

Do School Starting Age Generate Externalities on Siblings?

Evidence using Regression Discontinuity Design

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Abstract

Siblings share a bond that makes their relationship unique and, thus, probably influence each other's behavior and decisions. In this paper, we examine siblings' spillovers in elementary school. We explore the cutoff entry rule as an exogenous variation to school starting age and estimate spillovers causal effects using regression discontinuity design. Using data from state schools in São Paulo, our results show that entering school being older in the cohort generates negative spillovers in younger sisters' proficiency levels. These estimates are particularly significant in less affluent families with close-in-age siblings in the early years of elementary school. However, the results are quite different when analyzing spillovers from younger to older siblings. A younger student entering school later has positive spillovers on their older siblings' test scores. These results are driven by more affluent families with similar siblings (close in age and same gender). Our results provide empirical evidence that we should have externalities between siblings in the educational context. Moreover, these effects can diverge when considering the families' socioeconomic and demographic characteristics.

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JEL Classification: D13, I20, J13.

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1 Introduction

Researchers have been studying exhaustively how different factors affect human capital formation. In this context, a question that has been explored in recent years is if a variation in a persons' human capital produces externalities in other individuals. This type of spillover can arise in different environments such as the job market, schools, and households. Within family externalities, the literature usually investigates the role of parents on their children, but there is little evidence about the influence of siblings on learning. The main reasons for this absence of papers about siblings' peer effects are data limitations and identification challenges. Still, siblings' spillovers can be an important determinant of their learning since they share a bond, unlike any other relationship. Siblings typically grow up in the same home, with the same parents and similar genetics, and experience life events together ([Black et al., 2021](#)). Thus, this article studies sibling spillovers in the elementary school stage.

The literature recognizes two main types of mechanisms that may lead to siblings' peer effects. First, siblings could influence each other directly, being good or bad role models. This direct channel is more common when analyzing a spillover from an older to a younger sibling. Moreover, this mechanism usually appears more often in disadvantaged families or single-parent households in which the older sibling figure has a more important role ([Karbownik & Özek, 2021](#)). We may also have indirect spillovers effects driven by parents' resources allocation. For example, school performance may influence how parents divide their time, attention, and money between their children. [Becker & Tomes \(1976\)](#) defines that parents allocate their resources to compensate or reinforce different childrens' endowments. In the compensatory (reinforcement) behavior, parents would tend to invest more in siblings with fewer (more) endowments. Still, other factors can influence parental resource allocation, such as gender preference ([Karbownik & Myck 2017](#)), birth order ([Pavan, 2016](#)), and health ([Yi et al., 2015](#)). These indirect channels are frequent in older-to-younger and younger-to-older siblings' spillover effects. Regardless of the mechanism, the presence of siblings spillovers suggests that educational shocks that affect one child can have a significant positive

or negative multiplier effect ([Black et al., 2021](#)).

This article investigates if children entering later in school generate spillover effects on their siblings regarding proficiency levels and probability of grade failure. We use a well-known educational rule, the cutoff entry rule, as an exogenous variation to capture this causal effect. We define the siblings exposed to this rule as focal children and analyze the spillovers on focal childrens' siblings. The cutoff rule establishes that focal children born after (on the right) the cutoff date have to wait an extra year to enter elementary school compared to those born before (on the left). Thus, we use the regression discontinuity design approach to compare outcomes of siblings of right focal children to outcomes of siblings of the left focal children. Due to data availability, we study both spillovers from older-to-younger (older focal children in their younger siblings) and younger-to-older (younger focal children in their older siblings) separately.

As we mentioned, siblings have a lot in common, which should make their influence on each other interesting to analyze, but this also makes it difficult to identify the causal effect of one sibling on another. Those difficulties are the same that we face when estimating peer effects. Thus, as we mentioned, we use the educational cutoff rule that defines the school starting age to overcome the identification problems. This identification strategy has already been used to identify spillovers by other papers such as [Karbownik & Özek \(2021\)](#), and [Landersø et al. \(2020\)](#), in the United States and Denmark, respectively. The idea is to compare siblings of focal children born around the cutoff. We define the sibling-focal child pair as the treated group for focal children born after the cutoff (and consequently enrolled in elementary school later). We expect siblings whose focal children are near an admission threshold to be comparable in households with similar socioeconomic backgrounds, with siblings that share the same demographic characteristics, making our control pairs good counterfactuals for the treated.

We use detailed administrative data of the state education system in São Paulo, the Brazilian state with the largest student population. The data has the information if the

student has a sibling enrolled in the state school system, which makes identification of sibship composition straightforward. In addition, all students have a unique identifier that allows us to get their proficiency level from the São Paulo State School Performance Assessment System (SARESP) data. We also have information on participation in Bolsa Familia Program, which is often used as a proxy for less affluent households. As we will see further, the household socioeconomic status is key in our analysis since it clarifies spillovers mechanisms that drive our results.

Before discussing our spillover results, we need to clarify if starting the elementary school being older is a positive or negative shock for the focal child. According to our results, students born after the cutoff, which we define as old-for-grade, underperform students born on the left side of the cutoff. However, this effect is statistically significant only for less affluent families, confirming that we have a negative shock for the focal children from these households.¹

When we look for spillovers from an older focal child to a younger sibling, we find evidence of a negative effect driven by disadvantaged families. Our estimates show that old-for-grade children produce a negative spillover on their siblings of approximately 0.36 and 0.56 standard deviations in math and reading test scores, respectively. These estimates suggest that older siblings entering school later directly negatively influence their younger siblings through bad role modeling or potential competition for resources. Moreover, these spillover effects appear in siblings in the early years of elementary school, whose age difference with the older sibling is small and from older brothers to younger sisters.

However, when we analyze spillovers from younger focal children on their older siblings,

¹In developed countries, the literature that studies the impact of school starting age in education, labor, and health outcomes is extensive. In Brazil, this literature is limited. [Oliveira & Menezes Filho \(2018\)](#) evaluate the effect of starting elementary school later in the municipality of Belo Horizonte. Their estimates show that students born after the cutoff date have a smaller probability of grade failure in the long run. [Ryu et al. \(2020\)](#) jointly analyzes the effect of entry in school later and the elementary school reform to a nine-year cycle. They find that these two changes have a higher effect on the performance of fifth graders. As we may notice, these results came from very specific scenarios and could not be necessarily extended to our context. Thus, in the results section, we first present the cutoff rule effect on the focal children's proficiency level to verify if starting elementary school as an older student in the cohort is a positive or negative shock in our context.

we find a positive effect that is statistically significant in more affluent households. Our results show that, for more affluent families, having an old-for-grade younger sibling generates a positive effect of approximately 0.17 and 0.13 standard deviation on siblings' proficiency level of math and reading, respectively. These estimates are particularly pronounced between similar siblings who are close in age and have the same gender. Thus, our younger-to-older spillovers suggest evidence of parents' reinforcement behavior. We do not find robust sibling spillover effects on grade failure in younger-to-older and older-to-younger samples. In addition, both older-to-younger negative effects and younger-to-older positive effects are robust to different bandwidths, placebo cutoff, and different specifications using different arguments in the RD estimation. Results are also stable when dropping observations very close to the cutoff (Cattaneo & Titiunik, 2021).

Our study is related to the expanding literature of siblings externalities in the educational context. In part, our work resembles Qureshi (2018) since it brings evidence on spillovers in the context of developing countries. Still, they analyze only spillovers effects from older sisters on younger siblings in particular circumstances of gender segregation in Pakistan. Their estimates use the distance between home and school as an instrument and show that having an older sister with a high level of education improves younger brothers' school attendance, reading, writing, and numeracy skills.

Another example from the literature is Nicoletti & Rabe (2019), which employs fixed-effects strategies to estimate the spillover of having a higher perform sibling using data from England. They establish causality using older siblings' peer quality. Their estimates show that a "positive shock" on older siblings generates positive externalities on younger siblings' school achievement, which can be explained by the direct channel between siblings. Black et al. (2021), examines in three-plus-child families how having a third child that is disabled affects second born relative to firstborn. Using data from Denmark and Florida (US), they find statistically significant spillovers in proficiency levels for both samples. Both Nicoletti & Rabe (2019) and Black et al. (2021) differ from our work by using data from countries that

are very different from Brazil and an empirical strategy that requires stronger assumptions than the quasi-experimental method, such as the one that we utilize in our study.

Landersø et al. (2020), Zang et al. (2022), and Karbownik & Özek (2021) are the papers closest to ours since they use an empirical strategy based on discontinuities in school starting age created by school entry cutoff rules. In particular, Landersø et al. (2020) investigates the spillover effects of the younger child entering school later on family outcomes using Danish data. Their estimates show that if a child starts at school being one year older it improves older siblings' academic performance.² This study is different from yours as it aims to analyze spillovers in parents, looking at outcomes related to the labor market and the relationships within the household. Moreover, in their paper, families' socioeconomic status is not a central issue since Denmark is known to be a country with very low levels of inequality and poverty. However, it is very important to include the household's income level in the Brazilian scenario for two reasons. First, we can clearly understand whether our spillover effects' mechanism is through direct or indirect effects between the siblings. Second, it suggests evidence that the way educational rules are structures can contribute to an increase in inequality.

Following this line, Zang et al. (2022) and Karbownik & Özek (2021) are the closest articles to our study since both estimates siblings' spillover effects using school starting age cutoffs from the United States (North Carolina and Florida, respectively). They both find positive spillovers from older-to-younger siblings driven by less affluent families. These results are consistent with our estimates, since entering school later is a positive shock in the United States context, unlike Brazil, where we have a negative shock on school performance.³ However, related to younger-to-older spillover effects, Zang et al. (2022) do not find any significant effects, while Karbownik & Özek (2021) find negative spillovers driven by more affluent families. This difference to our results could be because school starting age has a

²As Landersø et al. (2020) results focus on family spillovers, they also show that if a child starts at school being one year older, it increases maternal employment and the likelihood that parents cohabit.

³The idea is that the focal child entering school later generates a positive spillover on siblings in the United States because the shock is positive for the focal child. Meanwhile, as in Brazil, we found that the shock is negative; we should expect negative spillovers. In both scenarios, results are significant, mainly in disadvantaged families.

different effect on the focal child in our context or even because of any other difference in the institutional background of Brazil compared to the United States. Thus, we may consider our evidence complementary to their papers since the same educational rule could yield very different externalities in countries at different levels of development.

In addition to estimating siblings' spillovers in a completely different context from the other papers, our work adopts new RD methodologies. Using the discontinuity generated by the birth date of cutoff entry rules implies being in RD with a discrete running variable framework. [Kolesár & Rothe \(2018\)](#) present that the old method by [Lee & Card \(2008\)](#), which all these papers use, has poor coverage properties and thus is not the best alternative in terms of inference. Therefore, we follow [Kolesár & Rothe \(2018\)](#) alternative method of estimation based on RD Honest and include covariates in the model using [Noack et al. \(2021\)](#), which we believe makes our estimates more reliable. Our work also provides an empirical application of new regression discontinuity design methodologies with discrete running variables.

In short, we present evidence of siblings externalities in the Brazilian educational context using recent literature on regression discontinuity design. Analyzing both older-to-younger and younger-to-older siblings' spillovers and using an income proxy to investigate the effects separately by household socioeconomic status, we have suggestive evidence of the channels through which the spillover may be acting. Our results highlight the importance of considering effects beyond, i.e., the spillovers, those usually considered when evaluating an institutional rule.

In the next section, we present briefly the Brazilian institutional background focusing on the elementary school system and its cutoff entry rule. In section 3, we show the data we used to develop this study and the descriptive statistics in our two samples. We follow with the empirical strategy in section 4, in which we also discuss internal validity. Then, we present our main results, heterogeneous effects, potential mechanisms, and robustness checks separately by the older focal child to younger sibling sample and younger focal child to older sibling sample in section 5. Finally, we conclude in section 6.

2 Institutional Background

Basic education in Brazil is divided between private and public schools, with municipal and state governments being responsible for public schools. The educational system has three stages: two years of preschool, nine years of elementary school, and three years of high school. Elementary school is divided into two cycles: first to fifth grade (early years) and sixth to ninth grade (final years). Students should learn how to read, write, perform basic quantitative operations and develop themselves socially and academically at elementary school. Children must be enrolled in elementary school at age 6, and ideally, should complete this stage by age 14 or 15, depending on their birth date.⁴

During most of the 2000s, each Brazilian state defined a cutoff date for school entry. In the São Paulo state, which will be the focus of this research, the rule has always been June 30th. However, in 2009 the federal government announced that there would be a unified cutoff date of March 31st.⁵ Thereby, some municipalities in the state of São Paulo continued to follow the cutoff date already established of June 30th, while others chose to adopt the new date of March 31st.⁶ This cutoff means that children who turned six by June 30th (or March 31st) could enter elementary school's first year. Children born after this date should wait until the next year to enroll. We will take advantage of the discontinuity generated by this rule for our identification. Therefore, we identified the cutoff date that each municipality followed and normalized it (Cattaneo et al. 2016).

Even though a variety of states implement the cutoff date rule, we choose the state of São Paulo for our analysis for several reasons. First, the state has a well-established cutoff. Since the March 31st cutoff was not officially mandatory, most Brazillian states let unclear when and if it started to follow the rule, which did not happen in São Paulo. The second

⁴Since 2009, basic education in Brazil has been mandatory from the age of 4 to 17, which includes preschool through high school. Elementary education has been mandatory throughout the country since the 1990s.

⁵It is important to emphasize that the cutoff rules mentioned here essentially apply to public schools (municipal and state schools).

⁶Later in 2018, all states had to adopt the cutoff rule of March 31st. Until then, some states such as Minas Gerais and Rio de Janeiro chose to follow their cutoff dates, while others were already transitioning to the new cutoff date.

reason is that by 2010 the state had already finished implementing the changes in basic education.⁷ This feature allows us to identify the school starting effect cleaned of potential confounder effects, which would not be possible with the implementation of several changes simultaneously. The third reason is data availability, which we will explain further in the next section.

3 Data and Descriptive Statistics

Our main source of data comes from the São Paulo State Department of Education (SEDUC-SP), which contains the record of all children enrolled in state schools. It is an administrative dataset available upon request. As we only need siblings in our data, SEDUC-SP sent us just the information of all students from families with at least two children enrolled in the state school system between 2010-2018. The idea is to consider only the students that we actually observe the exposure to the cutoff rule, which are our focal children, connected to their respective siblings. Moreover, we only used information from the focal children of the 2010-2018 cohorts because, before that, several changes were being implemented in the Brazilian education system. The focal children were born between 2004-2012.⁸ We structure the data so that each observation is a sibling whose treatment status is assigned based on the corresponding focal child. Therefore the data is a pooled cross-section at the focal child-sibling level.

Since the State Education Department has information if each student in the system has a sibling, the sibling-focal child pairing is straightforward.⁹ Furthermore, the dataset has information on the general characteristics of students such as birth date, race, gender, school,

⁷These changes include transitioning elementary school from eight to a nine-year cycle and making mandatory preschool and high school.

⁸Students born in 2004 compose the 2010s' cohort, and the logic is the same until the 2018s' cohort, which has focal children born in 2012.

⁹It is important to emphasize that our data only have information of students from the state educational system. We do not have information about students from municipal or private schools. In other words, to have information about the focal child-sibling pair in our data, we need the sibling and the focal child to be enrolled in a state school.

municipality code of child's residence, and school municipality. It also includes disability, grade failure, and school dropout. All this information is available for each cohort student (focal child) and their sibling.

We also have information if the children's family participates in the Bolsa Familia Program (BFP). The BFP is a conditional cash transfer policy operated by the federal government from 2004 to 2021. The program aims to help families from poor households and adolescents on the condition that they attend high school.¹⁰ Thus, the BFP is our proxy for socioeconomic status.

