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ABSTRACT

Minimum Age Requirements and the Impact of School Choice*

Using several data sources from Chile, we study the impact of school choice at the time of starting primary school. To study the contribution of school choice, we exploit the combination of multiple cutoffs defining the minimum age at entry, and the difference across municipalities in the composition of the schools according to these cutoffs. Children living in the same municipality, and whose birthday differs by a few days not only have their incentives to delay school entry affected, but also face, in case of not delaying, a different set of schools. We show that a larger set of schools increases the probability of starting in a better school, measured by 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. Secondly, 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.

JEL Classification: A21, I24, I25, I28

Keywords: age requirements, school choice

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1 Introduction

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 the time of enrollment in first grade of primary education, on individual educational outcomes.

A position in favor or against school choice does not clearly stand out in the existing literature (Epple, Romano, and Urquiola, 2017). The ambiguity resides in the heterogeneity in the programs that have been implemented/studied 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 with the size of the intervention.[1] Moreover, in large-scale interventions, a non-random sorting of students across schools and the existence of spillovers, not only makes it difficult to find a credible control group but also speaks for offsetting effects for some individuals in the population(see, for example Hsieh and Urquiola 2006). [2]

The basic problem in studying the contribution of school choice on student's attainment is the fact that it is nonrandom. Moreover, the characteristics of the families that choose a given school within a given school market are not only partially observed by the researcher, but these variables might directly affect student's future attainments. To address this problem, we rely on a quasi-experimental variation coming from an institutional feature of the Chilean educational system where families/children face multiple birthday cutoffs defining the minimum age when a student can start the first grade of elementary school. While minimum age requirements have been extensively used to estimate the impact of age of entry (Cascio and Lewis, 2006; Black, Devereaux, and Salvanes, 2011; McEwan and Shapiro, 2006),[3] here we use this particular feature to not only capture the effect of

[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]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 (forthcoming, 2018) show that those effects tend to wear off over time.

age of entry but also to estimate the impact of starting within a larger school choice set. Specifically, 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 reduced set of schools in the case in which they want them to start in the current year.[4]

Our paper is related to several strands of the literature. First, this paper contributes to the growing literature concerning the impact of attending a "better" school which, with Dale and Krueger (2002) and Cullen, Jacob, and Levitt (2006) as early examples, presents mixed results in general. On the positive side, for example, Dobbie and Fryer (2015) find a positive effect of attending New York high-performing charter schools on academics and risky behaviors measured by teen pregnancy and incarceration. Using data from public school choice lotteries in North Carolina, Deming (2011) finds that seven years after random assignment, lottery winners had been arrested for fewer serious crimes and had spent fewer days incarcerated. Also on the positive side, Angrist et al. (2002, 2006) find that a small scale policy where randomly distributed vouchers for (in excess demand) private secondary schools in Colombia have positive short and long-term educational effects of vouchers' winners. [5]Other studies, however, reveal mild or lack of effects. For example, Abdulkadiroglu, Pathak, and Walters (2018) use randomized lotteries to evaluate a voucher plan that provides public funds for disadvantaged students to attend private schools in Louisiana. They find that participation in the program lowers math scores by 0.4 standard deviations and also reduces achievement in reading, science, and social studies. These effects are likely explained by selection of low-quality private schools into the program. [6] ,[7]

[4]On top of the students' birthday, the fact that the distribution of schools across cutoffs varies by municipalities (and that students tend to go to a school in their municipality of residence) provides an additional source of variation that allows us to isolate the impact of the choice set, by instrumenting age of entry.

[5]Despite a positive effect on test scores (0.2σ) and on the probability to complete secondary school (15%-20%), Deming (2011) suggests as a possible channel explaining these results, the specific incentives associated to the renewal of the voucher rather than the voucher provision itself.

[6]In addition, Pop-Eleches and Urquiola (2013), studying the high school admission system in Romania, find that though students who have access to better high schools perform better in a graduation test, they realize that they are relatively weaker and feel marginalized, and parents reduce effort when their children attend a better school.Abdulkadiroglu, Angrist, and Pathak (2014) and Dobbie and Fryer (2014), studying the case of elite public schools in Boston and New York city find no effects.

[7]de Janvry, Dustan, and Sadoulet (2016) find that admission to an elite school in Mexico is associated to modest rewards on test scores (math) and a considerable increase in the likelihood of dropping out for many students. Also, Hoekstra, Mouganie, and Wang (2016) show for China that while attending a better quality school is associated to better peers, there is a positive effect on individual performance only among students enrolled in "Tier I" HS.

One contribution of this paper is the source of variation used to identify the treatment. Here 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 one more homogeneous in terms of family income or previous educational outcomes. Thus, we are also able to capture the potential heterogeneity across parents' education.

A second contribution of this paper is related to the timing of the treatment under analysis. Here we study the impact of school choice at the time of first enrollment in elementary school rather than at high school or college.[8] That is, we contribute to recent literature studying the impact of early interventions on children's outcomes.[9]

Finally, we study the impact of a marginal change in the school choice set for a country (Chile) with a national voucher system, that is, where in theory families are already making use of school choice to search, use and profit from better school quality.[10] Due to the methodological challenges associated to large-scale programs, most of the evidence for Chile points to the effect on cream skimming and mixed results on test scores.[11] For example, 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.[12] Our quasi-experimental variation allows us to study families' behavior and its consequent effects independently of schools' strategic behavior. [13]

[8]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.

[9]Banerjee et al. (2007), find for India that more resources helping students "lagging behind" have an important gain in term 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, Galiani, and Manacorda (2008) and Berlinski, Galiani, and Gertler (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.

[10]Around 93% of students in Chile attend schools being funded through this voucher.

[11]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.

[12]Moreover, 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.

[13]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

Our results reveal, first, that a larger set of available schools induces a relevant shift in the opportunity to start in a better school, measured by their average score in a standardized exam and by average parents' education. Second, using this quasi-experimental variation, we find that starting with a larger school set causes an important reduction in the likelihood of dropping out, and a reduction in the probability that a student switches schools over her/his school life. Secondly, for a subsample of students who have completed high school, we observe a positive effect on the probability of taking the college admission test the year of high school graduation. Moreover, for families with less educated parents, we also observe a positive effect on the probability of enrolling in college. In fact, for the majority of these outcomes, we show that the impact is greater among students whose parents have lower levels of education.

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 Section 3 we describe our empirical strategy and the selected outcomes in the analysis. In Section 4 we discuss the validity of our RD design. In Section 5 we present our results, and Section 6 concludes.

2 Chilean education system and data sources

Since a major educational reform in the early 1980s,[14] 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).[15] Though

schools she observed an increase in test score. Though the treatment from the perspective of the targeted families can be seen as an increase in the school choice set, the program demanded achievement goals from participating schools, which makes it difficult to isolate the findings to the voucher provision.

[14]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).

[15]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.

both municipal and private schools get state funding through a voucher scheme, the latter are usually called voucher schools.[16]

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. 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. Despite mixed evidence on the impact of a series of reforms introduced as of the early 1980's on the quality of education[17], Chile's primary and secondary education systems are comparable in terms of coverage to any system we can observe in any developed country.

We use three different sources of data in this paper. The first source of information comes from students' records from the Ministry of Education, available from 2002 to 2014.[18] 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. Specifically, we analyze the cohorts born between 1996 and 2001, keeping those students for whom we have information since the beginning of elementary education.

The second source of data comes the national standardized test (SIMCE)[19], usually given in 4th, 8th and 10th grades (the 2nd year of high school).[20] The SIMCE database also has a supplementary parents' survey, which allows us to know the parents' education.

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

[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 find evidence that the voucher reform was associated to 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 1980's, see Contreras et al. (2005).

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

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

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

Finally, the third source of data are individual records from the Centralized College Admission System, from 2014 to 2017 (cohorts of students born between 1996 and 1999). [21]These files contain the national national standardized college admission test (PSU, *Prueba de Selección Universitaria*) and the admission records of the 33 most prestigious Chilean universities.[22]

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. In order 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. 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. Specifically, the source of friction 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.

Minimum age requirement rules have been used extensively to address the impact of age of entry (Cascio and Lewis, 2006; Black, Devereaux, and Salvanes, 2011). These rules establish that children, in order 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.[23] 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 cutoff date are entitled to start at *any* eligible school the year

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

[22]Among colleges, we are able to 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 onwards, eight "private" universities participate in the system, with one more addition in 2017.

