



Let's stay together: The effects of repeat student-teacher matches on academic achievement[☆]

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ABSTRACT

We explore the effectiveness of repeating the student-teacher match on test scores, for the universe of 8th graders in Chile using information on all student-teacher matches across multiple subjects and years, and a national, anonymous measure of test scores. Using a fixed effect and a regression discontinuity approach, we find that repeating matches has a robust positive effect on test scores. We show that this positive effect aggregates up to the student, class, and school-level, and also has longer-term effects on university admission exams. As channels, we find a significant positive effect on attendance, progression, student behaviour and teacher expectations. Reallocating teachers to classes with which they are familiar appears to offer a feasible strategy to improve student performance at low cost.

1. Introduction

Each year, school managers must allocate teachers to groups of students. Consider a school with two maths teachers, and two groups of students who progress from grade 7 to grade 8. Each teacher could specialize in a particular grade: teacher 1 takes both groups in grade 7, and teacher 2 takes both groups in grade 8. Under this allocation, all students are matched with a new teacher in grade 8. An alternative arrangement is to repeat the student-teacher match, which is called “looping” in the educational literature. Under this allocation, each teacher is assigned to a single group of students which they teach in both grade 7 and 8. Students who remain in the same group between grades will be matched with the same teacher in both grades. Students who change group between grade 7 and 8 will be matched with a new teacher, but will typically still be in a group in which most students have the same teacher in both grades. Does looping have any impact on student achievement? If yes, how and through which mechanisms? This paper attempts to provide answers to these questions.

Understanding the effect of looping is important for at least two fundamental reasons. First, it is widely used in some school systems

around the world. Although systematic quantitative evidence on the prevalence of looping does not appear to be available, it seems to be widespread in German elementary schools (Zahorik & Dichanz, 1994), in Chinese schools at all levels (Liu, 1997) as well as in Finland, Japan, Sweden, Israel and Italy (Tourigny, Plante & Raby, 2019). In the case we study, Chile, over 50% of students progressing from year 7 to 8 have the same teacher in both grades. Thus, measuring the effect of looping-based teacher-student allocations on student outcomes is potentially of great importance. Second, repeating student-teacher matches only requires a re-assignment of existing teaching resources without significant additional costs. Thus, if it works, looping can be a budget-neutral way to improve student achievement, which, arguably, is particularly relevant in the context of developing economies.

The only other formal evaluations of repeat matches in a large-scale setting both come from the US, in a setting in which looping is infrequent. Hill and Jones (2018) assess the impact of repeat matches on academic achievement in elementary public schools in North Carolina, and Hwang, Kisida and Koedel (2021) consider the effects in elementary and middle schools in Indiana. Both use similar fixed-effect approaches which leverage the fact that the same students and teachers are observed

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over time.¹ We reinforce and build on their findings by exploiting data on the universe of Chilean students and teachers over a longer period in a setting in which looping is very common. To the best of our knowledge, there is no evidence of the effectiveness of looping at the national level or in the context of any country outside the US. Furthermore, we present the first evidence that the positive effect of looping is robust to relaxing the assumption of no selection on match quality. We also provide the first evidence that looping has longer-run effects on outcomes at the end of a student's school career, and we show that the positive effects of looping aggregate to student, class and school level. Finally, we contribute to the understanding of the mechanisms at play by showing that repeat matches are associated with more attendance and progression, better student behaviour and higher teacher expectations.

We use rich, comprehensive student-teacher data to explore the effect of repeating the student-teacher match on students' test scores for 8th graders in Chile. Unusually, we have information on all student-teacher matches across multiple subjects and multiple years, and we have a national, anonymous measure of student test scores which is uncontaminated by any teacher or school biases in grading. However, even with these data, estimating the causal effect of repeating the student-teacher match is challenging, for two reasons. First, because of non-random selection into repeat matches. Looping may be more common for certain types of school, teacher or student. Furthermore, student-teacher matches which are successful in one year may be more likely to repeat. Second, even if one could randomly allocate repeat matches, those matches will tend to have more experienced teachers because, in order to repeat a match, the teacher must have taught at the same school in the previous year, while new matches are drawn from a pool which includes teachers who are recently hired.

We explore two different complementary research strategies that deliver the same qualitative findings. First, we control for selection by schools, teachers or students by exploiting within-school, within-student and within-teacher variation in repeat matches which occurs across subjects and across time. Further, because the same teachers are observed in multiple classes in the same year, we can make within-teacher-by-year comparisons to control for the resulting experience gap. Using these fixed-effect methods we find that repeating a match increases student performance by about 0.02 standard deviations. This is equivalent to the effect of improving teacher quality by 0.1–0.2 standard deviations.² Value-added specifications yield similar results.³

Fixed-effects and value-added methods do not fully mitigate the concern that school managers (or teachers) might decide to repeat matches based on the performance of existing matches. We therefore provide new evidence that residual performance measures from the earlier grades have almost no explanatory power for the formation of new matches. In addition, we use a regression discontinuity (RD) design which arises because of large differences in retention which occur either side of the legal retirement age. Using this design, we obtain larger but less precise estimates of the benefit of repeating student-teacher matches. The RD estimates suggest that our fixed-effects estimates are not biased upwards by positive selection. The results from our different research strategies point in the same direction: repeating matches has a robust positive effect on test scores. This finding extends to different grades, different subjects and different kinds of teachers. In contrast to policies attempting to increase teacher quality, looping appears to be a straightforward and far more cost-effective policy, being essentially budget-neutral.

The positive effects we observe at the student-subject level may be misleading if there is substitution of a fixed amount of effort by each student towards subjects with familiar teachers, at the expense of subjects with new teachers. Student-subject level estimates may also overstate the benefit if schools face additional costs to implement additional repeat matches. We therefore investigate whether the effects of repeat matches aggregate up to the student, class or school level. Reassuringly, we find student, class and school-level estimates are all slightly larger than the equivalent student-subject level estimates, providing evidence that a reallocation of teachers towards classes with which they are familiar will improve students' test scores on aggregate. Our data also allow us to follow students up to the end of their school career, and we are therefore able to provide new evidence that the effects of looping accumulate over a students' school career, culminating in better university selection test scores.

We explore several potential channels through which looping may improve student outcomes. Using evidence from a survey of teachers, we assess the effect of repeat matches on the learning environment at the class level. Educational research has emphasized the positive relationship between school effectiveness and a co-operative school environment. The literature has shown that a positive and sustained school climate⁴ is correlated with higher levels of students' motivation and engagement, school attendance, graduation rates and teacher retention (Thapa et al (2013)). In addition, recent studies (Bryk, Sebring, Allensworth, Easton & Luppescu, 2010; Klugman, 2017; Kraft, Marinell & Shen-Wei Yee, 2016) have established a positive causal impact of school climate on students' achievement on standardized test scores. We find that in classes with more student-teacher matches, students have higher attendance, teachers report better classroom behaviour and have higher expectations of their students' academic potential. Our finding that the student-subject level effect is slightly smaller than the student, class, or school-level effect is consistent with the notion that greater student-teacher familiarity has a positive effect on school climate which may affect outcomes of students who do not themselves repeat the match, such as those who join a classroom where the other students are looping.

Repeating student-teacher matches necessarily implies greater student-teacher familiarity. In this sense, our analysis is related to Fryer (2018), who investigates the effect of teacher specialization by subject, and finds that specialization decreases students' achievement and attendance, and increases student behaviour problems. Fryer suggests that these findings could be explained by the decrease in interactions between teachers and students, caused by teachers' subject specialization. Our findings support this view in a different context, from a different policy, and provides complementary evidence on how student-teacher familiarity manifests in better classroom behaviour.

A recent literature emphasizes complementarities between teacher and student characteristics (e.g. Aucejo, Coate, Fruehwirth, Kelly & Mozenter, 2018; Bassi, Meghir & Reynoso, 2020; Graham, Ridder, Thiemann, & Zamarro, 2020). This implies that improving teaching-to-classroom assignments may lead to better student outcomes. Graham et al. (2020) experiment with different assignments to show that overall achievement in elementary schools in the US can increase by around 0.02 standard deviations without changes to existing teaching resources. Of course, a precise performance-improving assignment of teachers to classrooms requires information that it is not readily available to school managers. Our paper complements these findings by providing a simple and feasible assignment rule that delivers results which are at least as large.

A number of qualitative and small-scale quantitative studies in the educational literature have investigated the effectiveness of looping in

¹ See also Wedenoja, Papay and Kraft (2022) for a third recent study on looping using state-wide data from Tennessee.

² Using estimates from Rivkin, Hanushek and Kain (2005) and Rockoff (2004).

³ As Muralidharan and Sheth (2016); Rivkin et al. (2005); Harris and Sass (2011) or Chetty et al. (2014a), we include as a control lagged test scores at the student-subject level.

⁴ The school climate reflects the quality of the relations between the members of the educational community. For a comprehensive review on school climate literature, see Thapa, Cohen, Guffey and Higgins-D'Alessandro (2013).

some schools in advanced countries, including Bogart (2002), Cistone and Shneyderman (2004), Nichols and Nichols (2002), Tucker (2006) and Franz, Thompson, Fuller, Hare, Miller and Walker (2010). Cistone and Shneyderman note that looping is widespread in primary schools in certain countries, including Germany and Japan, but rarely used in others. Most of these studies consider elementary schools: Kerr (2002) stresses that very few studies consider effects on older children. These studies overwhelmingly argue that looping improves student outcomes. For example, Cistone and Shneyderman (2004) find that looping improved student attendance and increased the rate at which students progressed successfully to the next grade. It is commonly suggested that looping has these positive benefits because it saves considerable time at the start of the new school year. Cistone and Shneyderman argue that looping “allows teachers to save time at the beginning of the second year of the loop by making unnecessary the usual transitional period typically spent on getting acquainted with new students as well as setting classroom rules, expectations, and standards.” The same idea is also argued by Burke (1996), Little and Dacus (1999) and Black (2000). A teacher cited by Little and Dacus (1999, p.43) explains: “Gone were the lectures about daily procedures and classroom rules. Gone were the weeks of testing, trying to determine a student’s reading level. The teachers and students started the year with a bang and ended further along than the teachers had anticipated.” It is argued that looping allows teachers to build closer relationships with the students and parents, along with a better understanding of the strengths, weaknesses and personalities of their students. Looping also allows teachers to implement a smooth transition across grade levels and develop a more cohesive curriculum.