The student proficiency level information comes from publicly available data from SARESP (São Paulo State School Performance Assessment System). We match the two data using a unique student identifier from the state school system. The level of proficiency is measured through standardized math and reading tests applied to students from 3rd, 5th, 7th and 9th grades of elementary school. The measure uses the Item Response Theory (IRT) methodology that considers the quality of student answers in the test.¹¹ IRT allows for the proficiency levels to be comparable across grades and cohorts.

We restrict the sample in our main analysis to adjacent siblings pairs who took the SARESP test at least once during our sampling frame. We also drop twins because they have no variation in our treatment status and siblings whose focal child age difference exceeds eight years since we cannot credibly assert their sibship relationship (Karbownik & Özek 2021). In families where the focal child has more than one sibling in the data, we consider only the one with the closest age. This restriction avoids correlation between siblings of the same family

¹⁰In addition, if the family has a pregnant woman, beneficiaries are also required to monitor the pregnant woman's health and have their vaccinations up to date.

¹¹The IRT classifies the questions by the level of difficulty. It analyzes the consistency of the students' correct answers, reducing the chance that the student will get a good grade by luck. For instance, a student who gets all the easy questions right and none of the hard ones should receive a higher grade than a student who gets all the easy questions wrong and all the hard ones right. This happens because the IRT identifies that the second student must have got the difficult questions right by chance and not because of his knowledge. In this way, the students with the best results are those who, statistically, have a more steady and coherent evolution throughout the test.

in our data.¹² Our final dataset contains 138,859 unique focal child-sibling pairs.¹³

Table A1 presents the descriptive statistics for the siblings' raw data and our final sample after the above-mentioned restrictions. We also consider separately our two main samples: when the focal child is older (columns 1 and 2) and when the focal child is the younger sibling (columns 4 and 5). Columns 3 and 6 indicate the test of the difference of means between the raw and final samples.

Comparing columns 1-2 and 4-5, we notice that the characteristics in the raw and final data of both samples are very similar to each other, except for tests scores in 3-4. However, many differences are significant. We expect this difference with sibling birth year since we drop sibling-focal child pairs that exceed eight years. We also expect the difference with disability since a disabled child has a higher probability of not taking a test. Also, we only include the siblings who did the test in the final sample. Other differences may appear because our final sample is just a small fraction of the raw data. Yet, the test difference in the younger focal child sample may pose an external validity problem since it indicates that our sample is not representative of the population when we consider students' tests scores. This sample also excludes siblings who take the test before the focal child is exposed to the cutoff rule. This could also be a factor that leads to a sample selection of siblings with higher proficiency levels.

Table 1 introduces the siblings' descriptive statistics for the sample in which the focal child is older (columns 1-3), and the sample in which the focal child is younger than the sibling (columns 4-6). Looking to column 1, in our older focal child sample we have 62 percent of white siblings, a quarter of the families are on Bolsa Familia Program (BFP), and half of the students are girls. Only 1 percent of the kids have some disability. The dropout and school failure rates are very low. These characteristics are very similar to the younger focal child sample (column 4).

¹²Ideally, we would cluster at the family level to deal with this correlation and not exclude the other siblings. However, this is not possible since the empirical method, which is the RD Honest, did not have the cluster option when this dissertation was developed.

¹³The siblings in this final sample were born between 1996 and 2011.

Table 1: Siblings' descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Older Focal Child			Younger Focal Child		
	All	Not in BFP	In BFP	All	Not in BFP	In BFP
White	0.62 (0.49)	0.63 (0.48)	0.56 (0.50)	0.60 (0.49)	0.62 (0.49)	0.54 (0.50)
Bolsa Familia Program	0.24 (0.43)	- -	- -	0.24 (0.42)	- -	- -
Female	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)	0.49 (0.50)	0.49 (0.50)	0.49 (0.50)
Birth year	2,007.51 (1.74)	2,007.53 (1.73)	2,007.45 (1.75)	2,003.24 (2.81)	2,003.18 (2.79)	2,003.42 (2.86)
Disable	0.01 (0.11)	0.01 (0.11)	0.02 (0.12)	0.02 (0.13)	0.02 (0.12)	0.02 (0.13)
Reading test score	158.84 (82.56)	161.62 (82.41)	150.17 (82.43)	206.82 (79.29)	210.02 (79.60)	196.51 (77.37)
Math test score	179.94 (92.24)	182.56 (92.07)	171.77 (92.33)	218.33 (80.21)	221.26 (80.44)	208.86 (78.74)
Drop out	0.00 (0.03)	0.00 (0.02)	0.00 (0.03)	0.00 (0.04)	0.00 (0.05)	0.00 (0.04)
School failure	0.03 (0.17)	0.03 (0.16)	0.04 (0.20)	0.05 (0.21)	0.04 (0.21)	0.05 (0.23)
N	44,962	34,059	10,903	93,897	71,709	22,188

Note: Columns 1-3 include all younger siblings enrolled in the state public school with an older focal child (i.e., an older sibling exposed to the cutoff rule) from the 2010-2018 cohort. Columns 4-6 consider all older siblings enrolled in the state public school with a younger focal child from the 2010-2018 cohort. In columns 2 and 5, we have only siblings from households that do not participate in Bolsa Familia Program. On the other hand, in columns 3 and 6, we have only siblings from households participating in Bolsa Familia Program. Standard deviations in parentheses.

An important component of our analysis is the family socioeconomic status (measured by participation in BFP). Thus, columns 2-3 and 5-6 report the descriptives for each of our two samples of interest divided by siblings whose families do or do not participate in the Bolsa Familia Program. Comparing columns 2-3 and 5-6 with each other, we notice that the proportion of white students is lower for less affluent families, as expected. Moreover,

reading and math test scores are lower in those families. However, the other characteristics of siblings, like gender, birth year, disability, dropout, and school failure, seem well balanced in terms of family socioeconomic status.

Notably, the dropout rates are very low in all columns. One of the reasons for this is that according to *Pesquisa Nacional por Amostra de Domicílios* of 2019 (PNAD 2019),¹⁴ the state of São Paulo has one of the lowest dropout rates in Brazil (1.8 percent) which also has been decreasing in recent years. Moreover, dropout is a prevalent social problem among teenagers in Brazil, as it is strongly associated with child labor, early pregnancy, alcoholism, drugs, and other high school students issues. As our sample only includes much younger siblings in elementary school, the expected dropout rate for our subsample must be less than this general rate of 1.8 percent.

4 Empirical Strategy

4.1 Regression Discontinuity Design

Our goal is to estimate the siblings' spillover causal effects on proficiency level and probability of failure. However, this is usually difficult due to well-known problems with simultaneity, correlated unobservables, and reflection (Manski 1993, 2000). In principle, these issues could be a concern since siblings grow up in the same household, share several characteristics and experiences, and have genetic similarities.

To overcome this identification problem, the idea is to explore the discontinuity generated by the school-entry cutoff date. In families with similar socioeconomic backgrounds, with siblings that share the same demographic characteristics, we expect that those siblings whose focal children (i.e., siblings exposed to the rule) are near an admission threshold to be very similar.¹⁵ In this context, RD should eliminate concerns about correlated effects. We can

¹⁴*Pesquisa Nacional por Amostra de Domicílios* is a national representative sample survey held annually at the household level to investigate a series of characteristics of Brazilian society such as education, work, housing, health, migration, etc.

¹⁵We want to measure the spillover effect of the focal child on the sibling, and the idea is to use the spillover

also rule out issues related to the reflection problem since the spillover effect generated by the cutoff rule only works from the focal child to the sibling and not the other way around. In other words, we will be measuring spillover only in a direction that will not consider the reflection of the siblings' behavior on the focal child.

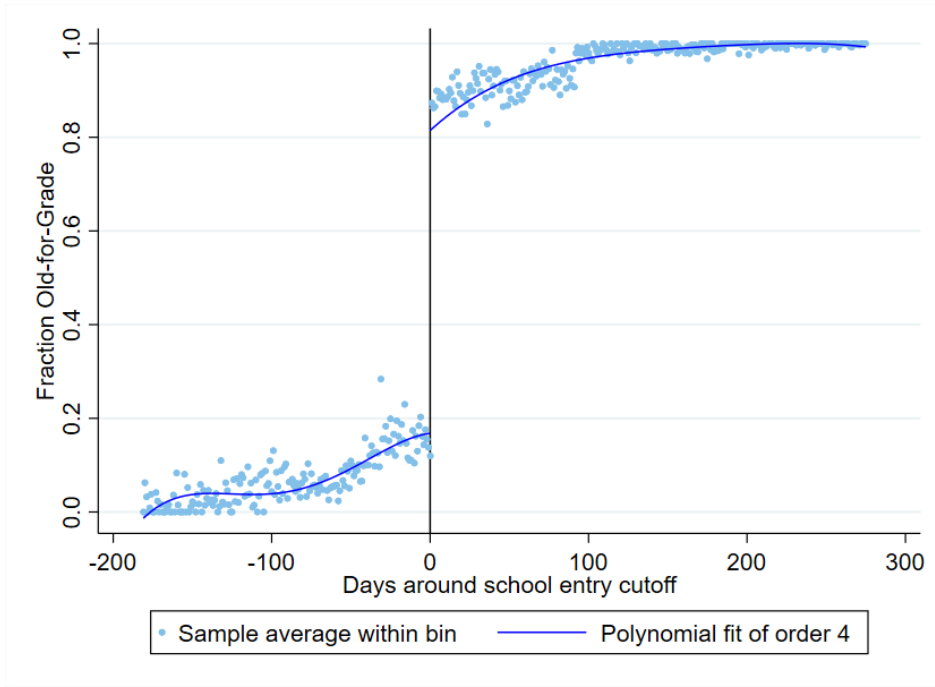
Students born right before the cutoff date will enroll in primary school before they complete six years old. In contrast, children born right after the cutoff date will have to wait the next year to enter school. Each student exposed to this rule is the focal child of your sample, who is linked to her or his sibling. Thus, as we want to analyze siblings' spillover effects, our RD design will compare the outcomes of siblings of children born right before and right after school-entry cutoff.

It is important to remember that we consider both spillovers from an older focal child to a younger sibling and spillovers from a younger focal child to an older sibling. Thus, we have two samples whose estimations of the spillover effects are conducted separately. We divide the analysis since the nature of spillover from an older sibling to a younger can be very different from the nature of younger siblings' influence on their older sibling, as discussed in the Introduction. Furthermore, since the mechanism behind the effect also can be very different if the focal child is older or younger, we keep the two analyses separately from the next section onwards. However, the methodology using regression discontinuity design is the same in both samples.

Figure 1 illustrates the fraction of old-for-grade children by birth date around the cutoff marked by the vertical line. The literature stipulates that old-for-grade children as those who are older in their cohort, i.e., with at least six years old completed, because they were born shortly after the cutoff rule and had to wait to enroll in elementary school. We define that the focal child-sibling pair is in the treatment group if a child is old-for-grade.

generated from the cutoff rule. Thus, we want the child-sibling focal pair from our control group to be a good counterfactual to the child-sibling focal pair from our treated group. These assumptions are true if the focal child is near the cutoff (RD assumptions) and includes siblings' covariates. The control variables are necessary since we don't want this spillover to be due to a gender or age difference, for instance, between siblings in the treated and control group.

Figure 1: Fraction of old-for-grade by date of birth



If we were in a sharp RDD scenario, the blue lines above and below the cutoff would be horizontal in one and zero, respectively. However, as we can see in Figure 1, we are in the fuzzy framework because a few students do not appear to be following the cutoff rule. We have some students enrolling late, which means a focal child born before the cutoff date but just enrolled in the first year of elementary school after she turned six years old. Those students explain why we saw dots above zero on the left of the cutoff line. As they were enrolled later, they were classified as old-for-grade students regardless of their birth date. We also have a few cases of students who enrolled at elementary school when they should have waited to enroll in next year according to the cutoff rule. They explain why we have a few dots below one on the right of the cutoff line. Apart from these few exceptions, it is clear in the graph that we have a large discontinuity after the central line of more than 60 percentage points.^{16,17}

¹⁶In addition, the noncompliance rate in our sample is 6 percent, which is a very low.

¹⁷Table A2 presents the results from the first stage regression using an indicator variable for born after the cutoff, which is our instrument for being old-for-grade. All coefficients in our specifications are significant at the 1 percent level, which indicates that the instrument is relevant. Estimates in the table show that being born after the cutoff date increases the likelihood of a child starting school being old-for-grade (i.e., starting

Therefore, as a standard fuzzy framework, the parameter of interest can be written as the ratio of two sharp RD estimands:

$$\beta = \frac{\alpha_y}{\alpha_t} = \frac{\mathbb{E}[Y_i | D_j = 0^+] - \mathbb{E}[Y_i | D_j = 0^-]}{\mathbb{E}[T_j | D_j = 0^+] - \mathbb{E}[T_j | D_j = 0^-]} \quad (1)$$

Y_i indicates the outcome of sibling i . D_j is the difference between the birth date of sibling i 's focal child j and the school-entry cutoff date (i.e., our running variable), with positive values indicating dates after the cutoff. In other words, if a focal child has $D_j \geq 0$ she is eligible to start school a year later than students with $D_j < 0$.¹⁸ T_j is an indicator for a focal child who had to wait until the following year to enter school, i.e., is old-for-grade.

As our running variable is discrete, we need to use an appropriate RD fuzzy estimator for this framework. In this scenario, the most recent literature that uses school starting age for identification follows [Lee & Card \(2008\)](#) methodology, which has poor coverage properties ([Kolesár & Rothe 2018](#)). Thus, to deal with these issues, we estimate the parameter of interest using the RD honest proposed by [Kolesár & Rothe \(2018\)](#). Moreover, we need siblings to have similar characteristics so that the parameter we estimate does not have any bias because siblings are not comparable. As mentioned at the beginning of the section, without siblings' covariates, we may not eliminate correlated effects. Thus, we need to include a vector of sibling and focal child characteristics following [Noack et al. \(2021\)](#). The control variables include the focal child's school-entry cohort indicators¹⁹ and gender, the sibling's year of birth, the month of birth, grade, gender, and race. We also include the age difference between the focal child and the sibling in days.

As mentioned in the previous section, we divide the sample of our main analysis into students from less affluent families and more affluent families, using as a proxy the indicator (school later) by 50 percentage points for both our samples (column 3 of panels A and B).

¹⁸For instance, if we have $D_j = -10$ means that focal j was born on June 20th, while $D_j = 10$ implies that student j was born on July 10th in the case of June 30th cutoff.

¹⁹Our focal child's school-entry cohort fixed effects are at the cutoff year level and control for time-specific shocks that may affect children around a given cutoff differentially across cohorts. Still, it is important to clarify that these fixed effects do not control for an additional year of schooling that focal children born before the cutoff get compared to those born after the cutoff.

for participating in Bolsa Familia Program. We also investigate the presence of heterogeneous effects dividing our sample by sibling grade, the age gap between sibling and focal child and both sibling and focal child gender. This analysis helps us understand the channels of the spillover effects, which we discuss in more detail in the results section.

4.2 Internal Validity

An important RD feature is that treated units are similar to control units in observable characteristics. This assumption means that except for their treatment status, units just above and just below the cutoff should be similar in all variables that could not have been affected by the treatment. We can test this assumption by estimating if there is a discontinuity around the focal child's school-entry cutoff date using sibling's predetermined covariates as outcomes. [Table 2](#) presents this falsification test in which panel A has older focal child spillovers sample, and panel B presents younger focal child sample.

The covariates considered are the sibling white and female dummy, if the sibling is from a less affluent family using the Programa Bolsa Família dummy as proxy, the age difference in days between the focal child and sibling, the sibling month and year of birth. We set our bandwidth to simplify our balance test analysis within 60 days of the school starting cutoff.^{20,21} The 60 days bandwidth choice is the widest option when using [Calonico et al. \(2017\)](#) optimal procedure and contains all students who can actually enter our RD analysis. It is also consistent with other papers using RD design in the context of school-entry cutoff ([Karbownik & Özek 2021](#)).

