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The causal effect of an extra year of schooling on skills and knowledge in Latin America. Evidence from PISA

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ABSTRACT

In this paper, we estimate the causal effect of an extra year of schooling on mathematics performance for seven Latin American countries based on PISA 2012. To that end we exploit exogenous variation in students' birthdates around the school entry cut-off date using both sharp and fuzzy Regression Discontinuity designs. We find strong effects of an extra year of schooling in most countries, which amount to a 30% increase in PISA test scores in Brazil, 18% in Uruguay, 7% in Argentina and 6% in Costa Rica. These effects differ from the typical estimates obtained from simple regressions or multilevel models and are large enough to allow 15-year-old students to reach higher proficiency levels, suggesting significant potential gains of reducing dropout rates in the region. Finally, we stress the importance of taking into account the effects of school entry cut-off dates on PISA samples to avoid making unfair international comparisons.

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1. Introduction

Transition from school to work can be very traumatic, especially in Latin America where the high levels of informality and the lack of good job opportunities may discourage adolescents from achieving their initial aspirations. Both cognitive and non-cognitive skills are valuable assets that may help young adults to face the challenges that this transition poses. Despite the importance of non-cognitive skills (Heckman & Kautz, 2012), cognitive abilities play a key role when pursuing higher level education or when entering the labour market, and are essential for future success (Hanushek & Woessmann, 2008). Among other domains, skills in mathematics are a key determinant of individual's life chances. For instance, evidence from the OECD's Survey of Adult Skills (first round 2008–2013) suggests that a poor development of mathematics skills severely limits young adults' ability to participate in post-secondary education and their labour prospects and earnings.

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The aim of this paper is to contribute to the understanding of the relationship between skills formation and schooling in Latin America. We estimate the effect of one additional year of schooling on mathematics skills and knowledge in seven Latin American countries (Argentina, Brazil, Chile, Costa Rica, Mexico, Peru and Uruguay), based on results from the Programme for International Student Assessment (PISA) conducted in 2012. PISA 2012 focuses on mathematics, not just looking at what 15-year-old students know, but also at what they can do with the skills developed at school. Therefore, the size of this effect may indicate the extent to which the curriculum being taught in schools at the age of 15 contributes to build the abilities needed to meet the challenges of adult life.

We estimate the causal effect of an extra year of schooling on test scores by exploiting exogenous variation in students' birthdates around the school entry cut-off date using a Regression Discontinuity (RD) design. Laws and regulations in the different countries of the region establish that children with a certain age by a particular date must enrol in the first year of primary education, which causes differences in school grades between students with almost the same age. If these regulations are enforced and it is unlikely or impossible to manipulate the date of birth near the cut-off, we can isolate the causal effect of an extra year of schooling on mathematics test scores by comparing the performance of students born just before and just after the school cut-off date. We apply both sharp and fuzzy RD approaches to take into account the fact that some students attend a different grade from the one that corresponds to their age because of grade retention or other reasons.

We find strong effects of an extra year of schooling on mathematical performance of 15-year-old students in Latin America. The estimated effect reaches the 113 PISA points in some states in Brazil, which represents almost a 30% increase in mean scores between the 10th and 11th school grades. The effect is also large in Uruguay (18%), Argentina (7%) and Costa Rica (6%). In terms of the contribution to skills formation, these effects are large enough to allow 15-year-old students in these countries to reach higher proficiency levels, suggesting significant potential gains of reducing dropout rates in the region.

Finally, we discuss the implications of our findings for the PISA sample design. Differences in school entry cut-off dates across countries result in sample imbalances that should be taken into account to avoid making unfair international comparisons. We perform a simple simulation exercise to highlight how global rankings may be affected by these imbalances and give a straightforward recommendation for survey development that may help to attenuate the biases.

The rest of the paper is organised as follows. Section 2 presents the data and discusses the methodology. Section 3 briefly describes school start age policies and their enforcement in the region, and also provides preliminary evidence on the impact of such policies on PISA test scores. Estimation results are reported and described in Section 4. Section 5 discusses the implications of our findings for the PISA sample design, while Section 6 concludes.

2. Data and methodology

We use data from the Programme for International Student Assessment (PISA) conducted in 2012. PISA is a programme undertaken by the OECD to assess whether 15-year-old students have acquired the skills and knowledge needed to meet the challenges of adult life (OECD, 2013a).[1] PISA 2012 focused on mathematics as the major domain, assessing mathematics skills developed in schools, but not just looking at what students know but also at what they can do with that knowledge. According to OECD (2014a),

> PISA seeks to measure not just the extent to which students can reproduce mathematical content knowledge, but also how well they can extrapolate from what they know and apply their knowledge of mathematics, in both new and unfamiliar situations. This is a reflection of modern societies and workplaces, which value success not by what people know, but by what people can do with what they know.

The assessment is carried out through standardised tests administered to students at randomly selected schools in every participating country. In addition to the tests, the programme collects information about students and schools using a background questionnaire for students and school principals.

Our analysis focuses on seven Latin American countries that participated in PISA 2012: Argentina, Brazil, Chile, Costa Rica, Mexico, Peru and Uruguay, where a total of 81,726 students (representing almost 5 million) were evaluated.[2]

In this paper, we aim at estimating the causal effect of an extra year of schooling on PISA test scores by exploiting exogenous variation in students' birthdates around the school entry cut-off date using a Regression Discontinuity (RD) design.[3] Laws and regulations in most of the countries of the region establish that children with a certain age by a particular cut-off date must enrol in primary school. Therefore, the validity of the RD design relies on the enforcement of such rules and the unlikely or impossible manipulation of the date of birth near the cut-off.

The use of fixed school entry dates as an exogenous source of variation of years of schooling has a long standing tradition in the Economics literature, starting with Angrist and Krueger (1991) who showed that in the United States the date of birth is related to school attainment due to school start age policies and compulsory attendance laws. More recently, other authors have used the same instrument to answer related questions. For instance, Black, Devereux, and Salvanes (2011) and Fredriksson and Öckert (2014) also exploit exogenous variation in school starting age due to month of birth and the school entry cut-off date to assess the long-run effects (e.g. IQ at the age of 18, school attainment, labour market outcomes) of school starting age for Norway and Sweden, respectively.

Within this literature, our paper is more related to studies that examine in-school outcomes based on international assessment data. For instance, Strom (2004) estimates the effects of age on achievement in Norway based on PISA 2000 and Wolff (2012) does the same for Germany using PISA 2003. More similar to ours are the papers that focus on the effect of one additional year of schooling on PISA test scores: Frenette (2008) for Canada using PISA 2000, Benton (2014) for England using PISA 2000 and 2003, Khaw and Wong (2012) for Singapore based on PISA 2000, and Lau and Wong (2013) for a group of high-performing countries based on PISA 2009. Interestingly, Kyriakides and Luyten (2009) also exploit exogenous variation in school starting age to assess the effect of one year of schooling on both curriculum and non-curriculum-based tests in Cyprus, but they extend the approach over the six grades of secondary education by taking into account multiple cut-off points. To the best of our knowledge, there is no other work that studies the causal effect on an additional year of schooling on skills and knowledge in Latin America based on standardised international assessment data such as PISA.

The existent rules on the age at school entry imply that 15-year-old students are expected to be attending one of two possible modal grades when participating in PISA, depending on whether they were born before or after the cut-off date. For instance, children in Argentina whose birthday is before June 30 must start primary school in the year they turn 6, while those whose birthday is after June 30 must start one year later, i.e. the year they turn 7. If they follow the normal rule, by the time they participate in PISA they should be attending the 11th and 10th grades, respectively. We therefore focus on assessing the effect of an additional year of schooling from the lower to the upper of the two modal grades in PISA, which are the 10th and 11th grades in most Latin American countries (see Section 3 and Appendix 1 for more details).

The most basic strategy to identify the causal effect of a school year on performance would be to restrict the sample to those students who were born just before and just after the cut-off date, argue that these two groups have the same average characteristics except from the fact that those who were born just before the cut-off date have an extra year of schooling, and finally attribute the difference in mean test scores to the extra year of schooling.[4] This would be a sharp RD design, in which the treatment (having an extra year of schooling) is a deterministic function of the birthdate that jumps from 0 to 1 at the cut-off date. Formally, the sharp RD design estimates Equation (1):

$$\beta_s = \lim_{B \to B_0^-} E(Y_i|B_i = B) - \lim_{B \to B_0^+} E(Y_i|B_i = B) \tag{1}$$

where Y_i is the test score of student i and B_i her date of birth; B_0 is the cut-off date and the probability of treatment is $T_i = 1\{B_i \leq B_0\}$. Under the assumption that the score for student i would have been the same just before and just after the cut-off, Equation (1) equals the average treatment effect at the cut-off.

One problem with the sharp RD design is that in our data there are some students attending a different school grade from the one that corresponds to their date of birth, even when the analysis focuses on the two modal grades. In other words, there is not a one-to-one correspondence between the date of birth and the grade a student attends, which can be due to grade retention or to other (unobserved) reasons such as early or late primary school enrollment. We refer to the first group as *repeaters* and the second group as *noncompliers*. Although grade retention is one of the causes for non-compliance, we prefer to keep these two groups differentiated for reasons that will be evident later on.

The presence of repeaters and other noncompliers makes the estimator in (1) biased for the effect of an additional year of schooling. For instance, consider the case of repeaters. Given the age range in PISA and the fact that we focus on the two modal school grades (10th and 11th grades in almost all Latin American countries), most of the repeaters we observe in our estimation sample were born before the cut-off date (although attending the 10th grade). Instead, repeaters in PISA who were born after the cut-off date should be attending grades 9th or lower and so they are not included in the estimation sample. Therefore, since repeaters usually perform worse than non-repeaters, this source of non-compliance generates a downward bias in the sharp RD estimates (1) based on the sample that includes repeaters.

