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THE EFFECT OF SCHOOLING ON SKILLS AND KNOWLEDGE IN LATIN AMERICA. EVIDENCE FROM PISA

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DEVELOPMENT BANK OF LATIN AMERICA

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ABSTRACT

In this paper we estimate the causal effect of an extra year of schooling on mathematics skills and knowledge for the eight Latin American countries (Argentina, Brazil, Chile, Colombia, Costa Rica, Mexico, Peru and Uruguay) that participated in PISA 2012. To that end we exploit exogenous variation in students' birthdates around the school entry cutoff date using a Regression Discontinuity (RD) design. We apply both sharp and fuzzy RD approaches to take into account the possibility of imperfect enforcement of school entry rules. Our estimates suggest strong effects 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. We compare these results to that from high-performing countries.

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EL EFECTO DE LA ESCOLARIDAD SOBRE LAS HABILIDADES Y LOS CONOCIMIENTOS EN AMÉRICA LATINA. EVIDENCIA A PARTIR DE PISA

Marchionni, Mariana
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CAF - Documento de trabajo N° 2015/08
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RESUMEN

En este trabajo estimamos el efecto causal de un año adicional de escolaridad sobre las habilidades y el conocimiento en matemáticas en los ocho países de América Latina (Argentina, Brasil, Chile, Colombia, Costa Rica, México, Perú y Uruguay) que participaron en PISA 2012. Para ello, explotamos la variación exógena en las fechas de nacimiento de los estudiantes alrededor de la fecha de corte de entrada en la escuela utilizando un diseño de regresión discontinua (RD). Aplicamos enfoques de regresión discontinua "sharp" y "fuzzy", para tener en cuenta la posibilidad de cumplimiento imperfecto de las reglas de entrada a la escuela. Nuestras estimaciones sugieren fuertes efectos de un año adicional de escolaridad en las puntuaciones de la prueba PISA para estudiantes de 15 años de edad, que tienen consecuencias directas en términos de sus habilidades y conocimientos. Comparamos estos resultados a los de los países de alto rendimiento.

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The Effect of Schooling on Skills and Knowledge in Latin America. Evidence from PISA*

Mariana Marchionni ** and Emmanuel Vazquez ***

Abstract

In this paper we estimate the causal effect of an extra year of schooling on mathematics skills and knowledge for the eight Latin American countries (Argentina, Brazil, Chile, Colombia, Costa Rica, Mexico, Peru and Uruguay) that participated in PISA 2012. To that end we exploit exogenous variation in students' birthdates around the school entry cutoff date using a Regression Discontinuity (RD) design. 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 apply both sharp and fuzzy RD approaches to take into account the possibility of imperfect enforcement of school entry rules. Our estimates suggest strong effects 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. We compare these results to that from high-performing countries.

Keywords: skills, PISA, Latin America, Regression Discontinuity Design

JEL codes: I21, J24

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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 and Kautz, 2012), cognitive abilities play a key role when pursuing higher level education or when entering the labor market, and are essential for future success (Hanushek and 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 labor prospects and earnings.

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 eight Latin American countries (Argentina, Brazil, Chile, Colombia, 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. Moreover, the effect can also inform on the contribution of the curriculum at what it is the end of formal education for many youngsters in Latin America: even though the limit for compulsory education at the age of 15 was recently extended in most countries in the region, dropout rates for 15-year-olds are still high compared to younger secondary school students.

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 cutoff 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 enroll 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 cutoff, we can isolate the causal effect of an extra year of schooling on test scores by comparing the performance of students born just before and just after the school cutoff date. We restrict the analysis to students who have never retained a grade; therefore, what we identify is the effect of an extra year of schooling in mathematics skills and knowledge of non-repeaters. Also, we apply both sharp and fuzzy RD approaches to take into account the fact that, even after dropping the repeaters, there are still students attending a different grade from the one corresponding to their age.

We also explore whether the contribution of an extra year of schooling on mathematics skills differs between Latin America, characterized by a poor mean performance in PISA, and high-performing countries. Of course, part of the gap between the two groups is explained by variables beyond the educational system, such as economic development. But cross-country comparisons of the effect on skills of an extra year of schooling may provide a different perspective on the contribution of the curriculum being taught to 15-year-olds in the region.

The rest of the paper is organized 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 compares the results found for Latin American countries to those of high-performing countries. Section 6 concludes.

2. Data and methodology

We use data from the Programme for International Student Assessment (PISA) conducted in 2012. PISA is a program 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 standardized tests administered to students at randomly selected schools in every participating country. In addition to the tests, the program collects information about students and schools using a background questionnaire for students and school principals.

Our analysis focuses on the eight Latin American countries that participated in PISA 2012: Argentina, Brazil, Chile, Colombia, Costa Rica, Mexico, Peru and Uruguay, where a total of 90,799 students (representing more than 5.5 million) were evaluated.[2] All these countries ranked among the worst 15 in mathematics out of the 65 economies participating in PISA

[1] Specifically, the target population is defined as students aged between 15 years and 3 months to 16 years and 2 months.

[2] All country samples are representative at the national level, with the exception of Brazil, Colombia and Mexico where samples are also representative at sub-national level. In Argentina, separate results for the city of Buenos Aires can also be provided.

2012. As a benchmark for comparison we use six high-performing countries/economies that ranked among the best 15: Shanghai-China, Hong Kong-China, Taiwan, South Korea, Finland and Estonia, where a total of 34,534 students were evaluated. The justification for this choice is that these high-performing economies have strict school enrolment rules, which is a prerequisite for applying the methodology explained below.

We estimate the causal effect of an extra year of schooling on PISA test scores by exploiting exogenous variation in students' birthdates around the school entry cutoff 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 cutoff date must enroll 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 cutoff.

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 different questions based on data from PISA. 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. As for assessments on the effect of school years on PISA test scores, there are the works of Frenette (2008) for Canada using PISA 2000, Benton (2014) for England using PISA 2000 and 2003, Khaw and Wong (2012) for Singapure based on PISA 2000, and Lau and Wong (2013) for a group of high-performing countries based on PISA 2009. To the best of our knowledge, there is no study that investigates the effect of schooling on skills and knowledge in Latin America.

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 cutoff date, argue that these two groups have the same average characteristics except from the fact that those who were born just before the cutoff date have an extra year of schooling, and finally attribute the difference in mean test scores to the extra year of schooling. 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 cutoff date. Formally, the sharp RD design estimates equation (1):

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

[3] Imbens and Lemieux (2008) review some of the practical and theoretical issues concerning RD designs.

where Y_i is the test score of student *i* and B_i her date of birth; B_0 is the cutoff 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 cutoff, equation (1) equals the average treatment effect at the cutoff.

One problem with the sharp RD design is that in our data there are some students attending a different grade from the one that corresponds to their date of birth. In other words, there is not a one-to-one correspondence between the date of birth and the grade a student attends. 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 11^{th} and 10^{th} grade, respectively. If some students are enrolled in other than these two grades it must be due to grade repetition or to other (unobserved) reasons such as early or late primary school enrolment. We refer to the first group as *repeaters* and the second group as *noncompliers*.

For repeaters, school year clearly depends on (potential) performance. Because of the age range covered by PISA, the proportion of repeaters is much larger in the group of students who attend the lower of the two grades of interest and their inclusion in the sample would bias our estimates. Therefore, our analysis considers only those students who never retain a grade, i.e. non repeaters. Moreover, the existence of noncompliers suggests that there is some room to manipulate school entrance rules for (unobserved) reasons that may or may not be related to performance. As a first step to deal with this issue, we further 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 cutoff date using a sharp RD approach.

The exclusion of noncompliers may lead to selection bias in a sharp RD design, so the next step is to incorporate this group into the analysis, which causes the probability of treatment (having an extra year of schooling) to not change from 0 to 1 at the cutoff 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 cutoff and the change in the proportion of students treated also at the cutoff. Formally, in a fuzzy RD design we can recover the treatment effect by estimating equation (2):

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

In a fuzzy design, equation (2) equals the average treatment effect at the cutoff for those induced to change treatment status at that discontinuity point.[4] Note that in the sharp design the denominator in equation (2) equals 1.

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 cutoff date.

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

where B_i is re-centered subtracting the cutoff 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 cutoff point. Typical specifications include mean comparison, local linear and polynomial regressions, and low order polynomials.[5]

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. With such data, it is pointless to consider different functional forms to weight observations around the cutoff date, so we simply adopt a mean comparison strategy and test the robustness of our results by using alternative bandwidths.[6]

The fuzzy RD can be implemented using two-stage least squares (Hahn, Todd and van der Klaauw, 2001). Formally, the fuzzy RD design can be summarized by a system consisting of the standard model in equation (3) and equation (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 cutoff date.

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[4] This is true if the following assumptions hold: "monotonicity" (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" (B_i crossing B_0 does not impact Y_i except through its effect on the receipt of treatment). For more detail see Hahn et al. (2001) and Imbens and Lemieux (2008).

[5] 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 (2014).

[6] 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 (Lee and Card, 2008). Our assumption is that there is not an effect of age on test scores for students who were born within a two-month or four-month period. This is consistent with the previous work of Strom (2004), who does not find an age effect in PISA for students born in contiguous quarters.

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

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 cutoff 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 as a robustness check.

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 the eight 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, then dropping this group of students should not bias our estimate. In that case, the comparison of the estimated average score of those who were born just before and just after the cutoff date is the local average treatment effect on the compliers. To avoid an assumption of this kind, we complement the analysis with a fuzzy RD design for all non-repeaters.

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

Most educational systems have a unique cutoff 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 cutoff 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.[7]

Nevertheless, enforcement of school entry rules is not perfect in the region and grade repetition 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

[7] 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.

cutoff date in force when they entered primary school.[8] 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 cutoff date, respectively.[9] Table 1 summarizes 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 the Appendix 1.

The rest of the students in the PISA samples attend a different grade from that corresponding to their age given the school entry rules. On the one hand, there are some students retaining one or more grades (repeaters).[10] On the other hand, since enforcement of school entry policies is not perfect, some students are able to enter primary school before or after they are supposed to (early or late enrollers). 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, non-compliance with the law is higher in the upper schooling grade (early enrollers), while the proportion of repeaters is higher in the lower schooling grade. The latter result is a consequence of the target population defined in the PISA sample, which makes it very difficult to find 15 years old repeaters in the upper grade.

Table 3 shows mean scores in mathematics for the upper and lower grades of interest.[11] 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 a consequence of the higher proportion of repeaters, which is the group with the lowest mean test scores, in the lower grade. The presence of repeaters in the sample creates a typical problem of selection that could bias our estimates of the effect of an extra year of schooling on scores, and this is the reason why we exclude repeaters from our analysis. We will return to this point later.

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[8] 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.

[9] 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.

[10] Also, there can be students skipping grades and therefore promoting faster than the normal rule, but this is very rare in the region.

[11] The scores in PISA are reported in a standardized 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.

Yet, we cannot attribute the difference in performance to the extra year of schooling alone, not even for compliers: students attending one grade or the other may differ in other dimensions related to performance, such as maturity, experiences and other observable and unobservable characteristics. In the next section we deal with this issue and estimate the causal effect of the extra school year using an RD design.

Before concluding this section, the cases of Brazil and Colombia deserve a separate discussion. The cutoff 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 three Brazilian states where data reveals that a uniform cutoff 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. In Colombia, the situation is even more complex. 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 cutoffs (or no cutoff at all) but we do not observe the cutoff applied to each student, thus we are unable to apply our identification strategy in Colombia. Nonetheless, we show the results when assuming the rules that were more popular when the students participating in PISA 2012 entered primary education: calendar A and cutoff in March 31. We expect to find no effect of an extra school year on test scores in Colombia.

4. Results from the RD design

4.1. Preliminary evidence based on a sharp RD design

This section 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 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 noncompliers, which we do in the next subsection where a fuzzy RD approach is applied.

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 cutoff 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 cutoff date and who were attending the higher of the two grades of interest when PISA 2012 was implemented. For instance, the cutoff 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 skip 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 cutoff date, thus attending the lower of the two grades of interest, which is the 10th grade in the Argentinean 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 cutoff line obeys to the conjunction of three elements that vary across countries: primary school entry age, cutoff date, and date of implementation of PISA 2012. Returning to the Argentinean 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 cutoff line is Uruguay.[12] In the rest of the countries there is more balance in the number of cohorts before and after the cutoff date.

Figure 2 shows that mean test scores jump at the cutoff 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 cutoff estimates the causal effect of that extra year of schooling. Here we claim that there are a priory reasons to think that students characteristics are balanced on both sides of the cutoff date. In the first place, even though parents have some control over the date of birth of their child, it is clear that the control is not precise around the cutoff date. Moreover, even with no strong enforcement of the law, it is not clear whether treatment is desirable or not: while some parents may prefer that their children enter primary education sooner and 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. We postpone a more rigorous analysis to sustain this point until next subsection.

At a first glance, the jump in mean scores at the cutoff 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. Against our expectations, there is a non-negligible jump in test scores in Colombia, even though the threshold we are assuming was not the actual cutoff date for school eligibility, as we discussed in the previous section. This counterintuitive result for Colombia vanishes when we incorporate noncompliers into the sample, which we do in the next subsection.

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

[12] 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

threshold.[13] 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 socioeconomic level.

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 1-month bandwidth, the estimated effects based on the sharp RD design range from 81 points in Brazil to 8 points (though not statistically significant) in Peru.[14] Between these two extremes is Uruguay with 47 points, and then Argentina, Costa Rica and Mexico with 28, 21 and 15 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 in Costa Rica and Mexico), the estimated (sharp) effect represents an increase of 23% in Brazil, 10% in Uruguay, 7% in Argentina, 5% in Costa Rica and 4% 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 (8 points). Consistent with Figure 2, the estimated sharp effect for Colombia is significant, but this result is not robust to noncompliance as we will see in the fuzzy analysis.

4.2. Results from a fuzzy RD design

The problem with the sharp RD approach is that excluding noncompliers may lead to bias in our estimates of the effect of an extra year of schooling on mathematics skills. Therefore, we incorporate noncompliers 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 cutoff 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 cutoff 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. On the contrary, there appears to be no discontinuity in Colombia, which is consistent with the abovementioned lack of a single school entry rule in this country.

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[13] 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.

[14] 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.

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 cutoff date. But the presence of noncompliers suggests that there is some room to manipulation. Although the imprecision of control cannot be proved and will often be nothing more than a conjecture, it has clear observable predictions (Lee and 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 cutoff date. Figure 4 shows the distribution of birthdates in the sample that includes both compliers and noncompliers. As expected, the distributions are relatively uniform in most of the countries, with no clear discontinuity at the cutoff date. The main exception is Argentina, where there is a significant drop in the density when crossing the threshold.[15] Peru and Chile also show some discontinuity at their cutoff dates, but these changes are not larger than those at other points in the support.[16]

Discontinuities in the distribution of birthdates around the cutoff date may be due to differences in the rates of repetition or school abandonment between the two grades of interest. From our discussion in the previous section, we know that repetition rates are higher in the lower grade. Therefore, the exclusion of repeaters from our sample may explain some discontinuity in the densities when crossing the threshold. The same would be true if students drop out school after finishing the lower of the two grades of interest, which is likely to be the case in Latin America where dropout rates in upper secondary education are much higher than in lower secondary education (SITEAL, 2015). While we exclude repeaters from our sample to avoid this particular source of selection bias, we cannot control for this other cause of bias (school dropout), which is likely to lead to overestimation of the effect of an extra year of schooling if students who get the treatment do not drop out because they have higher ability.

A natural way of assessing whether treatment is randomly assigned around the cutoff 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 skeptical 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. Table 5 defines the variables while Tables 6.1 to 6.8 report test results. Based on this evidence, we cannot reject that control and treatment groups are similar, i.e. there is

[15] This drop is not a consequence of restricting the sample to those students who never repeated a grade. The same discontinuity is observed when we use all data in the PISA 2012 sample.

[16] Although a formal test of discontinuity of the density at the threshold (e.g. McCrary 2008) would help us to assess the statistical significance of this changes, limitations in our data (basically, the fact that the exact date of birth of students is not available) prevents us from running such a test.

no covariate imbalance, in all countries for almost all the variables when using a 1-month bandwidth both before and after the cutoff date.[17] As expected, the two groups are less comparable when using a 2-month bandwidth.

