Do More School Resources Increase Learning Outcomes? Evidence from an Extended School-Day Reform[‡]

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Abstract

Whether allocating more resources improves learning outcomes for students in low-performing public schools remains an open debate. We focus on the effect of increasing instructional time, which is theoretically ambiguous due to possible compensating changes in effort by students, teachers, or parents. Using a regression discontinuity design, we find that a reform extending the school day increases math test scores. It also improved reading, technical skills and socio-emotional competencies. Our results are partly explained by reductions in home production by students, specialization by teachers and investments in pedagogical assistance to teachers, in addition to the extended instructional time.

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1. Introduction

In many settings, including education, more time on task should raise output. However, when it comes to public schooling this assumption is often challenged and there is a long-standing debate on whether more instructional time translates into better learning outcomes (Rivkin and Schiman, 2015).[1] Earlier theoretical models assumed that allocating more time to study increases human capital (Ben-Porath, 1967). More recent models has shown that students and teachers could alter their effort negatively in response to an increase in instructional time (Levin and Tsang, 1987; Todd and Wolpin, 2003). Empirically, the evidence is mixed, and it has been difficult to isolate the effect of extending instructional time from other factors.[2] Nonetheless, this expansion has become part of the toolset of recommended policies. For example, more instructional time is part of the "No Excuses" model used in charter schools in urban areas of the United States.[3] Similarly, an increasing number of developing countries are expanding the length of the school day in their public schools (Holland et al., 2014). Yet, in these countries, governments have a much higher opportunity cost of extending the school day as they have smaller local and national education budgets, and their school systems have lower levels and quality of complementary inputs.

We use a fuzzy regression discontinuity approach to measure the impact of an extended school-day reform in Peru. Peru is an upper middle-income country that consistently performs at the bottom of international standardized tests such as PISA (*Programme for International Student Assessment*). In every PISA round, the country has had one of the lowest shares of top performers

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[1] See Jackson (2020) for a debate on whether financial resources to public schools can improve child outcomes in general. See also Glewwe and Muralidharan (2016) for a more ample discussion relevant to developing countries.

[2] Cross-country studies (Lee and Barro, 2001; Wössmann, 2003) and those using state-level data within the United States (Card and Kruger, 1992) tend to find no relationship between time spent in school and learning or labor market outcomes. See Rivkin and Schiman (2015) for a recent assessment of micro-level studies in advanced economies exploring a longer school year.

[3] See Angrist et al (2013) for a discussion on how time in school compares to the impact of less traditional elements of the No Excuses model. See also Dobbie and Fryer (2013) and Fryer (2014) for additional analysis.

(only one percent in 2018) and very high shares of low achievers (more than double the average OECD figures). This low performance is confirmed by local national standardized tests, such as the one used in our study. For example, 85 percent of 8th graders are *below* grade-level in reading and a quarter of students are at least two grades behind. In order to reverse these outcomes, in 2015, Peru introduced the *Jornada Escolar Completa* (or JEC). The program's main goal was to add two pedagogical hours per day in 1,000 secondary schools nationwide to match the number of instructional hours in private schools, which tend to have better learning outcomes.

Selection into the program was decided by the national government. Critical to our identification strategy, schools with eight or more *secciones*, the equivalent to form classes, or homerooms in the United States, have a discontinuously higher probability of participating in the program compared to schools with seven *secciones* or less. At the threshold, the probability to be part of the program goes from near zero to almost 50 percent. As explained in section 2 below, this cutoff is arbitrary and was selected due to budgetary reasons. Smoothness tests applied to a large set of pre-intervention variables help validate the arbitrariness of the threshold. The program was announced at the end of the 2014 school year and the rules were based on data from the 2013 school year, making it impossible for schools to manipulate the assignment variable and affect their participation into the program.

Using administrative data and a fuzzy regression discontinuity approach, we find that participating in JEC increased math and reading scores measured by the national standardized test conducted at the end of the first year of intervention. The effects are larger and more robust in math where we document an increase of around 23 percent of a standard deviation (0.23SD). Using a household survey, we show that the program also increased socio-emotional competencies and technical skills (e.g., knowledge of English, access to digital devices).

Our results are larger than those observed in other developing countries. Lavy (2015) uses cross-country data (PISA) from 50 countries to show that one additional instructional hour in each subject (mathematics, science or reading) improves test scores by 0.06 SD of the test score distribution in that subject, holding overall instructional time constant. However, the effect falls to 0.025 SD for developing countries (see also Rivkin and Schiman, 2015). A small body of evidence on specific reforms extending the school-day in low and middle-income countries shows small effects up to a tenth of a standard deviation improvement in test scores (Bellei 2009; Cabrera-Hernández 2020; Cerdán-Infantes and Vermeersch 2007; Orkin 2013; Hincapie 2016). However, Padilla-Romo (2020) argues that in many of these country-specific studies it has been difficult to isolate the causal effect of the reforms due to lack of pre-trends and the recent criticisms to evaluations when the policy is implemented with a staggered roll-out (e.g., Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021).[4]

While an exhaustive list of reasons for the positive (and larger than previous studies') effect of the *JEC* is beyond the scope of this paper, we explore several channels. We use a student fixed-effects approach together with the discontinuity created by the reform to assess the impact of the extension in instructional time alone. Under the assumptions described in section 5.3, our results suggest an important role of the additional time. Also, a common theoretical concern with the effectiveness of extending the school day is a possible negative change in behavior (Levin and Tsang, 1987; Todd and Wolpin, 2013). For instance, teachers and students could reduce their effort per hour to keep the total amount of effort the same despite the increase in instructional hours. Parents could change their behavior by helping their children less now that they have longer hours

[4] Dominguez and Rufini (2021) explore the longer-term effects of the Chilean expansion analyzed by Bellei (2009). Padilla-Romo (2021) also explores longer-term effects but in Mexico. In both cases, the short-term impacts are much smaller that our estimates.

at school. This crowding-out behavior has been observed in other scenarios, for example, in response to attending better schools (Pop-Eleches and Urquiola, 2013) and receiving anticipated school grants (Das et al, 2013).

We find no evidence of these negative behaviors. Using a large student survey, we found no change in parental involvements such as them talking to their children, helping them with homework, explaining school topics or caring about their children's grades (all as reported by students). When asked about their perceptions in math and reading, students in JEC do not exhibit a lower self-perception in these subjects. This contrasts with the negative finding in self-perceptions when students attend better schools (e.g., Pop-Eleches and Urquiola, 2013). We do not find a major behavioral effect from teachers either. However, using a national survey of teachers, we do find a reduction in the number of courses taught due to JEC. This implies that specialization was an important component of the success of the program.

A second mechanism we explore is the students' reallocation of time. For instance, students could compensate longer school hours by reducing the amount of time they spend studying at home and taking on more household responsibilities. Using a household survey, we document a two-hour increase in time spent at school for students in JEC, which coincides with the two additional academic hours imposed by the program. This increase came at the costs of time allocated to household chores, other domestic tasks, leisure and sleeping. However, we find no reduction in the time spent studying at home. The substitution in time away from home production and leisure but in favor of time on task (at school and home combined) partly explain our results.

Our last mechanism focuses on the role of pedagogy and the provision of the additional inputs. Mbiti et al (2019) show that in developing countries simply augmenting school resources may have limited impact on learning outcomes because of the complementarities with other inputs that are not improved at the same time. A novelty of JEC was that the expansion in the length of the school day was accompanied by support in pedagogical assistance to teachers and information-technology to the school overall (e.g., language labs for English instruction). Through the analysis of administrative data, we document a clear impact of the program on the amount of pedagogical support received by JEC schools. We also show an increase in access to computers as well as an increase in support staff such as having a school psychologist and a school guard. The latter help explain the positive impact on socio-emotional skills. These changes in pedagogy allows JEC to deviate from the traditional expansion in the length of the school day as reviewed by Ganimian and Murnane (2016). These authors conclude that increasing the quantity (or quality) of resources, whether it is at home or at school or expanding the instructional times, has had at best modest impacts on student achievement in developing countries. They argue that these initiatives have mainly offered "more of the same" as they rarely changed children's daily experiences. Our analysis of JEC and its components shows a clear change in children's experiences with a corresponding increase in student achievement.

Our work also contributes to the recent literature on improving learning outcomes in developing countries which has mainly focused on solutions that bypass public school investments. Our focus on a program that tries to improve school learning by adjusting how the service is provided *within* public schools in developing countries offers a complementary view to work showing an improvement in learning outcomes from outsourcing the running of schools to the private sector (Romero et al, 2020; Barrera-Osorio et al, 2020), by reallocating children to private or more selective public schools (Aguirre, 2020; Angrist et al., 2002, 2006; Muralidharan and Sundararaman, 2015) or by investing in learning activities outside of school time (Muralidharan et al., 2019). Our results have comparable impact sizes and offer results that show that it is possible to improve learning outcomes within the public sector too. Our paper is also related to studies that have explored the expansion of the school year (e.g., Parinduri, 2014; Agüero and Beleche, 2013), the debate regarding year-round school calendars (e.g., McMullen and Rouse, 2012) as well as grade retention (e.g., Tafreschi and Thiemann, 2016; Manacorda, 2012).

In the next section, we describe the program structure and timetable. In section 3 we outline our data sources. In section 4 we explain the identification strategy, then present results and explore mechanisms of impact in section 5, accompanied by an exhaustive set of robustness checks. We conclude in the final section.

2. Expanding the school day: Peru's *Jornada Escolar Completa*

In Peru, prior to college, students attend elementary school (grades 1-6) and then secondary school (7-11) where the school calendar for public schools runs from mid-March to mid-December.[5] Starting in the 2015 academic year, Peru expanded the length of the school day in 1,000 public secondary schools nationwide as part of the new program called the *Jornada Escolar Completa* (JEC). Schools in this program added two pedagogical hours per day to their schedule making a total of 45 pedagogical hours (or just under 34 regular hours) per week, which represents an increase in the instructional week of almost 30 percent.[6] The program's goal was to improve not just the quantity of schooling hours but also the quality of the education service provided by public schools and to replicate some of the features observed in private schools. For instance, relative to regular public secondary schools, JEC schools also benefit from: (i) a pedagogical component, which includes support for teachers and students, teaching of English for students, and skill development for job market; (ii) improved school management through more and better organized

[5] Private schools have a longer calendar and tends to start two weeks earlier.
[6] A pedagogical hour is 45 minutes long.

school personnel; (iii) improved physical infrastructure; and, (iv) improved IT support (MINEDU 2014). Teachers' and principals' salaries increased to account for the additional hours and online support was offered to subject coordinators as part of the program.[7] In section 5.3, we study the role of these additional components to better understand the mechanisms behind the impact of JEC.

As shown in Table 1, in the regular system (column 1), math and reading are each allocated four pedagogical hours per week. Under JEC, math is increased to six hours and reading five (column 2). English added three hours (from 2 to 5) and science two (from 3 to 5). Also, the program paid special attention to tutoring students, especially for those found to be lagging behind. All other subjects either stayed the same or added an hour with JEC.[9]

The program was designed to be implemented only in public secondary schools and eligibility required schools to have only a morning shift (so that the expansion would not affect afternoon-attending students) and to be large enough: have eight or more "sections" and available space to accommodate, for example, a laboratory and a library. Personnel at the Ministry of Education used data from the 2013 *Censo Escolar* (school census) to identify the list of schools that satisfied all these requirements and found 1,360 schools nationwide. Schools that are recognized as "emblematic" were also added to the list (52 schools), despite not necessarily

[7] JEC had no significant provisions of meals as part of its design. Peru's public schools do not provide meals to students, except for breakfast in extremely poor districts and only in pre-K to 6 grade schools as part of the *Qali Warma* program. Only secondary schools in indigenous communities in the Amazon are part of this program, which are not part of JEC.

[9] However, note that schools in both the regular and the new system have unassigned hours (6 and 5 per week, respectively). In principle, nothing stops principals in schools that did not participate in JEC to allocate those hours to math, reading or any other subject. If that were the case and assuming JEC schools do not use unassigned hours in a similar fashion, our results would represent a lower bound of the impact of the increase in school hours, due to the possible "contamination" of the control group.

satisfying all the requirements.[10] Appendix Table A1 presents the detailed rules for selecting the secondary schools participating in JEC for 2015.

