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ORIGINAL ARTICLE

Minimum age requirements and the role of the school choice set

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Abstract

Using several data sources from Chile, we study the impact of the size of the school choice set at the time of starting primary school. With that purpose, we exploit multiple cutoffs defining the minimum age at entry, which not only define when a student can start elementary school, but also the set of schools from which she/he can choose. Moreover, differences across municipalities in the composition of the schools according to these cutoffs allow us not only to account for municipality fixed factors (educational markets) but also for differences in the characteristics between schools choosing different deadlines. That is, we compare the difference in outcomes for children living in the same municipality around the different cutoffs with those for children in other municipalities that experience a different change in the available set of schools across cutoffs (double difference in RD). We show that a larger set of schools increases the probability of starting in a better school, measured by a non-high-stakes examination. Moreover, this quasi-experimental variation reveals an important reduction in the likelihood of dropping out and a reduction in the probability that a child would switch schools during her/his school life. Second, for a subsample of students who have completed high school, we observe that a larger school choice set at the

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start of primary school increases students' chances of taking the national examination required for higher education and the likelihood of being enrolled in college.

Keywords Latin America · Chile · School choice set · School achievements

JEL Classification A21 · I24 · I25 · I28

1 Introduction

Using individual administrative records from Chile, we study the impact of the size of the school choice set, measured by the number of available slots per student in the municipality at the time of enrollment in first grade of primary education, on individual educational outcomes. We do this by relating the literature on school choice with the one on School Starting Age (SSA).

The recent literature does not take a clear position in favor or against school choice (Epple et al. 2017). The ambiguity resides in the heterogeneity of the programs implemented and the individuals affected by these interventions. While small-scale programs provide credible experimental or quasi-experimental variation in school choice, (see, for example Angrist et al. (2002; 2006)) the expected benefits are positively related to the size of the intervention.[1] However, in large-scale interventions, the non-random sorting of students across schools makes it difficult to find a credible control group and speaks for the offsetting effects for some individuals in the population.[2] In this paper, we study the impact of more/fewer school choices in a context of a country (Chile) with a national voucher system for more than 30 years which in theory allows families making use of school choices to search, use and profit from better school quality.[3] Even though most of the evidence for the Chilean voucher system points to the effect on "cream-skimming" and mixed results on test scores (see, for example, Hsieh and Urquiola 2006), here we study if a change in the set of schools available to students has an impact on the educational outcomes of these students.[4]

[1] Families being granted the right to choose a school creates the incentives, *competitive pressure*, that schools would provide a better education.

[2] This negative effect is usually addressed as the problem of "cream skimming." This term has been used in the voucher literature to describe the fact that a voucher is more likely to be used by the parents of high-ability students or, more generally, students who are less costly to teach. These parents/students move from public, lower-performing schools to better, private ones, leaving the students who are more costly to teach to the public schools. In this paper the concept of cream skimming is used to describe the active or passive process by which some schools end up capturing high-achievement students, while other schools, as a result of this process, are left with students from lower levels of capacity/achievement.

[3] As a result of the nationwide school choice system introduced in 1981, more than 1000 private schools entered the market, and the private enrollment rate increased by 20 percentage points. Currently, around 93% of students in Chile attend schools being funded through this voucher.

[4] Hsieh and Urquiola (2006) show that the equilibrium effects of the introduction of a generalized voucher scheme in Chile led mainly to increased sorting, finding no evidence that choice improved average educational outcomes. However, Gallego (2013), using the interaction of the number of Catholic priests in 1950 and the institution of the voucher system in Chile in 1981 as a potentially exogenous determinant of the supply of voucher schools, finds positive effects on average achievement.

To study the impact of the size of the school choice set, we connect with the literature on School Starting Age (SSA).[5] Specifically, we rely on a "quasi-experimental" variation coming from the institutional setting that until recently governed minimum age requirements in Chile.[6] Families/children in Chile faced i) multiple birthday cutoffs defining the minimum age when a student can start the first grade of elementary school and ii) a different composition of schools according to these cutoffs across municipalities. These two features lead students born around several cutoffs, who "endogenously" decided to start early but whose birthdays differ by a few days, to face different school choice sets. Moreover, these changes in the school choice set around cutoffs vary also across municipalities. While minimum age requirements have been extensively used to estimate the impact of SSA, here we use these particular features to estimate the contribution of the size of the school choice set.[7]

Different from a setting with a unique cutoff, by comparing students on either side of the cutoffs across different cutoffs and municipalities, we can address the endogeneity of early start and also learn the contribution of the school choice set.[8] Specifically, at any cutoff-municipality combination, families face a potentially different subset of the school choice set, whose size changes differently at each cutoff across municipalities. That is, this source of variation not only allows us to control for municipality fixed factors (such as systematic differences in educational markets) but also to account for systematic differences in schools using different deadlines. In other words, our quasi-experimental variation is given by the fact that families with children born just after one of these cutoffs (except for the last one) are not forced to wait for the next academic year to send their children to school, but they face a subset of schools in the case in which they want them to start in the current year. That is, the treatment (slots per student) is different if the student's birthday is at each side of a given cutoff (seven in total), with its intensity varying also by municipality. Moreover, our population under study are all Chilean children born around the first day of the month, from January to July. Therefore, we are studying the impact of the school choice set on a wider population of students, rather than on a more homogeneous one in terms of family income or previous educational outcomes. We are also able to capture the potential heterogeneity across parents' education. In addition, our quasi-experimental variation

[5] SSA has been negatively associated with grade retention (Elder and Lubotsky 2008; Caceres-Delpiano and Giolito 2019), mental health problems (Dee and Sievertsen 2018), ADD/ADHD diagnoses (Elder and Lubotsky 2008), and the probability of receiving special education (Dhuey and Lipscomb 2010) for children in primary/middle school. Moreover at these early stages, SSA has been positively linked to test scores (McEwan and Shapiro 2006; Elder and Lubotsky 2008; Attar and Cohen-Zada 2018) the probability of following an academic-oriented track (Mühlenweg and Puhani 2010) and the likelihood of being enrolled in a private school (Caceres-Delpiano and Giolito 2021).

[6] In 2016, the Ministry of Education implemented a Centralized Admission System for Public and Voucher schools with an unified cutoff date.

[7] McEwan and Shapiro (2006) are the first to notice these multiple cutoffs when estimating the impact of age of entry in Chile. They find that an increase in one year in the age of enrollment is associated with a reduction in grade retention, a modest increase in GPA during the first years, and an increase in higher education participation. In a related paper, Caceres-Delpiano and Giolito (2019) show that those effects tend to wear off over time.

[8] Even though our paper is related to the literature on school choice, it is beyond its scope to evaluate a voucher system or to compare public and private schools. Here we study the impact of more/fewer choices within a voucher system, independently of the school type.

allows us to study families' behavior and its consequent effects independently of schools' strategic behavior. [9]

Our results reveal, first, that a larger set of available schools causes modest increases in standardized test scores. However, we find a sizable reduction in the likelihood of dropping out (8% in terms of sample mean) and in the probability that a student switches schools over her/his school life (10% in terms of sample mean). Second, for a subsample of students who have completed high school, we observe a positive effect on the probability of taking the national college admission test the year of high school graduation. Specifically, an increase in a third of slots per student is associated with an increase of approximately 2 percentage points in the probability of taking the college admission test (or 3% in terms of sample mean). Moreover, for families with less-educated parents, we also observe a positive effect on the probability of enrolling in college. In fact, for most of these outcomes, we show that the impact is greater among students whose parents have lower levels of education.

This paper provides evidence on the interaction of SSA with school choice. Therefore, we shed light on one institutional feature setting limits in the capability of families to profit from school choice in voucher schemes like the one in Chile. By providing evidence about the potential heterogeneity in the impact of SSA according to the school choice set, we learn about school choice contribution and also about the families who profit the most from it. Furthermore, this evidence about the positive contribution of school choice in a country with a national voucher scheme enables us to learn about the role of school choice in other dimensions other than the non-random sorting of students across schools, but on long-term educational achievements. Finally, this paper stresses the importance of school choice early in students' academic life. We find evidence about the impact of school choice at the time of first enrollment in elementary school rather than at high school or college.[10] That is, we contribute to the recent literature studying the impact of early interventions on children's outcomes.[11]

The paper is organized as follows. Section 2 briefly sketches Chile's educational system, presents the data set and defines the sample used in the analysis. In Sect. 3 we describe our empirical strategy and the selected outcomes in the analysis. In Sect. 4

[9] Recently, Navarro-Palau (2017) analyzes the impact of a policy that increased the value of the voucher for a target population in Chile (*"Subvención Escolar Preferencial"*, established in 2008). She finds a small reduction in the probability of being enrolled in a public school and better school characteristics for those students who were more likely to use private school, but without improvement in test scores. However, among students who were more likely to stay in public schools she observed an increase in test scores. Although the treatment from the perspective of the targeted families can be seen as an increase in the school choice set, the program required achievement goals from participating schools, which makes it difficult to isolate the findings from the voucher provision.

[10] One exception is Dobbie and Fryer (2011), finding positive results in math and language for poor minority elementary school students for a school in Harlem.

[11] Banerjee et al. (2007) find for India that more resources helping students "lagging behind" have an important gain in terms of test scores. Araujo et al. (2016) find for Ecuador a sizable impact on cognitive and non-cognitive outcomes for a sample of kindergarten children exposed to better teacher practices. Berlinski et al. (2008) and Berlinski et al. (2009) find that an expansion of pre-primary education in Uruguay and Argentina, respectively, has long lasting effect in terms of years of education, and the likelihood of staying in school for Uruguay, and for Argentina, a positive effect on third graders in terms of test scores and on student's self-control.

we discuss the validity of our source of variation. In Sect. 5 we present our results, and Sect. 6 concludes.

2 Institutional background and data

2.1 Chilean education system

Since a major educational reform in the early 1980s,[12] Chile's primary and secondary educational system has been characterized by its decentralization and by a significant participation of the private sector. By 2012, the population of students was approximately three and half millions, distributed throughout three types of schools: public or municipal (41% of total enrollment), non-fee-charging private (51% of total enrollment), and fee charging private schools (7% of total enrollment).[13] Though both municipal and private schools get state funding through a voucher scheme, the latter are usually called voucher schools.[14]Between 1994 and 2015[15], public and subsidized private schools were allowed to charge a co-payment ("*financiamiento compartido*"). By 2015, almost all public schools were free of charge and around 70% of students in voucher schools were paying a co-payment.

Primary education consists of eight years of education while secondary education depends on the academic track followed by a student. A "Scientific-Humanist" track consists of four years and it prepares students for a college education. A "Technical-Professional" track has a duration in some cases of five years with a vocational orientation aiming to help the transition into the workforce after secondary education. [16] Despite mixed evidence on the impact of a series of reforms introduced as of the early 1980s on the quality of education[17], Chile's primary and secondary education

[12] The management of primary and secondary education was transferred to municipalities, payment scales and civil servant protection for teachers were abolished, and a voucher scheme was established as the funding mechanism for municipal and non-fee-charging private schools. Both municipal and non-fee-charging private schools received equal rates tied strictly to attendance, and parents' choices were not restricted to residence. Although with the return to democracy some of the earlier reforms have been abolished or offset by new reforms (policies), the Chilean primary and secondary educational system is still considered one of the few examples in a developing country of a national voucher system which in the year 2009 covered approximately 93% of the primary and secondary enrollment. For more details, see Gauri and Vawda (2003).

[13] There is a fourth type of schools, "corporations," which are vocational schools administered by firms or enterprises with a fixed budget from the state. In 2012, they constituted less than 2% of the total enrollment. Throughout our analysis, we treat them as municipal schools.

[14] Public schools and subsidized private schools may charge tuition and fees, with parents' agreement. However, these complementary tuition and fees cannot exceed a limit established by law.

[15] Law 20.845 of 2015 determined that schools charging tuition should gradually decrease the co-payment to zero unless they become unsubsidized private.

[16] Until 2003, compulsory education consisted of eight years of primary education; however, a constitutional reform established free and compulsory secondary education for all Chilean inhabitants up to the age of eighteen.