So far, we have no reasons to suspect of sample selection or precise manipulation of the treatment near the cutoff date, and therefore the assignment to treatment would be as good as random at that point, at least when using a 1-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 the ratio between the change in the test scores and the change in the proportion of treated students, both at the cutoff date. To that end, we use a two-stage procedure as earlier described in Section 2. Table 7 reports the fuzzy results for different model specifications: with and without controls, and for 1-month and 2-month bandwidths. As for the sharp RD estimates, controls include gender, preschool attendance, and family and school socioeconomic level.

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. Unlike sharp estimates, results from the fuzzy analysis for Colombia are never statistically significant.

In our most preferred specification, i.e. the model with controls using a 1-month bandwidth, the estimated effect of an extra year of schooling amounts to 106 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.[18] This is a very large effect (even larger than the preliminary sharp effect), which amounts to a 30% of the mean score in mathematics for compliers attending grade 10.[19] Second in the ranking is Uruguay, where the contribution of an extra year of schooling is 72 points (16% of the mean score for compliers in the lower grade). Then it is Argentina, Costa Rica and Mexico with 30, 25 and 13 points, respectively (or 7%, 6% and 3% of the mean score for the corresponding reference group).

These 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.

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[17] 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 and 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).

[18] Results are available upon request.

[19] Estimates for Distrito Federal increase dramatically from the sharp to the fuzzy analysis.

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 labor force (OECD, 2013a).

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.[20] 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 Mexico or 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 Peru and Chile there would be no significant effect of an extra year of schooling on 15-year-olds' mathematics skills.

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

[20] For a better understanding of the results, we separate estimates for Brazil by state. Results for Roraima (Brazil) and Colombia (not shown in Figure 5) are never significant.

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 labor 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 et al. (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 percent 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 0.05% of the GDP in Mexico to 1.5% in Brazil (see Appendix 2 for details). Despite of the oversimplification of this exercise, it helps to highlight the potential benefits of adopting policies aimed at halting school dropout at age 15 in the region.

5. Comparing Latin America to high-performing countries

This section compares the results obtained in the previous section for Latin America to those for several high-performing countries, where 15-year-old students have already acquired the basic skills and knowledge that they will need in adult life. Specifically, we estimate the return of an extra year of schooling in terms of mathematical skills and knowledge in Shanghai-China (1º in the PISA 2012 ranking), Hong Kong-China (3º), Taiwan (4º), South Korea (5º), Estonia (11º) and Finland (12º). We have chosen these high-performing countries as a benchmark since they have strict school enrolment rules, which is a prerequisite for applying the RD methodology.

Figure 6 shows the PISA mean score in mathematics by month of birth for compliers in each of these countries. Again, the vertical line indicates the school entry cutoff 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 cutoff date and who were attending the higher of the two grades of interest when PISA 2012 was implemented, while the points to the right of the vertical line correspond to students born after the cutoff date, thus attending the lower of the two grades of interest. These grades are the 9th and 10th in Shanghai-China, Hong Kong-China, Taiwan and Korea, and the 8th and 9th in Finland and

Estonia.[21] At a first glance, there is a jump in mean scores at the cutoff date that ranges between 20 and 30 points in all these countries, except from Hong Kong where there appears to be no jump, and from Korea where the jump is higher, at least when using a one-month bandwidth. Table 8 presents the sharp RD estimates and confirms these findings.

Despite the sharp RD estimates restricting the sample to compliers may be a good approximation if noncompliance is small or independent of the potential scores, a fuzzy design is more appropriate when the enforcement of the law is not perfect. Figure 7 displays the proportion of students attending the higher of the two grades of interest by month of birth in each of the high-performing countries under analysis. Even though compliance is in general higher than in Latin American countries, still the probability of treatment does not change from 1 to 0 at the cutoff. The discontinuity is less pronounced in Korea, which may be explained by the fact that early (at age 5) and late school enrolment (at age 7) are allowed in certain cases (Mullis et al. 2012). As in the previous section, we also test the validity of the fuzzy RD looking at the histogram of the forcing variable (Figure 8) and the baseline covariates (Tables 9.1 to 9.6). We do not find reasons to reject the validity of this design.[22]

Table 10 shows the fuzzy RD estimates of the effect of an extra year of schooling on mathematics skills and knowledge (as measured by PISA) for the high-performing countries. The estimated effects for Estonia, Finland, Taiwan and Shanghai-China are statistically significant and slightly smaller than the sharp estimates, ranging between 15 and 20 points. In our preferred specification, i.e. the model with controls using a 1-month bandwidth, the estimated effect of an extra year of schooling is around 20 points, which represents a 4% of the mean score in mathematics for compliers attending the lower grade in Estonia, Finland and Taiwan, and a 3% for the corresponding reference group in Shanghai-China. For the other two countries we find no statistically significant effect of an extra year of schooling, which is consistent with the sharp estimates for the case of Hong Kong but not for Korea. The differences between sharp and fuzzy estimates in the latter case suggest that the large noncompliance rates among those born after the cutoff are likely to be positively associated to a higher potential score.

In order to contrast these results with those obtained for Latin America, Figure 9 shows the mean score of compliers in the control group joint with the estimated (fuzzy) effect of an extra year of schooling for the high-performing countries in our study. In general, the size of the effect is smaller in these countries than in Latin America -both in absolute and

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[21] Note that in most of the cases, 15-year-old students in Latin America attend a higher grade than their counterparts in high-performing countries.

[22] As before, we find that control and treatment groups are less comparable when using the two-month bandwidth. Moreover, there is some imbalance in the school level variables in Shanghai-China and Taiwan, which may be due just to the small number of schools that remain in our sample.

relative terms. Nonetheless, it is important to keep in mind that these marginal effects take place on completely different locations of the conditional distribution of scores. Thus, when an average student aged 15 attending the grade that corresponds to her age in Argentina, Brazil, Mexico or Costa Rica learns, for instance, to *perform actions that are almost always obvious and follow immediately from the given stimuli*,[23] an average complier of that age in Estonia, Finland and Taiwan is learning to make *interpretations sufficiently sound to be the basis for building a simple model or for selecting and applying simple problem-solving strategies*,[24] while her counterpart in Shanghai is learning to *develop and work with models for complex situations*.[25] More importantly, if these students drop out school at age 15 in any of the six high-performing countries under analysis, they will do it with the basic skills that are required to participate fully in modern society, and not as functional illiterates as in most of the studied countries in Latin America.

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 the Latin American countries that participated in PISA 2012. Our strategy of identification exploited exogenous variation in students' birthdates around the school entry cutoff 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. To gain a broader international perspective of our findings, we also estimated the size of this effect for a group of high-performing countries in PISA.

The results in this paper 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 106 PISA points in some states in Brazil (30% of the corresponding mean score), and it is also large in other countries in the region: 72 points (16%) in Uruguay, 30 points in Argentina (7%), 25 points in Costa Rica (6%), and 13 points in Mexico (3%). We do not find a statistically significant effect in Chile or Peru, while we cannot apply our identification strategy in Colombia. Nevertheless, the size of the effect is large for those Latin American countries where we do find a statistically significant contribution. This becomes very clear after we compare these results with high-performing countries and find effects that are not higher than 20 points (or 4% of the mean score of the reference group).

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[23] A skill that is associated to proficiency level 1 (OECD, 2014a).

[24] A skill that is associated to proficiency level 3 (OECD, 2014a).

[25] A skill that is associated to proficiency level 5 (OECD, 2014a).

The findings have strong implications in terms of the cost of 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 labor 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 functional 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 programs- could help to enforce these laws (Edo and Marchionni, 2015).

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. Even though the returns of an extra year of schooling for students in high-performing countries are smaller, they represent skills that are much more advanced. 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.

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Appendix 1. School entry policies in Latin America

In this appendix, we briefly describe some features of the educational systems in the Latin American countries under analysis that help us understand the control and treatment groups in our study.

Argentina

Compulsory education in Argentina starts at the age of 4 and lasts 14 years, until students finish secondary education.[26] The school year begins in late February/early March and PISA 2012 was implemented in August. Students must turn 6 years old before June 30 in order to start primary education,[27] and data reveals that the enforcement of this regulation is relatively high. Therefore, if the cohort of students in the PISA sample progressed through the system without repeating or skipping a grade, those who were born before June 30, 1996 entered primary education in 2002 and should be in the eleventh grade in August 2012, while those who were born after June 30, 1996 entered primary education in 2003 and should be in the 10th grade in August 2012.

Brazil

Compulsory education in Brazil starts at the age of 4 and lasts 14 years, until students finish secondary education.[28] The school year begins in early February and PISA 2012 was implemented in March. The Law of Guidelines and Bases of National Education (Lei de Diretrizes e Bases da Educação) guarantees teaching and administrative autonomy to the different states and municipalities in Brazil. As a consequence of this, there is considerable heterogeneity in the admission criteria to primary education in the different states. In an attempt to set a criterion that makes the cut-off date uniform across states,[29] the Basic Education Chamber of the National Education Council established in 2010 that students had to be 6 years old by March 31 in order to enter primary education.[30] However, as any other regulation, this decision does not have force of law and therefore different states are free to modify it and implement a different cut-off date, as they actually do.[31] For the cohort

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[26] The Law 26,206, passed and promulgated on December 2006, made secondary education mandatory, while the Law 27,045, passed by the Parliament on December 2014 and promulgated on January 2015, modified the Law 26,206 extending compulsory pre-primary education from one year to two years.

[27] Federal Board of Education, Recommendation No. 7, August 27, 1980.

[28] In November 2009, the Constitutional Amendment No. 59 established that basic education was mandatory from age 4 to age 17.

[29] Lima, Marilene Barbosa. Matrícula no ensino fundamental: criança com aniversário no 2º semestre. Revista Jus Navigandi, Teresina, año 17, n. 3190, 26 mar. 2012. Available in: <http://jus.com.br/peticoes/21364>.

[30] Resolução CNE/CEB nº 07/ 2010. November 14, 2010.

[31] Thus, for instance, the cut-off date is March 31 in the state of Tocantis (Resolution CEE-TO No.23/2013) and Distrito Federal (Resolution CEDF No.1/2012), April 30 in the state of Mato Grosso (Resolution CEE-

of students in our PISA sample, who were born in 1996, the most popular cut-off date by the time they entered primary education was June 30. However, data reveals that the enforcement of this cut-off date was strong enough only in a few states (namely Amazonas, Distrito Federal, and Roraima), while there are other states were data reveals that there was only some enforcement of this cut-off (Bahia, Espírito Santo, and Pernambuco), and other regions were this restriction was not operative. Considering that the students in the PISA sample affected by this cut-off progressed through the system without repeating or skipping a grade, and taking into account that what is now the first grade of primary education was not mandatory by that time,[32] those who were born before June 30, 1996 entered to what is now the second year of primary education in 2003 and should be in the eleventh grade in March 2012, while those who were born after June 30, 1996 entered to this second grade in 2004 and should be in the 10th grade in March 2012.

Chile

Compulsory education in Chile starts at the age of 5 and lasts 13 years, until students finish secondary education.[33] The school year begins in late February/early March and PISA 2012 was implemented in July. A Ministry of Education decree[34] establishes that students must turn 6 years old by March 31 of the corresponding schooling year in order to be admitted to primary education. However, the same decree allows school principals to admit students turning six after that date, as long as their birthdate is before June 30. Therefore, Chilean schools may impose different cut-off dates between March 31 and June 30, but June 30 is the most common date used in practice (McEwan and Shapiro, 2008). If the latter cut-off date is operative, and the cohort of students in the PISA sample progressed through the system without repeating or skipping a grade, those who were born before June 30, 1996 entered primary education in 2002 and should be in the eleventh grade in July 2012, while those who were born after June 30, 1996 entered primary education in 2003 and should be in the 10th grade in July 2012.

MG No. 02/2009), June 30 in the state of Minas Gerais (State Law No. 20.817/2013), and December 31 in the states of Rio de Janeiro (State Law No. 5.488/2009) and Paraná (State Law No. 6.049/2009).

[32] Before laws No. 11114/05 and 11274/06 were passed, there were only 8 eight years of primary education in Brazil, and children had to turn 7 years old before the cut-off date in order to enter first grade. After these laws were passed, the last year of pre-primary education became the first year of primary education (*Ensino Fundamenal*) and was made mandatory.

[33] The Law 20,370, promulgated on August 2009, made secondary education compulsory, while pre-primary education was not mandatory until the Law 20,710 modified the Chilean Constitution on December 2013 and made the last year of this level compulsory. However, the implementation of this last reform is gradual, and children are not denied entrance to primary education on this basis yet.

[34] Decree No. 64 (March 23, 1992), then Decree No. 171 (March 17, 2005) and now Decree No. 1718 (October 3, 2011).

Colombia

Compulsory education in Colombia starts at the age of 5 and lasts 10 years, until students finish lower secondary education.[35] There are two different school years in this country: Calendar A, which starts at the end of January/beginning of February, and Calendar B, which starts at the end of August/beginning of September. Each school can decide which calendar to implement, as long as they get approval from the National Ministry of Education,[36] but there are clear differences among regions and also between public and private schools, since Calendar A is typically implemented in the former.

The Colombian legislation does not impose a cut-off date to enter primary education. The Decree No. 1860 (1994), which regulates the General Education Law, establishes that each educational establishment defines the minimum and maximum limits of age for their students. Moreover, it also states that they are subject to the ranges determined by the corresponding territorial entity, which by Law No. 715 (2001) are granted the autonomy to plan, manage and provide the educational service. Despite the Ministry of Education made an attempt in 2006 to impose a cut-off date at the beginning of the school year,[37] its resolution was rapidly suspended by the Constitutional Court.[38] Its sentence stated that age should not be the only criteria to admit a student in the first year of mandatory education, in accordance with the Decree No. 1860 (1996).[39]

A consequence of the above mentioned particularities (i.e. the inexistence of a uniform school year and cut-off date) is that we cannot estimate the effect of a schooling year on cognitive skills and knowledge with the data that is available in PISA database.[40] Nonetheless, we show the results that emerge from using the most used calendar year (the type A) and cut-off date (March 31) by the time the cohort of students evaluated by PISA in March 2012 entered mandatory education. In this case, if the cohort of students in PISA sample progressed through the system without repeating or skipping a grade, those who

[35] The Political Constitution of Colombia (1991) and the General Education Law No. 114 (1994) establish that the last of the 3 years of pre-primary education and the 9 years of basic education (i.e. primary and lower secondary) are compulsory. Students are not forced by law to attend the two years of upper secondary education (grades 10 and 11).

[36] Decree No. 1902, published on November 28, 1969.

[37] National Ministry of Education, Resolution No. 5380, September 7, 2006.

[38] Constitutional Court, Sentence T-1030/06, December 4, 2006.

[39] Therefore, “other aspects such as personal development, regional, cultural and ethical factors must be considered in the evaluation”.

[40] Despite we can identify students in Bogotá, Cali, Manizales, Medellín and the rest of Colombia, and even differentiate between private and public schools, it is not possible to know which cutoff date (if any) applied to each student. Therefore, it is also impossible to implement a strategy that is similar to the one we used in Brazil.

were born before March 31, 1996 entered primary education in 2002 and should be in the eleventh grade in March 2012, while those who were born after March 31, 1996 entered primary education in 2003 and should be in the 10th grade in March 2012.

Costa Rica

Compulsory education in Costa Rica starts at the age of 4 and lasts 14 years, until students finish secondary education (*Educación Diversificada*).[41] The school year begins in early February and PISA 2012 was implemented in May. Nowadays, students must be 6 years and 3 month old by February 15 in the year they start primary education.[42] However, before the introduction of this regulation in 2004,[43] students had to be 6 years and 3 month old by January 31.[44] This was the cut-off date applicable to the cohort of students in our sample, and data reveals that this regulation was strongly enforced. This means that if the students in PISA sample progressed through the system without repeating or skipping a grade, those who were born before October 31, 1996 entered primary education in 2003 and should be in the 10th grade in May 2012, while those who were born after October 31, 1996 entered primary education in 2004 and should be in the 9th grade in May 2012.

