DISCUSSION PAPER SERIES

IZA DP No. 16375

School Starting Age and the Impact on School Admission

Julio Cáceres-Delpiano
Eugenio Giolito

AUGUST 2023

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16375

School Starting Age and the Impact on School Admission

Julio Cáceres-Delpiano
Universidad Carlos III de Madrid

Eugenio Giolito
Universidad del CEMA and IZA

AUGUST 2023

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

School Starting Age and the Impact on School Admission*

This study employs Chilean administrative data to investigate the impact of School Starting Age (SSA) on the characteristics of students' initial enrolled schools. Employing minimum age requirements and an RD-design to mitigate endogeneity concerns, we identify benefits linked to commencing school at a later age. Our findings demonstrate that children starting school at an older age enroll in institutions with higher average scores in standardized tests and interact with older peers whose parents have higher education levels. Furthermore, they display a heightened likelihood of entering schools employing academic selection methods, a greater proportion of fulltime teachers, and a larger percentage of instructors with a 4-year college degree.The analysis by level of education of the parents and gender reveals that most of our results are driven by parents with lower levels of education and girls.

JEL Classification: A21, I24, I25, I28

Keywords: Latin America, Chile, school starting age, schools' characteristics

Corresponding author:
Eugenio Giolito
Universidad del CEMA
Avenida Córdoba 374 Avenida Córdoba 637
Reconquista 775
1054 Buenos Aires
Argentina
E-mail: egiolito@ucema.edu.ar

* Cáceres-Delpiano gratefully acknowledges financial support from the Spanish Ministry of Education (Grant ECO2009-11165 and ECO2019-00419-001), the Spanish Ministry of Economy and Competitiveness (MDM 2014-0431), and the Comunidad de Madrid MadEco-CM (S2015/HUM-3444). Errors are ours.

1 Introduction

There is an extensive literature studying the relationship between the age at school entry (School Starting Age, SSA) and short, medium, and long-run outcomes. In general, this literature on SSA shows a positive short- and medium-run impact of SSA on students (Bedard and Dhuey, 2006; Datar, 2006; McEwan and Shapiro, 2006; Elder and Lubotsky 2009; Dhuey and Lipscomb, 2010; Mühlenweg and Puhani, 2010; Lubotsky and Kaestner, 2016; Attar and Cohen-Zada, 2018; Dee and Sievertsen, 2018; Caceres-Delpiano and Giolito, 2019).[1] As pointed out by Dhuey et al. (2019), these previous results imply that initial variations in maturity, often referred to as readiness, can impact human capital accumulation across an individual's life with potentially significant implications for adult outcomes. However, studies investigating the long-term effects of SSA present more mixed results across diverse dimensions (Angrist and Kruger, 1991; Cascio and Lewis, 2006; Black et al., 2011; Dobkin and Ferreira, 2010; Fredriksson and Öckert, 2014; Cook and Kang, 2016; Landersø et al., 2016; Peña, 2017)[2].

The consistency observed in short- and medium-term outcomes, coupled with the less definitive conclusions regarding adult outcomes, may suggest that the mechanisms driving the persistence of early childhood advantages may operate differently depending on the characteristics of the educational system. This study specifically focuses on exploring one potential mediating channel: the elementary school where children begin their education. More specifically, we investigate the impact of SSA on the characteristics of the school where children first enroll, using Chilean public administrative records for the national population of students. On one hand, if the initial impact of age on readiness enables children to access better schools, it is more likely for this initial effect to persist over time. On the other hand, the extent of this mechanism will be influenced by the degree to which the institutional framework allows for such sorting.

To address potential unobserved variables influencing SSA, we utilize minimum age requirement rules to introduce a discontinuous change in the likelihood of starting school at an older age. Specifically, we employ a fuzzy Regression Discontinuity (RD) approach that relies on precise birth dates. It's important to emphasize that our study focuses on an educational context characterized by a generalized voucher system, a period during which schools were permitted to actively select their

[1] Numerous studies have consistently shown that children who start school at an older age tend to achieve higher scores on standardized exams (Bedard and Dhuey, 2006; Datar, 2006; McEwan and Shapiro, 2006; Elder and Lubotsky, 2009; Lubotsky and Kaestner, 2016; Attar and Cohen-Zada, 2018). For children in primary and middle school, research findings indicate a negative effect of SSA on grade retention (Elder and Lubotsky, 2009; Caceres-Delpiano and Giolito, 2019), positive effects on health outcomes such as a reduction in the likelihood of experiencing mental health problems (Dee and Sievertsen, 2018), a decrease in the probability of being placed in special education programs (Dhuey and Lipscomb, 2010), and a diagnosis of ADD/ADHD (Elder and Lubotsky, 2009).

[2] Some studies have shown insignificant or no effects on academic outcomes (Angrist and Kruger, 1991; Cascio and Lewis, 2006; Dobkin and Ferreira, 2010;Black et al., 2011), while others found positive effects on educational attainment (Cook and Kang, 2016; Peña, 2017; Celhay and Gallegos, 2022). Recent literature also shows mixed results on wages (Black et al., 2011; Fredriksson and Öckert, 2014; Peña, 2017) and crime (Cook and Kang, 2016; Landersø et al., 2016).

students. This particular context offers the opportunity to unveil a mechanism that could also apply to other educational settings, though its impact may vary.

Our findings reveal that children starting elementary school at an older age attend schools that differ across several dimensions. First, they enroll in schools with better perceived quality, as evidenced by their average scores on standardized exams. More precisely, we find that older children are 5 percentage points more likely to enroll in schools situated in the upper quartile of the score distribution. Secondly, they have older classmates, on average, and their classmates' parents possess 0.15 more years of education. Thirdly, they are more likely to attend schools employing some form of academic selection, featuring a higher proportion of full-time teachers, and boasting a larger fraction of teachers holding a 4-year college degree. The analysis, when stratified by parents' level of education and gender, reveals that the results are driven by parents with lower levels of education and female students.

These results indicate that starting school at an older age allows families to enroll their children in schools associated with better opportunities. However, it's crucial to interpret our reduced form analysis as reflecting the combined effects resulting from both parental school choice decisions and schools' selection of students. This selection process could potentially be influenced by the enhanced maturity of children starting school at a later age.

This paper is part of an expanding body of literature that focuses on the relationship between the persistent effects of School Starting Age (SSA) and the structure of the educational system, which could enhance the benefits of starting school at an older age (Bedard and Dhuey, 2006; Mühlenweg and Puhani, 2010; Zweimüller, 2013; Schneeweis and Zweimüller, 2014; Fredriksson and Öckert, 2014; Nam, 2014; Peña, 2017; Oosterbeek et al., 2021). Although the initial readiness advantage gained from starting school older naturally diminishes over time (Elder and Lubotsky, 2009; Lubotsky and Kaestner, 2016), the level of selectivity within an educational system or the presence of an early tracking system might contribute to sustaining these early advantages. The timing of the interaction with the educational system's characteristics also seems crucial. For instance, Fredriksson and Öckert (2014) discover that SSA boosts educational attainment in Sweden; however, this effect is less pronounced among those who delay tracking.[3]

In a paper analyzing the long-run effect of relative age in Mexico, Peña (2017)[4] presents a model to demonstrate that the extent of selectivity in the educational system and the wage premium can lead to larger relative-age effects, even under less age-biased allocation mechanisms than a tracking system.

[3] Fredriksson and Öckert (2014) also discover a rise in prime-age earnings among individuals with less-educated parents. Mühlenweg and Puhani (2010), studying Germany, find that older children are more inclined to pursue an academic track. Oosterbeek et al. (2021) show that older students in the Netherlands are more prone to being assigned to college and university tracks and enrolling in universities. The authors highlight the significance of timing in the tracking process; delaying tracking provides relatively young students with more time to bridge the gap with their older peers.

[4] He finds significant effects on college attainment, employment status, earnings, having employer-provided medical insurance, college attainment of the spouse, and the number of children.

Our paper is particularly relevant to Peña (2017) as we examine the impact of SSA in an institutional context where elementary schools actively select students. Specifically, if SSA contributes to school readiness, a selective environment could result in older children being more likely to enroll in schools with better resources or interact with more capable peers. Consequently, the impact of SSA might endure if it enables children to access enhanced opportunities at an early age.

Recognizing that we are examining a specific context, our findings could offer an extra avenue to elucidate recent discoveries about the long-term impacts of SSA within the same setting (Celhay and Gallegos, 2022)[5]. Nonetheless, it's probable that in different environments, certain levels of school selectivity exist (e.g., private schools). In such instances, it's conceivable that the influence of SSA on student outcomes might also encompass the influence of school attributes.

The paper is structured as follows. Section 2 provides a brief overview of Chile's educational system. In Section 3, we outline the conceptual framework for our study, our empirical approach, the data utilized in the analysis, and the defined outcomes. Section 4 presents the primary findings. Lastly, Section 5 offers concluding remarks.

2 Institutional background

2.1 Chilean educational system

Since the early 1980s, Chile's primary and secondary educational system has experienced substantial decentralization and increased private sector involvement.[6],[7] The student population, which is around 3.5 million, is divided among three types of schools: public or municipal schools (constituting 41% of total enrollment), subsidized private schools (making up 51% of total enrollment), and unsubsidized private schools (representing 7% of total enrollment). Municipal schools are overseen by the comunas (similar to counties in the U.S.), while the private sector manages the other two types of schools.[8]

Both municipal and subsidized private schools receive state funding through a voucher scheme, with the subsidy amount varying based on each school's enrollment. Subsidized private schools are often called voucher schools. Importantly, parents are not obligated to select a school within their county of residence, although approximately 90% of parents choose to do so. This system is similar to other extensive voucher programs in countries like the Netherlands, Denmark, and Sweden, as

[5] Celhay and Gallegos (2022) find that children who begin school at a later age in Chile not only exhibit a higher likelihood of enrolling in college but also in more selective institutions. They also observe that SSA leads to an increase in parental financial investments in their children.

[6] Primary and secondary education administration was shifted to counties, resulting in the elimination of payment scales and civil servant protection for teachers. Additionally, a voucher scheme was introduced to serve as the funding mechanism for municipal schools and initially for private schools that did not charge fees. For further information, refer to Gauri and Vawda (2003).

[7] For an overview of these and other reforms implemented since the early 1980s, please refer to Contreras et al. (2005).

[8] There is also a fourth type of school called "corporations," which are vocational schools managed by firms or enterprises with a fixed budget from the state. In 2012, they accounted for less than 2% of the total enrollment. Throughout our analysis, we consider them as municipal schools.

well as some smaller-scale programs in the USA (Epple et al., 2017).

Initially, public and subsidized private schools in Chile were intended to be free of charge. However, starting in 1994, these schools were permitted to charge tuition and fees in addition to the government subsidy they received. Subsequently, Law 20.845 of 2015 required schools with tuition to gradually reduce the co-payment to zero or transition to unsubsidized private schools. Despite the majority of public schools remaining free of charge, approximately 30% of students attended voucher schools with co-payment during this period.

This paper focuses on studying the population of Chilean students who entered primary school between 2002 and 2008, a time when schools were allowed to actively select students. The law of the year 2015 explicitly prohibited voucher schools from engaging in student selection. Instead, it established a Centralized Admission System for both public and voucher schools. This system was implemented gradually by regions and was fully in place at the national level by 2020.

2.2 Minimum age requirements

In Chile, the minimum age requirement rules stipulate that children must have turned six before a specific date during the academic year in order to be enrolled in first grade at primary school.[9] Children whose birthday falls before this cutoff date are eligible to start school in the year they turn six, while those born after this date must wait until the following academic year to begin school.

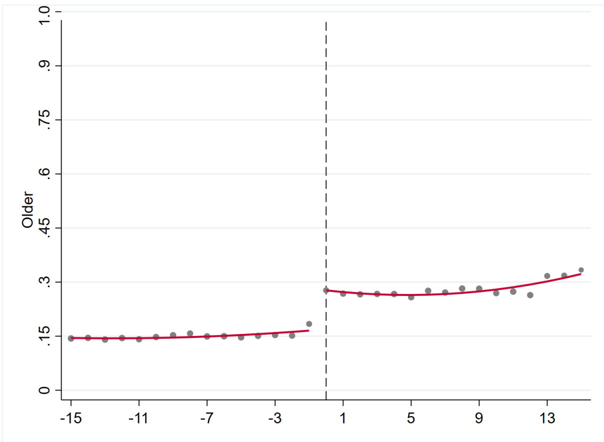
Initially, Chile's official enrollment cutoff was set on April 1^{st}. However, from 1992 until recently, the Ministry of Education allowed schools some flexibility to set other cutoff dates, but not later than July 1^{st}. Schools have distributed themselves over seven cutoff dates between January 1^{st} and July 1^{st}.[10] McEwan and Shapiro (2006) used the four most common cutoffs (April 1^{st}, May 1^{st}, June 1^{st}, and July 1^{st}) to estimate the impact of SSA on students' outcomes in Chile.

Figure 1 visually illustrates the variation induced by minimum age requirement rules. Each panel of the figure shows the fraction of students who start elementary education in the year of their seventh birthday ("Older") within a 15-day window around different age cutoffs. Panel (a) presents this fraction by stacking up all the cutoffs. Notably, approximately 15 percent of students to the left of any cutoff start school at an older age, and this fraction nearly doubles to the right of these cutoffs.