As a first preliminary step to deal with this issue, we restrict the sample to *compliers* only, i.e. those students attending the grade that corresponds to their date of birth in the two grades of interest, and estimate the effect of an extra year of schooling for compliers whose date of birth is close to the school entry cut-off date using a sharp RD approach. Of course, the exclusion of noncompliers and repeaters may lead to selection bias in a sharp RD design, so in the next step we include repeaters and noncompliers into the analysis, which causes the probability of treatment (having an extra year of schooling) to not change from 0 to 1 at the cut-off date. This is the so called fuzzy RD design, where treatment is a random variable given B_i, but the probability of treatment is discontinuous at the threshold B_0. In this case, to estimate the causal effect of an extra year of schooling we need to estimate the ratio between the change in the test scores at the cut-off and the change in the proportion of students treated also at the cut-off. Formally, in a fuzzy RD design we can recover the treatment effect by estimating Equation (2):

$$\beta_F = \frac{\lim_{B \to B_0^-} E(Y_i | B_i = B) - \lim_{B \to B_0^+} E(Y_i | B_i = B)}{\lim_{B \to B_0^-} Pr(T_i = 1 | B_i = B) - \lim_{B \to B_0^+} Pr(T_i = 1 | B_i = B)} \tag{2}$$

In a fuzzy design, Equation (2) equals the average treatment effect at the cut-off for those induced to change treatment status at that discontinuity point[5] as long as the following two assumptions hold: *monotonicity* (i.e. B_i crossing B_0 does not cause at the same time that some individuals take up the treatment and others reject it) and *excludability* (i.e. B_i crossing B_0 does not impact Y_i except through its effect on the receipt of treatment).[6] We postpone until Section 4 the discussion on the validity of these assumptions in our case.

Implementation of a sharp RD design consists in estimating and comparing means at the limit, as Equation (1) suggests. In the standard model in Equation (3), we are interested in estimating parameter β when birthdates are arbitrarily close to the cut-off date.

$$Y_i = \alpha + \beta T_i + f(B_i) + \varepsilon_i \tag{3}$$

where, by abuse of notation, B_i is re-centred subtracting the cut-off value from the birthdate. The question is what observations should we consider and how should we weight them to estimate the regression (3) at the limit. This involves the choice of the functional form (at least the polynomial degree) and the bandwidth around the cut-off point. Typical specifications include mean comparison, local linear and polynomial regressions, and low order polynomials.[7]

A limitation that we face is that students' exact date of birth is not available in PISA 2012 published databases. Instead, our B_i variable is just an indicator of the month of birth, which, of course, does not vary across days within a month and it is a rounded down discrete measure of age. As noted by Lee and Card (2008), when data on the variable that determines treatment is only available in discrete intervals, the researcher has to assume a parametric functional form, since the treatment effect is not non-parametrically identified. The standard practice is to estimate low-order polynomial models (see, for instance, Card & Shore-Sheppard, 2004; DiNardo & Lee, 2004; Lee & Card, 2008). However, a recent literature has emerged to address the issue of a discrete running variable in a RD design (Cattaneo, Frandsen, & Titiunik, 2015; Cattaneo, Idrobo, & Titiunik, 2017; Frandsen, 2017; Dong, 2015; Dong & Yang, 2017; Imbens & Wager, 2017; Kolesár & Rothe, 2017). Dong (2015) shows that standard RD estimation using a rounded discrete running variable leads to inconsistent estimates of treatment effects. Moreover, she provides formulas to correct the estimates and standard errors for the resulting discretisation bias. In a more recent article, Dong and Yang (2017) provide an implementation procedure that allows obtaining the bias-corrected coefficients and standard errors directly from a regression with transformed variables. In this paper, we follow their approach assuming a polynomial of degree

0 (mean comparison) and test the robustness of our results using alternative bandwidths and a polynomial of degree 1.[8, 9]

The fuzzy RD can be implemented using two-stage least squares (Hahn et al., 2001). Formally, the fuzzy RD design can be summarised by a system consisting of the standard model in Equations (3) and (4), which indicates that the treatment in the fuzzy RD design is in part determined by $D_i = 1(B_i \leq B_0)$, i.e. whether the student was born before the cut-off date.

$$T_i = \delta + \varphi D_i + h(B_i) + v_i \tag{4}$$

The relevant parameter β can then be estimated by two stages least squares instrumenting the treatment T_i (having an extra year of education) with the indicator D_i. This is equivalent to estimating the ratio between the jump in average test scores at the cut-off and the jump in the probability of treatment at that point. As in the sharp RD design, we will adopt a mean comparison strategy and use different bandwidths and a polynomial of degree 1 as robustness checks. We also correct for the discretisation bias following Dong (2015) and Dong and Yang (2017).

Summing up, we apply both sharp and fuzzy RD designs to estimate the effect of an extra year of schooling on mathematics skills using data from PISA 2012 for seven Latin American countries participating in the survey. Since grade retention is a common practice in most countries in the region and enforcement of school entry rules is not perfect, we apply a sharp RD approach to the sample that only includes compliers. If noncompliance is independent of the potential score, the comparison of the estimated average score of compliers who were born just before and just after the cut-off date is the local average treatment effect. However, to avoid having to rely on such a strong assumption we complement the analysis with a fuzzy RD design for all students in the two grades of interest.

3. School entry age, years of schooling and mathematics skills in Latin America

Most educational systems have a unique cut-off date for school eligibility that splits children of similar ages into two different school grades, and Latin American countries are no exception. Since PISA defines its target population based on students' age instead of the grade they attend, the combination of the cut-off date with students' birthdates provides a source of exogenous variation in years of schooling that we exploit to identify the effect of an extra year of schooling on skills as measured by PISA test scores in mathematics.[10] Nevertheless, as discussed in the previous section, enforcement of school entry rules is not perfect in the region and grade retention is common practice, causing that not all students attend the grade that corresponds to their age. As defined earlier, we refer to the group of students that follow the normal rule as compliers.

For most Latin American countries, compliers in the PISA 2012 samples are in grades 11 or 10, depending on whether they were born before or after the cut-off date in force when they entered primary school.[11] Therefore, our analysis focuses on the effect on skills of an extra year of schooling from the 10th to the 11th grade. The only exceptions are Mexico and Costa Rica, where compliers attend grade 10 and 9 depending on whether their birthdates are before or after the cut-off date, respectively.[12] The case of Brazil deserves a separate discussion. The cut-off date in Brazil varies by state and its enforcement was null or very low for students in our sample in most of the states. Thus, we restrict the analysis to the

Table 1. School year for compliers who were born before and after the cut-off date, based on the characteristics of the educational systems in Latin America and the PISA design.

Country	Beginning of school year	Implementation of PISA	Cohort in the sample	Primary school entry age	Cut-off date for the cohort	School year for a complier born: Before cut-off date	After cut-off date
Argentina	February/ March	August 2012	06/96–05/97	6 years	June 30	11	10
Brazil*	February	March 2012	01/96–12/96	6 years	Varies by state *(reference: June 30)*	11	10
Chile	February/ March	July 2012	05/96–04/97	6 years	June 30	11	10
Costa Rica	February	May 2012	03/96–02/97	7 years**	October 31	10	9
Mexico	August	March 2012	01/96–12/96	6 years	September 1	10	9
Peru	March	July 2012	05/96–04/97	6 years	July 31	11	10
Uruguay	March	July 2012	05/96–04/97	6 years	April 30	11	10

Notes: *In Brazil, the school year for a student in grade 11 (10) in the sample is in fact her 10th (9th) year of formal education, since the primary school entry age for this cohort was 7 years.
**The primary school entry age in Costa Rica is 6 years and 3 month. For the cohort of students in the sample, the requirement of being at least 6 years and 3 month old on January 31 is equivalent to have at least 7 years of age on October 31.
Sources: Laws and regulations detailed in the Appendix 1, PISA 2012 data bases and OECD (2014b).

three Brazilian states, where data reveals that a uniform cut-off date (June 30) was strongly enforced, i.e. Amazonas, Distrito Federal and Roraima. Fortunately, we are allowed to do this because the PISA sample is representative at the state level in Brazil.[13] Table 1 summarises the main characteristics of school entry policies in Latin America and their implications in terms of schooling years for the cohort participating in PISA 2012. A more thorough discussion is provided in Appendix 1.

As discussed in the previous section, repeaters and other noncompliers in the PISA samples attend a different grade from that corresponding to their age given the school entry rules. The former group includes students who report having retained a school year in the past. The other group, i.e. the group of noncompliers, report never having retained a grade, which suggests that enforcement of school entry policies is not perfect. While it is possible to identify the repeaters in the data, we do not observe other possible causes for non-compliance, e.g. early or late enrollment.[14] Table 2 reports the participation of these different groups of students in the PISA sample in each of the two grades of interest. In general, the share of noncompliers is higher in the upper schooling grade (presumably early enrollers), while the proportion of repeaters is higher in the lower schooling grade. The latter result is a consequence of the target age in the PISA sample combined with the cut-off date, which makes it very difficult to find 15-year-old repeaters in the upper grade.