[17] The bulk of research has focused on the impact of the voucher funding reform on educational achievements. For example, Hsieh and Urquiola (2006) find no evidence that school choice improved average educational outcomes as measured by test scores, repetition rates, and years of schooling. Moreover, they

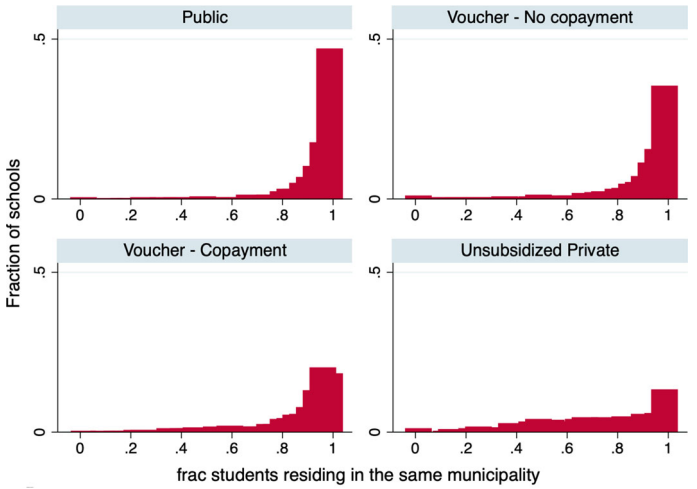


Fig. 1 Fraction of students attending school in their same municipality

systems are comparable in terms of coverage to any system we can observe in any developed country.

In Chile's public school system, families are not restricted to enrolling their children in the municipality of residence (while around 90% of the families do so) and the norm is to enroll a student the year she/he becomes six or seven years of age. Figure 1 shows the distribution of schools according to the fraction of students living in the same municipality. Schools are divided into four groups according their type and whether they receive or not co-payment: "Public" (do not require co-payment),[18] "Vouchers - Free of charge ," "Vouchers - Co-payment," *and "Unsubsidized private schools."* As shown in the figure, only unsubsidized primate schools appear to have a relevant fraction of students from other municipalities (around 30% in average).

2.2 Minimum age requirement rules

Minimum age requirement rules have been used extensively to address the impact of age of entry (Cascio and Lewis 2006; Black et al. 2011). These rules establish that children, to be enrolled in first grade at primary school in a given school, must have turned six before a given *date* during the academic year.[19] A common element in almost all of these studies is the feature that, in a given educational market, children face a unique cutoff *date*. That is, *all* children whose birthday takes place before this

Footnote 17 continued
find evidence that the voucher reform was associated with an increase in sorting. Other papers have studied the effect of the extension of school days on children outcomes (Berthelon and Kruger 2012), teacher incentives (Contreras and Rau 2012) and the role of information about the school's value added on school choice (Mizala and Urquiola 2013), among other reforms. For a review of these and other reforms since the early 1980s, see Contreras et al. (2005).

[18] In our sample, we find 12 out of 3850 public schools that appear to be requiring some co-payment.

[19] In Chile, the academic year goes from March to December.

cutoff date are entitled to start at *any* eligible school the year they turn six. Those whose birthday is after this specific date must wait until the next academic year to start in *any of the same* schools. In Chile's public school system, the norm is to enroll a student the year she/he becomes six or seven years of age.[20] Although the official enrollment cutoff was originally set on April 1, since 1992 until recently the Ministry of Education has provided schools with some degree of flexibility for setting other cutoffs between January 1 and July 1.[21],[22]

Different from a setting with a unique date, in this case, children whose birthday takes place after a given cutoff date are not forced to wait until the following year but are entitled to start school in the current year in any school with later cutoffs. Therefore, those whose birthday occurs after any specific cutoff (except for the last one) may either wait until the next academic year to start school (looking at the complete set of schools) or restrict the search to those schools with a deadline later in the year.

Figure 2 shows the distribution of schools around cutoffs during the period under analysis. *As shown in the figure, most schools with a cutoff date away from April* 1, had sorted around thresholds later in the year. Notice also that while the vast majority (around 80%) of fee-charging voucher schools have their cutoff date on July 1, public and free-of-charge voucher schools (and also unsubsidized private schools, around 7% of total enrollment) are distributed across several cutoffs dates.

2.3 Data

We use three different sources of data in this paper. The first one comes from students' records from the Ministry of Education, available from 2002 to 2014.[23] For each student and year (from 1st grade elementary to 4th year of high school), we know, together with their exact date of birth, which grade and school a student attended, her/his GPA, and whether or not a student passed, failed or left the class or school. These records allow us to track each student over her/his whole school life. We analyze the cohorts born between 1996 and 2001, keeping those students for whom we have information since the beginning of elementary education.

[20] Despite the general agreement about the negative effect of an early school start on students' achievements in elementary education, there is also the extensive literature documenting the cost in terms of female labor participation. Gelbach (2002) shows for the USA that mothers with a child five years old age-eligible to attend public school increase their attachment in the labor market. Moreover, though the positive evidence on several dimensions at elementary (see footnote 5.), the evidence on the impact of SSA on long-run outcomes is less conclusive, on the other hand. For a revision of this literature, see Caceres-Delpiano and Giolito (2021).

[21] In 2016, the Ministry of Education implemented a Centralized Admission System for Public and Voucher schools applied gradually by regions and in place at the national level in 2020. Therefore now Public and Voucher schools have a unified cutoff date.

[22] McEwan and Shapiro (2006), to estimate the impact of age of entry in Chile, use the four most used cutoffs in practice: April 1, May 1, June 1, and July 1.

[23] Data is publicly available from the site of the Ministry of Education: http://centroestudios.mineduc.cl/.

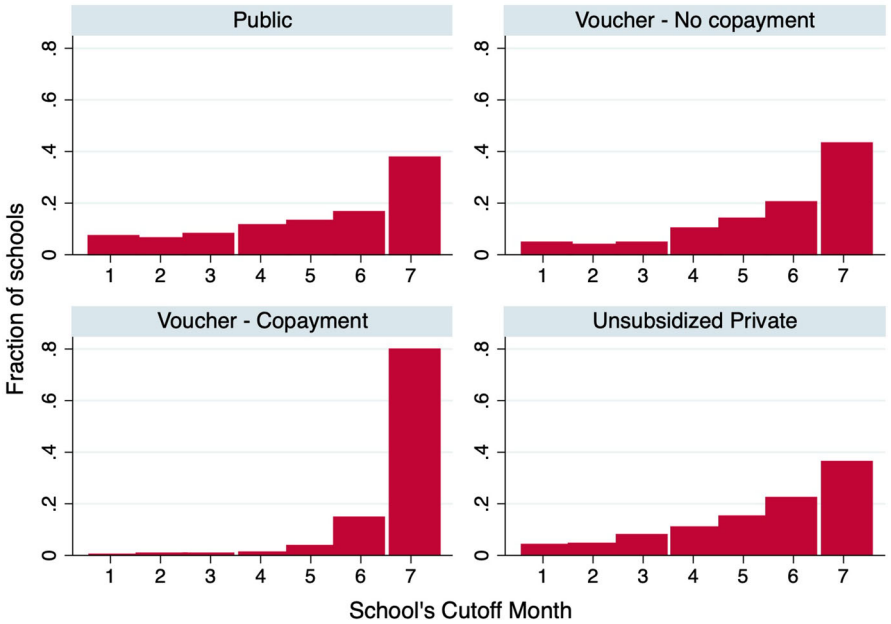


Fig. 2 School distribution around cutoffs by type of school

The second source of data comes from the national standardized test (SIMCE)[24], usually given in 4th, 8th, and 10th grades (the 2nd year of high school).[25] The SIMCE database also has a supplementary parents' survey, which allows us to know the parents' education.

Finally, the third source of data comes from the Centralized College Admission System, from 2014 to 2017 (individual records of students born between 1996 and 1999). [26]These files contain the national standardized college admission test (PSU, *Prueba de Selección Universitaria*) and the admission records of the 33 best Chilean universities.[27]

[24] *Sistema de Medición de la Calidad de la Educación*. Data is available from Agencia de Calidad de la Educación:http://www.agenciaeducacion.cl.

[25] The 4th-grade SIMCE exam has been given every year since 2005. The 8th-grade and 10th-grade exams have been administered every other year starting in 2007 and 2006, respectively.

[26] Information provided by the *Departamento de Evaluación, Medición y Registro* (DEMRE) from Universidad de Chile. Data is available under request from http://demre.cl

[27] Among colleges, we can distinguish those created before the year 1981 (often called "traditional"), which belong to the *Consejo de Rectores de las Universidades Chilenas* (CRUCh), from those created later on (usually called "private"). The 25 CRUCh universities participate in a centralized admission system coordinated by the *Universidad de Chile*. In 2017 two recently created public universities entered the system. From 2012 onward, eight "private" universities participate in the system, with one more addition in 2017.

3 Empirical specification

As a consequence of the non-random nature of school choice and researcher's limited information, the problem of evaluation of the impact of school choice is non-trivial. To circumvent this problem, we make use of the minimum age requirement rule and specifically, Chile's institutional feature of multiple "*soft*" cutoffs, as a source of a quasi-experimental variation. To identify the impact, we use differences in the size of the school choice set faced by families at the time of enrollment in primary school around those soft cutoffs and across municipalities. Specifically, the source of variation used in our analysis comes from the combination of three elements: i) the minimum age of entry rules; ii) the fact that there are in practice seven deadlines that schools can choose when defining the minimum age at which a child can start school; and finally iii) the difference in the composition of schools according to the deadlines across municipalities.

Figure 3 shows the ratio of *potentially* available vacancies around the seven cutoffs when a child is first eligible to start primary education (classifying schools according to the age of the youngest student) over the population of six- and seven-year olds (from the Population Census). Notice first that children whose sixth birthday is before January 1 or after June 30 face the complete school set. While students who become six years old around December 31 can start elementary school the following March (the academic year is March–December), those born on or after July 1 have to wait until the next year (the closest March to their seventh birthday).[28] Nevertheless, children born between January 1 and June 30 face, in the case in which they start school the year they become six, a reduction in the universe of schools potentially available. Notice also in Fig. 3 that the largest decline in the set of schools occurs around the fourth or later cutoffs since most schools use the June 1 and the July 1 cutoffs.

Figure 4 presents the fraction of students who start elementary education the academic year closest to their sixth birthday for every birthday ("early entry"). First, we observe that those children born after July 1 are "forced" to postpone enrollment until the following year. Second, except for February and March, we observe a distinguishable jump in the fraction of students entering early for the rest of the cutoffs (January and April–July) with a drop in a fraction starting early at the right side of each of these cutoffs. Notice also the larger jump for those children born around July 1, which is also explained by the large fraction of schools using this last cutoff. Together with the assumption that parents cannot fully manipulate the date of birth, this discontinuity in the age of entry has been used in the literature as quasi-experimental variation in the age of entry at the core of a "fuzzy"[29] Regression Discontinuity (RD) strategy

[28] Note that children whose birthday is before the first cutoff (December) were born in a previous calendar year than the rest of the children for a given academic year. Therefore, the expression "starting early" in this context should be interpreted as "starting in the closest March to their sixth birthday" even though those born in December will be first-time eligible to start school the year corresponding to their seventh birthday. For children born after the first cutoff (January–July), starting early means doing so the year they become six years old.

[29] In a fuzzy RD design, the probability of treatment (starting primary school at the age of six) changes in magnitude lower than one. On the other hand, in the case where the treatment is a deterministic function of the day of birth, the probability of treatment would change from one to zero at the cutoff day. For more details, see Lee and Lemieux (2010).

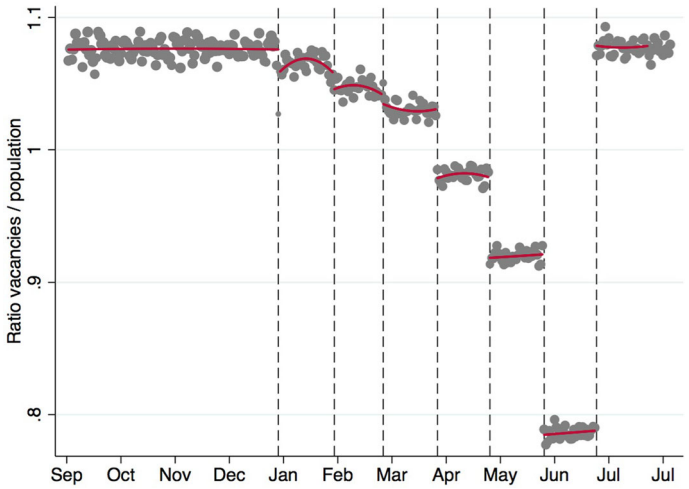


Fig. 3 Vacancies over population in the municipality by birthday

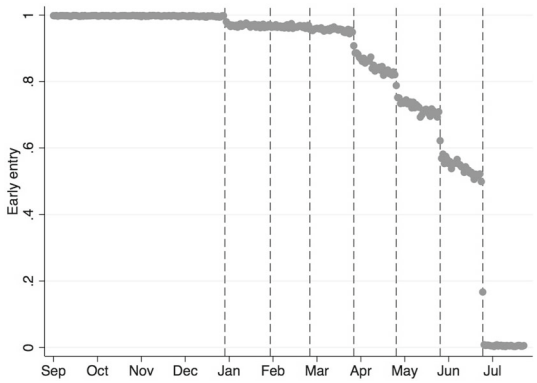


Fig. 4 Fraction of students who start school the year becoming six and average age of entry

in recent papers (McEwan and Shapiro 2006; Cascio and Lewis 2006). However, as we have just shown, around each of these cutoffs, we observe a combined treatment: school set and age of entry.