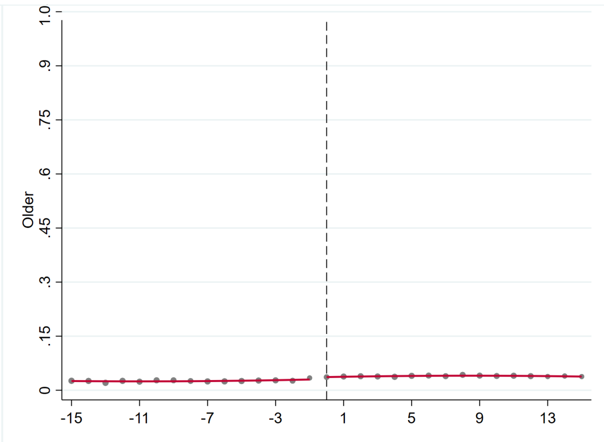
Upon closer examination in Panels (b) to (d), we observe that among students with early-year birthdays, only a small fraction are "redshirted" (i.e., they delay enrollment). Practically none of the students with birthdays before April 1^{st} are redshirted, but this fraction progressively increases to approximately 45 percent among students with a birthday in June. Two elements explain this pattern. First, students born early in the year are already older at the start of the academic year,

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

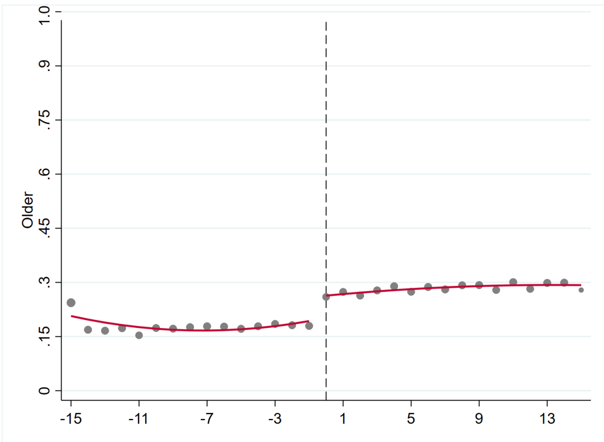
[10] See http://bcn.cl/1yw2h



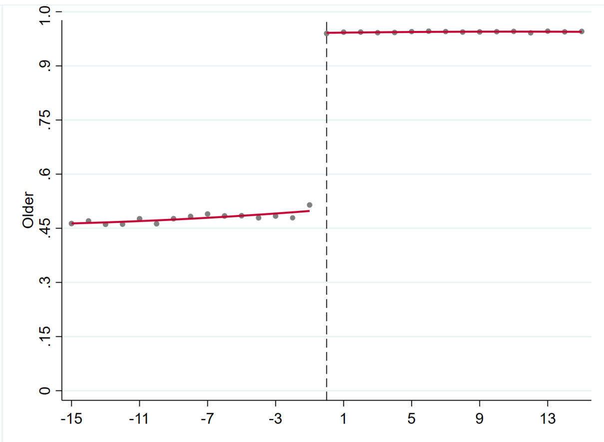
(a) All cutoffs



(b) January-March cutoffs



(c) April-June cutoffs



(d) July cutoff

Figure 1: Fraction of students starting older around different cutoffs.

making them less likely to postpone enrollment. Second, due to the Chilean institutional setting of multiple cutoffs across schools, children with birthdays closer to July 1^{st} face a larger increase in the pool of schools to choose from if they decide to wait.[11] These factors likely contribute to the heterogeneity in the discontinuity induced by minimum age rules.

Additionally, it is important to note that children born after June 30^{th} are "forced" to postpone enrollment until the next year, leading to an approximately 55 percentage point increase in the likelihood of starting school older compared to those born before July 1^{st}. The probability of waiting until the next year changes by less than 1 percentage point for the January-March cutoff (Panel (c)) and by around 10 percentage points around the April-June cutoff (Panel (d)).

[11] In a related paper (Cáceres-Delpiano and Giolito, 2023), we use the distribution of school vacancies across cutoffs and municipalities to study the effect of variations in the school choice set on educational outcomes for children who do not delay entry.

3 Methods

3.1 Conceptual framework

To illustrate the focus of our paper, we make a minor modification to the framework proposed by Todd and Wolpin (2003). Consider a three-period setting where $t = 0$ represents the time before school entry, and $t = 1$ and $t = 2$, the first and second years of school, respectively. We define A_t as children's achievement at the *beginning* of period t. Let F_t represents family investments in children's learning during period t, W be the family wealth, μ be children's innate ability, and a be their SSA. Therefore, if SSA influences children's intellectual maturity, we can define the achievement at school entry as:

$$A_1 = g_0(F_0, \mu, a).$$

Similarly, achievements at the beginning of second year depend on the history of family inputs, F_0 and F_1 and on school inputs, S_1, as well as children cognitive ability:

$$A_2 = g_1\left(S_1, F_{1,}F_0, \mu, a\right)$$

As Todd and Wolpin (2003), we distinguish between two types of school inputs: the amount of inputs chosen by the family at the time of schooling decision, denoted by $\overline{S}_1$, and the actual level of school inputs, S_1. We will refer to these inputs broadly as "school inputs," which include factors such as teachers' qualifications and potential sources of peer effects, like classmates' parental education or classmates' age. Within the context of our paper, families have the flexibility to approximate their desired school inputs ($\overline{S}_1$) by directly selecting schools in a voucher system. For the sake of simplifying notation in the analysis that follows, let's assume that family decisions regarding school inputs are solely determined by family wealth and children's innate ability (independent of A_1).

$$\overline{S}_1 = \theta\left(W, \mu\right)$$

Schools also decide how to allocate inputs to children, for example, by "tracking" based on their previous achievement. In the case of Chile during the period under consideration, schools were allowed to select students depending on their achievements at the time of school entry. Therefore,

$$S_1 = \psi\left(A_1, \mu\right)$$

Finally, the family decides on direct investments in children's learning depending on their wealth, previous children's achievement, their ability, and the deviation between actual and desired school inputs. Under the assumption that families observe these deviations before deciding on investments, we have:

$$F_1 = \phi\left(A_1, W, \mu, S_1 - \overline{S}_1\right).$$

In this context, we can examine the effect of an exogenous increase in SSA on children's achievement at the beginning of the second year, A_2. For simplicity, we assume that SSA has only a direct effect on A_1, meaning that pre-school family investments (other than school choice) are independent of SSA: $\frac{dF_0}{da} = 0$. Therefore, we have:

$$\frac{dA_2}{da} = \frac{\partial g_1}{\partial a} + \left[\frac{\partial g_1}{\partial F_1}\frac{\partial \phi}{\partial A_1} + \left(\frac{\partial g_1}{\partial S_1} + \frac{\partial g_1}{\partial F_1}\frac{\partial \phi}{\partial\left(S_1 - \overline{S}_1\right)}\right)\frac{\partial \psi}{\partial A_1}\right]\frac{\partial g_0}{\partial a}$$

The impact of SSA on students' achievement consists of a direct effect ($\frac{\partial g_1}{\partial a}$) and a series of indirect effects caused by changes in pre-school achievement ($\frac{\partial g_0}{\partial a}$):

(i) The impact of changes in school direct inputs: $\left(\frac{\partial g_1}{\partial S_1}\frac{\partial \psi}{\partial A_1}\frac{\partial g_0}{\partial a}\right)$.

(ii) The impact of family adjusting investments in reaction to changes in pre-school achievement: $\left(\frac{\partial g_1}{\partial F_1}\frac{\partial \phi}{\partial A_1}\frac{\partial g_0}{\partial a}\right)$.

(iii) The impact of changes in family investments in reaction to the deviation between actual and targeted school inputs $\left(\frac{\partial g_1}{\partial F_1}\frac{\partial \phi}{\partial\left(S_1 - \overline{S}_1\right)}\frac{\partial \psi}{\partial A_1}\frac{\partial g_0}{\partial a}\right)$.

In this paper, our main objective is to estimate the effect of SSA on school inputs, which are broadly defined and measured at the time of school entry. The reduced form parameter $\alpha = \frac{\partial \psi}{\partial A_1}\frac{\partial g_0}{\partial a}$ captures the combined impact of SSA on pre-school achievements $\left(\frac{\partial g_0}{\partial a}\right)$ and the subsequent effect of these achievements on school inputs $\left(\frac{\partial \psi}{\partial A_1}\right)$, considering the interaction between parents and schools. For simplicity, we have assumed that parents' choice of school characteristics is independent of pre-school achievement and, consequently, independent of SSA ($\frac{\partial \theta}{\partial A_1} = 0$). However, this reduced form does not allow us to distinguish between changes in school inputs resulting from parents' school choice decisions and schools selecting children based on their improved pre-school achievement due to SSA.Recent literature have shown evidence about the different channels. While some recent papers explore the immediate impact in terms of school readiness,$\frac{\partial g_1}{\partial a}$ (e.g. Dhuey et al., 2019), other studies have explored the contribution of families (Elder and Lubotsky, 2009; Landersø et al., 2020; Celhay and Gallegos, 2022).[12] Depending on the context, the papers that analyze the connection between SSA and family investments are either capturing the direct adjustment of parents to the improvement in children's skills (channel (ii)), or a combination of the direct effect and parents' response to changes in school inputs (channels (ii) and (iii) combined).

[12] For example, Elder and Lubotsky (2009) find an early impact of SSA on test scores, particularly among children from upper-income families, which they link to skill accumulation prior to kindergarten. Landersø et al. (2020) show that SSA also affects several household-level aspects, such as family structure, mother's labor participation, and the achievements of the other siblings, exerting an indirect influence on the student's educational performance. In the Chilean context, Celhay and Gallegos (2022) observe that parents of older children enhance their financial investments, while their temporal commitments remain unaffected.

3.2 Data

The primary data source for our analysis comes from public administrative records provided by the Ministry of Education of Chile for the period 2002-2008. Our analysis focuses on students born between the years 1996 and 2001. From this dataset, we obtain for each student in the population their masked identification number,[13] exact dates of birth, gender, municipality of residence, first school identifier, and type of school (public, voucher, or unsubsidized private). Additionally, we use the individual records to determine the average age of students' classmates.

Our second data source is the SIMCE standardized test records, obtained from the *Agencia de Calidad de la Educación*.[14] Firstly, we utilize the parental survey module within the SIMCE survey to determine the parents' level of education. Secondly, we aggregate scores at the school level for the year preceding each cohort's entry using individual student records, along with information about the average schooling of their classmates' parents. Finally, we gather parental opinions about the school selection process at the time of the survey through the parental survey.[15]

The third source of information comes from administrative records regarding teachers, provided by the Ministry of Education of Chile for the period 2003-2009. From this source, we acquire data on teachers' weekly total working hours and total teaching hours, which we aggregate at the school level. Additionally, we obtain information about teachers' education from the teacher evaluation database, which we also aggregate at the school level.[16]

3.3 Empirical specification

Estimating the effects of SSA poses challenges due to potential SSA manipulation, including practices like redshirting (Dhuey et al., 2019) or strategic birth timing (Buckles and Hungerman, 2013)[17]. As the determinants of school entry decisions are often unknown or unobserved by researchers, there is a risk of omitted variable bias when comparing outcomes among children who start school at different ages.

To tackle the issue of endogeneity, we employ a quasi-experimental approach by utilizing the minimum age requirement rule as source of variation. In particular, we utilize a *fuzzy* Regression Discontinuity (RD) strategy that relies on precise birth dates. To address concerns regarding weak instruments, we concentrate solely on the July cutoff, which exhibits the most significant and iden-

[13] Student identifiers, along with school identifiers, allow us to merge these records at the individual level with school characteristics and other public surveys.

[14] The national standardized test (SIMCE: *Sistema de Medición de la Calidad de la Educación*), is typically administered in 4th, 8th and 10th grades.

[15] This data can be obtained from https://informacionestadistica.agenciaeducacion.cl/#/bases

[16] Students records, teaching positions and teacher evaluation data can be obtained from https://centroestudios.mineduc.cl/datos-abiertos/

[17] Buckles and Hungerman (2013) demonstrate, in the context of the United States, that the season of birth correlates with the mother's characteristics. Specifically, they illustrate that children born in winter are more likely to have a mother with lower education, a teenage mother, or an African-American mother.

tifiable discontinuity among the seven different cutoffs in Chile.[18]

In the scenario where students are indexed by i, their year of birth by t, and their birthday over the calendar year by b, the specification used to estimate the impact of SSA on school characteristics for student i (y_{it}), can be expressed as follows:

$$y_{it}=\rho_t + X_i^{'}\psi + \alpha\, Older_i + g(b_i) + \epsilon_{it} \quad , \tag{1}$$

The variable $Older_i$ is a dummy variable that takes the value of one if a child started primary school during the academic year of their seventh birthday, and zero otherwise. Moreover, ρ_t represents the year of birth, and X_i a a vector of individual covariates.[19] Finally, $g(b_i)$ is a flexible polynomial specification in the day of birth for a student, which we allow to have a different slope at each side of the cutoff. By adding to the specification a polynomial specification in the day of birth, $g(b_i)$,[20] we deal with the possibility that students born at different dates differ in a systematic manner. We cluster the error term at the student's day of birth.

In equation (1), our primary focus is on the parameter α, which represents the reduced form impact of SSA on school characteristics. As explained in section 3.1, this parameter captures the interplay between schools, (which are allowed to select students), and parents (who may choose a better match from a larger number of schools). This interplay arises in response to the child's skills improvement due to SSA.

As stated above, the variable indicating whether or not a child starts schooling at an older age ($Older_i$) is a non-random variable. To address this endogeneity, we use the discontinuity determining SSA and estimate the following first-stage regression:

$$Older_i = \rho_t + X_i^{'}\psi + \delta \times 1\{b_i - C > 0\} + g(b_i) + v_i \tag{2}$$

Here, the parameter of interest, \delta, captures the discontinuity in the endogenous variable. The operator $1\{*\}$ serves as an indicator function, and C represents the July cutoff.

Following Calonico et al. (2014; 2017), we select two data-driven bandwidths for each of the

[18] As discussed in section 2.2, schools in Chile have implemented seven different cutoff dates, spanning from January 1^{st} to July 1^{st}. We present the results using February-July cutoffs in Appendix C.3. Our results are robust to considering children born around the other five discontinuities.We exclude children born around January 1^{st} due to potential confounding factors associated with holidays during the last week of the year.

[19] Covariates encompass fixed effects for parents' education, the student's municipality of residence, and the student's gender. We employ the highest observed education level between the father and mother, as reported in the parents' survey of the National standardized test (SIMCE), as parents' education. We create an extra category when the educational level is simultaneously missing for both mother and father. Additionally, we incorporate six dummy variables to indicate the day of the week a student was born, along with a dummy variable to distinguish whether a student was born on a national holiday.

[20] Considering a small interval around the discontinuity and a parametrization of $g(.)$, we can view the estimated function as a non-parametric approximation of the true relationship between a specific outcome and the day of birth variable. As a result, we have fewer concerns that the estimated impacts are influenced by an incorrect specification of $g(.)$.

outcomes. 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). The second bandwidth minimizes an approximation to the Coverage Error Rate (CER) of the confidence intervals, that is, the discrepancy between the empirical coverage of the confidence interval and the theoretical level. See Appendix A for details.[21]

The primary identifying assumption for our analysis is that there are no factors other than SSA changing discontinuously at cutoffs. To ensure the plausibility of that assumption, we perform several robustness checks.[22] In Appendix B.1 we show that the individual covariates (gender, parents' education and municipality of residence) behave smoothly at the cutoff. Additionally, in Appendix B.2, we present both visual and formal evidence, following McCrary (2008), that there is no manipulation based on birth dates around the cutoff.

In the context of "essential heterogeneity" (Heckman et al., 2006), the estimated parameters can be interpreted as weighted "Local Average Treatment Effects" (LATE)[23] across all individuals (Lee and Lemieux, 2010). However, for this interpretation to hold, it is crucial to ensure that the monotonicity assumption is not violated. In the context of our paper, the monotonicity assumption implies that a change in the instrument's value (in our case, being born after the July cutoff) should increase the school starting age (SSA) for all students or, at the very least, not decrease it. Although we cannot directly test this assumption, we provide some arguments to mitigate concerns below and discussed them in more detail in Appendix B.3.