In panel A (older focal child sample), it is visible that we have a covariate balance since the discontinuity is not significant for all predetermined covariates considered. However, in panel B (younger focal child sample), there is a significant difference between our treated (old-for-grade) and our control group regarding age difference and birth year. All these

²⁰Since we have different outcomes, if we didn't fix the bandwidth within 60 days we should expect different optimal bandwidths in each column since the optimal bandwidth procedure varies for each outcome.

²¹The only column we have fewer observations for is the white dummy because a small portion of our sample chooses not to report race.

Table 2: Discontinuities in background characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Female	White	BF Program	Age Difference	Birth Month	Birth Year
Panel A. Older Focal Child						
Focal child born after school entry	0.041 (0.03)	0.051 (0.03)	-0.039 (0.03)	31.530 (27.22)	-0.103 (0.22)	-0.171 (0.11)
Eff. sample size	3012	2400	3012	3012	3012	3012
Panel B. Younger Focal Child						
Focal child born after school entry	0.028 (0.02)	-0.032 (0.020)	0.044 (0.03)	-72.147*** (26.80)	0.105 (0.13)	-1.304*** (0.21)
Eff. sample size	6791	5638	6791	6791	6791	6791

Note: This table presents discontinuities in background characteristics using RD Honest method and fixed 60 days bandwidth for both samples: older focal child in panel A and younger focal child in panel B. The older focal child sample is the one we have an older sibling who was exposed to the cutoff rule, and we analyze the spillover effect in the younger sibling. The younger focal child sample is when we look over the spillover effect from a younger child exposed to the rule to their older sibling. Our outcomes variables are: indicator of being a female sibling, indicator of being a white sibling, indicator for family being on Bolsa Familia Program, the age difference between focal child and sibling, the sibling month and year of birth. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

results are the same if we consider the optimal bandwidth procedure of RD Honest instead of setting 60 days around the cutoff.

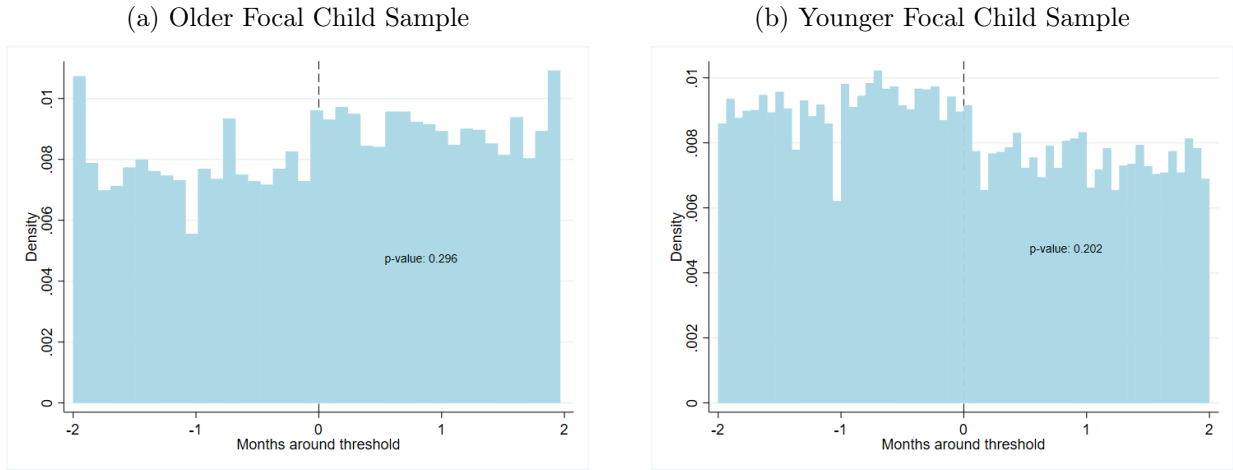
In panel B column 4 we have a negative and significant effect for age difference. Since the variable represents the age of the focal child minus the sibling age and we are in the younger focal child sample, we expect the range of age difference in this sample to be negative for all values. Thus, our analysis can be made in absolute value. In addition, we have that the treatment groups' age difference is larger than the control groups'. Since the literature recognizes that spillover effects are larger in siblings who are close in age, this imbalance will be underestimating our results.²² In column 6 of the same panel, we have a negative effect for birth year indicating that the siblings of the treated group were born in earlier years, i.e., are older than the ones of the control group. Those two discontinuities are significant and may bias our results. Therefore, as mentioned before, we will use all these predetermined covariates as controls in our causal investigation.

An important information in [Table 2](#) is that there are no discontinuities in our measure of socioeconomic status (column 3) in both panels. This result shows that, unlike what was found by [Landersø et al. \(2020\)](#), the socioeconomic status did not change due to some parental reaction to their child entering school sooner or later. In other words, our treated group (i.e., siblings whose focal child was born after the cutoff) was not more likely to come from low-income families than our control group (i.e., siblings whose focal child was born before the cutoff).

Another falsification test that we present is the number of observations near the cutoff. If there is no manipulation, we expect the same number of siblings just above the cutoff as just below it ([Cattaneo et al. 2020](#)). We can analyze this by looking at the distribution of the running variable in our samples. In [Figure 2](#), we have the distribution of sibling observations around the focal child school-entry cutoff for both our samples: older focal child and younger

²²As presented by [Joensen & Nielsen \(2018\)](#), siblings who are close in age are more likely to directly influence each other's as they may share the same social environment, like school and household, and also friends.

Figure 2: Distribution of sibling observations around focal child school-entry cutoff



Note: The figure shows the histograms of the density of children born around their focal child's school-entry cutoff. The sample in panel a. is the one that the focal child is older than the sibling, while panel b. is the one with the younger focal child. We restrict the bandwidth to 60 days around cutoff which contemplates our two main samples. P-values are based on density test, run at the daily level, as proposed in [Cattaneo et al. \(2018\)](#).

focal child. Each daily bin contains relatively few observations in the younger focal child sample, making the daily graph very noisy. Thus, following [Karbownik & Özek \(2021\)](#), we chose to bin the running variable every two days for exposition purposes. In addition, we also present a formal test of smoothness of density ([Cattaneo et al. 2018](#)) and show the result for each sample in each graph.

Therefore, based on the test and the distribution of the running variable, we cannot reject the hypothesis of no discontinuity in the distribution density around the cutoff in both samples. The p-values are 0.296 and 0.202 for the older focal child and younger focal child samples, respectively.

Considering that we are in the RD fuzzy framework, our estimates can be interpreted as a local average treatment effect (LATE). Thus, it needs to satisfy the exclusion restriction. This assumption would not be valid if the rule that makes a focal child enter school earlier affects parents' decision to have another child. This problem may appear in our older focal child sample and can lead us to misinterpret our reduced form effects. For instance, if the older brother enters school later, and then parents need to spend more time and attention

with the child at home, this could lead the parents to postpone the decision of having another child.

Since the focal child in our sample is exposed to the rule around six years old, this scenario is only possible with a focal child whose age difference with the siblings exceeds six years. Thus, in [Table A3](#), we estimate our main results for the older focal child sample considering only siblings whose age difference with the focal child is less than six years (i.e., we exclude siblings with an age difference between six and eight years). As our results do not change significantly, it is reasonable to conclude that the rule is not affecting parents' fertility decisions.

5 Results

This section presents results on sibling spillovers separately for both samples. The first is the older focal child sample (i.e., when the older sibling is exposed to the rule and we analyze the spillover effect on the younger sibling). The second is the younger focal child sample (i.e., when the younger sibling is exposed to the rule and we study the spillover effect on the older sibling).

For both samples, we present our main results, in which we answer if a school rule can generate siblings' spillovers. Then, we investigate heterogeneous effects that may help us understand the potential mechanism of the spillover. In the following, we also present our robustness checks.

However, before we show our siblings' spillover estimates, we need to understand if entering elementary school being older in the cohort is a positive or negative shock for the focal children.²³ [Table 3](#) presents the effect of being old-for-grade on the focal childrens' school

²³It is important to clarify here an issue related to preschool. The elementary school cutoff entry rule also applies to preschool. The difference is that the child needs to be close to turning six years old in elementary school, while the reference age is four years old in preschool. So, when the child is old-for-grade, she was outside school an additional year compared to the focal child in the control group before entering preschool. After two years in preschool, the child is exposed to the cutoff rule once again upon entering elementary school.

outcomes. Here we are considering the focal children of both samples (presented in columns 1 and 2). Then we restrict the full sample to families who are not in Bolsa Familia Program (columns 3 and 4) and who are in Bolsa Familia Program (columns 5 and 6). Each panel contains a different outcome: panel A has math standardized test scores, and panel B has the reading standardized test scores. In panel C, our outcome is an indicator variable for the student who has ever failed a grade in school. In columns 1,3 and 5, we do not have control variables, and in columns 2,4, and 6, we add focal children covariates. We present the standard errors in parenthesis.

Estimates show that being old-for-grade negatively affects students' proficiency level in math and reading (panel A and B). In our preferred specification with covariates, the effect is significant only for less affluent families (column 6). Therefore, the school cutoff rule can be considered a negative shock for children who need to wait until the next year to enroll in the first grade, mainly for students from disadvantaged families. This result is consistent with [Ryu et al. \(2020\)](#) that finds positive effects for a child that enters elementary school earlier on the proficiency level of math and reading.²⁴ These results on the focal child outcomes will help us understand the spillover mechanisms in our younger-to-older sample, and it will be indispensable to explaining our older-to-younger spillover effects.

²⁴In panel C of [Table 3](#), we find a negative effect on the probability of failure for more affluent families. Yet, as we will see in the following, this outcome is not important for our sibling spillover analysis, we chose not to focus on this result.

Table 3: Cutoff rule effect on the focal child

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Not BF Program		BF Program	
Panel A. Math Score						
Focal child born	-0.1538***	-0.0191	-0.1727***	-0.0219	-0.1043*	-0.0972*
after school entry	(0.040)	(0.037)	(0.051)	(0.034)	(0.060)	(0.055)
Eff. sample size	7285	7536	4994	8206	2419	2260
Panel B. Reading Score						
Focal child born	-0.1406***	-0.0110	-0.1584***	-0.0050	-0.1239***	-0.0940*
after school entry	(0.038)	(0.031)	(0.047)	(0.036)	(0.051)	(0.053)
Eff. sample size	7771	9867	5435	7396	3174	3214
Panel C. Probability of Failure						
Focal child born	-0.0156***	-0.0319	-0.0181***	-0.0443*	-0.0089	0.0169
after school entry	(0.005)	(0.023)	(0.006)	(0.023)	(0.013)	(0.052)
Eff. sample size	9536	16072	8682	14477	2245	3762
Controls		✓		✓		✓

Note: The table presents the effect of being old-for-grade (our treatment), i.e., starting the first year of elementary school with at least six years old completed, in the focal childrens' outcome using the RD Honest method. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-2, we have the full sample; in columns 3-4 and 5-6, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 3, and 5 do not include any controls; columns 2, 4, and 6 include student controls. These covariates are gender, race, cohort, grade, and month of birth. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.1 Older Focal Child Sample

Table 4 presents our main results for the older focal child sample. Columns 1-3 of the table consider our full sample of siblings. Then we restrict the sample to families who are in Bolsa Familia Program (columns 7-9) and who are not in Bolsa Familia Program (columns 4-6). Each panel contains a different outcome: panel A has math standardized test scores and panel B has the reading standardized test scores. In panel C, our outcome is an indicator variable for the sibling who has ever failed a grade in school. In columns 1,4 and, 7 we do not have control variables; in columns 2, 5, and, 8 we only add siblings covariates. We also

Table 4: Main results of sibling spillovers in the older focal child sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	-0.0539	-0.0952*	-0.1462*	-0.0102	-0.1297	0.0929	-0.2303**	-0.3231**	-0.3584**
after school entry	(0.055)	(0.056)	(0.085)	(0.062)	(0.089)	(0.124)	(0.108)	(0.164)	(0.175)
Eff. sample size	3275	3696	1595	2620	1507	863	570	426	311
	Panel B. Reading Score								
Focal child born	-0.0194	-0.2151**	-0.2780***	0.0063	-0.2057*	-0.1520	-0.1741*	-0.4522***	-0.5577***
after school entry	(0.056)	(0.070)	(0.081)	(0.074)	(0.105)	(0.113)	(0.104)	(0.157)	(0.165)
Eff. sample size	2851	2521	1821	1704	2861	1517	702	359	307
	Panel C. Probability of Failure								
Focal child born	-0.0039	-0.0621	0.0373	-0.0086	-0.0592	-0.0154	0.0102	0.1279	0.2018
after school entry	(0.013)	(0.055)	(0.056)	(0.011)	(0.046)	(0.046)	(0.031)	(0.137)	(0.132)
Eff. sample size	1891	3101	3103	2485	4099	4079	373	613	609
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

include focal child controls in the last specification (columns 3,6 and 9). We present the standard errors in parenthesis.

We notice that having an old-for-grade sibling generates a negative spillover effect in the student compared to other students whose sibling enrolled in elementary school early. This negative effect appears for proficiency level in math and reading (panels A and B). In addition, these estimated effects are statistically different from zero in columns 2-3 and 7-9. We observe this negative spillover in our full sample (columns 1-3). However, when we separate by our socioeconomic level proxy, the effect is significant and robust only in less affluent families (columns 7-9). In this subsample, the spillover is even larger, meaning that sibling pairs in less affluent families lead to these spillover effects.

In particular, in column 3, we find that having an old-for-grade older sibling generates a negative effect of approximately 0.15 standard deviation on sibling math proficiency level and 0.30 standard deviation on sibling reading test scores. For less affluent families, this negative effect is approximately 0.36 and 0.56 standard deviation in math and reading (column 9), respectively. The effect is negative and smaller in families with better socioeconomic conditions (approximately 0.10 and 0.15 standard deviation in our preferred specification) but not statistically significant. A larger effect for reading is consistent with the literature since reading skills is more associated with the family background than math skills.²⁵ Furthermore, the literature on teacher value-added shows that the teachers are the ones who have a larger influence on students' math performance (Hanushek & Rivkin 2010). We do not find significant spillover effects in the probability of grade failure for both less and more affluent families.

We illustrate our results in Figure 3. The figure presents the daily means and linear fits for the bandwidth of 60 days around the focal child school entry cutoff for our full sample (Graphs a. and b.), just for families who participate in Bolsa Familia Program (Graphs c. and d.) and only families who do not participate (Graphs e. and f.). On the vertical axis, we use each outcome cleaned from the covariate effect, i.e., the residual outcome as proposed by Noack et al. (2021). In graphs a., c., and e., we have the discontinuity in math test scores, and in graphs b., d., and f., the discontinuity in reading test scores. The graphs confirm a discontinuity in younger siblings' tests scores, and when we restrict to less affluent families, this difference seems larger. Thus, our conclusions of spillovers driven by less affluent families remain unchanged.

But why are these spillovers driven by disadvantaged families? First, as discussed in the introduction, the mechanisms through which those spillover effects may be acting are direct interactions between siblings. The literature (Karbownik & Özek 2021, Nicoletti &

²⁵Students from less affluent families are less likely to have experiences that encourage the development of fundamental skills for reading acquisition, such as phonological awareness, vocabulary, and oral language (Buckingham et al. 2014)

Rabe 2019, Qureshi 2018) recognizes that the direct effect is more plausible when looking at a spillover from an older to a younger sibling, mainly in disadvantaged households. Older siblings from less affluent families often take the role of the parents at home (Burton 2007), which is even more natural in single-parent households (Conley & Lareau 2008). This means that the probability of siblings spending more time together in those households is higher, and the older siblings can be the main tutor and role models of their younger brothers and sisters.²⁶

Zang et al. (2020) shows that older siblings in disadvantaged families have greater negative spillovers on their younger siblings' outcomes, concluding that sibling spillovers are an important channel through which inequality within families is produced. When the older sibling is a bad role model, less affluent families do not have the same resources that affluent families have to mitigate negative sibling spillovers.²⁷ This argument aligns with our spillover effects results driven by families who participate in the Bolsa Familia Program.