Relevant to our identification strategy is the selection of schools with eight or more "sections" (or *secciones*), the equivalent to form classes or homerooms in the United States. The smallest (full or complete) secondary school in Peru will have five sections: one per grade. However, bigger schools would have two or more sections per grade. Hence, the number of sections tends to be a multiple of five.[11] Thus, the choice of eight sections is quite arbitrary, it is not a multiple of five so there is not much density at the threshold, and helps us with the identification assumptions of smoothness around this threshold. Also, while not formally discussed in the directive creating the program, the choice of eight sections reflects the budgetary limits of JEC in its first year. As further discussed in section 4, our main source of identification comes from comparing schools around this threshold as the probability of participating in JEC changes discontinuously at eight sections.

The list containing 1,412 schools (1,360 regular schools and 52 emblematic schools) was then sent to local coordinators who validated it. This process added and removed some of the schools selecting a total of 1,343. The Ministry of Education then hired evaluators to obtain further information about these schools and selected 1,000 of them. This list was included in the directive creating the program in September of 2014 (RM Nº 451-2014-MINEDU). The list was amended one more time in February of 2015 replacing six schools from the original list (RM Nº 062-2015-

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[10] These are large urban schools created in the 1950s and were labeled as emblematic in recent years. These schools tend to be very large (often with more than 30 sections) and have little impact in our analysis.

[11] A secondary school with eight sections would have at least one section per grade and three grades with two sections each. The assignment of students to sections is not regulated by the Ministry of Education (MINEDU) and varies by school. In some cases, it would reflect tracking of students but in others depends on alphabetical order or other rules. Relevant to our empirical strategy, we do not expect the assignment of students to sections to *discontinuously* change in secondary schools with eight sections. We cannot test for this hypothesis because the MINEDU does not collect this information. Furthermore, personnel at the MINEDU do not think such a discontinuity exist at the threshold of eight sections.

MINEDU). Note that going from the original 1,412 schools to the final list of 1,000 schools is driven by unobservable characteristics, possibly reflecting the bargaining between the central administration and the local coordinators as well as the school districts. Thus, to causally identify the impact of the program we avoid comparing the left-out schools with the final list. Rather, in this paper we use the first set of rules as they depend on clear guidelines obtained from observables: schools with eight or more sections. The data sources and the methodology to identify causal effects are described next.

3. Data

We use three administrative data sources, a survey of teachers and the Young Lives panel dataset of individuals to estimate the effect of the JEC programme on test scores and the mechanisms explaining it. All datasets, except for Young Lives, come from the Ministry of Education. The main source of information is the 2015 *Evaluación Censal de Estudiantes de Secundaria* (ECE-S). ECE-S is the national standardized test administered to all eighth graders in public and private schools nationwide. The 2015 ECE-S, the first of its kind, has a coverage of 94.4 percent of students and 99.5 percent of secondary schools. This standardized test, applied at the end of the school year (17 and 18 of November of 2015), consists of 60 multiple choice questions in math and reading, respectively. The z-score transformations of the math and reading tests are our main variable of interest, using the national mean and standard deviation, which includes public and private schools.[12] The Ministry of Education classifies students into four groups depending on their performance in the test. In the highest group, students are at grade-level and are ready to face the

[12] This choice gives us conservative estimates of the impact of JEC. For example, if we were to use the mean and standard deviation from the pool of public schools only, our results would be bigger. Nonetheless, we opted for a conservative estimate in our paper.

challenges of the next grade. Only 14.8 percent of students achieve this level for reading and less than 10 percent in math (Table A2 in the Appendix). Students in the second group, “in process”, partially obtained the goals for their grade but secured knowledge of the previous grade. Beginner-level students failed to show mastery of the previous grade and around 40 percent of 8th graders scored at this level in each subject. Finally, 37.7 percent in math and 23.4 for reading are below the beginner-level. This dismal performance is consistent with the poor scores obtained by Peruvian students in international tests such as PISA or the Third Regional Comparative and Explanatory Study (TERCE). Thus, in our analysis we also explore whether JEC affected the distribution of students by estimating the impact on the probability of scoring in the highest level.

The ECE-S included a short questionnaire where students were asked about their demographic and socioeconomic characteristics (e.g., age, gender, parents’ education, native language). Relevant to our study, students were asked also about parental involvement regarding, for example, homework and book recommendations and their perception about their own abilities in math and reading. We use these variables to capture possible mechanisms. Finally, this questionnaire asked students about their perceptions regarding teachers’ behaviour.[13] Unfortunately, these questions requested students to combine their views for their math and reading teachers. This limits the possibility to explore changes in teaching practices as results of JEC, separately by subject.

The second administrative data source is the school census (*Censo Escolar*). This is an annual administrative dataset that captures the characteristics of the school in terms of infrastructure, personnel and enrolment. This is complemented at the end of the year with pass rates. Unlike the ECE-S, the census contains data at the school or grade level but not at the student

[13] These include 16 questions around clarity of the purpose of the class, explanations, speed of topics, participation in class and feedback.

level. The Ministry of Education used data from 2013 to identify the original list of schools from which to select the ones receiving the JEC program. Thus, we use the same data and year, to test for the smoothness assumptions to validate our identification strategy.

The third administrative data source used in this paper is the *Semáforo Escuela* which allows us to examine the changes in school quality affected by the JEC program. *Semáforo Escuela* is a data system used by the Ministry of Education to monitor the delivery of educational services provided by public schools. Data are collected monthly (between April and November, except for August) on a number of aspects, including characteristics of administrative staff and teachers, access to support for teachers and other programs run by the government, access to IT, teachers' level of specialization, etc. Each month, a representative sample of schools is selected and visited for this purpose. Information is provided by the principal (school module) and by up to three teachers per school, randomly selected (teachers' module). For our analysis, we aggregate data collected in 2015 and 2016 (if a school is visited twice, we choose the first observation).

The fourth data source is the National Survey of Teachers (*Encuesta Nacional a Docentes*, ENDO), a biannual survey administered to a representative sample of teachers from public and private schools. The survey collects information on teachers' professional trajectories, income, time use, access to training and IT, attitudes, and motivation, among other aspects. Up to three teachers are randomly chosen per school. For our analysis we used data from ENDO 2016.

Finally, we use data from a national sample of adolescents aged 15 years, the Peruvian younger cohort of the Young Lives study (YL), to extend the analysis of the effect of the JEC program on a set of child-level outcomes measured in 2016 that might, in turn, explain impacts on academic achievement: time use, socio-emotional competencies (measurements of self-concepts and aspirations) and technical skills (knowledge of English and digital skills). Children were selected at the age of one from a random sample of 20 districts, from the universe of district, excluding the 5% wealthiest districts. The original sample was composed of 2,052 children, with 100 children per district. This cohort was first visited in 2002 (at the age of 1) and revisited at ages 5, 8, 12 and 15 in 2006, 2009, 2013 and 2016 (respectively). In the last visit, data on the name of the school attended by the adolescent was collected.

We used data from *Semaforo Escuela*, ENDO and YL to explore the potential mechanisms through which the JEC programme might have an impact on test scores. Table A3 in the Appendix summarizes the information available from these data sources, unit of observation, and year in which the data were collected. As with ECE-S, these datasets are each linked with the 2013 school census to determine whether schools satisfy the JEC eligibility criteria.

4. Methodology

We use the selection rules of the JEC program to implement a fuzzy regression discontinuity design, using a 2SLS approach. As described above, having eight sections or more increases the participation in JEC but does not fully explain it, which implies a discontinuous increase in the probability of JEC. Thus, we use discontinuity at eight sections as the instrument for participating in JEC as the first stage. This is shown by Equation (1) below:

$$JEC_{ij}= \pi\, \mathbb{1}(S_j \geq 8)+h_1(S_j \geq 8)+h_2(S_j<8)+\theta X_{ij}+e_{ij} \qquad (1)$$

where JEC_{ij} takes the value of one for if a student (*i)* is attending a JEC school indexed by *j* and zero otherwise. The indicator function $\mathbb{1}(.)$ returns a one only for schools that have eight sections or more ($S_j \geq 8$). Functions $h_1(.)$ and $h_2(.)$ are flexible polynomials in the assignment variable S (number of sections) and X is a vector of students' characteristics (age, sex, mother's language, and school attainment) and about the school (e.g, urban/rural location, and school district fixed effects). The second stage, estimating the impact of JEC (as predicted by the discontinuity) on outcome Y_{ij} is given by Equation (2) and captured by parameter β:

$$Y_{ij}=\beta\widehat{JEC}_{ij}+h_1(S\geq 8)+h_2(S<8)+\lambda X_{ij}+u_{ij} \qquad (2)$$

Standard errors are heteroskedasticity-robust and clustered at each of the 215 local school districts (known as UGEL).[14] Our identification strategy relies on two assumptions discussed in the next subsections.

4.1 First stage

For the instrument to be valid, the discontinuity at eight sections should strongly predict participation in JEC. As discussed in section 2, and in particular, given the set of requirements listed in Table A1, the Ministry used data from 2013 to identify the schools that had eight or more sections as part of the decision process. In Figure 1 we provide visual evidence to validate such a rule. As expected, the probability that a public secondary school is part of JEC is zero for those with less than seven sections and near zero for those with seven sections. At eight sections, the probability discontinuously jumps to nearly 0.50 and remains high before decreasing for very large schools.[15] This feature implies a fuzzy discontinuity as the probability of being part of JEC is less

[14] We also explore an alternative clustering approach. Following Lee and Card (2008), when the running variable is discrete, as in our case, they propose clustering by the running variable. As shown below, our results do not change when using that approach (see Table 3). However, when we restrict the sample to schools with 7 and 8 sections, such a clustering approach would be invalid. Thus, we use the clustering by school district as our preferred method.
[15] Larger schools tend to have a morning and an afternoon shift and are less likely to be eligible. See Appendix Figure A3 for a breakup by shift-type.

than certain at the threshold. It therefore provides the necessary evidence for an instrumental variables approach.

The regression counterpart of this evidence is presented in Table 2 using public schools. In column 1, with linear splines for the running variable, we find that at the threshold of eight sections, a school is 47.9 percentage points more likely to be part of JEC. Under the same specification but limited to urban schools, given the urban bias in the initial rollout (see below for further discussion) column 3 shows a π parameter of 49.1 and when restricted the sample to schools with seven or eight sections (nationwide) the parameter implies an increase in 51.8 points (column 5). Using quadratic splines (columns 2 and 4) does not change our conclusions. That is, the rule of selecting schools based on them having eight sections is a strong predictor for JEC participation and provides a discontinuous jump that can be used as an instrument, as long as the exclusion restriction is satisfied.

4.2 Smoothness tests

To satisfy the exclusion restriction, the instrument should affect the outcomes only through its impact on participation in JEC. Thus, for all other variables, especially for those measured in 2013, there should be no discontinuity at the threshold. This is presented first, graphically, in Figure 2, and tested formally in Table 3.

The figure plots the average values of a set of predetermined variables grouping the sample of all public secondary schools by number of sections: start and end time of the school schedule, length of the school day, access to welfare programs, share of girl students enrolled, passing rates (all and by gender), use and teaching of indigenous language and whether the school has a morning shift only. The data come from the 2013 *Censo Escolar*, which reports information at the school level. It is easy to observe that for all the variables there is a smooth transition around the threshold of eight sections. As discuss in section 3, the initial selection of schools used the 2013 *Censo Escolar*. The rules for selection into JEC were devised in 2014 and made public in October of that year. Thus, schools were not able to alter the number of sections back at beginning of school year in 2013. Together, these results confirm that the timing of the reform eliminates the possibility of manipulation of the assignment variable. Note, however, that there is an urban bias in JEC. The program mainly targeted urban schools in its first year. Thus, our results for the urban schools only are presented alongside the results for the full sample.

Figure 3 similarly shows student-level pre-determined characteristics by number of sections: age, gender, whether s/he attended kindergarten, repeated a grade as well as her/his mother's education and language. We show the same smooth transitions around the threshold for this set of variables. Table 4 provides the regression results reinforcing the validity of our identification strategy. The results of using a fuzzy RD to estimate the impact of JEC, via 2SLS, on academic achievement are presented in the next section.

