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Renato Herdeiro

Alison Oliveira

Naercio Menezes Filho

Renato Herdeiro
Insper Instituto de Ensino e Pesquisa
Cátedra Ruth Cardoso
Rua Quatá, nº 300
04546-042 – São Paulo, SP – Brasil
ren.herdeiro@gmail.com

Alison Oliveira
Insper Instituto de Ensino e Pesquisa
Cátedra Ruth Cardoso
Rua Quatá, nº 300
04546-042 – São Paulo, SP – Brasil
alisonpablo@gmail.com

Naercio A. Menezes Filho
Insper Instituto de Ensino e Pesquisa
Cátedra Ruth Cardoso
Rua Quatá, nº 300
04546-042 – São Paulo, SP – Brasil
naercioamf@insper.edu.br

The Effect of Age at School Entry on Education and Labor Market Outcomes: A Regression Discontinuity Analysis

Renato Herdeiro[*] Alison Oliveira[**] Naercio Menezes-Filho[***]

Abstract

This paper uses administrative data from Brazil to examine the effects of a delayed school entrance on years of schooling, college admission and early labor market indicators. Regression discontinuity design estimates reveal that students born immediately after the school entry cutoff date accumulate 0.36 more years of education ten years after first school enrollment and finish high-school 8 months earlier, but leave formal education 0.3 year older than those born before the cutoff. We find that early school entrance does not impact formal sector employment, but does raise formal wages. Our results also show that age at school entry does not impact the probability of students entering college soon after completing their basic studies. Overall, the results suggest a trade-off between starting school younger or older in terms of education and labor market prospects.

Jel Classification: I21, J13, J24

Keywords: Education; School entry age; School performance; Wages.

[*] Insper and University of Campinas (IE-UNICAMP). E-mail: ren.herdeiro@gmail.com
[**] Fundação Instituto de Pesquisas Econômicas – Fipe. E-mail: alison.oliveira@fipe.org.br
[***] Insper and University of São Paulo (FEA-USP). E-mail:naercioamf@insper.edu.br

1. Introduction

The debate about the effects of school entry age is important and still unresolved. It is relevant not only from the educational and socioeconomic perspective of families and individuals, but also from its institutional approach, as a regulation directly linked to public legislation (Black et al., 2011). It is influential because contributions on the matter have encouraged parent's decision to intentionally delay children's first admission in elementary school ("red-shirting", Dhuey et al., 2019). And it is disputable because the empirical evidence on the subject has failed to converge to a single answer regarding the impacts of school entry age on long-term educational and labor market outcomes.

There is stronger consensus concerning the effects of school entry age on short-term education results, however, since many studies from distinct countries have consistently documented that older students do tend to perform better than their younger counterparts in the first part of the school cycle (Bedard & Dhuey, 2006; Crawford et al., 2007; McEwan & Shapiro, 2008; Elder & Lubotsky, 2009). Whether this pattern unfolds into better educational and labor market long-run outcomes is a thesis that holds much more controversy.

In this paper, we shed more light on this debate by using longitudinal data of students from the 26 Brazilian State capitals. Our setup allow us to track these students not only through their school cycles, but also across different data sets in order to examine how their age at school entry relates to college admission and early labor market performance using a Regression Discontinuity Strategy. We find that in the 10th year after school entry, which is the year of conclusion for those who progressed regularly through the school cycles, those with a delayed entrance accumulate 0.36 more complete years of education than their counterparts. This gap in educational level between both groups of students essentially disappears in the 13th year after school entrance, as most of the advanced students have already left school and the delayed ones catch up. We also find that children born after the cutoff date for school enrollment complete their studies 0.67 year quicker and leave school 0.3 year older than those with an early entrance.

Regarding labor market outcomes, we discover that students who started school earlier tend to have higher formal wages from the 2nd year after leaving formal education onwards, despite not being more experienced in this specific labor market, since the school entry age does not seem to affect formal employment. Considering the importance of informal labor contracts as a means of career advancements and its prevalence among the youngest components of the labor force (Seminario-Amez, 2021), this evidence suggests that these students were able to acquire experience in the informal labor market in the first year after high-school conclusion, while older school starters were still finishing their studies. To the best of our knowledge, this relation between the school entry age and informality has not been documented so far in the related literature.

We also find that having a delayed school entrance does not impact ingress in superior education right after school egress, since we find no significant difference in the probability of being enrolled in higher education institutions between both sets of students. Overall, these transition results suggest that while those with a delayed entrance fare better indeed from an average school duration point of view, it seems like starting school older does not impact the "quality" of school learning enough to either raise these youngsters productivity in the labor market, or raise their chances of enrolling in superior education institutions right after school completion.

Therefore, our results are more in line with the interpretation that the early edge in terms of educational outcomes found between the two groups of students should dissipate through time (Crawford et al., 2007, 2014; Dobkin & Ferreira, 2010; Nam, 2014). Moreover, our evidence emphasizes the relevance of the trade-off in terms of educational and labor market prospects imperative to this analysis setup, since having an extra year at home or preschool (and performing better in school life) also means having a somewhat delayed entrance in labor market that is a direct consequence of starting school older (Black et al. 2011).

The remaining of the article is organized as follows. Section 2 presents a conceptual framework analysis that relies on the theoretical and empirical literature to access the debate about the effects of school entry age. Section 3 details the data sources, section 4 presents the econometric strategy and some validation tests for the RDD technique. Section 5 presents the main results, split into two subsections: the first with results related to the student's school cycles, and the second accessing labor market and higher educational outcomes. Finally, section 6 concludes.

2. Conceptual Framework

The goal of this section is to access the controversy on the school entry age empirical literature mentioned in the opening section to present the debate that circumscribes our work. Since this controversy is related to the mechanisms through which the school entry age might affect the student's performance (Shigeoka, 2015), it is wise to approach this effort by relying on recent work to acknowledge that there are three main drivers of the contrast in performance of children who enroll in school at distinct ages (Cascio & Schanzenbach, 2016; Crawford et al., 2014; Black et al., 2011; McEwan & Shapiro, 2008). [1]

First, we have what is called the "age at test" effect, which refers to the fact that students in the same classroom are tested simultaneously despite being up to a year younger or older than each other (Peña, 2016). Second, there is the "strict age of starting school" [2] effect (Crawford et al., 2014), which states that children who ingress in formal education in different ages will relate with that experience in different ways precisely because they are each of a specific absolute age. And third, the "relative age" effect, which assesses that a student's age in relation to their classmates should affect his outcomes through social interactions and spillovers (McEwan & Shapiro, 2008). Since the "age at test" effect is also an absolute age one, we could split these three effects into two groups, one comprising of the first two effects mentioned, and the other group consisting of the latter.

One should note that all of these are intrinsically related (Crawford et al., 2014), and that the challenge of dissociating between these effects is a characteristic that marks this entire research agenda. Nonetheless, despite them being three distinct effects, considering that the branch of this research field focused on addressing the way these two absolute effects work is the one that has drawn more attention in the literature (Cascio & Schanzenbach, 2016) and relates directly to the controversy mentioned above, this elaboration will be centered on it.

The common idea behind them is that, on being absolute, both are deeply related to the maturity of the student (Bedard & Dhuey, 2006). While through this reasoning they concur in the assimilation that older school starters should indeed outperform younger ones in the school early years, as has been well documented by this research agenda (Shigeoka, 2015), they still dispute on what should be the expected behavior of this early advantage through time.

This setup is the result of two main theoretical frameworks. The "strict age at school entry" effect orientation on Cunha and Heckman (2007) model of skills accumulation[3] and the

[1] There are some nuances in the way we opt to decompose the delayed/early school entrance effects as a whole in comparison to some of the referred articles. Crawford et al. (2014) will split these into four, not three mechanisms. Black et al. (2011) differentiates the "age at test" effect from the "school starting age" effect and also emphasizes the way the relative age might impact the students outcome, but does not draw this explicit three-way division. McEwan & Shapiro (2008) and Cascio & Schanzenbach (2016) are the ones that actually rupture these into the three distinct effects.

[2] We utilize the adjective "strict" to characterize this specific "age of starting school" effect because in this case we are not referring to the simultaneous happening of the relative and absolute effects related to the school entry age that manifest once a student has an early/delayed entrance in formal instruction. In this particular theoretical framework, the strict "age of starting school effect" refers to how the absolute age effect at the moment of enrolling school might influence the student's performance (McEwan & Shapiro, 2008).

[3] Fundamentally, this model asserts that the skills of an individual result from a combination of innate skills and human capital investments carried out in different phases of childhood. Throughout the key properties of "self-production of skill" and "dynamic complementarity of skills" these authors endorse both, the idea that older

understanding that older school starters should benefit from their school "readiness"[4] (Stipek, 2002) in order to greater absorb what formal education has to offer (and achieve life lasting better outcomes in relation to their younger peers) (Elder & Lubotski, 2009). And the "age at test" effect comprehension that if the contrast in test scores in favor of those with a delayed entrance reflected mostly a difference of age and maturity at the time of the test application, this would not mean that older students had learn "better" than younger ones (Dee & Silverstein, 2018; Fredriksson & Öckert, 2013). Moreover, the idea is that this contrast should dissipate over time because "a year of maturation represents more learning among young children than among adolescents or adults" (McEwan & Shapiro, 2008, p. 3).

This dispute gained relevance among the literature with contributions that mutually pointed towards the acknowledgement of the "age at test" effect as the main component operating behind the contrast between older and younger school starters performance differential found within their estimations (Black et al., 2011; Crawford et al., 2007, 2014). In one side, somewhat corroborated by other studies that found no significant impact of the school entry age on long term outcomes (Dobkin & Ferreira, 2010; Nam, 2014), they have questioned the core interpretation of the "strict age at school entry" effect that this early school lead would necessarily translate into persistent and significant long-run benefits for the older school starters. On the other side, however, one cannot neglect also the good range of studies pointing towards the relevance and persistence of a somewhat edge for students with a delayed school entrance on late schooling, labor market, and even other adulthood related outcomes (Kawaguchi, 2011; Zweimüller, 2013; Fredriksson & Öckert, 2013; Peña, 2016), even though these effects also vary significantly within these works[5].

Still thinking about the processes through which the absolute age of students might affect their outcomes throughout life, there is also a particular point among this discussion that deserves attention because it sees to the way in which starting school at a later age also means having a somewhat delayed entrance in the labor market. Black et al. (2011) present this as an explicit trade-off between having an extra year at home or preschool (and allegedly performing better in early school years) versus entering the market a year older and compromising an

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children would actually perform better in school's first grades, and the assumption that this edge would persist throughout the whole school cycle and pass on into adulthood (Cunha & Heckman, 2007).

[4] According to Stipek (2002, p.4), the argument in favor of a delayed school entrance assumes that "with age come competencies that will improve children's chances for success in school." The authoress emphasizes how these comprehend not only intellectual competencies, but also other "dimensions of development", such as "physical well-being; motor, social and language development" (Stipek, 2002, p.4).

[5] As suggested by Peña (2016), we acknowledge the documented differences in magnitudes and directions of a delayed school entrance effects on labor market outcomes found across the literature might be partially explained by each countries particular labor market structures and distinct system of educational opportunities allocation.

earlier entrance in the labor market that should be of high relevance from a human capital investment perspective. If this early performance edge would not translate into long lasting advantages for these older school starters, this framework suggests that they could be worse off upon leaving formal education and starting their working life (Black et al., 2011).