To sort out the effect of age of entry from the one of facing the larger school set, we not only use the fact that some children are not eligible to start in some schools but also that the distribution of schools/cutoff differs across municipalities and that we observe the school set available for a student with a given birthday. The idea is simple: being born after a given cutoff implies a tradeoff between starting elementary school the current year, choosing a school from the reduced set still available, or starting the following year facing the full set of schools. Moreover, this variation in school choice set not only takes place across cutoffs within a given municipality but also differs between municipalities. Since the composition of schools around a cutoff is not the same across municipalities (or across cutoffs within a municipality), children born around the same date but residing in different municipalities (or in

later cutoffs for students residing in the same municipality) face a different reduction in the relevant school set available, and therefore a different cost in the case of not delaying entry.[30] That is, in case school choice would matter, the cost of an early start should be heterogeneous across different cutoffs in a given municipality and across cutoffs-municipalities.

Using administrative data for the total population of students, we can construct the full set of schools available in the municipality at each birthday for students in the case in which they start early. That is, the identification comes from the fact that the treated population (children starting school at the age of 6 or 7) were exposed to different *"doses"* of a second treatment (reduction in the school set) *if they do not delay school entry*. Therefore, the discontinuity in age eligibility, at least for some schools, generates a change in the school choice set that varies across municipalities and cutoffs. This change in the school choice set provides us with a quasi-experimental variation that addresses the endogeneity of age of entry and also the impact of school choice set in the context of a "fuzzy" regression discontinuity (RD) strategy.

Consider a scenario where students are indexed by i, birthdays by b. The specification we use to estimate the impact of the school choice can be expressed as follows,

$$y_{icnt} = \eta_{cs} + \rho_t + \varsigma_n + X_i^{'} + \alpha_1 EarlyEntry_i + \alpha_2 EarlyEntry_i * R_{cb} + \alpha_3 R_{cb} + g(b_i) + \epsilon_{icnt} \quad (1)$$

with y_{icnt} representing one of the outcomes at year t for student i, born the day b of year s, in the neighborhood around of n^{th} cutoff, and living in municipality c.

The variable $EarlyEntry_i$ is a dummy variable taking a value of one in the case in which the child started primary school the academic year closest to her/his sixth birthday and zero, otherwise. The variable R_{cb}, reflects a student choice set in the case of starting their elementary education early, corresponding to the ratio of available vacancies for a student with a birthday b in municipality c, over the population of 6- and 7- year olds in the municipality, the first year the child is eligible to start primary school.[31] Moreover, η_{cs}, ρ_t, ς_n and X_i represent municipality-year of birth, calendar year, cutoff neighborhood fixed effects and a vector of individual covariates, respectively.[32,33] Finally, $g(b_i)$ is a flexible polynomial specification in the day of birth for a student, taking the form:

$$g\left(b_i\right) = \sum_n \left(\sum_{k=1}^{K} \left[\beta_{nk} \left(b_i - C^n\right)^k + \rho_{nk} \left(b_i - C^n\right)^k \times 1\{b_i - C^n > 0\} \right] \right),$$

[30] As stated above, even though in theory families are not obligated to send their children to a school in their municipality of residence, around 90% of families do so.

[31] The population variable comes from the Chilean National Institute of Statistics (INE).

[32] Covariates include fixed effects for parents' education and students' gender. We use as parents' education the highest observed education between the father and the mother reported in SIMCE parents' survey. We define an additional category for the case when the educational level is missing simultaneously for the mother and the father. We also include six dummy variables indicating the day of the week on which a student was born, and a dummy to define whether or not a student was born on a national holiday.

[33] We cluster the error term at the municipality level.

Table 1 Descriptive Statistics

		Population	Within bandwidth
Student performance			
	Dropout	0.152	0.151
	Move schl. during elementary	0.142	0.143
	Took PSU the yr when completed HS	0.618	0.616
	Ever enrolled	0.198	0.198
School characteristics			
	First schl. private	0.079	0.08
	First schl. voucher	0.521	0.522
	Class size	27.895	27.899
		[9.127]	[9.122]
	Parents Educ at FG	11.037	11.036
		[2.164]	[2.166]
	Schl SIMCE	251.867	251.819
		[27.31]	[27.293]
Other student characteristics			
	Early start	0.789	
	Age at entry	6.138	
		[0.35]	
	Male	0.51	
	Number of sits	2593.436	
		[2263.688]	
	Population in first grade	2701.445	
		[2590.952]	
	Ratio:sits/pop	0.995	
		[0.261]	

Standard deviations between brackets. The standard deviations for proportion is not presented

where k is the degree of the polynomial, $1\{*\}$ is an indicator operator and C_n one of the n cutoffs values. That is, $1\{b_i - C^n > 0\}$ defines whether or not an individual has a birthday after a given cutoff n. Notice that the source of variation allows us to control for municipality fixed effects (systematic differences in educational markets) and cutoff neighborhood fixed effects (systematic differences between schools using different deadlines).[34]

Hahn et al. (2001) show that the estimation of causal effects in this regression discontinuity framework is numerically equivalent to an instrumental variable (IV) approach within a small interval around the discontinuity. By focusing on observations around these seven discontinuities, we concentrate on those observations where both treatments (school set and age of entry) can be considered as randomly assigned. This randomization of the treatment ensures that all other factors (observed and unobserved)

[34] Using Akaike's information criterion (AIC), we get a degree for $g(.)$ that is either one or two depending on the outcome. We use a local linear specification in our preferred specification.

Table 2 Optimal bandwidth by selected outcomes and cutoffs

	Method	January	February	March	April	May	June	July	Average
Class size	MSE	5.5	19.8	13.1	11.7	10.1	9.6	10.5	11.5
	CER	3.9	14.3	9.5	8.4	7.3	7.0	7.6	8.3
First schl private	MSE	2.3	16.7	14.2	12.5	15.9	8.0	7.4	11.0
	CER	1.7	12.1	10.2	9.0	11.5	5.8	5.3	7.9
First schl voucher	MSE	2.8	8.2	15.3	12.2	14.4	12.9	5.9	10.2
	CER	2.0	5.9	11.0	8.8	10.4	9.3	4.3	7.4
Schl SIMCE	MSE	3.5	1.8	9.8	13.8	9.1	5.3	7.2	7.2
	CER	2.5	1.3	7.1	10.0	6.5	3.8	5.2	5.2
Parents Educ at FG	MSE	2.8	4.8	7.8	5.7	8.4	10.0	4.7	6.3
	CER	2.0	3.5	5.6	4.1	6.0	7.2	3.4	4.5
Simce Math	MSE	8.6	6.2	6.5	7.2	9.3	6.3	5.1	7.0
	CER	6.2	4.5	4.7	5.2	6.7	4.5	3.7	5.1
Simce Language	MSE	6.6	7.3	5.3	5.7	6.6	5.9	5.9	6.2
	CER	4.8	5.3	3.9	4.1	4.7	4.3	4.3	4.5
Move schl during elementary	MSE	9.7	16.9	7.0	13.5	8.2	9.1	15.3	11.4
	CER	7.0	12.2	5.0	9.7	5.9	6.5	11.1	8.2
Dropout	MSE	4.3	11.1	7.6	6.0	6.3	7.8	6.3	7.1
	CER	3.1	8.0	5.5	4.3	4.6	5.6	4.6	5.1
Took PSU	MSE	7.6	8.1	4.1	7.9	7.2	9.8	5.6	7.2
	CER	5.5	5.9	3.0	5.7	5.2	7.1	4.0	5.2
Enrolled college	MSE	9.2	4.0	2.7	7.5	5.2	6.7	10.7	6.6
	CER	6.6	2.9	1.9	5.4	3.8	4.8	7.7	4.7
Male	MSE	2.4	11.4	11.8	9	9.2	10.6	7.4	8.8
	CER	1.7	8.2	8.5	6.5	6.6	7.6	5.3	6.3
Own Parents Educ.	MSE	6.9	19.1	6.6	19.2	19.1	6.8	14.5	13.2
	CER	5	13.8	4.7	13.8	13.8	4.9	10.5	9.5
Dist. Calendar	MSE	4.8	20.6	16.7	11.7	7.7	13.8	13.8	12.7
	CER	3.5	14.9	12	8.5	5.6	10	10	9.2
Enrollment/Births	MSE	7.9	11.6	13.7	10.7	12.6	8.8	13.2	11.2
	CER	5.7	8.4	9.9	7.7	9.1	6.3	9.6	8.1

MSE: Mean Squared Error (MSE) optimal bandwidth
CRE: optimal bandwidth that minimizes the asymptotic coverage error rate of the robust bias corrected confidence interval
Both bandwidths were chosen following Calonico (2017) and implemented with the command rdbwselect in STATA, a local linear polynomial and triangular kernel

determining a given outcome must be balanced at each side of these discontinuities. Specifically, to select an optimal bandwidth, we applied the method developed by Calonico et al. (2014) at each of the cutoffs, obtaining values that on average go from 4 to 12 days around the discontinuities (see Table 2).[35]

[35] Following Calonico et al. (2017), we use a specific bandwidth for each of the selected outcomes using two data-driven criteria that were implemented with the command `rdbwselect` in STATA, a local linear

In equation (1), the cost (or benefit) of age at entry is captured jointly by the parameters α_1 and α_2. Specifically, α_2 is the parameter of interest in our analysis and represents the change or difference in the effect of early entry associated with an additional slot per student in the municipality. A positive and significant estimate for α_2 not only tells us that the impact of early entry is not independent of the school choice set faced by families but also that it is decreasing on the size of the school choice set. Moreover, in equation (1) we allow that there is a direct effect of the vacancies per student in the municipality, R_{cb}.[36] Note that after including $EarlyEntry_i$, η_{cs} and ς_n, the parameter α_2 has a similar interpretation of a difference-in-difference estimator (DD). That is, we learn the impact of school choice by comparing the additional cost faced by early entrants (on top of the one captured by α_1) when we compare the difference in outcomes of students around several cutoffs within the municipality, to similar differences in outcomes in other municipalities that experience a different change in their school choice set between these cutoffs. However, different from the usual DD, the treated/control group status (early entrant status) is endogenous, and therefore, we instrumentalize it with minimum age requirements.

As stated above, the variables indicating whether or not a child starts school early ($EarlyEntry_i$) and its interaction with the vacancies per student ($EarlyEntry_i * R_{cb}$) are two non-random variables. To address this endogeneity, we make use of the seven discontinuities determining age requirements (the same across municipalities) and the differences in the available set of schools (across cutoffs and municipalities). For each of the two endogenous variables, $endog_i$, in equation (1) we estimate the following first-stage regression to formally inspect for a relevant variation:

$$\begin{aligned} endog_i = {} & \eta_{cs} + \rho_t + \varsigma_n + X_i^{'}\psi + \theta R_{cb} + \delta_1 \times 1\{\widetilde{b}_i > 0\} + \gamma_1 R_{cb} \\ & \times 1\{\widetilde{b}_i > 0\} + \delta_2 \times 1\{b_i - C^7 > 0\} \\ & + \gamma_2 R_{cb} \times 1\{b_i - C^7 > 0\} + g(b_i) + v_i \end{aligned} \tag{2}$$

where $\widetilde{b}_i = b_i - C^n$, is a centered running variable around the first six cutoffs ($n < 7$). The parameters of interest in equation (2) are δ_s and γ_s, which capture the discontinuity in the endogenous variable around the cutoffs. Specifically, $(\delta_1 + \gamma_1 R_{cb})$ represents the discontinuity in the endogenous variable in the first six cutoffs and $(\delta_1 + \delta_2 + (\gamma_1 + \gamma_2) * R_{cb})$ for the last one. Here, γ_1 is the contribution associated with the

Footnote 35 continued
polynomial and triangular kernel. Choosing a smaller bandwidth reduces the bias of the local polynomial approximation, but simultaneously increases the variance of the estimated coefficients because fewer observations will be used for estimation. The first method tries to balance some form of bias-variance trade-off by minimizing the mean squared error (MSE) of the local polynomial RD point estimator (MSE-optimal bandwidth). While this bandwidth is optimal for point estimates, it is not for inference. The MSE-optimal bandwidth presents a challenge because this bandwidth choice is not "small" enough to remove the leading bias term to conduct statistical inference. There are several ways to address this difficulty. We follow the under-smoothing approach; that is, using more observations for point estimation than for inference. Specifically, the second bandwidth minimizes an approximation to the coverage error of the confidence intervals, that is, the discrepancy between the empirical coverage of the confidence interval and the theoretical level.

