610
PANEL B: SECONDARY PREFERENCES FOR SIP				
Treat	-0,007	0,030	-1,110	-1,158
	(0,018)	(0,027)	(1,242)	(1,268)
Mean Control Group	0.05	0.04	8.93	2.53
Number of Observations	858	858	843	840
Treat*	-0,009	0,048*	-1,097	-1,330
	(0,019)	(0,028)	(1,275)	(1,267)
Mean Control Group*	0.04	0.03	8.76	2.37
Number of Observations*	769	759	745	742

Notes: All columns report TOT estimates of the effect of attending a school with SIP on satisfaction outcomes. Column (1) estimates the effect on changing the school during the year. Column (2) on participation in the following SAE. Column (3) and (4) has a dependent variable distance to school and change in distance to school compared to the previous year, respectively. Rows without * consider TakeTreat as an indicator variable which is 1 if the treatment was received and 0 otherwise (as in Tables 3 and 4). While, rows with * consider TakeTreat* as an indicator variable that is 1 if the treatment was received, missing value if someone of control does not assist their assigned school, and 0 otherwise. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard error, reported in parenthesis, are clustered by the school level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D10: The Impact of assist a SIP School in Satisfaction Outcomes

	Treatment on the Treated			
	Attendance	Approve	Fail	Drop-out
	(1)	(2)	(3)	(4)
PANEL A: PRIORITY PREFERENCES FOR SIP				
Treat	-0,992	-0,106**	0,104**	0,014
	(2,152)	(0,053)	(0,044)	(0,021)
Mean Control Group	85.99	0.76	0.15	0.04
Number of Observation	668	680	680	680
Treat*	-1,820	-0,122**	0,097*	0,005
	(2,732)	(0,062)	(0,056)	(0,027)
Mean Control Group*	85.65	0.75	0.17	0.05
Number of Observations*	609	615	615	615
PANEL B: SECONDARY PREFERENCES FOR SIP				
Treat	0,482	0,193***	-0,198***	-0,008
	(2,054)	(0,039)	(0,037)	(0,019)
Mean Control Group	86.9	0.73	0.21	0.04
Number of Observations	846	858	858	858
Treat*	0,469	0,206***	-0,213***	-0,010
	(2,197)	(0,041)	(0,039)	(0,020)
Mean Control Group*	86.90	0.73	0.21	0.04
Number of Observations*	751	759	759	759

Notes: All columns report TOT estimates of the effect of attending a school with SIP on academic outcomes. Column (1) estimates the effect on attendance. Column (2) on an indicator variable that is 1 if the student approves. Column (3) has a dependent variable, an indicator variable that is 1 if the student fails, while column (4) for drop-out. Rows without * consider TakeTreat as an indicator variable which is 1 if the treatment was received and 0 otherwise (as in Tables 3 and 4). While, rows with * consider TakeTreat* as an indicator variable that is 1 if the treatment was received, missing value if someone of control does not assist their assigned school, and 0 otherwise. All the specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county and region fixed effects. Standard error, re-ported in parenthesis, are clustered by the school level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

16.3. *Using both rounds of SAE*

As is explained in Section 4, I considered students who only participate in the main round because the students who participate in the complementary round could be potentially different. However, some students participate in both rounds; thus, the valid lottery comes from the complementary process. For simplicity, these students were removed from the sample. In this section, I used students' assignment lotteries from both rounds. I considered the complementary round assignment but the main round preferences because they have fewer restrictions and can better reveal their preferences for students who participated in both rounds. It is important to mention that the gains in the number of observations are small (less than 70 observations). Therefore, most of the conclusions presented above keep.

Table D11 shows the effect on satisfaction outcomes. Panel A, which used students with priority preference for SIP, shows that the estimated effects do not change their significance, however using students with secondary preferences for SIP, participate in the following SAE turn significant. This could be driven by students participating in the complementary round because they are restricted to schools that still have available seats. Therefore, It is more probable that parents assigned to SIP schools dislike the SAE assignment and decide to participate in the following SAE. Additionally, most of the coefficients are slightly bigger in absolute value.

Table D11: The Impact of being Assigned a SIP School in Satisfaction Outcomes

	Take-up	Change School		Part. in next SAE		Distance to School		Change in Distance	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)									
SIP School	0,113***	0,008	0,011	0,028	0,036	-1,495	-1,920	-0,104	-0,134
	(0,039)	(0,014)	(0,017)	(0,021)	(0,025)	(1,370)	(1,666)	(1,580)	(1,920)
Mean Control Group	0.66	0.02	0.03	0.07	0.09	8.14	7.64	1.38	1.06
Number of Observations	722	722	743	722	743	718	738	712	732
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)									
SIP School	-0,101***	-0,002	-0,003	0,053**	0,081***	-0,388	-0,586	-0,770	-1,159
	(0,032)	(0,013)	(0,019)	(0,021)	(0,031)	(0,887)	(1,274)	(0,879)	(1,256)
Number of Observations	906	906	940	906	940	893	925	889	921
Mean Control Group	0.78	0.04	0.05	0.04	0.05	8.76	8.48	2.49	2.31
Method	OLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS

Notes: Columns 1, 2, 4, 6, and 8 report ITT estimates from OLS regressions of an outcome on indicators for being assigned to a school with SIP. . Columns 3, 5, 7, and 9 report TOT estimates using a 2SLS specification, instrumenting for attending a SIP school with the treatment assignment indicators. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard errors, reported in parentheses, are clustered by schools. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in column 1 is an indicator for the student taking up a lottery assignation. For columns 2 and 3 is change the school during the year. For columns 4 and 5, the dependent variable is participating in the following admission process, while columns 6 and 7 are the distance to school. Finally, for columns 8 and 9 is the change in distance to the school in relation to the previous year.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D12 shows the effects on academic outcomes. Panel A, shows that School Integration Programs have a non-significant effect on all academic outcomes. This suggests that students from complementary round do well in school with SIP, driving the effect found in the table 4 to become 0. However, panel B shows that the results do not change for students with *secondary preferences for SIP*.