This is a crucial point of interest for our work since the school egress evidence we found pointed precisely to a contrast in terms of labor market and educational prospects of having a delayed/early entrance in school. As has been said in the article opening section, we are not able to dissociate the effects we find, nonetheless, it is with emphasis on this particular result setup that we seek to contribute to the maturing of the whole controversy described in this section.

3. Data

This article was built with the use of administrative data provided by the Ministry of Education, Labor Ministry, and the Brazilian Institute of Geography and Statistics (IBGE). Regarding the Ministry of Education, besides making use of their public access microdata concerning the national elementary School Census (Censo Escolar do Ensino Básico) and Higher Education Census (Censo da Educação Superior), we have also utilized their Service of Access to Protected Data (Sedap), which allows access to a protected database produced by the institute upon solicitation.[6] From Brazilian Labor Ministry we made use of the identified RAIS database, which is an annual data set on all employees in the formal labor market in Brazil, and from IBGE we made use of the PNADC, Brazil's National Household Survey.

As mentioned in the introduction of this paper, our results are split into two main subsections, and there is a contrast in the data sources each of them utilizes. The first of them relies solely on the use of School Census microdata from 2007 to 2017, concerning specifically data from Brazil's state capitals. This rich database produced administratively by schools contains individualized information on all elementary school admissions from 2007 to 2017, along with an identification variable that enables longitudinal monitoring of students since 2007 (although their anonymity is preserved). This setup was crucial for investigating the effects of age at school entry on the complete years of education, retention rate and the duration of time in required education, the main indicators we analyze in section 5.1.

[6] Through Sedap researchers may access confidential databases whether for institutional or scientific purposes. To obtain access a research project was submitted to the institute and analyzed accordingly. After Inep approved the project, the headquarters of the institute was visited multiple times in Brasilia, and the research was carried out in a secure office, where a computer and statistical software (R, SAS and Stata) were provided.

Meanwhile, in order to assess the effects of age at school entry on superior education and labor market outcomes, as shown in section 5.2, we followed this same group of students across the Higher Education Census and the identified RAIS database from 2014 through 2017.[7] Moreover, despite not being able to further chase them across the PNADC survey, we utilize this particular database to produce a few final labor market results that support our findings.

Overall, we follow two distinct cohorts in this paper, each of them allowing us to compare students in the limits of discontinuity generated by the relation between their dates of birth and the cutoff dates of school enrollment. Students around this discontinuity have practically the same age, but are first enrolled in school with a year difference. This means that our cohorts will consist of students born shortly before the date limit set for entering the formal education system (enrolled elementary school in the year t), and students born right after this same cutoff date (enrolled in the year $t + 1$).

More specifically, our cohorts consist of students who had first enrolled in elementary school in 2007/2008 and 2003/2004. The latter cohort enables us to look further away in these students' outcomes, since the most recent year of available data we utilize is 2017. However, while precise information regarding 2007/2008 entrants could be directly observed in the School Census microdata available for each of these years, this was not the case for students that first enrolled elementary school in 2003/2004, since individual-level data is available only from 2007 onwards.

Fortunately, given that a significant proportion of students conclude elementary school at the ideal age, the discontinuity observed in admissions in the first year of elementary school can still be observed along the following grades, though more attenuated overtime. Therefore, we could identify the date limits set for elementary school enrollment in 2003/2004 according to the date of birth of students that enrolled in the fifth (2003 entrants) or fourth grade (2004 entrants) of elementary school in 2007. The procedure for obtaining both cohorts will be detailed below.

In terms of education-related outcomes, we opted to compare the performance of students exposed to the same education time since their admission into elementary school. This means that to assess how a delayed entrance influences the performance of 2007/2008 entrants in the second grade, for example, results from incoming students in 2007 at the end of 2008 were

[7] At Inep, in Brasília, we were able to track the same students along different databases kept by the institution through confidential identification keys. Also, for us to be able to follow these students throughout RAIS as well, data from the identified RAIS database with social security numbers of employees was sent to Inep so they could treat the database in a way we could temporarily access the information we needed. The anonymity of individuals was kept during the whole process of conducting this research.

compared to those of incoming students in 2008 at the end of 2009. Since 2017 is the last year available in our data sources, this means that we could follow the 2007/2008 cohort until the 9th year after first school enrollment, and the 2003/2004 cohort until the 13th year after first school enrollment. Emphasis on the 10th year after first school enrollment is particularly important in the interpretation of our results because it represents the last year of elementary schooling for those that progressed regularly through the school cycle, for 11 years of study is the ideal school duration we are working with.[8] In terms of labor market outcomes, however, we compare students in the same year of the RAIS and PNAC database. Since these students are essentially of the same age, it is important to compare them at the same moment in time when accessing the overall effect of their age at school entry in terms of labor marker outcomes.

4. Econometric Methodology

4.1 Cohort building: criteria for school enrollment

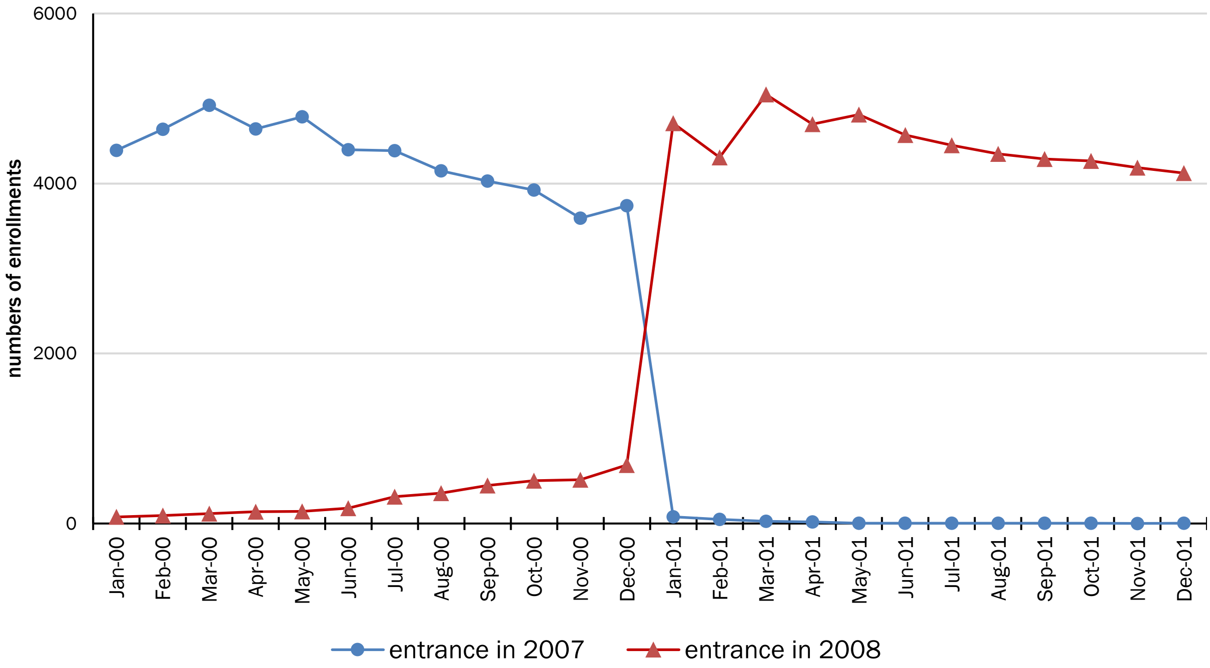
In Brazil's education system local governments (states and municipalities) have a relative autonomy when it comes to the implementation of education policies (Rosa et al., 2019). Moreover, the limit date set for elementary school enrollment differs across not only states, cities and education systems in a giver year, but also *across* years within the same city or education system. Therefore, we had to develop two similar but distinct strategies to find each cohort school entry criteria. Concerning the search for each city limit date set for enrollment in the school system, Figure 1 presents an example of its identification. It shows the number of first grade enrollments in the municipal school system of São Paulo per month for all children born between January 1st of 2000 and December 31st of 2001.

Amongst students that entered the school system in 2007, 3,739 were born in December of 2000, but only 78 were born in January of 2001. Simultaneously, amongst those that entered the school system in 2008, only 687 were born in December of 2000 whereas 4,711 were born in January of 2001. Such variations between December of 2000 and January of 2001 illustrate that the municipal educational network system of São Paulo city determined that children that turned seven years old by December 31st of 2007 should have been enrolled in elementary

[8] As will be detailed in the following section, the period we are analyzing was marked by a context of heterogeneous local implementation of a federal curriculum reform promulgated in 2006 that expanded the basic education first cycles duration from 8 to 9 years. Even so, it must be stressed that the standardization of all the analyzed youngster's respective cycles duration to 8 years was a part of our data treatment process. These 8 years combined with the 3 years of high school that are part of the curriculum add up to a total of 11 year ideal period of basic formal instruction.

school in the same year. Children born from January 1st of 2001 onward should have been first enrolled in elementary school in the following year (2008).

Figure 1 – Number of enrollments per month of birth in São Paulo city



Note: Number of enrollments (y-axis) per month of birth (x-axis) and per year of first enrollment in elementary school (different colors/forms) of the municipal education network of São Paulo City. Source: Microdata of the School Census

Although it is possible to visually identify the limit date set for enrollment as shown above, tests of structural breaks in time series were implemented to determine each date limit set for enrollment in elementary school per month of birth and location. For this analysis, the methodology of structural break and discontinuity identification was applied as previously described by Card et al. (2008) and Ozier (2018). Given a cohort of children that enrolled in elementary school in the municipal education network of a certain city m, in the years t or $t + 1$, 25 possible dates were tested as the limit date set for enrollment, starting at every day 01 of each month of 01 of January of the year $(t-7)$ until 01 of January of the year $(t-5)$[9]. For each date, the following models were estimated to analyze how "being born afterwards this date" predicted a school enrollment in the year $t+1$:

$$T_i = \alpha + \beta_j \, \mathbf{1}[n_i \geq c_j] + \epsilon_i \quad \forall\, i \mid (c_j - 90) \leq n_i \leq (c_j + 90) \qquad \textbf{(1)}$$

[9] More specifically, in the pursuit of the cutoff date for cohort 2007/2008, all first days of each month, from January 2000 to January 2002, were tested.