Mexico

In Mexico, education is mandatory from age 3 until the end of secondary education (*media superior*), with a theoretical duration of 15 years.[45] Since 2006, students must turn 6 years old before December 31 in order to start primary education.[46] Previously, the official cut-off date was September 1,[47] and this was the threshold applicable to the cohort of Mexican students in our sample. Data reveals that the degree of enforcement of this law was moderate, provided that many students who were born between September and December could manage to enter primary school one year before they had to. It is important to take into account that Mexico has a different school calendar from most of the other Latin

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[41] In 1997, the Law 7,676 amended the article No. 78 in the Constitution, making mandatory the two years of pre-primary education. In 2011, the law 8,954 made a new amendment to this article, making mandatory the last 3 years of secondary education.

[42] Executive Decree No. 35,589, Ministry of Public Education, published on November 17, 2009.

[43] Executive Decree No. 31,663, Ministry of Public Education, published on March 8, 2004.

[44] Executive Decree No. 28,876, Ministry of Public Education, published on August 29, 2000.

[45] According to the Constitutional Reform published in the Official Journal of the Federation on November 12, 2002, the 3 years of pre-primary education are mandatory in Mexico. However, the enforcement of this law in the first year of kindergarten has been postponed (Pérez Martínez, Pedroza Zúñiga, Ruiz Cuéllar and López García, 2010). Moreover, the last 3 years of secondary education (media superior) are mandatory since the Constitutional Reform published in the Official Journal of the Federation on February 9, 2012.

[46] Amend to the article 65 of the General Law on Education, published in the Official Journal of the Federation on June 20, 2006.

[47] Article 2, Agreement No. 209, published in the Official Journal of the Federation on March 13, 1996.

American countries considered in our study. In this country, classes begin in August. Moreover, PISA 2012 was implemented on March 20. Therefore, if the cohort of students in PISA sample progressed through the system without repeating or skipping a grade, those who were born before September 1, 1996 entered primary education in 2002 and should be in the 10th grade in March 2012, while those who were born after September 1, 1996 entered primary education in 2003 and should be in the 9th grade in 2012.

Peru

Compulsory education in Peru starts at the age of 3 and lasts 15 years, until students finish secondary education.[48] The school year begins in March and PISA 2012 was implemented in July. Nowadays, students must turn 6 years old before March 31 in order to start primary education,[49] but in the past the cut-off date was July 31, and the cohort of students in our PISA sample was affected by this law.[50] Data reveals that the degree of enforcement of this law was moderate, and very similar to Mexico, since many students who were born a couple of months after the cut-off date managed to enter primary education the year they turned 6. Among those students affected by the law who progressed through the system without repeating or skipping a grade, those who were born before July 31, 1996 entered primary education in 2002 and should be in the eleventh grade in July 2012, while those who were born after July 31, 1996 entered primary education in 2003 and should be in the 10th grade in the month PISA was implemented in Peru.

Uruguay

Compulsory education in Uruguay starts at the age of 4 and lasts 14 years, until students finish secondary education.[51] The school year begins in early March and PISA 2012 was implemented in July. Students must turn 6 years old before April 30 in order to start primary education, but those students who were born after that date but before May 30 are also allowed to enroll as long as they get a "favorable opinion" from their kindergarten teacher or their first grade teacher.[52] Data reveals that the enforcement of this regulation is relatively high, but only a small fraction of the students born in May gets admission to

[48] According to the Political Constitution of Peru (1993) and the General Education Law No. 28044 (2003), initial, primary and secondary education is mandatory. However, the General Education Law states that the implementation of compulsory initial education is gradual.

[49] Ministerial Resolution No. 556-2014. December 15, 2014.

[50] Supreme Decree No. 007-2001-ED, February 13, 2001; Ministerial Resolution No. Nº 168-2002-ED, March 14, 2002.

[51] The General Law on Education No. 18,437, passed by the Parliament on December 12, 2008, extended compulsory education 5 years, by declaring mandatory the last 2 years of pre-primary education and the last 3 years of secondary education.

[52] Primary Education Council, *Circular* No. 555, November 2, 2001.

primary school the year in which they turn 6. If these students in PISA sample progressed through the system without repeating or skipping a grade, they entered primary education in 2002 and they should be in the eleventh grade in July 2012, while those who were born after May 30, 1996 (or during May but did not get a "favorable opinion") entered primary education in 2003 and should be in the 10th grade in July 2012.

Appendix 2. The cost of dropout at age 15 in Latin America: a simple computation.

In this appendix, we provide the details of a simple computation made in order to give a sense of the social and economic cost of dropout in Latin America. Specifically, we estimate the productivity losses resulting from students dropping out school in the lower grade under analysis instead of continuing education and getting an additional schooling year.

Table A2 shows every step in this computation for the Latin American countries in which we have found a significant effect of schooling on PISA test scores, i.e. Argentina, Brazil, Costa Rica, Mexico, and Uruguay. Column (1) displays the fuzzy estimates (with controls and 1 month bandwidth) of the contribution of an additional year of schooling on mathematical skills and knowledge, expressed in standard deviations (sd) of scores instead of PISA points. In Uruguay, for instance, we found that an extra year of education has an effect of 72 points in mathematics proficiency, which represents almost a standard deviation of scores in the estimation sample for this country (0.96 sd).

In order to translate the effect of a schooling year on skills and knowledge into some measure of productivity, we need to know how these skills and knowledge are transformed into a higher productivity. Unfortunately, there is no data linking mathematics proficiency as measured by PISA and wages -the most widely used measure of labor productivity. The closest proxy to this comes from the Survey of Adult Skills, since "at least in the domains of literacy/reading and numeracy/mathematics, the Survey of Adult Skills and PISA can be regarded as measuring much the same skills in much the same way" (OECD, 2013b). Hanushek et al. (2013) use the information in this survey for 22 developed countries and estimate that the average return to a standard deviation in numeracy is at least an 18 percent increase in hourly wage. We suspect that the effect in Latin America could be even higher, given that they find a higher return in countries with more income inequality. However, ignoring heterogeneities due to the lack of data and using this imperfect lower bound as a proxy for the effect of a standard deviation in mathematic proficiency on hourly wages -column (2)-, we can translate the effect of not getting an additional year of education at age 15 into an average hourly wage loss (in %) -Column (3)- just by multiplying columns (1) and (2). Thus, continuing with the example for Uruguay, if those students that leave school in the 10th grade lose almost a standard deviation in mathematics proficiency and this standard deviation implies an 18% lower hourly salary, then the cost for this early dropout is almost an 18% lower wage per hour (specifically, 17.28%).

The next steps are then quite straightforward. Household surveys in the SEDLAC database provide information regarding the average hourly wage of dropouts in the lower grade[53] (in 2005 USD PPP) –column (4)-, the average annual hours worked by this group –column (6)- and the total number of dropouts in the lower grade in each country –column (8)-. We can then multiply the average hourly wage loss (in %) from not getting the extra year of education in column (3) by the average hourly wage of dropouts in the lower grade in column (4) to translate the potential hourly loss in % into dollars at 2005 PPP. This is shown in Column (5). In Uruguay, for instance, we are considering that each dropout in the 10th grade, which is now earning U$S 3.18 at 2005 PPP, could have earned a 17.2% higher hourly wage with the skills and knowledge that the 10th year of schooling would have provided,[54] i.e., U$S 0.55 at 2005 PPP. Then, we can translate the hourly loss in dollars into an annual per capita loss in dollars -Column (7)- by multiplying these hourly values in Column (5) by the average annual hours worked by a dropout in the lower grade in Column (6). Finally, we can extend these "per dropout" values to the whole population multiplying them by the total number of dropouts in the lower grade in Column (8). The resulting cost is shown in Column (9) and it is also expressed as a % of GDP in Column (10).

Despite this is a very simple computation that does not take into account the externalities and general equilibrium effects that may arise, we think that this exercise is useful to get an idea of the considerable cost of dropout in Latin America. Under the assumptions made -which are not few-, the annual cost for the economy of the dropout at age 15 is 0.05% of GDP in Mexico, 0.15% in Argentina, 0.24% in Costa Rica, 0.79% in Uruguay and 1.52% in Brazil.[55] Considering that this is only an approximation to the productivity losses resulting from students dropping out school in the lower grade under analysis instead of continuing education and getting one additional schooling year, the total benefits for the society of eradicating dropout in all the schooling years are potentially high.

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[53] Dropouts in the lower grade are those people in the population with 8 years of completed education in Brazil, Costa Rica and Mexico, and 9 years of completed education in Argentina and Uruguay.

[54] Of course, the return could have been different in the past and it can be different for future generations, but the lack of data force us to make some simplifying assumptions.

[55] The estimation for Brazil uses the estimated mean return of schooling for Distrito Federal, Roraima, and Amazonas as a proxy for the whole country.

Table A2. Cost of dropout at age 15 in Latin America.

Country	Effect of the year of education on mathematics proficiency (in sd)	Effect of a sd in mathematics proficiency on hourly wages (in %)	Effect of the year of education on hourly wages (in %)	Average hourly wage of dropouts in the lower grade (in 2005 USD PPP)	Effect of the year of education on hourly wages (in 2005 USD PPP)	Average annual hours worked by dropouts in the lower grade	Annual cost per dropout (in 2005 USD PPP)	Number of dropouts in the lower grade	Annual cost for the economy of dropout at age 15 (in 2005 USD PPP)	Annual cost for the economy of dropout at age 15 (as % of GDP)
	(1)	(2)	(3) = (1) x (2)	(4)	(5) = (3) x (4)	(6)	(7) = (5) x (6)	(8)	(9) = (7) x (8)	(10)
Argentina	0.42	18%	8%	3.66	$ 0.28	2,118	$ 592.17	1,358,517	$ 804,471,398	0.15%
Brazil	1.30	18%	23%	3.70	$ 0.87	2,152	$ 1,871.35	17,045,782	$ 31,898,688,709	1.52%
Costa Rica	0.39	18%	7%	3.17	$ 0.22	2,310	$ 518.77	222,529	$ 115,441,026	0.24%
Mexico	0.19	18%	3%	2.27	$ 0.08	2,255	$ 170.63	4,270,283	$ 728,654,842	0.05%
Uruguay	0.96	18%	17%	3.18	$ 0.55	2,133	$ 1,177.14	287,225	$ 338,103,306	0.79%

Sources: (1): Own calculations based on PISA 2012; (2): Hanushek et. al. (2013) based on PIAAC; (4),(6),and (8): SEDLAC database based on 2012 Household Surveys. (10): GDP from World Development Indicators (WDI)

Notes: (1): Fuzzy RDD estimate using controls and 1 month bandwidth (Table 7) divided by the standard deviation of math scores in the estimation sample

(2): Coefficient estimates on numeracy score (standardized to std. dev. 1 within each country) in a pooled regression for 22 countries of log gross hourly wage on numeracy, gender, and a quadratic polynomial in actual work experience, sample of full-time employees aged 35-54. The specification includes country fixed effects, gives same same weight to each country and it is robust to different earnings and skill measures, additional controls, and various subgroups.

(4), (6) and (8): Dropouts in the lower grade are those people in the population with 8 years of completed education in Brazil, Costa Rica and Mexico, and 9 years of completed education in Argentina and Uruguay

(10): (9)/GDP in 2012 (in 2005 USD PPP). GDP in 2012 in local currency units from WDI converted to 2005 USD PPP using the same price index and PPP conversion factor that is used for wages in SEDLAC.

Tables

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	Beggining 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
Colombia (calendar A)	January/February	March 2012	01/96 - 12/96	6 years	Varies by school *(reference: March 31)*	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

Sources: Laws and regulations detailed in the Appendix 1, PISA 2012 data bases and OECD (2014b).
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.

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

Country	No. of students		Proportion of non-compliers		Proportion of repeaters	
	Upper grade	Lower grade	Upper grade	Lower grade	Upper grade	Lower grade
Argentina	190	3,765	0.28	0.01	0.07	0.06
Brazil	561	820	0.28	0.10	0.04	0.37
Chile	417	4,773	0.02	0.08	0.00	0.02
Colombia	1,840	3,902	0.51	0.09	0.03	0.20
Costa Rica	1,799	1,952	0.05	0.06	0.01	0.34
Mexico	24,091	7,230	0.17	0.09	0.01	0.32
Peru	1,456	2,907	0.46	0.05	0.01	0.11
Uruguay	68	3,051	0.21	0.07	0.00	0.01

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.

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

Country	All students		Compliers		Non-compliers		Repeaters	
	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)
Colombia	419.5	391.5	416.3	393.6	422.2	386.0	411.6	386.8
	(4.3)	(3.4)	(5.1)	(3.8)	(4.8)	(6.5)	(15.1)	(4.1)
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)

Source: authors' own calculations based on PISA 2012 data bases.
Note: Balanced Repeated Replication (BRR) standard errors in parenthesis.

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

Country	Bandwidth: 2 months		Bandwidth: 1 month	
	Without controls	With controls	Without controls	With controls
Argentina	22.2**	21.5**	26.3**	27.5**
	(10.7)	(10.0)	(12.2)	(11.3)
Brazil	69.2***	51.1***	90.8***	80.6***
	(13.4)	(11.5)	(20.1)	(18.8)
Chile	4.3	10.2*	5.7	10.3
	(6.0)	(5.3)	(8.2)	(7.2)
Colombia	24***	26.7***	17.7	22.4***
	(9.3)	(6.6)	(12.6)	(8.0)
Costa Rica	21.5***	19.3***	24.9***	20.6***
	(6.4)	(5.6)	(7.5)	(6.9)
Mexico	22.8***	17.2***	20.2***	15.4***
	(3.8)	(3.3)	(5.5)	(4.9)
Peru	21.6***	10.6*	18.4*	8.4
	(6.7)	(5.8)	(10.6)	(8.2)
Uruguay	53.3***	45.4***	55.3***	46.6***
	(12.4)	(11.6)	(12.1)	(11.2)

Source: authors' own estimations based on PISA 2012 data bases.
Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) BRR standard errors in parenthesis. (c) Controls include: 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 (WEALTH) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED >12 years).

Table 5. Definition of variables.