5 Results

5.1 Main findings

We start with Figure 4, which shows the reduced-form graphs for the impact of JEC on test scores, using ECE-S data. In the top row, we consider two outcomes for reading: test scores and the probability that a student scores in the highest group (=1 if performed at grade level). In both cases, there is weaker evidence of a strong discontinuity for this subject. However, a jump is clearly observed for the math outcomes (bottom row). These findings are validated in Table 5 using 2SLS.

For each of the four outcomes (i.e., test scores and probability of scoring in the highest level for math and reading) we consider four samples of public secondary schools. In column (1) all schools are considered. However, due to the urban bias of JEC, we restrict the sample only to schools in urban areas (column 2), to schools with a morning shift (column 3) and to schools in urban areas but with morning shift only (column 4). There is robust evidence that the effect is statistically different from zero for both math and reading.

Note that the effect on reading is not as large as that for math, but still significant. This is consistent with the fact that in the JEC program, students received two extra hours for math, and only one extra for reading. Focusing on math only, the basic specification shows that JEC increased math test scores by 0.24SD (Panel C, column 1). Limiting the sample to urban schools with a morning shift (Panel C, column 4) shows that JEC increased the test scores by 0.23SD. Even our lower estimate is bigger than the effects reported by Bellei (2009) for Chilean secondary schools (0-0.12SD in math) and by more recent papers using data from PISA who tend to find an impact <0.04SD for developing countries (Lavy, 2015 and Rivkin and Schiman, 2015).[16] To put our results in a bigger context, in Figure A1 (Appendix), we compare them with the effects found in recent randomized controlled trials in education conducted in developing countries as reported by Kremer et al (2013). The lower (upper) bound of our results for math exceed 60 (95) percent of the effect size of studies reviewed by these authors. Furthermore, our impacts for math suggest that JEC reduced by one third the gap between public and private schools.

[16] Hincapie (2016) suggests that the expansion in Colombia led to an increase of at most 0.10SD for 9th graders. In Mexico, Padilla-Romo (2021) reports near-zero effects in the first year of the implementation and up to just under 0.14SD four years after but for students in primary school (third to sixth graders).

5.2 Further robustness checks

We now consider additional robustness checks to the sample restrictions used above. We first explore alternative ways of clustering the standard errors. In Table 5 we presented the results with clustering at the school district in squared brackets. Following Lee and Card (2008) we also explore clustering by the discrete running variable, number of sections, and display them in *curly* brackets in the same table. Using this clustering option does not change our results.

We consider a local randomization RD approach given the discrete nature of the assignment variable. The identifying assumption is that in the vicinity of the cutoff, assignment is as good as random (Cattaneo, Idrobo and Titiunik, 2018). Table 6 shows nonparametrically identified estimates of the first stage (column 1) as well as the reduced forms (columns 2-5). This is done using the smallest bandwidth possible: 7 and 8 sections. These new set of results confirm our previous findings: a strong effect for math and a weaker impact (but still positive) for reading.

We also explore the cumulative distribution functions of the test scores for math and reading of schools with seven sections against those in eight sections and limiting the sample to urban schools. This is shown in Figure A2. The results are consistent with those of Table 6. For reading, we do not find strong evidence of stochastic dominance. However, for math, the performance of students in schools with eight sections stochastically dominates those with seven sections, suggesting a clear effect of JEC on academic achievement.

We consider quadratic splines as an alternative specification, and the results are presented in Appendix Table A4, columns 1-4. Again, these modifications do not affect our conclusions. Furthermore, we use an alternative identification strategy by exploiting an additional source of variation: public schools with shifts other than only-morning are not eligible for JEC. For them, there should be no discontinuity at eight sections. Appendix Figure A3 validates that conjecture.

Thus, we can use a difference-in-discontinuity approach and compare the discontinuity at the threshold in eligible and ineligible schools. This is done in Table 7. In column (2), when considering only urban schools, the impact of JEC on math test scores, Panel C, is 0.29SD.[17]

As a final robustness check, we consider a placebo test using an alternative group of ineligible schools: private secondary schools. In Figure 5 we see that for these schools there are no discontinuities at the threshold of eight sections. Appendix Table A5 shows the regression counterpart of these graphs and confirming the lack of a discontinuities for the private schools.

We reproduce and expand the analysis of the impact of JEC on students' achievement using the Young Lives data.The learning outcomes are measured by Peabody Picture Vocabulary Test (PPVT), a reading comprehension and a math test (see Cueto et al., 2009; Cueto and León, 2012).[19] Although this sample is not fully nationally representative, Young Lives produce very similar estimates of the JEC impact on math and reading test scores to ECE (by 0.25SD and 0.30SD, respectively). See Table A6 in the Appendix for details.

Taken together, all these results indicate a strong and robust effect of JEC on math test scores and smaller (and less robust) on reading. In the next section, we discuss the impact of JEC on the behavior of parents, students, and teachers.

5.3 Mechanisms

We proceed to analyse the potential mechanisms explaining the improvement in learning outcomes. To do this, we use additional data sources and the same identification strategy to

[17] In Appendix Table A3 we consider quadratic splines for this case as well. Our results remain consistent (see columns 5 and 6).

[19] PPVT is designed to measure vocabulary knowledge. The test is composed of up to 125 items (in Spanish used adapted for Latin America (Cueto and Leon, 2012; Dunn et al. 1986)). In each item, the interviewer says a word to the child and from four pictures she must select the one that best represents the word heard. This instrument has been administered since Round 2. In turn, the reading comprehension and math achievement test scores were developed by Young Lives to measure children and adolescents' achievement according to aspects they should know given their age and grade and have been administered since Round 3. In this case, outcomes measure the total number of correct answers. For analysis, we used raw test scores standardized by age in years.

measure the impact of JEC on other dimensions of children and their educational environment. In all cases, we used data from 2016, about one year after the JEC program started operating. Those schools that became JEC in 2016 are dropped from the sample because in that year, a different eligibility rule was used. Each dataset was described in Section 3. When relevant, we created indexes to summarize our results as in Kling et al (2007).[20] Adjusted p-values correct for multiple hypothesis based on Benjamini and Hochberg (1995) and we apply the concept of a false discovery rate to allow inference when conducting many tests.[21]

The role of additional instructional time

Before using those additional data sources, we first try to assess the impact of extending the school day *net* of all the other components of JEC. To do so, we extend our RD design by using the methodology proposed by Lavy (2015, 2021) and Rivkin and Schiman (2015). These papers use within-student variation to estimate the impact on test scores based on the variation in instructional time by academic subject. Applied to our context, we take advantage that in non-JEC schools, math and reading had the same number of hours (four) but JEC expanded the time for math by two hours compared to only one additional hour for reading. This allows us to create a *difference in discontinuity* design where the *difference* angle comes for the math and reading comparison, for each student, while using the discontinuity at eight sections as before. The critical assumption here is that the effect of instructional time is the same across math and reading. However, Lavy (2015)

[20] Each index is the weighted average of a group of selected variables. Each of these variables is standardized with mean zero and variance equal to one. Prior to standardization, the order of the variables for which higher values reflect non-desirable results is reversed. When there are missing values and this is not due to filtering, we impute the average to the missing observation (two averages are considered, depending on whether the observation is from a school that has 8 sections or more or less than 8 sections).

[21] The intuition behind the FDR approach is to allow researcher to tolerate a certain number of tests to be incorrectly discovered. For instance, an FDR *adjusted* p-value of 0.05 implies that 5 percent of significant tests result in false positives compared with an *unadjusted* p-value of 0.05 that implies 5 percent of all tests result in false positives.

shows that such assumption does not seem very restrictive when considering a sample of developed and developing countries, including Peru.

With this caveat in mind, Table 8 shows the results of conducting such analysis focusing on the reduced form specification (similar to Table 6). In this case, the parameter of interest is the interaction *Math*1*(*Section*≥*8*) after controlling for the subject and splines of the running variable for each subject and the student fixed effects. For completeness, we show the results in four samples, but due to the analysis conducted earlier we focus our attention to the last two columns. In column (3), restricting the sample to urban schools with a morning-shift only, the additional pedagogical hour in math due to JEC increases test scores by 0.055SD. This is a relatively large magnitude. Compared to the reduced forms results shown at the bottom of the table, this effect would explain around 34% (=0.055/0.161) of the overall gains in math. In column (4), we restrict the sample to the smallest possible (schools with 7 or eight sections) and the gains from the mere extension in instructional time would represent 70% of the gains in math (0.049/0.07). This analysis suggests that an important part of the gains in test scores from JEC come from the increase in instructional time *alone*, under the assumptions described above.

Child's time use

Students and parents might choose to exert a lower level of effort (for students, especially outside school), and this could make the net impact on time dedicated to learning activities ambiguous. Todd and Wolpin (2003) show that in order to understand the full effect of education policies the behavioral changes of parents, students (and, indeed, teachers) should be incorporated. Recent work by Pop-Eleches and Urquiola (2013) has shown that behavioral responses are indeed possible in the context of education. In Romania, the authors find that when children attend a better school, they feel marginalized, their parents reduce their efforts when helping them with homework and teachers sort themselves within the schools. Furthermore, they show that these negative behavioral changes tend to occur early on and after a few years the effects reduce or even vanished. Thus, one could expect similar behavioral changes with the expansion of the school day. For example, Levin and Tsang (1987) introduce a model of effort and show that if the previous length of the school day represented an equilibrium, extending the number of hours could bring no effect on test scores because of students and teachers could reduce their effort levels per hour of instruction. Although pure effort is unobservable, we confirm that JEC has an overall positive impact on a child's time dedicated to study. Using data from Young Lives, in Table 9, we find that JEC increases time at school by 2.1 hours per day. This closely mirrors the increase in the length of the school day at JEC schools. This increase implied a reduction in time spent on almost all other activities, such as sleeping, household chores, domestics tasks, taking care of other household members, time spent studying at home, and leisure. The reduction in the time spent studying at home is comparatively small (0.1 hours per day) and statistically insignificant. These results suggest that the gains in test scores come, in part, from an increase in time spent in school at the cost of time allocated to home production.

Other school resources

As part of its design, JEC was also expected to increase the availability of school resources: IT infrastructure, staff, and the pedagogical resources available for both teachers and students. Our results, reported in Table 10, are consistent with improvements in all these areas by 0.8SD, 1.7SD, and 5.1SD respectively. The increase in IT infrastructure (Panel A) is explained by having more classrooms with computers and laptops, and a higher probability that IT equipment receives maintenance. These are statistically significant even after accounting for multiple hypothesis testing in the p-values. In relation to the school staff available (Panel B), the probability of having a complete teaching staff reduces, which could be an unintended consequence of JEC schools, had an extra demand for personnel including teachers. However, this is compensated by an increase in the number of non-teaching staff available, such as security guards.

The increase in the availability of pedagogical resources (Panel C), arguably one of the most important features of the program --together with the expansion of the school day-- is explained by a significant increase in the likelihood of having a psychologist at the school—required in JEC but not regular schools—, having teachers that provide support to parents, and that the school participates in the MINEDU program "*Acompañamiento pedagogico.*" This is also a component of the JEC program, though not unique to it. Schools that receive this program are visited by specialists who work with teachers to improve their pedagogical strategies. In turn, psychologists work both with tutor teachers and (directly) with students. These additional resources could explain not only the positive impact of JEC on learning outcomes but also why this intervention has higher impacts (at least in the short run) relative to expansions in the length of the school day in developing countries. These speaks to the importance of the complementarity of school inputs as discussed by Mbiti et al (2019).

Teachers' behaviors

We explore three possible changes in teachers' behaviors due to the program using data from ENDO, the teacher-level survey. We first consider teachers' time allocation. While teachers must work longer classroom hours due to JEC, the net impact on other school-related activities inside and outside school is ambiguous due to a possible substitution effect. In Table 11, Panel A, we show that the number of hours dedicated to all school activities has not changed. This would be consistent would a substitution effect on in-class activities --such as preparing lessons, grading students' homework, talking with parents, interacting with other teachers—due to the increase in instructional time.