Where:

T_i is a variable that assumes the value 0 if the child i enrolled in elementary school at the year t and value 1 if the child enrolled at the year t+1

n_i is the date of birth of the child i

c_j is the tested limit date set for elementary school enrollment

ϵ_i is the random error term

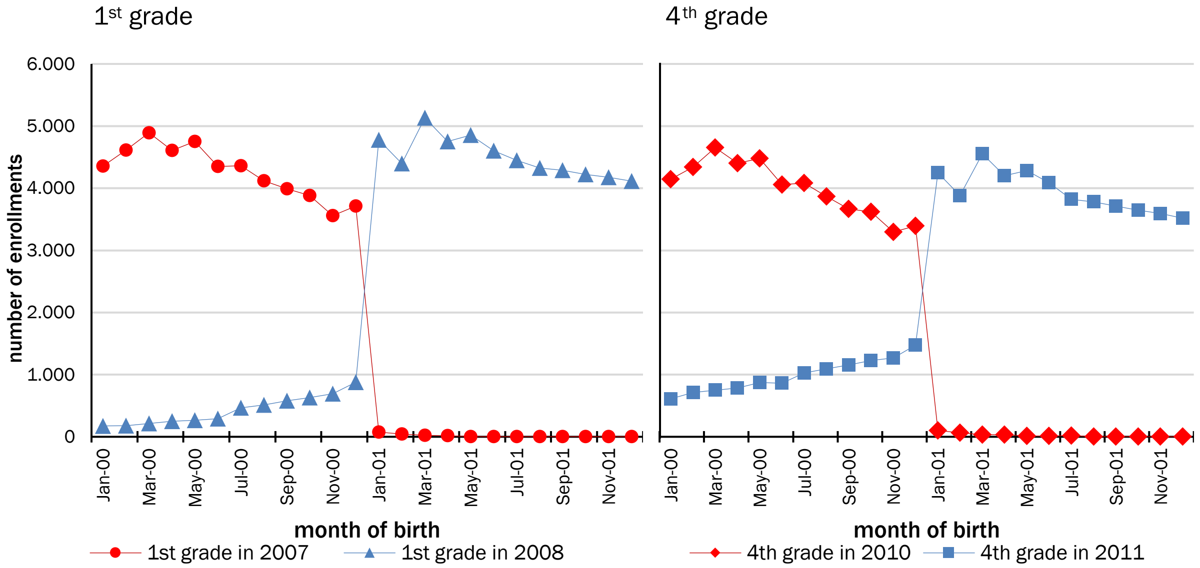
After model estimation, the date $\boldsymbol{c}^* = c_j$ which maximizes the R^2 of equation (1) was selected as cutoff for entrance in elementary school. Hansen (2000 *apud* Card *et al.*, 2008) demonstrates that if Equation 1 is correctly described, this procedure produces a consistent estimative of the true limit date for school enrollment, $\boldsymbol{c}^*$. As an example, Table 10 and Table 11 in Appendix A present the results of this procedure for the city of São Paulo, and for each state capital in Brazil, respectively.

After obtaining each cutoff date, the data was pooled to build our 2007/2008 cohort, and for this part of the process we adopted the standard methodology utilized in the literature to deal with the heterogeneity of the limit dates set for school enrollment in the counties. The dates of birth were normalized so that dates of school enrollment in all counties became zero and the dates of birth became distances in days in relation to the date limit set for school enrollment[10]. Estimations were performed considering individuals with normalized dates of births belonging to the interval$[-180, +180]$.

We adopted a similar yet particular strategy to find the date limit set for the cohort of students who first enrolled in elementary school in 2003/2004. As mentioned in section 3, even though there is no microdata of the School Census before 2007, it is possible to identify these cutoff dates by observing the distribution of enrollees (by date of birth) in the following school grades of elementary school. Estimates are possible because a significant part of students progress regularly through the school system without failures, retentions or evasions. This means that the discontinuities observed in the first grade of elementary school could also be identified in the 4th grade, although weakened overtime, as shown in Figure 2. In that way, it is possible to infer the limit date set for school enrolment in 2007/2008 either by observing the 2007/2008 first grade enrollments or the fourth-grade enrollees of 2010/2011.

[10] A detailed description of the standard procedure for data normalization can be found in Cattaneo et al. (2016).

Figure 2 – Number of enrollments in the county education network of São Paulo city by month of birth



Note: Number of first- and fourth-year elementary school enrollments by month of birth in the county education network of São Paulo city in 2007/2008 and 2010/2011, respectively. The figure shows how the observed discontinuity in first year school enrollment can also be observe three years later, although slightly attenuated, in the fourth grade. Source: Microdata of School Census.

Therefore, while it is not possible to observe enrollments in the first grade of the 2003/2004 students (there is no School Census microdata for 2003 or 2004), it is possible to determine enrollment in the fourth or fifth grade of 2007 to single out the dates set for school enrolment for this cohort. Once again, we rely on the estimation of structural breaks in the number of enrollments by month of birth with the same statistical procedure described above to find each cutoff date accordingly. Twenty-five possible dates set for school enrolment were tested for the municipality education system of each state capital.

More specifically, every first day of each month from January 1st of 1996 to January 1st of 1998 was tested. For each one of those possible limit dates, a regression following equation 1 was estimated, with the main difference that in this case we are accessing the probability of a child being enrolled in the 4th or 5th grade of elementary school in year 2007. Table 12 in Appendix A presents the estimated limit dates set for elementary school enrolment in the years 2003/2004 by each county's education network regarding the state capitals of Brazil. Finally, the limit dates set for elementary school enrolment for each municipality were also realigned through the normalization process mentioned above, so that data from all state capitals of Brazil could be analyzed together.

4.2 Identification Strategy

Once the cohort building procedure was set, we could then turn to our identification strategy. The Regression-Discontinuity Design has been widely utilized among this literature to produce evidence on the effects of age at school enrollment on a series of distinct outcomes (McEwan & Shapiro, 2008; Crawford et al., 2014). The basic notion behind this methodology revolves around the existence of a regulation assigned in a way that divides individuals above and below a specific threshold, forcing only a certain number of individuals to receive a specific treatment. The idea is that if these regulations attribute differences in treatment of individuals in a random manner and the pre-determined characteristics of them (observed and non-observed) evolve continuously, this means that individuals right below the cutoff line represent a good counterfactual to individuals right above the same regulation limit line. Therefore, by comparing these two groups, it becomes possible to measure the causal effects of interventions linked to these regulations (Crawford et al., 2014).

On employing this methodology for our particular case of study, from a formal standpoint we assume that: D_i is a binary variable that equals one if the individual i received a treatment (enrolled in elementary school relatively later) and zero if it did not receive this same treatment; c^* represents the cutoff date for the rule that makes the individual eligible for that treatment; n_i represents the date of birth of the individual i. There will be a discontinuity in the probability of the individual i receiving a treatment if

$$\lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] \neq \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] \tag{2}$$

The mean treatment effect on the result variable D can be calculated with the discontinuous *fuzzy* regression technique, which will be given by

$$\frac{\lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[Y_i|\mathrm{n_i} = \mathrm{c}^*] - \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[Y_i|\mathrm{n_i} = \mathrm{c}^*]}{\lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] - \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*]} \tag{3}$$

The model described above was estimated non-parametrically by local linear regression following Imbens and Lemieux (2008), Lee and Lemieux (2010), Imbens and Kalyanaraman (2012), Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019) and Calonico, Cattaneo and Farrell (2020). Following standard assumptions, Imbens and Lemieux (2008) demonstrate that these estimates will be an average treatment effect for discontinuity units and for children affected by the limit dates set for enrollment (compliers). These estimates are also known as the local average treatment effects (LATE).

While this procedure works fine with the 2007/2008 cohort, that is not the case when dealing with data for children who first enrolled elementary school in 2003/2004. Once we cannot observe which students enrolled for the first time in elementary school before or after the cutoff date for those years, we cannot estimate the *Fuzzy RDD* models to produce the local average treatment effects as described above. However, we can still use the *Sharp* discontinuity design as a tool to estimate intention to treat effects (ITT) when dealing with this cohorts, as in Dobkin and Ferreira (2010).

Following equation 3 above we have that *Sharp RDD* is a particular *Fuzzy RDD* case when

$$\lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] - \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] = 1 \quad \textbf{(4)}$$

Therefore, the average treatment effect calculated by S*harp* regression discontinuity design is given by

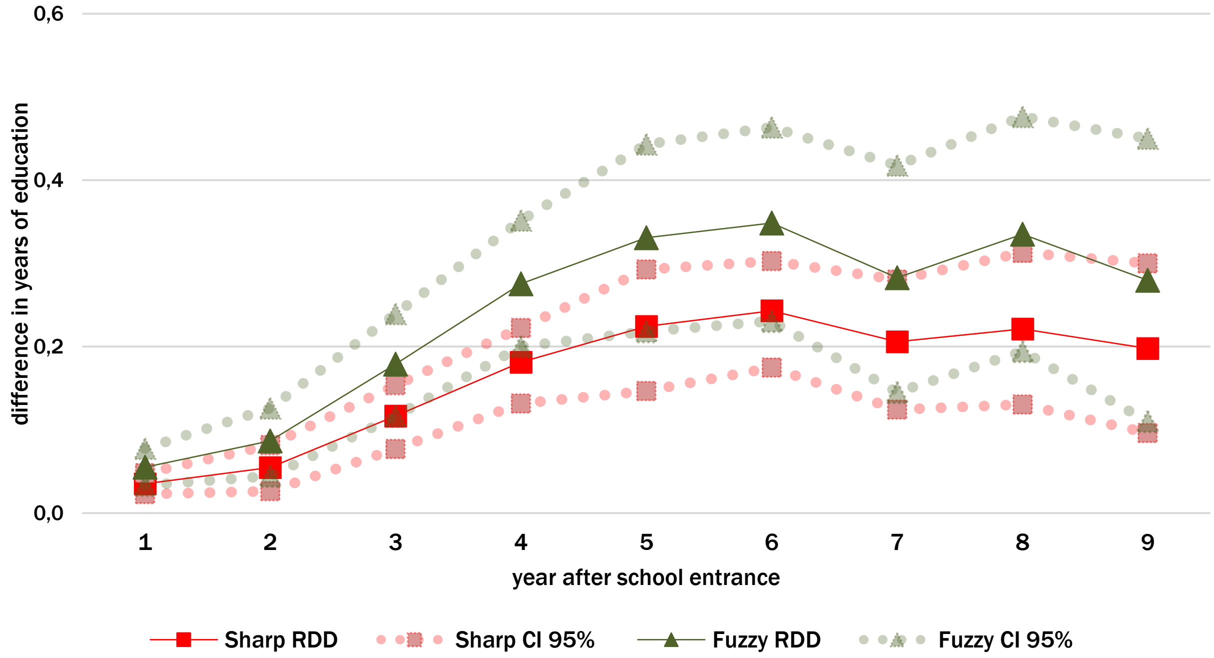
$$\sigma_{sharp} = \lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[Y_i|\mathrm{n_i} = \mathrm{c}^*] - \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[Y_i|\mathrm{n_i} = \mathrm{c}^*] \quad \textbf{(5)}$$

Assuming homogeneity of treatment effect at the region of discontinuity and given that differences in probability of treatment around discontinuity is less than 1 in the *Fuzzy RDD case*:

$$\lim_{\mathrm{n}\downarrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] - \lim_{\mathrm{n}\uparrow \mathrm{c}^*} E[D_i|\mathrm{n_i} = \mathrm{c}^*] < 1 \Rightarrow \sigma_{fuzzy} > \sigma_{s} \quad \textbf{(6)}$$