The need for the monotonicity assumption arises due to the presence of heterogeneity, combined with students strategically choosing their SSA based on expected outcomes ("sorting on gains"). To gauge the extent of this heterogeneity, we include covariates that can reasonably serve as indicators for income, such as parental education, and specifically within the Chilean context, the municipality of residence. If violations of monotonicity were a substantial concern in our analysis, we would expect the estimates to show sensitivity when covariates like these are included (Dhuey et al., 2019). Nevertheless, our estimates are generally insensitive to the incorporation of these covariates (see Appendix C.2). This observation suggests that potential breaches of monotonicity are unlikely to significantly affect our results.

If the violation of monotonicity posed a notable empirical concern in our analysis, we would anticipate the estimates to display sensitivity when covariates like these are incorporated (Dhuey

[21] In section 4 we present results using MSE method. Results using the CER Method are in Appendix C.1.

[22] Hahn et al. (2001) show that the estimation of causal effects in this regression discontinuity framework is numerically equivalent to an instrumental variable (IV) approach within a small interval around the discontinuity. By focusing on observations around the discontinuity, we concentrate on those observations where we can consider the treatment (SSA) as good 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 the discontinuity.

[23] In the context of this paper, the treatment is defined as starting school at an older age, with students compelled to begin the year of their seventh birthday if born after June 30th. The estimated Local Average Treatment Effect (LATE) for α corresponds to the Average Treatment Effect on the Untreated (ATU) (Angrist and Pischke, 2008).

et al., 2019). Nonetheless, our estimates are in general insensitive to the inclusion of these covariates (see Appendix C.2). This observation suggests that potential violations of monotonicity are unlikely to significantly influence our results.

However, we acknowledge that we cannot rule out the presence of heterogeneity and sorting on gains. Therefore, we will now examine the plausibility of the monotonicity assumption in relation to our paper, assuming independence between the instrument and the potential treatment assignment.

When heterogeneity and sorting on gains are present, as highlighted by Barua and Lang (2016), a violation of monotonicity can occur when there are "defiers" - children who start school early only when they are not allowed to do so. However, in our working sample, we find that approximately half of the June-born children are redshirted, while hardly any July-born children start school early.[24] This observation effectively rules out the biggest concerns regarding defiers.

The primary risk of potential monotonicity violation in our sample arises from redshirted June-born children, who would have faced a slight decrease in their SSA if they were born after July (simply because they would start school at the same time but be a few days younger). Nonetheless, our confidence is boosted by the availability of precise date of birth information, allowing us to include a daily time trend to mitigate these concerns. This daily time trend enables us to estimate the treatment effect at a singular point (or infinitely small approximation) of the cutoff (Dhuey et al., 2019). While we cannot definitively rule out a violation of monotonicity, following Fiorini and Stevens (2021), we can infer that the parameter we are estimating in our fuzzy RD design closely approximates the Local Average Treatment Effect (LATE) of interest.

3.4 Outcomes

We utilize the data described in Section 3.2 to construct variables related to school inputs and peer characteristics at the school when students enroll in first grade of elementary school. These variables help us characterize schools by their average score in the national standardized test, by their degree of selectivity as reported by parents, and certain direct observable measures such as parental schooling and teachers' characteristics.

To assess school "quality," we use the average SIMCE score of each school, measured a year before students enroll. We create three categories to show if a school's score is higher than the 25th, 50th, or 75th percentile. Additionally, we include a dummy variable indicating if the school is a subsidized private, which are addressed as *Voucher* school.

Even though test scores are not a perfect way to measure school quality because they can be influenced by other factors (Kramarz et al., 2008), in Chile, the average SIMCE score is frequently used to provide families insights into their school choices.(Mizala and Urquiola, 2013). Moreover,

[24]See Table B.5 in Appendix B.3.

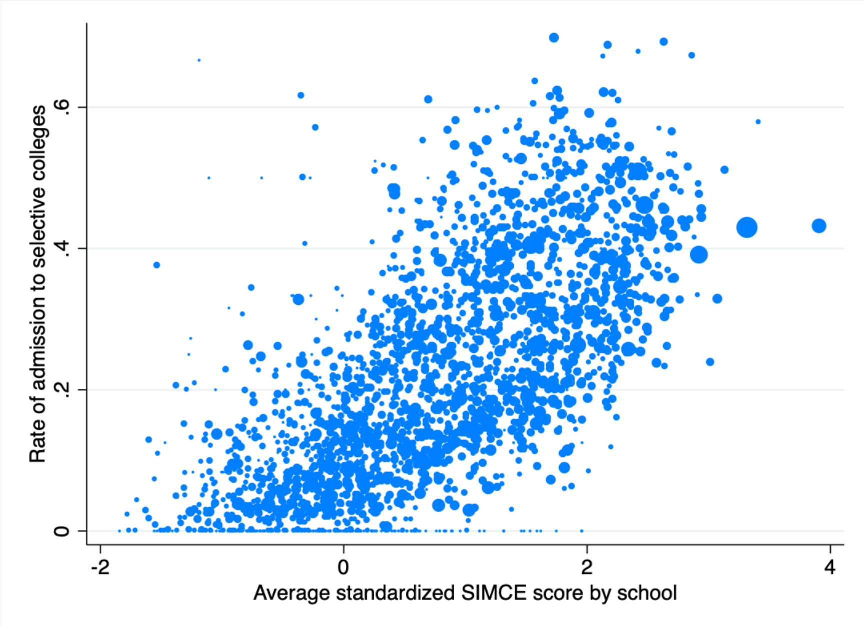


Figure 2: Average standardized SIMCE school and probability of admission to selective colleges

Source: Departamento de Evaluación, Medición y Registro Educacional (DEMRE) and Agencia de Calidad de Educación

school average scores are considered a good predictor of the probability of college admission. As shown in Figure 2, there is a strong correlation between the school average score and the probability of admission to selective universities in Chile.[25]

Until recent reforms, the school selection process typically involved active involvement from both families and schools. To gain a clearer understanding of how school choice worked, we use data from the parents' survey of the SIMCE standardized test. From this data, we create a variable that shows the percentage of parents who reported that their child went through a selection process mainly based on academic factors. We consider academic selection as situations where students take exams, participate in play sessions, or schools ask for past academic records (like preschool attendance).[26]

A second group of variables pertains directly to visible school resources, with a specific focus on teachers at the school level. These variables include the average age of teachers, the percentage of full-time teachers, the ratio of teaching hours to total hours, and the share of teachers possessing a four-year college degree.[27]

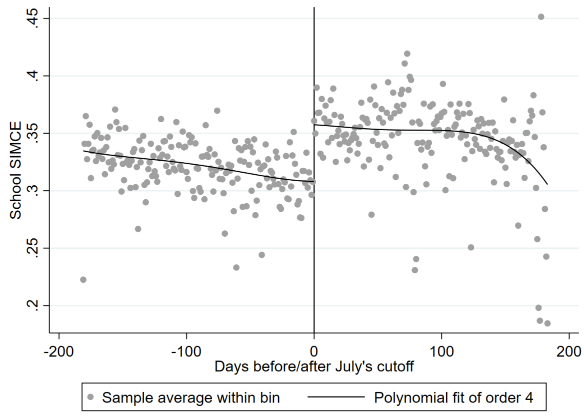
The final two variables in this second group, aim to characterize the students' classmates. These variables represent the average education level of parents among the classmates and the average age of the classmates themselves. The importance of the latter variable has been highlighted by recent research that examines how the age of peers can influence learning. (Cascio and Whitmore Schanzen-

[25] Those belonging to the *Consejo de Rectores de Universidades de Chile* (CRUCh). Celhay and Gallegos (2022) find that students born after the cutoff are more likely to enter in those universities.

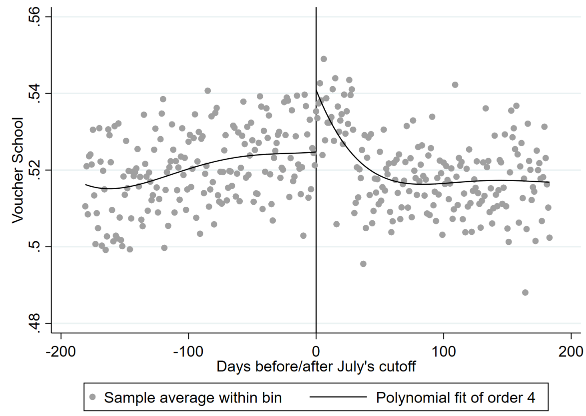
[26] The survey allows parents to choose multiple selection methods.

[27] Until 2015, a four-year college degree was not a prerequisite for elementary school teachers.

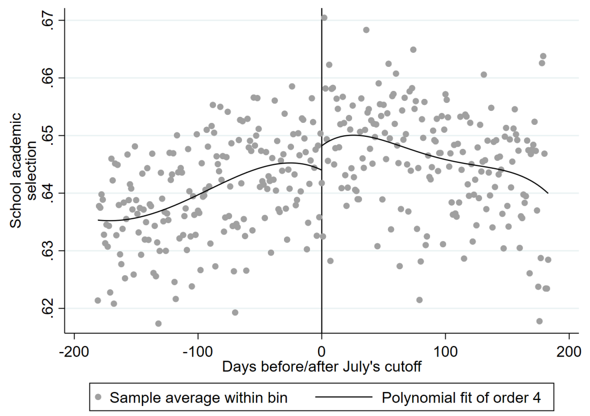
bach, 2016; Peña, 2017).[28]



(a) School SIMCE.



(b) Voucher school



(c) School academic selection

Figure 3: Evolution of selected outcomes according to the student's birthday.

Furthermore, in an educational system where schools have the authority to choose students, those who begin school at an older age might find themselves among older classmates. This situation could indicate that these students are enrolling in schools that consistently admit older students.

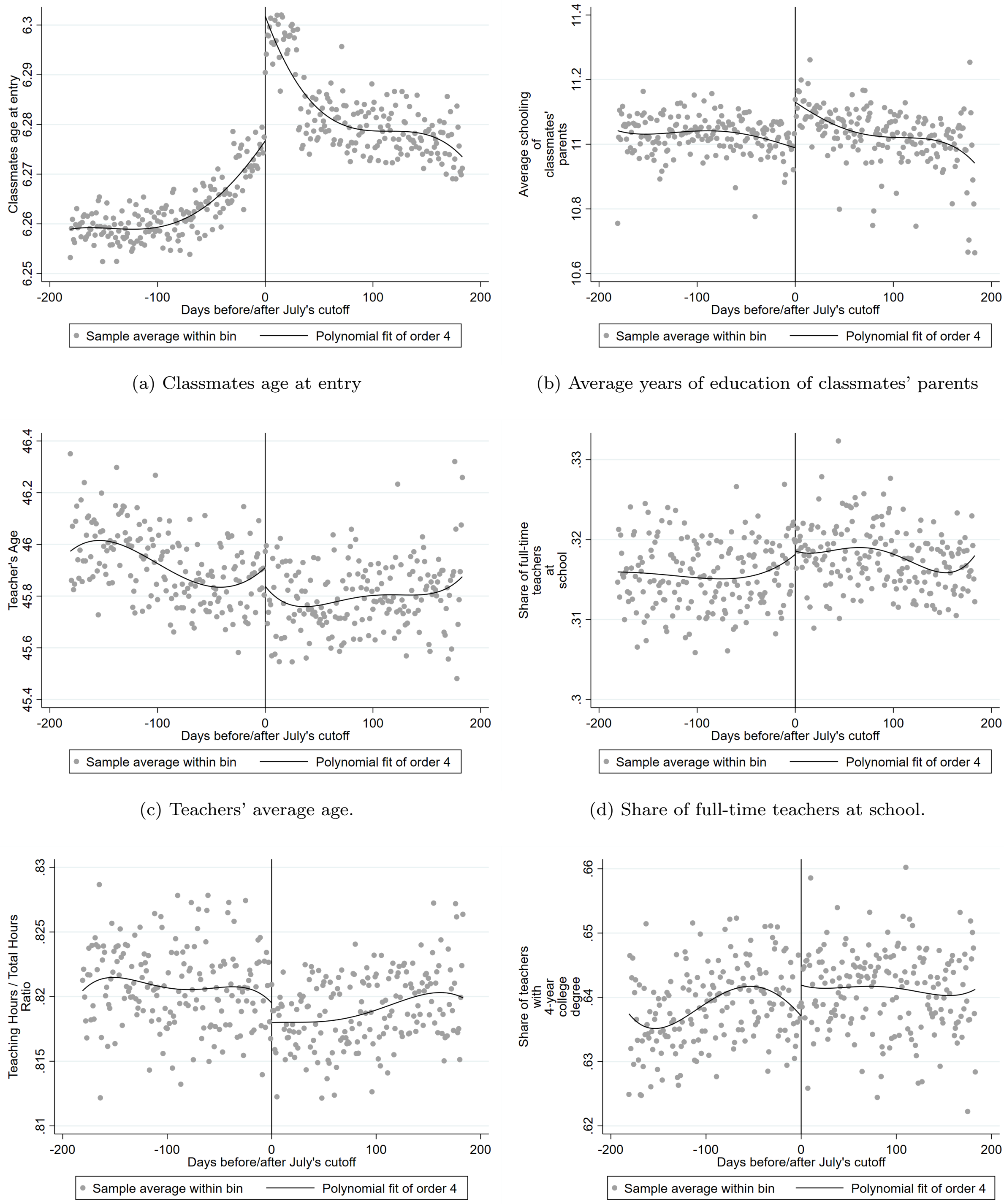
Table 1 presents the descriptive statistics. The upper panel reports the statistics for students born from January to July during the period under consideration, while the bottom panel shows the statistics for students with birthdays within an 8-day interval around July's cutoff.

Notably, the working sample's statistics closely resemble those of the whole population. Each cohort comprises roughly 250,000 students, split evenly between boys and girls. The average age of school entry is 6.14 years, with roughly 21% beginning school at a later age. This percentage increases around July's cutoff (Panel B), due to the explained institutional setup.

Concerning school type, about 52% of students are in voucher schools. The average schooling level of classmates' parents is approximately 11 years. For selection procedures, around 65% of students

[28]Cascio and Whitmore Schanzenbach (2016) find that having older classmates on average improves educational outcomes, and raising the probability of taking a college-entry exam.

attend schools historically known to pick students based on academics. The portion of full-time teachers is about 32%, with around 64% holding a 4-year college degree.



(a) Classmates age at entry

(b) Average years of education of classmates' parents

(c) Teachers' average age.