The literature recognizes that looping may also have disadvantages. First, teachers may find it more difficult to teach a multi-year rather than single-year curriculum. Second, teachers may lose grade-specific human capital, which Ost (2014) finds contributes up to one-third as much as general teaching experience, at least for maths scores. Finally, even if repeat matches are more efficient, they may also increase inequality in student outcomes, because, as noted by Bogart (2002), some unlucky students will spend two or more years with an ineffective teacher. Assigning students to new teachers each year mitigates these inequality concerns. The possibility of potential negative effects of looping reinforces the importance of systematic analyses at the country level to inform policy.

This educational literature provides useful insights on how looping may affect the learning process but does not provide a systematic assessment of its overall causal effect. Our paper is a contribution in that direction, and we show that the benefits of looping outweigh the costs, at least on average.

The remainder of the paper is organized as follows. Section 2 describes our data and the relevant institutional features of the Chilean school system. Section 3 explains the econometric framework and estimates the effect of repeat student-teacher matches at the student-subject level. We begin with fixed-effects methods which maintain the assumption that selection into repeat-matches is exogenous to the quality of existing matches. We then relax this assumption by exploiting the discontinuity at the LRA as a source of exogenous variation in repeat match formation. In Section 4 we estimate the effects of repeat matches at the student, class and school level, which may be more informative as to the effectiveness of a policy of repeating student-teacher matches, since there may be spillover or substitution effects within and between students. In Section 5 we investigate whether looping has effects which accumulate over a student’s school career by looking at impacts on University selection exams. In Section 6 we report the results from a large-scale teacher survey which support the hypothesis that repeat matches improve behaviour in the classroom and raise teacher expectations of future student performance. Section 7 concludes.

2. Data and institutional background

We use three different datasets provided by the Chilean Ministry of

Education. First, we use the complete school enrolment records of all students in Chile from 2002 onwards. The database contains yearly information on the students enrolled in primary school (grades 1 to 8) and high school (grades 9 to 12). These records contain a consistent student ID, a school ID and a “class” ID. In Chilean schools, a class is a fixed group of students who take subjects together: every student in our sample is in the same group (class) in grade 8 for all four subjects we consider. The enrolment records include individual school grades (awarded by teachers) in each subject and the individual attendance rate. The grading system in Chile is 1 to 7 in increments of 0.1, and schools are free to set their own grading standards. To make school grades comparable, we standardize school grades at the school level.⁵

Second, we use comprehensive teacher administrative records. These records contain information on teacher gender, age and experience. The teacher data includes the same class ID as in the enrolment records, which allows us to associate each class of students in each subject with a teacher in each year. The enrolment records matched to the teacher records allow us to measure whether a student has the same teacher in a subject for successive years.

Third, we use data on students’ achievement in *Sistema de Medición de la Calidad de la Educación* (SIMCE) tests. This is a standardized test administered by the Ministry of Education to all students in certain grades and is the main instrument to measure the quality of education in Chile. The SIMCE is administrated by external examiners and provides information about students’ performance relative to the country’s National Curriculum Framework. We use standardized test scores for 8th graders in four years: 2004, 2007, 2009 and 2011, in four different subjects: Spanish, maths, social sciences and natural sciences.⁶ In these three years, SIMCE tests were taken by 1,056,458 students, 97.8% of the students enrolled in grade 8, covering 98.4% of schools in operation.⁷

The SIMCE data also contain information on school characteristics (including whether a school is public or private) and information from surveys of parents and teachers. The parents’ survey provides information on family socio-economic background, including mother’s schooling and monthly household income (banded). For 2009 and 2011, the teachers’ survey provides information about perception of classroom behaviour and the future performance of the class. Teachers complete a separate survey for each class they teach.

We therefore have information on students $i = 1 \dots N$ who are each observed in one of four different years ($t = 2004, 2007, 2009, 2011$) in grade 8. Each student has SIMCE test scores in four subjects $s = 1, 2, 3, 4$. Students are grouped together in classes c . A class-subject combination has a specific teacher j , school k and year t . We start with a sample of 789,270 students. After excluding observations without valid test scores, student or teacher characteristics, we are left with a sample of 696,482 students, 46,256 teachers, 31,837 classes and 6,260 schools. Overall, the estimation sample represents 76.3% of the students enrolled in grade 8 who took all the SIMCE tests. Information from teachers about classroom behaviour and future class performance is available for 9,498 classes for each of the four subjects.

A *repeat match* takes place when a student has the same teacher in the

⁵ School grades are not our main outcome measure because they may reflect teacher biases as well as student performance (Contreras, 2019). However, in Appendix A we show that looping has much larger effects on school grades than on anonymized test scores.

⁶ We focus on grade 8 in these four years because we have information on all four subjects’ SIMCE test scores, and we exploit the variation across subjects. In Appendix A we examine the robustness of our results by estimating looping effects for all grades in which we have any SIMCE test score information.

⁷ The SIMCE test is not taken by students in special education or adult education. In addition, there are cases in which the test cannot be taken because schools are closed temporarily or because individual students cannot attend. Cuesta et al. (2020) find that high-performing students are more likely to take the SIMCE test, and that the size of this effect varies across school. Our findings, however are based on a within-student design.

Table 1
Descriptive statistics.

	Mean	Std. Dev.
<i>(a) Student-subject level i,s ($N = 2,785,928$)</i>		
SIMCE test score	0.00	1.00
1=Repeat match grade 7–8	0.58	0.49
1=Repeat match grade 6–7	0.41	0.49
<i>(b) Student level i ($N = 696,482$)</i>		
1=Repeat match (Spanish)	0.57	0.50
1=Repeat match (Mathematics)	0.59	0.49
1=Repeat match (Natural Sciences)	0.59	0.49
1=Repeat match (Social Sciences)	0.58	0.49
Number of repeat matches	2.32	1.30
1=Female	0.51	0.50
Mother's schooling (years)	10.95	3.75
Household's monthly income (000 s of CLP)	376.02	468.90
Past GPA	0.09	0.95
Past attendance rate (%)	94.40	5.81
Class size	26.68	8.47
<i>(c) Teacher level j ($N = 46,256$)</i>		
1=Female	0.68	0.47
Experience (average)	16.34	12.53
Age (average)	43.59	11.80
<i>(d) School level k ($N = 6,260$)</i>		
1=Public	0.50	0.50
1=Voucher	0.42	0.49
1=Private	0.07	0.26
1=SES 1 (Low)	0.25	0.43
1=SES 2 (Middle-low)	0.33	0.47
1=SES 3 (Middle)	0.23	0.42
1=SES 4 (Middle-high)	0.12	0.33
1=SES 5 (High)	0.07	0.25
1=Urban	0.73	0.44
School enrolment (average)	436.90	402.15
Number of teachers (average)	19.30	14.17
<i>(e) Class-subject level c,s ($N = 37,992$)</i>		
1=Problems to start the class	0.34	0.47
1=Classroom disruption	0.44	0.50
1=High teacher expectation	0.55	0.50

Notes: Sample comprises students in grade 8 in 2004, 2007, 2009 and 2011 who have valid test scores and a complete set of information on characteristics. Household monthly income is imputed from the mid-point of 15 income bands with widths of 100,000 CLP or 200,000 CLP. The class-subject information in panel (e) is only available for a subset of 9,498 classes out of 31,837 classes in total.

same subject as in the previous academic year. In our basic specification, we do not consider repeat matches to occur if a student has the same teacher in consecutive years, but not in the same subject. We also do not consider repeat matches to occur if a student returns to the same teacher after a gap.⁸

Students may repeat a grade due to academic failure. Grade retention depends on the students' performance during the school year, as well as their attendance rate. Grade retention is rare: about 1.8% of the students in grade 8 are repeating the grade. We do not exclude grade repeaters from our analysis because we implement a within-student comparison, as explained in Section 3.