[36] Students who face a higher number of vacancies per student are those with a residence in municipalities with a larger relative supply of establishments and with a higher concentration of establishments in later cutoffs.

school choice set in the first six cutoffs ($(\gamma_1 + \gamma_2)$, in the last cutoff). We allow a different effect between the first and last cutoffs because birthdays just after the first six deadlines do not stop students from looking for a school that enables them to start early, so its effect is expected to differ from the one in the last cutoff.[37] As previous studies regarding the impact of SSA have shown,[38] we expect $\delta_1(\delta_1 + \delta_2) < 0$ for the variable $EarlyEntry_i$. That is, children whose birthday is just over one of the deadlines are less likely to start school the year they become six. However, we also expect that the higher the number of slots available for a given population (that is, the larger R_{cb}) is, the greater the chance that a student will start school early

By adding to the specification a fully flexible polynomial specification on the day of birth, $g(b_i)$,[39] we deal with the possibility that students born at different dates differ systematically.[40] Nevertheless, even if the mother's characteristics were correlated simultaneously with the birthday and the child's educational outcomes, it does not invalidate our approach. What is required is that the effect of these observed and unobserved factors do not change discontinuously in the mentioned cutoffs.[41,42]

Outcomes

We construct two groups of outcomes from several data sources. By using data from public administrative records on educational achievement provided by the Ministry of Education of Chile for the period 2002–2014, the first group of outcomes attempts

[37] In a previous version, we allowed that the impact of school eligibility differs across all cutoffs. However, we did not find a consistent difference among the first six cutoffs but a difference between the first six and July. This seems natural given the fact that birthdays just after the first deadlines do not stop students from looking for a school that enables them to start elementary school the year that they turn six. We explored if the impact of school set differs across cutoffs but we did not find a significant difference in the variable $EarlyEntry_i$. To avoid weak instruments, we estimate δ_n and γ_n pooling cutoffs 1 to 6 (January–June), allowing for a separate effect for the July 1 cutoff. Moreover, in online appendix, we explore the heterogeneity of the impact across cutoffs. This analysis reveals that the main results of the paper come from the last four cutoffs where we observe the larger variation induced by the minimum age requirements. We cannot rule out, however, heterogeneity across cutoffs but the lack of precision in the first ones does not allow us to detect it.

[38] See, for example, Elder and Lubotsky (2008), Dee and Sievertsen (2018), or Dhuey and Lipscomb (2010) For recent revision of the literature on SSA, see Caceres-Delpiano and Giolito (2021)

[39] Given a small interval around the discontinuity and a parametrization of $g(.)$, the estimated function can be seen as a nonparametric approximation of the true relationship between a given outcome and the variable day of birth, that is, we are less concerned about the estimated impacts being driven by an incorrect specification of $g(.)$.

[40] Buckles and Hungerman (2013) show, for the USA, that season of birth is correlated with mother's characteristics. Specifically, they show that children born in winter are more likely to be born from a mother with lower levels of education, the mother is more likely to be a teen mother, and she is more likely to be African-American.

[41] In a context of "intrinsic heterogeneity" (Heckman et al. 2006), the estimated parameters can be interpreted as weighted "Local Average Treatment Effects" (LATE) across *all* individuals (Lee and Lemieux 2010). That is, this fuzzy RD design does not estimate the impact just for those individuals around the discontinuity but overall *compliers*. How close this weighted LATE is to the traditional LATE depends on how flat these weights are (Lee and Lemieux 2010).

[42] Since the last cut-off (July 1^{st}) is associated with practically perfect compliance in the age of entry, the use of only the last discontinuity is closer to sharp RD where the interpretation of the estimated parameter is a weighted "Average Treatment Effects."

to characterize the school where a student is enrolled in the first grade of elementary school. The first variable takes a value of one in the case of a student enrolled in a subsidized private (voucher) school and zero otherwise. The following two variables are proxies of school inputs: class size in first grade and the average education of the other parents in the school. The last outcome in this group is the average school SIMCE, measured the year prior to effective enrollment.

The second group of outcomes come from the administrative records and the SIMCE records and attempt to characterize the impact of school choice on students' performance. By using students' records we are able to build six variables that describe the students' progression over their school life. The first two outcomes in this group correspond to the standardized math and language test scores from the SIMCE examination. The third variable, *Dropout*, is a dummy variable taking a value of one in the case in which a student in the last year is observed (except for 2014) has not completed secondary education or in the year 2014 has not completed the academic year in any school and zero, otherwise. The following variable is a dummy variable, *Move school*, for students in elementary school that takes a value of one in the case where a child is observed in two (or more) different schools for two consecutive years. The idea of this variable is to capture simultaneously two factors. First, the impact of a poor match school-student when choosing the first school and families later on searching for better schools. That is, in case fewer school options imply attending a school that might not be the first option for the family, parents might be prone to look for another school later on. Second, because a worse match might adversely impact students' achievements, students might be expelled from a given school. We define this variable only for students in elementary school since more than 50% of the students change school when they move from elementary to high school.[43] The two final variables correspond to outcomes related to college application. These variables are constructed for the cohort of students born between the years 1996 and 1999 since they are the ones who should have completed their secondary education by 2017 (both those who delayed school entry and those who did not). *Took the PSU* indicates whether or not the student takes the national college examination (PSU)[44] the year of high school completion and zero otherwise. Finally, *Ever Enrolled College,* is a dummy variable indicating whether or not a student has been enrolled in one of the colleges in the system over the period under analysis.[45]

Descriptive statistics are presented in Table 1. The first column reports the statistics for the complete population of students, while the second column shows the statistics for the students in the sample under analysis. Noticeable is the similarity in the sample mean between the students in our sample and the complete population. Specifically, each cohort of students is composed of approximately 250,000 students; half of them are boys; 91 percent of the children starting primary school do so in a school in their own municipality. The average age of entry is 6.14 years of age, with approximately 78% of these students not delaying school entry. Figure 5 shows the distribution of

[43] The reason for this forced school switching comes from the fact that a large fraction of public schools with elementary education do not offer high school education.

[44] Prueba de Selección Universitaria.

[45] See footnote 27 on page 7.

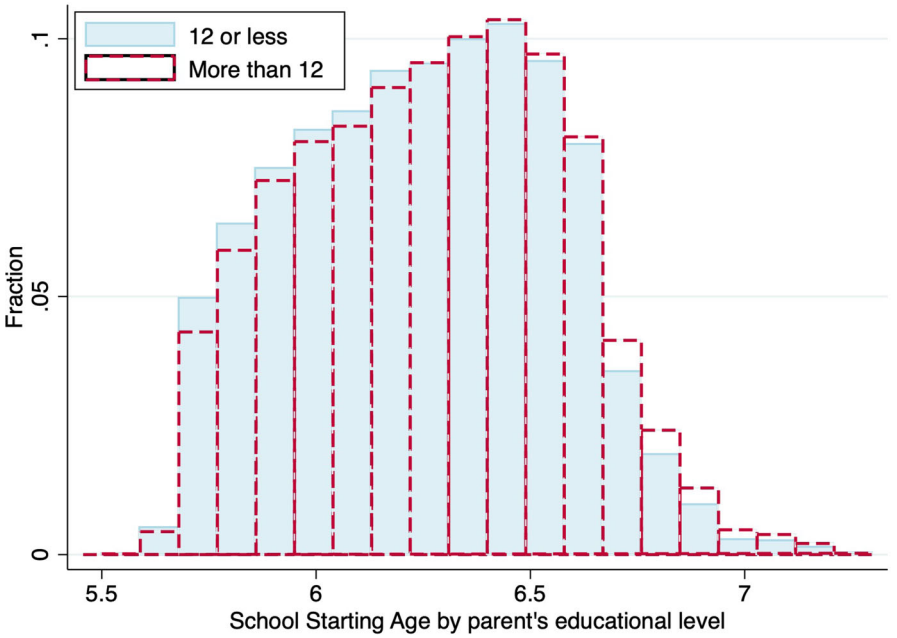


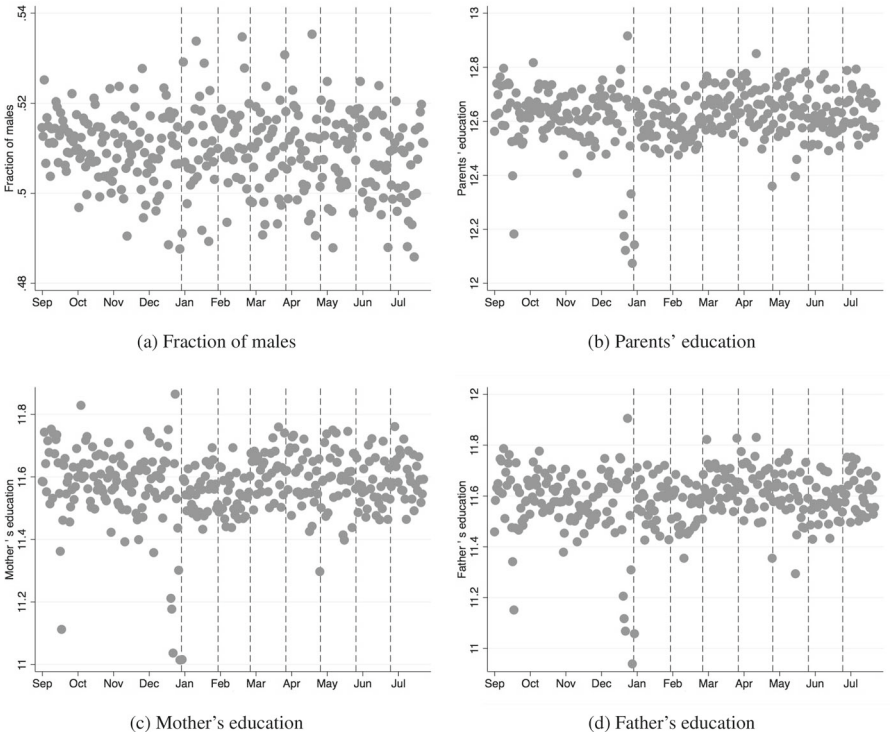
Fig. 5 Distribution of SSA by parent's educational label

SSA by parents' education label. Notice that the dashed histogram (for children with parents with more than 12 years of education) shows a higher fraction of students starting school closer to their seventh birthday. Conversely, the blue solid histogram (for children with parents with 12 years of education or less) shows a larger fraction of children starting before 6 and a half years of age.

In terms of the selected outcomes, approximately 24% of the students have failed a grade at least once. Moreover, around 60% percent of the children in secondary education follow an academic track with approximately 14% changing school during their elementary education. Regarding the type of school where the children are enrolled for the first time, approximately 8% and 50% of the students in a given year are enrolled in a private or voucher school, respectively. The average class size is approximately 28 students and the average education of the parents of the classmates is approximately 11 years.

The graphic analysis presenting the relationship between each of the outcomes and the student's birthday is reported in online appendix.[46]

[46] For each outcome, we fit a flexible second-degree polynomial at both sides of the seven discontinuities. Two elements are noticeable from the figures: first the existence of a series of discontinuities around the cutoff, and second, a heterogeneous jump around them. In fact, for all the outcomes, the largest and most evident jump is observed around July 1. These features are not only consistent with a positive effect associated with a larger school set, but also to an overlap with a second treatment, that is delaying school entry.



(a) Fraction of males

(b) Parents' education

(c) Mother's education

(d) Father's education

Fig. 6 Balancing covariates

4 Validity of the source of variation

4.1 Continuity in predetermined variables

Our analysis builds on the fact that changes in school eligibility and changes in the available set of schools around the cutoffs can be seen as good as a randomized assignment for those students with a birthday near these dates. Then, as in any random assignment, those pre-determined characteristics at the time of the randomization should be similar between treated (students with a birthday just after one of the seven cutoffs) and the control group (students with a birthday just before one of the cutoffs). Evidence of a systematic difference in these pre-determined characteristics around these dates would compromise the underlying assumption that individuals cannot precisely manipulate the running variable (Lee and Lemieux 2010).

Figure 6 inspects graphically the existence of a potential discontinuity among four baseline characteristics available in the data set: gender (fraction of male), mother's and father's education, and the highest education of the students' parents. The graphical representation does not show any sizable discontinuity or outliers for these selected variables with the exception of the days around January 1 , May 1 and September

18. These three dates correspond to three major holidays[47] and can be explained by a selected drop in the number of births during holidays. That is why in our preferred specification we control for whether or not a particular student was born on a legal holiday. We formally test for discontinuities in these baseline characteristics using different polynomial specifications and seven bandwidths (3, 5, 7, 10, and 15 days, plus two optimal data-driven bandwidths[48]). The p-values are reported in the first two columns of Table 3.[49] For only two out of 36 specifications the null is rejected at a significance level of 5%. Nevertheless, to avoid the results being affected by the January cutoff, our analysis excludes students around this first threshold. However, as a robustness check, we present the results including all the cutoffs in Appendix[50].