Table 3 shows mean scores in mathematics for the upper and lower grades of interest.[15] As expected, students attending the upper grade perform better than those in the lower grade, a stylized fact that triggered our analysis in the first place. Figure 1 compares the mean scores in the two grades of interest for all students (Panel A) and for compliers only (Panel B). For most countries, the gap in performance between the two grades is narrower for compliers. This is partly a consequence of the fact that repeaters, who perform worse in the tests, are more concentrated in the lower grade as Table 2 shows.

Table 2. PISA 2012 sample: Number of observations and proportion of repeaters and noncompliers in the two grades of interest.

	No. of students		Proportion of non-compliers		Proportion of repeaters	
Country	Upper grade	Lower grade	Upper grade	Lower grade	Upper grade	Lower grade
Argentina	190	3765	0.28	0.01	0.07	0.06
Brazil	561	820	0.28	0.10	0.04	0.37
Chile	417	4773	0.02	0.08	0.00	0.02
Costa Rica	1799	1952	0.05	0.06	0.01	0.34
Mexico	24,091	7230	0.17	0.09	0.01	0.32
Peru	1456	2907	0.46	0.05	0.01	0.11
Uruguay	68	3051	0.21	0.07	0.00	0.01

Notes: (a) The upper and lower grades are grade 11 and 10 respectively, except in Mexico and Costa Rica where the upper grade is the 10th and the lower is the 9th. (b) Brazil: Amazonas, Distrito Federal and Roraima.
Source: Authors' own calculations based on PISA 2012 data bases.

Table 3. Mean score in mathematics in the upper and lower grade under analysis for different subsamples. Latin American countries in PISA 2012.

	All students		Compliers		Non-compliers		Repeaters	
Country	Upper grade	Lower grade	Upper grade	Lower grade	Upper grade	Lower grade	Upper grade	Lower grade
Argentina	418.5	414.4	433.1	416.8	401.6	388.8	369.9	382.3
	(11.6)	(3.8)	(12.1)	(3.7)	(15.2)	(18.0)	(17.3)	(6.4)
Brazil+	433.9	383.6	433.5	392.9	439.8	377.1	392.8	370.5
	(8.0)	(6.0)	(10.1)	(8.0)	(8.3)	(10.5)	(24.8)	(6.0)
Chile	448.3	440.5	448.8	441.4	428.8	452.3	–	387.4
	(4.8)	(2.9)	(4.8)	(3.0)	(27.4)	(6.7)	–	(6.9)
Costa Rica	436.7	405.2	436.6	414.6	450.8	425.2	391.4	385.6
	(3.5)	(2.9)	(3.4)	(3.1)	(8.6)	(9.7)	(15.3)	(3.2)
Mexico	429.0	393.5	429.9	408.0	429.3	401.7	392.7	363.9
	(1.8)	(2.6)	(1.7)	(2.9)	(3.0)	(7.5)	(5.3)	(2.6)
Peru	408.9	381.3	405.2	386.5	414.9	364.4	351.7	352.2
	(4.0)	(4.3)	(4.6)	(4.2)	(4.6)	(12.3)	(16.3)	(5.2)
Uruguay	501.0	448.5	499.9	449.2	504.7	451.6	–	339.0
	(10.7)	(2.8)	(12.2)	(2.7)	(20.2)	(7.0)	–	(32.5)

Notes: (a) Balanced Repeated Replication (BRR) standard errors in parenthesis, computed following Pisa Data Analysis Manual (OECD 2009, chapters 7 and 8). (b) +Brazil: Distrito Federal, Amazonas and Roraima.
Source: Authors' own calculations based on PISA 2012 data bases.

In the next section, we use students' birthdates around the school entry cut-off date as an exogenous source of variation in years of schooling to estimate the causal effect of an additional year of schooling on PISA test scores. As discussed in Section 2, we apply a fuzzy RD approach to deal with imperfect compliance of the school-entry-age rule.

4. Results from the RD design

4.1. Preliminary evidence based on a sharp RD design

This subsection presents preliminary evidence on the effect of an extra year of schooling on mathematics skills and knowledge using a sharp RD design for the sample that includes compliers only, i.e. students attending the school grade that corresponds to their age. Even though this sample may be subject to some sort of selection, these preliminary results are quite robust to the inclusion of repeaters and other noncompliers, which we do in the next subsection where a fuzzy RD approach is applied.

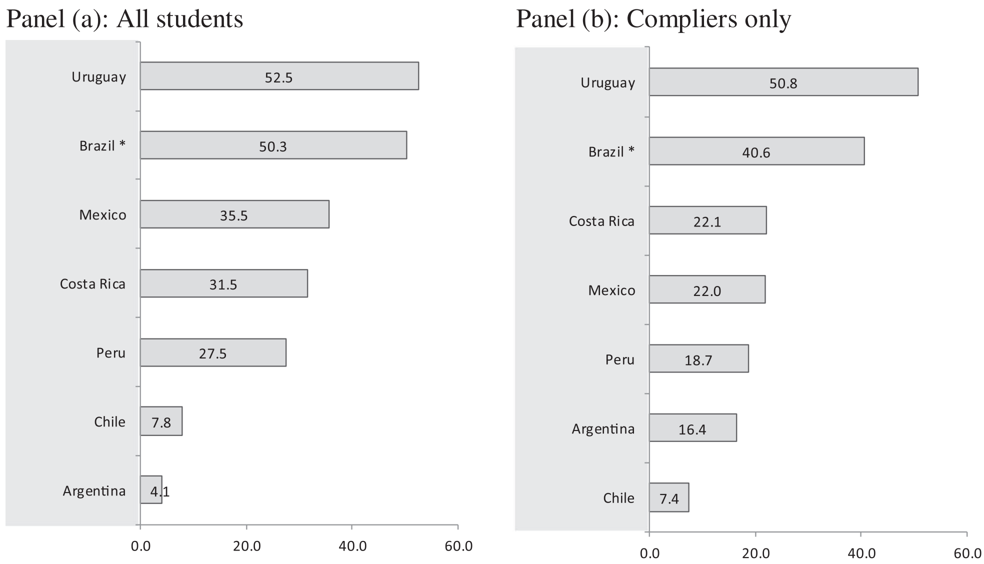

Figure 1. Difference between the mean score in mathematics in the upper and lower grade under analysis. Latin American countries in PISA 2012. Source: Authors' own calculations based on PISA 2012 data bases.
Notes: (a) The upper and lower grades are grade 11 and 10, respectively, except in Mexico and Costa Rica where the upper grade is the 10th and the lower is the 9th. (b) * Brazil: Amazonas, Distrito Federal and Roraima.

Figure 2 shows the mean mathematics performance in PISA 2012 by month of birth in each country. A vertical line has been added to indicate the school entry cut-off date in force at the time the students in our sample enrolled in primary education. The points to the left of that line correspond to students born before the cut-off date and who were attending the higher of the two grades of interest when PISA 2012 was implemented. For instance, the cut-off date in Argentina is June 30. Therefore, children born in June 1996 (first point from the left in the corresponding graph) entered primary school the year they turned 6 and if they followed the normal rule (they did not retain or advanced a grade) they should be attending the 11th grade in 2012. Following the same reasoning, points to the right of the vertical line correspond to students born after the cut-off date, thus attending the lower of the two grades of interest, which is the 10th grade in the Argentinian case.

Since PISA covers an age range from 15 years and 3 months to 16 years and 2 months, there are always 12 points in the graphs. But the number of points to the left or to the right of the cut-off line obeys to the conjunction of three elements that vary across countries: primary school entry age, cut-off date, and date of implementation of PISA 2012. Returning to the Argentinian case, PISA was applied in August 2012. By that time there was only one cohort of compliers attending the 11th grade: the group of students born in June 1996, who were 16 years and 2 months old when participated in the evaluation, i.e. the oldest cohort in the sample. Another example with only one cohort to the left of the cut-off line is Uruguay.[16] For the rest of the countries there is more balance in the number of cohorts before and after the cut-off date.

Figure 2 shows that mean test scores jump at the cut-off date in most of the countries. If students born just before and just after that threshold are similar in all observable and unobservable dimensions except that the former have an extra year of schooling due to exogenous rules concerning school entry age, the jump at the cut-off estimates the causal