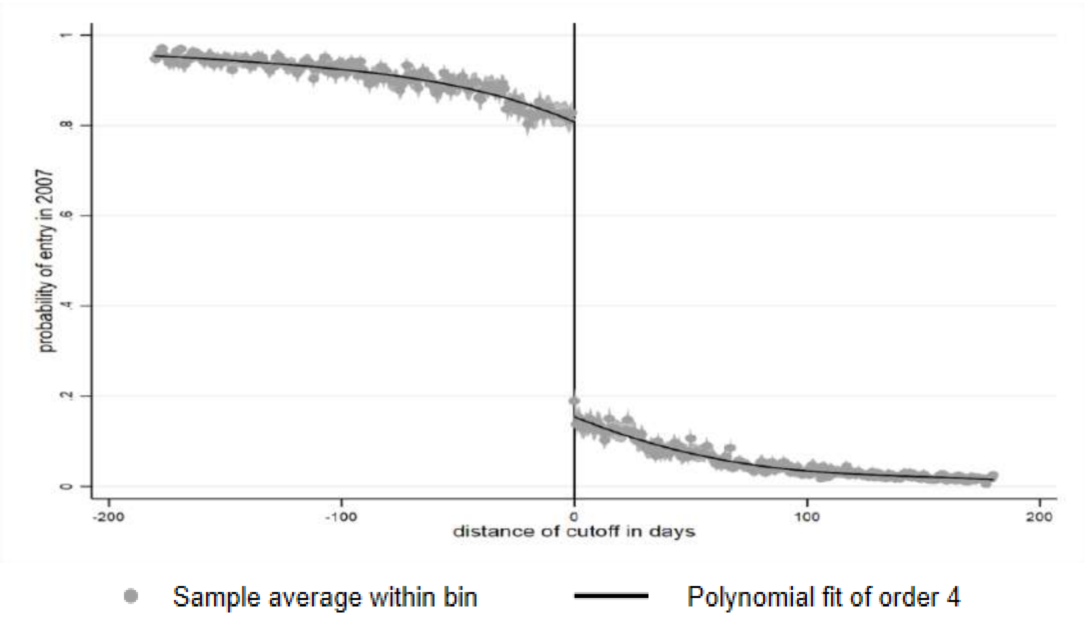
Therefore, *Fuzzy RDD* estimates would be obtained by dividing *Sharp RDD* estimates by the limit of the difference between probabilities of treatment in discontinuity, which is always lower than one. This means that it is possible to interpret the results built with the *Sharp RDD* and the 2003/2004 cohorts as lower bounds of the long-term effects of age at first elementary school enrolment, as can be seen in Figure 3. In it, we present a comparison of results that will be properly displayed in section 5 with the use of both methodologies.

Figure 3 – Comparison between Sharp RDD and Fuzzy RDD methodologies



Note: Comparison between *Fuzzy RDD,* and *Sharp RDD* methodologies on the impact of age at enrolment on complete years of study for the 2007/2008 cohort. Non-parametric estimations by local linear regression and triangular kernel function. Source: Microdata of the School Census.

Figure 4 – Probability of school enrollment in 2007 for the 2007/2008 cohort

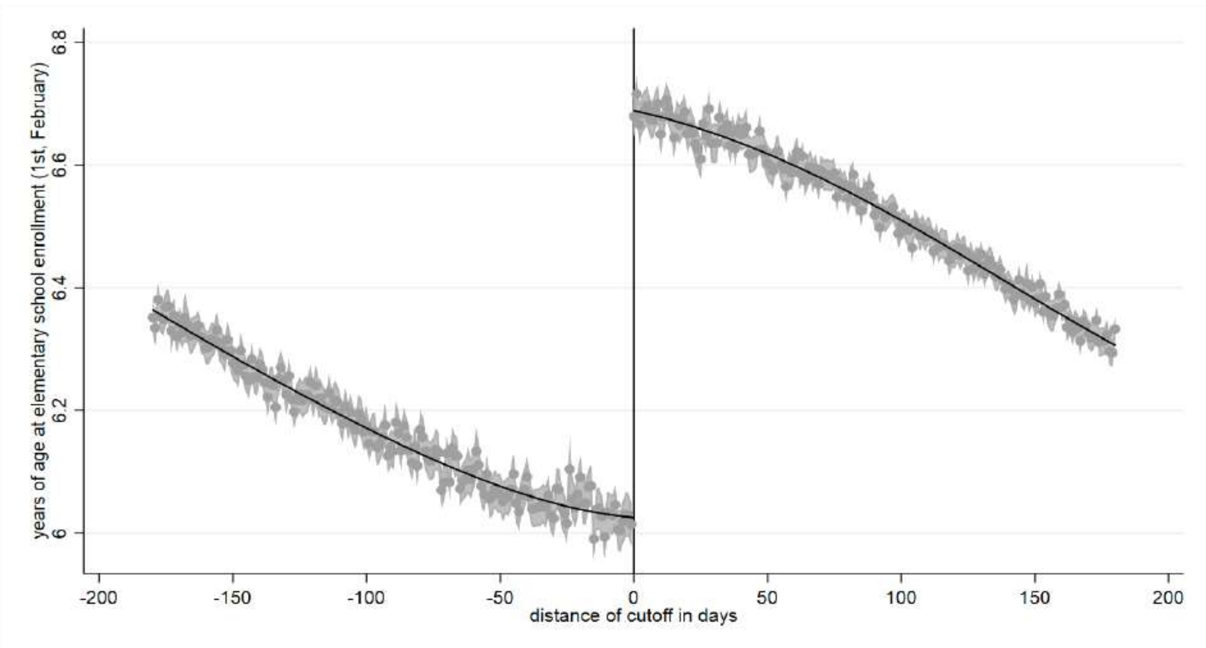


Note: The figure shows the probability of entering in 2007 (y-axis) according to the distance between the cut-off date and the day of birth (x-axis). The procedure used to calculate "the distance of cutoff in days" is described in section 4.1. Children enrolled in the 2007/2008 cohort in the municipal education systems of the capitals of the Brazilian states were considered. The number of points in the Figure was determined according to the estimation procedures developed and implemented by Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019) and Calonico, Cattaneo and Farrell (2020). The standard option of mimicking variance evenly-spaced method was used for this analysis. Source: National School Census.

Following the data treatment procedure mentioned in section 4.1 and the identification strategy described above, calculation of the first stage of the Fuzzy RDD models utilized to analyze the 2007/2008 cohort became possible. Figure 4 presents the probability of school enrollment in 2007 in terms of date of birth relation to the cutoff date. In the discontinuity, it is possible to observe a fall of approximately 60 percentage points in the probability of children born shortly after the cutoff date to enroll in elementary school in 2007. The probability of enrollment in 2007 was more than three times higher for children born shortly before the established limit dates for enrollment in the municipal education networks of the state's capitals of Brazil.

Figure 5 complements this analysis by estimating the exact age at the time of entering elementary school for children of the same 2007/2008 cohort. It should be noted that if all students respected the rules established by the cut-off dates, the age difference in the discontinuity would be exactly one year old. However, since that is not the case, the difference observed in the data for the cohort of entrants in 2007/2008 revolves around 0.7 years. It is also important to emphasize how the age difference between the youngest and oldest group of students of each side of the discontinuity entering elementary school in the same year is of approximately 0.4 years - within the 180-day window (about 0.5 year) from the cutoff date.

Figure 5 - Age of entry according to the distance between birth and cut-off date



The Figure shows the age of entry according to the distance between birth and cutoff dates established by municipal education networks in the capitals of Brazilian states. The procedure used to calculate "the distance of cutoff in days" is described in section 4.1. Children who entered in 2007 or 2008 were considered and the ages were calculated in relation to the February, 1st of the respective years of admission. The number of points in the Figure was determined according to the estimation procedures developed and implemented by Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019) and Calonico, Cattaneo and Farrell (2020). The standard option of mimicking variance evenly-spaced method was used for this analysis. Source: National School Census.

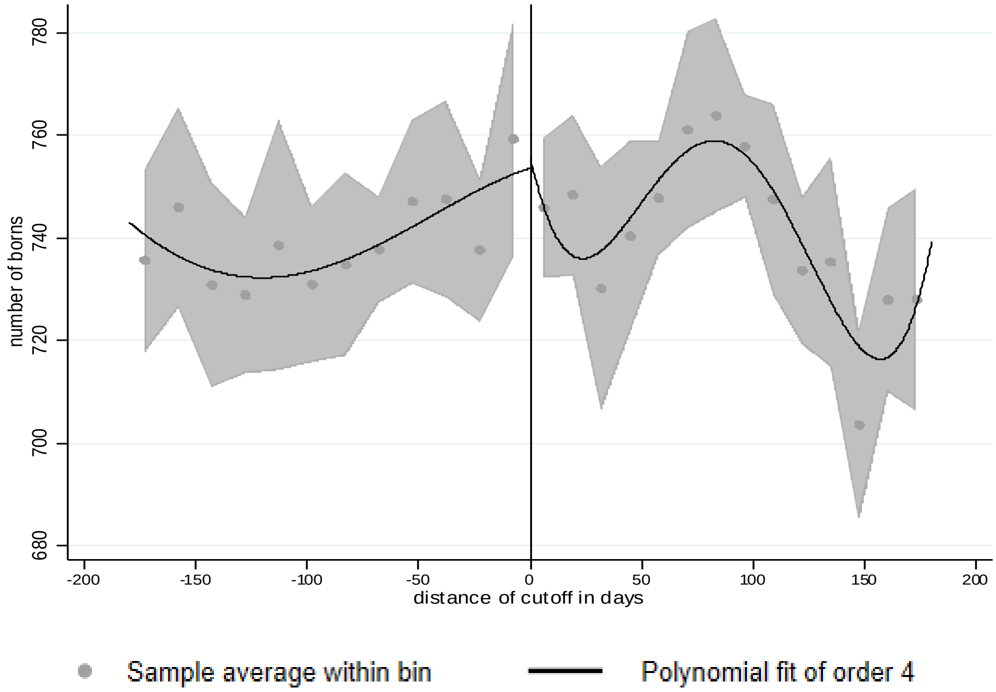
Apart from verifying the consistence of the first stage estimations of the *Fuzzy RDD* models used in this paper, it is also important as a validation tool for our identification strategy to investigate the possibility that Brazilian parents could be manipulating their children's birthdates on account of the age they would first enroll in elementary school. This behavior would violate the assumption that births around the school entry cutoff dates occurred in a random manner and consequently compromise the strategy we utilized to estimate causal effects. For instance, Shigeoka (2015) approached this question from Japan's perspective, where there are strict rules for school enrollment, and found that a significant percentage of parents were in fact intentionally delaying their children's birthdate as a means of early childhood investment[11].

In order to investigate this possibility concerning the date of birth manipulations around elementary school enrollment dates in Brazil we relied on some test implications that could corroborate the validity of the proposed methodology or reveal its unsuitability for this specific design. In case parents did not deliberately manipulate the date of birth of their children to bend the rule for school admission, the density of the children birth variable should be continuous around the school enrollment date set by each county. Also, if children are born at random around the cutoff date, other characteristics aside from age at school enrollment should be similar to individuals born above or below the date limit line for enrollment.

Figure 6 shows the number of daily births of students from the 2007/2008 cohort in relation to the distance in days between the date of school enrollment and the birth date of the children. As can be observed there is no concentration of births in neither side of cutoff dates of school enrollment. Additionally, we found no rejection of the null hypothesis of equality around the discontinuity of number of births per day (robust p value=0.772).

[11] Shigeoka (2015) links this behavior to the broad evidence produced by the school entry age literature that implies that starting school at an older age would lead to short and long run benefits in terms of outcomes for these same children.

Figure 6 – Number of children born per day of birth



Note: The figure shows the total number of births in the cohort of children enrolled in 2007/2008 (y-axis) according to the distance between the cut-off date and the date of birth (x-axis). The procedure used to calculate "the distance of cutoff in days" is described in section 4.1. The null hypothesis of equality is not rejected in discontinuity. The difference in the number of births before and after the limit date set for school enrollment was estimated Non-parametrically by using local linear regression and triangular kernel function. The estimate of difference in cutoff was statistically non-significant and equal to 7.38 children, with a robust p-value of 0.772. Source: Microdata of the School Census.