4.2 Manipulation of the running variable

In addition to the continuity of the predetermined variables, the randomization of treatment in the neighborhood of the discontinuities rests on the assumption that families cannot precisely select their children's day of birth. That is, the validity of an RD design can be compromised in cases in which individuals can *precisely* manipulate the running variable, day of birth, (Lee and Lemieux 2010). Since minimum-age entry rules are of public knowledge, we could expect that benefits/costs associated with a delay in the age of entry might induce some families to choose the season of birth.[51] Moreover, it is worth noting that Chile is (with Turkey and Mexico), one of the countries with the highest rate of c-sections in the world.[52] These two facts suggest some power to select the running variable.

Nevertheless, the fact that families can sort themselves over the calendar year does not invalidate this quasi-experimental design. The critical identification assumption, however, is that individuals lack the power to *precisely* sort themselves around these discontinuities. Under precise manipulation, we would find observations stacking up around the discontinuities or in other words, we would observe a discontinuous distribution for the day of birth (the running variable).

Panel (a) of Figure 7 presents the raw histogram for the day of birth for all individuals born from December 15 to July 6. Despite the high volatility, the figure hides a quite uniform distribution of birth during the calendar year but with a high dispersion across weekdays and years of birth. Specifically, we observe an average of 650 births within a range of 500 to 800 births per day. However, when we control for the day of the

[47] Christmas–New Year's, Independence Day, and Labor Day, respectively

[48] See footnote 35

[49] Specifically, we use the regression model $covariate_i = \eta_{wh} + \phi_b + \gamma_n * 1\{b_i - C^n > 0\} + g(b_i) + v_i$, for each of the predetermined variables. Here $1\{b_i - C^n > 0\}$ is an indicator variable taking a value of one for students whose birthday (b_i) is over the n cutoff (C^n), and zero otherwise. As in equation (1) , ϕ_b represents year of birth fixed effects, and η_{wh}, week day–holiday fixed effects. The null hypothesis for which the p-values are reported, $\gamma_n = 0$, corresponds to the scenario where there are not differences in the predetermined variables between children over and those below any of the cutoffs.

[50] The results reveal qualitative similar findings to the ones we present in the following sections.

[51] Buckles and Hungerman (2013) show that the season of birth is correlated with some mother's characteristics in the USA.

[52] For an analysis of the causes of the high rate of c-section in Chile see de Elejalde and Giolito (2021).

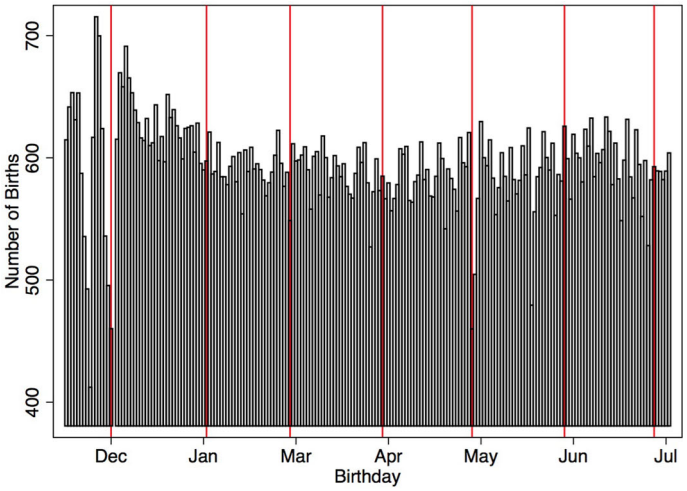
Table 3 Differences in predetermined variables between children born before and after the seven cutoffs. p-values reported

Degree	Bandwidth	Male	Parent's Education	Students distribution over calendar	Ratio Municipality Enrollment to Births
P3	15	0.768	0.187	0.775	0.637
	10	0.764	0.079	0.292	0.863
	7	0.157	0.031	0.803	0.864
	5	0.106	0.286	0.042	0.915
Optimal	MSE	0.42	0.59	0.160	0.862
	CER	0.056	0.086	0.120	0.496
P2	15	0.994	0.438	0.222	0.833
	10	0.825	0.638	0.655	0.714
	7	0.492	0.108	0.229	0.777
	5	0.131	0.225	0.589	0.579
	3	0.018	0.314	0.639	0.599
Optimal	MSE	0.970	0.140	0.049	0.833
	CER	0.670	0.660	0.112	0.934
P1	15	0.953	0.164	0.189	0.178
	10	0.984	0.169	0.244	0.412
	7	0.759	0.577	0.628	0.285
	5	0.974	0.062	0.439	0.638
	3	0.480	0.104	0.218	0.811
Optimal	MSE	0.780	0.510	0.086	0.689
	CER	0.970	0.200	0.067	0.640

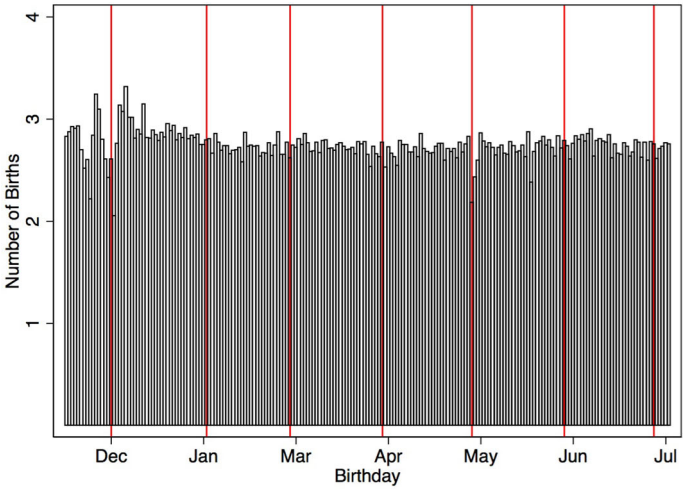
For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_{wh} + \phi_b + \gamma^s * 1\{x_i^s > 0\} + g(x_i^s) + v_{it}\ 1\{x_i^s = BD_i - C^s > 0\}$ is an indicator variable taken a value of one for students whose birthday (BD_i) is over the cutoff (C^s), and zero otherwise.
α_s is an specific constant for individuals around the s cutoff
η_{wh} and ϕ_b represent week day/holiday, and year of birth fixed effects, respectively. The null hypothesis for which the p-values is reported is $H0 : \gamma^s = 0$, that is, there are not differences in the predetermined variables between children over and below any of the cutoffs
The degree of $g(x_i^s)$ is reported in the first column, while the bandwidth used is in the second one
For each outcome, two data optimal driven bandwidths are used to inspect potential discontinuities. The values for these bandwidths are reported in Table 3

week, holidays, and year of birth fixed effects, as Panel (b) of Fig. 7 reveals, there is a uniform distribution of births across the calendar year and no discontinuities in the distribution of the running variable over the cutoffs. That is, in a given municipality and year of birth, we observe approximately 3 births per day.[53] Finally, following McCrary (2008), we formally test for a discontinuity in the distribution of the running variable by running a regression that has as the dependent variable the frequency of birthdays over the calendar year, as we did with the previous three pre-determined variables.

[53] Notice in Fig. 7b that, even though we do observe discontinuities around January 1 and May 1, they correspond to Christmas–New Year's and the Labor Day holidays, respectively.



(a) Day of Birth. Raw Histogram.



(b) Day of Birth. Conditional Histogram

Fig. 7 Raw and conditional histogram for day of birth

The p-values are reported in the third column of Table 3. Only for two specifications (cubic and quadratic polynomial) and two bandwidths (five and seven days), do we reject the null at a significance level of 5%. That is, parents might be able to select approximately the week or month of the birth but they cannot choose precisely the day of birth.

As we have already mentioned, part of the variation in the available school set comes from differences across municipalities in the composition of schools according to the date when the student must turn six to be age-eligible. This variation could

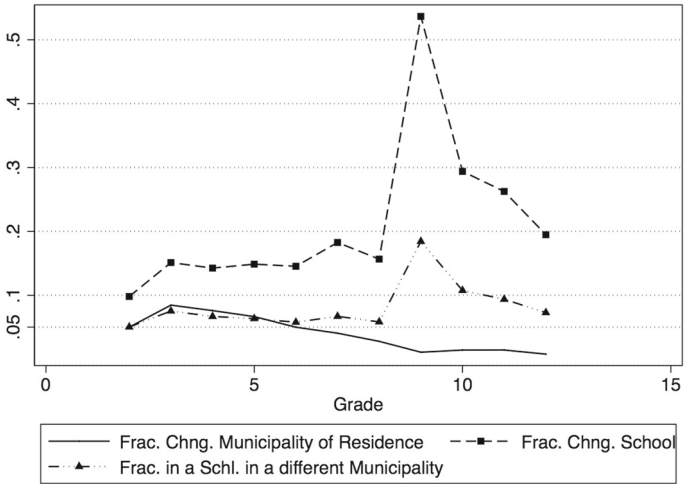


Fig. 8 Comparative Evolution of Municipality vs School change by grade

be potentially problematic in the case that families would choose the municipality of residence as a way to improve their school set. We believe this concern is not relevant in the context of our analysis. First, Chile runs a national voucher system where the school choice is not restricted to the municipality of residence. Second, the evidence does not support that families change municipality when selecting schools in Chile.

Using administrative records, Fig. 8 presents the evolution of three variables over school life: the fraction of students changing schools, the fraction of students changing municipality of residence, and the fraction of students moving to a school in a different municipality. First, the fraction of students switching municipality of residence is on average less than 5 percent per year, and it falls over school life. On the other hand, the fraction of students switching school over school life is on average 20 percent, and it increases over time. We observe a jump in grade 9 (first year of secondary school), with more than 50 percent of the students switching schools. Despite this jump in the fraction of students switching schools when moving to secondary school, there is not a jump in the fraction of students moving to another municipality. The data suggest that this switching of schools is accompanied by a change in the fraction of students looking for a school outside the municipality rather than moving to another municipality. [54]

Figure 9 shows the ratio between the first-grade enrollment and births in the municipality by day of birth, which is quite stable across cutoffs (ranging from 0.95 to 1.05). Again, if being born on the "wrong" side of a given cutoff had caused migration between municipalities, we should see discontinuities in his ratio around cutoffs. We formally test for evidence of migration between municipalities using data from birth records. Specifically, for every cell defined by day of birth and municipality, we con-

[54] These facts together suggest that the pattern of the probability of municipality switching over school life is more related to demographic factors (e.g., parents' age and their possibility to become homeowners) than to school switching.

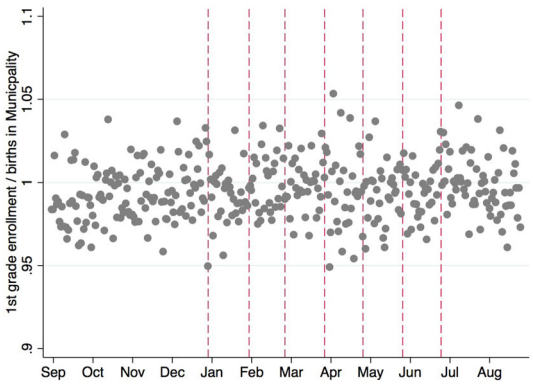


Fig. 9 Ratio 1^{st} grade enrollment/Births in the municipality, by day of birth

struct the ratio of enrollment to birth and we test for a discontinuity in this variable as we did for the rest of the variables. The results are presented in the last column of Table 3. For none of the specifications or bandwidths are we able to reject the null at a significance level of 5%.

4.3 Differences in schools across municipalities

The estimate of the contribution of school choice in equation (1) comes from the changes in a given outcome to the variation of the "doses of the treatment" (changes in school choice) that treated individuals (early entrants) were exposed to. While the exogeneity of the treated/control status is ensured by the use of the discontinuities in the probability of being an early entrant, the differences in the "*doses*" come from the relative reduction of the number of slots (over municipal population) at a given cutoff across municipalities. Even though schools choosing a specific cutoff can systematically differ across municipalities, municipality and cutoff fixed effects should account for this heterogeneity. However, the identification assumption behind our specification is that these relative reductions in the school set do not capture other school features of the schools in a given cutoff-municipality. Therefore, our results would raise concern in the case this source of variation was correlated with school characteristics choosing a given cutoff.