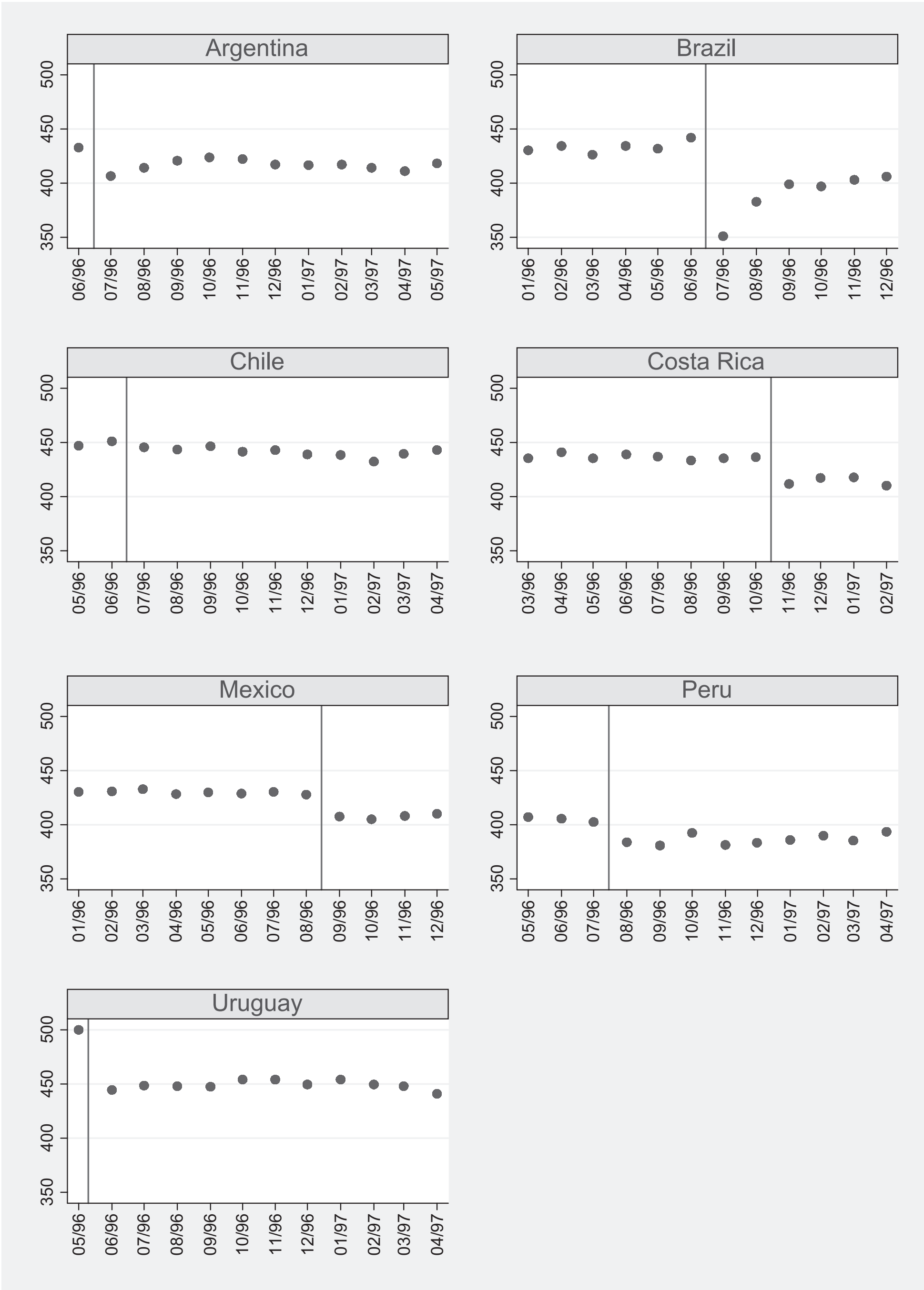

Figure 2. Mean score in mathematics by birthdate in Latin American countries – Compliers only. Source: Authors' own calculations based on PISA 2012 data bases.
Notes: Brazil: Amazonas, Distrito Federal and Roraima.

Table 4. Sharp regression discontinuity design. Effect of a schooling year on mathematics score. Latin American countries in PISA 2012.

	Bandwidth: 2 months			Bandwidth: 1 month		
Country	Baseline model	Model with controls	Obs.	Baseline model	Model with controls	Obs.
Argentina	22.2**	21.5**	696	28.6**	27.3***	380
	(10.6)	(8.8)		(11.7)	(10.5)	
Brazil+	69.6***	56.3***	243	92.2***	80.9***	115
	(14.0)	(11.2)		(21.3)	(18.7)	
Chile	4.4	9.1*	1289	5.4	10.0	638
	(6.2)	(5.3)		(8.5)	(7.4)	
Costa Rica	21.6***	20.4***	1061	23.4***	21***	511
	(6.5)	(5.4)		(7.6)	(6.9)	
Mexico	22.9***	16.2***	6957	20.3***	14.2***	3515
	(3.8)	(3.4)		(5.5)	(4.9)	
Peru	20.2***	10.8*	893	13.4	4.8	416
	(6.8)	(6.1)		(10.9)	(8.8)	
Uruguay	54.2***	45.4***	577	55.7***	48.8***	319
	(12.0)	(12.5)		(11.6)	(12.2)	
Latin America	21.9***	15.6***	11,716	21.2***	15.6***	5894
	(2.7)	(2.5)		(4.2)	(3.8)	

Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) Balanced Repeated Replication (BRR) standard errors in parenthesis, computed following Pisa Data Analysis Manual (OECD 2009, chapters 7 and 8). (c) The baseline model does not include controls and uses a polynomial of degree 0. (d) Controls are: gender (FEMALE), attendance to one year of pre-primary education (EDINFA1), attendance to more than one year of pre-primary school (EDINFA2), the socio-economic level of the student (PARED) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED > 12 years). (e) Estimations for Latin America control for country fixed effects in all specifications. (f) Coefficients and standard errors corrected following Dong (2015) and Dong and Yang (2017). (g) +Brazil: Distrito Federal, Amazonas and Roraima.
Source: Authors' own estimations based on PISA 2012 data bases.

effect of that extra year of schooling. However, one important difference between the two groups is the school starting age. Several studies find positive effects on tests performance of starting school at an older age, and the effect seems to be driven by absolute rather than relative age at the beginning of schooling (Fredriksson & Öckert, 2006, 2014; Strom, 2004). In terms of our analysis, the 'school starting age' effect would imply a potential downward bias in our RD estimates, because students born after the cut-off are almost one-year-older when they enter school than students born before the cut-off. Considering this, it is possible to interpret our RD estimates as a lower bound of the true effect of an additional year of schooling.[17] In the next subsection, we show that students' observable characteristics seem to be balanced on both sides of the cut-off date.

At a first glance of Figure 2, the jump in mean scores at the cut-off date is larger in the aggregate of the three Brazilian states under consideration (Amazonas, Distrito Federal and Roraima). Then follow Uruguay, Costa Rica, Mexico and Argentina. The jump in mean scores is relatively small in Peru while there appears to be no jump at all in Chile.

Table 4 presents the sharp RD estimates of the effect of having an extra year of schooling on mathematics skills as measured by PISA test scores. Estimates are obtained by mean comparison using alternative bandwidths of one and two months at both sides of the threshold.[18] Also, to further assess the robustness of the sharp results, the table reports unconditional as well as conditional estimates that control for gender, preschool attendance (none, one or two years), and family and school socio-economic level.[19]

For most countries, results are quite robust across specifications. The main exception is Brazil, where estimates vary considerably depending on the bandwidth and whether the model includes controls. However, regardless of the model, estimated effects in Brazil far exceed those of the other countries.

In our most preferred specification, i.e. the model with controls using a one month bandwidth, the estimated effects based on the sharp RD design range from 81 points in Brazil to 5 points (though not statistically significant) in Peru.[20] Between these two extremes is Uruguay with 49 points, and then Argentina, Costa Rica and Mexico with 27, 21 and 14 points, respectively. These figures suggest a strong effect of an extra year of schooling on test scores. In terms of the mean score for compliers in the 10th grade (or the 9th grade in Costa Rica and Mexico), the estimated (sharp) effect represents an increase of 21% in Brazil, 11% in Uruguay, 7% in Argentina, 5% in Costa Rica and 3% in Mexico. On the contrary, the contribution of an extra year of schooling for 15-year-olds seems to be relatively small and not statistically significant in Chile (10 points) or Peru (5 points). Results are very similar when, instead of a mean comparison, we use a polynomial of degree 1 (see Table A.1 in Appendix 2).

4.2. Results from a fuzzy RD design

The problem with the sharp RD approach is that excluding repeaters and noncompliers may lead to bias in our estimates of the effect of an extra year of schooling on mathematics skills. Therefore, we now incorporate these two groups into the sample and adopt a fuzzy RD approach to deal with the fact that now the probability of treatment (having an extra year of schooling) does not drop from 1 to 0 at the cut-off date. Figure 3 illustrates this point by showing the proportion of students attending the higher of the two grades of interest for each of the cohorts. It is evident that, even though the probability of treatment is discontinuous at the cut-off date, the change is smaller than 1. This shows clearly in Argentina and Costa Rica, suggesting strong enforcement of the school entry rule in these countries.

As discussed earlier, the validity of the RD design relies on the enforcement of rules concerning school entry age and the unlikely or impossible exact manipulation of the date of birth near the cut-off date. Although the imprecision of control cannot be proved and will often be nothing more than a conjecture, it has clear observable predictions (Lee & Lemieux, 2009). If agents are able to precisely manipulate the forcing variable, the treatment assignment rule is public knowledge, and treatment is desirable (or undesirable), there will presumably be some sorting of individuals around the threshold, and therefore, a jump in the density of the forcing variable at the cut-off date. Figure 4 shows the distribution of birthdates in the PISA 2012 samples for each of the countries under analysis. As expected, the distributions are relatively uniform in most of the countries, with no clear discontinuity at the cut-off date. The main exception is Argentina, where there is a significant drop in the density when crossing the threshold.