(d) Share of full-time teachers at school.

(e) Teaching Hours / Total Hours Ratio

(f) Share of teachers with 4-year college degree

Figure 4: Evolution of selected outcomes according to the student's birthday.

Figures 3 and 4 portray how each outcome corresponds to the student's birthday.[29] It's important

[29] For each outcome, we fit a flexible fourth-degree polynomial at every side of the four discontinuities. Specifically, we use the *rdplot* STATA command for the graphical analysis.

to note a distinct jump at the discontinuity for most of the considered variables. Specifically, Figure 3 emphasizes that students born just after the cutoff often enroll in schools with higher test scores and some degree of academic selection.

Furthermore, Figure 4 suggests that children born on or after July 1st are more likely to be surrounded by older classmates with better-educated parents. They also have a higher chance of being taught by teachers holding 4-year college degrees. This descriptive observation implies that starting school at an older age is linked to an overall "quality" improvement in the school environment.

4 Results

4.1 First stage: Impact of minimum age requirements on the probability to start school older.

Table 2 shows the calculated change in the likelihood of starting school at an older age (the year of the seventh birthday) for children born in the early days of July compared to those with a birthday in the final days of June. This difference, denoted as δ in equation (2), is displayed in three panels for different samples: Panel A shows the results for the complete sample, Panel B considers parents' education, and Panel C breaks down the results by by students' gender.

For each of these samples, we estimate δ using bandwidths of 5 days (columns 1-2), 10 days (columns 3-4), and 15 days (columns 5-6)[30]. For each of the bandwidths, the first column shows the results using only a local linear specification for $g(b_i)$ as a control[31], and the second column includes all other control variables.

Notably, the estimates remain qualitatively robust across all samples when additional variables are included in the model. This robustness to other covariates is consistent with the lack of correlation between observed predetermined variables and the source of variation coming from the minimum age requirements (see Appendix B.1). The estimates are also robust to changes in the bandwidth used.

Regarding the estimated δ, it is observed that being born after the cutoff increases the probability of starting school at an older age by approximately 45 to 50 percentage points compared to students with a birthday just before July 1^{st}. Figure 1 further supports this finding, as it shows that fewer than half of the children born before July are redshirted. Furthermore, in an IV approach equivalence, the value of the F-statistic (reported at the bottom of each panel/sample) suggests disregarding any concerns about weak instruments.[32]

[30] We report the results for these three-day windows since they cover the range of the optimal data-driven bandwidths found for the different outcomes and reported in Table A.1.

[31] In Appendix C.2 we show estimates for δ using quadratic and cubic specifications. The magnitudes of these estimates are comparable to the one obtained with a linear specification for the three selected bandwidths.

[32] Since the bandwidth is not the same for all the outcomes, in Tables 3 and 4 we also report the Olea and Pflueger (2013) F-statistic, which is robust to errors that are not conditionally homoskedastic and serially uncorrelated. The null hypothesis tests whether the relative bias exceeds a fraction of an asymptotic"worst-case" benchmark.

In Panels B and C of Table 2, we explore the heterogeneity in the source of variation by parents' education and students' gender. This analysis allows us to confirm relevant variation for these two subsamples. In addition, it also demonstrates the robustness of our estimates across families with varying levels of parental education and among male and female students.

The difference in point estimates suggests a larger complier group among families with less-educated parents and female students. Specifically, regardless of the specification used, we find that students from families with lower levels of parental education are approximately 8 percentage points more likely to start school older due to minimum age requirements. On the other hand, the differences by students' gender are relatively smaller, with girls showing an additional increase of approximately 5 percentage points in the probability to start at an older age, compared to boys. Despite these differences in the degrees of compliance, it is important to note that for all the subsamples, we observe a relevant source of variation induced by the discontinuity determined by the entry rules.

4.2 SSA and school characteristics.

Tables 3 and 4 present the estimates of α in equation (1) for two groups of variables: school quality and selectivity, and proxies for school inputs, respectively. Panel A of each table shows the OLS estimates,[33] while Panels B to D display the RD estimates for the complete sample, as well as sub-samples defined by parents' education (Panel C)[34] and students' gender (Panel D). The RD estimates reported are based on the specification using the MSE-optimal point estimate bandwidth.[35] Moreover, for each outcome and bandwidth, we provide the weak instrument Effective F statistic (Olea and Pflueger, 2013), which is valid under heteroscedastic or serially correlated errors.[36] These tests help us address concerns related to weak instruments.

Focusing on the RD estimates for the complete sample (Panel B), we see that starting school at an older age increases the average SIMCE score of the first school by 0.09 standard deviation. Columns (2) to (4) display results showing this improvement across the entire score distribution. There's a rise in the likelihood of attending a school above the 25th percentile by 2 percentage points, above the 50th percentile by 5 percentage points, and above the 75th percentile by 5 percentage points. When comparing OLS and RD estimates, we notice a positive bias in the OLS estimates regarding the impact of starting older on school SIMCE scores. This bias is evident in both the average SIMCE score and across the score distribution. This implies that some parents deliberately delay their children's school entry to enhance their chances of gaining admission to schools perceived to

[33] OLS estimates were obtained using a sample of students with birthdays within a 15-day range around the cutoffs.

[34] Parents with missing information about their years of education are grouped together with those who have 12 or fewer years of education.

[35] The RD estimates using the optimal bandwidth for inference are presented in Appendix C.1, and they reach similar conclusions to those reported in this section.

[36] The F values for each of the outcomes and specifications allow us to reject the null hypothesis of relative bias above 5%, 10%, or 20% of a conservative scenario bias with critical values of 37.41, 23.11, and 15.06, respectively.

have better quality. Moreover, we observe a 3 percentage point increase in the likelihood of attending a voucher school, which roughly corresponds to a 6% increase based on the sample mean.

In the Chilean context during the period analyzed, families compelled to postpone school entry might invest more time in searching for the most suitable school for their children. However, it's important to note that schools also have the freedom to choose students. Columns (6) and (7) in Table 3 present estimates for two variables crafted at the school level based on parents' responses from previous years. The outcomes in the first column reveal that students who start school at a later age enroll in schools where a higher proportion of parents reported some form of academic selection. Specifically, initiating school at an older age leads to a 7.5 percentage point increase in this proportion (roughly a 12% increase considering the sample mean).

The differences among parents' education (Panel C) imply that students from less educated backgrounds are primarily responsible for the improvements associated with SSA in terms of school average scores and selectivity. Among children with more educated parents, the benefits of SSA are confined to a higher likelihood of attending schools in the upper range of the SIMCE distribution (beyond the 75th percentile) and a 5 percentage point rise in the portion of parents reporting academic selection. This evidence, combined with a precisely zero impact on the likelihood of entering a voucher school for families with higher education levels, suggests that the impact of SSA is on intensive margins. To put it another way, some families who were already sending their children to private schools are able to transition to other private schools in the upper part of the SIMCE distribution, which tend to be more academically selective.

For families with less educated parents, we observe a significant increase of 0.11 standard deviations in the school SIMCE score across the entire score distribution. This group also experiences a 5.4 percentage point rise in the likelihood of starting in a voucher school and a greater proportion of parents (9 percentage points) reporting academic selection. Elder and Lubotsky (2009) discovered that the initial benefits of SSA in terms of test scores are more pronounced among higher-income families. They use this evidence, combined with the timing of the effect, to argue that SSA is influenced by skill development before kindergarten. In the context of a voucher system where school choice is itself an investment factor and schools actively select students, our findings lend support to the notion that families with lower incomes can offset these disparities in previous skill development.[37]

In Panel D, the variation by students' gender highlights greater improvements in SSA for girls, resulting in a significant 0.12 standard deviation increase in the school SIMCE score. This positive change is noticeable throughout the entire SIMCE distribution. Additionally, the probability of girls attending a voucher school is almost double that of male students, and they encounter a higher level

[37] As explained in footnote 23, the estimated α can be interpreted as the Local Average Treatment Effect on students who would have started school at a younger age (Untreated). The larger impact of SSA among less educated students is also consistent with lower levels of parental investment, which are at least partially offset by the waiting induced by minimum age requirements.

of academic selection compared to boys (8.6 percentage points versus 6.8 percentage points).

We have conducted robustness checks in three different ways to validate the findings. First, in Appendix C.1, we provide estimates for each of the different outcomes and samples using the CER-optimal bandwidth, which is our second data-driven bandwidth reported in Appendix C.1. The results obtained using this alternative bandwidth show similar magnitudes and conclusions.

Secondly, in Appendix C.2.2, we provide the reduced form estimates for each outcome both with and without covariates, using three different bandwidths (5, 10, and 15 days). These estimates consistently show a benefit for students born in July when compared to those born in the final days of June. Moreover, the results remain sturdy regardless of whether covariates are included and which bandwidth is employed.

Lastly, in Appendix C.4, we present the reduced form estimates for a Placebo analysis using different cutoffs, specifically at the 1^{st} of September and the 1^{st} of October, along with three distinct bandwidths, both with and without controls. Across all specifications, we do not detect a significant discontinuity, suggesting that the observed effects are not driven by spurious factors.

4.3 SSA, School Peers and School Teachers.

The impact of starting primary school at an older age on the characteristics of school peers and teachers is presented in Table 4, using a similar structure to the one used for school characteristics. It's important to highlight that the test for weak instruments helps alleviate any concerns regarding these outcomes.

The RD estimates for the complete sample indicate that students who begin school at an older age join a school with more "advantaged" peers, as measured by the education level of classmates' parents. Specifically, students entering school in the year of their seventh birthday show an increase of around 0.2 years in the average education level of parents within the school. Analyzing the variation by gender and parents' education, we find that, similar to estimates for school scores and selectivity, bigger improvements are seen for students with less educated parents and girls.

Column (2) of Table **??** demonstrates that starting school at an older age increases the average age of classmates by around 0.06 years. This outcome remains consistent across different samples. This finding holds significance for two reasons. Firstly, it implies that students commencing school at an older age are more likely to enroll in schools that systematically choose older students (using earlier cutoff dates) or employ academic selection (where older students tend to perform better in academic assessments). Secondly, this discovery aligns with previous evidence on the impact of relative age (Cascio and Whitmore Schanzenbach, 2016;Peña, 2017), suggesting an additional pathway through which SSA can benefit students.[38]

[38] Using data from the STAR project, Cascio and Whitmore Schanzenbach (2016) find that, holding constant own age, having older classmates on average improves educational outcomes. Peña (2017) find similar results for Mexico.

Examining teacher characteristics, we notice that students who begin school at an older age witness a decrease of around 0.7 years in the average age of teachers at the school. While this discovery might not inherently be positive, as it could signify a decline in teacher experience, we also identify that these students enroll in schools with a larger portion of teachers possessing a 4-year college degree. Concretely, there is a 2-percentage-point increase, which corresponds to a 3% difference compared to the sample mean.

Regarding school inputs, we discover that students who start school at an older age join institutions with about 1.3 percentage points more full-time contract teachers, signifying a 4% rise relative to the sample average. Furthermore, there's a decrease in the proportion of teaching hours compared to the total, implying a potential allocation of more time towards class preparation.

As for the previous group of outcomes, the RD results are robust to alternative bandwidths (Appendix C.1 and C.2.2). The placebo analysis in Appendix C.4 does not reveal a discontinuity for any of the outcomes, with the exception of the share of teachers with a 4-year college degree. However, this impact is considerably smaller than the main analysis, has an opposite sign, and is only statistically significant at the 10% level.

5 Conclusions

In this paper, we investigate how the School Starting Age (SSA) affects the attributes of the initial school that students attend, utilizing data from Chile's public administrative records. To address potential endogeneity, we leverage minimum age requirement rules as a source of variation in the likelihood of starting school at an older age.

We find that beginning school at an older age leads to noteworthy changes in school options. Firstly, students who delay school entry are placed in schools with an approximately 0.1 standard deviation higher average score on standardized tests. Secondly, their classmates tend to be older and have parents with higher education levels. Thirdly, these students are more likely to enter schools that implement some type of academic selection, have a higher portion of full-time teachers, and a greater share of teachers with a 4-year college degree. Further analysis by parental education level and gender highlights that most of these effects are driven by parents with lower education levels and girls.

This paper is connected to the existing literature that examines the relationship between the lasting impacts of SSA and the educational system's structure, which could enhance the benefits of beginning school at an older age. Our results contribute evidence to an alternative pathway through which SSA affects students' long-term outcomes.

References

Angrist, J. D. and A. B. Kruger (1991). Does compulsory schooling attendance affect schooling and earnings? *Quarterly Journal of Economics 106*, 979–1014.

Angrist, J. D. and J.-S. Pischke (2008, December). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Attar, I. and D. Cohen-Zada (2018). The effect of school entrance age on educational outcomes: Evidence using multiple cutoff dates and exact date of birth. *Journal of Economic Behavior & Organization 153*, 38–57.

Barua, R. and K. Lang (2016). School entry, educational attainment, and quarter of birth: A cautionary tale of a local average treatment effect. *Journal of Human Capital 10*(3), 347–376.

Bedard, K. and E. Dhuey (2006, 11). The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects*. *The Quarterly Journal of Economics 121*(4), 1437–1472.

Black, S., P. Devereaux, and P. Salvanes (2011). Too young to leave the nest? the effect of school starting age. *The Review of Economics and Statistics 93*(2), 455–467.

Buckles, K. and D. Hungerman (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics 95*(3), 711–724.

Cáceres-Delpiano, J. and E. Giolito (2023). Minimum age requirements and the role of the school choice set. *SERIEs 14*(1), 63–103.

Caceres-Delpiano, J. and E. P. Giolito (2019). *The impact of age of at entry on academic progression*, Chapter Data-Driven Policy Impact Evaluation: How Microdata is Transforming Policy Design. Springer.

Calonico, S., M. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2017). rdrobust: Software for regression-discontinuity designs. *Stata Journal 17*(2), 372–404.

Cascio, E. and E. Lewis (2006). Schooling and the armed forces qualifying test. evidence from school-entry laws. *Journal of Human Resources 41*(2), 294–318.

Cascio, E. U. and D. Whitmore Schanzenbach (2016, 07). First in the Class? Age and the Education Production Function. *Education Finance and Policy 11*(3), 225–250.

Celhay, P. and S. Gallegos (2022). Early skill effects on parental beliefs, investments and children long-run outcomes1. *Journal of Human Resources*.

Contreras, D., O. Larranaga, L. Flores, F. Lobato, and V. Macias (2005). Politicas educacionales en chile: Vouchers, concentracion, incentivos y rendimiento. *In Uso e impacto de la informacion educativa en America Latina, ed. Santiago Cueto. Santiago: PREAL.*, 61–110.