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

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. Although Chile's official enrollment cutoff was originally set in April 1,st since 1992 the Ministry of Education has provided schools with some degree of flexibility for setting other cutoffs between January 1^{st} and July 1^{st}.[24] In fact, McEwan and Shapiro (2006), in order to estimate the impact of age of entry in Chile, use the four most used cutoffs in practice: April 1^{st}, May 1^{st}, June 1^{st} and July 1^{st}. However, 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 next 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. In the case of students born shortly before January 1^{st}, they are in theory able to choose any school, regardless of whether they start elementary in the closest March after their sixth birthday or they wait to start the following year. Different is the case, for example, of students born between April 1^{st} and April 30^{th}, who, unless they postpone their enrollment, are restricted to choosing among schools with deadlines later than April 1.st That is, a child's birthday not only defines when a student becomes eligible to start primary school but also the the school set available in the case of starting the first year when the student is eligible.[25]

Figure 1 shows the ratio of potential 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^{st} or after June 30^{st} face the complete school set. While students who become six years old around December 31^{st} are able to start elementary school in in the following March (the academic year is March-December), those born on or after July 1^{st} have to wait until the next year (the closest March to their seventh birthday). Nevertheless, children born between January 1^{st} and June 30^{th} 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 Figure 1 that the largest

[24] See http://bcn.cl/1yw2h

[25] 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.

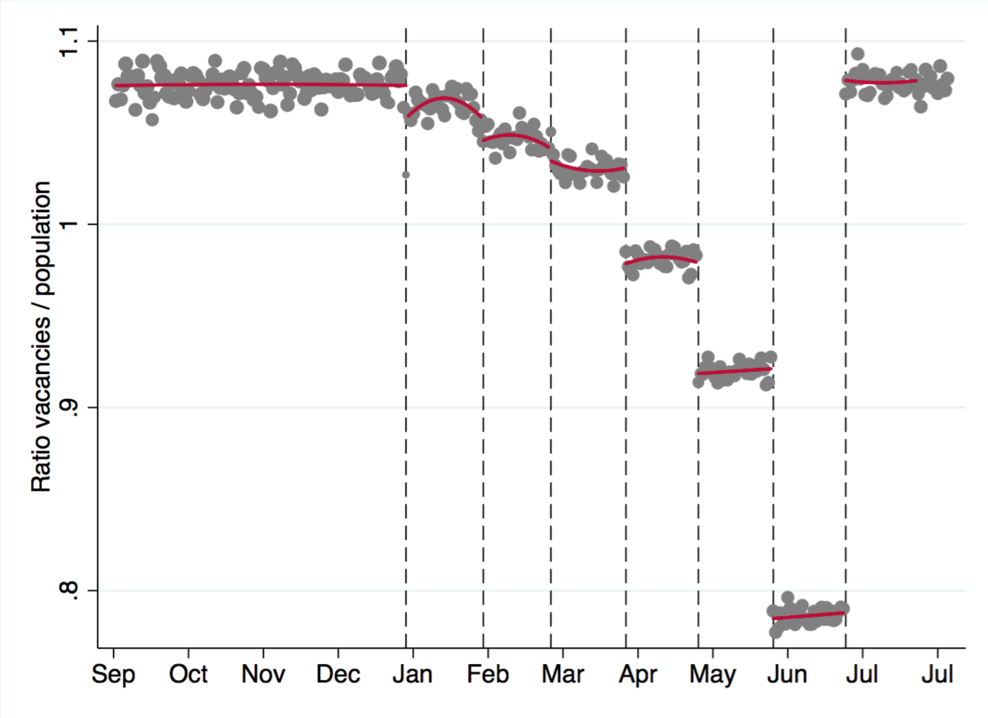


Figure 1: Vacancies over population in the municipality by birthday

decline in the set of schools occurs around the fourth or later cutoffs since the bulk of the schools use the June 1^{st} and the July 1^{st} cutoffs.

Despite the fact that schools' availability vary with children's birthdate, it is important to notice that in the case in which school quality was homogeneous, we should not observe an effect except for the one coming from delaying first grade enrollment. However, this does not appear to be the case in Chile. Figure 2 shows the distribution of the school's average score in SIMCE standardized (Math) exam along cutoffs. We observe that schools with higher SIMCE scores are the ones setting earlier deadlines. That is, children to the right of any of these cutoffs face not only a subset of the schools but also on average a reduction in the available quality of the schools.

Therefore, families with children born between January and June face a tradeoff between the school choice set and the cost of delaying the start of elementary school.

Figure 3 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^{st} are "forced" to postpone enrollment until the next year, that is, we observe a fraction close to zero. Second, while we do not observe an evident discontinuity in the fraction of students entering early around the February and March cutoffs, we do observe a distinguishable jump for the