Discontinuities in the distribution of birthdates around the cut-off date may be due to some parental control over the date of birth of their children near the cut-off or differences in the rates of school abandonment between the two grades of interest. The first hypothesis seems less likely, since parents do not have precise control of this date, and even if they had, it is not clear whether treatment is desirable or not: while some parents may prefer that their children enter primary education sooner so they do not have to 'lose a year', others may prefer that they enter a bit later, so that they enjoy the academic advantage of being the oldest in the class. Discontinuities in the distribution of birthdates may also emerge if

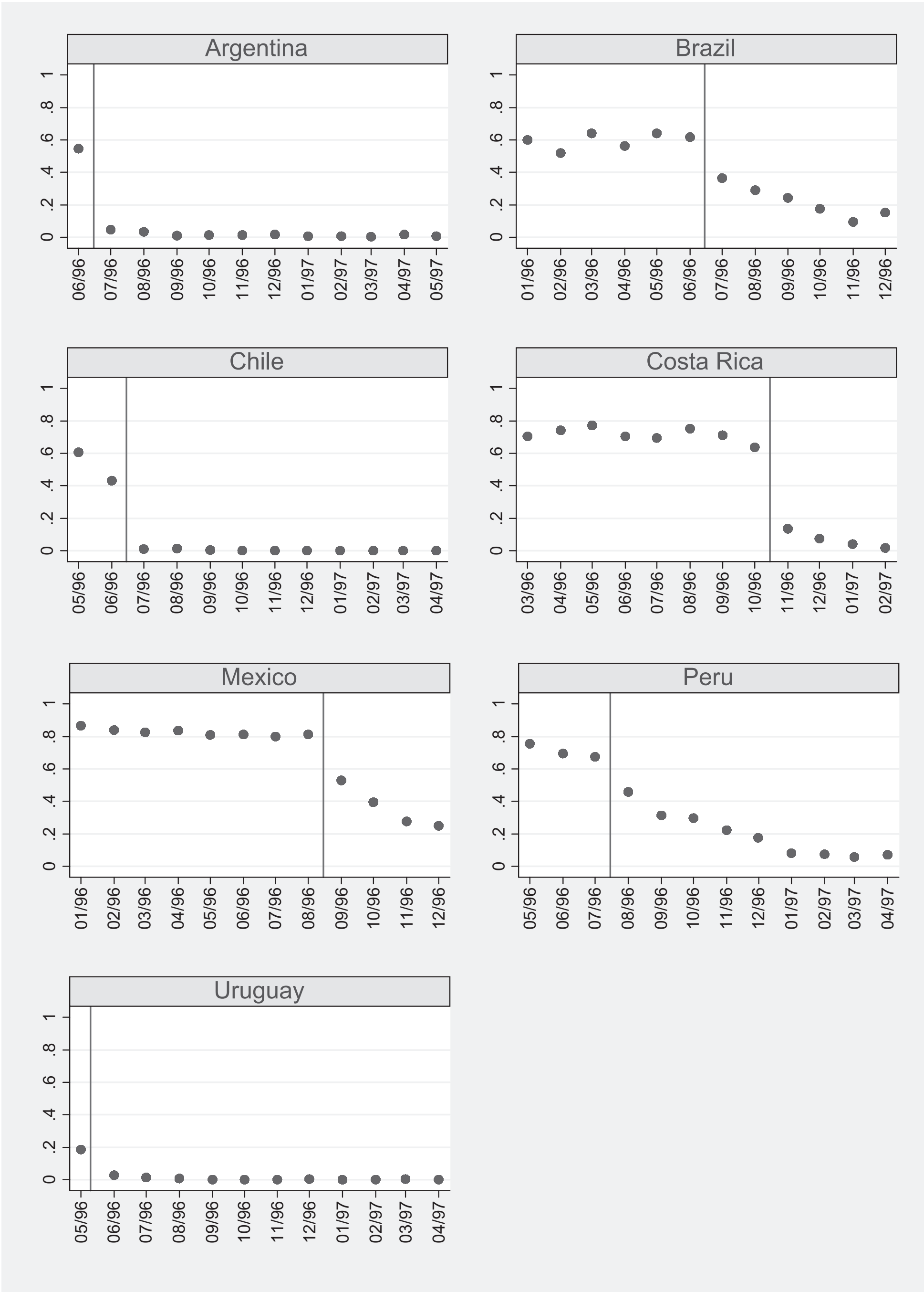

Figure 3. Probability of treatment (having an extra year of schooling) by month of birth – Latin American countries in PISA 2012. Compliers and noncompliers. Source: Authors' own calculations based on PISA 2012 data bases.
Note: Brazil: Amazonas, Distrito Federal and Roraima.

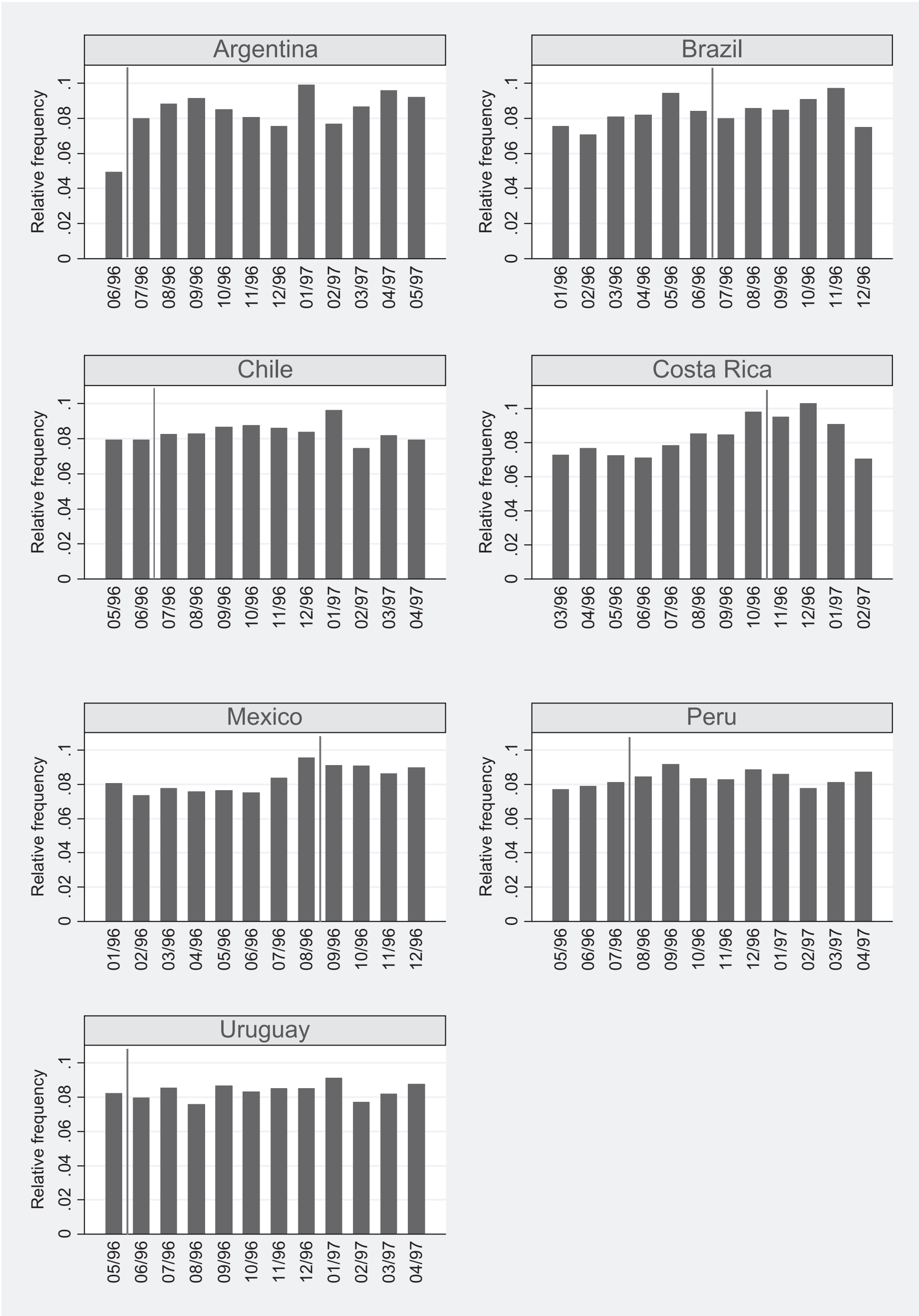

Figure 4. Histogram of the variable date of birth – Latin American countries in PISA 2012. Compliers and noncompliers. Source: Authors' own calculations based on PISA 2012 data bases.

Note: Brazil: Amazonas, Distrito Federal and Roraima.

students drop out school after finishing the lower of the two grades of interest. If students born before the cut-off date are more likely to dropout school than those who were born after that date then our estimates would probably suffer from an upward bias. Our sample of students born before the cut-off would be more selected in that case, with a higher intention to remain in education and presumably higher performance. Even though the histogram inspection suggests that this is not the case, we replicated our RD analysis using school attendance instead of PISA scores for all the countries where census or household survey's data allowed it, and we found no evidence in favour of a discontinuity in dropout rates near the cut-off.[21]

Additionally, we apply a formal test of manipulation of the running variable proposed by Frandsen (2017), which is better suited for a discrete running variable than the traditional test of discontinuity of the density at the threshold (McCrary, 2008). The test relies on three support points of the running variable: the threshold (the smallest treated support point) and its two immediately adjacent points. Limitations in our data prevent us from running such a test in Argentina and Uruguay, but results for Brazil, Chile, Costa Rica and Peru show no evidence of manipulation, while the evidence for Mexico is less conclusive.[22]

A natural way of assessing whether treatment is randomly assigned around the cut-off is to locally compare treatment and control groups based on their observed covariates. Although it is impossible to rule out differences in unobserved characteristics, a discontinuity in the relevant observable covariates at the threshold provides evidence enough to be sceptical about the appropriateness of the RD design. Hence, we test for differences between these two groups in each of the countries based on a large set of variables at the student and school level (see Table A.2 in Appendix 2 for the definition of the variables and Tables A.3.1 to A.3.7 for test results). Based on this evidence, we cannot reject that control and treatment groups are similar, i.e. there is no evidence of covariate imbalance in any of the countries for almost all the variables when using a 1-month bandwidth both before and after the cut-off date.[23] As expected, the two groups are less comparable when using a 2-month bandwidth.[24]

So far, we have no reasons to suspect of sample selection or precise manipulation of the treatment near the cut-off date, and therefore, the assignment to treatment would be as good as random at that point, at least when using a one month bandwidth. We now proceed to the estimation of the effect of an extra year of schooling on skills in a fuzzy RD setting, which consists on estimating Equation (2), i.e. the ratio between the change in the test scores and the change in the proportion of treated students, both at the cut-off date. To that end, we use a two-stage procedure as earlier described in Section 2. Table 5 reports the fuzzy results for different model specifications: with and without controls, and for one month and two month bandwidths. As for the sharp RD estimates, controls include gender, preschool attendance, and family and school socio-economic level. In addition, in all specifications we include a binary variable that identifies repeaters to control for this source of non-compliance.