Cook, P. J. and S. Kang (2016). Birthdays, schooling, and crime: Regression-discontinuity analysis of school performance, delinquency, dropout, and crime initiation. *American Economic Journal: Applied Economics 8*(1), 33–57.

Datar, A. (2006). Does delaying kindergarten entrance give children a head start? *Economics of Education Review 25*(1), 43–62.

Dee, T. and H. H. Sievertsen (2018). The gift of time? School starting age and mental health. *Health Economics 27*(5), 781–802.

Dhuey, E., D. Figlio, K. Karbownik, and J. Roth (2019). School starting age and cognitive development. *Journal of Policy Analysis and Management 38*(3), 538–578.

Dhuey, E. and S. Lipscomb (2010). Disabled or young? relative age and special education diagnoses in schools. *Economics of Education Review 29*(5), 857–872.

Dobkin, C. and F. Ferreira (2010). Do school entry laws affect educational attainment and labor market outcomes? *Economics of Education Review 29*(1), 40–54.

Elder, T. E. and D. H. Lubotsky (2009). Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers. *The Journal of Human Resources 44*(3), 641–683.

Epple, D., R. E. Romano, and M. Urquiola (2017). School vouchers: A survey of the economics literature. *Journal of Economic Literature 55*(2), 441–492.

Fiorini, M. and K. Stevens (2021). Scrutinizing the monotonicity assumption in iv and fuzzy rd designs*. *Oxford Bulletin of Economics and Statistics 83*(6), 1475–1526.

Frandsen, B. R. (2017). *Party Bias in Union Representation Elections: Testing for Manipulation in the Regression Discontinuity Design when the Running Variable is Discrete*, Volume 38 of *Advances in Econometrics*, pp. 281–315. Emerald Publishing Limited.

Fredriksson, P. and B. Öckert (2014). Life-cycle effects of age at school start. *The Economic Journal 124*(579), 977–1004.

Gauri, V. and A. Vawda (2003). Vouchers for basic education in developing countries. a principal-agent perspective. *World Bank Policy Research Working Paper 3005*.

Hahn, J., P. Todd, and W. van der Klaauw (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica 69*(1), 201–209.

Heckman, J., S. Urzua, and E. Vytlacil (2006). Understanding instrumental variables in models with essential heterogeneity. *Review of Economics and Statistics 88*(3), 389–432.

Kramarz, F., S. Machin, and A. Ouazad (2008). What Makes a Test Score? The Respective Contributions of Pupils, Schools and Peers in Achievement in English Primary Education. *SSRN Electronic Journal*.

Landersø, R., H. S. Nielsen, and M. Simonsen (2016). School Starting Age and The Crime Age Profile. *The Economic Journal 127*(602), 1096–1118.

Landersø, R. K., H. S. Nielsen, and M. Simonsen (2020). Effects of School Starting Age on the Family. *Journal of Human Resources 55*(4), 1258–1286.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of Economic Literature 48*, 281–355.

Lubotsky, D. and R. Kaestner (2016). Do 'skills beget skills'? evidence on the effect of kindergarten entrance age on the evolution of cognitive and non-cognitive skill gaps in childhood. *Economics of Education Review 53*, 194–206.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics 142*(2).

McEwan, P. and J. Shapiro (2006). The benefit of delayed primary school enrollment. discontinuity estimates using exact birth dates. *Journal of Human Resources 43*(1), 1–29.

Mizala, A. and M. Urquiola (2013). School markets: The impact of information approximating schools' effectiveness. *Journal of Development Economics 1003*, 313–335.

Mühlenweg, A. M. and P. A. Puhani (2010). The Evolution of the School-Entry Age Effect in a School Tracking System. *Journal of Human Resources 45*(2), 407–438.

Mühlenweg, A. M. and P. A. Puhani (2010). The evolution of the school-entry age effect in a school tracking system. *Journal of human Resources 45*(2), 407–438.

Nam, K. (2014). Until when does the effect of age on academic achievement persist? evidence from korean data. *Economics of Education Review 40*, 106–122.

Olea, J. L. M. and C. Pflueger (2013). A Robust Test for Weak Instruments. *Journal of Business & Economic Statistics 31*(3), 358–369.

Oosterbeek, H., S. ter Meulen, and B. van der Klaauw (2021). Long-term effects of school-starting-age rules. *Economics of Education Review 84*, 102144.

Peña, P. A. (2017). Creating winners and losers: Date of birth, relative age in school, and outcomes in childhood and adulthood. *Economics of Education Review 56*, 152–176.

Schneeweis, N. and M. Zweimüller (2014). Early tracking and the misfortune of being young. *The Scandinavian Journal of Economics 116*(2), 394–428.

Todd, P. E. and K. I. Wolpin (2003, 02). On the Specification and Estimation of the Production Function for Cognitive Achievement. *The Economic Journal 113*(485), F3–F33.

Zweimüller, M. (2013). The effects of school entry laws on educational attainment and starting wages in an early tracking system. *Annals of Economics and Statistics/ANNALES D'ÉCONOMIE ET DE STATISTIQUE*, 141–169.

Table 1: Descriptive Statistics

Panel A: Students born during February-July.	Mean	SD	N
Voucher School	0.52	0.50	622,994
Average schooling of classmates' parents	11.29	2.34	622,926
Classmates age at entry	6.26	0.11	620,634
School academic selection	0.64	0.24	224,871
Teacher's Age	45.90	5.94	533,300
Teaching Hours / Total Hours Ratio	0.82	0.13	533,323
Share of full-time teachers at school	0.32	0.23	533,323
Share of teachers with 4-year college degree	0.64	0.28	445,959
Older	0.27	0.44	622,994
SSA	6.14	0.39	622,994
Male students	0.51	0.50	622,994
Father's education	11.60	3.85	510,960
Mother's education	11.59	3.59	521,292
Panel B: Students born within 8 days around July cutoff			
Voucher School	0.53	0.50	57,872
Average schooling of classmates' parents	11.31	2.36	57,864
Classmates age at entry	6.29	0.12	57,614
School academic selection	0.65	0.24	25,160
Teacher's Age	45.85	5.94	54,155
Teaching Hours / Total Hours Ratio	0.82	0.13	54,156
Share of full-time teachers at school	0.32	0.23	54,156
Share of teachers with 4-year college degree	0.64	0.28	44,821
Older	0.77	0.42	57,872
SSA	6.44	0.42	57,872
Male students	0.51	0.50	57,872
Father's education	11.60	3.88	47,790
Mother's education	11.61	3.59	48,694

Table 2: First stage estimates. Impact of age eligibility requirement on the probability of entry at an older age.

	Bandwidth					
	5 days		10 days		15 days	
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A:	**Complete sample**					
	0.469***	0.470***	0.472***	0.472***	0.473***	0.471***
	(0.012)	(0.011)	(0.008)	(0.007)	(0.006)	(0.005)
Observations	40846	40791	75415	75291	105994	105831
F excluded instr.	1415.9	1906.8	3676.6	5209	5753.70	8365.1
Panel B:	**By Parents' educational level**					
	12 years of education or less					
	0.505***	0.509***	0.510***	0.511***	0.509***	0.509***
	(0.011)	(0.009)	(0.008)	(0.006)	(0.006)	(0.005)
Observations	22551	22496	41607	41483	58464	58301
F excluded instr.	2087.4	3064.3	4205.5	6878.3	6433.20	10443.5
	More than 12 years of education					
	0.423***	0.419***	0.424***	0.422***	0.426***	0.422***
	(0.015)	(0.017)	(0.009)	(0.009)	(0.008)	(0.007)
Observations	18295	18295	33808	33808	47530	47530
F excluded instr.	761.5	635.7	2198.6	2069.6	3169	3274.4
Panel C:	**By Student's gender**					
	Boys					
	0.445***	0.444***	0.452***	0.453***	0.452***	0.452***
	(0.014)	(0.010)	(0.010)	(0.008)	(0.008)	(0.006)
Observations	20823	20790	38313	38240	53788	53696
F excluded instr.	1013	2041.8	2073.1	2934.8	3350.3	5405
	Girls					
	0.494***	0.495***	0.493***	0.492***	0.493***	0.490***
	(0.012)	(0.013)	(0.008)	(0.007)	(0.007)	(0.006)
Observations	20023	20001	37102	37051	52206	52135
F excluded instr.	1780.3	1375.5	4258.3	4385.6	5532.70	6003.6
Controls		X		X		X

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.
The dependent variable, *LaterEntry*, is a dummy variable that takes a value one for children who start primary school later than the closest academic year to when they turn six. Specifications with additional controls include municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies.

Table 3: Impact of SSA on school characteristics. MSE-optimal bandwidth.

	Avg. SIMCE	Above Percentile 25	Above Percentile 50	Above Percentile 75	Voucher	Academic Selection
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: OLS Complete Sample						
	0.173***	0.047***	0.075***	0.079***	0.008**	0.047***
	(0.008)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)
Observations	105060	105831	105831	105831	105831	45796
Panel B: Complete Sample						
	0.089***	0.022**	0.046***	0.049***	0.031***	0.075***
	(0.019)	(0.009)	(0.008)	(0.009)	(0.009)	(0.013)
Observations	67873	68393	68393	68393	88768	23520
Mean	.31	.74	.49	.24	.52	.65
Weak Instr. (a)	4807.1	5072.70	5072.70	5072.70	7402.3	124.7
Panel C: By parents' educational level						
12 years of education or less						
	0.113***	0.035**	0.058***	0.036***	0.054***	0.088***
	(0.023)	(0.017)	(0.009)	(0.013)	(0.011)	(0.012)
Observations	37402	37678	37678	37678	48939	12495
Mean	.01	.65	.35	.13	.46	.58
Weak Instr. (a)	7980	8908.6	8908.6	8908.6	8482	147.8
More than 12 years of education						
	0.053	0.006	0.025	0.072***	-0.000	0.054*
	(0.033)	(0.014)	(0.018)	(0.016)	(0.016)	(0.028)
Observations	30471	30715	30715	30715	39829	11025
Mean	.67	.86	.66	.39	.61	.73
Weak Instr. (a)	1629	1617.8	1617.8	1617.8	2708.8	93.60
Panel D: By students' gender						
Boys						
	0.059	-0.006	0.044***	0.041**	0.022**	0.068***
	(0.037)	(0.016)	(0.016)	(0.020)	(0.009)	(0.020)
Observations	34432	34728	34728	34728	45080	11979
Mean	.29	.74	.48	.24	.52	.64
Weak Instr. (a)	4065.2	4044.5	4044.5	4044.5	4495.6	97.60
Girls						
	0.119***	0.051***	0.048***	0.056***	0.037***	0.086***
	(0.016)	(0.014)	(0.011)	(0.010)	(0.014)	(0.014)
Observations	33441	33665	33665	33665	43688	11541
Mean	.32	.75	.5	.25	.53	.65
Weak Instr. (a)	3175.4	3501.3	3501.3	3501.3	5519.40	135.1

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth. Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For each of the outcomes, the MSE-optimal bandwidth is reported in Table A.1.

(a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

Table 4: Impact of SSA on school Inputs. MSE-optimal bandwidth.

	Avg. Parents' Education	Avg. Classmates' SSA	Teachers: Age	Teachers: Full-time	Teachers: Teaching / Total hours	Teachers: 4-year College
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: OLS Complete Sample						
	0.371***	0.076***	-0.559***	0.011***	-0.020***	0.020***
	(0.023)	(0.002)	(0.033)	(0.002)	(0.001)	(0.002)
Panel B: Complete Sample						
	0.196***	0.061***	-0.652***	0.013**	-0.014***	0.021**
	(0.043)	(0.006)	(0.160)	(0.005)	(0.005)	(0.008)
Observations	75282	81575	82735	63843	63843	41885
Mean	11.24	6.27	45.9	.32	.82	.60
Weak Instr. (a)	5273.1	6334.40	3117	1789.5	1789.5	1459.6
Panel C: By parents' educational level						
12 years of education or less						
	0.275***	0.061***	-0.728***	0.008	-0.015***	0.019**
	(0.050)	(0.006)	(0.240)	(0.010)	(0.005)	(0.008)
Observations	41474	44856	45512	35078	35078	24132
Mean	10.31	6.26	46.8	.32	.83	.60
Weak Instr. (a)	7047.70	8336.5	3221.9	2474.9	2474.9	1506.9
More than 12 years of education						
	0.079	0.062***	-0.552**	0.024***	-0.015	0.021*
	(0.070)	(0.006)	(0.232)	(0.007)	(0.010)	(0.012)
Observations	33808	36719	37223	28765	28765	17753
Mean	12.41	6.29	44.77	.32	.81	.70
Weak Instr. (a)	2073.1	2696.4	1597.8	758.80	758.80	991.6
Panel D: By students' gender						
Boys						
	0.148***	0.062***	-0.719***	0.015*	-0.019**	0.024*
	(0.057)	(0.006)	(0.186)	(0.008)	(0.008)	(0.013)
Observations	38237	41477	42113	32466	32466	21293
Mean	11.22	6.27	45.87	.32	.82	.60
Weak Instr. (a)	2944.2	3415.7	2520.6	1289.5	1289.5	1493.8
Girls						
	0.237***	0.061***	-0.561**	0.011	-0.008	0.017*
	(0.049)	(0.006)	(0.225)	(0.008)	(0.005)	(0.010)
Observations	37045	40098	40622	31377	31377	20592
Mean	11.27	6.27	45.92	.31	.82	.60
Weak Instr. (a)	4448.8	5527.40	2702.3	1898.4	1898.4	989.7

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by day of birth. Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For each of the outcomes, the MSE-optimal bandwidth is reported in Table A.1.
(a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

Online Appendix not for publication

A Data-driven bandwidths

In line with Calonico et al. (2014), we opt for two data-derived bandwidths for each outcome. The initial approach aims to strike a balance between bias and variance, achieving this by minimizing the Mean Squared Error (MSE) of the local polynomial RD point estimator, often referred to as the MSE-optimal bandwidth.[39] Although the first bandwidth optimizes point estimates, it falls short in terms of inference since it's not sufficiently "narrow" to eliminate the predominant bias term for statistical reasoning. To address this issue, we adopt the under-smoothing approach, which involves utilizing a greater number of observations for point estimation than for making inferences.

For the second bandwidth, our focus shifts towards minimizing an approximation of the Coverage Error Rate (CER) of the confidence intervals. This quantifies the difference between the practical coverage of the confidence interval and its theoretical level. The selected bandwidths are presented in Table A.1. These bandwidths span a range of 5 to 12 days around the discontinuity, with the CER-optimal bandwidths consistently being narrower.