Our siblings' spillover effect generated by school starting age is consistent with the literature, but why are they negative? We discussed that effects from an older to a younger sibling are usually associated with the direct effect channel, especially among less affluent families. Then, following this assumption, the negative results mean that the older child who enters school later is a worse role model to their sibling than the older child who enters school earlier. Thus, we should expect that the cutoff rule somehow negatively affected the old-for-grade focal child. This result is what we find on Table 3, i.e., entering elementary school being older is a negative shock, mostly for focal children from disadvantaged families.

This negative effect on the proficiency level of the focal child that enters school later from less affluent families could be due to several factors. First, an older sibling from a less affluent family that needs to wait an additional year to enroll in school may be exposed to greater

²⁶An older sibling with good grades may help construct the younger siblings' aspirations in school. In addition, students with good grades will have a high probability of having good behavior in the school environment and influence their siblings (Zang et al. 2020). This mechanism may be happening in households where the older sibling enters school earlier (our control group).

²⁷By contrast, younger siblings from advantaged families may have a "compensatory advantage" and be less affected by older siblings' negative outcomes (Bernardi 2014).

resource scarcity when compared to the student who may enter school earlier. In [Table 2](#), we have already shown that there is no income difference (using Bolsa Familia participation as a proxy) between treated and control families. But, this shortage could be a lack of attention and food, which are resources that Brazilian public schools offer to children.²⁸ In this context, an additional year outside school may cause both siblings to compete for resources in the treated group. In contrast, the focal child starting school a year earlier could free up parental resources, which could, in turn, benefit the sibling.²⁹

In addition to this competition for resources, staying outside school longer correlates with greater behavior problems. For example, [Bertrand & Pan \(2013\)](#) shows that boys raised in broken families have twice the rates of behavioral and disciplinary issues as boys raised in more affluent families. Thus, once again, it makes sense to think that those older brothers from our treatment group are bad role models to their siblings compared to the brothers of our control group.

In short, entering school a year later has a negative effect (for several possible reasons) on the older students of disadvantaged families, negatively affecting their younger siblings in terms of their reading and math proficiency levels. In the next subsection we continue to explore the potential mechanism that drives our spillover through heterogeneous effects. Heterogeneous gender effects, for example, may clarify whether the behavior problems mentioned above are indeed a reasonable story for our context.

²⁸In line with this argument, [Levasseur \(2022\)](#) finds that entering elementary school later has a negative effect on the focal child's nutritional status, mainly in students from underprivileged settings. These results may correlate with our negative shock on proficiency levels since poor nutrition among children can be associated with decreasing learning ([Glewwe & Miguel, 2007](#)).

²⁹When we mentioned that our treated focal child stayed outside school an additional year compared to our control focal child, two issues need to be clear. First, this additional year was before preschool since this stage is mandatory during the period we are analyzing. Second, before preschool, we have no way of knowing if the child would be at home with their parents or in a daycare. Here, our main argument is that in treated families, parents need to take care of two children (focal child and sibling) for one more year than parents in the control group. And this "take care" may include them taking care of children, getting a third person to do it, or enrolling the children in daycare. Our point is that taking care requires parents' resources in all scenarios.

5.1.1 Heterogeneous Effects

Now that it is clear that our focal treated child experiences a negative shock upon entering school later, we can think about how this negative effect may be propagating to the sibling. Taking a step back, one discussion that may arise is when the older children are role modeling their siblings. These interactions occur during early childhood when they are outside school? Or does it happen when both focal child and sibling are at school (contemporaneous effect to sibling taking the test)? This question is hard because we do not observe the children before enrolling in elementary school.

However, we can check if the focal child is currently enrolled at school the year that the sibling takes the test. Suppose the focal child is not at school. In that case, the influence between the focal child and sibling probably happens in early childhood. Looking at our data, the dropout rates of the focal child are very low (6 percent), which means that the focal child was enrolled at school when the sibling took the test. Thus, it is plausible that this spillover mechanism may happen both in early childhood and when both siblings are in school. Heterogeneous effects by sibling grade and the age gap between siblings may help us understand these mechanisms.

First, we estimate heterogeneous effects by sibling grade, which can help explain if the spillover effect occurs when the sibling is at early years of elementary school or later. [Table 5](#) shows the spillover effects of having an old-for-grade sibling in the older focal child sample separated by sibling grade. We estimate the spillover effects considering siblings in third (columns 4-6) and fifth grade (columns 7-9) separately. The very small number of seventh and ninth-grade siblings were not considered since the sample is too small. Looking at [Table 5](#), we have a sibling spillover effect that is negative on the proficiency level of math and reading for both grades (columns 4-9). However, the effect is significant only for siblings of third grade (columns 5-6 of panel A and B), and the magnitude of the effect on the proficiency level of math and reading is similar to the effect of the full sample (columns 1-3). Once again, the spillover effect is larger for reading than math (approximately 0.30 and 0.20

Table 5: Heterogeneous effects divided by sibling grade - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			3rd grade			5th grade		
Panel A. Math Score									
Focal child born	-0.0539	-0.2435***	-0.1879**	-0.0626	-0.2378**	-0.1988*	-0.0472	-0.1443	-0.2651
after school entry	(0.055)	(0.056)	(0.085)	(0.079)	(0.120)	(0.117)	(0.089)	(0.199)	(0.199)
Eff. sample size	3275	3696	1595	1653	817	830	536	333	302
Panel B. Reading Score									
Focal child born	-0.0194	-0.3139***	-0.3279***	-0.1493*	-0.3500**	-0.3060***	-0.0456	-0.0523	-0.1290
after school entry	(0.056)	(0.070)	(0.081)	(0.091)	(0.125)	(0.103)	(0.086)	(0.185)	(0.173)
Eff. sample size	2851	2521	1821	1139	758	1050	565	375	399
Panel C. Probability of Failure									
Focal child born	-0.0039	-0.0621	0.0373	-0.0064	0.1297	0.2726***	0.0106	0.0665	-0.1504
after school entry	(0.013)	(0.055)	(0.056)	(0.016)	(0.091)	(0.109)	(0.010)	(0.192)	(0.191)
Eff. sample size	1891	3101	3103	1541	1016	726	584	351	350
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by sibling grade using RD Honest method for the sample in which the focal child (i.e., the student exposed to the cutoff rule) is older than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings who are in third and in columns 7-9 we restrict for siblings in fifth grade. As the sample was too small for siblings in the seventh grade, we didn't include it in our analysis. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

standard deviation, respectively). These results mean that the older focal child influences their younger sibling at the beginning of the sibling school path.

We also analyze spillover heterogeneous effects divided by sibling focal child age gap (Table 6). In columns 4 to 6, we consider only siblings whose age difference with the focal child is less than four years, which we will call small age gap. In columns 7 to 9, we consider siblings whose age difference with the focal child is from four years and one day to eight years (denoted a big age gap). These two groups are defined such that each group contains approximately half of our sibling sample. Results indicate that the negative spillover effect is driven by siblings who are close in age with their focal child (columns 4-6). The magnitude

Table 6: Heterogeneous effects divided by age gap - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Small Age Gap			Big Age Gap		
Panel A. Math Score									
Focal child born	-0.0539	-0.0952*	-0.1462*	-0.0695	-0.2209**	-0.1916**	-0.0253	-0.4870	-0.0207
after school entry	(0.055)	(0.056)	(0.085)	(0.080)	(0.101)	(0.097)	(0.169)	(0.381)	(0.240)
Eff. sample size	3275	3696	1595	1610	684	1236	201	146	257
Panel B. Reading Score									
Focal child born	-0.0194	-0.2151**	-0.2780***	-0.0025	-0.3356***	-0.3189***	-0.1159	-0.5051	-0.1506
after school entry	(0.056)	(0.070)	(0.081)	(0.073)	(0.134)	(0.100)	(0.131)	(0.398)	(0.216)
Eff. sample size	2851	2521	1821	1741	747	1164	288	141	296
Panel C. Probability of Failure									
Focal child born	-0.0039	-0.0621	0.0373	0.0076	-0.2774***	-0.2568***	-0.0594*	0.4537	0.6175**
after school entry	(0.013)	(0.055)	(0.056)	(0.014)	(0.109)	(0.092)	(0.034)	(0.321)	(0.269)
Eff. sample size	1891	3101	3103	1651	805	1040	387	158	212
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by age gap using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings whose age gap with the focal child is small and in columns 7-9 we restrict for siblings whose age gap is big. We define a small age gap as an age difference between sibling and focal child being less than four years. A large age gap is an age gap of four years or more. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and on panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include gender and cohort of entry. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of the effect on math and reading is slightly larger than the full sample (columns 3 vs 6). We also find a statistically significant spillover effects on the probability of failure (columns 6 and 9 of panel C), different from before that the effect was not significant.

Both heterogeneous effects, from [Table 5](#) and [Table 6](#) estimates, suggest that the spillover effects happens during siblings' early childhood and first years of school, probably before the focal child enters adolescence stage. This result also weakens the possibility of mentoring from the oldest to the youngest, but strengthens the idea of having a peer relationship in which the oldest is in fact a role model for the youngest in terms of behavior and school performance. As pointed out by [Zang et al. \(2020\)](#), siblings with close-spaced age are a special type of peers. This happens because those siblings may have interactions that are stable, intimate and emotionally intense. Siblings with small age spacing usually are associated with a direct competition for resources. This competition may also be associated with a larger scarcity of resources in the treatment family that we discussed in the previous subsection. Thus, we hypothesize that all these mechanisms contribute to a worse environment in the treated households, affecting the sibling directly or indirectly. As discussed by [Heckman \(2006\)](#), early family environments can shape childrens' cognitive and non-cognitive abilities.

But why do these spillover effects fade out as the siblings' first elementary school years? The next table of heterogeneous effects by gender may help us answer this. On [Table 7](#), we analyze siblings spillover effects divided by each possible gender pair between sibling and focal child. In the first three columns, we have the spillover effect on girl siblings, and in the last three, we only consider the spillover on boy siblings. In each panels' first set of lines, we only have the effect generated from the focal child girls. In the second set of each panel, we have the spillover effect generated by focal children boys.

When we analyze brothers' spillovers (columns 4-6), we can see that the estimates are negative but not statistically significant. Now, looking at sisters spillovers (columns 1-3), we have a negative effect on the proficiency level of math and reading (both panel A and B) generated by both female and male focal children. However, the estimates are significant

only when her older sibling is a boy (second set of lines in each panel), considering our specifications with controls variables (columns 2-3). This result is the same if we consider separately siblings' and focal child's gender heterogeneous effects (Table A4, Table A5).

These estimates indicate that older brothers being old-for-grade generates negative spillover effects in their youngest sisters. The magnitude of the effect is very close for reading tests scores when we compare to our main results of Table 4 (column 3 panel B). For math tests scores, the estimates are larger for siblings girls (column 3 panel A).

Hence, we have that our spillover results are driven by older brothers affecting their youngest sisters. This may be associated to the reason that results fade out after the sibling progress in elementary school. The first reason may be that after both child are at school, the scarcity of resources is mitigated, becoming just a question of income, in which there is no difference with the control group. The second reason is that the influence that the older brother has on his sister weakens. In addition, psychologists recognize that as adolescence approaches, children look more to their peers for approval, especially individuals of the same gender or with similar interests (Ardelt & Day 2002). This argument contributes to the idea that girls, after a while, have a stronger connection with a friend who shares the same gender and interests than her brother.

Therefore, our spillover effects from older brothers to younger sisters seem to be short run effects, that happen probably due to a bad role modeling and a bigger competition for non-monetary resources relative to the control group. As the early school years go by, the younger sister has her relationship with the older brother weakened, while he strengthens connections with other peers. This, together with the smoothing of resource scarcity, softens the competitiveness between siblings undermining the spillover effect.

5.1.2 Robustness

This subsection presents our robustness analysis to our main results of Table 4. First, we estimate the main results using fixed bandwidths of 30, 45, and 60 days. Table A8, Table A7

Table 7: Heterogeneous effects divided by sibling and focal child gender - Older FC sample

		(1)	(2)	(3)	(4)	(5)	(6)
		Female Sibling			Male Sibling		
Panel A. Math Score							
<i>Female</i>	Focal child born	-0.0299	-0.3124	-0.2078	-0.1933*	-0.2919	-0.2018
<i>Focal</i>	after school entry	(0.127)	(0.208)	(0.165)	(0.111)	(0.171)	(0.180)
<i>Child</i>	Eff. sample size	583	293	426	546	315	257
<i>Male</i>	Focal child born	-0.0813	-0.1900**	-0.2693***	-0.2026	0.0116	-0.1115
<i>Focal</i>	after school entry	(0.145)	(0.095)	(0.115)	(0.199)	(0.198)	(0.134)
<i>Child</i>	Eff. sample size	501	316	523	361	352	626
Panel B. Reading Score							
<i>Female</i>	Focal child born	0.0840	-0.2745	-0.1957	-0.1816*	-0.2399	-0.1728
<i>Focal</i>	after school entry	(0.093)	(0.205)	(0.163)	(0.099)	(0.165)	(0.179)
<i>Child</i>	Eff. sample size	1068	300	431	668	342	262
<i>Male</i>	Focal child born	-0.0541	-0.2157**	-0.2770**	-0.3011	-0.0778	-0.1068
<i>Focal</i>	after school entry	(0.125)	(0.106)	(0.127)	(0.187)	(0.185)	(0.135)
<i>Child</i>	Eff. sample size	593	286	473	352	386	627
Panel C. Probability of Failure							
<i>Female</i>	Focal child born	-0.0418	-0.0554	-0.0870	0.0132	0.0388*	-0.1878
<i>Focal</i>	after school entry	(0.032)	(0.158)	(0.179)	(0.023)	(0.214)	(0.221)
<i>Child</i>	Eff. sample size	346	387	290	290	207	182
<i>Male</i>	Focal child born	0.0292	0.3874	0.3796**	-0.0317	-0.0635	-0.0892
<i>Focal</i>	after school entry	(0.039)	(0.240)	(0.167)	(0.030)	(0.171)	(0.173)
<i>Child</i>	Eff. sample size	293	182	361	480	323	328
	Sibling controls		✓	✓		✓	✓
	Focal child controls			✓			✓

Note: The table presents our heterogeneous effects by both focal child and sibling gender using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. In columns 1-3, we restrict our sample for only female siblings and in columns 4-6 we restrict for male siblings. First set of lines in each panel we are considering just female focal child and in the second set just male. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1 and 4 do not include any controls; columns 2, and 5 include only siblings controls, and columns 3, and 6 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and [Table A6](#) show that our results are not sensitive to bandwidth choice, as the siblings' spillover effect is negative and significant for both math and reading proficiency levels. In addition, the magnitudes of reading are always higher than math, and results are driven by less affluent families, as in our preferred specification (columns 8 and 9).

Following RD standard falsification methods, we also estimate our main results using an artificial cutoff. Since the treatment status does not change at the artificial cutoff, we expect to find non-significant estimates. In [Table A9](#), we have our main results using an alternative date as the cutoff of 75 days after the real cutoff and a fixed bandwidth of 60 days.³⁰ Results show that spillover is not significant with this placebo cutoff.