STUDENT LEVEL	
FEMALE	1= Female
PRESCHOOL0	1= Did not attend <ISCED 0>
PRESCHOOL1	1= Attended <ISCED 0> one year or less
PRESCHOOL2	1= Attended <ISCED 0> more than one year
NUCLEAR	1= Lives in a nuclear family (with two parents)
SIBLINGS	1= Lives with one or more siblings at home
FIRSTGEN	1= First generation inmigrant (student and parents were born in a foreign country)
SECONDGEN	1= Second generation inmigrant (student is native and both parents were born in a foreign country)
LANGUAGE1	1= Native who speaks the national language at home
LANGUAGE2	1= Native who speaks a foreign language at home
LANGUAGE3	1= Immigrant who speaks the national language at home
LANGUAGE4	1= Immigrant who speaks a foreign language at home
MOTACTIVE	1= Mother is economically active (working or looking for a job)
FATHACTIVE	1= Father is economically active (working or looking for a job)
HISEI	Highest parental occupational status (PISA index)
PARED	Highest parental education in years
HEDRES	Home educational resources (PISA index)
WEALTH	Wealth (PISA index)
DISCLIMA	Disciplinary Climate (PISA index)
STUDREL	Teacher Student Relations (PISA index)
INSTMOT	Instrumental Motivation for Mathematics (PISA index)
INTMAT	Mathematics Interest (PISA index)
ANXMAT	Mathematics Anxiety (PISA index)
PERSEV	Perseverance (PISA index)
MATWKETH	Mathematics Work Ethic (PISA index)
SCHOOL LEVEL	
PUBLIC	1 = Public school
PRIVDEP	1 = Private government-dependent school
PRIVIND	1 = Private independent school
BIGCITY	1 = School located in a large city (more than 1,000,000 inhabitants)
BUDGAUTON	1 = School with autonomy in budget allocations
TEXTAUTON	1 = School with autonomy in textbook selection
GROUPING	1 = School groups students by ability between classes
HIGHEDUCLIMATE	1 = School with high educational climate (mean of PARED at school level > 12)
PROPQUAL	Proportion of qualified teachers (with tertiary education) at school
TCSHORT	Shortage of teaching staff (PISA index)
TCMORALE	Teacher Morale (PISA index)
TEACCLIM	Teacher related factors affecting school climate (PISA index)
STUDCLIM	Student related factors affecting school climate (PISA index)
SCMATBUI	Quality of physical infrastructure (PISA index)
SCMATEDU	Quality of school educational resources (PISA index)

Table 6.1. Baseline covariates - Argentina

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.56	0.52	0.596	0.56	0.56	0.986
PRESCHOOL0	0.07	0.05	0.376	0.07	0.03	0.144
PRESCHOOL1	0.12	0.19	0.103	0.12	0.19	0.056
PRESCHOOL2	0.82	0.77	0.366	0.82	0.78	0.491
NUCLEAR	0.86	0.81	0.345	0.86	0.78	0.097
SIBLINGS	0.83	0.88	0.353	0.83	0.90	0.190
FIRSTGEN	0.02	0.02	0.940	0.02	0.02	0.917
SECONDGEN	0.04	0.03	0.678	0.04	0.02	0.445
LANGUAGE1	0.93	0.94	0.799	0.93	0.95	0.538
LANGUAGE2	0.01	0.01	0.433	0.01	0.01	0.656
LANGUAGE3	0.05	0.03	0.628	0.05	0.03	0.463
LANGUAGE4	0.01	0.01	0.970	0.01	0.01	0.708
MOTACTIVE	0.70	0.68	0.790	0.70	0.67	0.639
FATHACTIVE	0.95	0.93	0.439	0.95	0.93	0.463
HISEI	50.40	44.65	0.115	50.40	46.47	0.263
PARED	13.33	12.68	0.179	13.33	12.97	0.353
HEDRES	-0.19	-0.27	0.670	-0.19	-0.34	0.375
WEALTH	-0.84	-0.76	0.330	-0.84	-0.77	0.358
DISCLIMA	-0.09	-0.46	0.018*	-0.09	-0.54	0.002*
STUDREL	0.26	0.03	0.202	0.26	0.03	0.099
INSTMOT	0.05	0.21	0.380	0.05	0.10	0.793
INTMAT	0.02	0.06	0.835	0.02	0.09	0.696
ANXMAT	0.42	0.46	0.799	0.42	0.50	0.544
PERSEV	0.10	0.21	0.417	0.10	0.18	0.493
MATWKETH	-0.06	-0.08	0.909	-0.06	-0.03	0.812
SCHOOL LEVEL						
PUBLIC	0.56	0.63	0.316	0.56	0.59	0.614
PRIVDEP	0.31	0.31	0.957	0.31	0.33	0.735
PRIVIND	0.13	0.07	0.309	0.13	0.08	0.328
BIGCITY	0.26	0.14	0.011*	0.26	0.15	0.012*
BUDGAUTON	0.18	0.25	0.188	0.18	0.24	0.214
TEXTAUTON	0.94	0.93	0.753	0.94	0.92	0.699
GROUPING	0.83	0.85	0.762	0.83	0.84	0.876
HIGHEDUCLIMATE	0.58	0.67	0.172	0.58	0.67	0.119
PROPQUAL	0.19	0.21	0.688	0.19	0.20	0.686
TCSHORT	-0.16	-0.11	0.705	-0.16	-0.10	0.573
TCMORALE	-0.05	-0.06	0.911	-0.05	-0.06	0.934
TEACCLIM	-0.31	-0.35	0.772	-0.31	-0.38	0.583
STUDCLIM	0.42	0.36	0.811	0.42	0.37	0.818
SCMATBUI	-0.29	-0.26	0.882	-0.29	-0.19	0.536
SCMATEDU	-0.41	-0.29	0.635	-0.41	-0.35	0.762

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.2. Baseline covariates - Brazil

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.51	0.48	0.669	0.51	0.52	0.697
PRESCHOOL0	0.12	0.10	0.663	0.19	0.16	0.597
PRESCHOOL1	0.28	0.24	0.585	0.27	0.31	0.471
PRESCHOOL2	0.60	0.65	0.546	0.54	0.53	0.881
NUCLEAR	0.83	0.72	0.148	0.80	0.76	0.409
SIBLINGS	0.86	0.86	0.944	0.84	0.82	0.881
FIRSTGEN	0.00	0.00	.	0.00	0.00	.
SECONDGEN	0.02	0.00	0.187	0.02	0.00	0.097
LANGUAGE1	0.98	1.00	0.178	0.98	0.99	0.754
LANGUAGE2	0.00	0.00	.	0.00	0.01	0.303
LANGUAGE3	0.02	0.00	0.187	0.02	0.00	0.097
LANGUAGE4	0.00	0.00	.	0.00	0.00	.
MOTACTIVE	0.67	0.66	0.844	0.66	0.69	0.726
FATHACTIVE	0.81	0.82	0.925	0.87	0.87	0.978
HISEI	48.05	46.15	0.627	51.38	49.88	0.625
PARED	12.60	11.77	0.210	12.17	11.99	0.707
HEDRES	-0.40	-0.26	0.194	-0.42	-0.47	0.561
WEALTH	-0.63	-0.70	0.699	-0.58	-0.73	0.151
DISCLIMA	-0.30	-0.23	0.753	-0.18	-0.28	0.556
STUDREL	0.44	0.63	0.434	0.31	0.49	0.334
INSTMOT	0.31	0.35	0.832	0.46	0.39	0.708
INTMAT	0.46	0.49	0.849	0.57	0.50	0.596
ANXMAT	0.16	0.62	0.003*	0.29	0.56	0.004*
PERSEV	0.17	0.08	0.528	0.10	0.19	0.377
MATWKETH	0.27	0.43	0.360	0.30	0.37	0.584
SCHOOL LEVEL						
PUBLIC	0.85	0.96	0.262	0.85	0.95	0.274
PRIVDEP	0.00	0.00	.	0.00	0.00	.
PRIVIND	0.15	0.04	0.262	0.15	0.05	0.274
BIGCITY	0.65	0.55	0.363	0.56	0.53	0.769
BUDGAUTON	0.29	0.16	0.228	0.25	0.16	0.322
TEXTAUTON	0.78	0.84	0.557	0.82	0.81	0.921
GROUPING	0.73	0.71	0.883	0.70	0.75	0.624
HIGHEDUCLIMATE	0.35	0.38	0.691	0.39	0.38	0.735
PROPQUAL	0.85	0.96	0.177	0.88	0.90	0.565
TCSHORT	-0.01	0.17	0.383	-0.01	0.15	0.163
TCMORALE	-0.20	-0.17	0.907	-0.22	-0.33	0.635
TEACCLIM	0.03	-0.16	0.537	0.06	-0.18	0.252
STUDCLIM	-0.49	-0.70	0.281	-0.47	-0.70	0.050
SCMATBUI	-0.06	-0.41	0.098	-0.03	-0.38	0.057
SCMATEDU	-0.51	-0.80	0.093	-0.47	-0.87	0.004*

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.3. Baseline covariates - Chile

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.50	0.52	0.523	0.52	0.55	0.185
PRESCHOOL0	0.09	0.07	0.265	0.08	0.07	0.497
PRESCHOOL1	0.61	0.59	0.493	0.61	0.59	0.363
PRESCHOOL2	0.30	0.34	0.205	0.31	0.34	0.199
NUCLEAR	0.75	0.77	0.609	0.77	0.78	0.812
SIBLINGS	0.81	0.84	0.376	0.82	0.83	0.635
FIRSTGEN	0.02	0.01	0.379	0.01	0.01	0.447
SECONDGEN	0.00	0.00	0.680	0.00	0.00	0.671
LANGUAGE1	0.98	0.99	0.297	0.98	0.99	0.424
LANGUAGE2	0.01	0.00	0.292	0.00	0.00	0.508
LANGUAGE3	0.02	0.01	0.478	0.01	0.01	0.624
LANGUAGE4	0.00	0.00	.	0.00	0.00	0.171
MOTACTIVE	0.65	0.60	0.267	0.63	0.61	0.544
FATHACTIVE	0.94	0.96	0.249	0.94	0.96	0.212
HISEI	42.45	41.98	0.770	42.83	42.40	0.699
PARED	13.06	12.67	0.102	12.90	12.74	0.405
HEDRES	-0.39	-0.49	0.204	-0.37	-0.45	0.155
WEALTH	-0.43	-0.52	0.228	-0.47	-0.47	0.901
DISCLIMA	-0.24	-0.27	0.701	-0.19	-0.21	0.669
STUDREL	0.24	0.13	0.209	0.23	0.14	0.223
INSTMOT	0.27	0.35	0.418	0.29	0.30	0.863
INTMAT	0.25	0.29	0.688	0.25	0.24	0.927
ANXMAT	0.37	0.38	0.891	0.37	0.39	0.624
PERSEV	0.31	0.26	0.611	0.33	0.29	0.466
MATWKETH	0.18	0.15	0.806	0.17	0.15	0.668
SCHOOL LEVEL						
PUBLIC	0.34	0.30	0.333	0.33	0.29	0.064
PRIVDEP	0.47	0.51	0.363	0.50	0.53	0.192
PRIVIND	0.19	0.19	0.998	0.17	0.18	0.397
BIGCITY	0.20	0.18	0.278	0.19	0.20	0.584
BUDGAUTON	0.65	0.70	0.175	0.60	0.67	0.006*
TEXTAUTON	0.79	0.80	0.695	0.79	0.80	0.362
GROUPING	0.65	0.61	0.284	0.62	0.61	0.771
HIGHEDUCLIMATE	0.64	0.62	0.630	0.65	0.64	0.964
PROPQUAL	0.94	0.95	0.204	0.92	0.94	0.142
TCSHORT	0.66	0.43	0.021*	0.61	0.47	0.026*
TCMORALE	-0.27	-0.20	0.491	-0.24	-0.16	0.210
TEACCLIM	-0.48	-0.43	0.504	-0.46	-0.40	0.323
STUDCLIM	0.15	0.21	0.494	0.14	0.25	0.104
SCMATBUI	-0.03	-0.02	0.990	-0.03	0.00	0.570
SCMATEDU	-0.38	-0.31	0.421	-0.35	-0.33	0.777

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.4. Baseline covariates - Colombia

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.61	0.54	0.261	0.60	0.52	0.043*
PRESCHOOL0	0.10	0.13	0.351	0.10	0.12	0.456
PRESCHOOL1	0.53	0.55	0.603	0.54	0.57	0.476
PRESCHOOL2	0.37	0.32	0.296	0.36	0.32	0.301
NUCLEAR	0.75	0.66	0.163	0.70	0.66	0.336
SIBLINGS	0.74	0.77	0.431	0.75	0.74	0.736
FIRSTGEN	0.00	0.00	.	0.00	0.00	0.319
SECONDGEN	0.00	0.00	.	0.00	0.00	0.320
LANGUAGE1	1.00	0.99	0.324	1.00	0.99	0.093
LANGUAGE2	0.00	0.01	0.324	0.00	0.01	0.210
LANGUAGE3	0.00	0.00	.	0.00	0.00	0.319
LANGUAGE4	0.00	0.00	.	0.00	0.00	0.319
MOTACTIVE	0.59	0.57	0.820	0.60	0.61	0.828
FATHACTIVE	0.86	0.86	0.958	0.88	0.89	0.767
HISEI	44.08	42.81	0.587	42.87	42.02	0.629
PARED	11.73	11.74	0.983	11.52	11.60	0.714
HEDRES	-0.77	-0.65	0.298	-0.79	-0.70	0.299
WEALTH	-1.60	-1.45	0.153	-1.59	-1.48	0.189
DISCLIMA	0.06	-0.05	0.271	0.04	-0.03	0.343
STUDREL	0.44	0.32	0.343	0.42	0.38	0.719
INSTMOT	0.41	0.36	0.616	0.38	0.39	0.858
INTMAT	0.53	0.63	0.241	0.53	0.55	0.817
ANXMAT	0.30	0.20	0.337	0.30	0.27	0.770
PERSEV	0.37	0.65	0.006*	0.43	0.56	0.110
MATWKETH	0.38	0.55	0.105	0.40	0.46	0.419
SCHOOL LEVEL						
PUBLIC	0.83	0.83	0.774	0.82	0.84	0.327
PRIVDEP	0.04	0.03	0.180	0.04	0.03	0.096
PRIVIND	0.12	0.14	0.490	0.14	0.13	0.655
BIGCITY	0.30	0.27	0.393	0.29	0.27	0.338
BUDGAUTON	0.40	0.47	0.111	0.39	0.43	0.289
TEXTAUTON	0.76	0.74	0.562	0.75	0.73	0.488
GROUPING	0.93	0.96	0.089	0.93	0.96	0.009*
HIGHEDUCLIMATE	0.38	0.46	0.056	0.36	0.41	0.139
PROPQUAL	0.90	0.90	0.905	0.91	0.90	0.513
TCSHORT	0.74	0.68	0.523	0.63	0.69	0.426
TCMORALE	0.22	0.17	0.427	0.21	0.19	0.701
TEACCLIM	-0.45	-0.46	0.865	-0.42	-0.47	0.375
STUDCLIM	-0.49	-0.47	0.744	-0.44	-0.50	0.292
SCMATBUI	-0.72	-0.71	0.941	-0.70	-0.73	0.701
SCMATEDU	-1.29	-1.31	0.874	-1.26	-1.32	0.436

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.5. Baseline covariates – Costa Rica

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.58	0.58	0.937	0.58	0.55	0.372
PRESCHOOL0	0.14	0.11	0.173	0.13	0.13	0.984
PRESCHOOL1	0.44	0.43	0.864	0.44	0.37	0.042*
PRESCHOOL2	0.42	0.47	0.306	0.43	0.50	0.050
NUCLEAR	0.81	0.77	0.300	0.79	0.79	0.893
SIBLINGS	0.91	0.85	0.069	0.91	0.89	0.415
FIRSTGEN	0.02	0.01	0.602	0.01	0.01	0.842
SECONDGEN	0.01	0.03	0.078	0.01	0.03	0.024*
LANGUAGE1	0.96	0.95	0.474	0.97	0.95	0.151
LANGUAGE2	0.01	0.01	0.787	0.01	0.01	0.832
LANGUAGE3	0.02	0.04	0.380	0.02	0.03	0.150
LANGUAGE4	0.00	0.00	.	0.00	0.00	0.043*
MOTACTIVE	0.44	0.47	0.512	0.47	0.51	0.253
FATHACTIVE	0.91	0.93	0.293	0.91	0.92	0.941
HISEI	44.43	45.89	0.506	45.71	45.47	0.897
PARED	12.69	12.38	0.352	12.65	12.58	0.777
HEDRES	-0.63	-0.68	0.489	-0.62	-0.63	0.875
WEALTH	-1.18	-1.26	0.386	-1.16	-1.20	0.436
DISCLIMA	0.14	-0.03	0.146	0.14	-0.01	0.046*
STUDREL	0.31	0.38	0.595	0.32	0.45	0.100
INSTMOT	0.17	0.32	0.169	0.20	0.32	0.114
INTMAT	0.29	0.22	0.560	0.30	0.28	0.808
ANXMAT	0.32	0.37	0.611	0.39	0.35	0.676
PERSEV	0.53	0.44	0.381	0.55	0.46	0.271
MATWKETH	0.62	0.72	0.364	0.62	0.71	0.256
SCHOOL LEVEL						
PUBLIC	0.80	0.83	0.490	0.81	0.82	0.866
PRIVDEP	0.05	0.05	0.333	0.05	0.05	0.770
PRIVIND	0.15	0.12	0.332	0.14	0.13	0.918
BIGCITY	0.03	0.02	0.260	0.03	0.02	0.470
BUDGAUTON	0.51	0.55	0.386	0.52	0.55	0.415
TEXTAUTON	0.84	0.87	0.408	0.83	0.85	0.621
GROUPING	0.62	0.60	0.741	0.60	0.64	0.427
HIGHEDUCLIMATE	0.69	0.62	0.068	0.68	0.63	0.110
PROPQUAL	0.87	0.85	0.136	0.86	0.84	0.259
TCSHORT	-0.05	-0.02	0.715	0.01	-0.04	0.504
TCMORALE	0.03	0.04	0.993	0.09	0.06	0.718
TEACCLIM	-0.38	-0.45	0.497	-0.33	-0.41	0.474
STUDCLIM	-0.55	-0.65	0.289	-0.50	-0.56	0.406
SCMATBUI	-0.51	-0.70	0.119	-0.52	-0.68	0.102
SCMATEDU	-0.77	-1.02	0.035*	-0.78	-0.98	0.035*