Table A.1: Optimal bandwidth by selected outcomes and cutoffs

	Method	Optimal bandwidth
Voucher School	MSE	12.6
	CER	10.3
Average schooling of classmates' parents	MSE	10.7
	CER	8.7
School SIMCE	MSE	10.0
	CER	8.1
Classmates age at entry	MSE	12.1
	CER	9.9
Teacher's Age	MSE	12.5
	CER	10.2
Teaching Hours / Total Hours Ratio	MSE	10
	CER	8.2
Share of full-time teachers at school	MSE	10.1
	CER	8.2
Share of teachers with 4-year college degree	MSE	8.4
	CER	6.8
School academic selection	MSE	8.2
	CER	6.6
School selects on family background	MSE	6.6
	CER	5.3

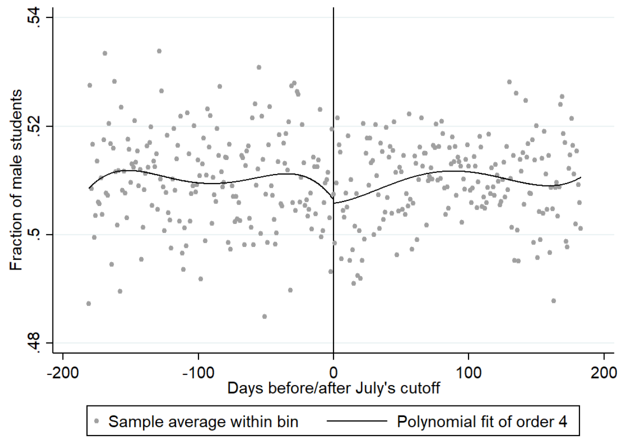
MSE: Mean Squared Error (MSE) optimal bandwidth.
CRE: optimal bandwidth that minimizes the asymptotic coverage error rate of the robust bias corrected confidence interval.
Both bandwidths were chosen following Calonico et al. (2017), and implemented with the command rdbwselect in STATA, a local linear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

[39] 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.

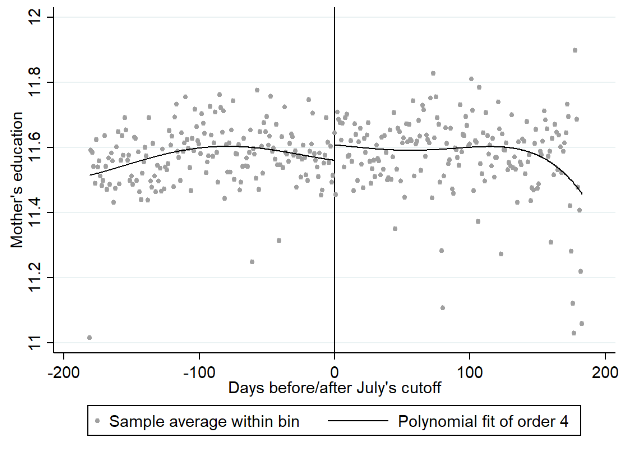
B Validity of the source of variation

B.1 Continuity in predetermined variables

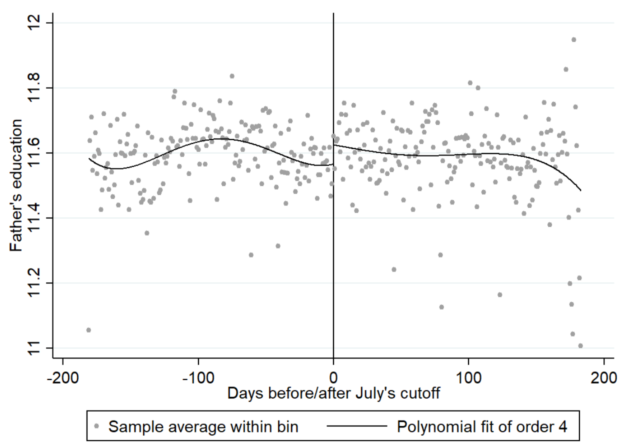
Our analysis relies on the concept that changes in school enrollment eligibility near a certain cutoff can be considered comparable to a randomized assignment. This assumption applies particularly to students whose birthdays fall near the cutoff date. Just as in standard random assignments, the attributes students have before this "randomization" should be comparable between the treated group (students born shortly after a cutoff) and the control group (students born just before the cutoff). If we identify consistent differences in these attributes around these dates, it would challenge the underlying assumption that individuals cannot precisely manipulate the running variable (Lee and Lemieux, 2010).



(a) Fraction of males



(b) Mother's education



(c) Father's education

Figure B.1: Balancing covariates

Figure B.1 visually examines the possible presence of a discontinuity in four fundamental characteristics found within the dataset: gender (percentage of males), educational levels of the mother and father, and the highest education attained by the student's parents. When contrasted with those in Figure 3, this graphical representation doesn't reveal any significant discontinuities or outliers for the specified variables. We carry out formal tests for potential discontinuities in these baseline characteristics, using various polynomial models and three bandwidths (5, 10, and 15 days). The resulting p-values are documented in Table B.2.[40] In Columns (2) and (4) of Table B.2, we check for discontinuities around July's cutoff, and in columns (1) and (3), we consider the February-July cutoffs. For only four out of 36 specifications, we reject the null at a significance level of 5%. Notice that discontinuities are detected only for a quadratic or cubic polynomial, probably due to an overfitting effect in the model.

[40]We utilize a regression model for each of the predetermined variables, as follows: $covariate_i = \eta_{wh} + \phi_b + \gamma * 1\{b_i - C > 0\} + g(b_i) + v_i$. In this equation, $1b_i - C > 0$ is an indicator variable that takes a value of one for students with birthdays (b_i) after the cutoff (C), and zero otherwise. Similar to equation (1), ϕ_b represents fixed effects for the year of birth, and η_{wh} accounts for fixed effects related to week days and holidays. The p-values reported correspond to the null hypothesis $\gamma = 0$, which implies that there are no differences in the predetermined variables between children born above and below the cutoff.

Table B.2: Differences in predetermined variables between children born before and after selected cutoffs. p-values reported.

Bandwidth	Male		Parents education	
	Panel A. $g(b_i)$ of degree 3			
15 days	0.217	0.601	0.170	0.579
10 days	0.248	0.562	0.087	0.577
5 days	0.000	0.070	0.000	0.185
	Panel B. $g(b_i)$ of degree 2			
15 days	0.972	0.941	0.598	0.785
10 days	0.494	0.694	0.348	0.630
5 days	0.001	0.297	0.000	0.390
	Panel C. $g(b_i)$ of degree 1			
15 days	0.820	0.387	0.097	0.328
10 days	0.884	0.522	0.162	0.410
5 days	0.757	0.898	0.407	0.908
Cutoffs	Feb-July	July	Feb-July	July

For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_{wh} + \phi_b + \gamma^s * 1\{b_i - C >\} + g(b_i) + v_{it}$ where $1\{b_i - C > 0\}$ is an indicator variable taking a value of one for sudents whose birthday (b_i) is over the cutoff (C), and zero otherwise. α_s is a 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 are 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. Selected cutoffs indicated at the bottom of the table.

Municipality variables

While it is not mandatory for students in Chile to reside in the same municipality as the school they attend, it is theoretically possible for parents to choose a neighborhood based on the school's cutoff date. However, considering that our observations begin at the commencement of schooling, our intention is to show that such a phenomenon does not appear to be prevalent in Chile. Figure B.2 illustrates the trajectory of three variables over the course of students' school life in the Santiago Metropolitan area, which comprises 52 municipalities. These variables include the fraction of students changing schools, the fraction of students changing their municipality of residence, and the fraction of students moving to a school located in a different municipality.

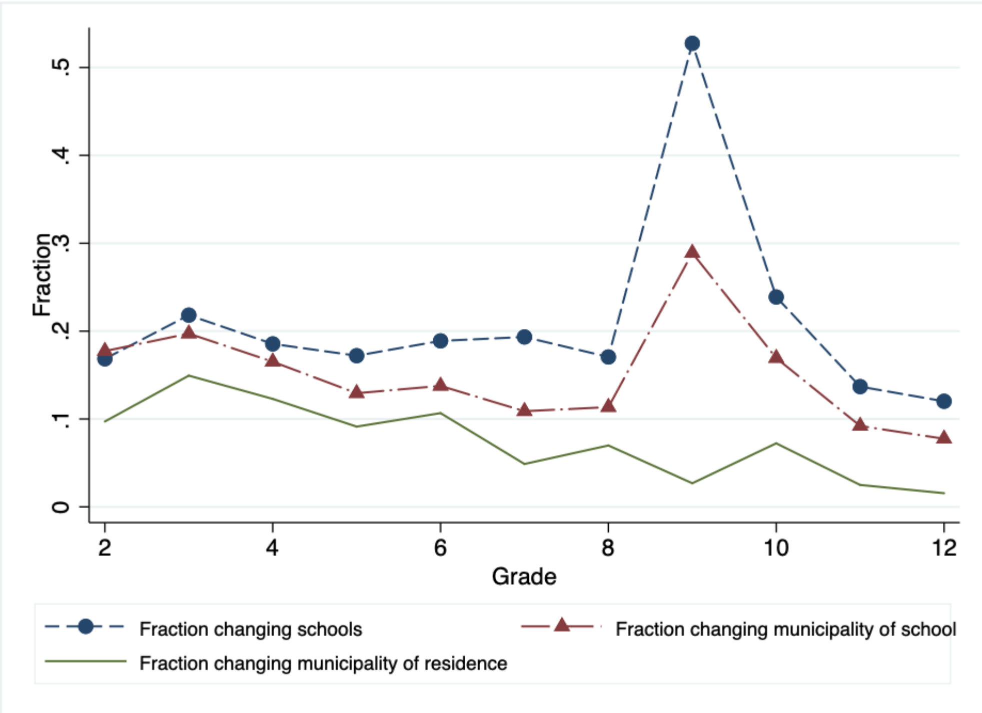


Figure B.2: Students changing schools vs changing municipality by grade. Santiago Metropolitan Area

To begin with, the average proportion of students changing their municipalities of residence annually is roughly 7.5 percent, and this percentage diminishes as their schooling progresses. In contrast, the mean proportion of students switching schools over the course of their school life is 21 percent per year, whereas the proportion of students changing the municipality of their school averages 15 percent per year (twice the proportion of those changing their municipality of residence). It is noteworthy that a marked spike occurs in grade 9 (the first year of secondary school), where over 50 percent of students change schools and about 30 percent of students change the municipality of their school. However, despite this considerable shift in the proportion of students changing municipalities for their schools, there is no corresponding surge in the proportion of students relocating to another municipality. This observation suggests that the likelihood of municipality changes during the school years is more heavily influenced by demographic factors, such as parents' age and their prospects of homeownership, rather than families actively choosing schools.

To strengthen our argument, we try to formally demonstrate that individuals don't sort themselves into different municipalities around the cutoff. If sorting were happening, we would expect to see differences between municipalities on each side of the cutoff. To investigate this, we select specific characteristics of municipalities and check for any abrupt changes in these characteristics, similar to how we looked at individual factors.

We focus on average family income, poverty rate,[41] and the average SIMCE (Math) score for municipalities in the Santiago Metropolitan Area. In Table B.3, we present the p-values for different mathematical specifications (ranging from linear to cubic) and three time intervals (5, 10, and 15 days). Our results don't provide enough evidence to reject the idea that there are no significant differences between municipalities at the 5% level of significance, and only one result is marginally significant at the 10% level.

	Avg. HH Income			Poverty rate			Avg. SIMCE Math		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$g(b_i)$ of degree 3	0.639	0.134	0.920	0.884	0.734	0.568	0.970	0.331	0.877
$g(b_i)$ of degree 2	0.851	0.843	0.599	0.983	0.892	0.999	0.637	0.427	0.949
$g(b_i)$ of degree 1	0.287	0.946	0.546	0.361	0.763	0.519	0.083	0.438	0.973
Bandwidth (days)	15	10	5	15	10	5	15	10	5
Observations	38,518	27,404	14,919	38,518	27,404	14,919	38,518	27,404	14,919

Note: For each of the variables (w_i), reported in the first row, we run the regression, $w_i = \eta_{wh} + \phi_b + \gamma * 1\{b_i > 0\} + g(b_i) + v_{it}$. $1\{b_i = BD_i - C^s > 0\}$ is an indicator variable taken a value of one for sudents whose birthday (BD_i) is over the cutoff (C), and zero otherwise. η_{wh} and ϕ_b represent week-day/holiday, and year of birth fixed effects, respectively. The null hypothesis for which the p-values are reported is $H0 : \gamma = 0$, that is, there are not differences in the variables at the municipality level between children over and below the July cutoff.

Table B.3: Differences in variables at the municipality level: Santiago Metropolitan Area

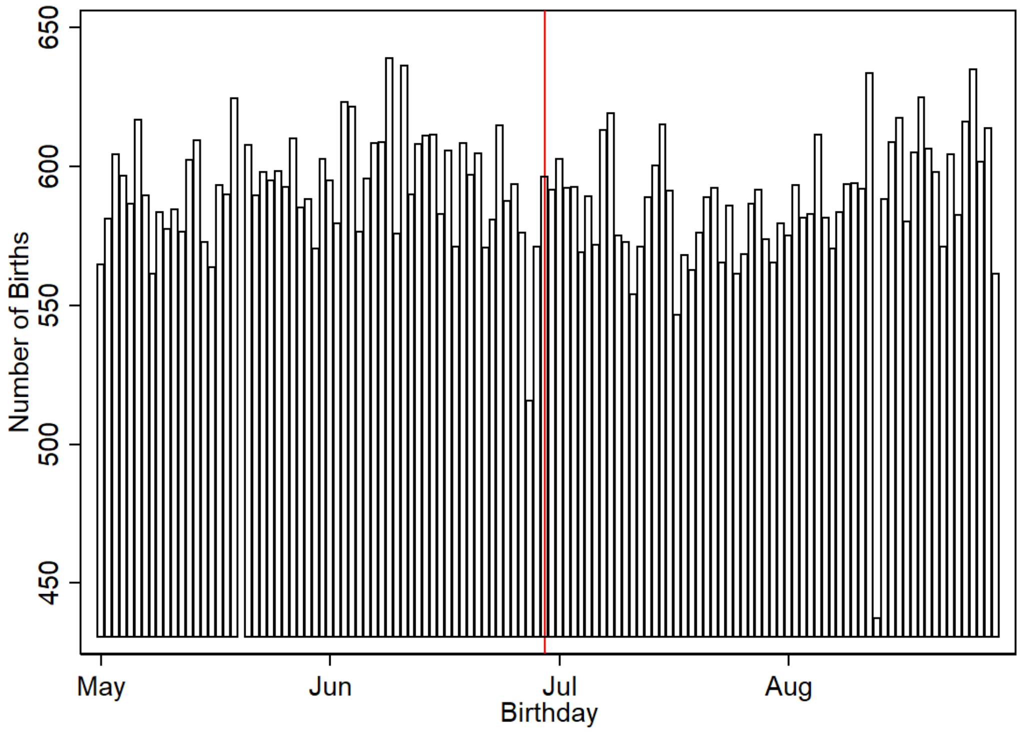
B.2 Manipulation of the running variable