Table 1 presents descriptive statistics. Panel (a) shows that the outcome (SIMCE test score) and treatment (repeat match) are measured

⁸ Both are infrequent cases. In the sample, 88.9% of the total matches occur in the same subject, and 97.2% of repeat matches have no gap. In Section 3, we also examine whether repeat matches with teachers in a different subject has an effect on test scores.

at the student-subject level in grade 8. Repeat matches are common in grade 8 of Chilean schools; in the estimation sample, 58% of the observations have a repeat match.⁹ In our setting repeat matches are typically associated with the whole class (or a large fraction of students in a class) moving together with the same teacher into grade 8, and repeat matches are therefore effectively synonymous with the definition of looping used in the literature.¹⁰ Panel (a) also shows that repeat matches are less common between grades 6 and 7 (41%) than between grades 7 and 8.¹¹ There are no substantial differences in the frequency of repeat matches by subject, shown in panel (b). Because each student has a probability of a repeat match of 0.58 in each subject, 8th graders can expect to have a repeat teacher in 2.32 of their four subjects. For each student we also observe sex, family background, past GPA, past attendance rate and class size in grade 8. In panel (c) we report information at the teacher level, which includes sex, age and experience. Teachers' experience and age correspond to the average across the four years.¹²

In panel (d) we report information at the school level including size according to enrolment and number of teachers. Schools in Chile may be one of three types: public, private but supported by vouchers and unsupported private.¹³ Schools are classified by the Ministry of Education according to the socio-economic status (SES) of their students, based on four variables: father's level of education, mother's level of education, monthly family income and a vulnerability index of the students. The variable ranges between 1 and 5, 5 being indicative of the wealthiest students. Finally, in panel (e) we show information from the SIMCE survey about teachers' perceptions of classroom behaviour¹⁴ and their expectations of their students in the future.¹⁵

In Table 2 we show how the characteristics of the treatment and control groups differ. The raw difference in test score is very small, but repeat matches are positively associated with several factors correlated with worse academic performance, including lower family income and lower previous test scores. Panel (a) shows that repeat matches in grade 8 are themselves correlated with repeat matches in grade 7, which may reflect differences at the school-level in terms of policy towards repeat matches. However, the distribution of repeat matches does not suggest that looping is primarily a school-level policy. Two-thirds of students have variation in repeat matches across subjects (which by definition are taken within the same school). Only 15% of the variation in the proportion of repeat matches at the school-subject-grade-year level is accounted for by school fixed effects, and very few schools never use repeat matches.

⁹ Grade 8 is the final year of primary education, and students will typically move to a different school and have different teachers in grade 9. Students typically remain in the same school between grades 5 and 8, and therefore repeat student-teacher interactions will be common in grades 6, 7 and 8.

¹⁰ On average, students that have a repeat match are in classes in which 93% of their classmates are also in a repeat match.

¹¹ We cannot identify repeat matches between grades 5 and 6 for the entire sample because we do not have enrolment data for 2001.

¹² In the estimation sample teachers are observed a different number of times across the four years: 52% (24,271 teachers) are observed once; 24% (11,276 teachers) are observed twice; 14% (6,558 teachers) are observed three times, and 9% (4,151 teachers) are observed four times.

¹³ For a detailed description of the Chilean school system and education providers, see Santiago et al. (2017).

¹⁴ Teachers were asked about how much they agree or disagree with the following statements: "In this class, it is very hard to start the class lessons" and "In this class, the lessons are often interrupted because I must silence or scold students". The rating scale is "I fully agree", "I agree"; "Disagree", "I entirely disagree". Both variables were coded as dummy variables, taking value of one if the teacher answers "I fully agree" or "I agree" and zero otherwise.

¹⁵ Teachers were asked "What do you think will be the highest level of education that most students in this class will achieve in the future?". The variable was coded as a dummy variable, taking value of one if the teacher expects that the majority of the class will complete higher education studies and zero otherwise.

Table 2

Characteristics of treatment and control groups Treatment group (same teacher in grade 8).

	Treatment group (same teacher in grade 8)	Control group (new teacher in grade 8)	Difference	Std. err.
SIMCE test score	0.001	−0.002	0.003***	(0.001)
<i>(a) Previous repeat matches</i>				
1=Repeat match grade 6–7	0.47	0.32	0.154***	(0.001)
<i>(b) Student characteristics</i>				
1=Female	0.51	0.50	0.001	(0.001)
Mother's schooling (years)	10.74	11.26	−0.521***	(0.005)
Household's monthly income	342.37	422.66	−80.287***	(0.567)
Past GPA	0.11	0.06	0.050***	(0.001)
Past attendance rate (%)	94.62	94.09	0.536***	(0.007)
Past SIMCE test score	0.15	0.21	−0.058***	(0.002)
Class size	26.94	26.33	0.613***	(0.010)
<i>(c) School characteristics</i>				
1=Public	0.55	0.44	0.110***	(0.001)
1=Voucher	0.41	0.49	−0.079***	(0.001)
1=Private	0.05	0.08	−0.032***	(0.000)
1=SES 1 (Low)	0.11	0.09	0.027***	(0.000)
1=SES 2 (Middle-low)	0.34	0.30	0.044***	(0.001)
1=SES 3 (Middle)	0.35	0.35	−0.000	(0.001)
1=SES 4 (Middle-high)	0.15	0.19	−0.037***	(0.000)
1=SES 5 (High)	0.05	0.08	−0.033***	(0.000)
1=Urban	0.88	0.91	−0.035***	(0.000)
School enrolment	698.74	820.18	−121.432***	(0.741)
Number of classes	20.09	23.34	−3.248***	(0.018)
Number of teachers	26.29	31.01	−4.722***	(0.023)
Number of subject-teachers	2.66	3.20	−0.542***	(0.002)
<i>(d) Teacher characteristics</i>				
1=Female	0.69	0.68	0.011***	(0.001)
Experience in 7th grade	20.06	16.93	3.124***	(0.015)
Experience in 8th grade	21.06	15.37	5.694***	(0.014)
Δ Experience	1.00	−1.57	2.570***	(0.012)
Age	47.51	42.55	4.962***	(0.013)
Observations	1,618,387	1,167,541		

Notes: The past SIMCE test score is the SIMCE score from grade 4 and is based on 338,941 and 440,192 observations in the control and treatment groups respectively. All comparisons are at the student-subject level. The number of subject-teachers is based on the number of teachers in the school between grades 5 and 8, because the majority of the teachers from the first cycle (grades 1–4) are general teachers, and they teach all the main subjects to a particular class. In the case of the four years analysed (2004, 2007, 2009, 2011), 95% of the teachers from the first cycle teach more than one subject. In contrast, 44% of the teachers from grades 5 to 8 are subject specialist and teach only one subject. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel (b) shows that students who have repeat matches come from lower-income families with less-educated mothers. Repeat matches are positively selected on those measures of academic effort and achievement which are observable by the teacher: past GPA and past attendance rate are both higher for repeat matches. However, repeat

matches are *not* positively selected on the anonymized SIMCE test score.¹⁶

Panel (c) of Table 2 shows that repeat matches are significantly more common in public schools, in low socio-economic status schools and in rural schools. There are also important differences in terms of school size and structure, some of which are mechanically related to the probability of repeat matches. Students in smaller schools in terms of enrolment, number of classes, number of teachers and number of teachers per subject are all more likely to have repeat matches. Holding other factors constant, a reduction in the number of teachers who are available to teach a particular subject will increase the probability of repeat matches.

Panel (d) shows that repeat matches have significantly older and more experienced teachers. Compared to new matches, repeat matches have teachers who had 3.1 years more experience in grade 7 (i.e. before the current match). This increases to 5.7 years more experience in grade 8. More experienced teachers are more likely to get repeat matches, and, by definition, repeat matches have a teacher with one more year of experience than in the previous year. In contrast, new matches draw a new teacher who has about 1.5 years less experience than their teacher in the previous year. This arises because, by definition, teachers who have repeat matches in grade 8 must have worked at the school in grade 7, whereas new matches may draw a teacher who is new to the school.

3. The effect of repeat matches at the student-subject level

As shown in Table 2, a simple comparison of repeat matches and new matches may be misleading because repeat matches are not randomly assigned: repeat matches have systematically different students, teachers and schools. These differences may arise because of teacher and student sorting within schools, and because of teacher and student mobility between schools. Previous research has established the existence of teacher sorting within schools: less-experienced, minority and female teachers are systematically sorted to lower-performing students (Clotfelter, Ladd & Vigdor, 2005, 2006; Feng, 2010; Kalogridis, Loeb & Bételle, 2013). Moreover, qualitative research shows that school leaders base their staffing decisions on a combination of teachers' performance (measured by their students' test scores) and teachers' preferences (Cohen-Vogel, 2011; Kalogridis et al., 2013; Osborne-Lampkin & Cohen-Vogel, 2014). Teacher and student mobility between schools may also cause differences in the proportion of repeat matches, and it seems likely that the decision to move schools will not be exogenous with respect to student outcomes.

Given these differences, it is important to note that we observe the same student (by definition in the same school) in multiple subjects, some of which are repeat matches and some of which are new matches, and we observe the same teacher with multiple classes,¹⁷ some of which are repeat matches and some of which are new matches. This enables us to control both for unobserved fixed student effects and unobserved fixed teacher effects, which greatly reduces any concerns about selection on the basis of these characteristics.¹⁸ In addition, since students attend the same school and the same class for all subjects, student fixed effects will also control for selection bias as a result of differences in school or class characteristics. The inclusion of student fixed effects also addresses

¹⁶ The SIMCE test is taken every year in grade 4, from 2005 onwards. Therefore, past SIMCE test scores are only available in 2009 (grade 4 in year 2005) and 2011 (grade 4 in year 2007). Grade 4 SIMCE scores are only available for three of the four subjects (Spanish, maths and natural sciences). As with current SIMCE test scores, scores in grade 4 are standardized to have mean zero and unit variance.

¹⁷ A small fraction of teachers are observed in more than one school.

¹⁸ Many cross-sectional studies exploit within-student variation to identify effects of teacher characteristics and teaching practices (Dee, 2007; Clotfelter et al., 2010; Bietenbeck, 2014; Bietenbeck et al., 2018; Paredes, 2014; Lavy, 2015; Comi et al., 2017).