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.6. Baseline covariates – Mexico

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.55	0.54	0.420	0.54	0.53	0.480
PRESCHOOL0	0.07	0.08	0.184	0.08	0.08	0.187
PRESCHOOL1	0.17	0.20	0.075	0.17	0.19	0.039*
PRESCHOOL2	0.76	0.72	0.017*	0.75	0.73	0.019*
NUCLEAR	0.83	0.83	0.906	0.82	0.83	0.551
SIBLINGS	0.88	0.88	0.774	0.88	0.88	0.882
FIRSTGEN	0.00	0.01	0.142	0.01	0.01	0.555
SECONDGEN	0.01	0.00	0.448	0.01	0.00	0.335
LANGUAGE1	0.97	0.97	0.824	0.96	0.97	0.441
LANGUAGE2	0.02	0.02	0.502	0.02	0.02	0.306
LANGUAGE3	0.01	0.01	0.689	0.01	0.01	0.751
LANGUAGE4	0.00	0.00	0.099	0.00	0.00	0.610
MOTACTIVE	0.49	0.47	0.253	0.48	0.46	0.252
FATHACTIVE	0.90	0.90	0.916	0.91	0.90	0.175
HISEI	41.91	41.24	0.522	42.17	40.17	0.005*
PARED	11.41	11.12	0.108	11.39	10.92	0.001*
HEDRES	-0.88	-0.93	0.279	-0.88	-0.96	0.016*
WEALTH	-1.27	-1.34	0.248	-1.29	-1.39	0.034*
DISCLIMA	0.04	0.14	0.052	0.08	0.10	0.763
STUDREL	0.45	0.46	0.894	0.44	0.47	0.327
INSTMOT	0.49	0.53	0.344	0.50	0.53	0.290
INTMAT	0.61	0.65	0.400	0.62	0.65	0.298
ANXMAT	0.41	0.42	0.881	0.41	0.43	0.455
PERSEV	0.36	0.31	0.236	0.35	0.33	0.411
MATWKETH	0.33	0.29	0.404	0.29	0.31	0.607
SCHOOL LEVEL						
PUBLIC	0.88	0.90	0.240	0.87	0.92	0*
PRIVDEP	0.00	0.00	0.311	0.00	0.00	0.335
PRIVIND	0.12	0.10	0.224	0.13	0.08	0.001*
BIGCITY	0.19	0.20	0.884	0.19	0.20	0.551
BUDGAUTON	0.55	0.55	0.770	0.55	0.51	0.087
TEXTAUTON	0.56	0.60	0.087	0.56	0.60	0.048*
GROUPING	0.69	0.72	0.252	0.70	0.72	0.239
HIGHEDUCLIMATE	0.36	0.32	0.107	0.36	0.30	0.004*
PROPQUAL	0.87	0.88	0.591	0.87	0.88	0.458
TCSHORT	0.44	0.46	0.715	0.45	0.51	0.208
TCMORALE	-0.03	0.00	0.470	-0.03	-0.06	0.605
TEACCLIM	-0.21	-0.23	0.720	-0.23	-0.29	0.205
STUDCLIM	0.04	0.07	0.551	0.04	0.02	0.611
SCMATBUI	-0.31	-0.37	0.253	-0.28	-0.44	0.001*
SCMATEDU	-0.70	-0.84	0.028*	-0.70	-0.91	0*

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.7. Baseline covariates – Peru

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.52	0.55	0.565	0.56	0.53	0.275
PRESCHOOL0	0.07	0.10	0.177	0.08	0.09	0.392
PRESCHOOL1	0.29	0.27	0.480	0.27	0.24	0.210
PRESCHOOL2	0.64	0.63	0.841	0.65	0.66	0.527
NUCLEAR	0.80	0.84	0.235	0.79	0.82	0.287
SIBLINGS	0.67	0.62	0.263	0.66	0.64	0.454
FIRSTGEN	0.00	0.00	0.295	0.00	0.00	0.296
SECONDGEN	0.00	0.00	.	0.00	0.00	0.161
LANGUAGE1	0.96	0.96	0.907	0.95	0.95	0.857
LANGUAGE2	0.04	0.04	0.779	0.04	0.04	0.860
LANGUAGE3	0.00	0.00	0.295	0.00	0.00	0.676
LANGUAGE4	0.00	0.00	.	0.00	0.00	.
MOTACTIVE	0.63	0.57	0.109	0.65	0.59	0.017*
FATHACTIVE	0.93	0.88	0.094	0.92	0.90	0.250
HISEI	38.26	38.45	0.913	38.47	39.01	0.663
PARED	12.24	12.01	0.456	12.15	11.99	0.468
HEDRES	-0.44	-0.42	0.868	-0.43	-0.43	1.000
WEALTH	-1.54	-1.55	0.965	-1.50	-1.58	0.240
DISCLIMA	0.04	0.03	0.849	0.02	0.02	0.991
STUDREL	0.37	0.40	0.781	0.35	0.36	0.953
INSTMOT	0.50	0.57	0.293	0.52	0.57	0.190
INTMAT	0.64	0.71	0.325	0.64	0.70	0.406
ANXMAT	0.26	0.28	0.768	0.32	0.27	0.325
PERSEV	0.40	0.55	0.207	0.42	0.52	0.157
MATWKETH	0.24	0.26	0.800	0.24	0.25	0.920
SCHOOL LEVEL						
PUBLIC	0.84	0.81	0.467	0.81	0.80	0.552
PRIVDEP	0.00	0.00	.	0.00	0.00	.
PRIVIND	0.16	0.19	0.467	0.19	0.20	0.552
BIGCITY	0.08	0.07	0.695	0.08	0.06	0.205
BUDGAUTON	0.70	0.70	0.972	0.70	0.70	0.949
TEXTAUTON	0.59	0.58	0.879	0.61	0.60	0.734
GROUPING	0.86	0.89	0.320	0.88	0.89	0.481
HIGHEDUCLIMATE	0.50	0.49	0.787	0.53	0.50	0.267
PROPQUAL	0.77	0.71	0.062	0.74	0.74	0.665
TCSHORT	0.59	0.58	0.913	0.58	0.54	0.365
TCMORALE	-0.13	-0.06	0.351	-0.12	-0.04	0.087
TEACCLIM	-0.26	-0.23	0.730	-0.28	-0.25	0.542
STUDCLIM	0.44	0.45	0.851	0.39	0.41	0.648
SCMATBUI	-0.37	-0.38	0.912	-0.30	-0.37	0.275
SCMATEDU	-1.03	-1.04	0.928	-0.98	-1.03	0.515

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 6.8. Baseline covariates – Uruguay

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.56	0.58	0.757	0.56	0.61	0.268
PRESCHOOL0	0.09	0.13	0.110	0.09	0.12	0.164
PRESCHOOL1	0.13	0.10	0.342	0.13	0.10	0.195
PRESCHOOL2	0.78	0.77	0.688	0.78	0.79	0.939
NUCLEAR	0.83	0.79	0.420	0.83	0.80	0.483
SIBLINGS	0.86	0.85	0.749	0.86	0.85	0.717
FIRSTGEN	0.01	0.00	0.183	0.01	0.00	0.183
SECONDGEN	0.01	0.00	0.106	0.01	0.00	0.106
LANGUAGE1	0.98	0.97	0.554	0.98	0.98	0.686
LANGUAGE2	0.01	0.03	0.069	0.01	0.02	0.184
LANGUAGE3	0.01	0.00	0.069	0.01	0.00	0.069
LANGUAGE4	0.00	0.00	.	0.00	0.00	.
MOTACTIVE	0.70	0.74	0.418	0.70	0.75	0.187
FATHACTIVE	0.90	0.92	0.422	0.90	0.92	0.620
HISEI	44.61	44.84	0.918	44.61	45.12	0.793
PARED	12.21	12.50	0.425	12.21	12.38	0.615
HEDRES	-0.10	-0.06	0.653	-0.10	0.01	0.067
WEALTH	-0.54	-0.58	0.587	-0.54	-0.54	0.953
DISCLIMA	0.17	0.01	0.082	0.17	0.01	0.072
STUDREL	0.13	0.09	0.706	0.13	0.08	0.616
INSTMOT	-0.08	0.07	0.189	-0.08	0.08	0.132
INTMAT	0.15	0.17	0.790	0.15	0.20	0.567
ANXMAT	0.19	0.29	0.340	0.19	0.25	0.524
PERSEV	0.47	0.27	0.076	0.47	0.37	0.392
MATWKETH	0.15	0.03	0.135	0.15	0.13	0.819
SCHOOL LEVEL						
PUBLIC	0.77	0.77	0.958	0.77	0.77	0.999
PRIVDEP	0.00	0.00	.	0.00	0.00	.
PRIVIND	0.23	0.23	0.958	0.23	0.23	0.999
BIGCITY	0.39	0.33	0.185	0.39	0.33	0.123
BUDGAUTON	0.46	0.37	0.063	0.46	0.37	0.038*
TEXTAUTON	0.70	0.74	0.267	0.70	0.72	0.487
GROUPING	0.90	0.88	0.465	0.90	0.90	0.955
HIGHEDUCLIMATE	0.46	0.47	0.697	0.46	0.46	0.949
PROPQUAL	0.09	0.08	0.353	0.09	0.08	0.390
TCSHORT	0.20	0.19	0.947	0.20	0.20	0.985
TCMORALE	0.02	-0.11	0.108	0.02	-0.14	0.021*
TEACCLIM	-0.43	-0.40	0.781	-0.43	-0.45	0.870
STUDCLIM	0.30	0.32	0.885	0.30	0.31	0.985
SCMATBUI	-0.29	-0.19	0.425	-0.29	-0.20	0.423
SCMATEDU	0.10	0.08	0.819	0.10	0.06	0.673

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 7. Fuzzy regression discontinuity design. Effect of a schooling year on mathematics score. Latin American countries in PISA 2012.

Country	Bandwidth: 2 months		Bandwidth: 1 month	
	Without controls	With controls	Without controls	With controls
Argentina	17.4	22.3*	22.5	29.6**
	(12.1)	(11.9)	(14.4)	(13.8)
Brazil	63.5***	55.7***	103.2***	106.3***
	(20.0)	(17.3)	(35.2)	(30.4)
Chile	10.0	9.0	14.8	7.6
	(8.8)	(7.7)	(14.2)	(13.3)
Colombia	9.3	46.7	-40.6	11.4
	(31.4)	(30.0)	(63.2)	(60.9)
Costa Rica	23.2***	22.6***	28.8***	25.2***
	(6.8)	(6.1)	(9.5)	(8.8)
Mexico	24***	17.2***	16.8*	13.1*
	(5.5)	(4.7)	(8.8)	(7.9)
Peru	4.3	-0.8	-1.6	-10.9
	(11.8)	(11.9)	(22.4)	(18.4)
Uruguay	76.3*	63.3	88.9*	71.5*
	(42.1)	(38.8)	(45.7)	(42.9)

Source: authors' own estimations based on PISA 2012 data bases.
Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) BRR standard errors in parenthesis. (c) Controls include: 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 (WEALTH) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED >12 years).

Table 8. Sharp regression discontinuity design. Effect of a schooling year on mathematics score. High-performing countries in PISA 2012.

Country	**Bandwidth: 2 months**		**Bandwidth: 1 month**	
	Without controls	With controls	Without controls	With controls
Finland	25.9***	28.3***	27.9***	30.8***
	(6.0)	(5.9)	(7.0)	(6.8)
Estonia	24.6***	25.8***	27.2***	28.4***
	(4.8)	(5.0)	(6.7)	(6.8)
Hong Kong - China	14.1**	14.1**	8.6	11.6
	(6.8)	(6.2)	(8.2)	(8.1)
Korea	27**	28.3**	39.6**	40.6**
	(11.9)	(11.9)	(17.8)	(17.9)
Shanghai - China	18.5***	20.5***	20.4**	22.5***
	(7.1)	(6.6)	(8.6)	(8.1)
Taiwan	12.8*	12.4	25.6**	24**
	(7.5)	(7.7)	(10.9)	(11.0)

Source: authors' own estimations based on PISA 2012 data bases.
Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) BRR standard errors in parenthesis. (c) Controls include: 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 (WEALTH) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED >12 years).

Table 9.1. Baseline covariates – Finland

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.51	0.47	0.099	0.50	0.47	0.272
PRESCHOOL0	0.01	0.03	0.026*	0.02	0.03	0.236
PRESCHOOL1	0.37	0.33	0.128	0.36	0.33	0.179
PRESCHOOL2	0.62	0.64	0.387	0.62	0.64	0.354
NUCLEAR	0.82	0.86	0.156	0.82	0.86	0.139
SIBLINGS	0.77	0.79	0.512	0.77	0.79	0.272
FIRSTGEN	0.01	0.02	0.360	0.02	0.02	0.943
SECONDGEN	0.01	0.01	0.659	0.01	0.01	0.881
LANGUAGE1	0.96	0.95	0.746	0.95	0.95	0.885
LANGUAGE2	0.02	0.01	0.526	0.02	0.01	0.639
LANGUAGE3	0.00	0.01	0.478	0.01	0.01	0.856
LANGUAGE4	0.02	0.02	0.495	0.02	0.02	0.859
MOTACTIVE	0.88	0.90	0.390	0.88	0.90	0.436
FATHACTIVE	0.91	0.93	0.454	0.91	0.93	0.339
HISEI	55.90	55.42	0.705	56.41	55.42	0.399
PARED	14.99	14.90	0.456	15.15	14.90	0.021*
HEDRES	-0.31	-0.31	0.989	-0.34	-0.31	0.479
WEALTH	0.25	0.23	0.614	0.27	0.23	0.345
DISCLIMA	-0.28	-0.32	0.593	-0.34	-0.32	0.878
STUDREL	-0.10	0.03	0.138	-0.16	0.03	0.013*
INSTMOT	0.11	0.02	0.279	0.01	0.02	0.827
INTMAT	-0.17	-0.20	0.632	-0.23	-0.20	0.541
ANXMAT	-0.40	-0.36	0.593	-0.38	-0.36	0.795
PERSEV	0.01	-0.05	0.467	0.04	-0.05	0.226
MATWKETH	-0.31	-0.23	0.247	-0.34	-0.23	0.053
SCHOOL LEVEL						
PUBLIC	0.97	0.97	0.348	0.96	0.97	0.05*
PRIVDEP	0.03	0.03	0.348	0.04	0.03	0.05*
PRIVIND	0.00	0.00	.	0.00	0.00	.
BIGCITY	0.00	0.00	.	0.00	0.00	.
BUDGAUTON	0.99	0.97	0.236	0.98	0.97	0.428
TEXTAUTON	0.90	0.90	0.715	0.89	0.90	0.472
GROUPING	0.63	0.66	0.396	0.63	0.66	0.331
HIGHEDUCLIMATE	1.00	1.00	.	1.00	1.00	0.169
PROPQUAL	0.93	0.92	0.470	0.92	0.92	0.643
TCSHORT	-0.46	-0.41	0.267	-0.46	-0.41	0.216
TCMORALE	0.31	0.27	0.525	0.33	0.27	0.324
TEACCLIM	-0.09	-0.10	0.843	-0.07	-0.10	0.554
STUDCLIM	-0.50	-0.49	0.847	-0.50	-0.49	0.874
SCMATBUI	-0.32	-0.29	0.657	-0.30	-0.29	0.872
SCMATEDU	-0.19	-0.19	0.968	-0.18	-0.19	0.802

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 9.2. Baseline covariates – Estonia