The success of randomizing the treatment around the points of discontinuity hinges on the assumption that families cannot precisely control their children's birthdates. In simpler terms, the credibility

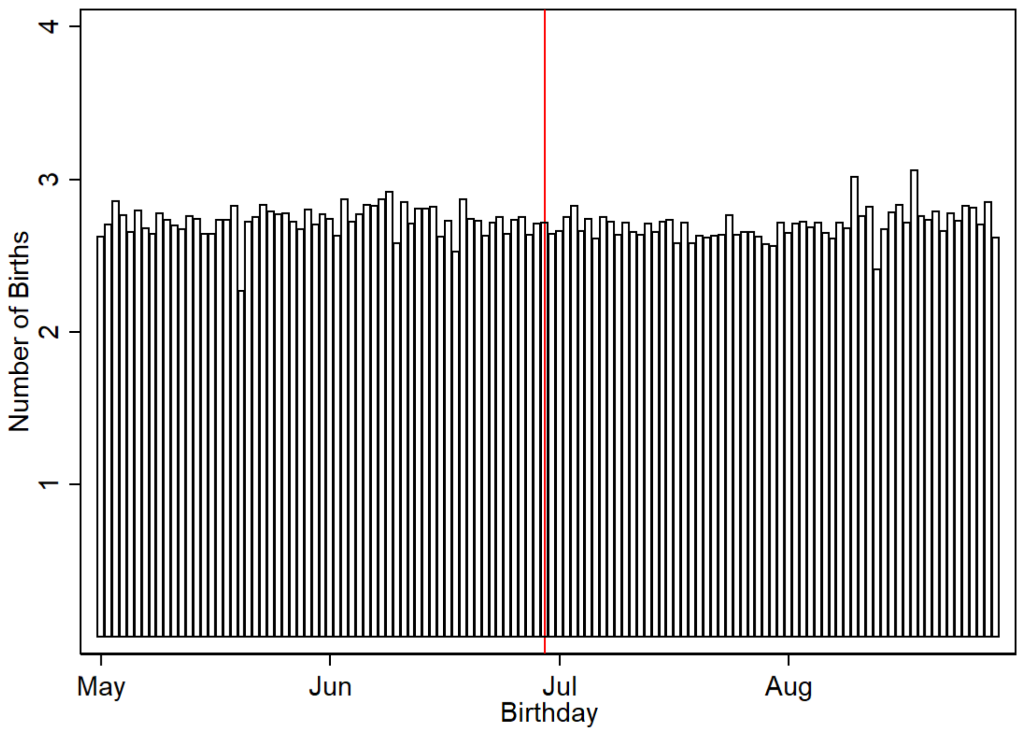
[41]The information for these two variables is taken from *Encuesta CASEN 2006. Ministerio de Desarrollo Social y Familia.* Available from https://datasocial.ministeriodesarrollosocial.gob.cl/portalDataSocial/descarga

of a Regression Discontinuity (RD) design could be compromised if individuals have the ability to manipulate the running variable (Lee and Lemieux, 2010). Given that the rules regarding the minimum age for school entry are publicly known, it's plausible that the potential advantages or disadvantages associated with delaying a child's entry into school might influence some families to strategically choose the birth season. Additionally, it's noteworthy that Chile, along with Turkey and Mexico, is among the countries with the highest rates of caesarean sections (c-sections) globally.[42] These two facts suggest some power to select the running variable.

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



(a) Day of Birth. Raw Histogram.



(b) Day of Birth. Conditional Histogram

Figure B.3: Raw and conditional histogram for the day of birth

Nevertheless, the possibility of families arranging their choices throughout the calendar year does not invalidate the quasi-experimental design. The crucial assumption for identification is that individuals lack the capacity to precisely organize themselves around these points of discontinuity. If precise manipulation were at play, we would observe data clustering around these points, leading to a discontinuous distribution for the day of birth (the variable we are studying).

Panel (a) of Figure B.3 displays the original histogram for the day of birth, encompassing individuals born between December 15th and July 6th.[43] Despite considerable fluctuations, the figure conceals a fairly even distribution of births throughout the calendar year, with significant dispersion concerning weekdays and birth years. More specifically, we note an average of 650 births daily, varying between 500 and 800 births. However, when we factor in fixed effects for the day of the week, holidays, and birth years, as seen in Panel (b) of Figure B.3, we observe a uniform spread of births over the calendar year and no apparent disruptions in the distribution of the variable of interest across the discontinuity points. As a result, within a specific municipality and birth year, we observe an approximate average of 3 births per day.[44]

In line with the methodology introduced by McCrary (2008), we execute a formal assessment to ascertain any discontinuity in the distribution of the running variable. To achieve this, we carry out a regression where the frequency of birthdays across the calendar year serves as the dependent variable, mirroring the approach taken with the previous three predetermined variables.[45] These results are documented in Table B.4, with examinations conducted across all the cutoffs (column 1) and specifically for July's cutoff (column 2). Only in the case of one particular specification (quadratic polynomial) and a specific bandwidth (five days), do we reject the null at a significance level of 5%. This implies that parents might be able to select a certain week or month for their child's birth, but precision down to the exact birth date seems unlikely.

Finally, we perform a test for manipulation of the running variable that is consistent when the running variable is discrete, as proposed by Frandsen (2017). The obtained p-value is 0.761, indicating that we cannot reject the null hypothesis of no manipulation.[46]

[43]Data on births was obtained from *Departamento de Estadisticas e Información de Salud*, Ministerio de Salud de Chile, https://deis.minsal.cl/#datosabiertos

[44]Notice in Figure B.3b 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.

[45]See footnote 40.

[46]We employ the Stata command "rddisttestk" with parameter $k = 0$ to conduct the test. This parameter sets the maximum degree of nonlinearity in the probability mass function that is considered compatible with no manipulation. By choosing $k = 0$, we are stringent in detecting even small deviations from linearity, leading the test to reject with high probability.

Table B.4: Differences in the number of births before and after selected cutoffs. p-values reported.

Bandwidth		
	Panel A. $g(b_i)$ of degree 3	
15 days	0.219	0.076
10 days	0.260	0.499
5 days	0.318	0.122
	Panel B. $g(b_i)$ of degree 2	
15 days	0.352	0.114
10 days	0.195	0.063
5 days	0.891	0.008
	Panel C. $g(b_i)$ of degree 1	
15 days	0.072	0.473
10 days	0.058	0.280
5 days	0.519	0.166
Cutoffs	Feb-July	July

The dependent variable is the number of births in a specific day of the calendar year, in the different counties, for the different cohorts in the analysis. We run the regression, $w_i = \alpha_s + \eta_{wh} + \phi_b + \gamma^s * 1\{b_i - C >\} + g(b_i) + v_{it}$ where $1\{b_i - C > 0\}$ is an indicator variable taking a value of one for students whose birthday (b_i) is over the cutoff (C), and zero otherwise. α_s is a 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 are 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. Selected cutoffs indicated at the bottom of the table.

B.3 Monotonicity

The monotonicity assumption is essential because it allows us to interpret the estimated parameter as a Local Average Treatment Effect (LATE). In the context of our paper, the monotonicity assumption implies that a change in the instrument's value (in our case, being born after the July cutoff) should increase the school starting age (SSA) for all students or, at the very least, not decrease it.

The need for the monotonicity assumption arises due to the presence of heterogeneity, combined with students strategically choosing their SSA based on expected outcomes ("sorting on gains"). To gauge the extent of this heterogeneity, we include covariates that can reasonably serve as indicators for income, such as parental education, and specifically within the Chilean context, the municipality of residence. If violations of monotonicity were a substantial concern in our analysis, we would expect the estimates to show sensitivity when covariates like these are included (Dhuey et al., 2019). Nevertheless, our estimates are generally insensitive to the incorporation of these covariates (see Appendix C.2). This observation suggests that potential breaches of monotonicity are unlikely to significantly affect our results.

However, we acknowledge that we cannot rule out the presence of heterogeneity and sorting on gains. Therefore, we will now examine the plausibility of the monotonicity assumption in relation to our paper, assuming independence between the instrument and the potential treatment assignment.

	Born before cutoff	Born after cutoff
Early entry	0.000 (0.00)	0.006 (0.08)
On Time entry	0.500 (0.50)	0.994 (0.08)
Late entry	0.500 (0.50)	0.000 (0.00)

Table B.5: Fraction of students starting school on time, early or late by month of birth

Following the terminology in Fiorini and Stevens (2021), the term "On time" is used to describe children who start school as soon as they become eligible. "Late Entry" refers to the situation where children enter school one year later than their eligibility, while "Early Entry" pertains to children who begin school one year before becoming eligible. Consequently, if all children in our sample started "On time," we would expect to observe an increase in SSA from approximately 5.7 years for June-born children to 6.7 years for July-born children, thereby upholding the assumption of monotonicity.

As highlighted by Barua and Lang (2016), monotonicity is violated when there are defiers—

children who start early only when they are not allowed to do so. Table B.5 outlines the distribution of students in the Early, Late, and On-time groups on both sides of the cutoff in our working sample. Notably, we find no instances of June-born children starting school before their first eligible year (which would be at age 4.7), nor do we observe July-born children starting school a year later than their first eligible year (they should have entered at age which would be at age 7.7). This leaves us with four counterfactual scenarios:

(i) June-born students starting "On time" who would also start "On time" if they were born in July (resulting in an increase in SSA from approximately 5.7 to 6.7 at the cutoff).

(ii) June-born children starting "Late" but would start "On time" if they were born in July (resulting in a slight decrease in SSA).

(iii) June-born children starting "Late" but would start "Early" if they were born in July (leading to a decrease in SSA from 6.7 to 5.7).

(iv) June-born children starting "On time" but would start "Early" if they were born in July (also leading to a decrease in SSA from 6.7 to 5.7).

Monotonicity is violated if, in addition to the complier group (i), we have students whose SSA diminishes as the instrument changes. As shown in Table B.5, approximately half of the June-born students are redshirted (i.e., they delay their school entry). In contrast, almost all July-born children start school on time (99.4%). With this information in mind, we can rule out scenarios (iii) and (iv), both of which entail a year's decrease in SSA. Consequently, our primary concern regarding monotonicity pertains to scenario (ii), which we cannot dismiss. As shown in Table B.5, both the complier group (i) and the defier group (ii) are potentially of similar size: $p_{complier} = p_{defier} = 0.5$.

While we cannot conclusively rule out the possibility of monotonicity violation, the utilization of precise birth dates within the framework of a fuzzy RD design serves to alleviate potential concerns associated with this assumption. For instance, in a 1-day discontinuity sample, the complier group (i) would observe an increase in SSA at the cutoff by 364/365 years, while the defier group (ii) would encounter a decrease in SSA by 1/365 years. Within the context of essential heterogeneity, the annual benefits derived from SSA would vary between these two groups ($\bar{\beta}_{complier} \neq \bar{\beta}_{defier}$). Following the approach outlined by Fiorini and Stevens (2021):

$$\beta^{RD}\left(July\ 1^{st}, June\ 30^{th}\right) = \frac{\frac{364}{365}\bar{\beta}_{complier}p_{complier} - \frac{1}{365}\bar{\beta}_{defier}p_{defier}}{\frac{364}{365}p_{complier} - \frac{1}{365}p_{defier}} \approx \bar{\beta}_{complier}$$

This approximation arises from the fact, provided both groups are of equal size, that the complier group counts 364 times more than the hypothetical defier group. Even when using a larger discontinuity sample, as we do in our paper, the inclusion of flexible function on the day of birth enables us to capture the effect at the discontinuity. In summary, while we cannot definitively rule out a violation of monotonicity, we have reasonable confidence that the parameter we are estimating in our fuzzy RD design closely approximates the Local Average Treatment Effect (LATE) of interest.

C Robustness checks

C.1 CER optimal bandwidth

Table C.6: Impact of SSA on school characteristics. CER-optimal bandwidth

	Avg. SIMCE	Above Percentile 25	Above Percentile 50	Above Percentile 75	Voucher	Academic Selection
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Complete Sample						
	0.107***	0.033***	0.053***	0.053***	0.038***	0.073***
	(0.019)	(0.011)	(0.006)	(0.013)	(0.012)	(0.013)
Observations	53796	54209	54209	54209	68393	20765
Mean	.31	.74	.49	.24	.52	.65
Weak Instr. (a)	3030.7	3215.8	3215.8	3215.8	5072.70	48.40
Panel B: By parents' educational level						
12 years of education or less						
	0.116***	0.039**	0.050***	0.038***	0.065***	0.086***
	(0.025)	(0.019)	(0.009)	(0.015)	(0.012)	(0.017)
Observations	29603	29824	29824	29824	37678	11024
Mean	.01	.65	.35	.13	.46	.58
Weak Instr. (a)	4613.5	5176.6	5176.6	5176.6	8908.6	55.90
More than 12 years of education						
	0.098***	0.027***	0.059***	0.077***	0.001	0.049
	(0.025)	(0.009)	(0.013)	(0.019)	(0.021)	(0.031)
Observations	24193	24385	24385	24385	30715	9741
Mean	.67	.86	.66	.39	.61	.73
Weak Instr. (a)	1147.4	1124.5	1124.5	1124.5	1617.8	37.8
Panel C: By students' gender						
Boys						
	0.113***	0.018	0.071***	0.060**	0.020	0.078***
	(0.036)	(0.013)	(0.013)	(0.028)	(0.013)	(0.019)
Observations	27281	27522	27522	27522	34728	10576
Mean	.29	.74	.48	.24	.52	.64
Weak Instr. (a)	2813.2	2855	2855	2855	4044.5	46.8
Girls						
	0.112***	0.051***	0.040***	0.048***	0.054***	0.073***
	(0.017)	(0.017)	(0.010)	(0.014)	(0.013)	(0.015)
Observations	26515	26687	26687	26687	33665	10189
Mean	.32	.75	.5	.25	.53	.65
Weak Instr. (a)	2053.2	2310.1	2310.1	2310.1	3501.3	47.7

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by day of birth.

Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For each of the outcomes, the CER-optimal bandwidth is reported in Table A.1. (a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

Table C.7: Impact of SSA on school Inputs. CER-optimal bandwidth.