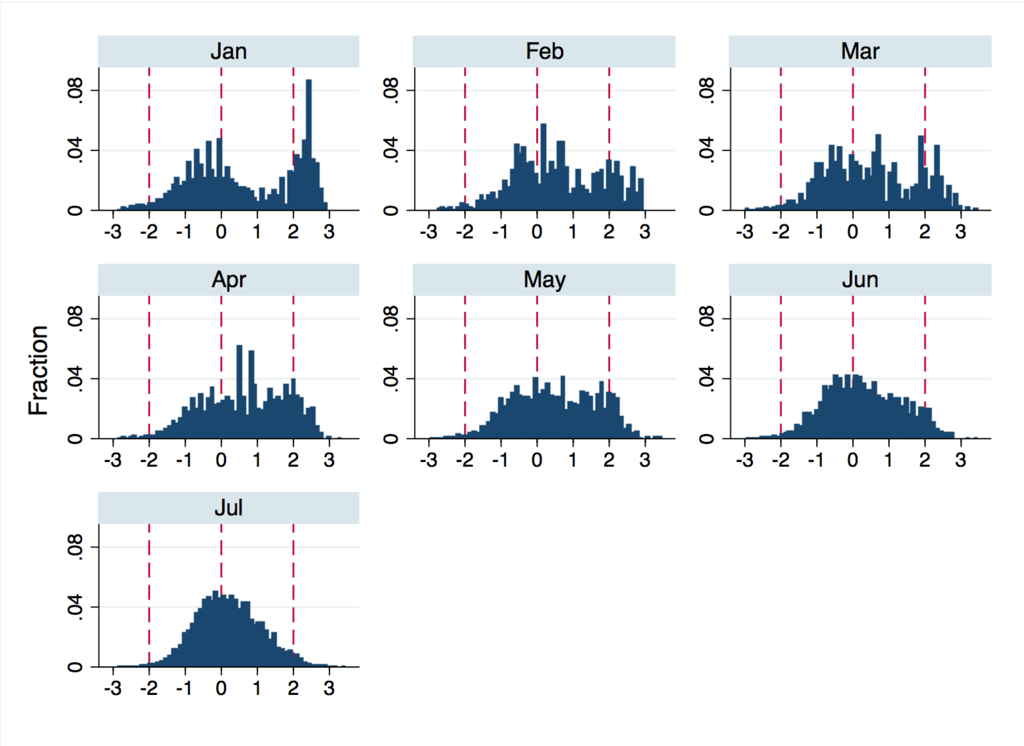


Figure 2: School's SIMCE (Math) Distribution by Cutoff

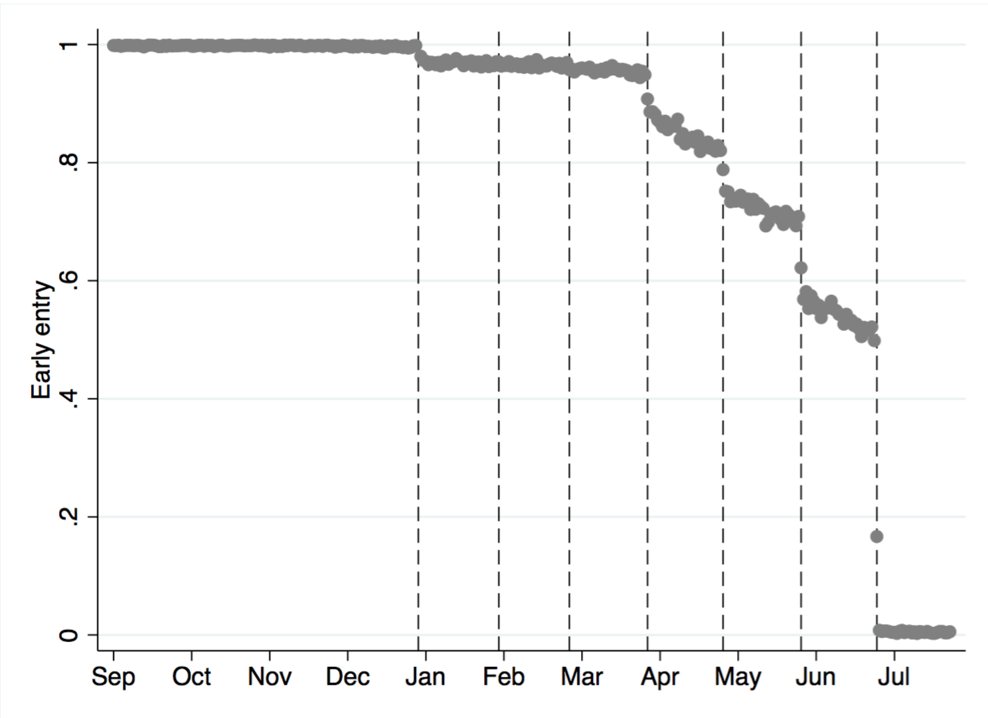


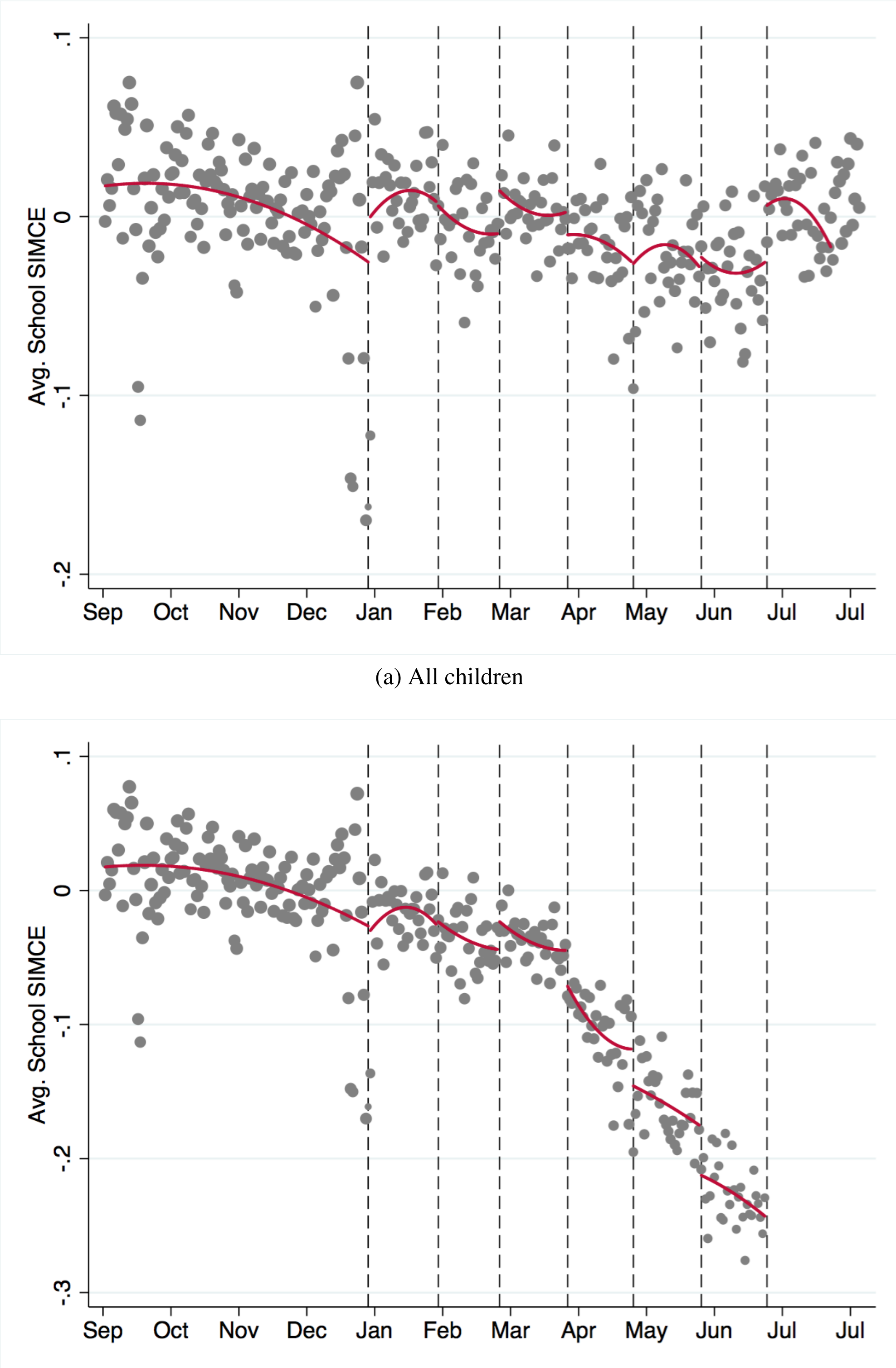
Figure 3: Fraction of students who start school the year becoming six and average age of entry

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^{st}, which is jointly explained by the perfect compliance associated to the rule of becoming six years of age after July 1^{st}, and the higher fraction of schools using this last cutoff. This discontinuity in the age of entry, together with the assumption that parents cannot fully manipulate the date of birth, has been used in the literature as quasi-experimental variation in the age of entry at core of "fuzzy"[26] Regression Discontinuity (RD) strategy 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.

Figure 4a shows the average standardized school SIMCE score of the school where the students started first grade for every birthday. Under the assumption that these test scores are correlated with school quality, the figure does not show a clear pattern (jump) over the different cutoffs. That is, it is not clear that those children born later in the calendar year, but before July, were more likely to start their primary education in a school with a lower SIMCE score (lower quality) when facing more friction in their school search. A potential explanation for this increase is related to the delay in school entry. For descriptive purposes, Figure 4b shows that, when the population is restricted to children who actually started school early, the scores monotonically decrease as we move along the calendar year. Notice also that the discontinuity in the reduction of SIMCE score (which is clear from the April 1^{st} cutoff onwards) is consistent with the discontinuity of the set of available schools around each of the cutoffs, shown in Figures 1 and 3.

In order 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. Since the composition of schools around a cutoff is not the same across municipalities (or across cutoffs within a municipality),

[26]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 day of birth, the probability of treatment would change from one to zero at the cut-off day. For more details, see Lee and Lemieux (2010).



(a) All children

(b) Children starting school early

Figure 4: School quality at first grade of elementary school by birthday

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. [27] Therefore, in order to sort out these two effects, we use, additionally to quasi-experimental variation in school eligibility around a specific cutoff, the fact that the distribution of schools across cutoffs is not the same across municipalities.

Using administrative data for the complete population of students, we are able to construct the full set of schools available in the municipality at each birthday for students in the case in which they start primary school at the closest date to their sixth birthday. 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) *in the case that they do not delay school entry*. In other words, for children who started primary school early, the cost or benefit for not delaying school entry is likely to depend on the school set faced. Specifically, even though children born before January 1,st for example, are equally eligible as children born later, those born later in the year (but before July 1st) to start at the age of six face a smaller set of available schools which also differs across municipalities. Therefore, the discontinuity in age eligibility, at least for some schools, generates at the same time a discontinuity in the school choice set that varies across municipality and cutoffs, which provides us with a quasi-experimental variation that not only addresses the endogeneity of age of entry but also the impact of school choice set in a 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 school choice can be expressed as follows,

$$y_{ict}=\eta_{cy}+\rho_t+X_i^{'}\psi+\alpha_1 EarlyEntry_i+\alpha_2 EarlyEntry_i*R_{cb}+\alpha_3 R_{cb}+g(b_i)+\varepsilon_{it} \tag{1}$$

with y_{ict} representing one of the outcomes at year t for student i, born the day b of year y, 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.