We explore this concern by estimating the following specification at the school level:

$$y_{scnt} = \eta_c + \rho_t + \alpha_n + \omega|\Delta R_{cn}| + X_i^{'} + \textbf{scnt}$$

with y_{scnt} representing one of the outcomes at year t for school s, with a minimum age requirements in the n^{th} cutoff, and located in municipality c. Like in the previous specifications, η_c, ρ_t, α_n and X_i represent municipality, calendar year, school minimum age cutoff fixed effects, and a vector of individual covariates, respectively.[55] Finally, the variable $|\Delta R_{cn}|$ represents the absolute change in the measure of school choice at

[55] Specifically, we include a cubic polynomial in the relative school enrollment over the municipality to account for school size.

Table 4 School characteristics and change of school choice set

Variables	(1) Public Schl.	(2) Voucher Schl.	(3) SIMCE score	(4) Parents Education	(5) Class size
$\|\Delta R\|$	0.027	−0.002	−0.467	−0.198	−0.085
	(0.020)	(0.019)	(1.401)	(0.125)	(0.493)
Sample mean	0.50	0.45	243.30	11.42	18.06
Observations	52,168	52,168	50,833	51,445	52,168
R-squared	0.296	0.262	0.211	0.450	0.703

Robust standard errors in parentheses clustered at the municipality level
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Other covariates are municipality, time and cutoff fixed effects
Also a cubic polynomial in relative enrollment to municipality population is included

a given cutoff and municipality. The parameter of interest in this specification is ω, that tell us whether or not municipalities with a larger change in a given cutoff differ in a given observed school characteristic.

Table 4 presents the estimates of ω for the probability of being a public or voucher school, average SIMCE score, average parents' education, and class size. For none of these characteristics, we find significant estimates.

5 Results

First stage

The evidence in Sect. 4 supports the first requirement in our empirical strategy as a source of randomization: other observed and unobserved factors seem uncorrelated with the treatments. Specifically, we found that the induced variation resulting from the different cutoffs is not only uncorrelated with other pre-determined variables but also there is no sign that families can to either manipulate precisely the day of birth or to move strategically between municipalities. Nevertheless, we also need the minimum age requirements to produce a relevant variation in our potentially endogenous variables, early entry and its interaction with the potential slots per student. In Figs. 3 and 4 we have already shown that the age cutoffs are associated with an evident jump not only in the fraction of students entering school as soon they are eligible but also in the school choice set for those who are not delaying. We formally test for a relevant variation in the two endogenous variables in the analysis by estimating equation (2). For clarity in the exposition, we focus on the estimates of δs (the impact of holding a birthday over any of the first six cutoffs (δ_1) or the last cutoff ($\delta_1 + \delta_2$)) and γs (the difference in the impact of early entry associated with a higher school choice set) in Tables 5 to 7, that is, the impact of the excluded instruments. Moreover, as a matter

of completeness, we also report the direct impact of the potential number of slots per student (R_{cb}). [56]

Table 5 presents the estimates for a local linear specification using a three (first four columns) and a six (last four columns) day bandwidth. For each of these two bandwidths, we show the estimates with and without other covariates (on top of the potential number of slots per student). The first element to be noticed is the qualitative robustness of the estimates when including additional variables in the model. This robustness to the inclusion of other covariates is consistent with the evidence presented in the previous section about the lack of correlation between observed predetermined variables and the treatments around the discontinuities. The second element to be noticed is the robustness of the estimates to the bandwidth used.

Regarding the estimated coefficients for the equations for $EarlyEntry$ in the models with controls (columns 3 and 7), we observe first that, keeping slots per student constant, while being born after the first six cutoffs reduces the probability of delaying school entry in approximately four to five percentage points, this probability falls to approximately 38/40 percentage points for individuals around the last cutoff. That is, while students who lose eligibility for schools with an early cutoff are still able to continue searching for a school that grants them admission the academic year closest to their sixth birthday, students crossing the last cutoff (July 1or later) are forced to start school the following year.[57] Second, notice that the impact of the available school set also has the expected positive sign on the probability of early entry. Specifically, an increase in one slot per student (R_{cb}), that is, doubling the average number of slots in the municipality, increases the probability of starting early in approximately 32 percentage points for students with a birthday before any of the seven datelines (θ in equation 2). Moreover, for students with a birthday after the first six datelines, but before July, the impact is approximately one to two percentage points higher; and for individuals with a birthday just after June 30, the impact falls approximately 20 percentage points, which is consistent with the fact that these last students cannot start school before the calendar year of their seven birthday.

The results for the equations of the interaction of $EarlyEntry$ with the potential slot per student (R_{cb}), which can be interpreted as the potential school choice set for early starters, reveal that it is positively correlated with the potential ratio (R_{cb}), and negatively correlated with having a birthday after the first six cutoffs. Notice that, not surprisingly, this negative correlation is decreasing in the school choice set.[58]

Following the equivalence with an IV approach, the value of the F-statistic for the null about the joint relevance of the excluded instruments (reported at the bottom of the table) suggests disregarding any concern about weak instruments for both variables. Specifically, we report the F-statistic for the null of all instruments being insignificant

[56] We also tried a specification where the impact differs across all the cutoffs. To avoid the inclusion of weak instruments, we opted for this more parsimonious specification (see footnote 37).

[57] Note in Fig. 4, that a little less than half of the children born before July start school the academic year closest to their sixth birthday. Therefore, our estimates for the last cutoff suggest perfect compliance with the minimum age rule.

[58] The impact of holding a birthday after any of the first six cutoffs in terms of the population parameters can be expressed as $(\delta_1 + \gamma_1 * R_{cb})$. Therefore, for an estimated $\widehat{\delta_1} < 0$ and $\widehat{\gamma_1} > 0$, this impact is decreasing in R_{cb}.

Table 5 First stage estimates

Bandwidth/Variables	3 days				6 days			
	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Birthday After Cutoff	−0.0623***	−0.0491***	−0.0547***	−0.0440***	−0.0333***	−0.0236***	−0.0468***	−0.0388***
	[0.0131]	[0.0132]	[0.0109]	[0.0128]	[0.0073]	[0.0074]	[0.0080]	[0.0080]
Bday A. Cutoff*July 1st	−0.6105***	0.0957**	−0.3847***	0.2935***	−0.6195***	0.0880**	−0.4011***	0.2861***
	[0.0756]	[0.0405]	[0.0556]	[0.0363]	[0.0753]	[0.0373]	[0.0465]	[0.0258]
Ratio*After cutoff	0.0744***	0.0585***	0.0188*	0.0099	0.0457***	0.0348***	0.0155**	0.0090
	[0.0139]	[0. 0145]	[0.0101]	[0.0127]	[0.0075]	[0.0075]	[0.0060]	[0.0072]
Ratio*After cutoff*July 1	−0.2219***	−0.9458***	−0.1985***	−0.9008***	−0.2113***	−0.9357***	−0.1842***	−0.8884***
	[0.0758]	[0.0428]	[0.0513]	[0.0216]	[0.0756]	[0.0396]	[0.0485]	[0.0210]
Ratio	0.1508**	0.9025***	0.3194***	0.9871***	0.1678**	0.9134***	0.3156***	0.9760***
	[0.0698]	[0.0367]	[0.0484]	[0.0392]	[0.0724]	[0.0393]	[0.0472]	[0.0368]
F all instruments	3005.09	4567.52	244.6	748.06	2708.08	4763.51	530.57	837.43
F Just cutoffs	67.02	7.28	52.36	33.92	57.24	7.7	56.77	61.66
F Other	14.45	287.07	7.87	977.75	18.67	278.49	10.04	905.5
Controls			X	X			X	X
Degree Pol.	1	1	1	1	1	1	1	1
Obs	1,445,864	1,445,864	1,445,864	1,445,864	2,703,506	2,703,506	2,703,506	2,703,506

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered at the municipality level

$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to when they turn six

R_{cs} is the potential number of slots per students with a birthday s at municipality c

Specifications with additional controls include municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday and calendar year fixed effects

Table 6 First stage estimates: Impact of age eligibility requirements and relative capacity. Sensitivity to bandwidth selection and degree of $g(x_i^n)$

Bandwidth/Variables	3 days			6 days		
	Early Entry (EE) [1]	R_{cb}*EE [2]	Early Entry (EE) [3]	R_{cb}*EE [4]	Early Entry (EE) [5]	R_{cb}*EE [6]
Birthday After Cutoff	−0.0379***	−0.0309**	−0.0399***	−0.0342***	−0.0464***	−0.0446***
	[0.0142]	[0.0154]	[0.0083]	[0.0088]	[0.0109]	[0.0112]
Bday A. Cutoff*July 1st	−0.5209***	0.1712**	−0.4901***	0.2306***	−0.5616***	0.1837**
	[0.0845]	[0.0829]	[0.0591]	[0.0462]	[0.0770]	[0.0756]
Ratio*After cutoff	0.0130	0.0061	0.0099*	0.0052	0.0084	0.0044
	[0.0094]	[0.0122]	[0.0057]	[0.0072]	[0.0056]	[0.0071]
Ratio*After cutoff*July 1st	−0.1538***	−0.8736***	−0.1492***	−0.8674***	−0.1189***	−0.8506***
	[0.0485]	[0.0218]	[0.0461]	[0.0209]	[0.0439]	[0.0215]
Ratio	0.3699***	1.0174***	0.3440***	0.9916***	0.3733***	1.0081***
	[0.0490]	[0.0526]	[0.0474]	[0.0437]	[0.0478]	[0.0509]
F all instruments	53.73	519.28	175.68	659.25	64.41	512.42
F Just cutoffs	32.78	2.88	56.73	15.71	45.44	9.26
F Other	5.29	888.55	6.64	870.84	4.82	784
Controls	X	X	X	X	X	X
Degree Pol.	2	2	2	2	3	3

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.

$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to when they turn six.

R_{cs} is the potential number of slots per students with a birthday s at municipality c.

Specifications with additional controls include municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects

Table 7 First stage estimates. Heterogeneity by parents' educational level

Bandwidth/Variables	6 days; 12 or less yrs education				6 days; more than 12 yrs education			
	Early Entry (EE) [1]	R_{cb}*EE [2]	Early Entry (EE) [3]	R_{cs}*EE [4]	Early Entry (EE) [5]	R_{cb}*EE [6]	Early Entry (EE) [7]	R_{cb}*EE [8]
Birthday After Cutoff	−0.0441***	−0.0375***	−0.0429***	−0.0377***	−0.0491***	−0.0394***	−0.0353***	−0.0290**
	[0.0096]	[0.0086]	[0.0107]	[0.0098]	[0.0101]	[0.0109]	[0.0111]	[0.0123]
Bday A. Cutoff*July 1st	−0.4335***	0.2751***	−0.5054***	0.2342***	−0.3500***	0.3155***	−0.4490***	0.2515***
	[0.0341]	[0.0230]	[0.0481]	[0.0399]	[0.0601]	[0.0323]	[0.0743]	[0.0571]
Ratio*After cutoff	0.0108	0.0063	0.0045	0.0024	0.0186**	0.0102	0.0136*	0.0064
	[0.0079]	[0.0082]	[0.0077]	[0.0081]	[0.0076]	[0.0094]	[0.0074]	[0.0094]
Ratio*After cutoff*July 1st	−0.1770***	−0.9102***	−0.1419***	−0.8902***	−0.1972***	−0.8751***	−0.1643***	−0.8548***
	[0.0313]	[0.0144]	[0.0286]	[0.0143]	[0.0649]	[0.0325]	[0.0626]	[0.0319]
Ratio	0.2893***	0.9866***	0.3113***	0.9980***	0.3401***	0.9670***	0.3727***	0.9852***
	[0.0356]	[0.0293]	[0.0375]	[0.0348]	[0.0610]	[0.0531]	[0.0600]	[0.0620]
F all instruments	366.86	1333.28	139.19	1235.38	313.46	330.06	79.75	232.49
F Just cutoffs	124.19	71.79	91.02	19.77	26.74	47.67	25.84	10.11
F Other	16.33	1998.68	12.35	1938.19	7.21	373.2	4.89	372.94
Controls	X	X	X	X	X	X	X	X
Degree Pol.	1	1	2	2	1	1	2	2

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.

$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to when they turn six.

R_{cs} is the potential number of slots per students with a birthday s at municipality c.