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.49	0.49	0.922	0.51	0.48	0.261
PRESCHOOL0	0.04	0.07	0.217	0.06	0.07	0.636
PRESCHOOL1	0.09	0.07	0.324	0.09	0.07	0.275
PRESCHOOL2	0.86	0.86	0.967	0.85	0.86	0.635
NUCLEAR	0.79	0.84	0.153	0.78	0.82	0.093
SIBLINGS	0.77	0.80	0.546	0.78	0.79	0.695
FIRSTGEN	0.00	0.01	0.440	0.00	0.01	0.370
SECONDGEN	0.09	0.08	0.793	0.08	0.09	0.491
LANGUAGE1	0.87	0.89	0.390	0.88	0.86	0.489
LANGUAGE2	0.04	0.02	0.205	0.04	0.04	0.975
LANGUAGE3	0.05	0.07	0.408	0.06	0.08	0.138
LANGUAGE4	0.03	0.02	0.208	0.02	0.01	0.200
MOTACTIVE	0.87	0.86	0.645	0.88	0.89	0.486
FATHACTIVE	0.95	0.94	0.323	0.95	0.94	0.278
HISEI	52.87	50.36	0.157	51.06	50.38	0.614
PARED	14.14	13.95	0.321	14.03	13.92	0.401
HEDRES	0.28	0.14	0.048*	0.28	0.19	0.074
WEALTH	-0.15	-0.24	0.146	-0.17	-0.19	0.683
DISCLIMA	0.14	0.21	0.448	0.19	0.21	0.708
STUDREL	-0.09	-0.13	0.677	-0.14	-0.04	0.152
INSTMOT	0.04	0.07	0.801	0.07	0.05	0.797
INTMAT	0.03	0.01	0.820	0.02	0.02	0.987
ANXMAT	-0.13	-0.13	0.931	-0.09	-0.23	0.026*
PERSEV	0.29	0.23	0.531	0.35	0.21	0.033*
MATWKETH	-0.13	-0.10	0.761	-0.12	-0.10	0.747
SCHOOL LEVEL						
PUBLIC	0.98	0.99	0.550	0.98	0.97	0.403
PRIVDEP	0.01	0.01	0.969	0.01	0.02	0.426
PRIVIND	0.01	0.00	0.090	0.01	0.01	0.811
BIGCITY	0.00	0.00	.	0.00	0.00	.
BUDGAUTON	0.95	0.96	0.726	0.96	0.94	0.253
TEXTAUTON	0.86	0.86	0.864	0.87	0.87	0.893
GROUPING	0.89	0.89	0.982	0.90	0.89	0.750
HIGHEDUCLIMATE	1.00	0.97	0.046*	1.00	0.98	0.077
PROPQUAL	.	.	.	.	.	.
TCSHORT	0.05	-0.04	0.209	0.02	-0.03	0.313
TCMORALE	0.07	-0.04	0.121	0.07	-0.03	0.037*
TEACCLIM	0.18	0.22	0.492	0.17	0.17	0.992
STUDCLIM	-0.04	-0.01	0.672	-0.05	-0.02	0.450
SCMATBUI	-0.01	-0.02	0.928	0.10	0.06	0.511
SCMATEDU	-0.22	-0.11	0.082	-0.23	-0.08	0.001*

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 9.3. Baseline covariates - Hong Kong - China

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.51	0.49	0.581	0.46	0.49	0.398
PRESCHOOL0	0.00	0.00	0.322	0.00	0.00	0.074
PRESCHOOL1	0.03	0.01	0.155	0.02	0.01	0.471
PRESCHOOL2	0.96	0.99	0.107	0.97	0.99	0.233
NUCLEAR	0.88	0.89	0.657	0.86	0.89	0.339
SIBLINGS	0.75	0.71	0.415	0.75	0.71	0.355
FIRSTGEN	0.10	0.07	0.188	0.08	0.07	0.462
SECONDGEN	0.25	0.20	0.190	0.25	0.20	0.152
LANGUAGE1	0.63	0.72	0.034*	0.63	0.72	0.027*
LANGUAGE2	0.02	0.00	0.009*	0.03	0.00	0.002*
LANGUAGE3	0.31	0.24	0.053	0.30	0.24	0.079
LANGUAGE4	0.03	0.03	0.806	0.02	0.03	0.546
MOTACTIVE	0.65	0.68	0.490	0.63	0.68	0.144
FATHACTIVE	0.91	0.91	0.984	0.92	0.91	0.530
HISEI	47.17	48.08	0.614	47.14	48.08	0.579
PARED	11.80	11.62	0.409	11.74	11.62	0.533
HEDRES	-0.20	-0.24	0.573	-0.22	-0.24	0.701
WEALTH	-0.91	-0.84	0.149	-0.90	-0.84	0.173
DISCLIMA	0.32	0.38	0.549	0.38	0.38	0.991
STUDREL	0.08	-0.07	0.085	0.05	-0.07	0.098
INSTMOT	-0.04	-0.18	0.095	-0.13	-0.18	0.474
INTMAT	0.49	0.27	0.024*	0.44	0.27	0.028*
ANXMAT	0.00	0.13	0.170	0.05	0.13	0.348
PERSEV	0.06	0.15	0.128	0.12	0.15	0.701
MATWKETH	0.07	-0.07	0.132	0.10	-0.07	0.031*
SCHOOL LEVEL						
PUBLIC	0.06	0.08	0.405	0.07	0.08	0.755
PRIVDEP	0.93	0.92	0.526	0.91	0.92	0.933
PRIVIND	0.01	0.00	0.229	0.01	0.00	0.240
BIGCITY	1.00	1.00	.	1.00	1.00	.
BUDGAUTON	0.97	0.97	0.570	0.96	0.97	0.328
TEXTAUTON	0.99	0.99	0.297	0.99	0.99	0.303
GROUPING	0.87	0.90	0.168	0.89	0.90	0.428
HIGHEDUCLIMATE	0.31	0.33	0.618	0.32	0.33	0.819
PROPQUAL	0.97	0.98	0.429	0.97	0.98	0.469
TCSHORT	-0.24	-0.19	0.481	-0.24	-0.19	0.494
TCMORALE	-0.36	-0.36	0.967	-0.34	-0.36	0.796
TEACCLIM	-0.31	-0.33	0.793	-0.31	-0.33	0.733
STUDCLIM	0.40	0.50	0.110	0.35	0.50	0.003*
SCMATBUI	-0.01	-0.06	0.416	-0.02	-0.06	0.471
SCMATEDU	0.48	0.39	0.180	0.46	0.39	0.285

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 9.4. Baseline covariates – Korea

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.47	0.48	0.842	0.46	0.48	0.618
PRESCHOOL0	0.04	0.03	0.763	0.04	0.04	0.472
PRESCHOOL1	0.12	0.14	0.303	0.13	0.15	0.141
PRESCHOOL2	0.84	0.82	0.465	0.83	0.81	0.348
NUCLEAR	0.89	0.90	0.768	0.90	0.91	0.320
SIBLINGS	0.87	0.90	0.167	0.88	0.90	0.222
FIRSTGEN	0.00	0.00	.	0.00	0.00	.
SECONDGEN	0.00	0.00	.	0.00	0.00	.
LANGUAGE1	1.00	1.00	.	1.00	1.00	0.321
LANGUAGE2	0.00	0.00	.	0.00	0.00	0.321
LANGUAGE3	0.00	0.00	.	0.00	0.00	.
LANGUAGE4	0.00	0.00	.	0.00	0.00	.
MOTACTIVE	0.63	0.64	0.670	0.61	0.61	0.977
FATHACTIVE	0.92	0.91	0.615	0.92	0.92	0.616
HISEI	53.91	54.71	0.546	53.38	54.68	0.201
PARED	14.12	14.14	0.906	13.97	14.14	0.190
HEDRES	-0.10	-0.12	0.815	-0.12	-0.12	0.993
WEALTH	-0.68	-0.68	0.995	-0.69	-0.70	0.635
DISCLIMA	0.17	0.13	0.631	0.17	0.11	0.348
STUDREL	-0.02	-0.18	0.045*	-0.04	-0.17	0.022*
INSTMOT	-0.47	-0.44	0.739	-0.42	-0.42	0.987
INTMAT	-0.26	-0.24	0.747	-0.22	-0.24	0.728
ANXMAT	0.22	0.25	0.729	0.23	0.26	0.564
PERSEV	-0.04	-0.13	0.158	-0.08	-0.12	0.434
MATWKETH	-0.64	-0.62	0.863	-0.56	-0.58	0.761
SCHOOL LEVEL						
PUBLIC	0.53	0.59	0.150	0.51	0.61	0.029*
PRIVDEP	0.30	0.28	0.540	0.31	0.27	0.235
PRIVIND	0.17	0.13	0.060	0.17	0.12	0.012*
BIGCITY	0.44	0.46	0.541	0.44	0.44	0.910
BUDGAUTON	0.63	0.71	0.042*	0.65	0.71	0.056
TEXTAUTON	0.75	0.81	0.182	0.78	0.81	0.249
GROUPING	0.92	0.85	0.107	0.92	0.85	0.162
HIGHEDUCLIMATE	0.98	0.99	0.311	0.98	0.99	0.191
PROPQUAL	1.00	1.00	0.725	1.00	1.00	0.390
TCSHORT	-0.05	-0.01	0.664	0.04	-0.02	0.578
TCMORALE	-0.33	-0.32	0.887	-0.33	-0.28	0.614
TEACCLIM	0.07	0.00	0.505	0.03	0.02	0.935
STUDCLIM	0.05	-0.07	0.193	0.05	-0.09	0.131
SCMATBUI	-0.12	-0.23	0.247	-0.20	-0.20	0.974
SCMATEDU	0.14	-0.00	0.132	0.07	0.04	0.765

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 9.5. Baseline covariates - Shanghai – China

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.55	0.54	0.648	0.54	0.50	0.060
PRESCHOOL0	0.01	0.02	0.545	0.02	0.01	0.468
PRESCHOOL1	0.08	0.06	0.320	0.08	0.07	0.568
PRESCHOOL2	0.91	0.92	0.503	0.90	0.92	0.374
NUCLEAR	0.89	0.87	0.602	0.89	0.87	0.245
SIBLINGS	0.17	0.16	0.756	0.19	0.17	0.476
FIRSTGEN	0.00	0.01	0.085	0.00	0.01	0.099
SECONDGEN	0.00	0.00	0.319	0.00	0.00	0.061
LANGUAGE1	0.99	0.98	0.176	0.99	0.98	0.056
LANGUAGE2	0.01	0.00	0.737	0.00	0.00	0.999
LANGUAGE3	0.00	0.01	0.119	0.00	0.01	0.063
LANGUAGE4	0.00	0.00	0.239	0.00	0.00	0.138
MOTACTIVE	0.80	0.77	0.448	0.79	0.80	0.949
FATHACTIVE	0.90	0.90	0.891	0.91	0.91	0.939
HISEI	52.77	51.73	0.456	51.82	51.46	0.720
PARED	13.07	13.02	0.835	13.02	13.10	0.651
HEDRES	0.04	0.07	0.654	0.04	0.05	0.814
WEALTH	-0.66	-0.67	0.936	-0.66	-0.69	0.591
DISCLIMA	0.47	0.81	0*	0.53	0.81	0*
STUDREL	0.36	0.53	0.064	0.45	0.59	0.046*
INSTMOT	-0.10	0.03	0.077	-0.07	0.06	0.022*
INTMAT	0.38	0.52	0.095	0.39	0.53	0.034*
ANXMAT	0.08	0.00	0.331	0.06	-0.03	0.175
PERSEV	0.27	0.29	0.837	0.27	0.30	0.558
MATWKETH	0.29	0.44	0.078	0.28	0.49	0.004*
SCHOOL LEVEL						
PUBLIC	0.94	0.85	0.006*	0.94	0.86	0.007*
PRIVDEP	0.00	0.00	.	0.00	0.00	.
PRIVIND	0.06	0.15	0.006*	0.06	0.14	0.007*
BIGCITY	1.00	1.00	.	1.00	1.00	.
BUDGAUTON	0.52	0.62	0.104	0.54	0.61	0.252
TEXTAUTON	0.53	0.36	0.002*	0.53	0.36	0.004*
GROUPING	0.94	0.95	0.464	0.94	0.95	0.781
HIGHEDUCLIMATE	0.72	0.75	0.656	0.72	0.74	0.803
PROPQUAL	0.96	0.94	0.008*	0.96	0.94	0.004*
TCSHORT	0.75	0.76	0.938	0.70	0.76	0.661
TCMORALE	-0.13	0.15	0.023*	-0.15	0.17	0.009*
TEACCLIM	-0.67	-0.62	0.806	-0.64	-0.63	0.941
STUDCLIM	0.06	0.54	0.058	0.06	0.50	0.056
SCMATBUI	-0.14	-0.28	0.364	-0.14	-0.28	0.354
SCMATEDU	0.13	0.22	0.585	0.13	0.19	0.692

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 9.6. Baseline covariates – Taiwan

	Bandwidth: +/- 1 month			Bandwidth: +/- 2 months		
	Mean before cut-off date	Mean after cut-off date	pvalue	Mean before cut-off date	Mean after cut-off date	pvalue
STUDENT LEVEL						
FEMALE	0.50	0.49	0.816	0.50	0.50	0.942
PRESCHOOL0	0.00	0.01	0.130	0.01	0.02	0.014*
PRESCHOOL1	0.15	0.14	0.894	0.17	0.14	0.116
PRESCHOOL2	0.85	0.84	0.783	0.83	0.84	0.415
NUCLEAR	0.88	0.81	0.019*	0.88	0.84	0.014*
SIBLINGS	0.88	0.86	0.503	0.88	0.88	0.588
FIRSTGEN	0.00	0.00	0.321	0.00	0.00	0.303
SECONDGEN	0.00	0.01	0.448	0.00	0.01	0.185
LANGUAGE1	0.83	0.82	0.848	0.84	0.83	0.847
LANGUAGE2	0.16	0.17	0.814	0.15	0.16	0.942
LANGUAGE3	0.01	0.01	0.779	0.00	0.01	0.327
LANGUAGE4	0.00	0.00	.	0.00	0.00	.
MOTACTIVE	0.67	0.69	0.496	0.71	0.70	0.684
FATHACTIVE	0.90	0.89	0.935	0.92	0.90	0.241
HISEI	45.98	46.35	0.792	46.89	47.10	0.859
PARED	13.12	12.85	0.086	13.10	13.00	0.474
HEDRES	-0.32	-0.37	0.470	-0.27	-0.31	0.413
WEALTH	-0.48	-0.55	0.255	-0.47	-0.48	0.764
DISCLIMA	-0.11	-0.08	0.633	-0.09	-0.04	0.467
STUDREL	-0.07	-0.03	0.662	0.02	-0.04	0.365
INSTMOT	-0.36	-0.25	0.149	-0.34	-0.25	0.051
INTMAT	0.07	0.08	0.897	0.05	0.08	0.537
ANXMAT	0.41	0.30	0.242	0.36	0.27	0.152
PERSEV	-0.08	0.01	0.262	-0.11	-0.08	0.624
MATWKETH	-0.18	-0.28	0.199	-0.15	-0.28	0.038*
SCHOOL LEVEL						
PUBLIC	0.50	0.87	0*	0.49	0.89	0*
PRIVDEP	0.08	0.01	0.005*	0.09	0.01	0.001*
PRIVIND	0.41	0.12	0*	0.42	0.10	0*
BIGCITY	0.33	0.27	0.322	0.32	0.28	0.488
BUDGAUTON	0.78	0.70	0.203	0.76	0.70	0.402
TEXTAUTON	0.86	0.89	0.610	0.85	0.88	0.652
GROUPING	0.91	0.68	0*	0.90	0.67	0*
HIGHEDUCLIMATE	0.86	0.86	0.945	0.87	0.87	0.947
PROPQUAL	0.92	0.86	0.275	0.92	0.88	0.372
TCSHORT	-0.31	0.11	0.008*	-0.28	0.09	0.025*
TCMORALE	0.01	-0.35	0.015*	-0.00	-0.34	0.025*
TEACCLIM	0.17	-0.22	0.044*	0.15	-0.25	0.035*
STUDCLIM	0.79	0.51	0.167	0.80	0.52	0.183
SCMATBUI	0.19	-0.17	0.016*	0.20	-0.17	0.017*
SCMATEDU	0.68	0.45	0.112	0.66	0.45	0.183

Source: authors' own calculations based on PISA 2012 data bases.
Notes: (a) * Significant at 5%. (b) p-value for the null hypothesis that the mean before and after the cut-off is the same, with standard errors computed using BRR to adjust for clustering of students within schools.