[27] 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.

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.[28] Moreover, η_{cy}, ρ_t and X_i represent municipality-year of birth, calendar year and a vector of individual covariates, respectively.[29,30]Finally, $g(b_i)$ is a flexible polynomial specification in the day of birth for a student , taking the form:

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

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.[31]

Hahn, Todd, and van der Klaauw (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 observation where both treatments (school set and age of entry) are as randomly assigned. This randomization of the treatment ensures that all other factors (observed and unobserved) determining a given outcome must be balanced at each side of these discontinuities. Specifically, in order to select an optimal bandwidth, we applied the method developed by Calonico, Cattaneo, and Titiunik (2014) at each of the cutoffs, obtaining values that on average go from 4 to 12 days around the discontinuities (see table 2). [32]

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

[29]Covariates include fixed effects for parents' education and student's gender. We use as parents' education the highest observed education between the father and the mother reported in SIMCE parents' survey. We define as an additional category for the case when the educational level is missing simultaneously for mother and 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.

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

[31]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.

[32]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 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 estimate, it is not for inference. The MSE-optimal bandwidth presents a challenge because this bandwidth choice is not "small" enough to remove the

In equation 1, the cost (or benefit) of age at entry is captured jointly by the sum of the population 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 to 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 from 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}, because the location of schools across municipalities might not be necessarily random.[33]

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. In order 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:

$$\begin{aligned} endog_{ic} \quad = \quad & \zeta_{cb} + \mu_b + \lambda_t + X_i^{'}\delta + \theta R_{cb} + \delta_1 \times 1\{\widetilde{b}_i > 0\} + \gamma_1 R_{cb} \times 1\{\widetilde{b}_i > 0\} + \delta_2 \times 1\{b_i - C_7 > 0\} \\ & + \gamma_2 R_{cb} \times 1\{b_i - C_7 > 0\} + g(b_i) + v_i \end{aligned} \tag{2}$$

where $\widetilde{b}_i$ is a centered running variable. The parameters of interest in equation (2) are δ_n and γ_n, 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 to the 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 enable them to start early, so its effect is expected to differ with respect to the one in the last cutoff.[34] As previous studies have shown, we expect $\delta_1(\delta_1 + \delta_2) < 0$ for the variable $EarlyEntry_i$.

leading bias term to conduct statistical inference. There are several ways to address this difficulty. We follow the undersmoothing 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.

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

[34] 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 the July. This seems

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 in the day of birth, $g(b_i)$,[35] we deal with the possibility that students born at different dates differ in a systematic manner. Buckles and Hungerman (2013) show, for the United States, 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. Nevertheless, even if the mother's characteristics were correlated simultaneously with birthday and child's educational outcomes, it does not invalidate our RD approach. What it is required is that the effect of these observed and unobserved factors do not change discontinuously in the mentioned cutoffs.[36,37]

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, a first group of outcomes attempts to characterize the school where a student is enrolled in first grade of elementary school. The first (second) dummy variable takes a value one in the case of a student enrolled in 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

natural given the fact that birthdays just after the first deadlines do not stop students looking for a school that enables them to start elementary school the year that they turn six years of age. We explored if the impact of school set differs across cutoffs but we did not find a significant difference in the variable $EarlyEntry_i$. In order 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^{st} cutoff.

[35] Given a small interval around the discontinuity and a parametrization of $g(.)$, the estimated function can be seen as non-parametric approximation of the true relationship between a given outcome and the variable day of birth, that is, we face a lower concern that the estimated impacts are driven by an incorrect specification of $g(.)$.

[36] In a context of "intrinsic heterogeneity" (Heckman, Urzua, and Vytlacil, 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).

[37] Since the last cut-off (July 1^{st}) is associated to a 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."

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. Administrative records contain individual information for the whole population of students, with individual's identification that allows each student to be tracked over her/his whole school life. By using students' records we are able to build seven variables that describe the students' progression over their school life. The first 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. 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 public schools.[38] The next two outcomes in this group correspond to the standardized math and language test scores from the SIMCE examination. 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 delay or not school entry). *Took the PSU* indicates whether or not the student takes the national college examination (PSU)[39] 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 colleges in the system over the period under analysis.[40]

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

[38] 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.

[39] Prueba de Selección Universitaria.

[40] See footnote 22 on page 6.

approximately 78% of these students not delaying school entry. 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 student's birthday is reported in the online appendix. [41]

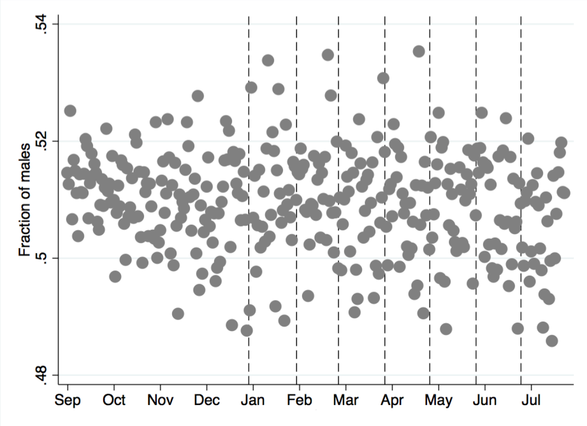
4 Validity of the RD design

Continuity in predetermined variables

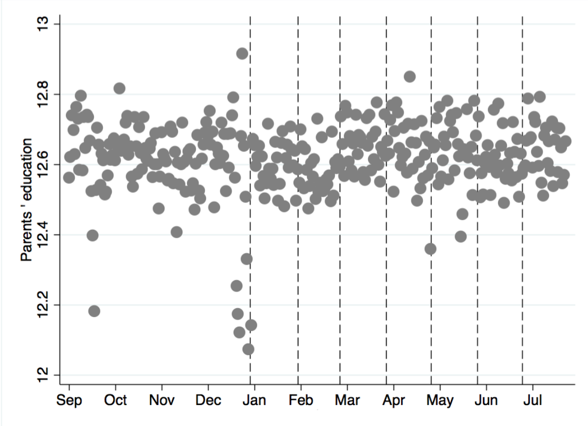
Our analysis using an RD design builds on the fact that changes in school eligibility and changes in the available set of schools around the cutoffs can be seen as good as a randomized assignment for those students with a birthday near these dates. Then, as in any random assignment, those predetermined characteristics at the time of the randomization should be similar between treated (students with a birthday just after one of the four 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 5 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

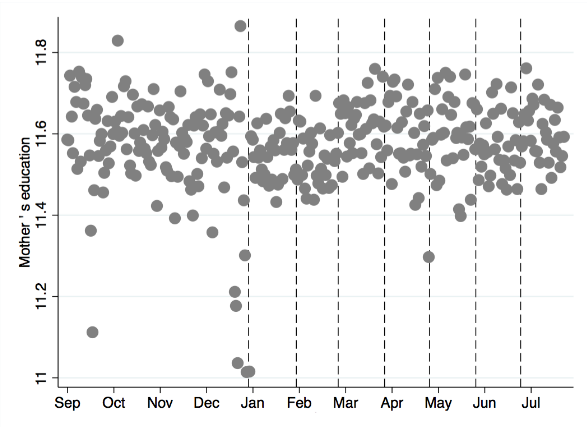
[41] For each outcome, we fit a flexible second degree polynomial at every side of the four discontinuities. Two elements are noticeable from the figures: first the existence of a series of discontinuities around the cutoff, and secondly, a heterogeneous jump around them. In fact, for all the outcomes,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



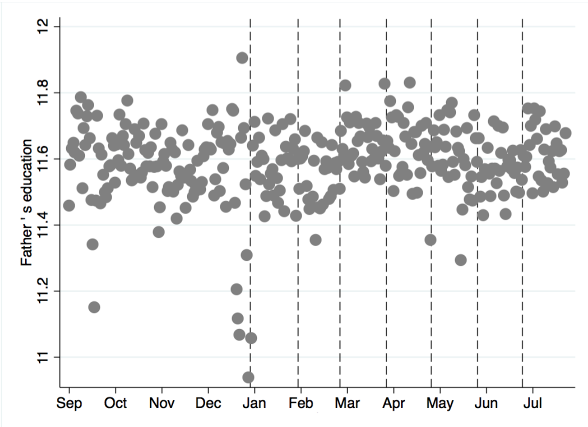
(a) Fraction of males



(b) Parents' education



(c) Mother's education



(d) Father's education

Figure 5: Balancing covariates

1^{st} , May 1^{st} and September 18^{th}. These three dates correspond to three major holidays[42] 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. First, we formally test for discontinuities in this baseline characteristics using different polynomial specifications and five bandwidths (3, 5, 7, 10 and 15 days). The p-values are reported in the first two columns of Table 3.[43] For only two (three) out of 28 specifications the null is rejected at a significance level of 5% (10%). Nevertheless, in order 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

[42] Christmas-New Year's, Independence Day and Labor Day, respectively

[43] 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 is 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.

results including all the cutoffs in the Appendix[44].