Table 3

Effect of repeat student-teacher match on test scores: fixed-effect estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
Repeat match grade 7–8	0.003**	0.026***	0.017***	0.019***	0.021***	0.020***
$R_{is} = 1$	(0.002)	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)
SIMCE score in grade 4					0.276***	0.276***
					(0.002)	(0.002)
$R_{is} = 1$ grade 6–7						0.014***
						(0.003)
$R_{is} = 1$ grade 5–6						0.007***
						(0.002)
$R_{is} = 1$ grade 4–5						0.006
						(0.004)
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes
Student FE		Yes	Yes	Yes	Yes	Yes
Teacher FE			Yes			
Teacher FE x Year FE				Yes	Yes	Yes
R-squared	0.000	0.793	0.808	0.812	0.849	0.849
Observations	2,785,928	2,785,928	2,785,928	2,785,928	759,597	759,597

Notes: dependent variable is the student's SIMCE test score in grade 8. In all columns, treatment is the student-subject measure of repeat match R_{is} in grade 8. Standard errors are clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

two specific sources of selection bias: parental choice of school and grade retention. First, parents' decision whether to move their child to another school could lead to a selection issue if parents take this decision based on, for instance, how well their children are matched with their teachers in a particular school. In the estimation sample 7.8% of the students change school between grade 7 and grade 8. Second, students who repeat the grade due to academic poor performance are significantly less likely to have a repeat match. In the estimation sample, about 1.8% of the students are grade repeaters, of which 65.7% do not have the same teacher again. Grade repeaters are more likely to come from low-income families, to have less educated mothers, and to have lower test scores. The inclusion of student fixed effects deals with both these potential biases, since children attend the same school for all subjects, and grade repeaters re-take all subjects.

As well as addressing selection bias, the inclusion of student fixed effects allows us to estimate the effectiveness of repeat-matches independent of any effect of a group of students staying together between grades. It seems possible that student-student familiarity (in addition to student-teacher familiarity) has a causal effect on student outcomes, and the process of assigning the same teacher to a group of children necessarily implies that the group (or at least the majority of the group) stay together between grades. The fixed-effect strategy we use compares the same student across subjects in the same year, and this student will have the same classmates for all subjects, so we are effectively comparing outcomes for the same group of students, some of whom have a repeat match and some of whom do not.

Our method also allow us to control for differences in fixed teacher characteristics by using the within-teacher variation across classes, taking advantage of the fact that we observe the same teacher in several classes.¹⁹ Further, and in contrast to students, we observe the same teacher in multiple classes at four different points in time (2004, 2007, 2009 and 2011) which allows for the inclusion of teacher-by-year fixed effects. As was clear from Table 2, there is inevitably a strong relationship between repeating the student-teacher match and teacher experience. Even if repeat-match teachers were drawn randomly, these teachers by definition must have worked in the same school at $t - 1$, but new match teachers are drawn from the pool of available teachers which includes those who are new to the school. In addition, repeat-match teachers are not drawn randomly: they have about three more years of experience, on average. Thus, an unconditional comparison of classes

which have a repeat match with those that do not conflates the advantages of a repeat match with any advantages of having a teacher who has nearly six years more experience (see panel (d) of Table 2). Since experience is fixed for a given teacher in a given year, the inclusion of teacher-by-year fixed effects controls for this large difference in experience.

Thus, our first model to identify the effect of a repeat match is:

$$y_{is} = \beta_1 R_{is} + \mu_i + \mu_s + \mu_{jt} + \varepsilon_{is}, \quad (1)$$

where y_{is} is the standardized SIMCE test score of student i in grade 8 in subject $s = 1, 2, 3, 4$ (maths, Spanish, social sciences, natural sciences). Each student is observed in grade 8 in one year $t = 2004, 2007, 2009, 2011$, and therefore i identifies t . For a particular student-subject-year combination we observe the identity $j = J(i, s, t)$ of the teacher. In Eq. (1) each student i appears in only one school in one year, whereas teachers j appear in multiple classes and years and may also be observed in more than one school. R_{is} is an indicator variable which takes the value 1 if there is a repeat match, which occurs if $J(i, s, t - 1) = J(i, s, t)$. As discussed, the model includes student, subject and teacher-by-year fixed effects.²⁰

Table 3 presents estimates of versions of Eq. (1) with the inclusion of different fixed effects. Across all specifications, the results show a positive and significant effect of repeating the student-teacher match on student's SIMCE test scores. The raw effect in Column (1) is small, but recall from Table 2 that repeat matches are far from randomly assigned, and are often associated with baseline characteristics which themselves are associated with lower test scores. Including student fixed effects in column (2) increases the effect to 0.026σ , while the inclusion of both student and teacher effects in columns (3) and (4) reduces the effect to 0.017σ . The inclusion of teacher-by-year fixed effects in column (4) controls for any effect of differential experience between teachers who repeat matches and those who do not and increases the estimate to 0.019σ .²¹ We find no evidence that the size of the effect varies across subjects: an F -test of the interactions between R_{is} and μ_s is insignificantly different from zero. It is also possible to replace the teacher-by-year fixed effects μ_{jt} with teacher-by-subject-year fixed effects μ_{jst} to ensure that we are not conflating looping with an effect from non-looping teachers teaching different (possible less preferred) subjects. The inclusion of μ_{jst} slightly reduces the estimate to 0.016σ .

¹⁹ By exploiting within-teacher and within-student variation, we follow Metzler and Woessmann (2012) who study the effect of teacher subject knowledge in the case of Peruvian 6th-grade students.

²⁰ The model is estimated using the methods developed by Correia (2017) and Guimaraes and Portugal (2010).

²¹ Excluding students who have no variation in R across subjects makes almost no difference, with an estimated effect of 0.018 (0.002).

In column (5), we include as a control lagged test scores at the student-subject level (Chetty, Friedman & Rockoff, 2014a; Harris & Sass, 2011; Muralidharan & Sheth, 2016; Rivkin, Hanushek & Kain, 2005). This is a value-added model which controls for within-student differences in ability across subject which may be correlated with the looping decision. However, the SIMCE test score information for these students is only available in grade 4 and grade 8, so this does not deal with the problem that the decision to loop may be based on match quality in grade 7. The sample in column (5) is significantly smaller because the grade 4 SIMCE score is only available in 2009 and 2011, and only in three of the four subjects.²² The inclusion of lagged SIMCE scores makes almost no difference to the estimate. Finally, in column (6) we deal with the concern that repeat matches may be correlated with earlier looping decisions by including as controls the value of R_{is} in grades 5, 6, and 7. Once again, this makes almost no difference to our estimate of the effect of looping on test scores in grade 8.

Our estimates are very similar to those reported by Hill and Jones (2018) for maths scores in North Carolina elementary schools (grades 3–5), and by Hwang et al. (2021) for English and maths scores in Indiana elementary and middle schools (grades 3–8), both using a similar specification.²³

In Table A1 in Appendix A we examine the robustness of our finding by stacking all years and grades for which we have any SIMCE test score results. This allows us to include test scores in those years in which SIMCE tests took place for grade 6 and grade 10 students. For these additional grades and years we have test scores only for maths and Spanish. Using a similar specification as in Table 3 (but with additional interactions for grade fixed effects) we find that looping has a positive and significant effect, but the estimated effect is somewhat smaller once we include teacher-by-year fixed effects. Table A2 reports estimates for grade 6 and grade 10 students separately. Interestingly, the raw comparison of repeat matches with new matches (shown in column 1 of each table) yields quite different estimates for different grades, but the estimates become far more similar once we include student fixed effects in column 2.

In Table A3 we show that the estimated impact of repeat matches on teacher-assigned classroom grades is significantly larger than the impact on anonymised test scores — the estimated coefficient is approximately 3 times larger than for anonymised test scores using the same sample.²⁴ This could be because increased familiarity causes students to exert more effort in classroom tests, or because increased familiarity causes teachers to increase the generosity of their marking.

Is looping a successful policy for different types of teacher? One argument against looping which is made in the educational literature (e.g. Bogart, 2002) is that students will be “stuck” with a less effective teacher for several years. Hill and Jones (2018) provide some evidence that looping may be more effective for teachers with lower value-added measures, but their estimates are imprecise and they find no significant difference between quartiles of teacher quality. To investigate this, we classify the teachers in our sample using teacher evaluation scores.²⁵ Since 2004, public school teachers in Chile have been evaluated every four years and receive a score based on peer review, a reference from their headteacher and a performance portfolio which is marked blind by

an independent assessor. These evaluation scores are highly predictive of students’ SIMCE test scores.²⁶ We use the total and portfolio evaluation scores as an independent measure of teacher quality, and classify teachers into low, medium and high quality and interact these groups with the looping indicator R_{is} . Table A4 reports results for this interacted model. Looping has positive and significant effects on test scores for all three teacher-quality groups, and there is some evidence that effects are larger for teachers in the medium and high quality groups, but the effect is not monotonically increasing in teacher quality. Our estimates are considerably more precise than those of Hill and Jones, and do not suggest that looping is more beneficial for low quality teachers.

Our definition of a repeat match excluded cases where students form a repeat match with a teacher, but in a new subject. If the benefit of repeat matches comes from student-teacher familiarity alone (rather than student-teacher familiarity in a particular subject) then repeat matches in new subjects might also cause an increase in test scores. In Table A5 in Appendix A we report estimates of variants of Eq. (1) which include R_{i-s} , an indicator for a repeat match in a new subject. For example, $R_{i-s} = 1$ if the student has a teacher for maths in grade 7, and then has the same teacher for science in grade 8. The coefficient on the dummy for “repeat match in a new subject” is also significant and, surprisingly, slightly larger than the coefficient on the dummy for repeat match in the same subject. However, the vast majority (95%) of cases where $R_{i-s} = 1$ also have $R_{is} = 1$. This arises if the student had the same teacher for multiple subjects in grade 7. For example, if the student has a single teacher for maths and science in grade 7 and has the same teacher for maths in grade 8. When we include the interaction term to capture these cases, we find that there is no positive effect for those cases where $R_{i-s} = 1$ but $R_{is} = 0$. This suggests that student-teacher familiarity in the subject is required, but the effect is enhanced by increased familiarity between the student and teacher in the previous year.