Second, we observe, however, an increase in time allocated to income generating activities, that is significant even when accounting for multiple hypothesis testing. This is somehow a surprising effect given the salary increase for teachers in JEC schools but consistent with an increase in the dissatisfaction among JEC teachers. In Panel B of Table 11, the index shows a decline in satisfaction of around 0.10SD. This is mainly coming from teachers answering that they are less willing to choose this career again. An increase in dissatisfaction after an increase in classroom time (with no change in overall time for school activities) that is accompanied by increase in earnings speaks to the evaluation that teachers have in terms of effort and the possibility that is altered by income effects. These unintended consequences could attenuate the effects of JEC.

Third, our analysis uncovers changes in teachers' pedagogical practices (Panel C, Table 11) summarized by an improvement in the index of 0.27SD. This is mainly explained by a higher level of specialization by teachers as they have fewer subjects to teach as part of JEC. This is followed by a higher probability that JEC teachers receive pedagogical support from "*Acompañamiento pedagogico*" as discussed earlier but now using teacher-level data. Overall, the negative impact from the decrease in teachers' satisfaction seems to be lessened by their exposure to better pedagogical practices and their specialization by teaching fewer subjects.

Sorting effects

Sorting effects of both teachers and students might also be relevant. Better teachers might be more likely to apply for a position in JEC schools, due to the salary increase. More involved parents might choose to transfer their children to JEC schools, and these children are likely to have better learning outcomes, generating positive externalities for the rest of students.[22] In these two cases, our estimates for the impact of JEC on learning outcomes could be biased upwards. On the other hand, teachers could leave to leave because of the longer classroom hours while parents could send their low-performing children if they anticipate positive gains from JEC. If these are true, our impacts could be biased downwards due to the negative externality for low-performing students. In Table 11 (Panel D), we show that teachers' characteristics are similar in JEC and non-JEC schools, which suggests there are no sorting effects. We consider age, sex, experience, credentials, and type of contract and find no differences by type of school even when considering the regular p-values that do not account for multiple hypothesis testing or for the overall index (0.03SD).

We do not find evidence of students' sorting that could bias our findings upwards either. We compare several features of students in JEC based on where they were before the program started. For that we use the longitudinal nature of the Young Lives data. This is shown in Table A7 in the Appendix. Using pre-JEC outcomes (measured in 2013; JEC started in 2015), we show that JEC did not attract better students. In terms of aspirations, self-esteem and agency, students moving to JEC schools are not different from their peers already enrolled in JEC-to-be schools ("stayers"). If any, the table shows that for math and reading test scores as well as for indices of self-efficacy and pride, movers were already at a disadvantaged compared to stayers before the

[22] Peru has no legal restrictions for enrolment of children that do not live in the district where the school is located.

reform began. This could imply a possible attenuation of the effects on learning outcomes as JEC may have attracted low-performing students.

Child non-cognitive and technical skills

We also look at the potential impact of JEC on educational aspirations, socio emotional as well as technical skills. The first two might improve due to the increase in the number of psychologists available at the school (that are meant to interact directly with students), and the latter by having access to more IT resources.[24]

For educational aspirations we consider two outcomes: whether or not the child aspires to complete higher education (including any post-secondary education), and whether the child aspires to go to university. For socio-emotional competencies (Yorke and Ogando Portela, 2018), we consider the notions of self-esteem and self-efficacy, both of which have been found to matter to predict life outcomes, including access to higher education, risk behaviours, and teenage pregnancy (Sánchez and Singh, 2018; Favara and Sánchez, 2017; Favara, et al., 2020).

For technical skills, we look at two types of outcomes. First, we look at child self-report of her abilities speaking English (63% of children report that they speak English "well" or "a little bit"). Second, we use three scales designed by the YL to measure digital skills (Cueto et al., 2018). The first scale measures access to digital devices (computers, laptops, tablets, smartphones) and to the Internet (72% of the sample reports having used digital devices recently). The second and third scales, which are only applied if the child has access to digital devices and to the Internet (respectively), measure the skills that child has using computer and browsing the Internet. In all cases, information is self-reported.

[24] According to information collected by us from Ministry of Education, computer labs at JEC schools were used to teach students how to use software such as Microsoft Word and Microsoft Excel.

Our analysis uncovers a positive impact of the program on socio-emotional skills and technical skills, summarized in our constructed indexes with effects by 0.17 and 0.16SD, respectively (Table 12). The former is explained by an increase in aspirations, self-efficacy, and self-esteem, whereas the latter is driven by an increase in access to digital media and self-reported knowledge of English.

Other channels

Finally, we use data from ECE-S to explore whether JEC led to changes in parental involvement.[25] Appendix Figure A4 displays the reduced-form relation for all five outcomes against the number of sections. This visual inspection does not suggest a change in parental behavior. Appendix Table A8 indicates a null effect from JEC, using 2SLS, on whether students talk to their parents about homework (column 1), whether parents explain topics (column 3) and if parents care about the students' grade (column 4). There is a slight increase in the probability that parents help with homework (column 2) that is significant at $p<0.05$, but also a marginal decline in the probability that parents recommend books to their children (column 5). If anything, this negative effect is consistent with the weaker impact of JEC on reading test scores.

Next, we investigate students' own perception about reading and math separately in response to JEC. The reduced-form graphs tend to suggest an overall lack of behavioral change (Figure A5, Appendix) but negative for some reading outcomes (Panel A) and less so for math (Panel B). The 2SLS estimates reported in Appendix Table A9 confirm that, if anything, the negative responses tend to be centered on reading (Panel A). Students in JEC schools declare that they are less likely to understand hard topics (column 3), feel less confident on tests (column 4)

[25] Each question had a multiple-choice response (i.e., never, rarely, very often, always). Answers selecting "very often" or "always" were coded as one and zero otherwise.

and on passing the course (column 7) as well as less likely to seeing themselves as good at solving reading-related problems (column 8). In math, Panel B, such negative effects are not found. Students, however, are less likely to help their peers, suggestive a more competitive attitude. Again, the differential impact of JEC on math and reading could be explained by how the reform has altered the students' own perceptions as we observe that students in reading are more likely to feel marginalized and to have a lower self-perception.

This analysis is complemented with the students' report about their teachers and their courses. An important drawback is the fact that the survey framed this set of questions by asking students to combine their view of the math and reading courses. The reduced-form graphs, Figure A6 (Appendix), suggest very little change around the threshold and it is confirmed with the 2SLS estimates reported in Table A10. Given the combination of subjects it is impossible to conclude whether there is no change in teachers at all or if, as before, the negative behavioral responses are more likely to be present in reading than in math, leading to null net effect.

6. Conclusions

This paper evaluates a policy that seeks to improve education quality and learning outcomes in a developing country by increasing pedagogical hours, whilst also adding accompanying investments in school inputs. The JEC program expanded the school day from 35 to 45 pedagogical hours a week in Peruvian public schools. We exploit an arbitrary rule used in the selection of schools into the program to identify the effect on math, reading and other outcomes.

We find that the JEC program leads to more learning as measured by standardized test scores. The effects are robust and larger for math relative to the literature. They are somewhat smaller and slightly less robust, but positive, for reading. Exploring several other datasets allows us to investigate key mechanisms of impact including school resources, teacher attitudes and time use, and pupil behavioral responses. We find that students do not substitute their time from studying but rather work fewer hours outside of school and decrease their leisure time. We also see improvements in student access to digital technology, increased technical and socioemotional skills, and greater support to teachers to improve their pedagogical practice, in addition to the gains from increasing instructional time.

Overall, our results suggest that, with targeted investment, it is possible to improve the quality of public sector education.

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Tables and Figures

Table 1: Distribution of hours by type of public high school

Subject	Simple	JEC
Mathematics	4	6
Reading	4	5
English	2	5
Science	3	5
History	3	3
Work education	2	3
Civics	2	3
Person, family & community	2	2
Physical education	2	2
Art	2	2
Religion	2	2
Tutoring	1	2
Free	6	5
Total	35	45

Note: A pedagogical hours is 45 minutes long. Source: MINEDU

Table 2. First stage: Participation in JEC

	Dependent variable: School participates in JEC (=1)				
Sample:	All	All	Urban	Urban	7 and 8 sections
	(1)	(2)	(3)	(4)	(5)
Section≥8	0.479***	0.461***	0.491***	0.446***	0.518***
	[0.020]	[0.027]	[0.025]	[0.039]	[0.048]
Spline	Linear	Quadratic	Linear	Quadratic	
N	8473	8473	4419	4419	571
R^2	0.438	0.441	0.444	0.448	0.458

Note: Robust standard errors clustered at the school district are in brackets. The unit of observation is the school. All regressions were estimated using a linear probability model and include splines for the running variable (except for column 5), fixed effects by school district, urban location and shift. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Smoothness test at school level

	Dependent variable								
	School/District receives welfare program		Length of school day (hours)	Share of students with indigenous languages	Schools teaches in indigenous languages	Proportion of girls in the school	Passing rate		
	Juntos	Crecer					Boys	Girls	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean of the dependent variable	0.470	0.762	5.68	0.375	0.113	43.1	0.665	0.721	0.691
Panel A. All schools									
Section>=8	-0.003	0.008	-0.028	-0.035	0.006	0.015**	0.033***	0.031***	0.035***
	[0.020]	[0.018]	[0.038]	[0.025]	[0.017]	[0.007]	[0.009]	[0.009]	[0.008]
N	8473	8473	8074	8067	8059	8473	8291	8379	8473
adj. R-sq	0.649	0.506	0.059	0.355	0.136	0.059	0.238	0.161	0.211
Panel B. Only urban schools									
Section>=8	-0.037	-0.017	0.022	-0.019	0.008	0.006	0.006	0.015	0.011
	[0.024]	[0.029]	[0.065]	[0.034]	[0.023]	[0.011]	[0.018]	[0.017]	[0.016]
N	4419	4419	4251	4247	4244	4419	4242	4334	4419
adj. R-sq	0.645	0.523	0.033	0.262	0.089	0.037	0.219	0.225	0.233
Panel C. Schools with 7 or 8 sections									
Section>=8	0.022	0.022	0.038	-0.075	-0.042	-0.011	-0.002	-0.003	-0.004
	[0.041]	[0.037]	[0.043]	[0.056]	[0.036]	[0.009]	[0.017]	[0.016]	[0.014]
N	571	571	543	543	542	571	566	570	571
adj. R-sq	0.618	0.527	0.037	0.374	0.037	0.313	0.206	0.203	0.229

Note: Robust standard errors clustered at the school district are in brackets. The unit of observation is the school. All regressions were estimated using a linear probability model and include linear splines for the running variable (except for panel C), fixed effects by school district, urban location and shift. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Smoothness test at the student level

	Dependent variable:						
	Mother's characteristics		Student characteristics				
						Grade repetition (1^{st}-7^{th} grade)	
	High school graduate or more (=1)	Speaks Spanish (=1)	Age in years	Sex (Girl=1)	Attended kindergarten (=1)	Ever (=1)	Number of grades
	(1)	(2)	(1)	(2)	(5)	(6)	(7)
Section≥8	-0.023	0.022	-0.003	-0.018	-0.004	0.000	0.002
	[0.019]	[0.019]	[0.033]	[0.017]	[0.014]	[0.015]	[0.022]
N	13233	13222	13118	13499	13124	13067	13506
adj. R^2	0.109	0.373	0.037	0.008	0.038	0.033	0.020
Mean dep. var.	0.319	0.859	13.76	0.482	0.787	0.316	0.398

Note: Robust standard errors clustered at the school district in brackets. Sample is restricted to urban public schools with seven or eight sections. Each column reports OLS estimates using the discontinuity at eight sections. All regressions include fixed effects by school district and shift. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. Impact of JEC on academic achievement (2SLS)

Sample:	All	Urban	Morning shift	Urban and morning shift
	(1)	(2)	(3)	(4)
Panel A. Dependent variable: Reading test scores				
JEC	0.149***	0.240***	0.096**	0.185***
	[0.048]	[0.064]	[0.041]	[0.054]
	{0.044}	{0.049}	{0.038}	{0.048}
N	360,154	296,063	189,630	131,337
Adj-R2	0.262	0.191	0.315	0.260
F-stat	395.9	213.5	502.6	316.2
Panel B. Dependent variable: Reading Pr(grade level)				
JEC	0.043***	0.061***	0.029***	0.046***
	[0.009]	[0.013]	[0.008]	[0.013]
	{0.009}	{0.011}	{0.007}	{0.010}
N	360,154	296,063	189,630	131,337
Adj-R2	0.083	0.072	0.123	0.111
F-stat	395.9	213.5	502.6	316.2
Panel C. Dependent variable: Math test scores				
JEC	0.243***	0.307***	0.179***	0.233***
	[0.053]	[0.073]	[0.046]	[0.068]
	{0.045}	{0.046}	{0.034}	{0.040}
N	360,076	295,986	189,609	131,316
Adj-R2	0.197	0.159	0.240	0.206
F-stat	396.2	213.8	502.3	315.9
Panel D. Dependent variable: Math Pr(grade level)				
JEC	0.053***	0.072***	0.042***	0.058***
	[0.011]	[0.015]	[0.010]	[0.014]
	{0.009}	{0.012}	{0.007}	{0.009}
N	360,076	295,986	189,609	131,316
Adj-R2	0.055	0.051	0.074	0.072
F-stat	396.2	213.8	502.3	315.9

Note: Robust standard clustered at the school district are shown in brackets and by section in {}. Each column reports 2SLS using the discontinuity at 8 sections and with linear splines. All regressions include controls for age and gender of the student, as well as fixed effects for their mothers' educational attainment and language spoken together with fixed effects for urban/rural, shift and school district. *F-stat* refers to the instrument in the first stage. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 6. First stage and reduced form using local randomization

	First stage	Reduced-form			
		Reading		Math	
	JEC	Z-score	Grade-level	Z-score	Grade-level
	(1)	(2)	(3)	(4)	(5)
1(S>=8)	0.494	0.037	0.007	0.095	0.020
P-values	[0.000]	[0.010]	[0.112]	[0.000]	[0.000]
Sample size					
S=7	6631	6626	6626	6625	6625
S=8	6875	6871	6871	6869	6869

Note: P-values for local randomization shown in brackets. Each column represents a separate regression. Sample is restricted to public urban schools with seven or eight sections.