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 are able to *precisely* manipulate the running variable, day of birth, (Lee and Lemieux, 2010). In fact, since minimum-age entry rules are of public knowledge, we could expect that benefits/costs associated to a delay in the age of entry might induce some families to choose the season of birth. 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. Buckles and Hungerman (2013) show that the season of birth is correlated with some mother's characteristics in the United States. 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).

Figure 6 presents the raw histogram for the day of birth for all individuals born from December 15th to July 6th. Despite the high volatility, the figure hides a quite uniform distribution of birth during the calendar year but with a high dispersion across week days 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 week, holidays, and year of birth fixed effects, as Figure 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[45]. Finally, following McCrary (2008), we formally test for a discontinuity in the distribution of the running variable by running a regression that has as dependent variable

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

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

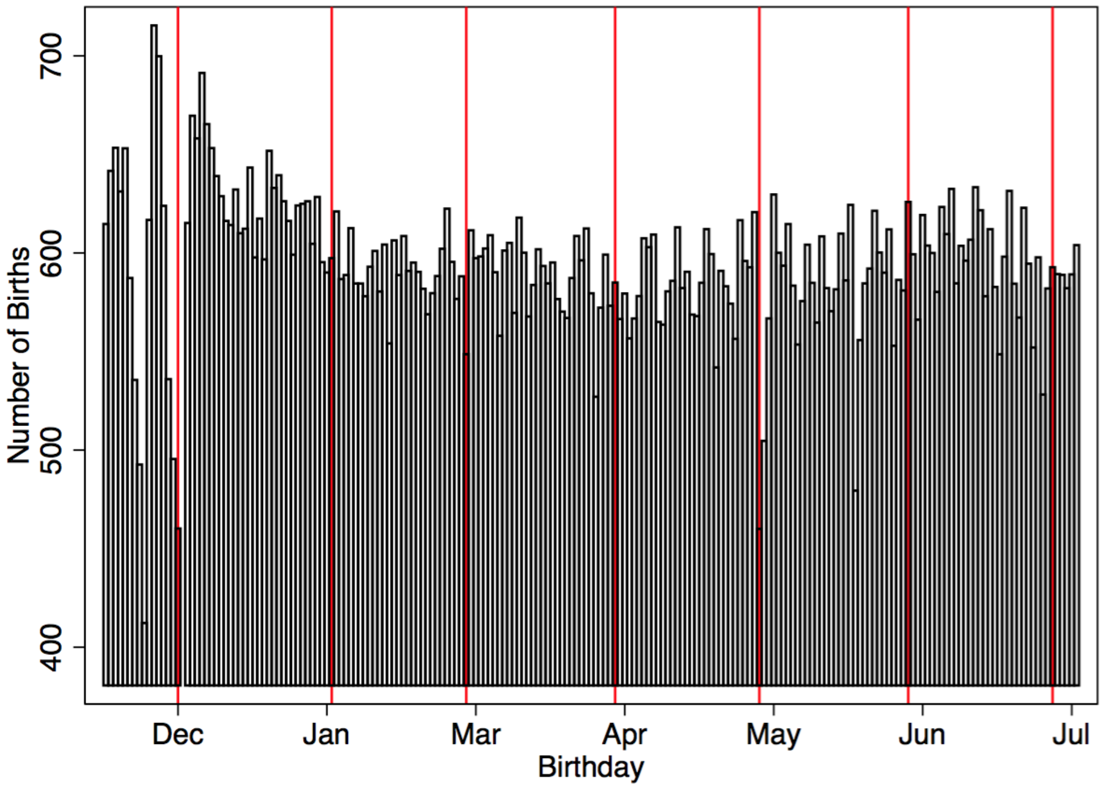


Figure 6: Day of Birth. Raw Histogram.

the frequency of birthdays over the calendar year, as we did with the previous three pre-determined variables. The p-values are reported in the third column of Table 3. Only for one specification (cubic polynomial) and bandwidth (five days), are we able to 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 in order to be age eligible. This variation could 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 for the case of Chile that families change municipality when selecting schools.

Using administrative records, Figure 8 presents the evolution of three variables over school life: fraction of students changing schools, fraction of students changing municipality of residence, and