To sum up, using both rounds of lottery assignment, most of the results found in section 6 do not change. Except, the effect of being assigned to a SIP school in the passing probability for students with *priority preferences for SIP* which turn to a non-significant effect.

Table D12: The Impact of being Assigned a SIP School in Academic Outcomes

	Attendance		Approve		Fail		Drop-out	
	ITT (1)	TOT (2)	ITT (3)	TOT (4)	ITT (5)	TOT (6)	ITT (7)	TOT (8)
PANEL A: PRIORITY PREFERENCE FOR SIP (SAMPLE 1)								
SIP School	-0,926	-1,178	-0,060	-0,078	0,060	0,077	0,010	0,013
	(1,746)	(2,099)	(0,047)	(0,057)	(0,039)	(0,048)	(0,016)	(0,020)
Number of Observations	708	730	722	743	722	743	722	743
Mean Control Group	87.64	86.65	0.82	0.80	0.13	0.13	0.03	0.04
PANEL B: SECONDARY PREFERENCE FOR SIP (SAMPLE 2)								
SIP School	0,312	0,466	0,130***	0,198***	-0,131***	-0,199***	-0,008	-0,012
	(1,590)	(2,249)	(0,031)	(0,043)	(0,027)	(0,040)	(0,015)	(0,021)
Number of Observations	892	927	906	940	906	940	906	940
Mean Control Group	86.43	86.05	0.73	0.74	0.21	0.19	0.05	0.05
Method	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS

Notes: Columns 1, 3, 5, and 7 report ITT estimates from OLS regressions of an outcome on indicators for being assigned to a school with SIP. . Columns 2, 4, 6, and 8 report TOT estimates using a 2SLS specification, instrumenting for attending a SIP school with the treatment assignment indicators. The specifications include baseline covariates, such as gender, age, sibling priority, number of applications, and percentage of SIP applications. Also, add county fixed effects. Standard errors, reported in parentheses, are clustered by schools. Panel A restricts the sample to children with priority preference for SIP while panel B for secondary preferences for SIP. The estimates in panels A and B are obtained from separate regressions. The dependent variable in columns 1 and 2 is attendance. For columns 3 and 4 is an indicator variable of approbation. For columns 5 and 6, an indicator variable of failing while columns 7 and 8 for drop-out.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

16.4. *Multiple Hypothesis Testing*

This study tests multiple null hypotheses simultaneously for two reasons. First, there are multiple outcomes of interest, and I try to determine which outcomes the treatment affects. Second, I try to identify if the effect of being assigned to a school with SIP is heterogeneous across subgroups. If the multiple hypothesis tests are not taken into account, the probability of a false rejection may be much higher than desired (List et al., 2016). Including satisfaction and academic outcomes, nine different variables were tested. At a 5% significance level, the probability of one or more false rejections equals 36.98% $(1-(1-\alpha)^N)$ which is much greater than 5%.

Table D13 shows the adjusted p-values using Westfall and Young, Bonferroni-Holm, and Sidak-Holm methodologies. After correcting the p-value for these methods, using sample 1, the school integration programs only significantly affect take-up. While using sample 2, the null hypothesis is rejected for approval and failure. These results should be read with caution because they do not consider that standards errors are clustered at the school level as my preferred specification. Adding clusters to the standard errors could cause that being assigned to a school with SIP to have statistically significant effects on the probability of failure for students with priority preference for these schools.

Table D13: Multiple Hypothesis Testing

	Sample 1				Sample 2			
Outcome	P-value	P-Wyoung	P-Bonf	P-Sidak	P-value	P-Wyoung	P-Bonf	P-Sidak
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Take-up	0.000	0.003	0.000	0.000	0.023	0.142	0.161	0.151
Change School	0.568	0.938	1.000	0.955	0.746	0.969	1,000	0.978
Part. Next SAE	0.425	0.924	1.000	0.937	0.240	0.763	1,000	0.808
Distance to School	0.133	0.603	0.799	0.576	0.500	0.917	1,000	0.955
Change in Distance	0.789	0.938	1.000	0.955	0.463	0.917	1,000	0.955
Attendance	0.668	0.938	1.000	0.955	0.832	0.969	1,000	0.978
Approve	0.032	0.236	0.227	0.206	0.000	0.002	0.000	0.000
Fail	0.190	0.169	0.152	0.143	0.000	0.000	0.000	0.000
Drop-out	0.539	0.938	1.000	0.955	0.720	0.969	1,000	0.978

Notes: The table shows the p-values using different methods to control for multiple testing. The first column indicates the outcome which is considered. Columns 2 and 6 show the p-value without considering multiple testing; columns 3 and 7 used Westfall and Young method, and columns 4 and 8 used the Bonferroni-Holm method. Finally, columns 5 and 9 used the Sidak-Holm method. After considering multiple testing only take-up in sample 1 and approve and fail in sample 2 are statistically significant.