Table 1 – Differences in observed characteristics around the discontinuity

Variables	Differences (in p.p.)
Percentage of females	-0.514
	(0.559)
Percentage of whites	0.499
	(0.509)
Percentage of children with special needs	-0.092
	(0.879)

Note: Non-parametric estimations by local linear regression and triangular kernel function. The robust p values are presented in brackets. Source: Microdata of the School Census.

Regarding the test of other characteristics aside from age at school enrollment, unfortunately the School Census is not rich in variables that would allow further testing of the children's previous attributes. Concerning race, gender, and classification on the requirement of special needs, however, Table 1 results reveal that we cannot find statistically significant differences among children born around the cutoff dates established by each cities. We assume these tests as sufficient evidence to corroborate the validity of the econometric methodology described throughout this section for our specific setup.

5. Results

5.1 The effects of age at school entry on grade repetition, school progress and school duration in Brazil

As stated previously, the results of this first subsection were elaborated solely with the use of the School Census microdata from 2007 to 2017. We relied on the procedure described in section 4 in order to follow 2007/2008 and 2003/2004 entrants over time, consolidating two data panels[12] with nine time periods that were utilized to build the evidence we will display here. Each period refers to a specific number of years past since the first school enrollment of the child, and in an effort to make it easier to interpret our results, Figure 7 below reveals the years after school entry relation with school cycle and school transitions for students that progressed regularly through each grade. Throughout this first result section, we will focus mainly on investigating three outcome variables: "complete years of education"[13], "probability of grade retention" and "school duration".

Figure 7 – Years after school entry relation with school cycle and school transitions

School cycle/school completion transition	Elementary				Middle				Highschool			Labor market & Higher Education		
Years after school entry	0	1	2	3	4	5	6	7	8	9	10	11	12	13
School grade/years in transition	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	1st	2nd	3rd

Note: Figure displays the moment in time for students that progressed regularly through the school cycle. School duration was standardized for a total of 11 years of study for all students of the analyzed cohorts.

Format wise, in the effort of presenting our results, we find it interesting to initiate this discussion presenting the Fuzzy RDD age effects on complete years of education for the 2007/2008 cohort. This is shown in Table 2 and Figure 8. While Figure 8 has compiled results for the whole period analyzed, Table 2 reveals in more details the effects of age at school entry

[12] While the 2007/2008 data panel consisted of 267,136 children born shortly before or after the cutoff date for enrollment (131,863 and 131,865, respectively) that were observed over time, the 2003/2004 data panel consisted of 305,150 children, with 145,998 of them on the left side of the discontinuity, and 159,152 on the right side.
[13] Defined by the last grade attended by the child in a given year of the School Census panel.

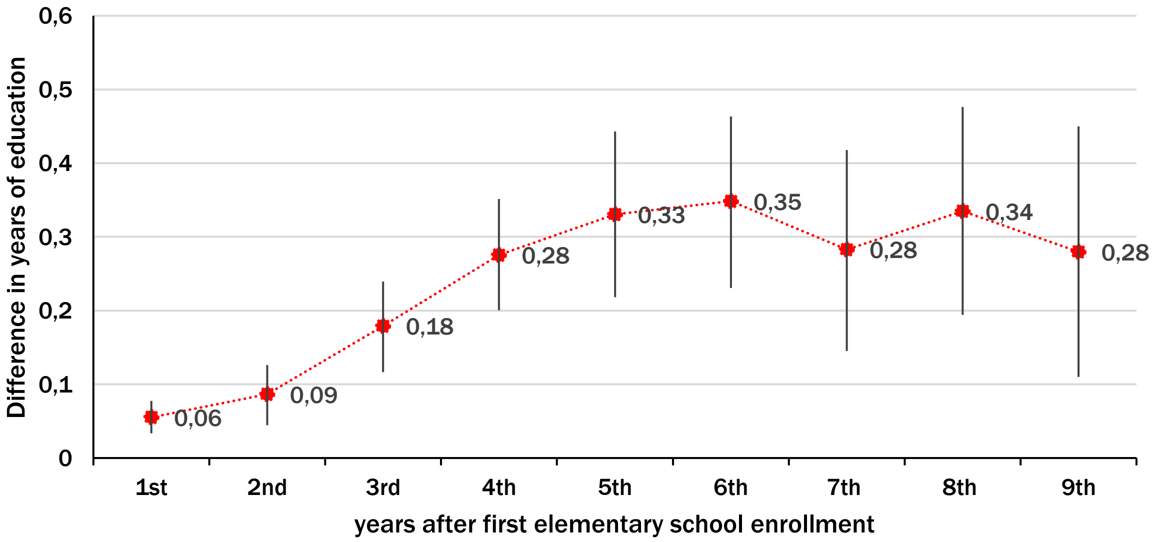
on complete years of study for the last year of the elementary and middle school cycles and the last year available for this respective data panel.

Table 2 – Effects of age at school entry on complete years of education

		Year after elementary school entry		
		3th	7th	9th
Coef.		0.179	0.283	0.280
Std. Err.		(0.0318)	(0.0645)	(0.0803)
Z	Conventional	5.63	4.39	3.48
	Robust	5.67	4.05	3.23
P>|Z|	Conventional	0.000	0.000	0.000
	Robust	0.000	0.000	0.001
N-		26,214	21,727	23,953
N+		26,591	22,308	24,392
M-		2.760	6.016	7.448
M+		2.902	6.285	7.732
BW est. (h) - (both sides)		35.39	29.53	32.16
BW bias. (b) - (both sides)		94.667	64.58	66.89
rho (h/b) - (both sides)		0.374	0.457	0.481

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for municipalities were included and standard errors were clustered according to municipality. Estimates were carried out for all children of each cohort, and N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point for the estimated bandwidth. Source: Microdata of the School Census.

Figure 8 – Effects of age at school entry on complete years of education



Note: The results are non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for counties were included and standard errors were clustered according to municipality. Estimates were carried out for all children of each cohort. Source: Microdata of the School Census.

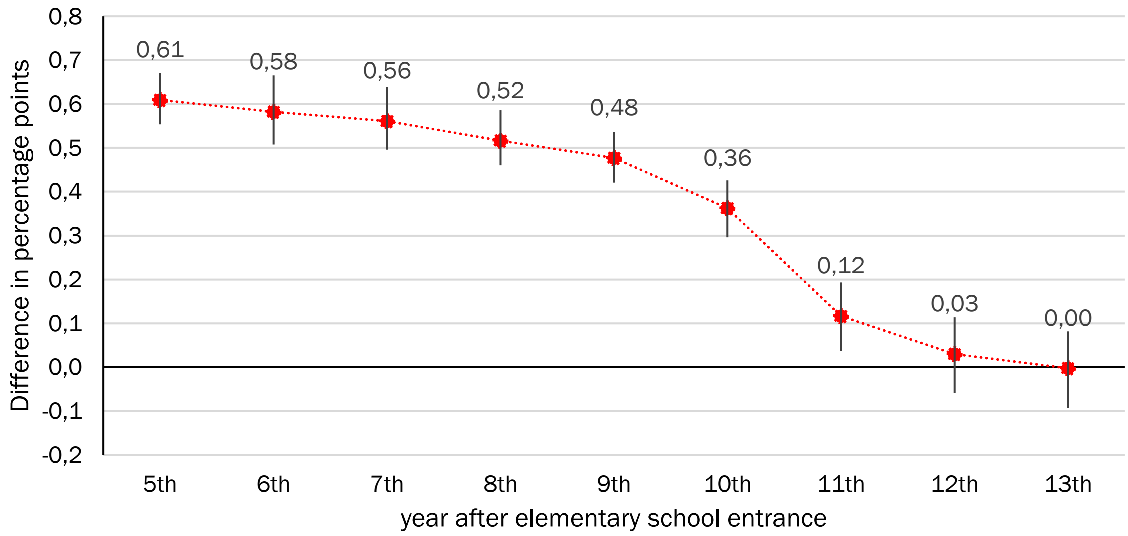
There are two main points to attain here. For starters, these results reveal that children born after the cutoff date for school enrollment are more efficient in progressing through formal instruction, since they were able to accumulate more years of study than early school starters in the same education time exposure. Moreover, they also show that the difference in this outcome rises up until the 6th year after school entry and seems to stabilize afterwards it, which suggests that the contrast in educational performance between both sets of students is in fact more incisive in the first half of their school life. Let's turn now to the investigation of this same outcome for the years available in the second cohort.

Table 3 – Effects of age at school entry on complete years of education (ITT)

		Year after elementary school entry		
		7th	10th	13th
Coef.		0.561	0.362	-0.003
Std. Err.		(0.0363)	(0.0303)	(0.0388)
Z	Conventional	15.45	11.94	-0.07
	Robust	15.55	10.88	-0.14
P>|Z|	Conventional	0.000	0.000	0.946
	Robust	0.000	0.000	0.893
N-		23,582	32,083	29,028
N+		26,118	35,544	32,088
M-		6.377	8.211	8.755
M+		6.838	8.550	8.798
BW est. (h) - (both sides)		30.12	41.89	37.16
BW bias. (b) - (both sides)		66.338	73.20	70.41
rho (h/b) - (both sides)		0.454	0.572	0.528

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for municipalities were included and standard errors were clustered according to municipality. Estimates were carried out for all children of each cohort, and N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point for the estimated bandwidth. Source: Microdata of the School Census.

Figure 9 – Effects of age at school entry on complete years of education (ITT)

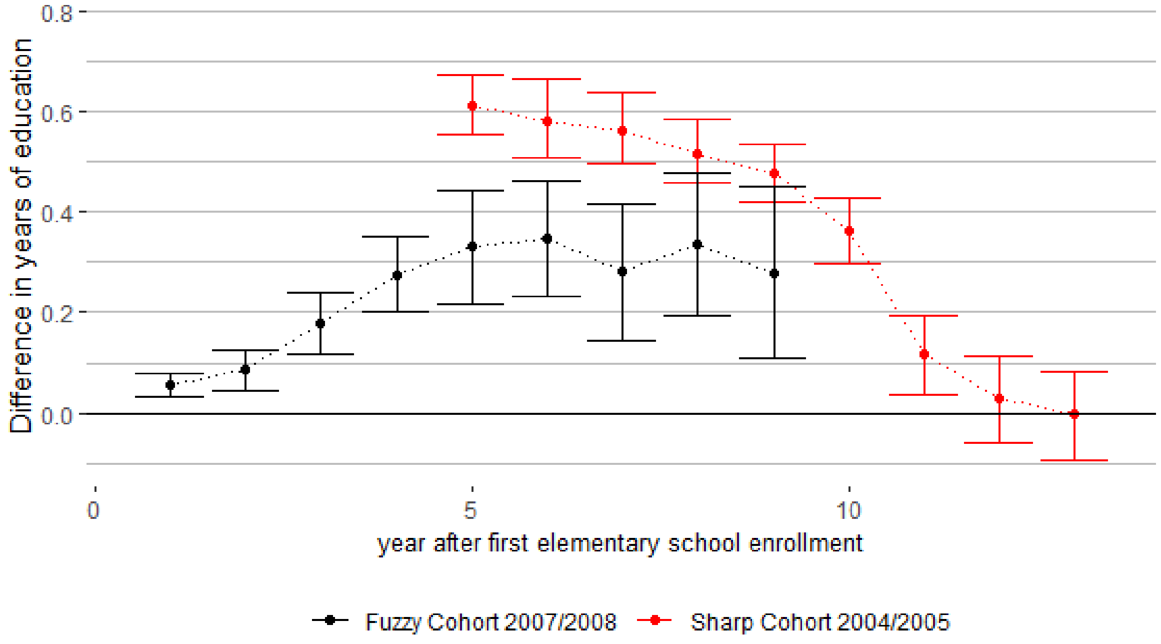


Note: The results are non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for counties were included and standard errors were clustered according to municipality. Estimates and robust 95 confidence intervals are compatible with results shown in Table 3. Source: Microdata of the School Census.