Table 10. Fuzzy regression discontinuity design. Effect of a schooling year on mathematics score. High-performing countries in PISA 2012.

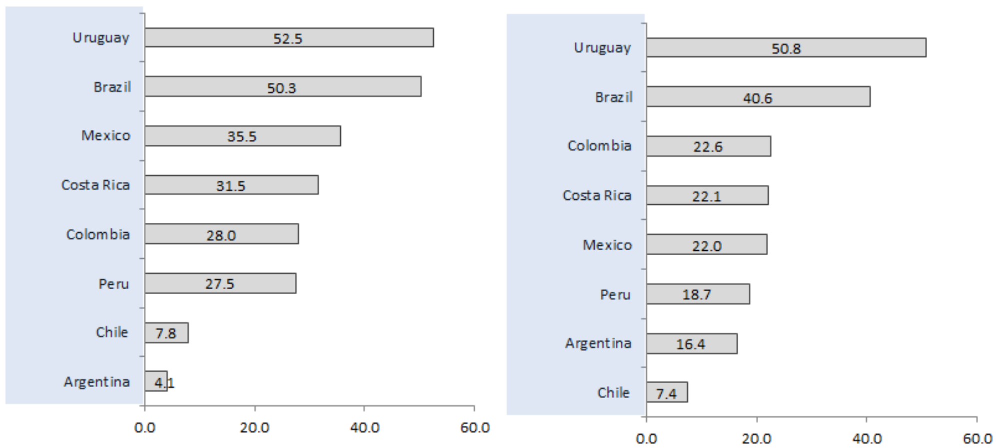
Country	Bandwidth: 2 months		Bandwidth: 1 month	
	Without controls	With controls	Without controls	With controls
Finland	17.8***	20***	16.7**	19.3**
	(6.8)	(6.8)	(8.3)	(8.1)
Estonia	18.2***	19.8***	23.3**	22**
	(6.8)	(6.9)	(10.7)	(11.2)
Hong Kong - China	11.2	12.7	3.6	11.4
	(12.5)	(10.9)	(17.1)	(15.8)
Korea	11.0	9.2	-0.9	-2.1
	(21.7)	(21.1)	(38.8)	(37.4)
Shanghai - China	14.8*	17.9**	15.0	18.8*
	(8.2)	(7.7)	(10.7)	(10.2)
Taiwan	10.0	10.0	21*	19.7*
	(7.7)	(8.0)	(12.1)	(12.0)

Source: authors' own estimations based on PISA 2012 data bases.
Notes: (a) ***Significant at 1%. **Significant at 5%. *Significant at 10%. (b) BRR standard errors in parenthesis. (c) Controls include: 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 (WEALTH) and a dummy that indicates whether the school to which the student attends has a relatively high educational climate (mean of PARED >12 years).

Figures

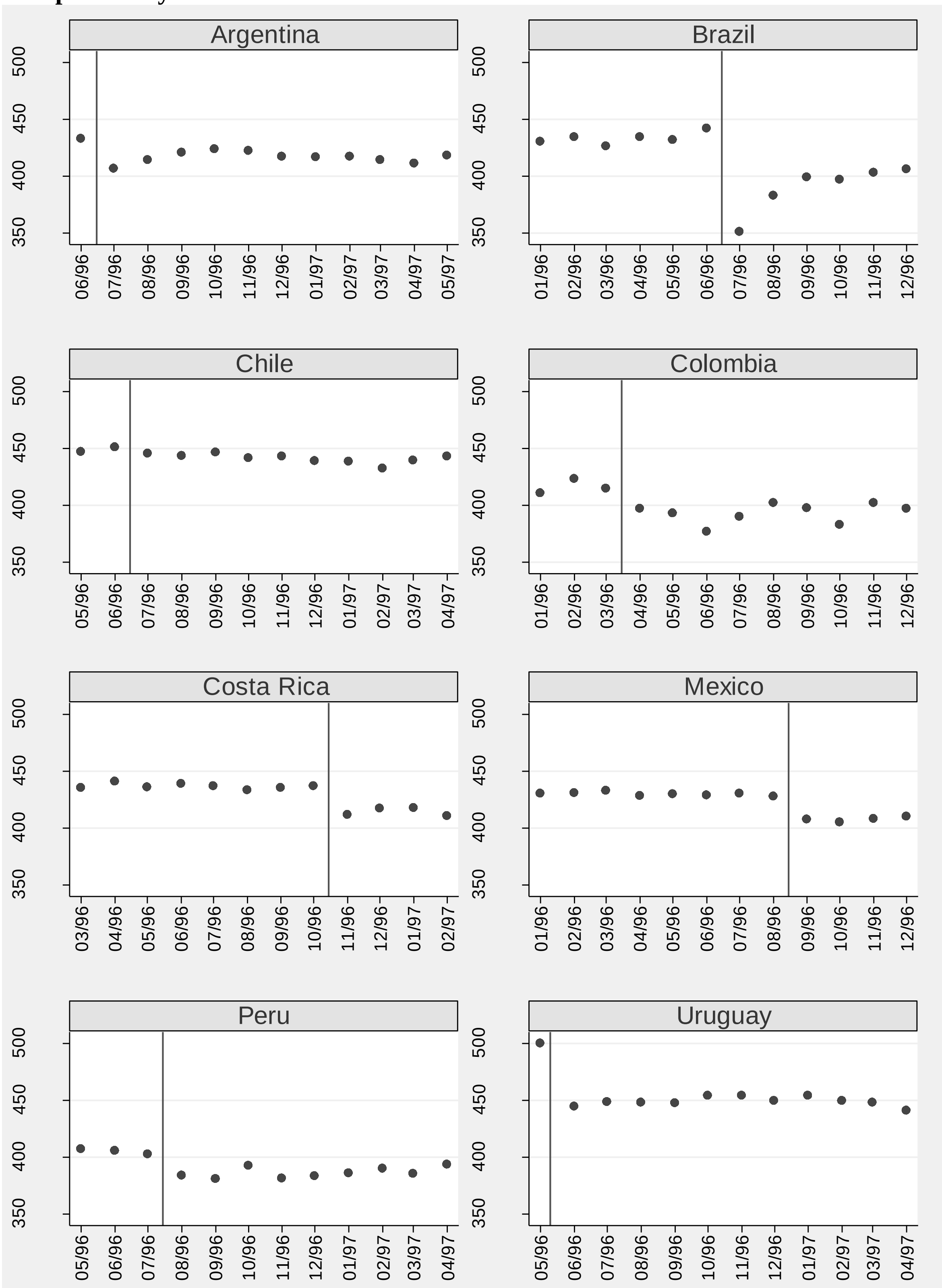
Figure 1. Difference between the mean score in mathematics in the upper and lower grade under analysis. Latin American countries in PISA 2012.

Panel (a): All students **Panel (b): Compliers only**



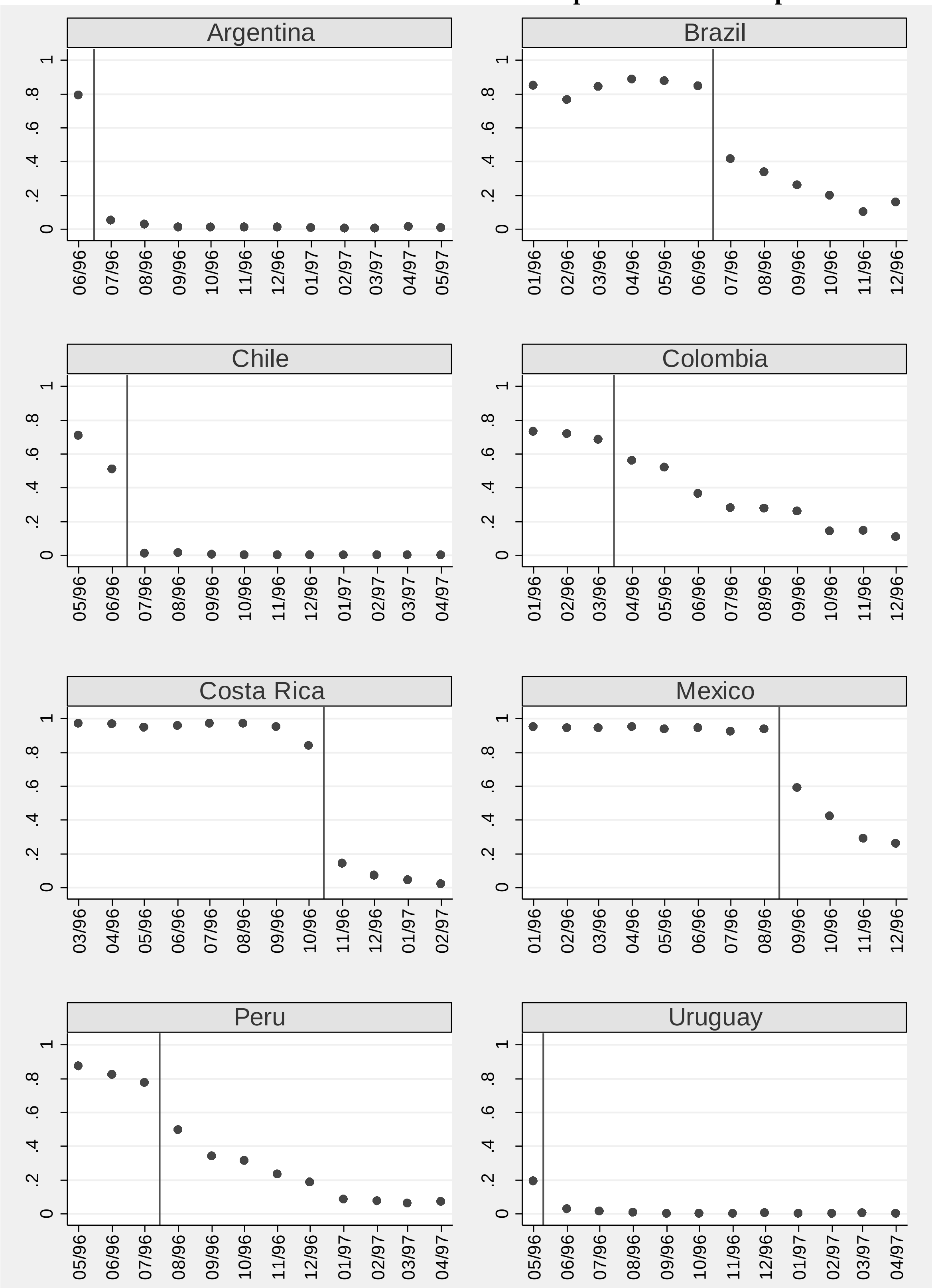
Source: authors' own calculations based on PISA 2012 data bases.
Note: The upper and lower grades are grade 11 and 10 respectively, except in Mexico and Costa Rica where the upper grade is the 10th and the lower is the 9th.

Figure 2. Mean score in mathematics by birthdate in Latin American countries - Compliers only



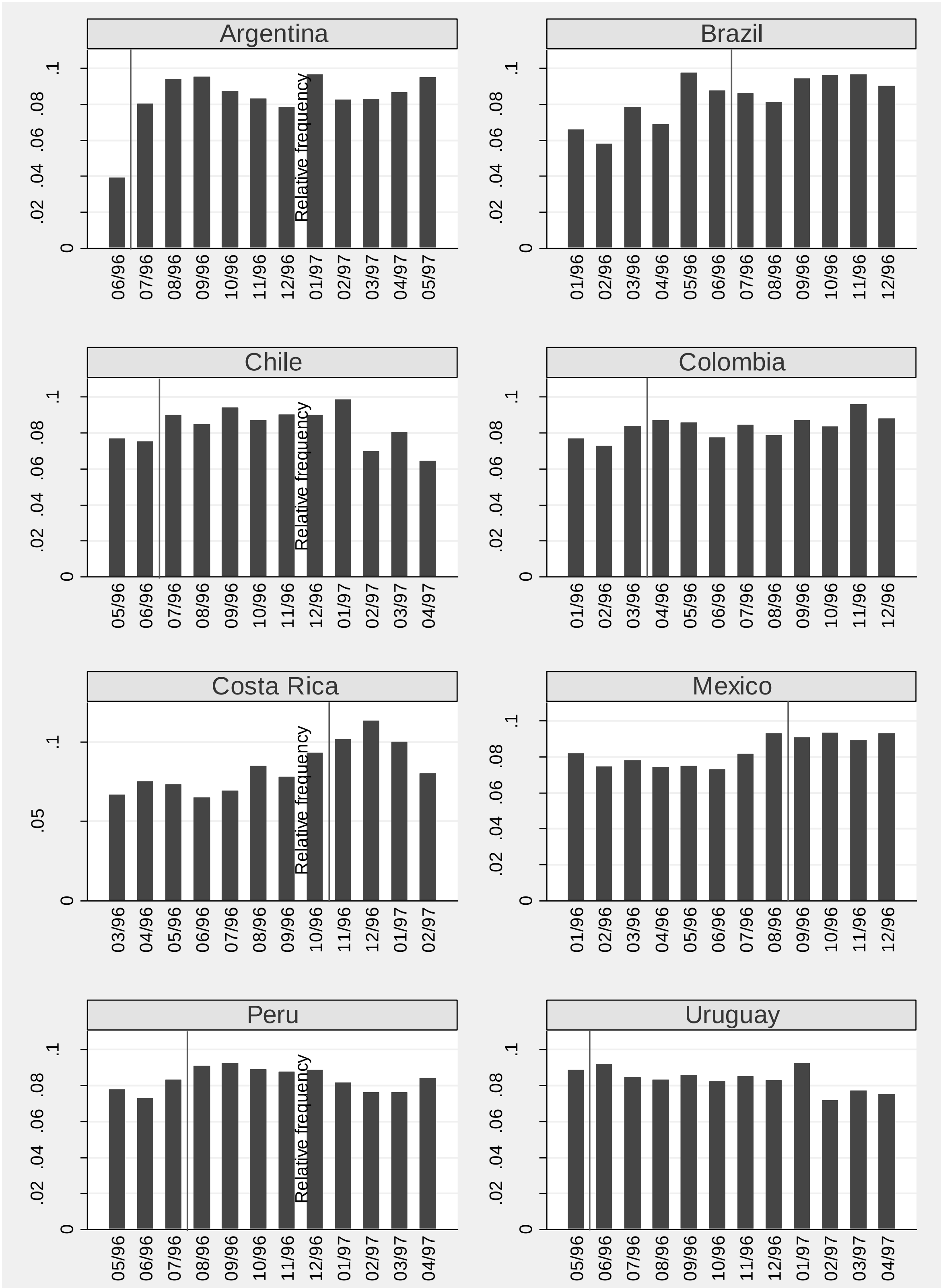
Source: authors' own calculations based on PISA 2012 data bases.

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.

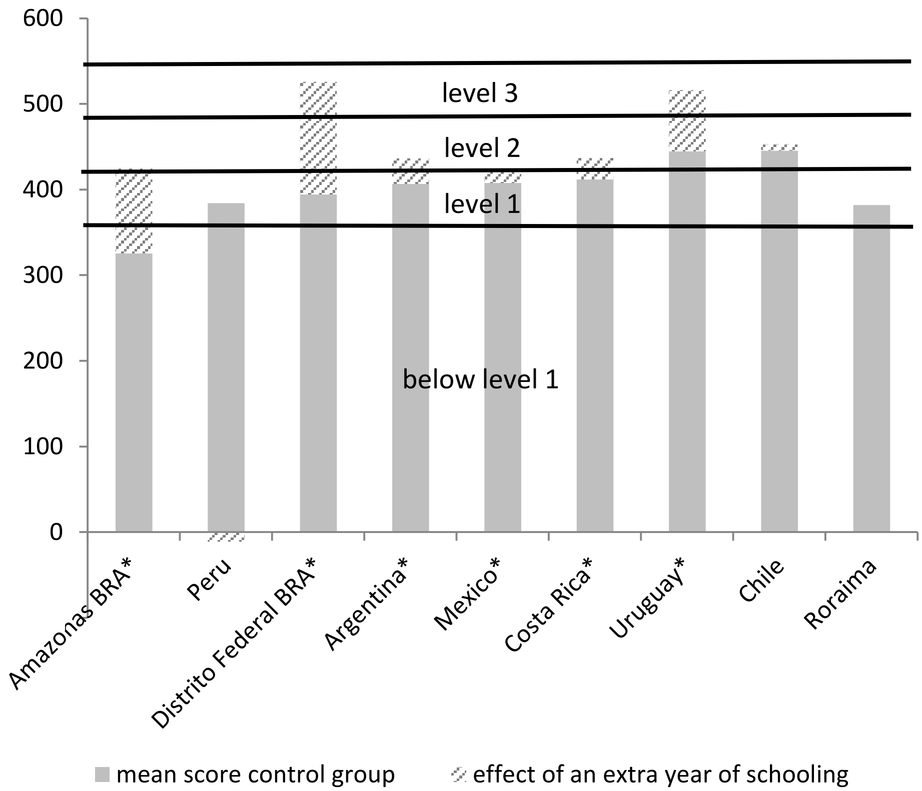
Figure 4. Histogram of the variable date of birth – Latin American countries in PISA 2012. Compliers and noncompliers.