Moreover, in [Table A10](#) we show our estimates using alternative RD arguments, which include changing the kernel function, the optimum bandwidth criterion, and the smoothness class. In columns 1 and 5, we have for comparison our preferred (with all covariates) main results from [Table 4](#) for our full sample and the sample with only families who participate in Bolsa Familia Program, respectively. In columns 2-5 and 7-10, we modify one RD argument each time. Looking for columns 1-5 and 6-10 in panels A and B, we notice that the results change only marginally and are qualitatively the same. Those results indicate that our estimates are not sensitive to RD arguments choice.

Forth, in [Figure 4](#) we re-estimate our main results using donut-RD models where we remove observations very close to the focal child school-entry cutoff. In Figures a. and b., we have the estimates of spillover effects on math test score of your preferred specification (the one with all covariates). In figure a. we have the full sample and in figure b. we restrict to less affluent families. In figures c. and d., we have spillover on reading for whole sample and less affluent families only, respectively. In all these graphs, our results are stable regardless of the number of observations that are dropped when we expand the donut role. This is consistent with no manipulation at the cutoff and balance in auxiliary covariates.

³⁰This definition means that if we are in the June 30th cutoff, we consider the alternative cutoff as August 14th, and the sample is defined as 60 days before and after this date.

5.2 Younger Focal Child Sample

Table 8 shows our main results for the younger focal child sample. Columns 1-3 of the table consider our full sample of siblings. Then, we restrict the sample to families who are in Bolsa Familia Program (columns 7-9) and who are not in Bolsa Familia Program (columns 4-6). Each panel contains a different outcome: panel A has math standardized test scores and panel B has the reading standardized test scores. In panel C, our outcome is an indicator variable for the sibling who has ever failed a grade in school. In columns 1,4, and 7, we do not have control variables; in columns 2, 5, and 8, we only add siblings covariates. We also include focal child controls in the last specification (columns 3,6, and 9). We present the standard errors in parenthesis.

We observe that having a younger sibling who enrolls in school later generates a positive spillover effect in the student compared to other students whose sibling enrolled in elementary school early. This positive effect appears for proficiency levels in math and reading (panels A and B). Moreover, these estimated effects are statistically different from zero in columns 1-3 and 4-6. When we separate the effect by Bolsa Familia Program, it appears that the effect is coming from more affluent families (columns 4-6), with the spillover on the math proficiency level being more robust. Still, we have a positive but not significant effect on less affluent families.

In particular, in column 3, we find that having an old-for-grade younger sibling generates a positive effect of approximately 0.19 standard deviation on sibling math proficiency level and 0.16 standard deviation on sibling reading test scores. For more affluent families, this effect is approximately 0.17 and 0.13 standard deviation in math and reading (column 6), respectively. We also notice that the effect on math test scores is larger than reading in all samples. Finally, we do not have a statically significant spillover effects in grade failure (panel C).

The literature recognizes that spillover effects from a younger to an older sibling usually occurs through an indirect effect from their parents. This means that the cutoff rule generates

Table 8: Main results of sibling spillovers in the younger focal child sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born after school entry	0.1728*** (0.058)	0.1850*** (0.057)	0.1864*** (0.076)	0.1988*** (0.066)	0.1931*** (0.062)	0.1729*** (0.072)	0.1042 (0.089)	0.1771 (0.115)	0.2407 (0.168)
Eff. sample size	2890	2623	2349	2279	2101	2377	900	745	487
	Panel B. Reading Score								
Focal child born after school entry	0.2160*** (0.050)	0.1398*** (0.056)	0.1574** (0.074)	0.2575*** (0.057)	0.200*** (0.060)	0.1311* (0.072)	0.0932 (0.081)	0.0792 (0.106)	0.2221 (0.180)
Eff. sample size	3638	3003	2419	2890	2457	2370	1093	844	496
	Panel C. Probability of Failure								
Focal child born after school entry	-0.0035 (0.015)	0.0054 (0.044)	-0.0176 (0.050)	-0.0033 (0.014)	0.0289 (0.046)	-0.0005 (0.049)	-0.0101 (0.034)	-0.0763 (0.071)	-0.0863 (0.097)
Eff. sample size	2521	3791	3821	2545	3821	3905	594	903	866
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

a different behavior in the parents of our treated group, in terms of resource allocation, compared to the parents of the control group. Since we have a positive spillover effect, what may be happening is that when the younger sibling from our treated family delays his school entry, the treated parents have more resources to allocate to the older sibling who is already in school. Thus, this rule somehow benefits the treated older sibling, reflecting in higher math and reading scores than the control older sibling.

This parental behavior that intermediates the indirect channel can be compensatory (attempting to balance resources among children) or reinforcement (investing in the one that brings the greatest return). As we found at the beginning of this section that the effect of starting school later generated by the rule on the focal child is a negative shock, the idea of a

reinforcement behavior is more closely associated with our results since the cutoff rule seems to be positively affecting the sibling while having a negative effect on their focal child.

In addition, we have spillover effects driven by more affluent families. [Grätz & Torche \(2016\)](#) recognizes that resource redistribution within family varies by socioeconomic status: upper-class families parents can have a compensatory behavior because they are able to afford it. However, [Becker & Tomes \(1976\)](#) finds that this compensatory behavior is usually associated with parents investing more in human capital for their better-endowed children and investing in non human capital for the others siblings. Thus, advantaged families, which are those with well-educated parents, may provide more cognitive stimuli to higher-ability children ([Grätz & Torche 2016](#)). And under this assumption, those parents may observe the relatively higher-performing older sibling and allocate more resources toward (reinforce) this child because a higher return on the investment is more likely ([Becker & Tomes 1976](#)).

Still, this idea of parents having a reinforcement behavior because the older sibling is higher-ability is just one of the potential channels of the indirect effect. We also can have the possibility of a reinforcing behavior that is associated with gender-specific investments and parents' gender preferences ([Karbownik & Myck 2017](#); [Barcellos et al. 2014](#)). A third possibility would be a reinforcement behavior by the simple fact that the oldest sibling is the firstborn ([Keller & Zach, 2002](#)). This hypothesis is also associated with firstborns outperforming their younger siblings in cognitive exams, wages, educational attainment, and employment ([Pavan, 2016](#)). Thus, these three channels (higher performance, gender preferences, or firstborn preference) are potential mechanisms through which parents may be exercising reinforcement behavior.

[Figure 5](#) also illustrates these results. The figure shows the daily means and linear fits for the bandwidth of 60 days around the focal child school entry cutoff for our full sample (Graphs a. and b.) and just for families who are (Graphs b. and d.) and who are not in Bolsa Familia Program (Graphs e. and f.). On the vertical axis, we use each outcome cleaned from the covariate effect, i.e., the residual outcome as proposed by [Noack et al. \(2021\)](#). In graphs

a., c., and e., we have the discontinuity in math test scores, and in graphs b., d., and f., the discontinuity in reading test scores. The graphs confirm a discontinuity in older siblings' tests scores, and when we restrict to more affluent families, this difference seems larger. Thus, our conclusions of spillovers driven by more affluent families remain unchanged.

In the next subsection, we will try to understand better the potential mechanism of this parents' reinforcing behavior by analyzing heterogeneous effects.

5.2.1 Heterogeneous Effects

Similar to our previous analysis, we divide our sample by siblings' grade on [Table 9](#). These heterogeneous effects can clarify how the spillover effects differ throughout the siblings' school path. For math proficiency level (panel A), we notice a positive and significant effect in our preferred specification for the 3rd, 5th, and 7th grades. The spillover effect on reading test scores is very similar (panel B), but the effect is significant for 5th, 7th, and 9th-grade siblings. Thus, it seems that we have medium-term spillover effects that are present throughout the elementary school path.

Still, the effect appears to be more robust in the 5th grade for both math and reading tests scores, which is an important grade for being the last of elementary schools' first cycle and be the first test after the sibling skill was already revealed (with the 3rd-grade test score). These results are consistent with those found in [Landersø et al. \(2020\)](#). The authors explain that entering later in elementary school is an easier school start for the treated focal child. This event probably improves their older siblings' performance through a higher quality study environment at home with parental resources free to help with homework. Moreover, they argue that those effects are concentrated when these resources can make the most difference, and the improvement is relatively straightforward. This could be the case for our siblings in 5th grade.

We can understand other mechanisms when looking at these siblings' grade heterogeneous results and the heterogeneous effect by the age gap. [Table 10](#) presents the results divided by

Table 9: Heterogeneous effects divided by sibling grade - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	3rd grade			5th grade			7th grade			9th grade		
Panel A. Math Score												
Focal child born	0.3909	0.3686**	0.5874***	0.0864*	0.2642**	0.4083***	-0.0500	0.2627	0.5775***	0.0559	-0.1779	-0.1792
after school entry	(0.352)	(0.178)	(0.149)	(0.051)	(0.128)	(0.107)	(0.103)	(0.256)	(0.177)	(0.054)	(0.118)	(0.114)
Eff. sample size	432	144	848	1556	868	1205	423	219	368	1618	743	957
Panel B. Reading Score												
Focal child born	-0.0306	0.1726	0.2967	0.5357*	0.2285*	0.4358***	-0.1149	0.2586	0.5423***	0.0726	0.1726	0.2193**
after school entry	(0.050)	(0.181)	(0.288)	(0.322)	(0.124)	(0.100)	(0.134)	(0.256)	(0.178)	(0.057)	(0.115)	(0.101)
Eff. sample size	436	151	408	1661	876	1326	323	218	368	1611	775	1114
Panel C. Probability of Failure												
Focal child born	-0.0177	0.3246	0.3884	0.0049	-0.1322	0.2985**	-0.0271	-0.0416	0.2046	-0.0031	-0.0058	0.2375**
after school entry	(0.069)	(0.361)	(0.262)	(0.008)	(0.137)	(0.141)	(0.033)	(0.349)	(0.190)	(0.033)	(0.120)	(0.107)
Eff. sample size	351	255	387	1102	641	703	302	129	286	862	746	942
Sibling controls		✓	✓		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓		✓	

Note: The table presents our heterogeneous effects by sibling grade using RD Honest method for the sample in which the focal child (i.e., the student exposed to the cutoff rule) is younger than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings who are in third and in columns 7-9 we restrict for siblings in fifth grade. As the sample was too small for siblings in the seventh grade, we didn't include it in our analysis. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and on panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

siblings' age gap. We consider a small gap between siblings whose age difference with the focal child is less than four years (columns 4-6) and a big age gap with an age difference of more than four years (columns 7-9). We notice that siblings' small age gap seems to drive our math and reading proficiency levels results. Therefore, parents' reinforcing behavior benefits the older sibling even after the focal child is already enrolled at elementary school. Furthermore, these results strengthen our hypothesis that parents allocate their resources in a way that reinforces the older sibling since it is easy for them to compare their children since the siblings are close in age (Karbownik & Özek 2021).

Our gender heterogeneous effects also confirm that the spillover effects come from families with similar sibling-focal child pairs. We look by sibling and focal child gender heterogeneous

Table 10: Heterogeneous effects divided by age gap - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Small Age Gap			Big Age Gap		
	Panel A. Math Score								
Focal child born	0.1728***	0.1850***	0.1864***	0.3105***	0.2567***	0.2911***	0.0221	0.2123*	0.2480***
after school entry	(0.058)	(0.057)	(0.076)	(0.086)	(0.096)	(0.071)	(0.071)	(0.117)	(0.092)
Eff. sample size	2890	2623	2349	1663	1385	2333	1167	871	1177
	Panel B. Reading Score								
Focal child born	0.2160***	0.1398***	0.1574**	0.3427***	0.2703***	0.3144***	0.0513	0.1803*	0.0769
after school entry	(0.050)	(0.056)	(0.074)	(0.085)	(0.100)	(0.073)	(0.058)	(0.1073)	0.0820
Eff. sample size	3638	3003	2419	1744	1334	2171	1697	960	1363
	Panel C. Probability of Failure								
Focal child born	-0.0035	0.0054	-0.0176	-0.0074	0.1613*	0.2771***	-0.0170	0.1194	-0.1495
after school entry	(0.015)	(0.044)	(0.050)	(0.011)	(0.097)	(0.088)	(0.026)	(0.101)	(0.100)
Eff. sample size	2521	3791	3821	2960	1170	1482	935	933	964
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by age gap using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings whose age gap with the focal child is small and in columns 7-9 we restrict for siblings whose age gap is big. We define a small age gap as an age difference between sibling and focal child being less than four years. A large age gap is an age gap of four years or more. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include gender and cohort of entry. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

effects at [Table 11](#) where we estimate spillover effects separately by each sibling-focal child gender pair.³¹ First, panel A presents that the math test scores are positive and significant when we have both siblings girls (first set of lines columns 2-3) or when we have both siblings boys (second set of lines columns 5-6). This same pattern occurs with the reading test scores in panel B: significant effects for sisters and brothers. Thus, our younger to older spillover effects are probably not coming from parents' gender preferences due to significant results in families with same-gender siblings.

Thus, a focal child entering school a year later positively affects older's sibling reading and math proficiency levels. More affluent parents may explain this spillover by reinforcing their resources to the older sibling. This behavior could be because the eldest is high-ability, firstborn, or even other possibilities that we don't present here. Heterogeneous effects show that this effect is given throughout the elementary school path between similar siblings (close in age and same gender).

5.2.2 Robustness

Similar to our older to younger spillover effects analysis, in this subsection, we present the robustness checks to our main younger to older spillover results from [Table 8](#). To start, we have our main estimates using a fixed bandwidths of 30, 45, and 60 days ([Table A13](#), [Table A14](#) and [Table A15](#), respectively). As expected, the estimates do not vary much with those modifications in the cutoff neighborhood, and our results are qualitatively the same. In other words, our spillover effects of math and reading proficiency levels on the older siblings are positive and significant (columns 2-3 and 5-6 in both panels) and come from more affluent families (not in Bolsa Familia Program).

Moreover, when we estimate our main results using an artificial cutoff, we find no significant effect ([Table A16](#)). This result is expected since we consider a cutoff of 75 days after

³¹In [Table A11](#) and [Table A12](#) we estimate gender heterogeneous effects separately by siblings' and focal childrens' gender. Results seem to indicate that there is no gender difference in younger to older spillover effects. However, as we present in [Table 11](#) we have spillovers driven by same-sex siblings.

Table 11: Heterogeneous effects divided by sibling and focal child gender - Younger FC sample

		(1)	(2)	(3)	(4)	(5)	(6)
		Female Sibling			Male Sibling		
Panel A. Math Score							
<i>Female</i>	Focal child born	0.0795	0.2368*	0.2555**	0.2131*	0.0925	0.0800
<i>Focal</i>	after school entry	(0.089)	(0.132)	(0.121)	(0.110)	(0.161)	(0.102)
<i>Child</i>	Eff. sample size	1003	643	761	715	603	977
<i>Male</i>	Focal child born	0.1680	0.0939	0.3165*	0.2907***	0.4314***	0.2633***
<i>Focal</i>	after school entry	(0.124)	(0.086)	(0.174)	(0.101)	(0.131)	(0.089)
<i>Child</i>	Eff. sample size	745	1308	531	856	616	1157
Panel B. Reading Score							
<i>Female</i>	Focal child born	0.1950*	0.2596**	0.2578***	0.0333	0.0536	0.0807
<i>Focal</i>	after school entry	(0.102)	(0.127)	(0.111)	(0.117)	(0.168)	(0.102)
<i>Child</i>	Eff. sample size	907	617	761	716	581	979
<i>Male</i>	Focal child born	0.1146	0.0954	0.3168***	0.2452***	0.4568***	0.2652***
<i>Focal</i>	after school entry	(0.104)	(0.103)	(0.174)	(0.085)	(0.146)	(0.089)
<i>Child</i>	Eff. sample size	912	1015	531	1069	533	1156
Panel C. Probability of Failure							
<i>Female</i>	Focal child born	-0.0130	0.0019	-0.2448*	-0.0263	0.1560	0.0255
<i>Focal</i>	after school entry	(0.020)	(0.150)	(0.145)	(0.014)	(0.155)	(0.098)
<i>Child</i>	Eff. sample size	914	467	545	1317	584	954
<i>Male</i>	Focal child born	0.0294	-0.0103	-0.2428	-0.0037	-0.0291	-0.1849
<i>Focal</i>	after school entry	(0.023)	(0.201)	(0.166)	(0.039)	(0.182)	(0.108)
<i>Child</i>	Eff. sample size	1118	392	454	56	347	811
	Sibling controls		✓	✓		✓	✓
	Focal child controls			✓			✓

Note: The table presents our heterogeneous effects by both focal child and sibling gender using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. In columns 1-3, we restrict our sample for only female siblings and in columns 4-6 we restrict for male siblings. First set of lines in each panel we are considering just female focal child and in the second set just male. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and on panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1 and 4 do not include any controls; columns 2, and 5 include only siblings controls, and columns 3, and 6 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

the real cutoff rule, and with this fake cutoff, the treatment status does not change.