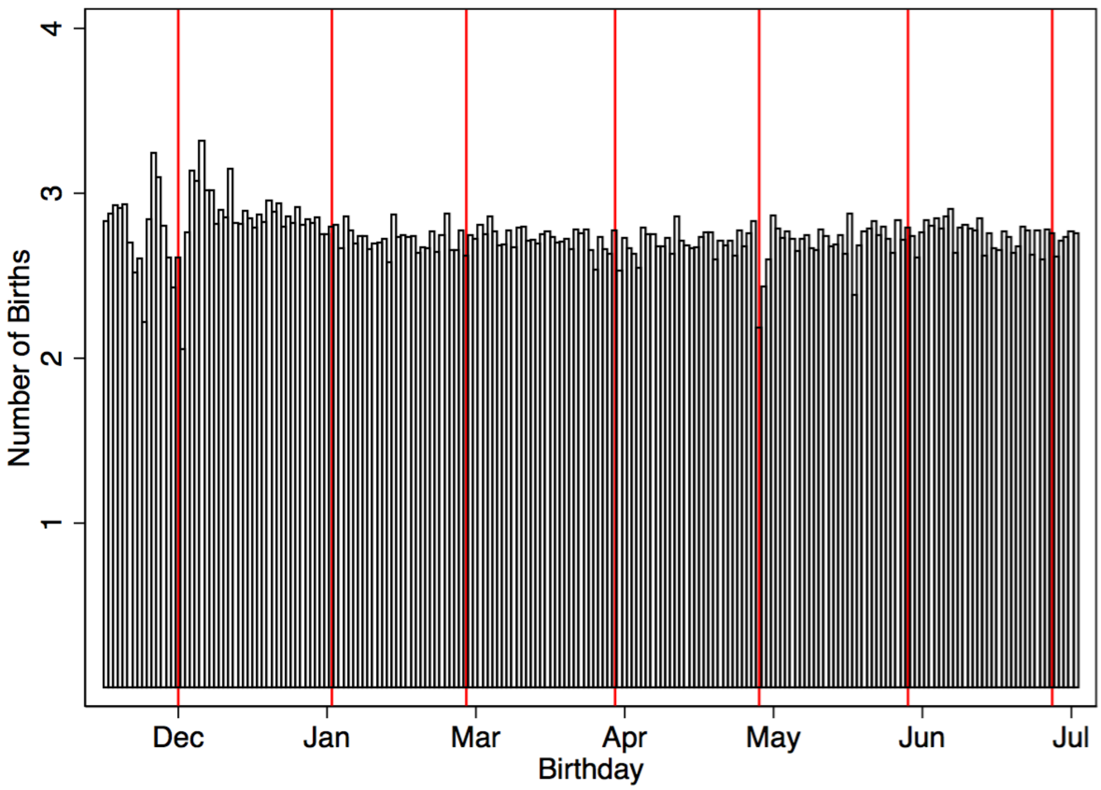


Figure 7: Day of Birth. Conditional Histogram

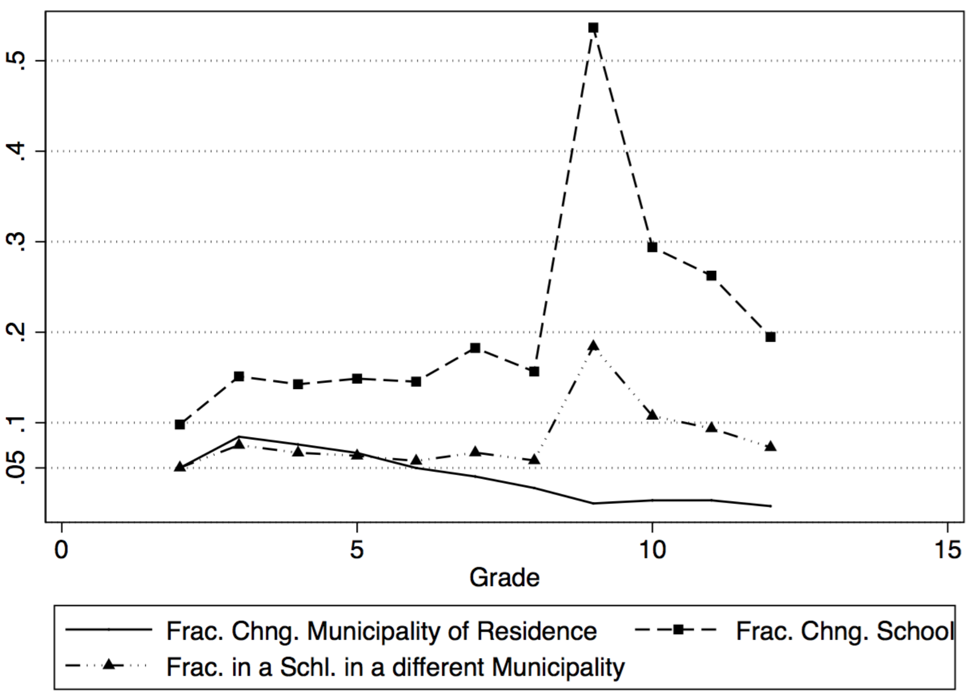


Figure 8: Comparative Evolution of Municipality vs School change by grade

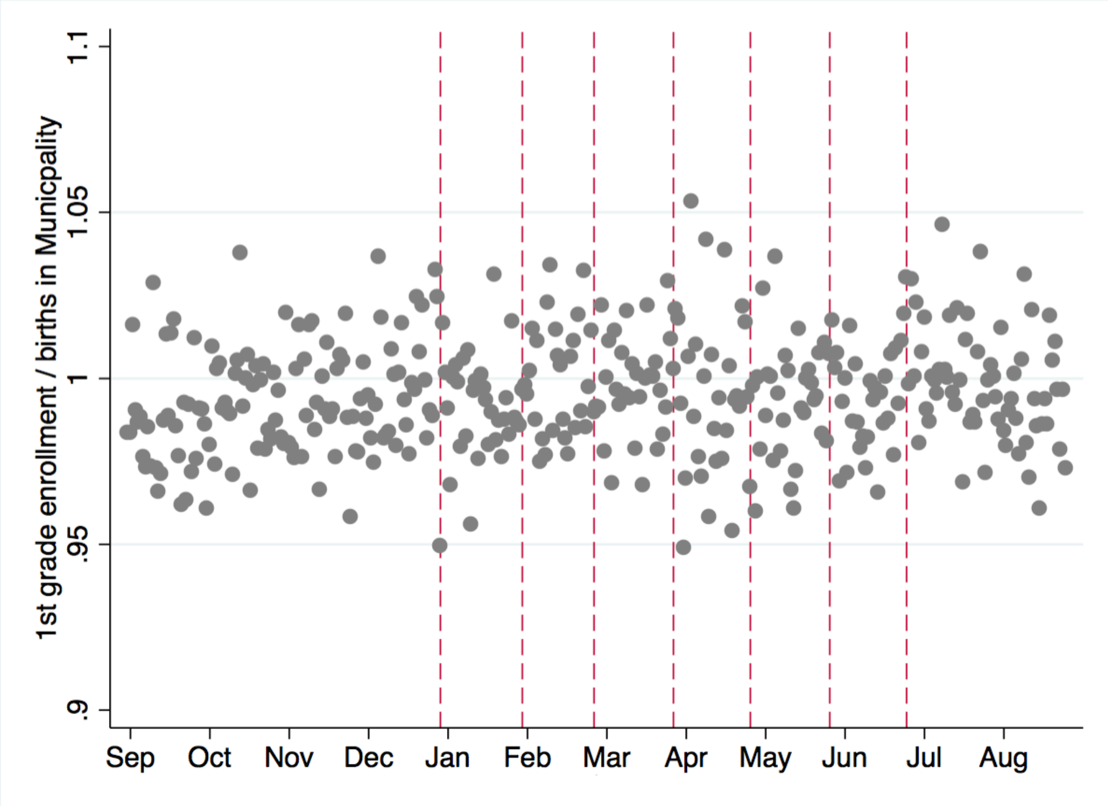


Figure 9: Ratio 1^{st} grade enrollment/Births in the municipality, by day of birth

fraction of students moving to a school in a different municipality. First, the fraction of students switching municipality of residence every year is on average less than 5 percent every year, and it falls over the school life. On the other hand, the fraction of students switching school over the school life is on average 20 percent, and it increases over time. In fact, a large jump is observed in grade 9 (first year of secondary school) with more than 50 percent of the students switching schools. Despite this major 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. In fact, the data suggests that behind this switching of school is accompanied by before a change in the fraction of students looking for a school outside the municipality rather than deciding to move to another municipality. [46]

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

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

using data from birth records. Specifically, for every cell defined by day of birth and municipality, we construct 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 bandwidth are we able to reject the null at a significance level of 5%.

5 Results

First stage

The evidence in Section 4 supports a first requirement in our RD design 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 are able 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 Figures 1 and 3 we have already shown that, in fact, the age cutoffs are associated to 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 associated to a higher school choice set) in Tables 4 to 6, 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}). [47]

Table 4 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 qualitatively robustness of the estimates when including additional variables in the

[47] We also tried a specification where the impact differs across all the cutoffs. In order to avoid the inclusion of weak instruments, we opted for this more parsimonious specification (see footnote 34).

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 45 percentage points for individuals around the last cutoff. That is, while students who lose eligibility for schools with a 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 1^{st}or later) are forced to start school the following year.[48] 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 33 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 point higher; and for individuals with a birthday just after the 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 students (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. [49]

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

[48]Note in Figure 3, 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.

[49]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}.

the null of all instruments being insignificant (F all instruments), just the one associated to the age cutoffs (F just cutoffs), and the one associated to the interaction of the cutoffs with the slots per students (F others). Although it is not a formal test for underidentification, 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. [50]

Table 5 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 6 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 RD 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 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.[51]

The estimates of Equation 1 on school characteristics are shown in Tables 7 (complete sample)

[50] 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.