Table 7. Impact of JEC using differences-in-discontinuity (2SLS)

Sample:	All	Only Urban
	(1)	(2)
Panel A. Dependent variable: Reading test scores		
JEC	0.121***	0.230***
	[0.042]	[0.058]
N	360154	296063
R2-adjust.	0.262	0.192
F-stat	254.1	167.3
Panel B. Dependent variable: Reading Pr(grade level)		
JEC	0.033***	0.054***
	[0.008]	[0.014]
N	360154	296063
R2-adjust.	0.084	0.072
F-stat	254.1	167.3
Panel C. Dependent variable: Math test scores		
JEC	0.208***	0.289***
	[0.048]	[0.071]
N	360076	295986
R2-adjust.	0.197	0.160
F-stat	254.0	167.2
Panel D. Dependent variable: Math Pr(grade level)		
JEC	0.047***	0.069***
	[0.010]	[0.015]
N	360076	295986
R2-adjust.	0.056	0.052
F-stat	254.0	167.2

Note: Robust standard clustered at the school district are shown in brackets. Each regression reports 2SLS estimates using the difference in discontinuity at 8 sections and with linear splines, comparing schools with only-morning shifts and all other schools . All regressions include controls for age and gender of the student, as well as fixed effects for their mothers' educational attainment and language spoken together with fixed effects for urban/rural, shift and school district. F-stat refers to the instrument in the first stage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Subject differences: reduced form

	All	Only urban	Only urban and morning shift	Only 7 and 8 sections
	(1)	(2)	(3)	(4)
Math (=1)	0.059***	0.022	0.047*	0.070***
	[0.018]	[0.023]	[0.025]	[0.017]
Math*1(S≥8)	-0.013	0.007	0.055**	0.049**
	[0.016]	[0.022]	[0.023]	[0.020]
N	720230	592049	262653	37934
adj. R^2	0.013	0.003	0.010	0.019
Mean	-0.166	-0.054	-0.116	-0.416
Reduced form gains in math	0.127	0.156	0.161	0.070

Note: Robust standard errors clustered at the school district in brackets. Each column reports reduced form regression. All regressions include students fixed effects as well as linear splines that vary by subject. The binary variable for the threshold is dropped due to the inclusion of students fixed effects. In column 4, due to the narrower bandwidth, splines are not included. The last row (reduced form gains in math) is the coefficient for 1(S≥8) in a reduced form regression for math including the controls as in Table 5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Impact of JEC on time use of students (in hours, 2SLS)

	Coef	s.e.	Adjusted p-value	n
Student time use in a typical school day				
Sleeping	-0.219*	(0.116)	0.105	1,174
Caring for household members	-0.083	(0.106)	0.553	1,174
Household chores	-0.393***	(0.087)	0.000	1,174
Domestic tasks	-0.514***	(0.131)	0.000	1,174
Paid activity	0.057	(0.097)	0.668	1,174
In school	2.145***	(0.136)	0.000	1,174
Studying outside school	-0.085	(0.092)	0.476	1,174
Leisure activities	-0.728***	(0.151)	0.000	1,174

Notes: Young Lives data. Robust standard errors clustered at the community level in brackets. Adjusted p-values correct for multiple hypothesis testing. Sample is restricted to children attending public schools. Each column reports 2SLS estimates using the discontinuity at eight sections. All regressions include linear splines, controls for child's age, sex and language, mother's language plus fixed effects by community and shift. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Impact of JEC on school infrastructure, staff resources, and pedagogy (2SLS)

	Coef	s.e.	Adjusted p-value	n
Panel A: School access to IT (index)	***0.807****	***(0.131)***		***6,226***
Number of classrooms with computer	0.557*	(0.302)	0.078	6,226
Number of classrooms with laptop	3.722***	(0.710)	0.000	6,226
Technical equipment receives maintenance	0.434***	(0.034)	0.000	6,141
Kit Robotica	-0.017	(0.035)	0.657	6,141
Panel B: School staff (index)	***1.691****	***(0.161)***		***6,226***
Complete teaching staff	-0.091**	(0.039)	0.023	6,226
Number of security staff	1.379***	(0.094)	0.000	6,226
Number of maintenance staff	0.109	(0.150)	0.499	6,226
Panel C: School pedagogical support (index)	***5.109****	***(0.148)***		***6,226***
School has a psychologist	0.807***	(0.025)	0.000	6,226
Schools receives program "*Acompañamiento Pedagogico*"	0.732***	(0.034)	0.000	6,226
Teachers provide support to parents	0.296***	(0.031)	0.000	6,226

Note: SEMAFORO data. Sample includes all public schools in the sample. Robust standard errors clustered at the school district level in brackets. Adjusted p-values correct for multiple hypothesis testing. Sample is restricted to children attending public schools. Each column reports 2SLS estimates using the discontinuity at eight sections. All regressions include linear splines, an urban dummy, and fixed effects fpr school district, shift, and month of interview. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 11: Impact of JEC on teacher characteristics, training, and behaviors (2SLS)

	Coef	s.e.	Adjusted p-value	n
Panel A: Teacher's time use				
Teacher time dedicated to school activities (weekdays) index	-0.127	(0.128)	0.363	2,825
Teacher time dedicated to school activities (weekend) index	0.051	(0.097)	0.639	2,825
Teacher time dedicated to activities outside school (weekdays) index	0.055	(0.116)	0.673	2,825
Teacher time dedicated to activities outside school (weekend) index	0.084	(0.083)	0.354	2,825
Spent time in other income activities	0.148**	(0.071)	0.048	2,825
Other income activity: work in other school (public)	0.027*	(0.014)	0.076	2,825
Other income activity: work in other school (private)	0.066**	(0.031)	0.046	2,825
Other income activity: own business	0.068	(0.051)	0.217	2,825
Teach in more than 1 high-school	0.011	(0.049)	0.840	2,801
Panel B: Teacher attitudes and satisfaction (index)	***-0.098***	***(0.087)***		2,825
Total score on items indicating teacher's satisfaction	-0.279	(0.595)	0.679	2,825
Teacher's satisfaction with his/her job	0.018	(0.101)	0.877	2,824
Positive perception of teacher profession	-0.019	(0.069)	0.811	2,825
Would choose again to be a teacher	-0.155**	(0.063)	0.019	2,825
Teacher is happy with current work	-0.009	(0.050)	0.878	2,823
Decided to be a teacher by choice	-0.037	(0.057)	0.565	2,817
Panel C: Teacher training & pedagogy (index)	***0.271****	***(0.083)***		2,825
Number of subjects currently teaching	-0.556***	(0.123)	0.000	2,805
Developed innovative practices	-0.059	(0.074)	0.473	2,825
Top quintile in good teaching practice score	0.043	(0.061)	0.526	2,817
Received "Acompañamiento pedagógico"	0.248***	(0.074)	0.001	2,825
Received ICT training	0.110	(0.074)	0.169	2,824
Panel D: Teacher predetermined characteristics (index)*	***0.028***	***(0.066)***		2,825
Age	-0.615	(1.312)	0.678	2,819
Female	0.074	(0.081)	0.406	2,825
Completed studies at university	0.089	(0.081)	0.313	2,621
Completed studies at private institution	-0.035	(0.058)	0.592	2,617
Completed any post-graduate studies	0.020	(0.061)	0.779	2,825
Number of years teaching in current secondary school	1.939	(2.175)	0.419	1,770
Top Levels at Escala Magisterial	-0.081	(0.050)	0.134	2,820
Teacher has a permanent contract ("Nombrado/a")	-0.083	(0.079)	0.334	2,825

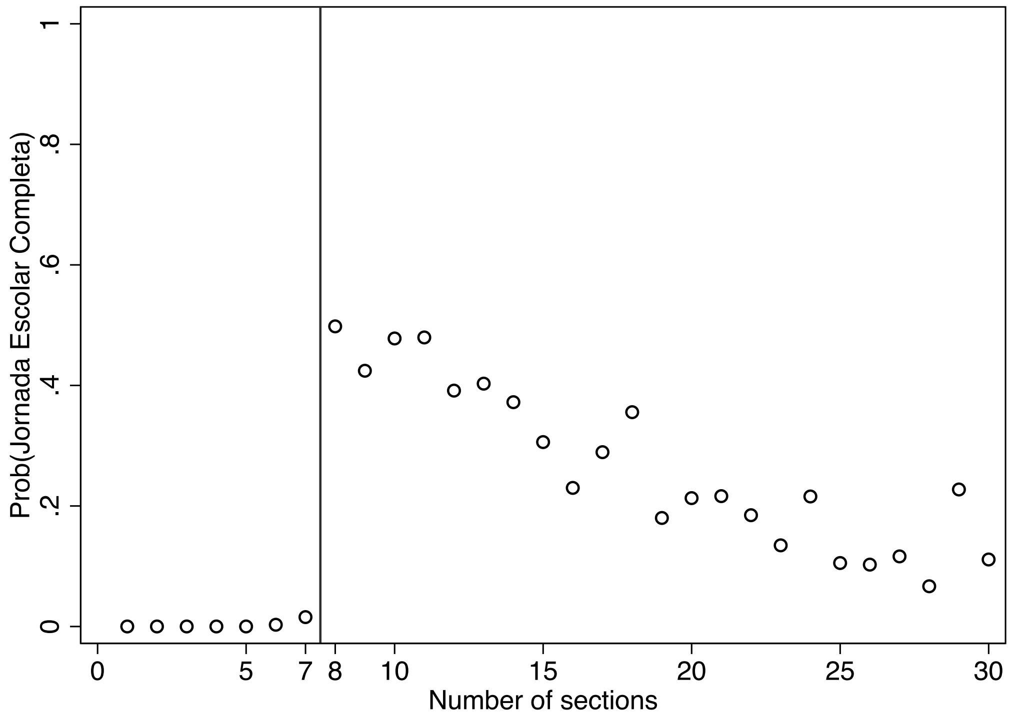
Note: ENDO data. Sample includes all public schools in the sample. Robust standard errors clustered at the school district level in brackets. Adjusted p-values correct for multiple hypothesis testing. Each column reports 2SLS estimates using the discontinuity at eight sections. Sample is restricted to children attending public schools. All regressions include linear splines, controls for child's age, sex and language, mother's language plus fixed effects by community and shift. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 12. Impact of JEC on non-cognitive and technical skills (2SLS)