We now consider the possibility that repeat matches in grade 8 are formed non-randomly with respect to match quality in grade 7. The remaining source of variation in Eq. (1) is the error term ε_{is} , which varies at the student-subject (equivalent to the student-teacher) level. If repeat matches are formed non-randomly with respect to this “match quality” term, then estimates of β_1 will still be biased even after controlling for student and teacher fixed effects. Schools or parents may both make decisions about which class-teacher matches to keep together in grade 8 on the basis of their performance in grade 7. As a result, class-teacher matches are endogenously destroyed and the effect of a repeat match will be confounded by survivor bias.

SIMCE tests are not taken in grade 7, but we can use information on SIMCE scores in grade 6 to predict match formation in grade 7. To do this, we estimate Eq. (1) on a sample of all grade 6 students for whom we have SIMCE test scores²⁷ and calculate $\hat{\varepsilon}_{is,6}$, the residual for each student-subject observation. We then calculate, for each student-subject observation, the average residual of their classmates, $\bar{\varepsilon}_{i\cdot s,6}$ and estimate whether the “own” and “classmate” residuals have any effect on the formation of repeat matches in grade 7:

$$R_{is,7} = \gamma_1 \hat{\varepsilon}_{is,6} + \gamma_2 \bar{\varepsilon}_{i\cdot s,6} + \mu_{jt} + \eta_{is,7} \quad (2)$$

In this model, γ_1 captures whether students whose grade 6 individual residual is high are more likely to remain with the same teacher in grade 7, while γ_2 captures whether students whose classmates have high grade 6 residuals are more likely to remain with the same teacher in grade 7. Our estimate of γ_1 is small, precisely estimated and insignificantly different from zero (−0.0003 (0.0004)). Our estimate of γ_2 is slightly

²² Repeating the column (4) model on this reduced sample yields an estimate of 0.020 (0.004).

²³ Hill and Jones (Table 2) report an effect size of 0.018 σ (0.005); Hwang et al. report an effect size of 0.014 σ (0.004), both for maths. Note however the much wider prevalence of repeat matches in our data; it appears that only 3–4% of students experience a repeat match in US schools.

²⁴ Using this sample, the effect on SIMCE test scores from the model in column (6) is estimated to be 0.023 with a standard error of 0.006.

²⁵ We are not able to use a conventional value-added measure of teacher quality because the SIMCE test score is not available for the same student in consecutive school years.

²⁶ The coefficient of a regression of SIMCE score on teachers’ overall evaluation score is 0.38 with a standard error of 0.004. The coefficient on teachers’ portfolio score is 0.28 with a standard error of 0.004.

²⁷ We have information on SIMCE scores for Spanish and maths in 5 years (2013, 2014, 2015, 2016 and 2018).

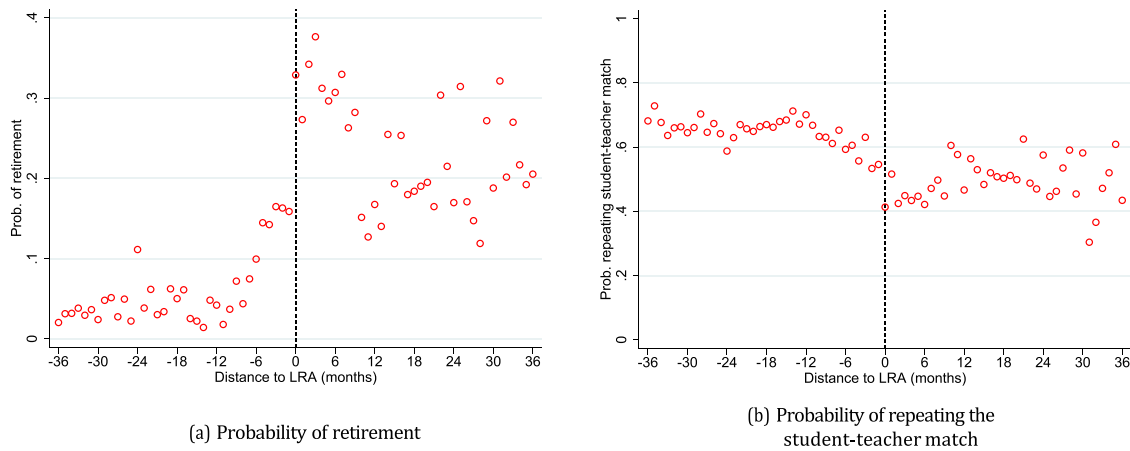


Fig. 1. Discontinuity in retirement at the LRA and repeat matches, distance in months.

Notes: A teacher is considered retired if she does not appear in the next five consecutive years in the administrative records of Ministry of Education. The distance to the legal retirement is the difference between the current age and the LRA, recorded in months. The distance to the legal retirement is zero for those teachers whose birthdays are in February and therefore reach the LRA in the last month of the previous school year.

larger but still insignificantly different from zero at conventional levels (-0.009 (0.005)). Thus, we find no evidence that student-subject combinations which perform better than expected are more likely to lead to repeat matches.

Nevertheless, because we cannot directly control for endogenous selection, we also consider a regression discontinuity approach which exploits the fact that the probability of a repeat match jumps because of small differences in teachers' date of birth which affect the time at which they reach the legal retirement age (LRA). Each teacher's exact date of birth is recorded in the teacher records, and we calculate age for each teacher on the last day before the school year starts. Our key identifying claim is that teachers who reach the LRA just before the 1 March are significantly more likely to retire than teachers who reach the LRA just after 1 March. For example, a grade 7 class in the 2006 school year whose teacher reaches the LRA in February 2007 is less likely to have the same teacher in grade 8 than a class whose teacher reaches the LRA in March 2007. We infer retirement from the disappearance of a teacher from the data for the next five years. In the left-hand panel of Fig. 1 we show that the probability of retirement jumps by over 10 percentage points between teachers who reach the LRA in February (distance to LRA = 0) and those who reached it in March (distance to LRA = -1). In the right-hand panel of Fig. 1 we show that this discontinuity is reflected in a 15 percentage point reduction in the probability of a repeat match.

We therefore have a fuzzy-RD design with distance to the LRA of each student-subject combination in grade 7, denoted D_{is} , as the running variable, which can be measured in days.

Following Imbens and Lemieux (2008) the RD estimator is defined as:

$$\tau_{RD} = \frac{\lim_{D_{is} \downarrow 0} E[y_{is} | D_{is} = 0] - \lim_{D_{is} \uparrow 0} E[y_{is} | D_{is} = 0]}{\lim_{D_{is} \downarrow 0} E[R_{is} | D_{is} = 0] - \lim_{D_{is} \uparrow 0} E[R_{is} | D_{is} = 0]} = \frac{\tau_Y}{\tau_R} \quad (3)$$

As before, y_{is} denotes the SIMCE test score in grade 8. The RD estimator corresponds to the ratio between the average intention-to-treat effect (τ_Y) and the first-stage effect (τ_R).

In Fig. A1 we show that the estimated differences in means across a wide range of characteristics at the discontinuity are insignificantly different from zero once we include controls for school type (public, private, voucher). The only exception is household income, which is slightly higher for children whose teacher's age is just below the LRA. The manipulation test of Cattaneo, Jansson and Ma (2020) estimates the density of the running variable either side of the cutoff using a local polynomial and yields a p -value of 0.1314.

The exclusion restriction requires that the effect of the discontinuity at the LRA is *only* driven by its effect on repeat matches. However, even

if the variation in repeat matches which is caused by the discontinuity is as good as randomly assigned, this variation also causes (quite large) variation in teacher experience.

For these two reasons we prefer parametric RD models which allow for the inclusion of student and teacher-by-year fixed effects. The inclusion of student fixed effects yields within-student comparisons which are not affected by any imbalance in student or school characteristics. The inclusion of teacher-by-year fixed effects means we exploit the discontinuity in grade 7 teacher behaviour which occurs across different classes of the same grade 8 teacher in the same year. The parametric RD models therefore remove any effect from the difference in experience which occurs at the discontinuity.

Our RD estimates are reported in Table 4. The first stage is negative and highly significant, consistent with the graphical evidence in Fig. 1.

Table 4

Effect of repeating the student-teacher match on test scores: regression discontinuity results.

	Linear with fixed effects (1)	Quadratic with fixed effects (2)	Non-parametric (3)
τ_R (First stage)	-0.121^{***} (0.004)	-0.114^{***} (0.005)	-0.137^{***} (0.004)
τ_Y (Reduced form)	-0.013^{***} (0.005)	-0.014^{**} (0.006)	-0.022^{**} (0.009)
TRD	0.110^{***} (0.038)	0.124^{**} (0.051)	0.158^{**} (0.063)
Student FE	Yes	Yes	
Subject FE	Yes	Yes	
Teacher FE $\times$ Year	Yes	Yes	
FE			
First-stage	0.873	0.873	
R-squared			
First-stage F statistic	1041	566	
Effective observations: Left			200,343
Effective observations: Right			109,731
Optimal Bandwidth			964.830

Notes: dependent variable is the student's SIMCE test score in grade 8. All regressions have 2785,928 observations. Column (1) includes distance to the LRA linearly, and the interaction between the distance to the LRA and the indicator variable for reaching the LRA. Column (2) includes a quadratic interaction between distance to the LRA linearly and the indicator variable for reaching the LRA. Column (3) presents results based on Calonico et al. (2014) with a polynomial of order one and weighted by a triangular kernel. Standard errors in Column (3) are calculated using Calonico et al. (2014). Standard errors are clustered at the student-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The probability of a repeat match falls by about 12 percentage points at the discontinuity. This induces a small fall in test scores, which gives us an RD estimate of around 0.11σ . In column (2) we allow for a quadratic functional form for τ_R and τ_y , which yields a very similar estimate with a slightly larger standard error than the linear model. Finally in column (3) we use the local polynomial modelling approach with the bandwidth selection procedure proposed by [Calonico, Cattaneo and Titiunik \(2014\)](#). This method does not permit the inclusion of large numbers of fixed effects, and yields a slightly larger estimate, which is consistent with our finding that repeat matches also cause an increase in teacher experience which also improves test scores.