Generally speaking, fuzzy estimates are similar to the preliminary results from the sharp analysis: the aggregate of the three Brazilian states with strong enforcement of the school entry rule (Amazonas, Distrito Federal and Roraima) lead the ranking, followed by Uruguay, and then Argentina, Costa Rica and Mexico. Again, effects for Chile and Peru are small and not statistically significant. It is important to note that, even after improving precision with the addition of covariates, standard errors are large, thus making the effects in some countries undistinguishable from others in terms of statistical significance.

Table 5. Fuzzy regression discontinuity design. Effect of a schooling year on mathematics score. Latin American countries in PISA 2012.

	Bandwidth: 2 months			Bandwidth: 1 month		
Country	Baseline model	Model with controls	Obs.	Baseline model	Model with controls	Obs.
Argentina	12.8	19.9	857	24.8	28.8*	509
	(15.4)	(14.1)		(16.2)	(15.0)	
Brazil+	72.1***	64.8***	450	104.6***	113.5***	211
	(21.2)	(21.5)		(37.4)	(30.9)	
Chile	9.8	7.9	1770	13.6	7.5	918
	(9.0)	(7.9)		(14.6)	(13.2)	
Costa Rica	23.6***	23.3***	1383	25***	23.2***	704
	(7.1)	(6.0)		(9.4)	(8.7)	
Mexico	21.2***	15***	11,076	15.7*	11.1	5880
	(5.7)	(5.2)		(9.2)	(8.5)	
Peru	4.7	−2.2	1504	−3.2	−16.8	761
	(12.7)	(12.5)		(24.4)	(21.5)	
Uruguay	69.5	67.7*	816	73.8	79.9*	553
	(44.0)	(41.1)		(47.0)	(44.1)	
Latin America	18.6***	13.6***	17,856	18.3***	14.4**	9536
	(4.2)	(4.1)		(6.9)	(6.6)	

Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) Balanced Repeated Replication (BRR) standard errors in parenthesis, computed following Pisa Data Analysis Manual (OECD 2009, chapters 7 and 8). (c) The baseline model only includes a dummy variable that equals 1 for repeaters among controls and uses a polynomial of degree 0. (d) Controls are: gender (FEMALE), attendance to one year of pre-primary education (EDINFA1), attendance to more than one year of pre-primary school (EDINFA2), the socio-economic level of the student (PARED) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED > 12 years). (e) Estimations for Latin America control for country fixed effects in all specifications. (f) Coefficients and standard errors corrected following Dong (2015) and Dong and Yang (2017). (g) +Brazil: Distrito Federal, Amazonas and Roraima.
Source: Authors' own estimations based on PISA 2012 data bases.

In our most preferred specification, i.e. the model with controls using a one month bandwidth, the estimated effect of an extra year of schooling amounts to 113 points in Brazil. From a state-by-state analysis we conclude that this result is driven by Distrito Federal and Amazonas, while results are never statistically significant in Roraima.[25] This is a very large effect (even larger than the preliminary sharp effect), which amounts to almost 30% of the mean score in mathematics for compliers attending grade 10.[26] Second in the ranking is Uruguay, where the contribution of an extra year of schooling is 80 points (18% of the mean score for compliers in the lower grade). Then it is Argentina, Costa Rica and Mexico with 29, 23 and 11 points, respectively (or 7, 6 and 3% of the mean score for the corresponding reference group). Robustness to the order of the polynomial is evaluated in Table A.4 in Appendix 2, where a similar ranking of countries emerges in most specifications.

In most countries, our figures suggest a strong effect of an extra year of schooling on PISA test scores for 15-year-old students, with direct implications in terms of their skills and knowledge. PISA 2012 proficiency levels provide a way to interpret student mean scores in substantive terms. There are six levels of mathematical proficiency, from the lowest, Level 1, to the highest, Level 6. Students with proficiency within the range of Level 1 are likely to be able to successfully complete tasks that require that level of knowledge and skills, but are unlikely to be able to complete tasks at higher levels. Scores below Level 2 suggest that students' skills are insufficient to meet the challenges of adult life. This lack of mathematics skills and knowledge is usually referred to as functional mathematical illiteracy. Students who perform below Level 2 often face severe disadvantages in their transition into higher education and the labour force (OECD, 2013a).

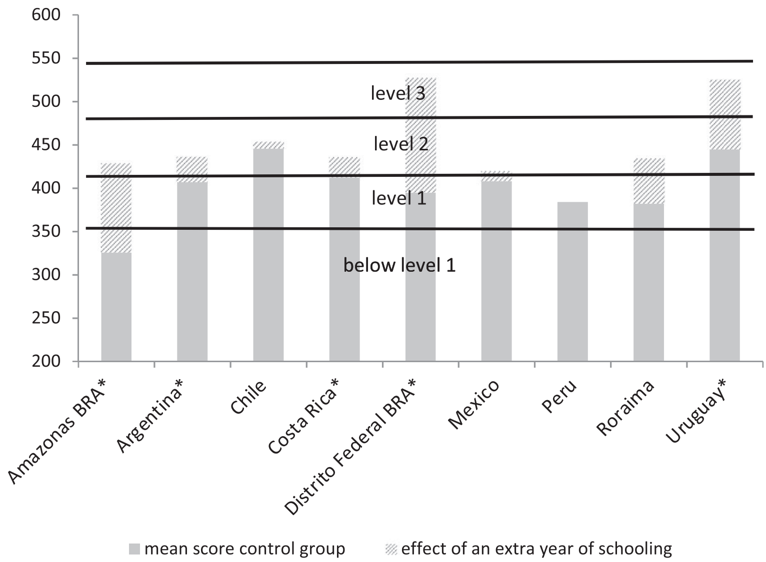

Figure 5. Mean score and the contribution of an extra year of schooling. Fuzzy RD estimates for compliers in the lower grade. Source: Authors' own estimations based on PISA 2012 data bases.
Notes: (a) *statistically significant. (b) Estimates using a one month bandwidth. (c) Controls include gender, preschool attendance, and family and school socioeconomic level. (d) Proficiency Level 1: scores higher than 358 but lower than or equal to 420 points; Level 2: scores higher than 420 but lower than or equal to 482 points; Level 3: scores higher than 482 but lower than or equal to 545 points.

All Latin American countries participating in PISA 2012 have an average performance in mathematics that corresponds to proficiency Level 1, except Chile where the mean score corresponds to Level 2. This illustrates the degree of difficulty countries in the region face in providing their youngsters with a minimum level of competencies. Even though compliers perform better than an average student, their skills and knowledge are still too low. Figure 5 shows the mean score of compliers in the control group joint with the estimated (fuzzy) effect of an extra year of schooling.[27] In most countries, mathematics skills of compliers in the 10th grade (or the 9th in Mexico and Costa Rica) correspond to Level 1. In the Brazilian state of Amazonas the situation is even worse, because an average complier in grade 10 does not even reach that level. On the other hand, compliers attending grade 10 in Chile and Uruguay manage to overcome, on average, the threshold to reach Level 2.

As Figure 5 shows, the contribution of an extra year of schooling is substantial in terms of students' skills and knowledge. In most cases, the effect is large enough to raise mathematics skills to the next proficiency level and beyond. After an additional year of schooling, compliers in the 10th grade in Argentina, or in the 9th grade in Costa Rica, would acquire the extra skills needed to move from proficiency Level 1 to Level 2. Students in grade 10 in Amazonas would also reach Level 2 but starting from a lower performance (below Level 1). The only two cases that would attain Level 3 are Uruguay and Distrito Federal in Brazil. In Mexico, Peru and Chile there would be no significant effect of an extra year of schooling in terms of average proficiency levels.

Results in this section suggest that an additional year of education at the age of 15 provides students in Latin America with new skills and knowledge needed to face adult life challenges. In terms of the substantial contribution to mathematics skills and knowledge, the results indicate that an extra year of schooling at this age helps to avoid functional illiteracy of many youngsters in the region. Moreover, this finding highlights the social and economic costs of the high dropout rates in the upper secondary school in most Latin American countries. Except in Chile and Uruguay, an average student who drops out in the 10th grade (9th in Costa Rica and Mexico) leaves school as a functional illiterate, without the most essential mathematics skills she will need in the labour market in particular and, in general, in her adult life. Provided the high number of students in this situation in Latin America, the cost for the society as a whole should be far from negligible.

The lack of data linking skills and knowledge with wages in the region makes it impossible to obtain a rigorous estimate of the cost of school-dropout in terms of productivity losses. However, a simple computation using the results in this section can give us a sense of the magnitude of this cost in the lower grade under analysis. Hanushek, Schwerdt, Wiederhold, and Woessmann (2013) use data for 22 developed countries that participated in the OECD Survey of Adult Skills and estimate that the return to a standard deviation in mathematics skills and knowledge is at least an 18 per cent increase in hourly wages. We use this lower bound to translate our estimated effect of an extra year of schooling on mathematics skills and knowledge into an individual earning loss. Based on data from national household surveys, we impute the estimated earning loss to all the individuals who dropped out school in the 10th grade (9th in Mexico and Costa Rica), and then estimate an annual cost for society that ranges from 0.04% of the GDP in Mexico to 1.65% in Brazil (see Appendix 3 for details). Despite the oversimplification of this exercise, it helps to highlight the potential benefits of adopting policies aimed at fighting the high dropout rates in upper secondary school the region.