	Coef	s.e.	Adjusted p-value	n
Student socio-emotional skills (index)	***0.172*****	***(0.079)***		1,174
Aspirations for higher education	0.019	(0.019)	0.455	1,174
Aspirations for university	0.065**	(0.026)	0.024	1,174
Self-efficacy	0.148**	(0.070)	0.066	1,174
Self-esteem	0.142**	(0.066)	0.059	1,174
Pride	0.088	(0.105)	0.517	1,174
Agency	0.102	(0.094)	0.395	1,174
Student technical skills (index)	***0.164****	***(0.097)***		1,174
Speak English	0.289***	(0.050)	0.000	1,174
Access Digital	0.269***	(0.066)	0.000	1,174
Computer skills	-0.057	(0.061)	0.472	1,174
Internet skills	-0.070	(0.055)	0.302	1,174

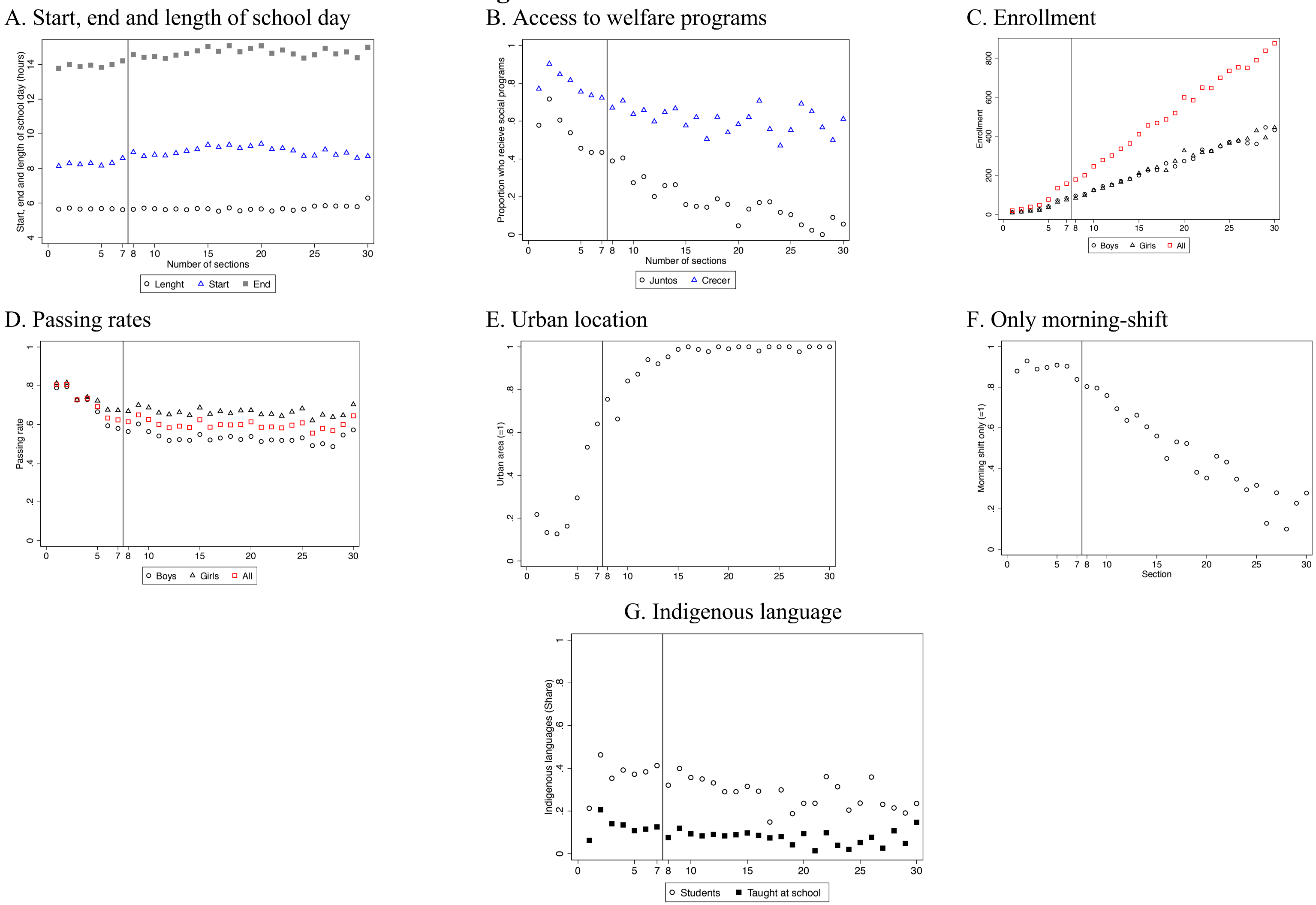
Notes: Young Lives data. Robust standard errors clustered at the community level in brackets. Adjusted p-values correct for multiple hypothesis testing. Sample is restricted to children attending public schools. Each column reports 2SLS estimates using the discontinuity at eight sections. All regressions include linear splines, controls for child's age, sex and language, mother's language plus fixed effects by community and shift. * p < 0.10, ** p < 0.05, *** p < 0.01.

Figure 1. First Stage: participation in JEC by number of sections



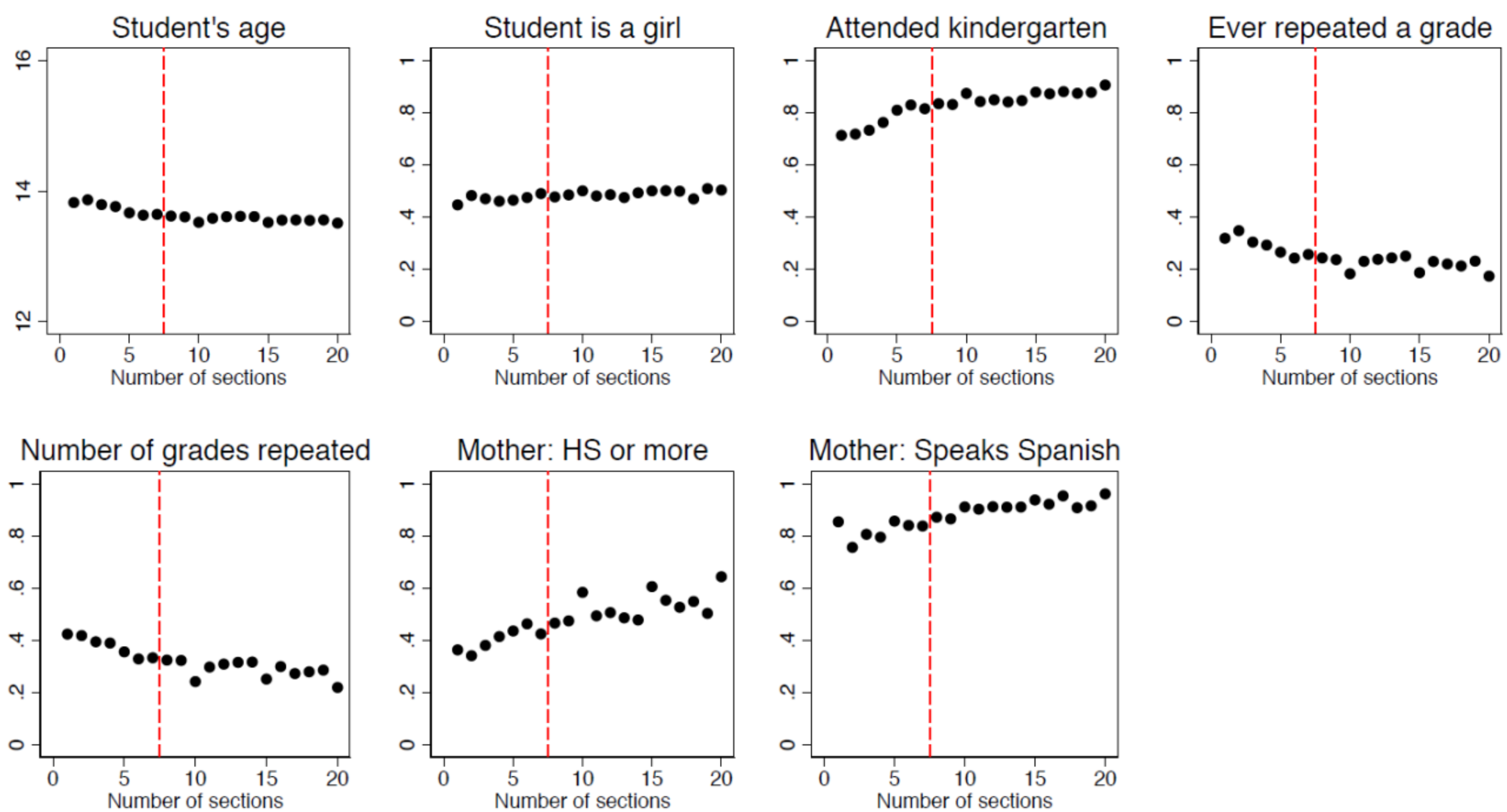
Note: Each circle represents the share of schools that belong to JEC by their number of sections. Sample restricted to all public secondary schools. *Source*: Author's calculation based on *2013 Censo Escolar*.

Figure 2. Smoothness tests at the school level



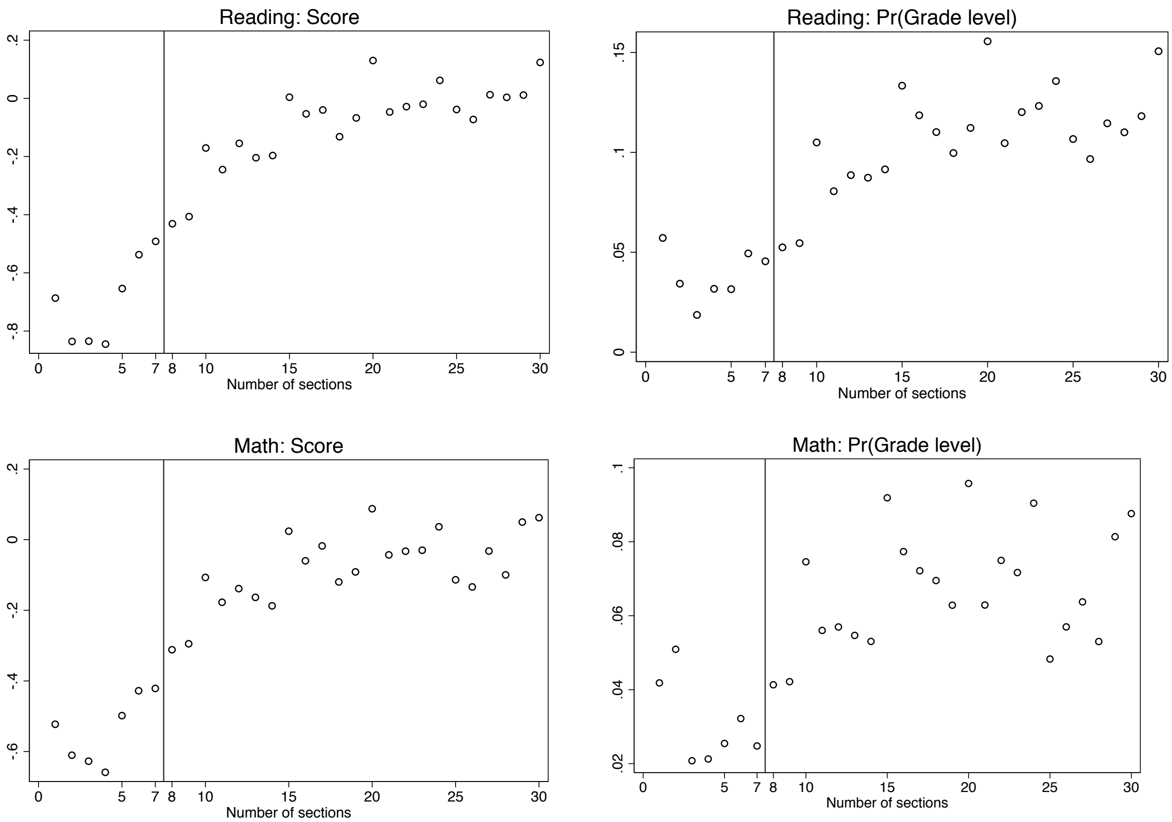
Note: Each symbol represents the sample average by section. Sample restrcited to all public schools. Data source: 2013 *Censo Escolar*.