Figure 9 depicts results on complete years of study for the whole period analyzed, while Table 3 details them for the last year of middle and high school cycles and the last year available for this respective data panel. Further examining students after their ideal school period (10th year after school entry should be the last school year for those that progressed regularly through the school cycle) is important because a significant proportion of them were not able to finish their studies in this time range.

We'll focus again on two main points. The first aspect of note is the magnitude of the differential in complete years of study on the last year of high school for those that progressed regularly through the school cycle. An absolute value of 0.36 year of study in favor of older school starters that is also worth 4.41% of the mean value of the outcome variable to the left of the cutoff point for the estimated bandwidth. And the second aspect of interest concerns how these results show that the difference in this outcome between both groups disappears completely in the last year analyzed, which means that students born before and after the cutoff date for school enrollment end up with the same educational level 13th years after school entry.

Figure 10 – Effects of age at school entry on complete years of education (ITT x LATE)



Note: The results are non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for counties were included and standard errors were clustered according to municipality. Estimates were carried out for all children of each cohort. Source: Microdata of the School Census.

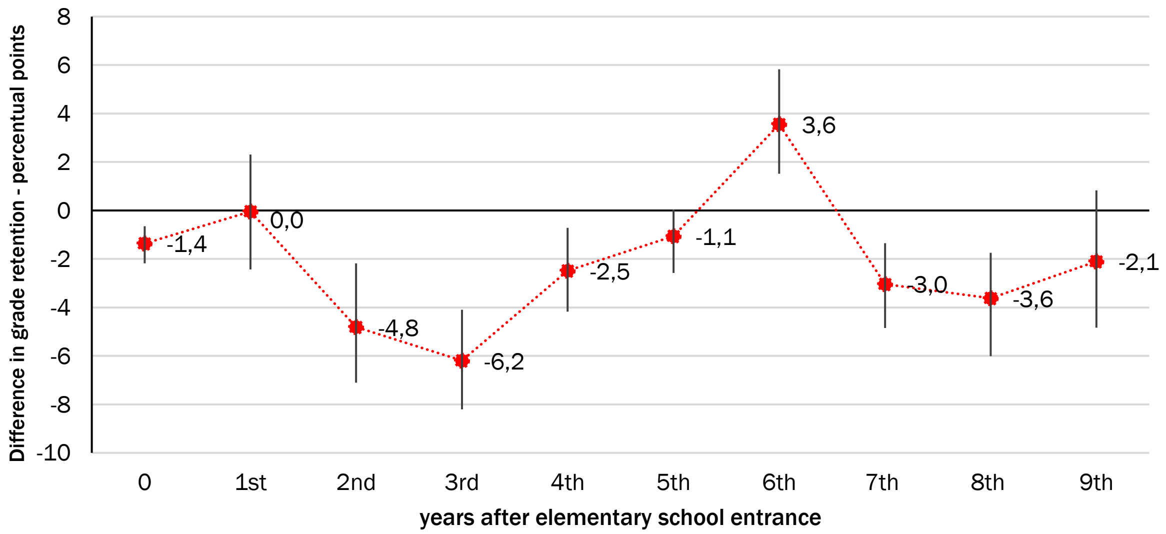
Figure 10 is an interesting one to consolidate our analysis for this particular outcome. It is worth to mention that the difference in magnitude of the coefficients found between both data panels is due to particularities of each cohort, as can be further accessed in appendix B, and therefore we will focus on the trend of each series here. With this joint figure it becomes clear that it is in the first half of the student's school life that this gap in educational level between both groups increases, suggesting that the contrast in performance between older and early school starters is in fact more incisive when students are at a younger age. In that sense, a look upon the way that the date of birth of students in relation to the cutoff date influences their rates of grade retention is efficient to help us understand the behavior of the complete years of study outcome throughout these years. Therefore, Figure 11 and Table 4 present evidence on the school entry age effects on the grade retention rates for the 2007/2008 cohort, obtained with the Fuzzy RDD.

Table 4 – Effects of age at school entry on grade retention rates

		Year after elementary school entry		
		3th	7th	9th
Coef.		-0.0619	-0.0304	-0.021
Std. Err.		(0.0100)	(0.0077)	(0.0135)
Z	Conventional	-62.10	-39.65	-15.55
	Robust	-58.66	-34.74	-13.88
P>|Z|	Conventional	0.000	0.000	0.120
	Robust	0.000	0.001	0.165
N-		22,304	21,934	10,504
N+		23,303	22,453	10,725
M-		0.123	0.146	0.168
M+		0.079	0.130	0.148
BW est. (h) - (both sides)		33.60	33.79	17.92
BW bias. (b) - (both sides)		67.76	68.18	51.42
rho (h/b) - (both sides)		0.496	0.496	0.349

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects for municipalities were included and standard deviations were clustered according to municipality. Estimates were carried out for all children of the 2007/2008 cohort that met the identification criteria, and N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point for the estimated bandwidth. Source: Microdata of the School Census.

Figure 11 – Effects of age at school entry on grade retention rates



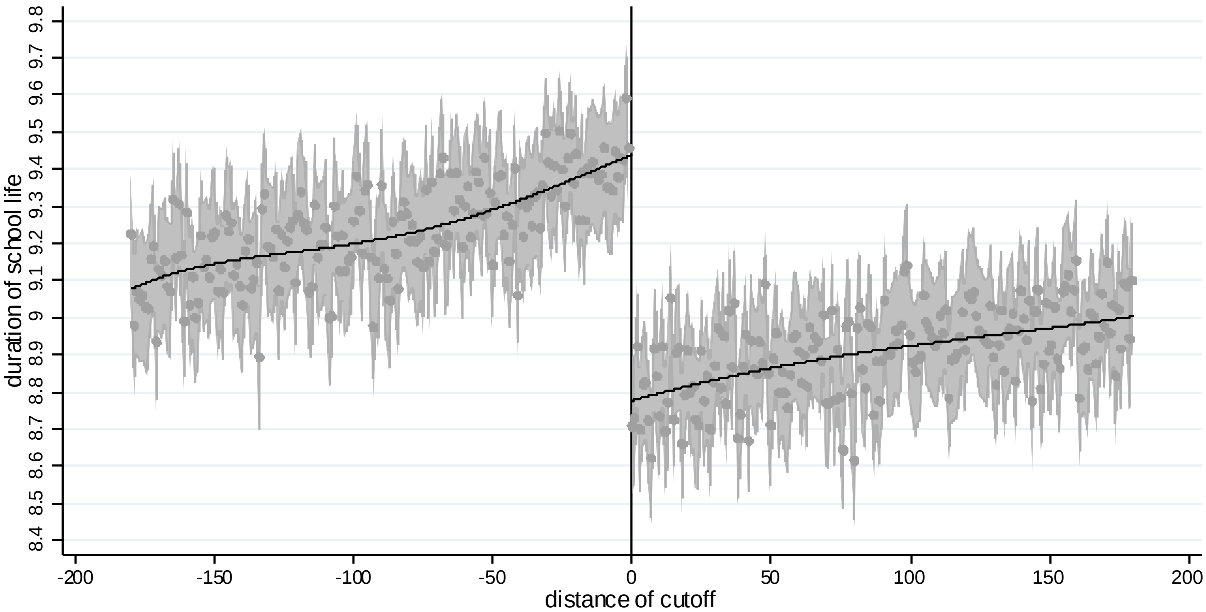
Note: The figure presents the effects of age on grade retention rates from the year of enrollment to the ninth year after enrollment into elementary school. Estimates and robust 95 confidence intervals are compatible with robust results shown in Table 4. Source: Microdata of the School Census.

While the difference regarding this outcome at the end of the entrance year is of 1.4 percentage points, the impact rises to an absolute value of 6.2 percentage points in the third year after school entry, and despite decreasing from then on until the 6^{th} year after school enrollment, the second major inflection in the series on its latter half consolidates a significant negative behavior of the trend throughout the entire time period. A behavior that remains more incisive, however, in the first five years analyzed. Also important to emphasize here is the magnitude of the age at school entry effects for this particular outcome, since in these mentioned years (entrance year and third year after school entry) they are worth respectively 44.85% and 50.36% of the mean value of the outcome variable to the left of the cutoff point for the estimated bandwidth. Nonetheless, as a whole, these results reveal not only that early school starters fail more than older school starters, corroborating the hypothesis that those born after the cutoff date for enrollment are more efficient in progressing through grades, but also that this failure rate contrast between both groups is also more incisive when students are younger.

Since older school starters fail less and accumulate more years of study in the same amount of time than early school starters, one would expect this setup to directly influence the average school duration for students on each side of the discontinuity. Investigating this particular outcome is important for the continuation of our analysis because the average time spent in school is also directly linked to the average age with which student's exit school. [14] Figure 12 below reveals that from the last year of enrollment in the School Census (13th years after first school entry for the 2003/2004 cohort), it is possible to estimate that students born right after the cut-off date for entering elementary school spent about 0.67 years (8 months) less than their counterparts in school, although they both got the same number of full years of schooling. Essentially, since these students wait a whole year before entering formal education, this result reveals that they are indeed older in an absolute sense at the moment of basic instruction egress, as will be further investigated in the next results section.

[14] "School duration" in our analysis stands for the difference between the student's last and first year of enrollment in the school census data that could be observed in the 13th year after enrollment.

Figure 12 – Duration of school life cycle for cohort 2003/2004



Note: The Figure shows the duration of time that students spend enrolled in required education (y-axis) as a function of the distance between the cutoff and the date of birth (x-axis). At discontinuity the estimated difference was -0.67044 years with a robust z statistic of -18.0246 and the robust confidence interval (95%) was [-0.748426, -0601623]. The result is a non-parametric estimation by local linear regression and triangular kernel function. Fixed effects for municipalities were included and standard errors were clustered according to municipality. Source: Microdata of the School Census.