All our RD estimates are larger than the fixed-effects and value-added estimates. This seems unlikely to be the result of strong negative selection into repeat matches. A natural concern is that, instead, this reflects a failure of the exclusion restriction. Since our parametric models include student effects and grade 8 teacher-by-year fixed effects, any failure of the exclusion restriction can only plausibly come from teacher effects in grade 7. A possibility is that the discontinuity may have an effect on teacher effort in grade 7 which may in turn effect outcomes in grade 8. For example, a teacher who knows that they will reach the LRA in grade 7 and will retire as a result may exert less effort than one who knows they will continue to teach the following year. We test this hypothesis by considering a sample of students who change school between grade 7 and grade 8. These students cannot loop, and their grade 8 teacher is selected independently of the grade 7 discontinuity, which leaves grade 7 teacher effort as the only channel by which the discontinuity can affect test scores in grade 8. Reassuringly, when we re-estimate the reduced form linear model (column (1) of [Table 4](#)) using only a sample of school-movers, our estimate of τ_y is extremely small and insignificantly different from zero: -0.0004 with a standard error of 0.010 . Thus, the discontinuity at the LRA in grade 7 has no effect on test scores in grade 8 for a sample in which the path to treatment is eliminated.

The remaining explanation for the fact that the RD estimates are larger than the fixed-effects and value-added estimates is that the effect of looping varies across complier types and by teacher experience. To confirm that our RD estimates are local in that they relate to very experienced teachers whose retirement decision is affected by reaching the LRA, we implement the intuitive test for the external validity of fuzzy RD designs proposed by [Bertanha and Imbens \(2019\)](#). This test involves comparing the conditional means of treated observations either side of the LRA (a comparison of compliers and always-takers) and comparing the conditional means of untreated observations either side of the LRA (a comparison of compliers and never-takers).

We reject the null of no discontinuity at the LRA for these two comparisons ($p = 0.033$), from which we conclude that the local RD estimate is, as expected, unlikely to be externally valid for values of teacher experience further away from the LRA. Nevertheless, it is reassuring that both the RD and the fixed-effects estimates find a positive effect. It also very reassuring to find that match-specific performance is not predictive of repeat matches (from [Eq. \(2\)](#)). For this reason, we put greater weight on the precisely estimated overall effect of 0.02 standard deviations reported in [Table 3](#).

4. The effect of repeat matches on students, classes and schools

The comparison we made in [Section 3](#) was between individual student-teacher matches that repeat and those that do not. The great advantage of this comparison is that it allows us to make within-student and within-teacher comparisons, and our RD strategy also allows us to control for endogenous matches at the student-subject level. However, repeat matches may have spillover effects on untreated units. At the student level, a student may allocate greater effort to subjects in which there is a repeat match, but at the same time allocate less effort to non-repeat-match subjects. If this was the case, increasing the number of matches at the student level would be less effective. At the class level, if repeat matches allow teachers to save time, there will be benefits to all

students in the class, regardless of whether students are individually repeating the match. On the other hand, if repeat matches are beneficial because of greater familiarity between teacher and student, it might not be beneficial for those who join a class in which most other students have a familiar teacher. Indeed, it seems possible that it might actually be harmful if teachers focus their efforts on students with whom they are familiar. At school-level, the allocation of teachers is a joint problem where repeating a match for one teacher has some implication for all other allocations within that school. In this section we therefore aggregate our data and use fixed-effect methods to examine whether the positive effects at the student-subject level carry over to student, class and school-level.

Our student-level model is:

$$\bar{y}_i = \beta_1 \bar{R}_i + \beta_2 x_i + \mu_c + \varepsilon_i, \quad (4)$$

where $\bar{y}_i$ is student i 's average SIMCE score across all four of their grade 8 subjects, and $\bar{R}_i$ is the proportion of their four subjects in which they have the same teacher as in grade 7. The model includes class fixed effects μ_c and a set of pre-determined student-level characteristics x_i . The variation we are exploiting here is the within-class variation in repeat matches which arises because not all students in a particular class in grade 8 will have had the same teacher in grade 7.

Our class-subject model is:

$$\bar{y}_{cs} = \beta_1 \bar{R}_{cs} + \beta_2 x_j + \mu_c + \mu_s + \varepsilon_{cs}, \quad (5)$$

where $\bar{y}_{cs}$ is the average SIMCE score of all students in class c and subject s in grade 8, and $\bar{R}_{cs}$ is the proportion of the class-subject combination who have the same teacher as in grade 7. The model includes class μ_c and subject μ_s fixed effects and a set of pre-determined teacher-level characteristics x_j . The variation we are exploiting here comes from that the fact that $\bar{R}_{cs}$ varies across subject within class. Note that in both [Eq. \(4\)](#) and [Eq. \(5\)](#) there is no time variation because each student and class is observed in only one year.

Finally, our school-subject-level model is:

$$\bar{y}_{kst} = \beta_1 \bar{R}_{kst} + \beta_2 x_{ks} + \mu_{kt} + \mu_s + \varepsilon_{kst} \quad (6)$$

where $\bar{y}_{kst}$ and $\bar{R}_{kst}$ are the school-subject-year level averages of y_{is} and R_{is} in [Eq. \(1\)](#); μ_{kt} is a school-by-year fixed effect; μ_s is a subject fixed effect; x_{ks} is a vector of characteristics of the school that vary across subjects and years (specifically, the proportion of female teachers and average experience). At the school level we have four cohorts of grade 8 students from 2004, 2007, 2009 and 2011, and hence [Eq. \(6\)](#) relies on variation within schools across subjects and across time for identification. This allows us to rule out selection into schools which might occur if, for example, better schools have more (or less) repeat matches. Also,

Table 5
Effect of repeat matches on test scores at student, class and school-level.

	Student level $\bar{R}_i$ (1)	Class-subject level $\bar{R}_{cs}$ (2)	School-subject- year level $\bar{R}_{kst}$ (3)
Proportion of repeat matches	0.039*** (0.006)	0.029*** (0.002)	0.032*** (0.002)
Class FE	Yes	Yes	Yes
Subject FE		Yes	Yes
School-by-year FE			Yes
Student controls	Yes		
Teacher controls		Yes	Yes
R-squared	0.414	0.916	0.911
Observations	696,482	127,348	82,524

Notes: In each model the dependent variable is the proportion of repeat matches at that level. Model (1) includes controls for students' gender, household income, mother's education and attendance rate in grade 7. Models (2) and (3) include controls for teachers' gender and experience. Standard errors are clustered at the class-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

exploiting the fact that we observe the same school for different cohorts, it is possible to include a school-by-year fixed effect μ_{kt} . This effect will remove all differences between school cohorts which might arise if repeat matches are used for some cohorts and are related to cohort-specific unobservable shocks.

Results from the aggregated models are reported in Table 5. All three estimates are positive and significant, consistent with a positive effect of repeat matches on students, classes and schools. It is striking that all three estimates are larger than the comparable student-subject level estimates in Table 3. This can partly be explained by the fact that these models do not control for teacher fixed effects — the exclusion of teacher effects in Column (2) of Table 3 produces larger estimated effects at the student-subject level as well. Larger effects are also consistent with positive spillovers from repeat matches within students, classes and schools.

5. Longer-term effects of repeat matches

Our evidence so far has focused on a short-term effect: the effect on test scores at the end of the year in which the repeat match occurs. Some evidence that looping has effects which persist beyond the current school year is provided by the results in Column (6) of Table 3: repeating a match in grades 6 and 7 has a positive, but smaller, effect on test scores in grade 8 compared to repeating a match in grade 8. Do these effects accumulate over a student's school career? Although we do not observe anonymized test scores in every year, we do observe all student-teacher pairs, so we can construct a measure of how many repeat matches a student is exposed to during their entire middle- and high-school career, and examine whether it has an impact on the University Selection Test (*Prueba de Selección Universitaria* or PSU), which is taken at the end of grade 12. The PSU consists of two mandatory exams: maths and Spanish, and two optional exams, social sciences and natural sciences. The Chilean Department of Evaluation and Educational Testing Service (*Departamento de Medición, Registro y Evaluación*) has a complete record of PSU test scores. In addition, the dataset contains information about student characteristics, such as gender, family income, household size and working household members.

We focus on five student cohorts of first-time PSU test takers between the years 2014 and 2018. We track these students and their teachers in Spanish and maths, between grade 5 (students aged 10–11) and grade 12 (students aged 17–18). The estimation sample consists of students with valid PSU scores for maths and Spanish, a complete history of student-teacher matches from grades 5 to 12, and SIMCE test scores in grade 4. The final sample comprises 551,320 students, covering 52% of the first-time test takers between the years 2014 and 2018. Table A6 in Appendix A reports descriptive statistics for this long-term treatment indicator. We find that, on average, students have repeat matches in slightly more than one-third of their grades between grades 5 and 12. There are no substantial differences by subjects or educational level.²⁸

To identify the effect, we again use variants of Eq. (1), where the dependent variable is now the standardized PSU test score for student i in subject s . The dummy variable R_{is} is replaced by $\bar{R}_{is}$, the proportion of repeat matches in grades 6–12. We have observations on two different subjects for the same student, so we can include student fixed effects, which control for student-invariant characteristics related to academic outcomes. The results are shown in Table 6.