Figure 3: Smoothness tests at the student level



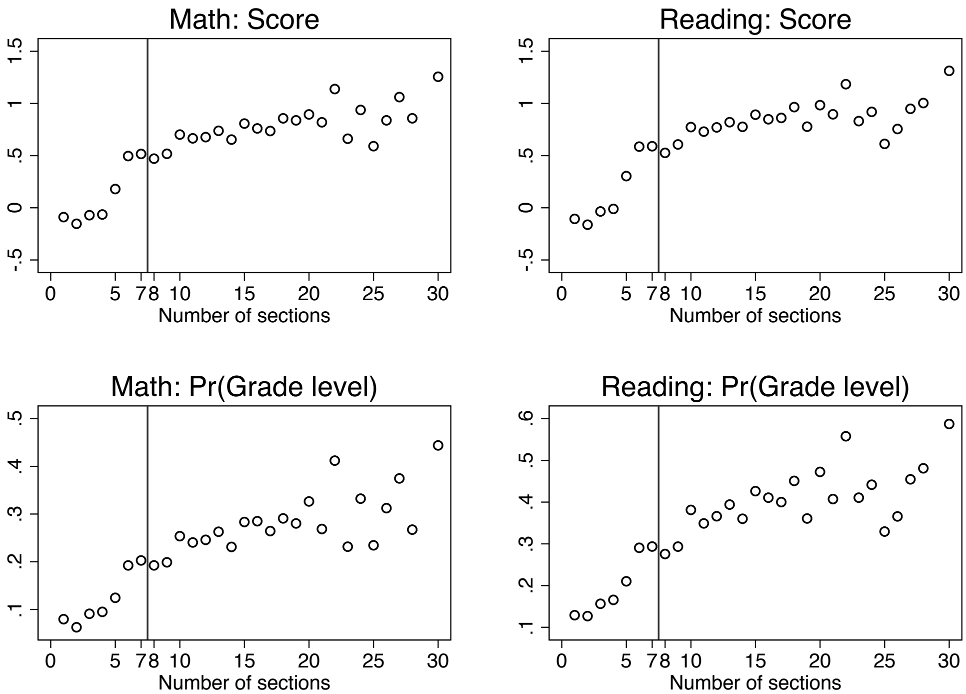
Note: Each circle represents the sample average for students by the number of sections in their schools. Sample restricted to all public secondary schools. *Source*: Author's calculation based on *ECE-S*.

Figure 4. Impact of JEC on test scores (reduced form)



Note: Each symbol represents the sample average by section. Sample restricted to all public schools.
Data source: 2015 ECE-S.

Figure 5: Placebo test: number of sections and test scores for private schools (reduced form)



Note: Each symbol represents the sample average by section. Sample includes private schools only. Data source: 2015 ECE-S.

Appendix

Tables and Figures

Table A1: Selection process: JEC high schools

1.	Rules for selection chosen: - Public high schools - Schools with morning shift only - Eight or more 'Sections' - School facilities used only in the morning - Schools with sufficient space to install new classrooms. *These 5 criteria yielded 1,360 high schools*
2.	52 "emblematic" high schools were added (1,412 schools)
3.	List was sent to local coordinators for validation. New list with 1,343 schools. These schools now needed to provide additional information.
4.	Based on this information and depending on the date of arrival, 1,000 public schools were selected for JEC.
5.	In September of 2014 JEC is created (RM Nº 451-2014-MINEDU).
6.	On February 10, 2015 the list was modified replacing six schools (RM Nº 062-2015-MINEDU). This was the final list of schools included in JEC in 2015.

Source: MINEDU (2015) "Proceso de selección la JEC 2015" and "Criterios de Selección IIEE 2015 a 2017". Note that a section is equivalent to a homeroom in the US system, and a form class in the UK system.

Table A2: Summary statistics: Standardized test (ECE-S)

		Test score				
	N	Z-score	Lowest level	2 years behind	1 year behind	Grade-level
		Panel A: All schools				
Reading	477,088	0.000	0.234	0.391	0.227	0.148
Math	476,962	0.000	0.373	0.404	0.127	0.096
		Panel B: All public schools				
Reading	360,154	-0.181	0.281	0.421	0.200	0.098
Math	360,076	-0.152	0.424	0.407	0.107	0.062
		Panel C: Public schools in urban areas only				
Reading	296,063	-0.050	0.223	0.434	0.227	0.115
Math	295,986	-0.058	0.378	0.430	0.120	0.072

Note: Author's calculation based on 2015 ECE-S. Scores were transformed into a z-score with mean zero and standard deviation equal to one based on the full sample.

Table A3. Datasets used in the mechanism analysis

Name of dataset	Year of data collection	Unit of observation
Young Lives (math and reading test scores, socio-emotional outcomes, technical skills)	2016	Child
Encuesta Nacional a Docentes (time use of teachers)	2016	Teacher
Semáforo Escuela (school infrastructure including staff, IT, pedagogical programs and teacher's characteristics	2016	Schools and teachers

Table A4: Robustness checks: Quadratic splines (2SLS)

Sample:	All	Urban	Morning shift	Urban and morning	All: Diff in RD	Urban: Diff in RD.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: Reading test scores						
JEC	0.137*	0.203*	0.080	0.126	0.112	0.170**
	[0.083]	[0.107]	[0.066]	[0.078]	[0.069]	[0.083]
N	360154	296063	189630	131337	360154	296063
Adj-R2	0.262	0.192	0.315	0.262	0.263	0.194
F-stat	152.8	63.7	314.2	199.5	179.2	119.7
Panel B. Dependent variable: Reading Pr(grade level)						
JEC	0.032**	0.049**	0.020	0.032*	0.025*	0.040**
	[0.015]	[0.024]	[0.013]	[0.018]	[0.013]	[0.019]
N						
Adj-R2	360154	296063	189630	131337	360154	296063
F-stat	0.084	0.072	0.123	0.112	0.084	0.073
Panel C. Dependent variable: Math test scores						
JEC	0.260***	0.255**	0.166**	0.134	0.204***	0.186**
	[0.086]	[0.120]	[0.069]	[0.086]	[0.072]	[0.091]
N						
Adj-R2	360076	295986	189609	131316	360076	295986
F-stat	0.196	0.161	0.241	0.209	0.198	0.163
Panel D. Dependent variable: Math Pr(grade level)						
JEC	0.051***	0.072***	0.034***	0.043***	0.041***	0.052***
	[0.015]	[0.021]	[0.012]	[0.015]	[0.013]	[0.016]
N						
Adj-R2	360076	295986	189609	131316	360076	295986
F-stat	0.055	0.052	0.075	0.074	0.056	0.054

Note: Robust standard clustered at the school district are shown in brackets. Each column reports 2SLS estimates using the discontinuity at 8 sections and with quadratic splines. Columns 5 and 6 do not include splines and are estimated using differences in discontinuities. F-stat refers to the instrument in the first stage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Placebo test: private schools (reduced form)

	Sample: private schools with 7 or 8 sections only			
	All	Only urban	Only morning-shift	Only urban and morning shift
	(1)	(2)	(3)	(4)
Panel A: Reading (dependent variable: z-score)				
1(Section≥8)	-0.001	-0.001	0.031	0.031
	[0.054]	[0.054]	[0.053]	[0.053]
N	8652	8604	7677	7629
adj. R^2	0.102	0.095	0.113	0.106
Y-bar	0.553	0.558	0.566	0.573
Panel B: Reading (dependent variable: placed at grade level)				
1(Section≥8)	0.001	0.001	0.011	0.011
	[0.025]	[0.025]	[0.025]	[0.025]
N	8652	8604	7677	7629
adj. R^2	0.055	0.053	0.061	0.059
Y-bar	0.283	0.284	0.289	0.290
Panel C: Math (dependent variable: z-score of test scores)				
1(Section≥8)	0.015	0.015	0.051	0.051
	[0.072]	[0.072]	[0.072]	[0.072]
N	8654	8606	7678	7630
adj. R^2	0.086	0.084	0.089	0.086
Y-bar	0.489	0.493	0.509	0.513
Panel D: Math (dependent variable: placed at grade level)				
1(Section≥8)	0.004	0.004	0.009	0.009
	[0.021]	[0.021]	[0.021]	[0.021]
N	8654	8606	7678	7630
adj. R^2	0.046	0.046	0.046	0.045
Y-bar	0.197	0.197	0.204	0.205

Note: Robust standard errors clustered at the school district in brackets. Each column reports reduced form using discontinuity at eight sections. All regressions include controls for students' and their parents' characteristics as in Table 5 in the main text. Y-bar refers to the mean of the outcome variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Impact of JEC on academic achievement using the Young Lives sample (2SLS)

Dependent variable:	YL math test score	YL reading test score
	(1)	(2)
JEC	0.248***	0.297**
	[0.095]	[0.132]
N	1,174	1,174
R2-adjust.	0.135	0.157

Notes: Young Lives data. Robust standard errors clustered at the community level in brackets. Sample is restricted to children attending public schools. Each column reports 2SLS estimates using the discontinuity at eight sections. All regressions include linear splines, controls for child's age, sex and language, mother's language plus fixed effects by community and shift. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Evidence of sorting in JEC schools

Pre-JEC Cognitive and socio-emotional skills (measured in 2013)	Mean of stayers (attending school eligible for JEC, in 2014)	N	Mean of movers (attending school non-eligible for JEC, in 2014)	N	P-value
Math test scores	0.05	325	-0.40	57	0.002
Reading test scores	-0.03	325	-0.57	57	0.000
Aspirations for higher education	0.96	321	0.96	56	0.787
Aspirations for university	0.86	321	0.84	56	0.640
Self-efficacy index	0.00	324	-0.20	57	0.006
Self-esteem index	0.01	324	-0.10	57	0.154
Pride index	-0.03	324	-0.28	57	0.019
Agency index	-0.02	324	-0.18	57	0.118

Notes: The information about whether the child was attending a school eligible for JEC in 2014 comes from the education history module administered in 2016 (Round 5). Selected outcomes (pre-JEC) were measured in 2013 (Round 4).

Table A8: Parental behaviour with respect to students' coursework (2SLS)

Dependent variable:	Student talks to parents	Parents help	Parents explain topics	Parents care about grades	Parents recommend books
	(1)	(2)	(3)	(4)	(5)
JEC	0.006	0.034*	-0.021	0.020	-0.041**
	[0.022]	[0.020]	[0.022]	[0.017]	[0.019]
N	287194	286529	285389	285136	285265
R2-adjust.	0.023	0.026	0.038	0.031	0.024
F-stat	219.0	219.0	217.4	218.4	218.6
Mean	0.455	0.292	0.413	0.811	0.599

Note: Robust standard clustered at the school district are shown in brackets. Each column reports 2SLS estimates using differences in discontinuity at 8. The sample is limited to public schools in urban areas. *F-stat* refers to the instrument in the first stage. Mean refers to the average of the dependent variable.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Students' self-perceptions (2SLS)

	Understands any topic	Learns without difficulty	Understand hard topics	Confident on test	Helps her/his peers	Does homework without help	Confident on passing course	Good at solving problems	Feels capable as I learn	Feels h/she is good at the subject
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Perceptions about reading										
JEC	-0.014	-0.033	-0.036*	-0.040**	-0.013	-0.015	-0.033*	-0.048**	-0.026	-0.026
	[0.017]	[0.021]	[0.019]	[0.018]	[0.020]	[0.021]	[0.018]	[0.022]	[0.019]	[0.021]
N	278307	282404	282986	281505	279835	282129	282342	282022	282260	282877
Panel B. Perceptions about math										
JEC	-0.014	-0.012	0.005	0.018	-0.061**	-0.030	0.020	-0.010	-0.002	-0.009
	[0.024]	[0.023]	[0.022]	[0.024]	[0.025]	[0.028]	[0.022]	[0.025]	[0.019]	[0.024]
N	285074	284009	281519	283751	283502	283679	282155	281325	281593	279529