Specifications with additional controls include municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects

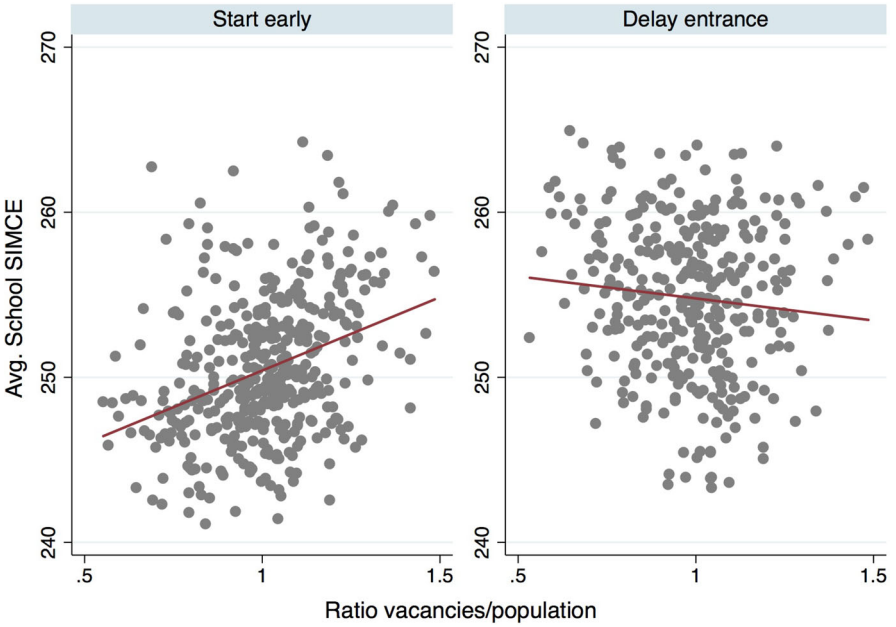


Fig. 10 Average school's SIMCE score and ratio vacancies/population, by age of entry

(F all instruments), just the one associated with the age cutoffs (F just cutoffs), and the one associated with the interaction of the cutoffs with the slots per student (F others). Although it is not a formal test for under-identification, the F-statistics suggest that the identification of early entry comes from the minimum age eligibility rules (cutoffs) and its interaction with potential slots, while the interaction of early entry with potential slots, is mainly from the interaction between the cutoffs and potential slots.[59]

Table 6 reports the previous estimates using a quadratic and cubic (for the 6 day bandwidth) specification for $g(b_i)$. The estimates are pretty robust to the one observed with a linear specification. Finally, Table 7 reports the estimates for a 6-day bandwidth, dividing the sample according to the highest educational level of the parents. From this analysis, we not only confirm a relevant variation for these two subsamples, but also the robustness of our estimates across families with different educational levels.

School characteristics

Our results on outcomes related to the first schools' characteristics suggest that, even though children who start school early do so in "worse" schools (in terms of their average SIMCE score and parents' education) than children who delay, this impact is a function of the size of the school set. Therefore, a larger school choice set increases the probability that children who start early would end up in voucher schools, in schools

[59] We formally report the under-identification, and weak instruments test for each of the outcomes in the second stage. We proceed in this way since the sample for each of the outcomes is not necessarily the same.

with a higher average SIMCE score and with more educated parents. Figure 10 helps us summarize the main findings. The left panel presents the correlation between the average school's SIMCE score and the ratio of vacancies to population for students who started school the first year that were eligible. The right panel depicts the same relation for children who delayed entry, that is, did not face any restriction in the set of schools. Moreover, to facilitate the comparison, we fit a linear model. Notice in the figure the positive association between the school choice set (measured by the ratio) and the "quality" of the school (measured by the average SIMCE score) for those children who start early, compared to those who delay entrance.[60]

The estimates of equation (1) on school characteristics are shown in Tables 8 (complete sample) and 9 (by parents' level of education). Specifically, Panel A of Table 8, shows the OLS estimates for the impact of early entry, and the potential slots per student.[61] Panel B and C show the RD estimates using the point estimate optimal and the inference optimal bandwidths, respectively. Finally, Panel D presents the underidentification and weak instruments test associated with the results presented in Panel B. From this last panel, we not only confirm an independent source of variation for each of the endogenous variables but also that the source variation induced by minimum age eligibility requirements are such that they allow us to rule out a concern about weak instruments. First, for the complete sample, and different from what is observed from the OLS, a larger school choice is associated with an increase in the probability that children would be enrolled in voucher schools of around 5 percentage points. Taking the difference between January and June in slots per student (around 0.3 slots), this implies an impact of an approximately 1.5 percentage point increase in the probability that a student would be enrolled in a voucher school (2.9% with respect to the sample mean). In terms of the penalty associated with SSA, closing the average gap in school set between students born in June and those born in January would imply a reduction of approximately 35% in the cost of SSA in terms of the reduction of the probability to start in a voucher school.[62] Focusing on the estimates for the rest of the outcomes, we observe that an increase in the number of slots available per student is associated not only with an increase in the school's average SIMCE scores but also in the average parents' education of children enrolled in the school. Specifically, an increase in a third of the slots per student is associated with an approximately 0.02σ increase in school's SIMCE scores and approximately 0.09 years more in average parent's education (0.8% in terms of the sample mean). These magnitudes in terms of the penalty of SSA would imply a 25% and 20% reduction in this toll in terms of school SIMCE and average parents education. The only result that may seem counterintuitive is in the case of class size, where we observe a positive impact of slots per student in class size. This finding is jointly consistent with parents/schools prioritizing other

[60] A similar relation is observed for the other outcomes, but for reasons of space, we leave it in Appendix.

[61] OLS estimates were obtained by using a sample of students with a birthday 15 days around the cutoffs.

[62] In terms of equation 1, the estimated penalty of SSA for an average student born in June with approximately 0.8 slots per student is $\widehat{\alpha_1} + 0.8\widehat{\alpha_2}$. This penalty for a student born in January facing approximately 1.1 slots per student would fall to $\widehat{\alpha_1} + 1.1\widehat{\alpha_2}$. Therefore, the calculated percentage reduction by closing the average gap in school choice set is $\frac{0.3\widehat{\alpha_2}}{(\widehat{\alpha_1}+0.8\widehat{\alpha_2})}100$. We are also reporting in online appendix the estimated impact of SSA at the 10, 25, 50, 75, and 90 percentile of the distribution of our measure of school choice set.

Table 8 Impact of early entry and school choice set. School characteristics

		First schl. voucher	Class size	Parents Educ at FG	Schl SIMCE
A	OLS				
	Early entry (EE)	0.0167	−2.1176***	−0.7532***	−0.3251***
		[0.0234]	[0.4289]	[0.0974]	[0.0514]
	EE*Ratio	−0.0017	1.6440***	0.4055***	0.1861***
		[0.0210]	[0.4139]	[0.0908]	[0.0471]
	Ratio	−0.0085	-0.1834	0.0265	0.0144
		[0.0095]	[0.1562]	[0.0401]	[0.0181]
	RD estimates	MSE-optimal bandwidth			
B	Early entry (EE)	−0.0814***	−1.9828***	−0.5986***	−0.1701***
		[0.0194]	[0.3372]	[0.1235]	[0.0440]
	EE*Ratio	0.0501***	0.7641***	0.2950***	0.0760***
		[0.0129]	[0.2705]	[0.0889]	[0.0290]
	Ratio	-0.0028	-0.1290	-0.0612	−0.0084
		[0.0079]	[0.1241]	[0.0438]	[0.0156]
	Obs	413,413	495,242	208,254	287,095
		Inference valid bandwidth			
C	Early entry (EE)	−0.0910***	−2.0261***	−0.4960***	−0.1615***
		[0.0257]	[0.3818]	[0.1341]	[0.0530]
	EE*Ratio	0.0542***	0.7166**	0.2604***	0.0759**
		[0.0153]	[0.2965]	[0.0891]	[0.0324]
	Ratio	−0.0028	−0.0905	−0.0535	−0.0113
		[0.0086]	[0.1326]	[0.0434]	[0.0169]
	Obs	289,211	330,156	173,360	204,470
D					
	Kleibergen-Paap rk LM statistic Under-identification (p-values)				
		0.000	0.000	0.000	0.000
	Weak identification test				
	Kleibergen-Paap rk Wald F statistic				
		539,059	593,607	340,56	451,015

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.
$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to becoming six years of age.
R_{cs} is the potential number of slots per students with a birthday s at municipality c.
Additional controls: municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects.
(a) Stock-Yogo weak ID test critical values for 5, 10, and 15 percent maximal IV relative bias are 11.04, 7.56, and 5.57, respectively

school inputs over class size and the larger class size observed in voucher schools (Epple et al. 2017).

Table 9 shows the heterogeneity of the previous results by parents' education level. The upper two panels present the results using a MSE-optimal bandwidth while the bottom two show those for an inference valid bandwidth. For a given bandwidth type, the upper panel presents the estimates for parents with 12 years of education or less, while the lower panel shows those families with more than 12 years of education. [63] For this group of outcomes, the results are similar for both groups of parents.[64]

Student's individual performance

As in the case of school characteristics, Figure 11, which shows the correlation between the school choice set and standardized (math) scores by age of entry, summarizes our results on individual performance. Notice that, even though children who delay entrance tend to have better scores on average, scores for children who start early depend positively on the ratio vacancies/population.[65] Our results below also suggest that a larger school choice set decreases the probability of dropping out and the likelihood of switching schools during elementary school and has positive effects on standardized test scores (SIMCE). Moreover, students with more choices are more likely to take the college examination exam (PSU) immediately after high school graduation and, only for children of less educated parents, to enroll in a selective college.

Table 10 presents the estimates of equation (1) for the second group of outcomes and Table 11 shows the results by parents' education.

Panel A of Table 10 shows that OLS estimates suggest a negative effect associated with starting school early ($\alpha_1 + \alpha_2 R_{cb} < 0$) but also that this impact is decreasing in the school choice set ($\alpha_2 \neq 0$), for all the selected outcomes. Looking at the point estimates (Panels B and C), notice that, with the exception of the outcome of "dropout" and the one associated with the probability of taking the PSU examination the year of high school graduation, the point estimates tend to be lower than those from OLS. [66] Notice also that, while the school performance outcomes are robust to the bandwidth selection, the two outcomes related to college admission are only significant when using the MSE-optimal bandwidth.

Specifically, we observe a significant effect on dropout, with one more slot per student being associated with a decrease of approximately 4 percentage points. If

[63] Parents with missing information about years of education are pooled with those having 12 or fewer years of education.

[64] As an additional robustness check (Tables B.9 and B.10 in online appendix), we estimated the model using the whole sample, but allowing the impact of school choice set to differ across the two groups defined by parents' education. We do not detect a difference in the contribution of school choice between these families. However, the results must be taken with caution, given the loss of precision when we include additional endogenous variables with a limited number of instruments.

[65] A similar relation is explored and reported for the rest of the outcomes in Appendix.

[66] In the context of student's ability as one of the omitted variables, the previous evidence suggests that more able students are the ones less likely to delay school entry, and who profit more from larger school choice.

Table 9 Impact of early entry and school choice set. School characteristics by parent's education

		First schl. voucher	Class size	Parents Educ at FG	Schl SIMCE
A	MSE-optimal bandwidth				
	12 years of education or less				
	Early entry (EE)	−0.0923***	−2.7418***	−0.4826***	−0.1900***
		[0.0289]	[0.4252]	[0.1493]	[0.0587]
	EE*Ratio	0.0548***	1.2255***	0.2604***	0.0725**
		[0.0183]	[0.3029]	[0.0955]	[0.0368]
	Ratio	0.0052	−0.2843*	−0.0509	−0.0130
		[0.0103]	[0.1691]	[0.0556]	[0.0203]
	Obs	229,024	274,402	97,808	158,949
	More than 12 years of education				
	Early entry (EE)	−0.0713*	−1.2326**	−0.7556***	−0.1544**
		[0.0386]	[0.4821]	[0.1837]	[0.0697]
	EE*Ratio	0.0474*	0.3464	0.3503***	0.0729
		[0.0249]	[0.3319]	[0.1251]	[0.0455]
	Ratio	−0.0134	−0.0423	−0.0597	−0.0017
		[0.0122]	[0.1882]	[0.0532]	[0.0228]
	Obs	184,389	220,840	110,446	128,146
B	Inference valid bandwidth				
	12 years of education or less				
	Early entry (EE)	−0.0974***	−2.5955***	−0.3948**	−0.1204
		[0.0346]	[0.5064]	[0.1615]	[0.0762]
	EE*Ratio	0.0656***	1.0827***	0.2448***	0.0563
		[0.0195]	[0.3520]	[0.0933]	[0.0453]
	Ratio	0.0061	-0.2001	−0.0288	0.0009
		[0.0113]	[0.1895]	[0.0584]	[0.0239]
	Obs	159,979	182,809	81,347	113,198
	More than 12 years of education				
	Early entry (EE)	−0.0908**	−1.5074***	−0.6173***	−0.2411***
		[0.0389]	[0.5779]	[0.2065]	[0.0838]
	EE*Ratio	0.0443*	0.4337	0.2919**	0.1004*
		[0.0233]	[0.3809]	[0.1321]	[0.0545]
	Ratio	-0.0150	-0.0360	-0.0613	-0.0205

Table 9 continued

	First schl. voucher	Class size	Parents Educ at FG	Schl SIMCE
	[0.0112]	[0.1967]	[0.0554]	[0.0263]
Obs	129,232	147,347	92,013	91,272