In this sample, we are investigating spillover on older siblings, and then we have cases in which siblings had taken the test before the focal child was exposed to the cutoff rule. Thus, we also estimate as a robust test the effect of the focal child being old-for-grade on siblings' math and reading proficiency level at baseline. Those results are on [Table A17](#) and, as expected, our significant spillover effect disappears, indicating that the results found previously are generated from the exposure of the focal child to a cutoff rule.

In addition, in [Table A18](#) we show our estimates using alternative RD arguments, which include varying the kernel function, the optimum bandwidth criterion, and the smoothness class. In columns 1 and 5, we have for comparison our preferred (with all covariates) main results from [Table 8](#) for our full sample and the sample with only families who do not participate in Bolsa Familia Program, respectively. In columns 2-5 and 7-10, we modify one RD argument each time. Looking for columns 1-5 and 6-10 in panels A and B, we notice that the results change only marginally and are qualitatively the same. Those results indicate that our estimates are not sensitive to RD arguments choice.

To conclude, in [Figure 6](#) we re-estimate our main results using donut-RD models where we remove observations very close to the focal child school-entry cutoff. In figures a. and b., we have the estimates of spillover effects on the math test score of your preferred specification (the one with all covariates). In figure a. we have the full sample, and in figure b., we restrict to more affluent families (those who do not participate in Bolsa Familia Program). In figures c. and d., we have spillover on reading for the whole sample and more affluent families only, respectively. In all these graphs, our results are stable regardless of the number of observations that are dropped when we expand the donut role. We notice a small reduction in the magnitude of the spillover effects as we increase the number of days we exclude around the cutoff. Still, the results remain qualitatively the same; that is, they are all positive and significant regardless of the number of observations dropped around the cutoff. These results are consistent with no manipulation at the cutoff and balance in auxiliary covariates.

6 Conclusion

Siblings share a bond that makes their relationship unique, and thus, they should influence each others' behavior and decisions. Still, there is little evidence if a school rule or policy that affects one child generate externalities on the other. This paper analyzes siblings' spillover causal effects generated by the school starting age rule in elementary school. We find statistically significant spillovers in math and reading test scores using Brazilian data and a regression discontinuity design.

In particular, when we look for spillovers from an older focal child to her younger sibling, we show that starting school a year later has a negative effect on siblings' proficiency level compared to siblings of focal children who enter school earlier. Disadvantaged families drive this effect, and its magnitude is 0.36 and 0.56 standard deviation in math and reading tests scores, respectively. Moreover, heterogeneous effects estimates show that these spillover effects occur at the beginning of siblings' schooling path, mostly from older brothers to younger sisters that are close in age. Thus, we have suggestive evidence of a direct channel of spillovers between siblings that may be happening due to a bad role modeling or a bigger competition for non-monetary resources between our treated compared to our control families. In this scenario, sibling spillovers may be an important channel through which inequality within households is produced.

On the other hand, in our younger-to-older spillover effects, we find that the younger focal child entering school later positively affects her older sibling's proficiency level. The effect is positive for our full sample but only statistically significant for more affluent families (0.17 and 0.13 standard deviation in math and reading, respectively). In addition, heterogeneous effects results indicate that these spillovers may be influencing the siblings throughout the entire elementary school path. These estimates are driven by siblings similar to their focal children (close in age and same gender). These results channel is probably indirect and suggests that parents have a reinforcement behavior focused on the older sibling. Thus, this evidence clarifies how more affluent families are allocating resources among children. We do

not find any spillover effects on the probability of grade failure in both samples.

Still, our results have several limitations. First, we cannot find the exact mechanism that drives our spillovers due to a lack of data. To better understand the channels through which externalities are acting, we need more information about household dynamics and parents' choices. Second, the cutoff rule may affect the siblings indirectly through parental decisions that are necessarily related to the focal children (for instance, through mother labor market outcomes). If that is the case, we will be estimating family spillovers and not siblings spillovers as mentioned by [Landersø et al. \(2020\)](#). These issues open space for future research on family externalities in the Brazilian context.

The literature recognizes that spillover effects may vary according to the institutional background, the exogenous variation considered, and birth order between siblings. Thus, our study contributes to the growing literature of siblings spillovers in the educational context, bringing evidence on a developing country scenario. With our data, we investigated both older-to-younger and younger-to-older siblings' externalities showing that the effect and mechanisms of each case can be quite different and that the household socioeconomic status are key in this analysis.

To conclude, the existence of siblings spillovers effects from an educational rule highlights the importance of considering these externalities in any cost-effectiveness analyses since ignoring it could underestimate the costs and benefits associated with a particular policy. In addition, it is also very interesting to think about the family background in this scenario since sibling spillovers can be a channel to increase or decrease inequality, and it may also help us understand how parents allocate resources between their children. Indeed, as discussed by [Zang et al. \(2022\)](#), the process behind sibling influences could be a crucial understudied pathway for inter-and intra-generational transmission of (dis)advantages.

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7 Appendix

Table A1: Descriptive statistics before and after sample restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Older Focal Child			Younger Focal Child		
	All Sample	Final Sample	Difference (1)-(2)	All Sample	Final Sample	Difference (4)-(5)
White	0.62 (0.48)	0.62 (0.49)	0.00	0.61 (0.49)	0.60 (0.49)	0.00
Bolsa Familia Program	0.24 (0.43)	0.24 (0.43)	0.00	0.25 (0.43)	0.24 (0.43)	0.01**
Female	0.49 (0.50)	0.50 (0.50)	-0.01*	0.49 (0.50)	0.49 (0.50)	0.00
Birth year	2008.12 (2.81)	2007.51 (1.74)	0.61***	2003.87 (3.71)	2003.24 (1.80)	0.63***
Disable	0.01 (0.12)	0.01 (0.11)	0.00***	0.02 (0.13)	0.02 (0.13)	0.00***
Reading test score	158.38 (82.41)	158.89 (82.52)	-0.57	166.78 (100.49)	206.75 (79.35)	-40.03***
Math test score	179.03 (92.23)	179.89 (92.19)	-0.93	178.20 (105.11)	218.28 (80.25)	-40.16***
Drop out	0.00 (0.03)	0.00 (0.03)	0.00	0.00 (0.05)	0.00 (0.04)	0.00
School failure	0.03 (0.21)	0.03 (0.17)	-0.01***	0.06 (0.24)	0.05 (0.21)	0.01***
N	282,955	44,962		1,036,743	93,897	

Note: Column 1 includes all siblings enrolled in the state public school with an older focal child (i.e., the student exposed to the rule) from the 2010-2018 cohort. Column 4 includes the siblings of a younger focal child from those same cohorts. Columns 2 and 5 restrict the sample from columns 1 and 4, respectively, to siblings with at least one test score observation, who is not the twin of their focal child, whose age difference with the focal child does not exceed eight years and who took the test after their focal child was exposed to the rule. Columns 3 and 6 show the difference between columns 1-2 and 4-5, respectively. Standard deviation in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: First stage results for both samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not in BF Program			In BF Program		
Panel A. Older to Younger									
Focal child born	0.5437***	0.5391***	0.4987***	0.5176***	0.5163***	0.1635***	0.6678***	0.6349***	0.1473***
after school entry	(0.024)	(0.026)	(0.015)	(0.027)	(0.029)	(0.017)	(0.041)	(0.041)	(0.019)
Observations	1089	881	569	887	708	430	318	269	195
Panel B. Younger to Older									
Focal child born	0.5320***	0.5363***	0.5132***	0.5368***	0.5471***	0.5168***	0.5869***	0.5643***	0.5466***
after school entry	(0.012)	(0.013)	(0.013)	(0.014)	(0.015)	(0.014)	(0.023)	(0.025)	(0.024)
Observations	3065	2557	2589	2490	2117	2121	891	710	743
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

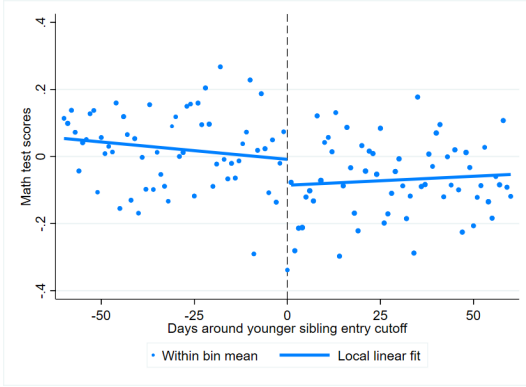
Note: Table shows first stage results using RD Honest method for both samples: older focal child in panel A and younger focal child in panel B. Recall that the older focal child sample is the one we have an older sibling who was exposed to the cutoff rule, and we analyze the spillover effect in the younger sibling. The younger focal child sample is when we look over the spillover effect from a younger child exposed to the rule to their older sibling. The outcome is the indicator for being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In columns 1-3, we have the full sample; in columns 4-6 and 7-9 we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Siblings controls include gender, race, grade, year and month of birth. Focal child controls include cohort of entry, gender and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A3: Main results in the older focal child sample for siblings of closest age

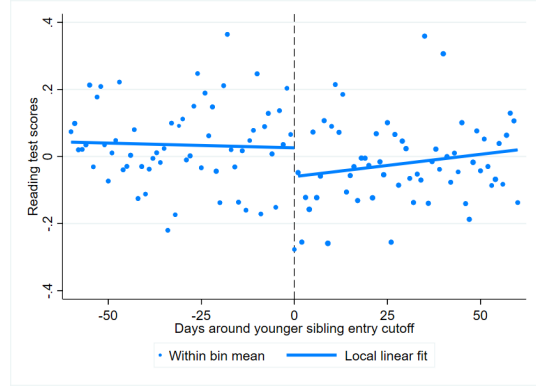
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	-0.0790	-0.2145***	-0.2144***	-0.0399	-0.1388	-0.1138	-0.2130**	-0.2178*	-0.2484**
after school entry	(0.064)	(0.087)	(0.084)	(0.080)	(0.101)	(0.107)	(0.105)	(0.117)	(0.114)
Eff. sample size	2351	1517	1557	1616	1192	1056	828	487	389
	Panel B. Reading Score								
Focal child born	-0.0134	-0.1557**	-0.3114***	0.0149	0.0019	-0.1316	-0.1794*	-0.2452**	-0.5035***
after school entry	(0.069)	(0.076)	(0.079)	(0.085)	(0.092)	(0.103)	(0.107)	(0.110)	(0.139)
Eff. sample size	1832	2031	1854	1332	1428	1162	678	365	330
	Panel C. Probability of Failure								
Focal child born	-0.0182	-0.0937	0.0667	-0.0390	-0.0845	0.0450	0.0150	-0.0264	0.1511
after school entry	(0.015)	(0.062)	(0.063)	(0.022)	(0.082)	(0.083)	(0.028)	(0.122)	(0.131)
Eff. sample size	1481	2452	2450	858	1423	1425	383	625	625
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results restricted to siblings whose age difference is a maximum of 6 years instead of 8 like [Table 4](#). Results are obtained using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

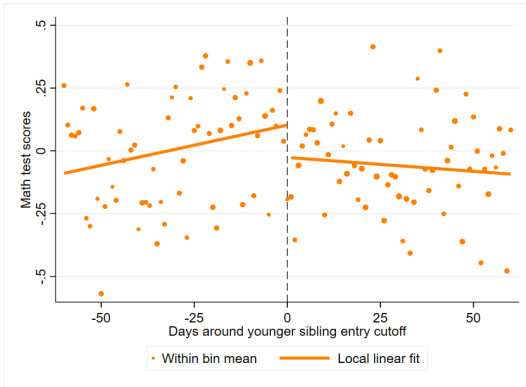
Figure 3: Discontinuity in test scores - Older focal child sample



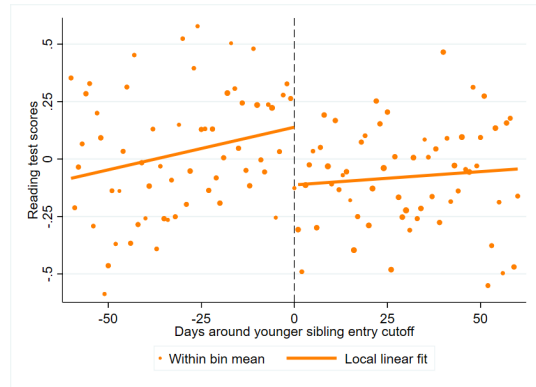
(a) Math test scores in complete sample



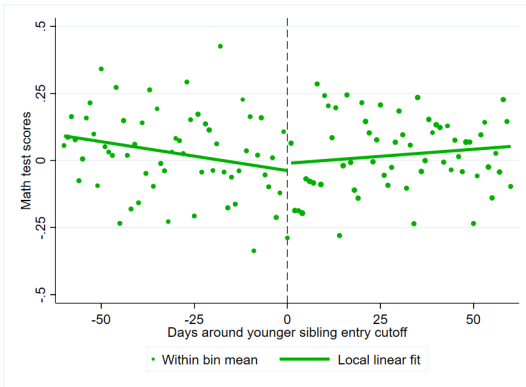
(b) Reading test scores in complete sample



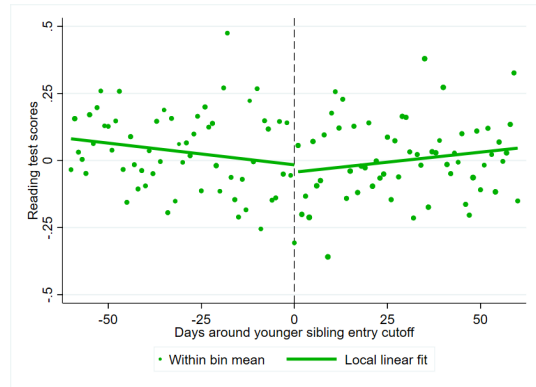
(c) Math test scores in BF Program sample