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

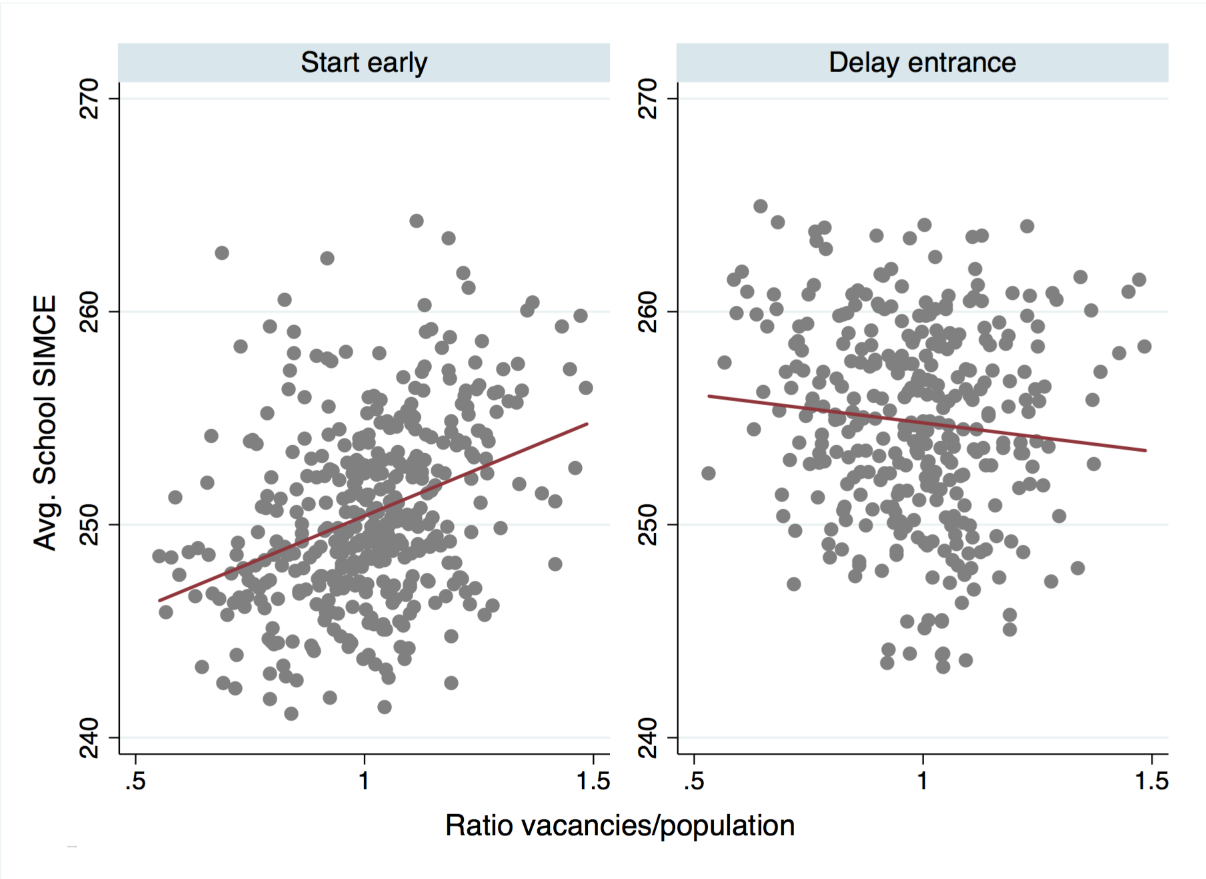


Figure 10: Average school's SIMCE score and ratio vacancies/population, by age of entry.

and 8 (by parents' level of education). Specifically, Panel A of Table 7, shows the OLS estimates for the impact of early entry, and the potential slots per students.[52] Panel B and C show the RD estimates using the point estimate optimal and the inference optimal bandwidths, respectively. Finally Panel D presents the under-identification and weak instruments test associated to 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 January/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). Focusing on the RD 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 to an approximately 0.02σ increase in school's SIMCE scores and an approximately 0.09 years more in average parent's education (0.8% in terms of the sample mean). The only result that may seem counterintuitive is in the case of class size, where we observe a positive impact of slots per students in class size. This finding is jointly consistent with parents/schools prioritizing other school inputs over class size.

Table 8 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. [53] For this group of outcomes, results are similar for both groups of parents.

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

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

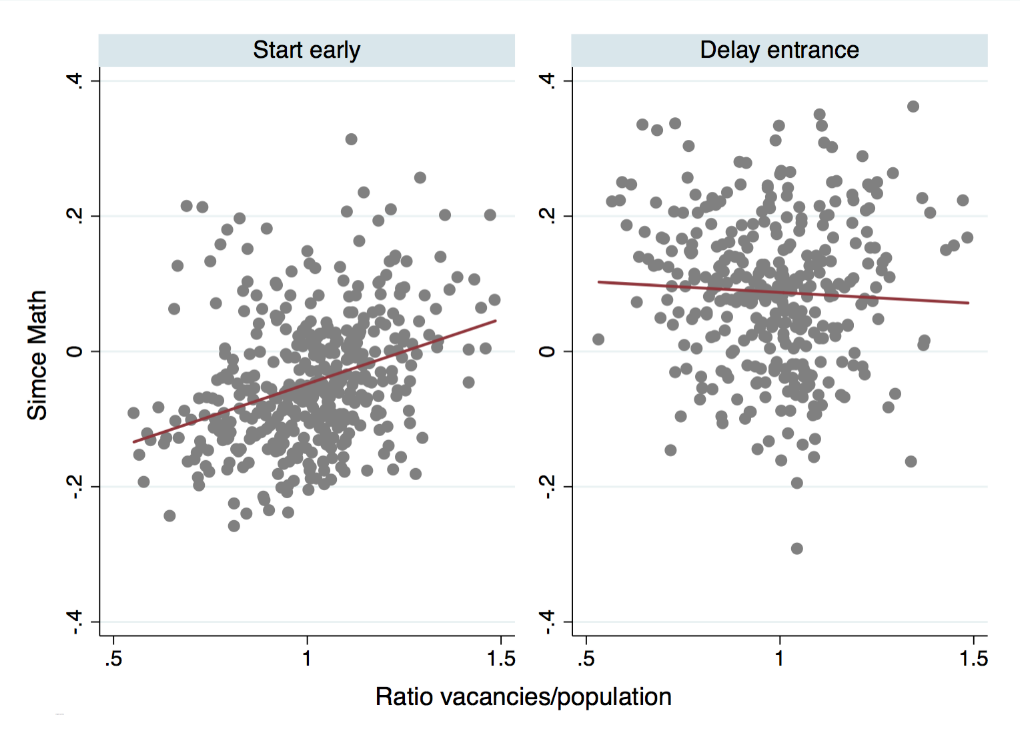


Figure 11: Individual SIMCE score (Math) vs. ratio vacancies/population

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.[54] 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 9 presents the estimates of equation 1 for the second group of outcomes and Table 10 shows the results by parents' education.

Panel A of Table 9 shows that OLS estimates suggest a negative effect associated to 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

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

all the selected outcomes. Looking at the RD point estimates (Panels B and C), notice that, with the exception of the outcome of "dropout" and the one associated to the probability of taking the PSU examination the year of high school graduation, the RD point estimates tend to be lower than those from OLS. [55] 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 more slots per student being associated with a decrease of approximately 4 percentage points. If 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.

Our RD 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 elementary school (or a little more than 10% reduction in terms of the sample mean).

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 0.06σ in the math SIMCE.

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 most prestigious Chilean universities. 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 PSU (or 3% in terms of sample mean) and approximately an increase in one percentage point of the probability of being enrolled in one of these colleges.

Table 10 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

[55] 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.

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 of a 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%.

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 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 to 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 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. Secondly, 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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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.

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

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.

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
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
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

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 sudents 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.

Table 4: 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	-0.0385***	-0.0394***	-0.0497***	-0.0525***	-0.0192***	-0.0167***	-0.0425***	-0.0411***
	[0.0129]	[0.0114]	[0.0097]	[0.0104]	[0.0071]	[0.0059]	[0.0069]	[0.0067]
Bday A. Cutoff*July 1st	-0.6492***	0.0909**	-0.3927***	0.3134***	-0.6597***	0.0834**	-0.4067***	0.3060***
	[0.0725]	[0.0385]	[0.0543]	[0.0345]	[0.0713]	[0.0349]	[0.0459]	[0.0244]
Ratio*After cutoff	0.0637***	0.0623***	0.0156*	0.0208**	0.0395***	0.0362***	0.0130**	0.0134**
	[0.0130]	[0.0120]	[0.0088]	[0.0099]	[0.0069]	[0.0058]	[0.0051]	[0.0059]
Ratio*After cutoff*July 1st	-0.1998***	-0.9584***	-0.1995***	-0.9268***	-0.1891***	-0.9500***	-0.1896***	-0.9184***
	[0.0714]	[0.0397]	[0.0506]	[0.0193]	[0.0701]	[0.0361]	[0.0486]	[0.0191]
Ratio	0.1394**	0.9113***	0.3294***	1.0015***	0.1517**	0.9262***	0.3321***	1.0022***
	[0.0648]	[0.0335]	[0.0491]	[0.0400]	[0.0656]	[0.0348]	[0.0491]	[0.0374]
F all instruments	3390.63	5158.59	249.78	819.95	3183.96	5654.27	573.53	964.44
F Just cutoffs	75.52	6.17	58.42	41.41	90.5	5.37	67.39	78.56
F Other	11.96	355.62	8	1225.04	17.6	351.44	9.3	1218.92
Controls			X	X			X	X
Degree Pol.	1	1	1	1	1	1	1	1
Obs	1,642,766	1,642,766	1,642,766	1,642,766	3,125,631	3,125,631	3,125,631	3,125,631