Column (1) is the equivalent of Column (2) in Table 3, and suggests that the effects of looping do accumulate over time, since the estimated effect is about twice as large as the effect of a single repeat match. In Column (2) we include the student's past SIMCE test score in grade 4 in

Table 6

Effects of repeat matches on PSU test scores.

	(1)	(2)	(3)
Proportion of repeat matches between grade 5–12	0.058*** (0.007)	0.055*** (0.007)	
$R_i = 1$			0.004 (0.006)
$R_i = 2$			0.010* (0.006)
$R_i = 3$			0.018*** (0.006)
$R_i = 4$			0.024*** (0.006)
$R_i = 5$			0.033*** (0.006)
$R_i \geq 6$			0.041*** (0.008)
SIMCE score in grade 4		0.267*** (0.002)	0.267*** (0.002)
Student FE	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes
R-squared	0.851	0.859	0.859
Observations	1,102,640	1,102,640	1,102,640

Notes: dependent variable is the student's PSU test score, taken in the final year of school (Grade 12). Standard errors are clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the same subject and estimate a value-added model. As expected, past SIMCE test score is highly significant, but its inclusion does not change the main result. In Column (3) we show that the number of repeat matches over a student's school career has a linear effect on PSU test scores, and so the simple linear specification in Columns (1) and (2) seem quite reasonable. Overall, the results in Table 6 support the hypothesis that the more often a student experiences a repeat match during the school career, the better their university selection test scores.

6. Classroom behaviour and teacher expectations

Our results consistently show that repeating the student-teacher match results in a positive effect on student test scores. We find these effects at various different levels of aggregation and over the longer-term. In this section, we provide further evidence of the effectiveness of repeat matches on the behaviour of students and the views of their teachers. Specifically, we estimate the effect of repeat matches on student attendance, student behaviour and teacher expectations of their students.

The student enrolment data contains a record of student attendance measured at the student level (we not observe attendance by subject separately for each student), so we estimate a variant of Eq. (4) and regress the standardized attendance rate on $\bar{R}_i$, the proportion of subjects in which the student has a repeat match in grade 8. As in Eq. (4), the model includes class fixed effects and therefore relies on within-class variation.

An independent measure of student behaviour is available from the survey of teachers about their perception of classroom behaviour and the future performance of the class, which is available in 2009 and 2011. Although teachers who complete these surveys are clearly aware of whether their class is a repeat match or not, it is nevertheless a measure which is entirely independent of the anonymized SIMCE test score. Teachers do not know what their students' test scores are, and so this cannot influence their responses to the survey.²⁹ There are three survey

²⁸ To ensure that this subsample is representative, we repeat the model shown in Column (4) of Table 3 for those students who were in grade 8 in 2009 and 2011 and who take the PSU test in admission years 2014 and 2016. This sample gives an estimate of 0.024σ with a standard error of 0.004, close to the estimate for the full sample.

²⁹ 41% of the classes in data have this survey information for each subject. Table A7 in Appendix A reports a mean comparison test of classroom observable characteristics for the estimation sample and the restricted sample. The restricted sample has more socio-economically advantaged students, and also has students with a better average performance in the SIMCE test. Although the differences between the two samples are statistically significant, they are not large.

Table 7
Effect of repeat matches on student behaviour and teacher expectations.

	Attendance (1)	Problems to start the class (2)	Classroom disruption (3)	High teacher expectations (4)
Proportion of repeat matches	0.052*** (0.008)	−0.041*** (0.007)	−0.044*** (0.008)	0.017** (0.007)
Class FE	Yes	Yes	Yes	Yes
Subject FE		Yes	Yes	Yes
Student controls	Yes			
Teacher controls		Yes	Yes	Yes
R-squared	0.516	0.418	0.439	0.566
Observations	696,482	37,992	37,992	37,992

Notes: Model (1) is at the student level and include controls for students' gender, household income, mother's education and attendance rate in grade 7. Models (2), (3) and (4) are at the class-subject level and include controls for the teacher's gender and experience. Standard errors are clustered at the class level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

responses of interest. Teachers are asked if they face behavioural problems at the beginning of the class and disruptions during the class. These two outcomes are coded as binary variables, taking value of 1 if they are strongly agree or somewhat agree, and 0 otherwise. In addition, teachers are asked about the level of education that most of the class will achieve. The teacher expectation is coded as a binary variable, taking value of 1 if the teacher expects the majority of the class would finish any type of higher education (either a professional degree or a technical degree) or postgraduate studies. Our data is at the class-subject level, so we use a variant of Eq. (5) where the dependent variable is our measure of teacher perception (behaviour, expectations) for class c subject s , and the treatment is $\bar{R}_{cs}$, the proportion of the class c that repeat the match in the subject s . Fixed effects at class level are included to capture all the subject-invariant characteristics (observable and unobservable) of the class.

Results are displayed in Table 7. Column (1) indicates that repeat matches have a positive effect on attendance, increasing it by 0.05σ , an effect size which seems plausible given the estimated effect on test scores. Repeat matches also improve the teacher's perception of classroom behaviour and teacher expectations, shown in Columns (2)–(4). In particular, teachers are 4.1 percentage points less likely to have behavioural problems at the beginning of the class and 4.4 percentage points less likely to experience disruptive student behaviour. There are smaller but still significant effects on teacher expectations: teachers are 1.7 percentage points more likely to hold higher expectations for their students if their class is entirely made up of repeat matches.

These results are consistent with the qualitative evidence from teachers who claim that “looping” is beneficial for classroom behaviour. Students are familiar with the expectations of behaviour set by the teacher in previous years, and as a result behaviour improves. Of course, we cannot tell if the positive effects of repeat matches are jointly responsible for improved student behaviour and improved test scores, or whether improved behaviour is a mechanism by which academic performance improves.

7. Conclusions

There is a large literature which stresses the importance of teacher quality for student outcomes. But teacher quality is hard to improve. In this paper we have provided evidence that there are significant benefits to reallocating existing teachers to students they have taught before. Qualitative evidence from teachers suggests that repeating the match saves time, engenders greater familiarity, and hence aids learning. However, estimating the causal effect of student-teacher familiarity is challenging for two reasons. First, because student-teacher matches are

non-randomly selected. Second, because, even if student-teacher matches were chosen randomly, a repeat match may affect student performance for reasons other than student-teacher familiarity: we have seen that repeat matches have more experienced teachers and may also have more within-class familiarity.

We have provided a range of evidence from a new setting to suggest that repeating the student-teacher match has a significant positive effect on student test scores: we consider older (grade 8) children in a situation where repeat matches are common. A multidimensional fixed-effects framework which controls for selection by student or teacher into repeat matches suggests that repeat matches have test scores about 0.02σ higher, a result which is very consistent with the evidence from the US in a setting in which looping is an unusual event (Hill & Jones, 2018; Hwang et al., 2021). Our results show that looping continues to be effective even when it is widely used, suggesting that there is scope for expansion of this policy in settings in which it is less widely used.

Based on US evidence on the effects of teacher quality, including Rivkin et al. (2005) and Chetty et al. (2014a), our results suggest that looping has an effect equivalent to an increase in teacher quality of 0.1–0.2 standard deviations, a considerable gain which can be achieved from a reorganization of existing resources. Chetty, Friedman and Rockoff (2014b) further suggest that there are large monetary benefits for students from a 1 standard deviation improvement in teacher quality. Since repeat matching is revenue neutral, even a fraction of these benefits would provide a strong justification for promoting more repeat matches.

Our results also support a wide range of case-study and qualitative findings from the educational literature. The fixed-effects methods effectively hold constant many of the other channels by which repeat matches might affect student outcomes. We find no evidence of selection into repeat matches on the basis of match quality, and in addition a regression discontinuity design allows us to reject the null of no effect while relaxing the assumption of no selection on match quality.

We have also shown that these effects aggregate to the class and school-level, which implies that the positive effects for treated classes are not simply at the expense of untreated classes, which would be the case if, for example, schools simply allocate more effective teachers to repeat matches. Our final piece of evidence suggests that the effects continue over time, and that university test scores increase with the number of repeat matches over a student's school career. Consistent with our findings of positive effects on test scores, we also find positive effects in teachers' perceptions of classroom behaviour and their expectations of their students' achievements.³⁰

Allocating teachers to groups of students with whom they have interacted in the past appears to bring significant improvements in student performance without incurring additional costs on schools. An important question for future research is whether these results, which are estimated from variation in repeat matches in observational data, can be verified in a randomized setting.

CRedit authorship contribution statement

Facundo Albornoz: Conceptualization, Methodology, Formal analysis, Writing – review & editing. **David Contreras:** Conceptualization, Methodology, Formal analysis, Writing – review & editing. **Richard Upward:** Conceptualization, Methodology, Formal analysis, Writing – review & editing.

Data availability

The data that has been used is confidential.

³⁰ Note that our measure of test scores comes from an anonymous national test which is not marked by the teacher, so there is no mechanistic relationship between test scores and teachers' perceptions.