5. Implications for the PISA sample design

It has been pointed out that certain imbalances in PISA samples due to educational practices that differ across countries may lead to unfair international comparisons. For instance, OECD (2010) suggests taking into account the differences in repetition rates between countries when assessing their relative performance. In the same way, the results that we find in this paper show that the sample design of PISA can also cause imbalances between countries that should be taken into account.

The fact that school entry cut-off dates vary across countries is mostly ignored in PISA sample design. Indeed, regardless of the cut-off date, PISA defines a fixed six month window (from March to August) and each country chooses a particular month within that time-window for the implementation of the survey. These two facts combined (fixed time window and different cut-off dates) cause the sample shares of students in the lower and upper modal grades (usually the 10th and 11th in Latin America) to significantly differ across countries. Since – as we have already shown – an additional year of schooling has a considerable effect in PISA test scores, such differences in the composition of samples affect the global ranking of countries, imposing limitations on international comparisons. A straightforward recommendation for survey development is therefore to consider the school entry cut-off dates when designing the sample or at least to make the corresponding adjustment of average scores for the purposes of international comparisons.

Considering the school entry cut-off dates in the design of the sample to attenuate some of the above mentioned biases could be done by simply adjusting the implementation dates of the survey. Nowadays, PISA has to be conducted in a 42-day period within the time-window comprised between 1 March and 31 August of a particular year. An extension of this testing period beyond March and August to take into account the school entry cut-off dates would allow a different cohort of 15-year-old students to enter the sample. Therefore, it is possible to define an implementation date for each country that guarantees that all compliers attend the same school year in every country. This could make the mean score comparison across countries fairer than it is today.

Table 6 shows the composition of the samples for the Latin American countries that participated in PISA 2012, and alternative counterfactual samples that would have resulted from changing the implementation dates to get a balanced composition of samples across countries. Of course, the actual samples suffer from an imbalance in terms of the school year compliers attend. For instance, in Argentina, only one out of the 12 monthly cohorts in the sample (the compliers born in June 1996) was attending the upper modal grade (grade 11) when PISA 2012 was carried out during August of that year. Instead, there were three out of the 12 monthly cohorts in the Peruvian sample (the compliers born between May and July 1996) attending grade 11 in July 2012, when the survey was implemented. With the alternative implementation dates, a perfect balance in terms of both students' age and the theoretical grade attended by compliers is achieved, i.e. all the 15-year-old students should be attending the tenth school year if they follow the normal school year progression.

In order to illustrate this point, we perform a simple simulation exercise that consists on moving back (forward) the implementation date (the fake dates are those in Table 6) while pretending that each student in the sample was born after (before) her actual birthday. In other words, we move back the implementation date to a later month, and proceed as if the students whose actual birthday is before the cut-off date, were born after that date in the alternative sample, thus having one less year of schooling and, as a consequence, a lower average PISA score. Similarly, when in the simulation we move the implementation date forward to an earlier month, we also proceed as if those students whose actual birthday was after the cut-off date, were born before that date in the alternative sample, thus having an extra year of schooling, and a higher average PISA score.[28] Figure 6 shows the results of this simulation. In spite of being merely illustrative, this exercise highlights how global rankings could be affected. In this particular setting, some countries could gain in the international comparison if PISA achieved more balanced sample composition across countries. For instance, the gap between Chile and Costa Rica shrinks in our simulation, which is in line with results from other international student assessments.[29]

6. Final remarks

This paper was aimed at contributing to the understanding of the relationship between skills formation and schooling in Latin America. To that end, we estimated the causal effect of an extra year of schooling on mathematics skills and knowledge for seven Latin American countries that participated in PISA 2012 (Argentina, Brazil, Chile, Costa Rica, Mexico, Peru and Uruguay). Our strategy of identification exploited exogenous variation in students' birthdates around the school entry cut-off date using a Regression Discontinuity (RD) design. Both sharp and fuzzy RD approaches were applied to take into account the possibility of imperfect enforcement of school entry rules.

Table 6. Actual and alternative sample designs using PISA 2012 as reference.

	Actual sample design (PISA 2012)						Alternative sample design (PISA 2012)				
			Cohort born:		School year for a complier born:				Cohort born:		
Country	Implementation date	Cohort in the sample	Before cut-off date	After cut-off date	Before cut-off date	After cut-off date	Implementation date	Cohort in the sample	Before cut-off date	After cut-off date	School year for compliers
Argentina	August 2012	06/96–05/97	06/96 (1 month)	07/96–05/97 (11 months)	11	10	September 2012	07/96–06/97	– (0 months)	07/96–06/97 (12 months)	10
Brazil*	March 2012	01/96–12/96	01/96–06/96 (6 months)	07/96–12/96 (6 months)	10	9	September 2011**	07/95–06/96	07/95–06/96 (12 months)	– (0 months)	10
Chile	July 2012	05/96–04/97	05/96–06/96 (2 months)	07/96–04/97 (10 months)	11	10	September 2012	07/96–06/97	– (0 months)	07/96–06/97 (12 months)	10
Costa Rica	May 2012	03/96–02/97	03/96–10/96 (8 months)	11/96–02/97 (4 months)	10	9	January 2012	11/95–10/96	11/95–10/96 (12 months)	– (0 months)	10
Mexico	March 2012	01/96–12/96	01/96–08/96 (8 months)	09/96–12/96 (4 months)	10	9	November 2011	09/95–08/96	09/95–08/96 (12 months)	– (0 months)	10
Peru	July 2012	05/96–04/97	05/96–07/96 (3 months)	08/96–04/97 (9 months)	11	10	October 2012	08/96–07/97	– (0 months)	08/96–07/97 (12 months)	10
Uruguay	July 2012	05/96–04/97	05/96 (1 month)	06/96–04/97 (11 months)	11	10	July 2012	05/96–04/97	– (0 months)	05/96–04/97 (12 months)	10

Notes: * Primary school entry age in Brazil is 6 years, but it was 7 years for the cohort of students evaluated in PISA 2012. Therefore, compliers in this sample were in their 10th and 9th school year.
** The implementation date would have been September 2012 if the cohort of students in the PISA 2012 sample would have entered primary school at the entry age that is currently in force in Brazil.

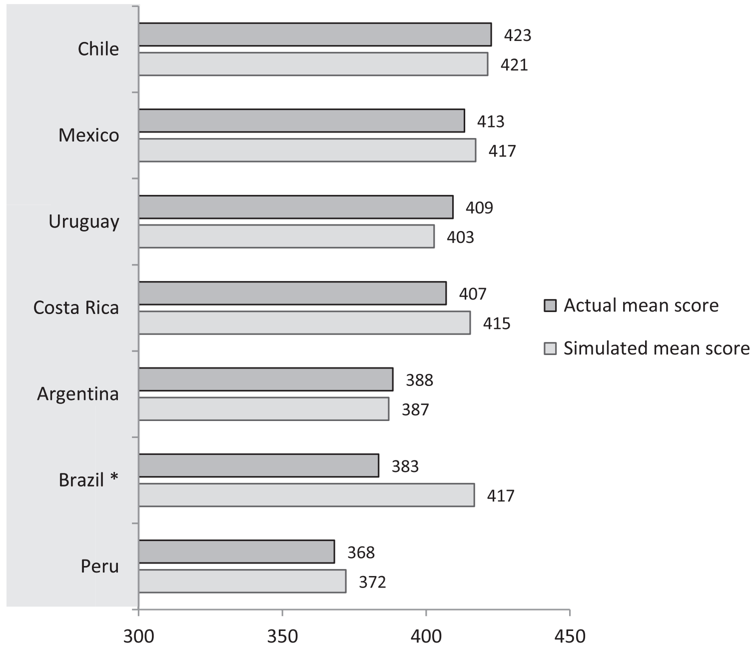

Figure 6. Actual and simulated mean score in mathematics if PISA considered school-entry cut-off dates in the sample design. Latin American countries in PISA 2012.
Notes: (a) For those countries where the proposal is to bring forward the implementation date (Brazil, Costa Rica, and Mexico), simulated mean scores were obtained by increasing the plausible values of each student who was born after the school-entry cut-off date in the estimated effect of a school year on mathematics score (last column in Table 5). For those countries where the proposal is to postpone the implementation date (Argentina, Chile, Peru and Uruguay), simulated mean scores were obtained by decreasing the plausible values of each student who was born before the school-entry cut-off date in the estimated effect of a school year on mathematics score (last column in Table 5). (b) *In Brazil, results are restricted to Amazonas, Distrito Federal and Roraima.

Our results suggest that the contribution of an extra year of schooling in Latin America is substantial in terms of students' skills and knowledge. The estimated effect of an additional year of schooling at age 15 on mathematics proficiency reaches the 113 PISA points in some states in Brazil (representing almost a 30% increase in mean scores between the lower and upper modal grades, which in the region are usually the 10th and 11th grades), and it is also large in other Latin American countries: 80 points in Uruguay (18%), 29 points in Argentina (7%), and 23 points in Costa Rica (6%). While the effects in Chile and Peru are never statistically significant, and those of Mexico depend on the specification chosen, the size of the effect is large in terms of the contribution to skills and knowledge for those countries where we do find statistically significant effects. Additionally, based on these results, we discuss how the PISA global ranking of countries may be affected by the grade imbalances arising from not considering the school entry cut-off dates in the design of the sample.