5.2 The effects of age at school entry on labor market and higher educational outcomes

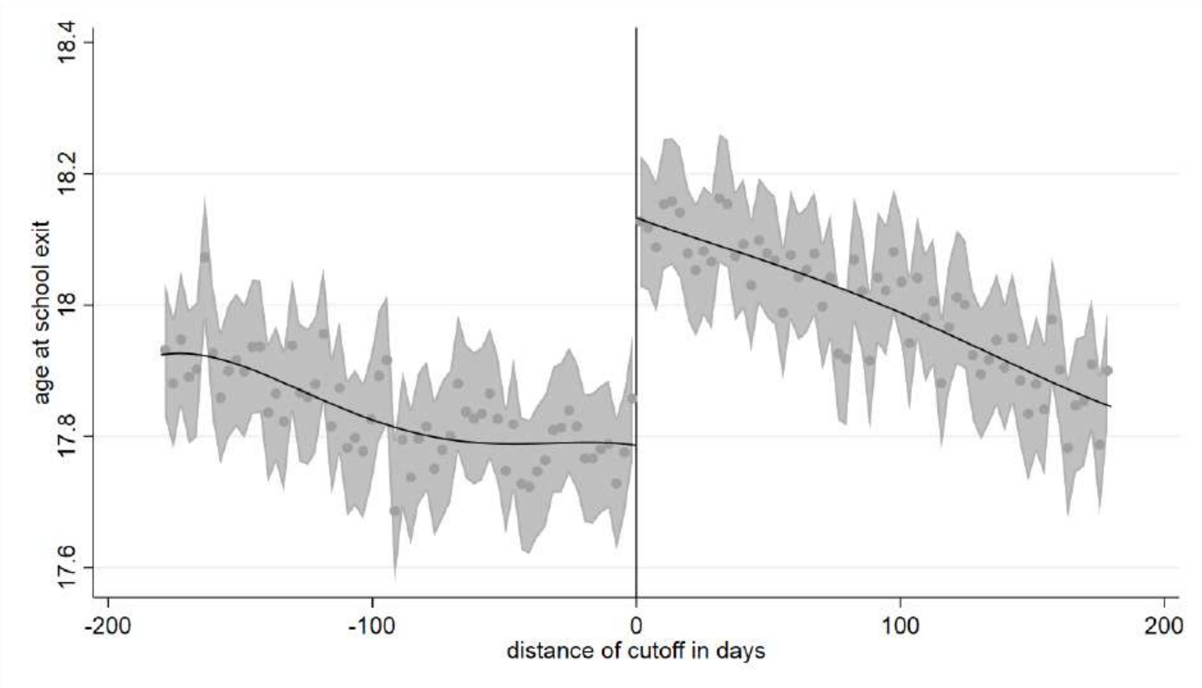
Given the more disputable scenario concerning the effects of school entry age on longer-run outcomes (Black et al., 2011; Peña, 2016), we now examine how these age at school entry effects affect labor market and higher education outcomes such as: probability of employment, formal labor market wages, and higher education enrollment rates. To do so, we follow the students from the 2003/2004 cohort through the Higher Education Census and RAIS data sets.

There are two main effects that operate in a way to affect these transition outcomes. One of them is the tradeoff emphasized in the conceptual framework section. That is, those with a delayed entrance await a whole year before enrolling in formal instruction, and this means that if they progressed through school at the same rate than those born before the cutoff, they would be a whole year older when finishing their studies (Black et al., 2011). The other effect concerns precisely the way investigated in the previous section through which these students *do not* progress at the same rate throughout the school cycle, since those born after the cutoff are more efficient in doing so.

While the evidence raised in Figure 12 already revealed that the net effect of both outcomes is a diminished, but still older absolute age at the moment of school egress for those with a delayed entrance, Figure 13 reports in a more precise manner how the average age gap at the school egress moment between students of both groups is of 0.33 year, since older school starters tend to leave school at around 18.1 years of age and early school starters tend to leave school at around 17.8 years of age.

The results concerning the transition from elementary school to the working life were consolidated from 2014 onwards because this period corresponds to the 11th year after school enrollment for 2003 entrants, precisely the year after school completion for those that progressed regularly through the school cycle. Table 5 presents evidence on the school entry age effect in the probability of employment in the formal labor market per year of RAIS, and these estimations reveal that there is no significant difference in this outcome between children born before or after the cutoff date for enrollment. More importantly, since we are accessing these results since 2014, the lack of difference in this probability throughout the whole period analyzed suggests that they accumulated the same amount of experience in the formal labor market along the investigated years.

Figure 13 – Age at school exit for students of 2003/2004 cohort (ITT)



Note: The Figure shows the average age at school exit for students of the 2003/2004 cohort (y-axis) as a function of the distance between the cutoff and the date of birth (x-axis). At discontinuity the estimated difference was of 0.3295 years with a robust z statistic of 8.6762 and the robust confidence interval (95%) was [0.251515, 0.398312]. The result is a non-parametric estimation by local linear regression and triangular kernel function. Fixed effects for municipalities were included and standard errors were clustered according to municipality. Each student age at school exit was calculated assuming that this event happened on 31 of December of the last year of enrollment on the School Census. Source: Microdata of the School Census.

Regarding the age effect on formal wages, however, Table 6 reveals how despite remaining insignificant in the first year analyzed, this differential turned slightly significant in 2015 and remained stable in a significant value of around R$ 36 in favor of early school starters from 2016 to 2017. In order to understand the magnitude of such contrast, it is worth to stress that an R$ 36 difference in wages represents 6% and 8% of the average wage of the 2003/2004 cohort in 2016 and 2017, respectively.

Table 5 – Effects of age at school entry on the probability of employment in the formal labor market (ITT)

		RAIS year			
		2014	2015	2016	2017
Coef.		0.0030	-0.0058	0.0019	0.0080
Std. Err.		(0.0088)	(0.0044)	(0.0079)	(0.0114)
Z	Conventional	0.2770	-14.0940	0.2543	0.8660
	Robust	0.3429	-13.1600	0.2414	0.6996
P>|Z|	Conventional	0.7817	0.1587	0.7993	0.3865
	Robust	0.7317	0.1882	0.8092	0.4842
N-		26,989	34,378	27,589	22,696
N+		26,163	33,207	26,791	22,215
M-		0.109	0.167	0.194	0.194
M+		0.081	0.122	0.162	0.183
BW est. (h) – both sides		44.57	56.60	45.02	37.18
BW bias. (b) – both sides		97.44	100.75	83.42	70.41
rho (h/b) – both sides		0.457	0.562	0.540	0.528

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects of the counties were performed and standard errors were clustered according to municipality. Estimates were carried out for all 305,150 children of the 2003/2004 cohort born 180 days before (145,998) or after (159,152) the limit date set for elementary school enrollment. N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point Source: School Census and RAIS.

Table 6 – Effects of age at school entry on formal labor market wages (ITT)

		RAIS year			
		2014	2015	2016	2017
Coef.		-12.6622	-39.1336	-36.2275	-36.0840
Std. Err.		(13.2243)	(22.2082)	(16.5724)	(14.9114)
Z	Conventional	-1.0493	-1.87	-2.426	-2.3482
	Robust	-0.9575	-1.7621	-2.186	-2.4199
P>|Z|	Conventional	0.2940	0.0615	0.0153	0.0189
	Robust	0.3383	0.0780	0.0288	0.0155
N-		1,813	3,613	7,408	6,654
N+		1,687	3,368	7,068	6,585
M-		693	868	487	592
M+		586	807	453	558
BW est. (h) – both sides		26.53	34.19	56.00	48.84
BW bias. (b) – both sides		52.30	65.44	93.24	96.54
rho (h/b) – both sides		0.507	0.522	0.601	0.506

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects of the counties were performed and standard errors were clustered according to municipality. Estimates were carried out for children of the 2003/2004 that met the identification criteria, and N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point Source: School Census and RAIS.

Since we have accessed that there is no significant difference in educational level around discontinuity at the end of the school cycle for these students, the basic human capital model (Mincer, 1974) suggests that experience in the labor market should be one of the most prominent factors causing the differences in wages favoring individuals born before the limit date set for school enrolment. [15] Given the peculiarities of the Brazilian labor market structure and the documented lack of difference in accumulation of experience in the *formal* labor market for students born before and after the cutoff date for enrollment, however, we understand this setup as evidence in favor of the hypothesis that it was the accumulation of experience in the *informal* labor market that played a major role behind the results we found.

This hypothesis is in line with Seminario Amez (2021) findings, which emphasize how informal labor contracts in Brazil are both: important as a mean of career advancements, and prevalent among the youngest components of the labor force. It is also in line with the fact that these early school starters complete their school cycle at a younger age than their counterparts do, which means they are more likely to have the earlier ingress in working life that should

[15] 2016 is the 13th year after school entry for the 2003 entrants and the 12th year after school entry for the 2004 entrants. 2017 is the 13th year after school entry for the 2004 entrants.

translate in the higher salaries in the formal labor market accessed in Table 5. Given this setup, it is very likely that these youngsters were able to accumulate experience in the informal sector in the 1st year after school egress, while those with a delayed entrance were still concluding their studies.

Table 7 – Mean of the outcome variable per group and year of the PNADC

	Age		Labor Market Force		Labor Market Employment		Informal Labor Market Employment	
Year	-	+	-	+	-	+	-	+
2012	15.54	15.03	16.64%	11.11%	11.55%	6.84%	9.60%	5.98%
2013	16.54	16.04	28.05%	19.61%	18.52%	13.55%	15.10%	10.13%
2014	17.49	17.06	37.59%	26.98%	26.71%	20.12%	13.29%	11.95%
2015	18.54	18.03	54.91%	45.30%	39.51%	30.00%	19.42%	15.51%
2016	19.53	19.05	62.80%	57.83%	40.39%	33.33%	19.52%	16.07%
2017	20.51	20.02	69.15%	67.48%	45.76%	46.93%	21.69%	19.87%

Note: Sample restricted to youngsters found in each year of the PNADC that were born 180 days before and after the 2003/2004 cutoff date for enrollment of their respective UF. "-" and "+" below each outcome represents the mean value of the outcome variable to the left and right of the cutoff point, respectively. Informal employment in this analysis comprises of informal employees and own-account workers.

Although Table 7 does not provide evidence to validate this hypothesis, it does serve as a support for it. [16] On spite of its methodological limitations, it reveals how students from the 2003/2004 cohort born 180 days before the cutoff date for school enrollment are indeed more prone than their counterparts to be in an informal employment position in the years surrounding the school egress.

In addition to measuring its influence in the formal labor market, we also sought to understand how age at elementary school entry would affect admission into higher education. We found, however, no significant statistical differences between both groups in terms of the probability of being enrolled in superior education institutions 11th, 12th and 13th years after school entry, as can be seen in Table 8 (Appendix C presents results for this same outcome concerning exclusively public education institutions).

[16] In our PNADC analysis, due mostly to lack of sample, we were not able to investigate the differential in the mean of each outcome variable presented in Table 7 around the discontinuity generated by the relation between the youngster's dates of birth and cutoff dates for school enrollment. There are also other setbacks that hinder this approach, such as our incapacity of observing the exact city of first school enrollment of each youngster.

Table 8 – Effects of age at school entry on the probability of higher education access (ITT)

		All institutions		
		Year after school entry		
		11th	12th	13th
Coef.		0.0424	0.0329	0.0298
Std. Err.		(0.0289)	(0.0266)	(0.0172)
Z	Conventional	1.461	1.242	1.752
	Robust	1.465	1.236	1.739
P>|z|	Conventional	0.1441	0.2144	0.0797
	Robust	0.1429	0.2166	0.0820
N-		35,110	28,537	24,499
N+		37,323	30,400	26,108
M-		0.016	0.064	0.102
M+		0.037	0.086	0.118
BW est. (h) - both sides		42.80	34.68	29.49
BW bias. (b) - both sides		77.15	65.73	58.00
rho (h/b) - both sides		0.555	0.528	0.508

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects of the counties were performed and standard errors were clustered according to municipality. Estimates were carried out for all 305,150 children of the 2003/2004 cohort born 180 days before (145,998) or after (159,152) the limit date set for elementary school enrollment. N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point. Source: School Census and Higher Education Census.