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.
$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to becoming six years of age.
R_{cs} is the potential number of slot per students with a birthday s at municipality c.
Additional controls: municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects.
12 years of education or less indicates the sample of students whose parents obtained 12 or less years of education. More than 12 years of education, the sample of children whose parents attained more than 12 years of education

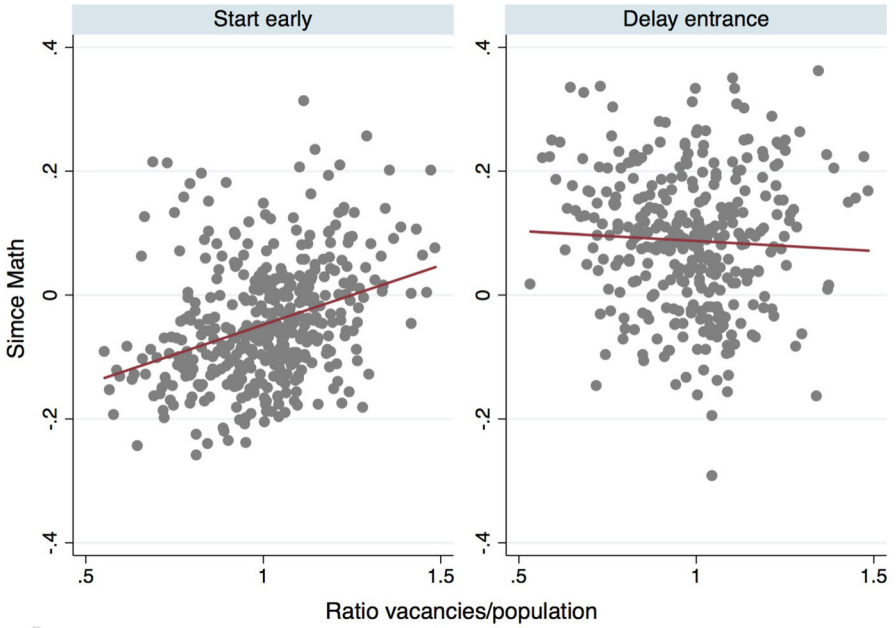


Fig. 11 Individual SIMCE score (Math) vs. ratio vacancies/population

we consider a change of around a third of a vacancy in the municipality (January–June), this implies an 8% reduction in terms of the sample mean or a reduction of approximately 60% in the penalty of an early start when closing the average gap between students born in June respect to the ones born in January.

Our estimates also suggest a reduction in the probability of switching schools later in elementary school. Specifically, an increase in one additional slot per student reduces by approximately 1.5 percentage points the probability of moving later on to another

Table 10 Impact of early entry and school choice set. School performance

		SIMCE					
		Math	Language	Dropout	Move schl. during elementary	Took PSU yr HS graduation	Ever Enrolled
A	OLS						
	Early entry (EE)	−0.3908***	−0.3903***	0.0058	0.0484***	0.0021	−0.0508***
		[0.0225]	[0.0196]	[0.0059]	[0.0058]	[0.0108]	[0.0153]
	EE*Ratio	0.1144***	0.1176***	−0.0070	−0.0219***	0.0396***	0.0440***
		[0.0212]	[0.0191]	[0.0055]	[0.0058]	[0.0105]	[0.0140]
	Ratio	0.0647***	0.0571***	0.0132***	0.0019	-0.0076	0.0030
		[0.0122]	[0.0123]	[0.0028]	[0.0030]	[0.0070]	[0.0076]
	RD estimates				MSE-optimal bandwidth		
B	Early entry (EE)	−0.5007***	−0.4442***	0.0583***	0.0335***	−0.0595*	−0.0275
		[0.0751]	[0.0851]	[0.0212]	[0.0075]	[0.0346]	[0.0289]
	EE*Ratio	0.0684*	0.0537	−0.0443***	−0.0146***	0.0618**	0.0319*
		[0.0351]	[0.0385]	[0.0127]	[0.0053]	[0.0242]	[0.0177]
	Ratio	0.0023	−0.0044	−0.0014	0.0062**	−0.0141	0.0014
		[0.0192]	[0.0223]	[0.0066]	[0.0030]	[0.0113]	[0.0109]
	Obs	411,032	351,521	187,895	3,368,048	180,182	180,182

Table 10 continued

	SIMCE					
	Math	Language	Dropout	Move schl. during elementary	Took PSU yr HS graduation	Ever Enrolled
	Inference valid bandwidth					
Early entry (EE)	−0.5584***	−0.4649***	0.0583***	0.0384***	−0.0115	−0.0001
	[0.0986]	[0.0939]	[0.0212]	[0.0084]	[0.0381]	[0.0404]
EE*Ratio	0.0855*	0.0593	−0.0443***	−0.0157***	0.0232	0.0111
	[0.0455]	[0.0444]	[0.0127]	[0.0058]	[0.0236]	[0.0234]
Ratio	−0.0023	0.0036	−0.0014	0.0067**	−0.0034	0.0059
	[0.0216]	[0.0229]	[0.0066]	[0.0032]	[0.0127]	[0.0125]
Obs	292,832	292,701	187,895	2,454,089	128,595	128,595
D						
Kleibergen-Paap rk LM statistic Under-identification (p-values)						
	0.000	0.000	0.000	0.000	0.000	0.000
Weak identification test						
Kleibergen-Paap rk Wald F statistic						
	236,401	199,161	251,189	544,176	293,739	293,739

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.
$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to becoming six years of age.
R_{cs} is the potential number of slots per students with a birthday s at municipality c.
Additional controls: municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects.
(a) Stock-Yogo weak ID test critical values for 5, 10, and 15 percent maximal IV relative bias are 11.04, 7.56, and 5.57, respectively

Table 11 Impact of early entry and school choice. School performance by parent's education

		SIMCE					
		Math	Language	Dropout	Move schl. during elementary	Took PSU yr HS graduation	Ever Enrolled
A	MSE-optimal bandwidth						
	12 years of education or less						
	Early entry (EE)	−0.5495***	−0.5347***	0.0634*	0.0416***	−0.0932**	−0.0846***
		[0.0873]	[0.0921]	[0.0342]	[0.0096]	[0.0426]	[0.0292]
	EE*Ratio	0.0940**	0.1152**	−0.0512***	−0.0238***	0.0837***	0.0612***
		[0.0466]	[0.0503]	[0.0193]	[0.0067]	[0.0308]	[0.0189]
	Ratio	−0.0303	−0.0260	0.0010	0.0060	−0.0245	−0.0158
		[0.0262]	[0.0315]	[0.0111]	[0.0040]	[0.0180]	[0.0109]
	Obs	191,915	164,495	101,451	1,798,300	101,565	101,565
	More than 12 years of education						
	Early entry (EE)	−0.4957***	−0.3641***	0.0367*	0.0280**	-0.0187	0.0468
		[0.1241]	[0.1328]	[0.0217]	[0.0118]	[0.0523]	[0.0538]
	EE*Ratio	0.0289	-0.0347	-0.0265**	−0.0081	0.0335	-0.0059
		[0.0471]	[0.0486]	[0.0119]	[0.0073]	[0.0317]	[0.0291]
	Ratio	0.0232	0.0131	−0.0035	0.0072*	−0.0059	0.0126
		[0.0249]	[0.0268]	[0.0052]	[0.0039]	[0.0156]	[0.0189]
	Obs	219,117	187,026	86,444	1,569,748	78,617	78,617
B	Inference valid bandwidth						

Table 11 continued

	SIMCE					
	Math	Language	Dropout	Move schl. during elementary	Took PSU yr HS graduation	Ever Enrolled
12 years of education or less						
Early entry (EE)	−0.5844***	−0.5519***	0.0634*	0.0465***	−0.0545	−0.0764**
	[0.1157]	[0.1013]	[0.0342]	[0.0112]	[0.0515]	[0.0374]
EE*Ratio	0.1201**	0.1385***	−0.0512***	−0.0248***	0.0458	0.0515**
	[0.0549]	[0.0521]	[0.0193]	[0.0077]	[0.0323]	[0.0227]
Ratio	−0.0285	−0.0243	0.0010	0.0053	−0.0104	-0.0125
	[0.0299]	[0.0312]	[0.0111]	[0.0044]	[0.0208]	[0.0127]
Obs	136,611	136,765	101,451	1,308,665	72,499	72,499
More than 12 years of education						
Early entry (EE)	−0.6135***	−0.4080***	0.0367*	0.0323**	0.0518	0.1023
	[0.1690]	[0.1493]	[0.0217]	[0.0136]	[0.0621]	[0.0766]
EE*Ratio	0.0467	−0.0396	-0.0265**	-0.0089	-0.0113	-0.0464
	[0.0664]	[0.0619]	[0.0119]	[0.0077]	[0.0337]	[0.0411]
Ratio	0.0150	0.0265	-0.0035	0.0086**	0.0013	0.0173
	[0.0289]	[0.0289]	[0.0052]	[0.0040]	[0.0177]	[0.0211]
Obs	156,221	155,936	86,444	1,145,424	56,096	56,096

***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors clustered at the municipality level.

$EarlyEntry$ is a dummy variable that takes a value one for children who start primary school the closest academic year to becoming six years of age.

R_{cs} is the potential number of slots per students with a birthday s at municipality c.

Additional controls: municipality-year of birth, parents' education, gender, weekday of birth, born on a holiday, and calendar year fixed effects.

12 years of education or less indicates the sample of students whose parents obtained 12 or less years of education. More than 12 years of education, the sample of children whose parents attained more than 12 years of education

elementary school (or a little more than 10% reduction in terms of the sample mean). In terms of the penalty associated with SSA, the estimates suggest a reduction of 20% when closing the average gap in school choice set between students born in June with respect to those born the start of the year. This result suggests that a larger school choice set increases the likelihood of a better early match which reduces the future search for other schools and therefore reduces the future potential penalty of switching schools.

Regarding test scores, we also observe that a larger school choice set when being enrolled in primary school is associated with an increase of approximately 0.07σ in the math SIMCE, approximately 4% reduction in terms of SSA penalty.

Finally, the last two outcomes reveal that a larger school choice set is not only associated with an increase in the likelihood that a student would take the PSU, but we also observe an impact on the probability of being enrolled in one of the 33 best Chilean universities. Specifically, an increase in slots per student by one-third is associated with an increase of approximately 2 percentage points in the probability of taking the PSU (or 3% in terms of sample mean) and one percentage point in the probability of being enrolled in one of these colleges.

Table 11 presents the heterogeneity by parents' educational level. The picture is clear. The previously reported findings are mainly driven by families with lower levels of education. Specifically, we observe that for SIMCE scores an increase of one slot per student is associated with an increase of approximately a 0.1σ, a reduction of 5 percentage points in the likelihood of dropping out (30% with respect to the sample mean), and a decrease of 2 percentage points (14%) in the probability of switching school later on. We also observe, for this group of students, that an increase in the number of slots per student is associated with an increase in the probability of being enrolled in college that is robust to the bandwidth selected. Specifically, an increase by one-third in the number of slots per student is associated with an increase of approximately 1.6 percentage points in the probability of being enrolled in college, which in terms of the sample mean is approximately 8%.[67] This larger effect among families with less educated parents is in line with recent evidence reported for Denmark where SSA has an impact on the family as a whole. Specifically, (Landersø et al. 2020) show that SSA has an effect on mother labor force participation. Moreover, Gelbach (2002) shows for the USA that mothers with a child five years old age-eligible to attend public school increase their attachment in the labor market.

6 Conclusions

Using individual administrative records from Chile, we study the impact of school choice measured by the number of available slots per student in the municipality at

[67] For the alternative specification (available in online appendix), that jointly estimate for both sample the contribution of school choice set, we only detect a significant difference in the contribution of school choice set for the outcome of college enrollment. Nevertheless, consistent with the results when splitting the sample, a significant contribution of school choice reducing the penalty of early start is observed consistently just for students with less educated parents. For students with more educated parents, however, a significant contribution of school choice is only observed for the probability of dropping out. As we noticed for the previous group outcomes, these results must be taken with caution considering the loss of precision.

the time of enrollment in first grade of primary education on a series of individual educational outcomes. By using a quasi-experimental source of variation in school choice, we uncover positive effects associated with a larger school choice.

We show first that the set of available schools induces a relevant shift in the opportunity to start in a better school measured by a non-high-stake examination. Moreover, this quasi-experimental variation reveals an important reduction in the likelihood of dropping out and a reduction in the probability that a child would switch schools over her/his school life. Second, for a subsample of students who have completed high school, we observe that a larger school choice at the start of primary school increases students' chance of taking the national examination required for higher education and the likelihood of being enrolled in a selective college.

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