*** 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 5: 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)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE
	[1]	[2]	[3]	[4]	[5]	[6]
Birthday After Cutoff	-0.0343**	-0.0407***	-0.0364***	-0.0368***	-0.0428***	-0.0471***
	[0.0134]	[0.0136]	[0.0075]	[0.0079]	[0.0104]	[0.0105]
Bday A. Cutoff*July 1st	-0.5269***	0.2002**	-0.5008***	0.2507***	-0.5750***	0.2068***
	[0.0817]	[0.0800]	[0.0574]	[0.0449]	[0.0740]	[0.0738]
Ratio*After cutoff	0.0105	0.0178*	0.0081	0.0104*	0.0069	0.0099
	[0.0083]	[0.0096]	[0.0051]	[0.0060]	[0.0050]	[0.0060]
Ratio*After cutoff*July 1st	-0.1516***	-0.8993***	-0.1518***	-0.8971***	-0.1187***	-0.8795***
	[0.0473]	[0.0195]	[0.0458]	[0.0188]	[0.0431]	[0.0195]
Ratio	0.3775***	1.0291***	0.3592***	1.0166***	0.3869***	1.0315***
	[0.0498]	[0.0527]	[0.0495]	[0.0441]	[0.0500]	[0.0508]
F all instruments	52.34	638.97	170.89	815.7	62.23	679.87
F Just cutoffs	35.21	5.45	70.01	19.49	53.31	11.48
F Other	5.24	1141.11	5.97	1216.6	4.13	1089.82
Controls	X	X	X	X	X	X
Degree Pol.	2	2	2	2	3	3
Obs	1,642,766	1,642,766	3,125,631	3,125,631	3,125,631	3,125,631

*** 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. Heterogeneity by parents' educational level.

Bandwidth/Variables	6 days; 12 or less yrs education				6 days; more than 12 yrs education			
	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cs}*EE	Early Entry (EE)	R_{cb}*EE	Early Entry (EE)	R_{cb}*EE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Birthday After Cutoff	-0.0401***	-0.0378***	-0.0395***	-0.0383***	-0.0401***	-0.0378***	-0.0325***	-0.0335***
	[0.0086]	[0.0075]	[0.0097]	[0.0088]	[0.0086]	[0.0075]	[0.0100]	[0.0112]
Bday A. Cutoff*July 1st	-0.4407***	0.2887***	-0.5139***	0.2503***	-0.4407***	0.2887***	-0.4644***	0.2724***
	[0.0325]	[0.0214]	[0.0462]	[0.0380]	[0.0325]	[0.0214]	[0.0735]	[0.0573]
Ratio*After cutoff	0.0081	0.0083	0.0023	0.0050	0.0081	0.0083	0.0125*	0.0137*
	[0.0068]	[0.0069]	[0.0066]	[0.0069]	[0.0068]	[0.0069]	[0.0070]	[0.0082]
Ratio*After cutoff*July 1st	-0.1743***	-0.9302***	-0.1377***	-0.9105***	-0.1743***	-0.9302***	-0.1706***	-0.8908***
	[0.0307]	[0.0136]	[0.0276]	[0.0130]	[0.0307]	[0.0136]	[0.0622]	[0.0296]
Ratio	0.2924***	1.0030***	0.3129***	1.0131***	0.2924***	1.0030***	0.3981***	1.0184***
	[0.0373]	[0.0307]	[0.0394]	[0.0358]	[0.0373]	[0.0307]	[0.0600]	[0.0620]
F all instruments	385.31	1510.79	139.28	1443.77	354.52	389.7	79.93	328.6
F Just cutoffs	130.02	90.92	98.08	24.88	38.74	61.62	36.17	11.66
F Other	16.88	2354.29	12.59	2452.74	5.96	534.13	4.03	583.93
Controls	X	X	X	X	X	X	X	X
Degree Pol.	1	1	2	2	1	1	2	2
Obs	1,657,854	1,657,854	1,657,854	1,657,854	1,467,777	1,467,777	1,467,777	1,467,777

*** 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: 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)	0.0147	-2.1113***	-0.7393***	-0.3192***
		[0.0228]	[0.4230]	[0.0862]	[0.0464]
	EE*Ratio	0.0133	1.5963***	0.3019***	0.1413***
		[0.0203]	[0.4109]	[0.0782]	[0.0409]
	Ratio	-0.0149	-0.1628	0.0725*	0.0337**
		[0.0092]	[0.1571]	[0.0380]	[0.0166]
	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 Underidentification (p-values)				
		0.000	0.000	0.000	0.000
	Weak identification test				
	Kleibergen-Paap rk Wald F statistic				
		539.505	594.018	341.121	451.553

*** 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 8: 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
	[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.

Table 9: Impact of early entry and school choice set. School performance.

		SIMCE Math	SIMCE Language	Dropout	Move schl. during elementary	Took PSU yr HS graduation	Ever Enrolled
A	OLS						
	Early entry (EE)	-0.3916***	-0.3904***	0.0070	0.0481***	-0.0009	-0.0504***
		[0.0230]	[0.0198]	[0.0055]	[0.0055]	[0.0111]	[0.0151]
	EE*Ratio	0.1188***	0.1182***	-0.0161***	-0.0194***	0.0515***	0.0426***
		[0.0224]	[0.0201]	[0.0050]	[0.0057]	[0.0112]	[0.0138]
	Ratio	0.0630***	0.0569***	0.0172***	0.0007	-0.0114	0.0035
		[0.0125]	[0.0126]	[0.0031]	[0.0030]	[0.0074]	[0.0075]
	RD estimates			MSE-optimal bandwidth			
B	Early entry (EE)	-0.5032***	-0.4524***	0.0583***	0.0335***	-0.0595*	-0.0275
		[0.0750]	[0.0890]	[0.0212]	[0.0075]	[0.0346]	[0.0289]
	EE*Ratio	0.0689**	0.0556	-0.0443***	-0.0146***	0.0618**	0.0319*
		[0.0351]	[0.0392]	[0.0127]	[0.0053]	[0.0242]	[0.0177]
	Ratio	0.0020	-0.0051	-0.0014	0.0062**	-0.0141	0.0014
		[0.0192]	[0.0225]	[0.0066]	[0.0030]	[0.0113]	[0.0109]
	Obs	411,032	351,521	187,895	3,368,048	180,182	180,182
				Inference valid bandwidth			
C	Early entry (EE)	-0.5673***	-0.4650***	0.0583***	0.0384***	-0.0115	-0.0001
		[0.0982]	[0.0940]	[0.0212]	[0.0084]	[0.0381]	[0.0404]
	EE*Ratio	0.0876*	0.0593	-0.0443***	-0.0157***	0.0232	0.0111
		[0.0457]	[0.0445]	[0.0127]	[0.0058]	[0.0236]	[0.0234]
	Ratio	-0.0032	0.0036	-0.0014	0.0067**	-0.0034	0.0059
		[0.0216]	[0.0230]	[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 Underidentification (p-values)						
		0.000	0.000	0.000	0.000	0.000	0.000
	Weak identification test						
	Kleibergen-Paap rk Wald F statistic						
		236.599	199.358	251.646	544.231	294.298	294.298

*** 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 10: Impact of early entry and school choice. School performance by parent's education.

	SIMCE Math	SIMCE 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)	-0.5490***	-0.5358***	0.0634*	0.0416***	-0.0932**	-0.0846***
	[0.0867]	[0.0923]	[0.0342]	[0.0096]	[0.0426]	[0.0292]
EE*Ratio	0.0939**	0.1154**	-0.0512***	-0.0238***	0.0837***	0.0612***
	[0.0465]	[0.0503]	[0.0193]	[0.0067]	[0.0308]	[0.0189]
Ratio	-0.0302	-0.0261	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)	-0.4555***	-0.3641***	0.0367*	0.0280**	-0.0187	0.0468
	[0.1343]	[0.1328]	[0.0217]	[0.0118]	[0.0523]	[0.0538]
EE*Ratio	0.0213	-0.0347	-0.0265**	-0.0081	0.0335	-0.0059
	[0.0487]	[0.0486]	[0.0119]	[0.0073]	[0.0317]	[0.0291]
Ratio	0.0252	0.0131	-0.0035	0.0072*	-0.0059	0.0126
	[0.0251]	[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**					
	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.