(d) Reading test scores in BF Program sample



(e) Math test scores not in BF Program sample



(f) Reading test scores not in BF Program sample

Table A4: Heterogeneous effects divided by sibling gender - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Female			Male		
	Panel A. Math Score								
Focal child born	-0.0539	-0.2435***	-0.1879**	-0.0814	-0.2515**	-0.2901**	0.0366	0.0370	-0.0495
after school entry	(0.055)	(0.056)	(0.085)	(0.072)	(0.123)	(0.141)	(0.104)	(0.137)	(0.100)
Eff. sample size	3275	3696	1595	1715	682	552	1016	691	1205
	Panel B. Reading Score								
Focal child born	-0.0194	-0.3139***	-0.3279***	-0.0648	-0.2389**	-0.2691**	0.1053	0.0277	-0.1106
after school entry	(0.056)	(0.070)	(0.081)	(0.070)	(0.118)	(0.130)	(0.104)	(0.129)	(0.107)
Eff. sample size	2851	2521	1821	1669	744	581	865	778	1040
	Panel C. Probability of Failure								
Focal child born	-0.0039	-0.0621	0.0373	-0.0135	-0.0982	0.1696	-0.0094	-0.0906	0.2833**
after school entry	(0.013)	(0.055)	(0.056)	(0.020)	(0.133)	(0.156)	(0.028)	(0.180)	(0.137)
Eff. sample size	1891	3101	3103	611	534	419	615	380	548
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by sibling grade using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample to only siblings who are girls, and in columns 7-9 we restrict for siblings boys. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A5: Heterogeneous effects divided by focal child gender - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Female			Male		
	Panel A. Math Score								
Focal child born	-0.0539	-0.0952*	-0.1462*	-0.0680	-0.0592	-0.1078	-0.1560*	-0.3350***	-0.2814**
after school entry	(0.055)	(0.056)	(0.085)	(0.126)	(0.187)	(0.110)	(0.091)	(0.132)	(0.124)
Eff. sample size	3275	3696	1595	748	490	984	1110	633	665
	Panel B. Reading Score								
Focal child born	-0.0194	-0.2151**	-0.2780***	-0.1190	-0.4986***	-0.2849**	-0.1247*	-0.1926*	-0.2876***
after school entry	(0.056)	(0.070)	(0.081)	(0.108)	(0.191)	(0.133)	(0.075)	(0.115)	(0.108)
Eff. sample size	2851	2521	1821	862	473	726	1552	731	929
	Panel C. Probability of Failure								
Focal child born	-0.0039	-0.0621	0.0373	-0.0339	-0.2765*	0.1551	0.0226	-0.1092	0.1571
after school entry	(0.013)	(0.055)	(0.056)	(0.023)	(0.159)	(0.114)	(0.021)	(0.140)	(0.116)
Eff. sample size	1891	3101	3103	771	454	800	667	510	721
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by focal child gender using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings whose focal child is female, and in columns 7-9 we restrict for siblings whose focal child is male. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Main results using fixed bandwidth of 30 days - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	-0.0875	-0.2994***	-0.1752**	-0.0668	-0.1441	-0.1496	-0.1753	-0.3277**	-0.2870**
after school entry	(0.080)	(0.100)	(0.097)	(0.088)	(0.120)	(0.119)	(0.120)	(0.150)	(0.145)
Eff. sample size	1522	1203	1203	1163	909	909	359	293	293
	Panel B. Reading Score								
Focal child born	-0.0231	-0.2801***	-0.2883***	0.0057	0.1266	-0.1589	-0.1647	-0.4234**	-0.4583***
after school entry	(0.076)	(0.100)	(0.098)	(0.093)	(0.121)	(0.120)	(0.112)	(0.183)	(0.169)
Eff. sample size	1522	1203	1203	1163	909	909	359	293	293
	Panel C. Probability of Failure								
Focal child born	-0.0165	-0.1344	0.0855	-0.0272	-0.1102	0.0472	0.0138	-0.2121	0.2460
after school entry	(0.017)	(0.103)	(0.103)	(0.020)	(0.123)	(0.124)	(0.030)	(0.185)	(0.190)
Eff. sample size	1246	968	968	948	726	726	298	241	241
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Main results using fixed bandwidth of 45 days - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	-0.0719	-0.2727***	-0.1624**	-0.0393	-0.1309	-0.1091	-0.1978*	-0.3127**	-0.2773**
after school entry	(0.065)	(0.080)	(0.079)	(0.078)	(0.095)	(0.094)	(0.118)	(0.146)	(0.141)
Eff. sample size	2247	1774	1774	1717	1344	1344	530	431	431
	Panel B. Reading Score								
Focal child born	-0.0086	-0.2576***	-0.2591***	0.0208	0.1883*	-0.1760	-0.1286	-0.3741***	-0.4528***
after school entry	(0.061)	(0.081)	(0.079)	(0.073)	(0.105)	(0.115)	(0.102)	(0.155)	(0.145)
Eff. sample size	2247	1774	1774	1717	1344	1344	530	431	431
	Panel C. Probability of Failure								
Focal child born	-0.0107	-0.1558*	0.1014	-0.0176	-0.1410	0.0666	0.0113	-0.2135	0.2526
after school entry	(0.014)	(0.083)	(0.083)	(0.016)	(0.097)	(0.098)	(0.027)	(0.156)	(0.160)
Eff. sample size	1848	1437	1437	1406	1082	1082	441	355	355
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C, it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Main results using fixed bandwidth of 60 days - Older FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	-0.0344	-0.1180*	-0.1430**	0.0103	-0.0732	-0.0035	-0.1838*	-0.3138***	-0.4182***
after school entry	(0.058)	(0.071)	(0.070)	(0.068)	(0.082)	(0.081)	(0.112)	(0.139)	(0.141)
Eff. sample size	2991	2360	2360	2269	1775	1775	722	585	585
	Panel B. Reading Score								
Focal child born	0.0185	-0.2311***	-0.2677***	0.0601	-0.1810**	-0.1773**	-0.1857*	-0.4415***	-0.5380***
after school entry	(0.055)	(0.071)	(0.071)	(0.064)	(0.082)	(0.081)	(0.106)	(0.143)	(0.145)
Eff. sample size	2991	2360	2360	2269	1775	1775	722	585	585
	Panel C. Probability of Failure								
Focal child born	-0.0086	0.1195	0.1181	-0.0168	0.0942	0.1215	0.0197	0.2313	0.2301
after school entry	(0.011)	(0.073)	(0.073)	(0.013)	(0.083)	(0.083)	(0.026)	(0.150)	(0.149)
Eff. sample size	2462	1914	1914	1861	1431	1431	600	482	482
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Placebo cutoff effects in our older focal child sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
Panel A. Math Score									
Focal child born	-0.0309	-0.0511	-0.0568	0.0255	0.0100	0.0678	-0.2044*	-0.2088	-0.2188
after school entry	(0.057)	(0.070)	(0.069)	(0.067)	(0.083)	(0.081)	(0.106)	(0.132)	(0.153)
Eff. sample size	3012	2377	2377	2286	1783	1783	726	594	594
Panel B. Reading Score									
Focal child born	0.0184	-0.1033	-0.1300*	0.0691	-0.1086	-0.1045	-0.1420	-0.1809	-0.1459
after school entry	(0.04)	(0.070)	(0.070)	(0.063)	(0.082)	(0.081)	(0.101)	(0.135)	(0.135)
Eff. sample size	3012	2377	2377	2286	1783	1783	726	594	594
Panel C. Probability of Failure									
Focal child born	-0.0056	0.1291*	0.1025	-0.0114	0.0988	0.0638	0.0146	0.1665	0.1575
after school entry	(0.012)	(0.073)	(0.072)	(0.013)	(0.084)	(0.083)	(0.025)	(0.132)	(0.139)
Eff. sample size	2474	1922	1922	1873	1436	1436	601	486	486
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

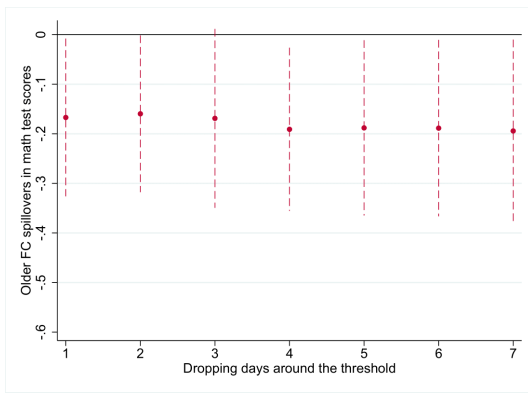
Note: The table presents our placebo cutoff results using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. The placebo cutoff considered is after 75 days after the last day of our fixed bandwidth. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, is the standardized reading test scores, and in panel C, is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Spillover effects with different RD arguments in our older focal child sample

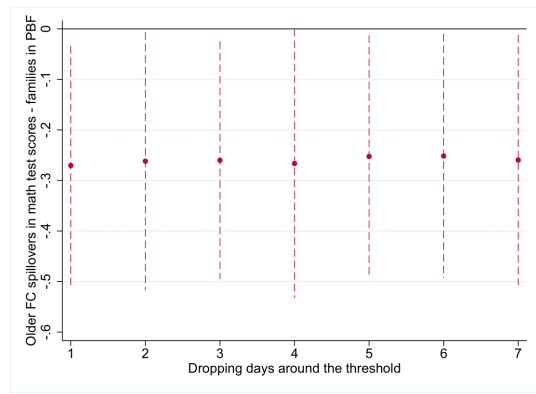
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All					BF Program				
	Panel A. Math Score									
Focal child born	-0.1462*	-0.1711**	-0.1672*	-0.1540*	-0.1505*	-0.3584**	-0.2910**	-0.3005**	-0.3138**	-0.3383**
after school entry	(0.085)	(0.086)	(0.087)	(0.086)	(0.082)	(0.175)	(0.147)	(0.150)	(0.149)	(0.164)
Eff. sample size	1595	1356	1410	1455	1109	311	394	418	431	326
	Panel B. Reading Score									
Focal child born	-0.2780***	-0.2844***	-0.2605***	-0.2516***	-0.2775***	-0.5577***	-0.4483***	-0.4905***	-0.5141***	-0.5895***
after school entry	(0.081)	(0.073)	(0.071)	(0.070)	(0.079)	(0.165)	(0.163)	(0.164)	(0.165)	(0.191)
Eff. sample size	1821	2116	2206	2283	1731	307	323	339	349	264
Sibling controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Focal child controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table presents our main results with different RD arguments (kernel, optimum bandwidth criterion and smoothness class) for the sample where the focal child (i.e., student exposed to the cutoff rule) is older than the sibling. In columns 1 and 6, we repeat our main results for the full sample and only families in the Bolsa Familia Program sample, respectively. We use the triangular kernel, MSE optimum bandwidth criterion, and Hölder smoothness class (default arguments) in those columns. In columns 2-3 and 7-8, we changed just the kernel to uniform and epanechnikov, respectively. In columns 4 and 9, we use the default arguments changing the optimum bandwidth criterion to FLCI. In columns 5 and 10, we use the default arguments changing the smoothness class to Taylor. In columns 1-5, we have our full sample, and in columns 6-10, we restrict to siblings whose families participate in Bolsa Familia Program. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores and, in panel B, it is the standardized reading test scores. In all columns, we use our preferred specification with all covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

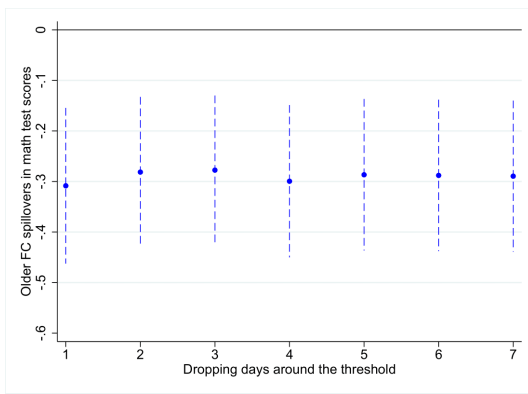
Figure 4: Donut regression - Older focal child sample



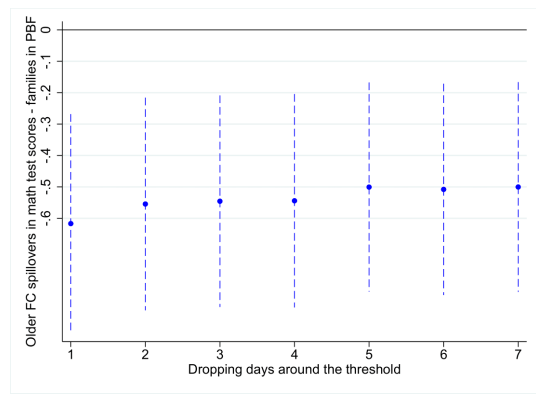
(a) Math spillovers in complete sample



(b) Math spillovers in BF Program sample

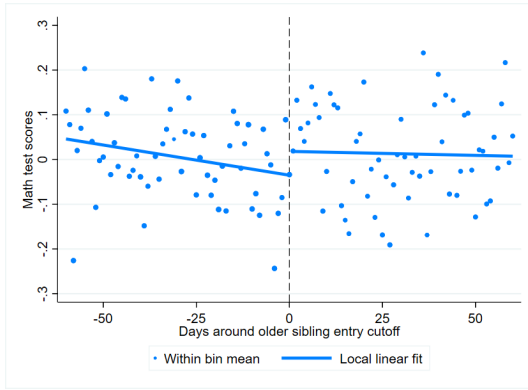


(c) Reading spillovers in complete sample

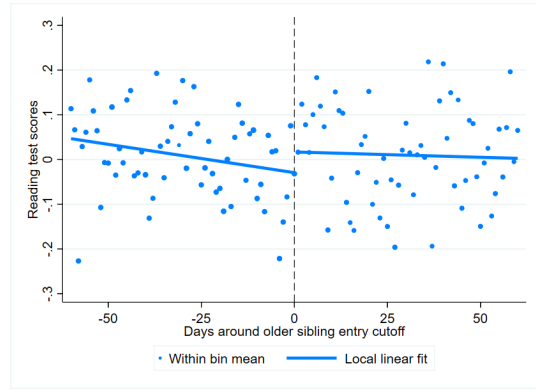


(d) Reading spillovers in BF Program sample

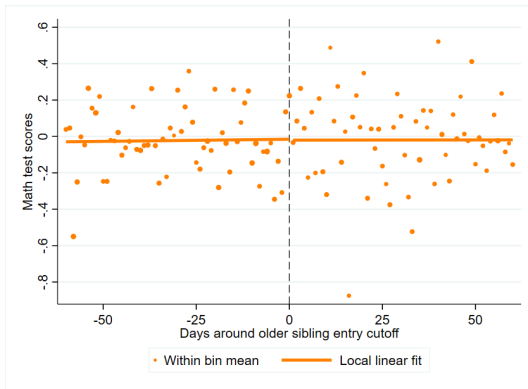
Figure 5: Discontinuity in test scores - Younger focal child sample



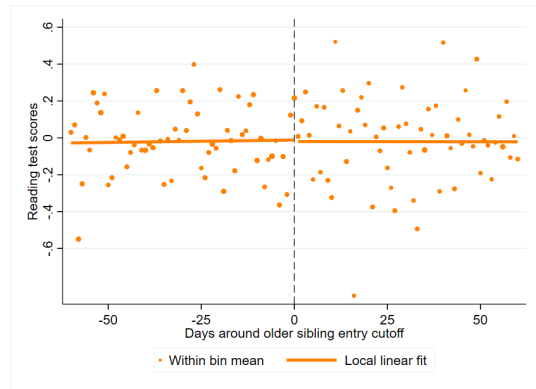
(a) Math test scores in complete sample



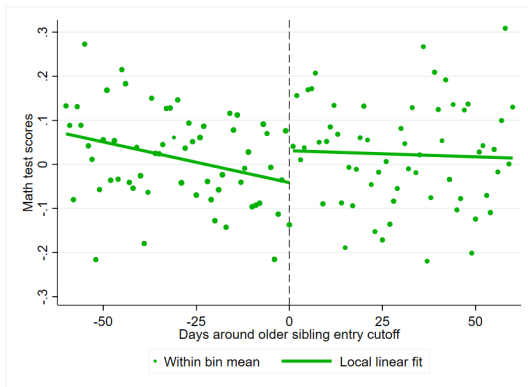
(b) Reading test scores in complete sample