This combined school transitions evidence suggests that the advantage in educational outcomes that translates into a faster school completion for older school starters does not impact their learning enough to either raise their productivity from a labor market perspective, or increase their chances of pursuing a superior qualification around the moment of school egress. Therefore emphasizing the importance of the trade-off in terms of educational and labor market prospects of starting school at an older age among this analytical setup. While older school starters outperform their peers with more emphasis in the early school years and are able to finish their studies at a faster pace, it seems like this educational advantage did not imply in a "better learning" for this same group. Moreover, since they waited a whole year before entering school, the subsequent delayed entrance in the labor market they are subject to puts them in a worse labor market position than early school starters in terms of wages around the moment of school egress (Black et al., 2011).

6. Conclusion

The present article made use of the RDD methodology on longitudinal administrative data from Brazilian students to produce estimates of causal effects of age at school entry on a series of outcomes related to these youngsters educational and labor market performance. Emphasis on the latter part of the school cycle was relevant throughout this work in face of the relative controversy that exists among this literature regarding the effects of the starting school age on more long-run outcomes of the individuals (Peña, 2016).

In that sense, our main results reveal that in the last grade of schooling for those that progressed regularly through the school cycle, students with a delayed entrance were able to accumulate 0.36 more years of complete study than early school starters. While this educational gap between both groups disappears within 2~3 years, when we examine it in the 13th year after school enrollment, a look upon the average school duration of children in this same period reveals that older school starters finish their studies in 0.67 year less time. A result that is mostly due to the fact that students with a delayed entrance fail less than their counterparts, and are more efficient in progressing through the school cycles.

In terms of these youngsters' absolute age at the moment of school egress, the efficiency of older school starters in progressing through the school cycle is not enough to offset the 1 year wait for enrolling into elementary school that they had to experience as a consequence of being born after the school cutoff date for enrollment. The combined sum of these effects is a 0.3 increase in absolute age at the moment of school egress for those with a delayed entrance, a setup that directly impacts the early labor market life of both groups of individuals, since early and late school starters share the same employment levels in the years following school conclusion, but do not share the same wages.

Given the importance of informal labor contracts among the Brazilian labor structure as a mean of career advancements and its prevalence among the youngest components of the labor force (Seminario-Amez, 2021), this evidence suggests that it was the acquisition of experience in the informal labor market by early school starters in the 1st year after leaving school (while those with a delayed entrance were still concluding their studies) that was determinant of their higher formal wages. As far as we are concerned, this probable relation between school entry age and informality is one that has not been approached before in the literature.

Moreover, we found no significant statistical difference between early and late school entrants in terms of the probability of being enrolled in superior education institutions 11th, 12th and 13th years after school entry. This combined labor market and higher educational transitions results suggest that the advantage in educational outcomes that translates into a faster school completion for older school starters does not impact their learning enough to either raise their productivity from a labor market perspective, or increase their chances of pursuing a superior qualification around the moment of school egress.

Concerning the controversy approached along the article, this set of results are more in line with the hypothesis that the documented early edge on school performance of starting school at an older age should dissipate through time, and not necessarily translate into better long term academic and labor market outcomes for these same individuals. The evidence presented here emphasizes how the trade-off in terms of educational and labor market prospects of starting school at an older age is one that deserves attention among this analytical setup. Further investigating the educational and labor market performance of these individuals for a longer period and with emphasis on the mechanisms operating behind the effects found remain as valuable tools to contribute to this particular point of interest and to the maturing of this research agenda as a whole.

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Appendix A

Table 9 – 2001 R^2 statistics to identify the limit date set for school enrollment in São Paulo city

Cutoff day	2007/2008 Cohort
Jan 1st	**0.8892**
Feb 1st	0.7251
Mar 1st	0.58
Apr 1st	0.4171
May 1st	0.273
Jun 1st	0.1339
Jul 1st	0.0066
Aug 1st	0.0038
Sep 1st	0.002
Oct 1st	0.0013
Nov 1st	0.001
Dec 1st	0.001

Note: The table presents estimated limit dates for enrollment into elementary school of the cohorts of children admitted into the schools of the municipal education network of São Paulo city in 2007/2008**.** Twenty-five possible dates were tested, every 1st day of each month of January 1st of the year $(\boldsymbol{t}-\mathbf{7})$ to January 1st of the year $(\boldsymbol{t}-\mathbf{5})$. $\boldsymbol{t}$ is the shortest year of enrollment among the two years that encompasses the cohort.

Table 10 - Estimated limit dates set for elementary school enrollment per state capital of Brazil for the 2007/2008 cohort.

State capital	Limit date for enrolment
Porto Velho (RO)	01/04/2001
Rio Branco (AC)	01/05/2001
Manaus (AM)	01/07/2001
Boa Vista (RR)	01/06/2001
Belém (PA)	01/07/2001
Macapá (AP)	01/01/2001
Palmas (TO)	01/04/2001
São Luís (MA)	01/06/2001
Teresina (PI)	01/08/2001
Fortaleza (CE)	01/07/2001
Natal (RN)	01/03/2001
João Pessoa (PB)	01/10/2001
Recife (PE)	01/04/2001
Maceió (AL)	01/07/2000
Aracaju (SE)	01/04/2001
Salvador (BA)	01/04/2001
Belo Horizonte (MG)	01/07/2001
Vitória (ES)	01/07/2000
Rio de Janeiro (RJ)	01/03/2001
São Paulo (SP)	01/01/2001
Curitiba (PR)	01/01/2002
Florianópolis (SC)	01/03/2001
Porto Alegre (RS)	01/03/2001
Campo Grande (MS)	01/03/2001
Cuiabá (MT)	01/06/2001
Goiânia (GO)	01/04/2001

Note: The table presents estimated limit dates set for enrollment into elementary school for children admitted into the schools of the municipal education network of the state capitals of Brazil in 2007/2008. Twenty-five possible dates were tested for each state, every 1st day of each month from January 1st of 2000 to January 1st of 2002.

Table 11 – Estimated limit dates set for school enrolment per state capital of Brazil for the 2003/2004 cohort.

State capital	Limit date for enrolment
Porto Velho (RO)	08/01/1997
Rio Branco (AC)	01/01/1997
Manaus (AM)	07/01/1997
Belém (PA)	07/01/1997
Macapá (AP)	01/01/1997
Palmas (TO)	10/01/1996
São Luís (MA)	08/01/1997
Teresina (PI)	08/01/1997
Fortaleza (CE)	07/01/1997
Natal (RN)	04/01/1997
João Pessoa (PB)	12/01/1997
Recife (PE)	05/01/1997
Maceió (AL)	10/01/1997
Aracaju (SE)	08/01/1997
Salvador (BA)	03/01/1997
Belo Horizonte (MG)	07/01/1997
Vitória (ES)	07/01/1997
Rio de Janeiro (RJ)	03/01/1997
São Paulo (SP)	01/01/1997
Curitiba (PR)	03/01/1997
Florianópolis (SC)	01/01/1997
Porto Alegre (RS)	07/01/1997
Campo Grande (MS)	01/01/1998
Cuiabá (MT)	08/01/1997
Goiânia (GO)	07/01/1997

Note: Estimated limit dates set for enrolment are presented for children admitted into the 4th and 5th grades of elementary school on the municipal education network of the state capitals of Brazil on 2007. Twenty-five possible dates were tested: every 1st date of each month, from January 1st of 1996 to January 1st of 1998. There were no municipal schools in Brasília (DF) and at the time municipal schools that met the criteria used for inclusion were not found in Boa Vista (RR).

Appendix B

Table 12 – Difference in years of study 9 years after school entry. Sharp RDD cohort comparison.

State capital	2003/2004 (A)	2007/2008 (B)	(A-B)
Porto Velho (RO)	0.61	-0.02	0.63
Rio Branco (AC)	0.83	-0.94	1.77
Manaus (AM)	0.69	0.2	0.49
Belém (PA)	0.06	-0.04	0.1
Macapá (AP)	0.56	-0.2	0.76
Palmas (TO)	0.28	0.05	0.23
São Luís (MA)	0.62	0.28	0.34
Teresina (PI)	0.59	-0.05	0.64
Fortaleza (CE)	0.24	0.68	-0.44
Natal (RN)	0.52	0.28	0.24
João Pessoa (PB)	0.52	-0.06	0.58
Recife (PE)	0.57	-0.24	0.81
Maceió (AL)	0.35	-0.07	0.42
Aracaju (SE)	0.67	0.38	0.29
Salvador (BA)	0.42	-0.04	0.46
Belo Horizonte (MG)	0.34	-0.02	0.36
Vitória (ES)	0.22	0.11	0.11
Rio de Janeiro (RJ)	0.5	0.34	0.16
São Paulo (SP)	0.46	0.16	0.3
Curitiba (PR)	0.73	0.42	0.31
Florianópolis (SC)	1.02	-0.1	1.12
Porto Alegre (RS)	0.03	0.22	-0.19
Campo Grande (MS)	0.42	0.06	0.36
Cuiabá (MT)	0.32	0.33	-0.01
Goiânia (GO)	0.32	0.14	0.18
Brazil	0.48	0.20	0.28

Appendix C

Table 13 – Effects of age at school entry on the probability of higher education public institutions access (ITT)

		Public institutions		
		Year after school entry		
		11th	12th	13th
Coef.		0.0072	0.0096	0.0064
Std. Err.		(0.0054)	(0.0055)	(0.0035)
Z	Conventional	1.346	1.701	1.855
	Robust	1.338	1.746	1.854
P>|z|	Conventional	0.1785	0.089	0.0636
	Robust	0.1808	0.0808	0.0637
N-		26,329	32,622	26,072
N+		28,108	34,782	27,742
M-		0.003	0.012	0.020
M+		0.006	0.017	0.024
BW est. (h) - both sides		33.55	39.92	31.88
BW bias. (b) - both sides		84.66	78.30	61.70
rho (h/b) - both sides		0.396	0.51	0.517

Note: Non-parametric estimations by local linear regression and triangular kernel function. Fixed effects of the counties were performed and standard errors were clustered according to municipality. Estimates were carried out for all 305,150 children of the 2003/2004 cohort born 180 days before (145,998) or after (159,152) the limit date set for elementary school enrollment. N- and N+ represent the effective number of observations to the left and right, respectively, of cutoff point for the estimated bandwidth. M- and M+ represent the mean value of the outcome variable to the left and right, respectively, of cutoff point Source: School Census and Higher Education Census.

Table 14 – Average formal wage per age group and educational level

Age group	Average formal wage		
	High school graduate (a)	College graduate (b)	Differential (a/b)
25-30 years	R$ 1,639.02	R$ 3,608.14	2.2
31-40 years	R$ 1,871.83	R$ 5,332.97	2.85
41-50 years	R$ 2,015.82	R$ 7,060.91	3.5
51-60 years	R$ 2,203.76	R$ 8,393.74	3.81

Note: Higher education graduates and high school graduates average wage per group and educational level. Nominal values for December of 2017. Source: 2017 RAIS Microdata.