Appendix A. Additional results

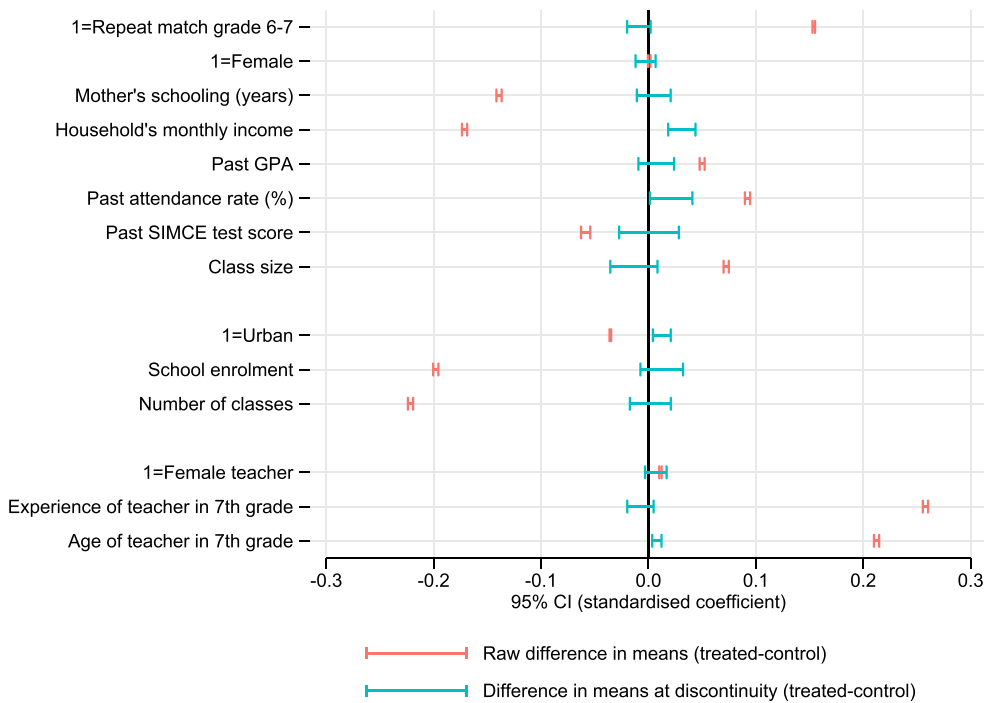


Fig. A1. Balancing tests at the discontinuity. Notes: Figure shows 95% confidence intervals on the difference in means between the treated and controls in the overall sample and at the discontinuity in the LRA. All variables are standardized to have zero mean and unit standard deviation to enable comparison. The difference at the discontinuity is estimated using methodology proposed by [Calonico et al. \(2014\)](#), with a polynomial of order one and weighted by triangular kernel. The number of observations for all the regressions is 2,785,928. Standard errors calculated using [Calonico et al. \(2014\)](#) and clustered at the student level.

Table A1

Effect of repeat student-teacher match on test scores: all years and grades.

	(1)	(2)	(3)	(4)	(5)	(6)
Repeat match	0.064***	0.029***	0.016***	0.013***	0.013***	0.013***
($R_{isg} = 1$)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
SIMCE score in grade 4 Lagged repeat match					Yes	Yes
Lagged repeat match						
Subject FE × Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Student FE		Yes	Yes	Yes	Yes	Yes
Teacher FE × Grade FE			Yes			
Teacher FE × Grade FE × Year FE				Yes	Yes	Yes
R-squared	0.001	0.819	0.836	0.841	0.862	0.862
Observations	8,867,050	8,867,050	8,867,050	8,867,050	4,980,753	4,980,753

Notes: Specifications in columns (1)–(6) are the same as those reported in [Table 3](#) with the addition of interactions with grade fixed effects. Not all subjects and grades are available in each year. The VA estimates in columns (5) and (6) use a smaller sample because grade 4 SIMCE test scores are not available for all students.

Table A2

Effect of repeat student-teacher match on test scores: results for other grades.

(a) looping in grade 6	(1)	(2)	(3)	(4)	(5)	(6)
Repeat match grade 5–6	0.049***	0.030***	0.017***	0.007**	0.007**	0.007**
($R_{is} = 1$)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
SIMCE score in grade 4					0.395***	0.395***
					(0.001)	(0.001)
$R_{is} = 1$ grade 4–5						0.006***
						(0.003)
R-squared	0.000	0.830	0.851	0.854	0.875	0.875
Observations	1,948,020	1,948,020	1,948,020	1,948,020	1,743,188	1,743,188
(b) looping in grade 10	(1)	(2)	(3)	(4)	(5)	(6)
Repeat match grade 9–10	0.111***	0.031***	0.016***	0.011***	0.014***	0.014***
($R_{is} = 1$)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
SIMCE score in grade 4					0.297***	0.297***
					(0.001)	(0.001)
$R_{is} = 1$ grade 8–9						0.009***
						(0.003)
$R_{is} = 1$ grade 7–8						0.005***
						(0.001)

(continued on next page)

Table A2 (continued)

(b) looping in grade 10	(1)	(2)	(3)	(4)	(5)	(6)
$R_{is} = 1$ grade 6–7						0.004*** (0.001)
$R_{is} = 1$ grade 5–6						0.004*** (0.001)
$R_{is} = 1$ grade 4–5						0.006*** (0.002)
R-squared	0.003	0.831	0.847	0.854	0.857	0.857
Observations	4,133,102	4,133,102	4,133,102	4,133,102	2,477,968	2,477,968

Notes: Specifications in columns (1)–(6) are the same as those reported in Table 3. Standard errors are clustered at the student level. In contrast to Table 3, these estimates use only Spanish and maths scores because SIMCE scores from the other subjects are not available for these grade-year combinations. In panel (a), the sample comprises school years 2013–2016 and 2018. In panel (b) the sample comprises 2003, 2006, 2008, 2010, 2012–2018. The VA estimates in columns (5) and (6) use a smaller sample because grade 4 SIMCE test scores are not available for all students.

Table A3

Effect of repeat student-teacher match on teacher-assigned grades.

	(1)	(2)	(3)	(4)	(5)	(6)
Repeat match ($R_{is} = 1$)	0.053*** (0.002)	0.017*** (0.001)	0.044*** (0.002)	0.055*** (0.003)	0.075*** (0.006)	0.074*** (0.006)
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes
Student FE		Yes	Yes	Yes	Yes	Yes
Teacher FE			Yes			
Teacher FE $\times$ Year FE				Yes	Yes	Yes
R-squared	0.001	0.791	0.802	0.803	0.831	0.831
Observations	1,417,856	1,417,856	1,417,856	1,417,856	396,438	396,438

Notes: Specifications in columns (1)–(6) are the same as those reported in Table 3 with the student's teacher-assigned grade in each subject as the dependent variable. The teacher- assigned grade is standardized at school-subject level.

Table A4

Effect of repeat student-teacher match by measures of teacher quality.

	(1)	(2)	(3)	(4)
Repeat match R_{is}	0.019*** (0.002)	0.025*** (0.003)	0.014** (0.005)	0.016*** (0.006)
Repeat match $\times$ medium evaluation			0.023** (0.007)	0.013* (0.007)
Repeat match $\times$ high evaluation			0.012 (0.007)	0.013* (0.007)
R -squared	0.812	0.783	0.783	0.783
Observations	2,785,928	632,776	632,776	632,776

Notes: Column (1) repeats the basic result from column (4) of Table 3. Column (2) reports the result from the same specification applied to the sample for which we have information on teacher evaluations. Teacher evaluations are only available for 45% of teachers in public schools. Column (3) includes interaction terms with teacher effectiveness as measured by their total evaluation score. Column (4) includes interaction terms with teacher effectiveness as measured by their portfolio score, which accounts for 60% of their total evaluation. All models include subject FE, student FE and teacher-by-year FE. Standard errors are clustered at the student level.

Table A5

Effect of repeat student-teacher match in the same and different subjects.

	(1)	(2)	(3)
Repeat match grade 7–8	0.019*** (0.002)	0.017*** (0.002)	0.016*** (0.002)
$R_{is} = 1$			
Repeat match new subject $R_{i-s} = 1$		0.022*** (0.004)	0.006 (0.009)
Interaction $R_{is} = 1 \times R_{i-s} = 1$			0.018** (0.008)
R-squared	0.812	0.812	0.812
Observations	2,785,928	2,785,928	2,785,928

Notes: Column (1) repeats the basic result from column (4) of Table 3. Column (2) adds a dummy which takes the value one if the student has a repeat match with a teacher in a different subject. Column (3) adds the interaction term between the two different kinds of repeat match, which can only equal one if a student has had the same teacher in multiple subjects in grade 7. All models include subject FE, student FE and teacher-by-year FE. Standard errors are clustered at the student level.

Table A6

Proportion of grades 5–12 which have a repeat match.

	Mean	Standard deviation	Observations
Estimation sample	0.36	0.18	1,102,640
By subject:			
Spanish	0.36	0.18	551,320
Mathematics	0.37	0.18	551,320
By educational level:			
Second cycle of primary school (grade 5–8)	0.37	0.25	1,102,640
High school (grade 9–12)	0.36	0.26	1,102,640

Table A7

Mean comparison test of classroom characteristics, full sample versus estimation sample.

	Estimation sample	Sample with teachers' perception	Difference	Std. err.
1=Female	0.50	0.50	−0.005**	(0.002)
Mother's schooling (years)	10.67	11.12	−0.443***	(0.032)
Household's monthly income	378.51	437.00	−58.486***	(5.033)
Average SIMCE test score	−0.06	−0.03	−0.028***	(0.007)
Past GPA	0.08	0.09	−0.017***	(0.004)
Past attendance rate	94.34	94.08	0.257***	(0.041)
Class size	21.88	21.02	0.855***	(0.120)
1=Public	0.53	0.44	0.084***	(0.006)
1=Urban	0.82	0.82	−0.008*	(0.005)
Observations	31,837	9498		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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