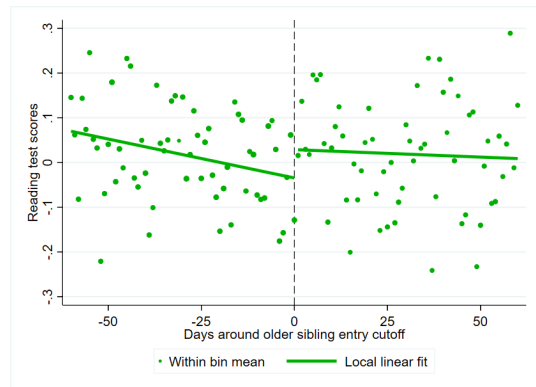
(c) Math test scores in BF Program sample



(d) Reading test scores in BF Program sample



(e) Math test scores not in BF Program sample



(f) Reading test scores not in BF Program sample

Table A11: Heterogeneous effects divided by sibling gender - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Female			Male		
	Panel A. Math Score								
Focal child born after school entry	0.1728*** (0.058)	0.1850*** (0.057)	0.1864*** (0.076)	0.1204** (0.061)	0.1562* (0.091)	0.1646* (0.093)	0.2055** (0.090)	0.3616*** (0.119)	0.2206** (0.105)
Eff. sample size	2890	2623	2349	1986	1226	1385	1421	1077	1256
	Panel B. Reading Score								
Focal child born after school entry	0.2160*** (0.050)	0.1398*** (0.056)	0.1574** (0.074)	0.2308*** (0.063)	0.1568* (0.088)	0.1655* (0.093)	0.1762** (0.078)	0.1825* (0.110)	0.2216** (0.106)
Eff. sample size	3638	3003	2419	2098	1411	1381	1676	1167	1252
	Panel C. Probability of Failure								
Focal child born after school entry	-0.0035 (0.015)	0.0054 (0.044)	-0.0176 (0.050)	-0.0304 (0.015)	0.0541 (0.110)	-0.0881 (0.104)	0.0411 (0.023)	-0.0217 (0.191)	-0.1737 (0.103)
Eff. sample size	2521	3791	3821	1460	1011	1011	1370	612	1101
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by sibling grade using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample to only siblings who are girls, and in columns 7-9 we restrict for siblings boys. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A12: Heterogeneous effects divided by focal child gender - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Female			Male		
	Panel A. Math Score								
Focal child born after school entry	0.1728*** (0.058)	0.1850*** (0.057)	0.1864*** (0.076)	0.1107 (0.076)	0.1844* (0.112)	0.2561*** (0.110)	0.2795*** (0.083)	0.2841*** (0.128)	0.1894*** (0.065)
Eff. sample size	2890	2623	2349	1638	1013	1163	1329	929	2218
	Panel B. Reading Score								
Focal child born after school entry	0.2160*** (0.050)	0.1398*** (0.056)	0.1574** (0.074)	0.1462*** (0.057)	0.1648* (0.060)	0.1271 (0.072)	0.2915*** (0.081)	0.1398* (0.106)	0.1790** (0.180)
Eff. sample size	3638	3003	2419	2 439	960	1146	1375	1550	1511
	Panel C. Probability of Failure								
Focal child born after school entry	-0.0035 (0.015)	0.0054 (0.044)	-0.0176 (0.050)	0.0101 (0.015)	0.0535 (0.130)	-0.0075 (0.114)	-0.0318 (0.023)	0.0280 (0.126)	0.0611 (0.119)
Eff. sample size	2521	3791	3821	2036	792	962	1135	895	913
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our heterogeneous effects by focal child gender using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. In columns 1-3, we have our main result for the whole sample (first three columns of Table 4); in columns 4-6, we restrict our sample for only siblings whose focal child is female, and in columns 7-9 we restrict for siblings whose focal child is male. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A13: Main results using fixed bandwidth of 30 days - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
Panel A. Math Score									
Focal child born	0.1918***	0.2672***	0.2224***	0.2205***	0.3669***	0.1941***	0.1290	0.0491	0.2810*
after school entry	(0.050)	(0.069)	(0.069)	(0.058)	(0.078)	(0.081)	(0.099)	(0.148)	(0.156)
Eff. sample size	3473	2665	2665	2645	2027	2027	828	638	638
Panel B. Reading Score									
Focal child born	0.2073***	0.1259*	0.2131***	0.1726***	0.1535**	0.1901***	0.1146	0.0465	0.1831
after school entry	(0.0521)	(0.067)	(0.069)	(0.060)	(0.077)	(0.081)	(0.101)	(0.146)	(0.136)
Eff. sample size	3473	2665	2665	2645	2027	2027	828	638	638
Panel C. Probability of Failure									
Focal child born	0.0021	0.0669	-0.1288*	0.0020	0.0600	-0.0439	-0.0003	-0.0926	-0.2700*
after school entry	(0.014)	(0.073)	(0.076)	(0.015)	(0.082)	(0.088)	(0.033)	(0.151)	(0.148)
Eff. sample size	2744	2095	2095	2085	1590	1590	659	505	505
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Main results using fixed bandwidth of 45 days - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
Panel A. Math Score									
Focal child born	0.1785***	0.2159***	0.1938***	0.1729***	0.3051***	0.1822***	0.1208	0.0224	0.1754*
after school entry	(0.038)	(0.052)	(0.052)	(0.043)	(0.059)	(0.061)	(0.074)	(0.107)	(0.101)
Eff. sample size	5120	3953	3953	3901	3013	3013	1219	941	941
Panel B. Reading Score									
Focal child born	0.1310***	0.1089**	0.1580***	0.1706***	0.1051*	0.1821***	0.1281	-0.0028	0.1695*
after school entry	(0.039)	(0.051)	(0.052)	(0.046)	(0.059)	(0.061)	(0.077)	(0.106)	(0.100)
Eff. sample size	5120	3953	3953	3901	3013	3013	1219	941	941
Panel C. Probability of Failure									
Focal child born	-0.0016	0.0584	-0.0679	-0.0034	0.0321	-0.0081	0.0025	0.0062	-0.2387**
after school entry	(0.011)	(0.056)	(0.057)	(0.012)	(0.064)	(0.067)	(0.024)	(0.114)	0.1094
Eff. sample size	4052	3112	3112	3081	2368	2368	971	744	744
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Main results using fixed bandwidth of 60 days - Younger FC sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
	Panel A. Math Score								
Focal child born	0.1494***	0.1003***	0.1120***	0.1832***	0.0910*	0.1288**	0.0645	-0.0102	0.0707
after school entry	(0.031)	(0.043)	(0.043)	(0.037)	(0.049)	(0.051)	(0.062)	(0.086)	(0.082)
Eff. sample size	6786	5268	5268	5193	40345	4034	1592	1234	1234
	Panel B. Reading Score								
Focal child born	0.2028***	0.096**	0.1053***	0.2524***	0.1105**	0.1193***	0.0717	-0.0093	0.0693
after school entry	(0.032)	(0.043)	(0.043)	(0.038)	(0.050)	(0.051)	(0.064)	(0.085)	(0.082)
Eff. sample size	6786	5268	5268	5193	4034	4034	1592	12349	1234
	Panel C. Probability of Failure								
Focal child born	0.0001	-0.0159	-0.0508	-0.0081	-0.0283	-0.0466	0.0240	-0.0241	-0.0531
after school entry	(0.009)	(0.047)	(0.047)	(0.010)	(0.055)	(0.056)	(0.019)	(0.094)	(0.089)
Eff. sample size	5387	4159	4159	4114	3180	31802	1273	979	979
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our main results with a fixed bandwidth using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A16: Placebo cutoff effects in our younger focal child sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All		Not BF Program			BF Program		
Panel A. Math Score									
Focal child born	0.0577*	0.0679	0.0701	0.0890*	0.047	0.0742	0.0846	0.0494	0.1095
after school entry	(0.032)	(0.043)	(0.044)	(0.047)	(0.05)	(0.051)	(0.063)	(0.080)	(0.084)
Eff. sample size	6791	5263	5263	5195	4021	4021	1596	1241	1241
Panel B. Reading Score									
Focal child born	0.0197	0.0305	0.0729*	0.0655*	0.0530	0.0835	0.1050	0.0511	0.1092
after school entry	(0.033)	(0.042)	(0.044)	(0.038)	(0.049)	(0.051)	(0.066)	(0.080)	(0.084)
Eff. sample size	6791	5263	5263	5195	4021	4021	1596	1241	1241
Panel C. Probability of Failure									
Focal child born	-0.0025	0.0416	-0.0429	-0.0078	0.0303	-0.0224	0.0117	-0.0214	-0.0938
after school entry	(0.009)	(0.048)	(0.047)	(0.010)	(0.055)	(0.055)	(0.020)	(0.092)	(0.089)
Eff. sample size	5383	4147	4147	4109	3161	3161	1274	986	986
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

Note: The table presents our placebo cutoff results using RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. The placebo cutoff considered is after 75 days after the last day of our fixed bandwidth. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Placebo effects in our younger focal child sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Not BF Program			BF Program		
Panel A. Math Score									
Focal child born	-0.1310	-0.0045	-0.1071	-0.1246	0.0016	-0.1196	-0.0611	0.0093	-0.1636
after school entry	(0.080)	(0.069)	(0.076)	(0.090)	(0.083)	(0.085)	(0.129)	(0.090)	(0.142)
Eff. sample size	1542	1734	1650	1238	1204	1717	487	910	335
Panel B. Reading Score									
Focal child born	-0.1077	0.0068	-0.0942	-0.0959	0.0117	-0.1068	-0.0620	-0.0296	-0.1636
after school entry	(0.077)	(0.065)	(0.066)	(0.088)	(0.064)	(0.065)	(0.115)	(0.123)	(0.143)
Eff. sample size	1559	1884	1657	1215	1828	1704	529	529	332
Panel C. Probability of Failure									
Focal child born	0.0053	-0.0871	-0.1615	-0.0188	-0.1596	-0.1513	0.0446	0.1021	-0.2162
after school entry	(0.020)	(0.082)	(0.105)	(0.022)	(0.092)	(0.095)	(0.032)	(0.133)	(0.146)
Eff. sample size	740	1101	1111	542	843	846	304	409	415
Sibling controls		✓	✓		✓	✓		✓	✓
Focal child controls			✓			✓			✓

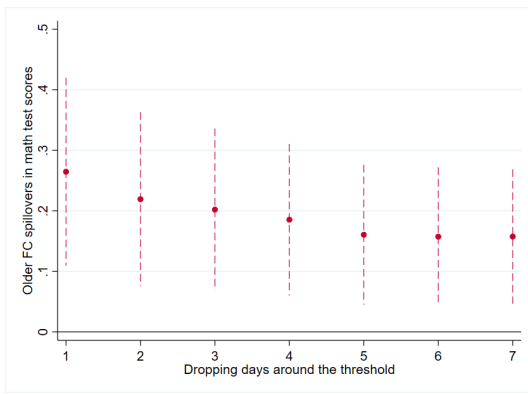
Note: The table presents our placebo results for test scores obtained before exposure of focal child to cutoff rule. Results use RD Honest method for the sample in which the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores; in panel B, it is the standardized reading test scores, and in panel C it is an indicator variable for the student who has already failed a grade in school. In columns 1-3, we have the full sample; in columns 4-6 and 7-9, we divide our sample into families that are not and are in Bolsa Familia Program, respectively. Columns 1, 4, and 7 do not include any controls; columns 2, 5, and 8 include only siblings controls, and columns 3, 6, and 9 also include focal child covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A18: Spillover effects with different RD arguments in our younger focal child sample

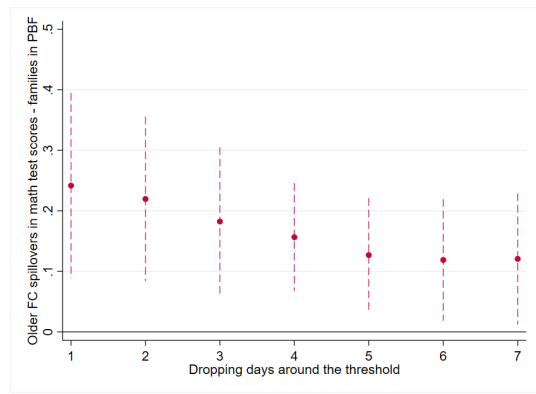
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All					Not BF Program				
	Panel A. Math Score									
Focal child born	0.1864***	0.1804***	0.2156***	0.2217***	0.2425***	0.1729***	0.1953***	0.1827***	0.1705***	0.1734**
after school entry	(0.076)	(0.072)	(0.072)	(0.072)	(0.092)	(0.072)	(0.067)	(0.067)	(0.066)	(0.081)
Eff. sample size	2349	2291	2452	2541	1942	2101	2401	2543	2633	2008
	Panel B. Reading Score									
Focal child born	0.1574**	0.1701***	0.2042***	0.2134***	0.2393***	0.1311*	0.1762***	0.1628***	0.1525***	0.1596*
after school entry	(0.074)	(0.070)	(0.071)	(0.072)	(0.092)	(0.072)	(0.068)	(0.067)	(0.067)	(0.082)
Eff. sample size	2419	2397	2484	2557	1949	2370	2401	2537	2625	2008
Sibling controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Focal child controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table presents our main results with different RD arguments (kernel, optimum bandwidth criterion and smoothness class) for the sample where the focal child (i.e., student exposed to the cutoff rule) is younger than the sibling. In columns 1 and 6, we repeat our main results for the full sample and only families who are not in Bolsa Familia Program sample, respectively. We use the triangular kernel, MSE optimum bandwidth criterion, and Hölder smoothness class (default arguments) in those columns. In columns 2-3 and 7-8, we changed just the kernel to uniform and epanechnikov, respectively. In columns 4 and 9, we use the default arguments changing the optimum bandwidth criterion to FLCI. In columns 5 and 10, we use the default arguments changing the smoothness class to Taylor. In columns 1-5, we have our full sample, and in columns 6-10, we restrict to siblings whose families do not participate in Bolsa Familia Program. Our treatment is the focal child of the sibling being old-for-grade, i.e., starting the first year of elementary school with at least six years old completed. In panel A, the outcome is the standardized math test scores and, in panel B, it is the standardized reading test scores. In all columns, we use our preferred specification with all covariates. Siblings' controls include gender, race, grade, year, and month of birth. Focal child controls include the entry cohort, gender, and age difference between sibling and focal child. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

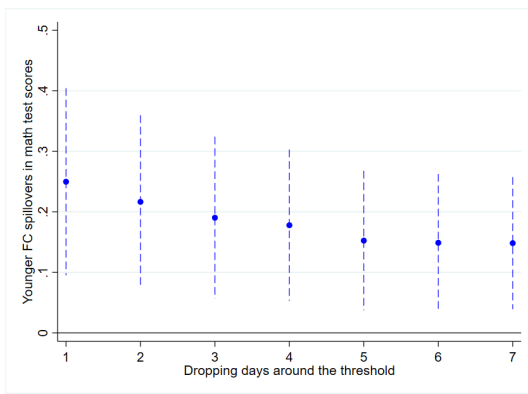
Figure 6: Donut regression - Younger focal child sample



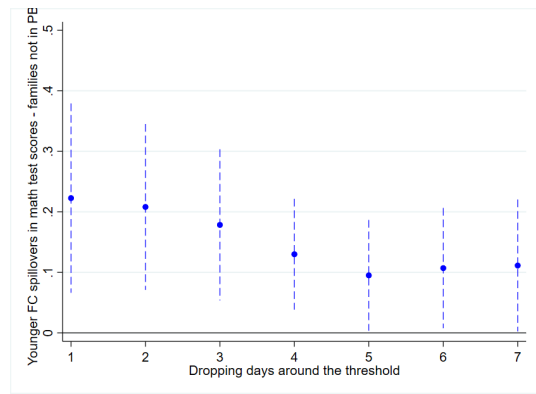
(a) Math spillovers in complete sample



(b) Math spillovers not in BF Program sample



(c) Reading spillovers in complete sample



(d) Reading spillovers not in BF Program sample