Source: authors' own calculations based on PISA 2012 data bases.

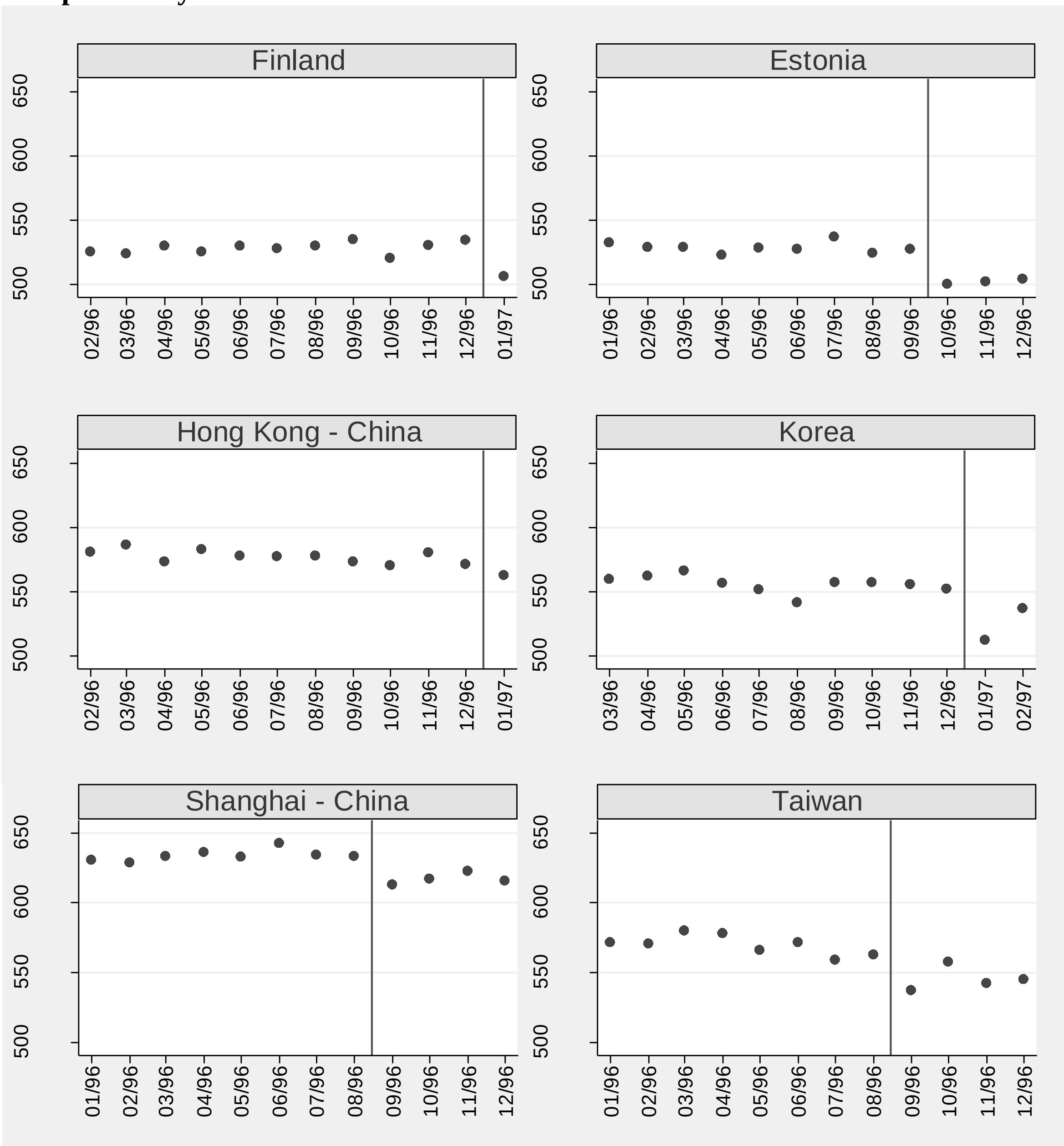
Figure 5. Mean score and the contribution of an extra year of schooling

Fuzzy RD estimates for compliers in the lower grade



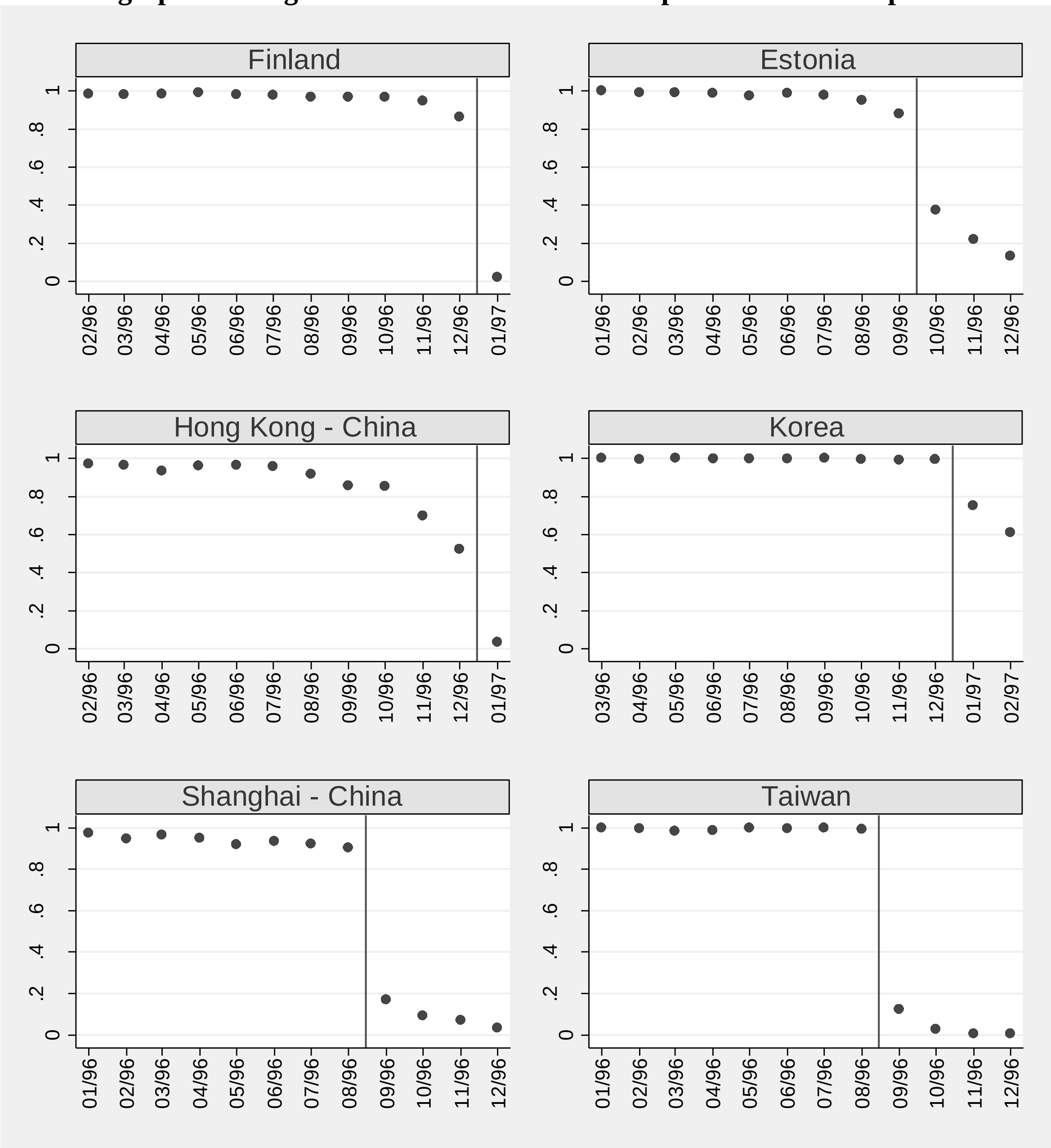
Source: authors' own estimations based on PISA 2012 data bases.
Notes: (a) *statistically significant. (b) Estimates using a 1-month bandwidth. (c) Controls include gender, preschool attendance, and family and school socioeconomic level. (d) Proficiency Level 1: scores higher than 358 but lower than 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.

Figure 6. Mean score in mathematics by birthdate in High-performing countries - Compliers only



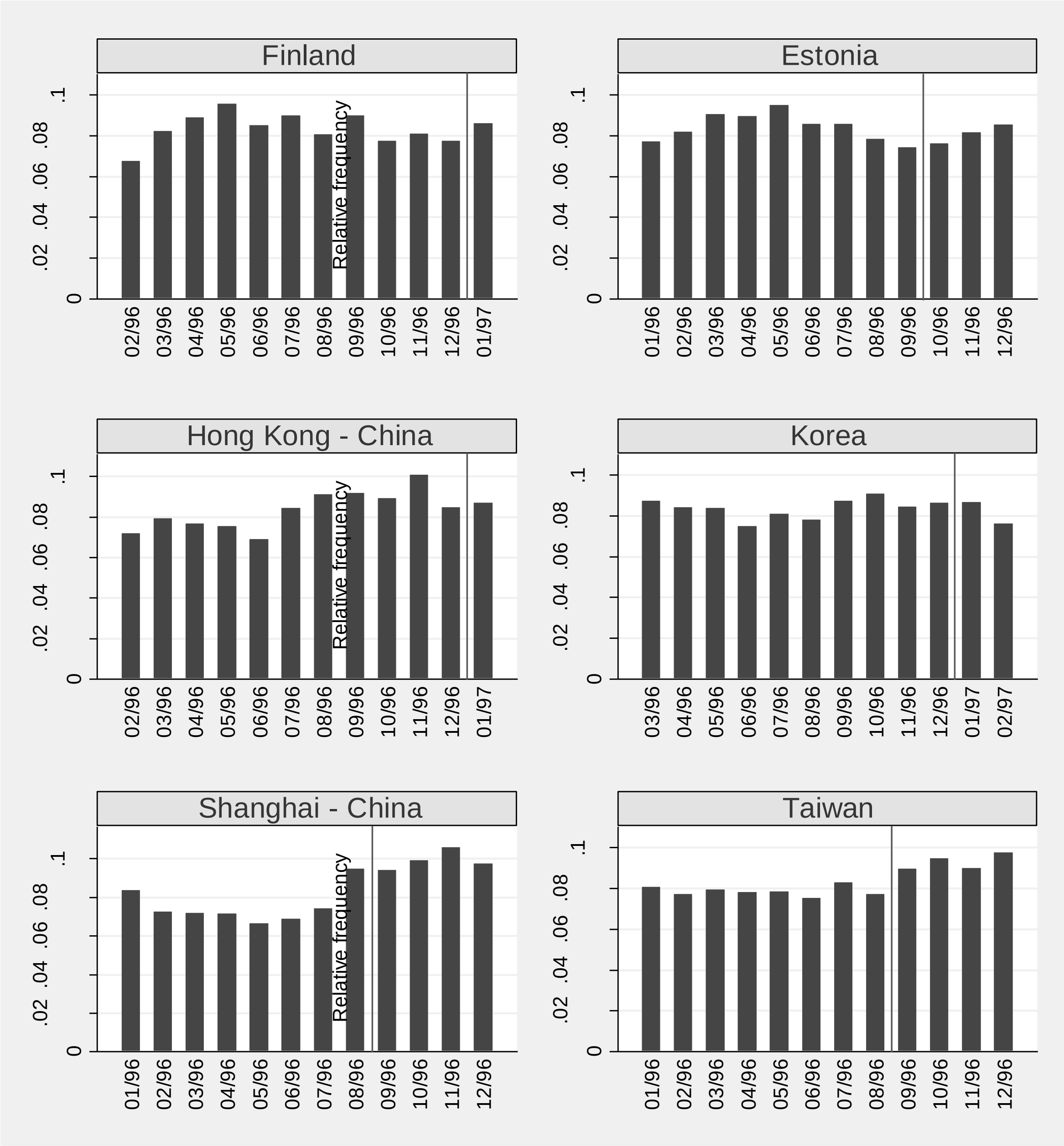
Source: authors' own calculations based on PISA 2012 data bases.

Figure 7. Probability of treatment (having an extra year of schooling) by month of birth - High-performing countries in PISA 2012. Compliers and noncompliers.



Source: authors' own calculations based on PISA 2012 data bases.

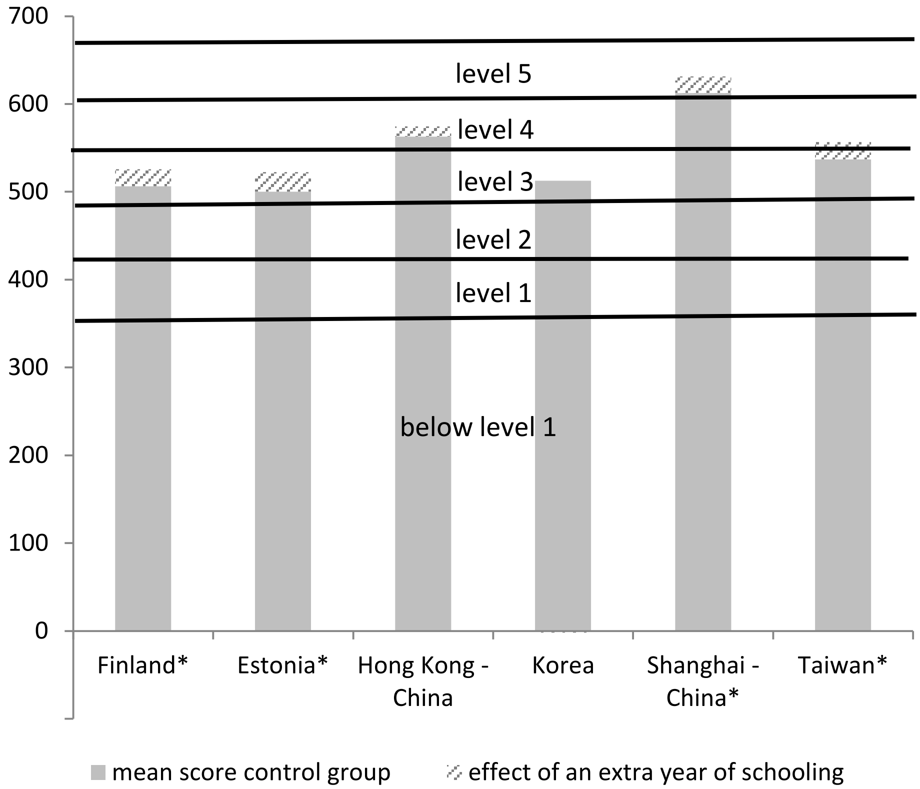
Figure 8. Histogram of the variable date of birth – High-performing countries in PISA 2012. Compliers and noncompliers.



Source: authors' own calculations based on PISA 2012 data bases.

Figure 9. Mean score and the contribution of an extra year of schooling. High-performing countries in PISA 2012.

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 1-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; Level 4: scores higher than 545 but lower than or equal to 607 points; Level 5: scores higher than 607 but lower than or equal to 669 points.