	Avg. Parents' Education	Avg. Classmates' SSA	Teachers			
			Age	Full-time	Teaching / Total hours	4-year College
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Complete Sample						
	0.264***	0.063***	-0.531***	0.015***	-0.010**	0.018**
	(0.036)	(0.006)	(0.159)	(0.005)	(0.005)	(0.008)
Observations	61251	68084	63841	50582	50582	36659
Mean	11.24	6.27	45.9	.32	.82	.60
Weak Instr. (a)	3948.2	5018.6	1790	1279.2	1279.2	1063.7
Panel B: By parents' educational level						
12 years of education or less						
	0.312***	0.061***	-0.716**	0.008	-0.013**	0.018**
	(0.040)	(0.006)	(0.310)	(0.009)	(0.005)	(0.008)
Observations	33683	37406	35076	27764	27764	21137
Mean	10.31	6.26	46.8	.32	.83	.60
Weak Instr. (a)	7577.70	9132.70	2478.2	1841.9	1841.9	1146.7
More than 12 years of education						
	0.203***	0.068***	-0.308	0.029***	-0.009	0.014
	(0.063)	(0.007)	(0.215)	(0.007)	(0.012)	(0.012)
Observations	27568	30678	28765	22818	22818	15522
Mean	12.41	6.29	44.77	.32	.81	.70
Weak Instr. (a)	1245.8	1608.8	758.80	590.9	590.9	701.5
Panel C: By students' gender						
Boys						
	0.221***	0.063***	-0.541***	0.017*	-0.014*	0.021
	(0.064)	(0.006)	(0.207)	(0.009)	(0.008)	(0.013)
Observations	31053	34564	32464	25725	25725	18656
Mean	11.22	6.27	45.87	.32	.82	.60
Weak Instr. (a)	3251.8	3734.2	1287.5	886.5	886.5	1247.5
Girls						
	0.303***	0.065***	-0.509**	0.012	-0.005	0.012
	(0.044)	(0.006)	(0.224)	(0.008)	(0.006)	(0.012)
Observations	30198	33520	31377	24857	24857	18003
Mean	11.27	6.27	45.92	.31	.82	.60
Weak Instr. (a)	2942.3	3761.3	1898.4	1582.7	1582.7	700.7

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For each of the outcomes, the CER-optimal bandwidth is reported in Table A.1. (a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

C.2 Robustness analysis for the first stage and reduced form

C.2.1 First Stage. Impact of minimum age requirements on the probability to start school older.

Table C.8 show the sensitivity of the first stage estimates to three different bandwidths (5, 10 and 15 days) for quadratic and cubic polynomials. Notice that the results are robust to changes in both dimensions.

Table C.8: First stage estimates. Impact of age eligibility requirement on the probability of entry at an older age. Sensitivity to the degree of the polynomial specification.

	Bandwidth					
	5 days		10 days		15 days	
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A:	**Complete sample**					
	0.447***	0.409***	0.467***	0.453***	0.471***	0.462***
	(0.008)	(0.009)	(0.011)	(0.012)	(0.010)	(0.013)
Observations	40791	40791	75291	75291	105831	105831
F excluded instr.	2823.3	2047.1	1865.1	1446.1	2114.4	1301.9
Panel B:	**By Parents' educational level**					
	12 years of education or less					
	0.496***	0.437***	0.504***	0.503***	0.513***	0.503***
	(0.012)	(0.012)	(0.009)	(0.014)	(0.010)	(0.013)
Observations	22496	22496	41483	41483	58301	58301
F excluded instr.	1771.5	1297.7	2886.5	1295.8	2414.1	1449
	More than 12 years of education					
	0.383***	0.367***	0.418***	0.386***	0.417***	0.407***
	(0.008)	(0.014)	(0.017)	(0.013)	(0.013)	(0.016)
Observations	18295	18295	33808	33808	47530	47530
F excluded instr.	2376.2	647.90	622.30	927.6	1054.4	686.2
Panel C:	**By Student's gender**					
	Boys					
	0.430***	0.388***	0.439***	0.439***	0.449***	0.433***
	(0.012)	(0.024)	(0.010)	(0.016)	(0.011)	(0.011)
Observations	20790	20790	38240	38240	53696	53696
F excluded instr.	1205.7	262.6	1837.4	757.5	1754.7	1436.6
	Girls					
	0.462***	0.432***	0.497***	0.465***	0.494***	0.494***
	(0.008)	(0.007)	(0.014)	(0.010)	(0.012)	(0.017)
Observations	20001	20001	37051	37051	52135	52135
F excluded instr.	3154.1	3368.2	1243.8	2318.2	1795.7	847.90
Degree Pol.	2	3	2	3	2	3

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.
The dependent variable, *Older*, is a dummy variable that takes a value one for children who start primary school the year they turn seven. Specifications with additional controls include municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies.

C.2.2 Reduced form. Impact of minimum age requirements on selected outcomes

Tables C.9 and C.10 show the reduced form estimates from the regression:

$$y_{ict} = \rho_t + \rho_c + X_i^{'}\psi + \delta \times 1\{b - C > 0\} + g(b_i) + v_i,$$

where y_{ict} represents one of the outcomes for student i, born the day b of year t, and living in municipality c. Here δ, the parameter of interest, captures the discontinuity around the cutoff, $1\{*\}$ is an indicator operator, and C the July cutoff.

Table C.9: Reduced form anaysisis: School characteristics

	Avg. SIMCE	Above Percentile 25	Above Percentile 50	Above Percentile 75	Voucher	Academic Selection
	[1]	[2]	[3]	[4]	[5]	[6]
			5 days bandwidths			
with controls						
	0.042***	0.007	0.023***	0.013**	0.027***	0.016**
	(0.012)	(0.005)	(0.003)	(0.005)	(0.007)	(0.006)
Observations	33590	33855	33855	33855	33855	14836
Mean	.33	.75	.5	.26	.53	.65
without controls						
	0.064***	0.017***	0.032***	0.022**	0.020***	-0.004
	(0.013)	(0.005)	(0.005)	(0.007)	(0.005)	(0.011)
Observations	33644	33908	33908	33908	33908	14863
Mean	.33	.75	.5	.26	.53	.65
			10 days bandwidths			
with controls						
	0.042***	0.011**	0.022***	0.023***	0.018***	0.020***
	(0.009)	(0.005)	(0.004)	(0.004)	(0.006)	(0.005)
Observations	67870	68390	68390	68390	68390	30000
Mean	.33	.75	.5	.26	.53	.65
without controls						
	0.053***	0.016**	0.024***	0.027***	0.013*	0.006
	(0.013)	(0.006)	(0.007)	(0.006)	(0.007)	(0.009)
Observations	67986	68507	68507	68507	68507	30048
Mean	.33	.75	.5	.26	.53	.65
			15 days bandwidths			
with controls						
	0.035***	0.012**	0.018***	0.017***	0.016***	0.018***
	(0.008)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	101595	102338	102338	102338	102338	44283
Mean	.33	.75	.5	.26	.53	.65
without controls						
	0.052***	0.018***	0.023***	0.023***	0.012**	0.005
	(0.011)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
Observations	101757	102503	102503	102503	102503	44353
Mean	.33	.75	.5	.26	.53	.65

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

The specification without controls comprises a linear specification based on the day of birth, which can differ on each side of the threshold, and year of birth fixed effects. The specification with controls further incorporates variables such as municipality, parents' education, gender, weekday of birth, and a dummy variable for being born on a holiday.

Table C.10: Reduced form anaysisis: School Inputs

	Avg. Parents' Education	Avg. Classmates' SSA	Teachers Age	Teachers Full-time	Teachers Teaching / Total hours	Teachers 4-year College
	[1]	[2]	[3]	[4]	[5]	[6]
			5 days bandwidths			
with controls						
	0.103***	0.026***	-0.128*	0.003	0.001	0.009*
	(0.025)	(0.006)	(0.068)	(0.002)	(0.001)	(0.004)
Observations	33852	33714	31548	31549	31549	26113
Mean	11.31	6.29	45.84	.32	.82	.64
without controls						
	0.139**	0.026***	-0.085	-0.000	-0.000	0.008**
	(0.046)	(0.004)	(0.092)	(0.002)	(0.001)	(0.003)
Observations	33905	33769	31601	31602	31602	26159
Mean	11.31	6.29	45.84	.32	.82	.64
			10 days bandwidths			
with controls						
	0.115***	0.030***	-0.222***	0.005**	-0.006**	0.009**
	(0.018)	(0.003)	(0.071)	(0.002)	(0.002)	(0.003)
Observations	68381	68081	63838	63840	63840	52797
Mean	11.31	6.29	45.84	.32	.82	.64
without controls						
	0.133***	0.028***	-0.137*	0.004*	-0.007***	0.008***
	(0.037)	(0.003)	(0.075)	(0.002)	(0.002)	(0.003)
Observations	68498	68197	63954	63956	63956	52890
Mean	11.31	6.29	45.84	.32	.82	.64
			15 days bandwidths			
with controls						
	0.098***	0.030***	-0.259***	0.004	-0.004**	0.012***
	(0.017)	(0.003)	(0.065)	(0.002)	(0.002)	(0.003)
Observations	102328	101901	95457	95461	95461	79042
Mean	11.31	6.29	45.84	.32	.82	.64
without controls						
	0.141***	0.029***	-0.221***	0.004**	-0.005***	0.011***
	(0.032)	(0.002)	(0.076)	(0.002)	(0.002)	(0.003)
Observations	102493	102064	95618	95622	95622	79171
Mean	11.31	6.29	45.84	.32	.82	.64

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by day of birth.

The specification without controls comprises a linear specification based on the day of birth, which can differ on each side of the threshold, and year of birth fixed effects. The specification with controls further incorporates variables such as municipality, parents' education, gender, weekday of birth, and a dummy variable for being born on a holiday.

C.3 Results using January-July cutoffs

Table C.11: Impact of SSA on school characteristics. February-July cutoffs.

	Avg. SIMCE	Above Percentile 25	Above Percentile 50	Above Percentile 75	Voucher	Academic Selection
	[1]	[2]	[3]	[4]	[5]	[6]
			RD estimates for two data driven bandwidths			
			MSE-optimal bandwidth			
	0.055	-0.018	0.047**	0.040**	0.039**	0.033
	(0.037)	(0.021)	(0.019)	(0.019)	(0.017)	(0.020)
Observations	409484	412476	412476	412476	535654	119068
Mean	.32	.75	.5	.25	.52	.64
Weak Instr. (a)	32.8	32.8	32.8	32.8	43.1	28.5
			CER-optimal bandwidth			
	0.096**	-0.003	0.055***	0.059***	0.037*	0.023
	(0.040)	(0.022)	(0.020)	(0.021)	(0.020)	(0.023)
Observations	326785	329183	329183	329183	412476	104030
Mean	.32	.75	.5	.25	.52	.64
Weak Instr. (a)	26.4	26.3	26.3	26.3	32.8	25.3

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For the outcome of switching school, also years since first enrollment fixed effects are included. For each of the outcomes, two data driven bandwidths are used. These bandwidths are reported in Table A.1.
(a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

Table C.12: Impact of SSA on school Inputs. February-July cutoffs cutoffs.

	Avg. Parents' Education	Avg. Classmates' SSA	Teachers			
			Age	Full-time	Teaching / Total hours	4-year College
	[1]	[2]	[3]	[4]	[5]	[6]
	RD estimates for two data driven bandwidth					
	MSE-optimal bandwidth					
	0.067	0.050***	-0.592**	0.022**	-0.013	0.042***
	(0.075)	(0.006)	(0.250)	(0.010)	(0.008)	(0.015)
Observations	452224	492475	458208	352884	352884	235349
Mean	11.28	6.26	45.93	.32	.82	.60
Weak Instr. (a)	35.8	39.3	48.7	36.8	36.8	26.7
	CER-optimal bandwidth					
	0.106	0.050***	-0.450*	0.033***	-0.013	0.037**
	(0.083)	(0.006)	(0.267)	(0.013)	(0.009)	(0.016)
Observations	370405	410921	352868	281475	281475	206103
Mean	11.28	6.26	45.93	.32	.82	.60
Weak Instr. (a)	29.4	32.9	36.8	29.2	29.2	23.1

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

Additional controls: municipality, year of birth, parents' education, gender, weekday of birth, and born on a holiday dummies. For the outcome of switching school, also years since first enrollment fixed effects are included. For each of the outcomes, two data driven bandwidths are used. These bandwidths are reported in Table A.1.
(a) Effective F statistic (Montiel-Pflueger robust weak instrument test). Critical values for 5, 10 and 20 percent maximal IV bias are 37.41, 23.11, and 15.06, respectively.

C.4 Placebo analysis. Reduced Form estimates for a threshold at the 1^{st} of September or 1^{st} of October.

Table C.13: Impact of SSA on school characteristics. Placebo analysis.

	Avg. SIMCE	Above Percentile 25	Above Percentile 50	Above Percentile 75	Voucher	Academic Selection
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: September and October 1st cutoffs. 10 days bandwidth; without controls.						
	0.000	-0.005	-0.004	0.001	-0.002	-0.007*
	(0.014)	(0.005)	(0.006)	(0.006)	(0.005)	(0.004)
Observations	169504	171155	171155	171155	171155	67840
Mean	.36	.76	.51	.26	.52	.65
Panel B: September and October 1st cutoffs. 10 days bandwidth; with controls.						
	0.002	-0.005	-0.004	0.002	-0.003	-0.003
	(0.008)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
Observations	169286	170935	170935	170935	170935	67748
Mean	.36	.76	.51	.26	.52	.65

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

The specification without controls comprises a linear specification based on the day of birth, which can differ on each side of the threshold, and year of birth fixed effects. The specification with controls further incorporates variables such as municipality, parents' education, gender, weekday of birth, and a dummy variable for being born on a holiday.

Table C.14: Impact of SSA on school Inputs. Placebo analysis.

	Avg. Parents' Education	Avg. Classmates' SSA	Teachers			
			Age	Full-time	Teaching / Total hours	4-year College
Panel A: September and October 1st cutoffs. 10 days bandwidth; without controls.						
	0.001	-0.002	0.089	-0.001	-0.000	-0.008**
	(0.036)	(0.003)	(0.076)	(0.003)	(0.002)	(0.003)
Observations	171135	170413	143634	143637	143637	118364
Mean	11.34	6.28	45.78	.32	.82	.64
Panel B: September and October 1st cutoffs. 10 days bandwidth; with controls.						
	0.004	0.000	0.065	-0.001	-0.001	-0.006*
	(0.017)	(0.001)	(0.054)	(0.002)	(0.001)	(0.003)
Observations	170915	170194	143419	143422	143422	118196
Mean	11.34	6.28	45.78	.32	.82	.64

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by day of birth.

The specification without controls comprises a linear specification based on the day of birth, which can differ on each side of the threshold, and year of birth fixed effects. The specification with controls further incorporates variables such as municipality, parents' education, gender, weekday of birth, and a dummy variable for being born on a holiday.