Our findings have strong implications in terms of the cost of school dropout in Latin America. The rate of youngsters leaving school in upper secondary education is relatively high in the region. Except for Chile and Uruguay, an average student who drops out school in the 10th grade (9th in Costa Rica and Mexico) has a set of mathematical knowledge and skills that is insufficient to meet the challenges of adult life. Since these youngsters often face severe disadvantages in their transition into the labour force (OECD, 2013a), it is natural to wonder whether things would have been different if they had stayed one more year at school. Our results show that an extra year of schooling at this age helps to avoid mathematical illiteracy of many youngsters in the region. This suggests not only that the high dropout rates imply high costs in terms of knowledge and abilities lost, but also that school has a lot to provide that may help young adults in their transition from school to work. In that sense, the recent extension of compulsory secondary education in several Latin American countries (e.g. Argentina in 2006, Uruguay in 2008, Brazil in 2009, Costa Rica in 2011 and Mexico in 2012) should be viewed as a policy that goes in the right direction. The mechanisms through which compulsory education laws could effectively alter school attendance rates by themselves are nonetheless limited, and other policies, such as CCT programmes could help to enforce these laws (Edo, Marchionni, & Garganta, 2017).

Moreover, our results differ from the typical estimates obtained from simple regressions or multilevel models, in particular from those published by the OECD in their dissemination documents, which are widely used in public policy discussions. Since the standard estimates compare students who are less homogeneous than in our RD design, we think that our strategy is better suited for the task of estimating the contribution of an additional year of schooling. Although our results are local in nature, a comparison of the OECD (2013c) estimates with ours reveals that the effect of schooling for Brazil and Uruguay could be considerably underestimated by the OECD; while the effects on Mexico, Peru and Chile could be largely overestimated.[30] More importantly, the traditional estimates would be masking a considerable amount of heterogeneity across countries, since our estimates range from effects that are statistically non-different from zero to 1.07 and 1.42 standard deviations in Uruguay and Brazil, respectively. Therefore, our paper calls for a revision of the strategies usually employed to estimate the effect of schooling on skills and knowledge.

A word of caution is needed before we end. Despite we find large returns of schooling at age 15, these returns represent gains on the most elementary mathematics skills and knowledge, i.e. those corresponding to the lower proficiency levels in PISA. This piece of evidence stresses the need to pay attention to the knowledge and abilities that are taught in previous school years – preschool, primary and lower secondary education, when students should have learnt these basic abilities. In this sense, policies that focus either on improving the transmission of knowledge and the development of cognitive skills during primary education, or even earlier, encouraging investments in early childhood development (e.g. preschool education, child care services and nutrition) are key to close the substantial gap that exists between Latin American countries and the most successful educational systems in the world.

Notes

1. Specifically, the target population is defined as students aged between 15 years and 3 months to 16 years and 2 months.
2. Although Colombia also participated in PISA 2012, we excluded it from the analysis because we cannot apply our identification strategy for this case. See note 13 for more details. All country samples are representative at the national level, but in Brazil and Mexico samples are also representative at the sub-national level. In Argentina, separate results for the city of Buenos Aires can also be provided.

3. Imbens and Lemieux (2008) review some of the practical and theoretical issues concerning RD designs.
4. Of course, one important difference between the two groups is the school starting age, which can affect long-term achievement. We address this point in Section 4, where we discuss the international evidence on this effect and the implications on the interpretation of our results.
5. Note that in the sharp design the denominator in Equation (2) equals 1.
6. For more detail see Hahn, Todd, and Klaauw (2001) and Imbens and Lemieux (2008).
7. Despite high order (third, fourth, or higher) polynomials were typically employed in the RD literature, their use has been recently discouraged by Gelman and Imbens (2017).
8. As it will become clearer later, the small number of data points that are available either before or after the cut-off discourages the use of higher order polynomials.
9. An alternative to the standard RD approach adopted here is the local randomisation framework (see Cattaneo et al., 2015, 2017; Cattaneo, Idrobo, & Titiunik, 2018; Cattaneo, Titiunik, & Vazquez-Bare, 2016, 2017; Sekhon & Titiunik, 2017). While this may be a useful alternative when the running variable is discrete, it relies on stronger assumptions. Therefore, we prefer to maintain the standard framework and deal with the discretisation bias following Dong (2015) and Dong and Yang (2017).
10. This is not the case in other cross-country student assessments such as the international TIMSS or PIRLS, or the Latin American PERCE, SERCE and TERCE, which evaluate students on a particular school year instead of a particular age range.
11. The grade attended by a complier in PISA samples depends not only on the school entry age and the cutoff date, but also on the beginning of the school year and the date in which PISA was implemented.
12. This is because PISA 2012 was applied at the end of the previous school year in Mexico while in Costa Rica children enter primary school later than in the rest of the countries. Also, even though compliers in Brazil are in grades 11 and 10, they are actually attending their tenth and ninth school year, respectively, since the cohort participating in PISA 2012 entered primary school at the age of 7, while nowadays primary education starts at the age of 6 in this country.
13. The situation is even more complex in Colombia, which led us to leave it out of the analysis. Two different school calendars are used in this country (Calendar A and Calendar B) and schools are free to choose between them. Moreover, schools can apply different cut-offs (or no cut-off at all) but we do not observe the cut-off applied to each student, thus we are unable to apply our identification strategy for Colombia.
14. Also, there can be students skipping grades and therefore promoting faster than the normal rule, but grade advancement is very rare in the region.
15. The scores in PISA are reported in a standardised scale with an average score of 500 points among OECD countries and a standard deviation of 100, meaning that about two-thirds of students across OECD countries score between 400 and 600 points.
16. As we will see later, this limits the possibility to use bandwidths wider than one month to the left of the cutoff line for these two countries.
17. Another effect related to age is the so-called 'age at sitting test' effect: when exams are taken at a fixed date for a given school year, some students sit them up to a year older than others. Several studies find that this effect explains why older students perform significantly better compared to their younger classmates when the age at sitting test differs in almost a full year (Black et al., 2011; Crawford, Dearden, & Meghir, 2010). However, we believe that this effect is not so relevant in our case (or at least not so relevant as to compensate the 'school starting age' effect) since the age of the students born on either side of the cut-off differs in a couple of months only.
18. The only exceptions are Argentina and Uruguay, where there is only one cohort of students born before the cutoff date and therefore a unique one-month window is used at the left of that threshold.
19. Covariates enter in an additive-separable, linear-in-parameters way, and the estimation model does not include treatment-covariate interactions or centering, as recommended in Calonico, Cattaneo, Farrell, and Titiunik (2017).

20. From a state-by-state analysis, we conclude that the effect for Brazil is driven by Distrito Federal and Amazonas, while results are never statistically significant in Roraima. Estimates by state are available upon request.
21. Specifically, we used the census of population 2010 in Argentina, the *Pesquisa Nacional por Amostra de Domicílios* (PNAD) in Brazil 2012, and the census of population 2011 in Uruguay. These sources provide a measure of both school attendance and month of birth for 15-year-old students. In all cases, the differences in enrollment rates before and after the cut-off date with the one-month and two-month bandwidths are small (around one percentage point) and not statistically significant. Results are available upon request.
22. Specifically, we performed the test for all the possible bound coefficients k and could not reject the null hypothesis that implies no manipulation for any k in any country, except in Mexico where no rejection requires assuming a greater curvature of the probability mass function at the threshold (i.e. k must be greater that 0.164 to avoid rejection at 5% of significance). Even though Frandsen (2017) recognises that 'a smaller k leads to a more powerful test, but may also detect manipulation when none is present', we still call for a cautious interpretation of results in Mexico.
23. We should not be alarmed by a few significant differences in these tables since some of them will be statistically significant by pure random chance (Lee & Lemieux, 2009). Assuming that tests are independent, we would expect to find a significant difference in 1 out of 20 covariates at the 5% level (Dunning, 2012).
24. We have also paid attention to the magnitude of the differences in covariates beyond their statistical significance. After careful examination, we did not find any systematic imbalance pattern in any of the variables for the two bandwidths considered (one or two months around the cut-off date), and this is true for all the countries under analysis.
25. Results separated by state in Brazil are available upon request.
26. Estimates for Distrito Federal increase dramatically from the sharp to the fuzzy analysis.
27. For a better understanding of the results, we separate estimates for Brazil by state. Results for Roraima are never significant.
28. Specifically, in the simulation we postpone the implementation dates for Argentina, Chile, Peru and Uruguay. For these countries, the simulated mean scores are obtained by subtracting from the plausible values of each student born before the school-entry cutoff date, the estimated effect on the mathematics score of one extra year of schooling (last column in Table 5). For the rest of the countries (Brazil, Costa Rica and Mexico), we simulate earlier implementation dates. The simulated mean scores are obtained by adding to the plausible values of each student born after the school-entry cutoff date, the estimated effect of a school year on mathematics score (last column in Table 5).
29. In other cross-country student assessments that evaluate students on a particular school year (such as the third and sixth graders in the Latin American PERCE, SERCE and TERCE), Costa Rica performs much better in comparison to the other countries in the region, which suggests that this country could be seriously affected by the PISA sample design.
30. Estimates reported in OECD (2013c) based on PISA 2012 are 25 PISA points in Peru, 26 in Mexico and Costa Rica, 31 in Argentina and Brazil, 33 in Chile, and 39 in Uruguay (see Table A1.2 in OECD, 2013c).

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