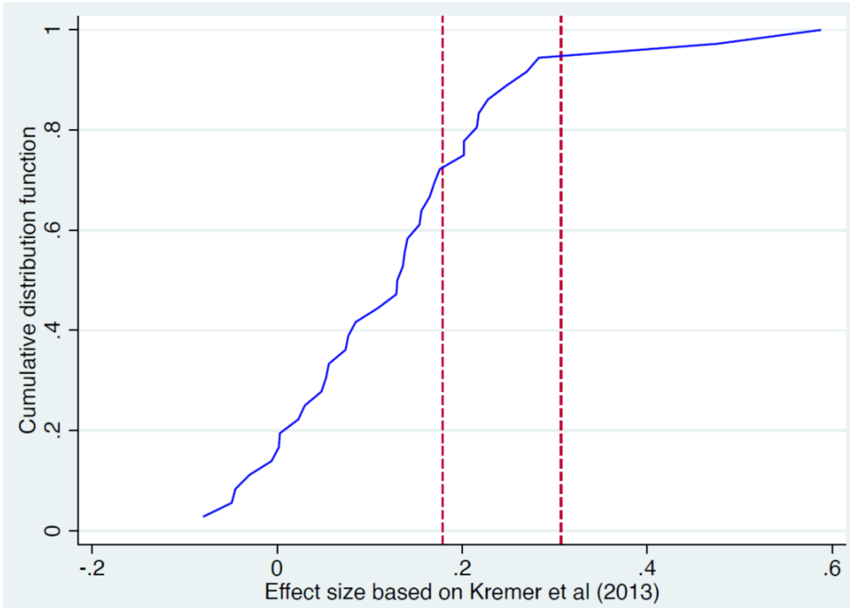
Note: Robust standard clustered at the school district are shown in brackets. Each column reports 2SLS estimates using differences in discontinuity at 8. The sample is limited to public schools in urban areas. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Teachers' responses (2SLS)

	Math and reading teachers combined:							
	Start class explaining what's ahead	Start class with summary of previous lecture	Use real world examples	Use different materials	They relate topics	Start class by asking what we know	Move too fast to next topic	Ask for our arguments and ideas
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
JEC	-0.006	-0.012	-0.005	0.047**	0.002	-0.008	0.000	0.024
	[0.018]	[0.017]	[0.018]	[0.021]	[0.018]	[0.018]	[0.015]	[0.022]
N	282258	281548	278013	278454	279613	278245	276299	275067
	Demand verbatim responses	Make sure we understood	Supervise we all participate	Leave comments about how to improve	We receive comments about what did wrong	Recognize our errors and explain	Give suggestions on how to learn	Explain what we will learn with homework
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
JEC	-0.021	0.011	-0.006	-0.016	-0.012	0.003	-0.017	-0.008
	[0.019]	[0.016]	[0.017]	[0.021]	[0.022]	[0.017]	[0.016]	[0.018]
N	274432	277826	276131	274650	275711	276078	276367	276799

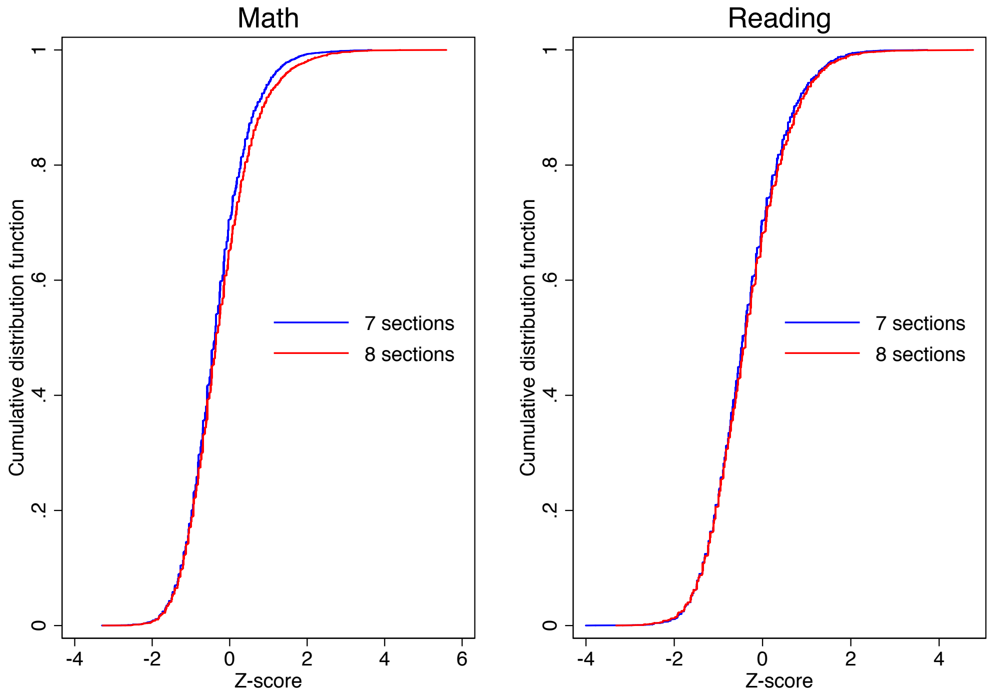
Note: Robust standard clustered at the school district are shown in brackets. Each column reports 2SLS estimates using differences in discontinuity at 8 sections. The sample is limited to public schools in urban areas. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Comparing estimates from JEC against recent randomized studies in education



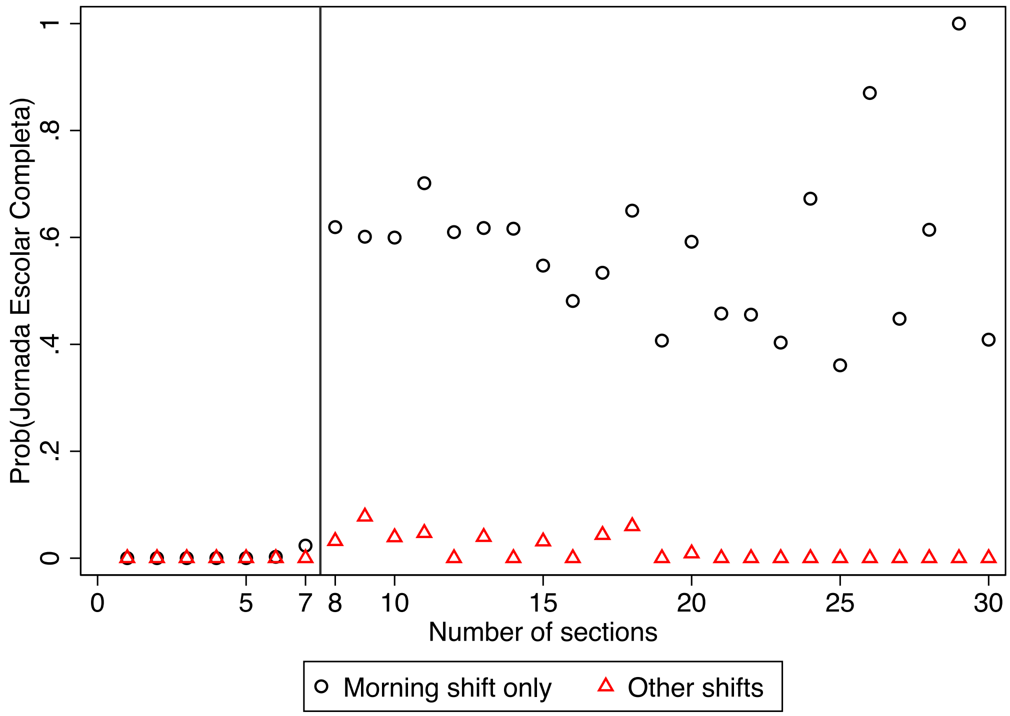
Note: The dashed vertical lines represent the range of estimates of the impact of JEC for math test scores as reported Table 2, Panel C (0.179-0.307). The CDF was obtained from the reported effect sized in Kremer et al (2013) from recent randomized controlled trials on education in developing countries.

Figure A2: Robustness check: Stochastic dominance (reduced form)



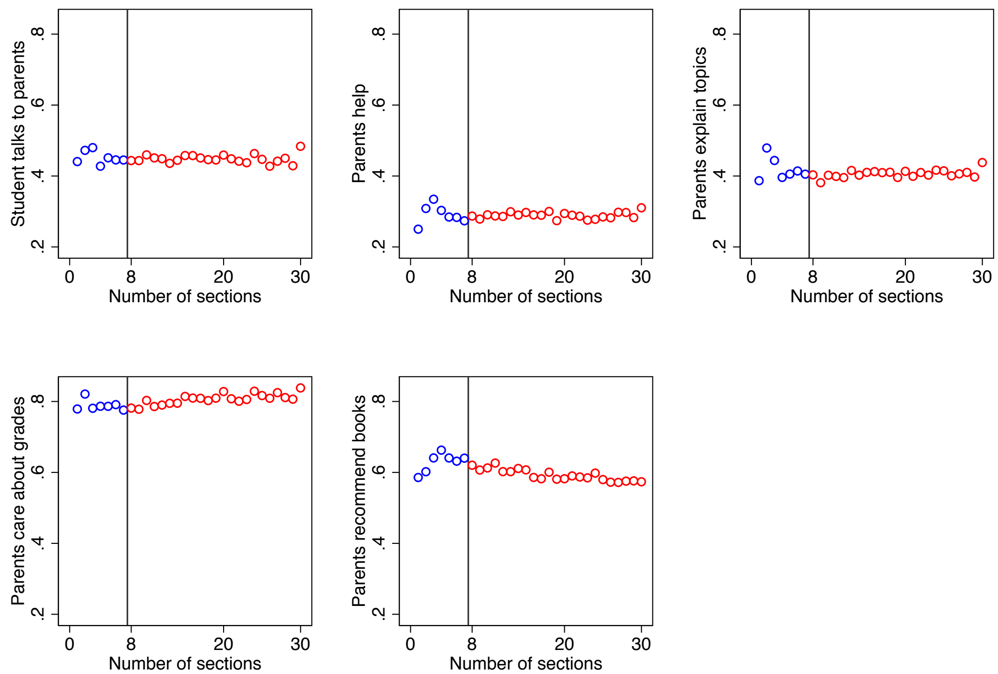
Note: CDF functions estimated separately for students in public schools with seven and eight sections. Data source: 2015 ECE-S.

Figure A3: Participation in JEC by section and type of shift



Note: Each circle represents the share of schools that belong to JEC by their number of sections. Sample includes all public high schools. *Source*: Author's calculation based on *2013 Censo Escolar*.

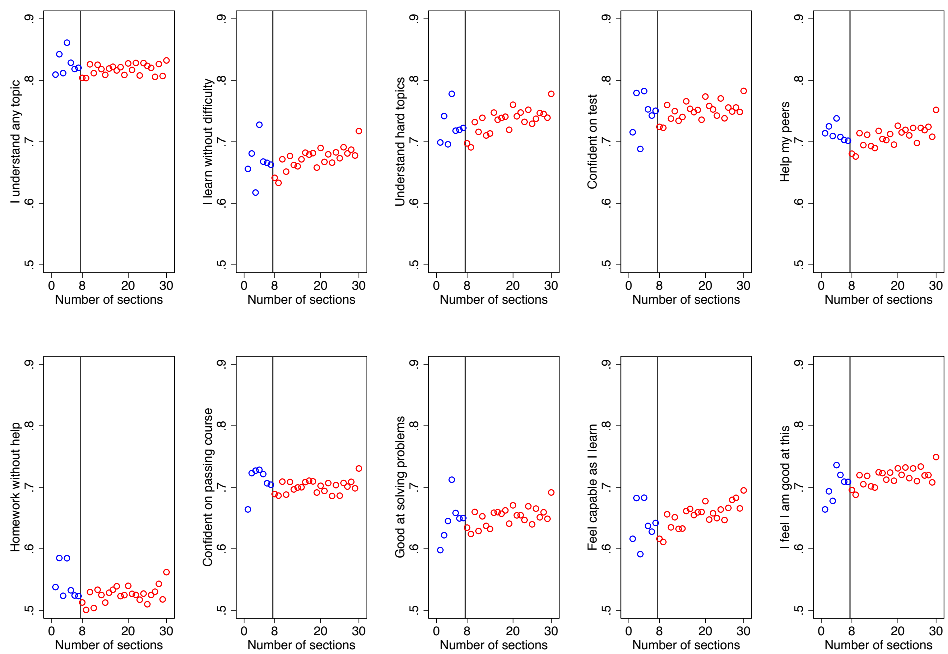
Figure A4: Parental behaviour (reduced form)



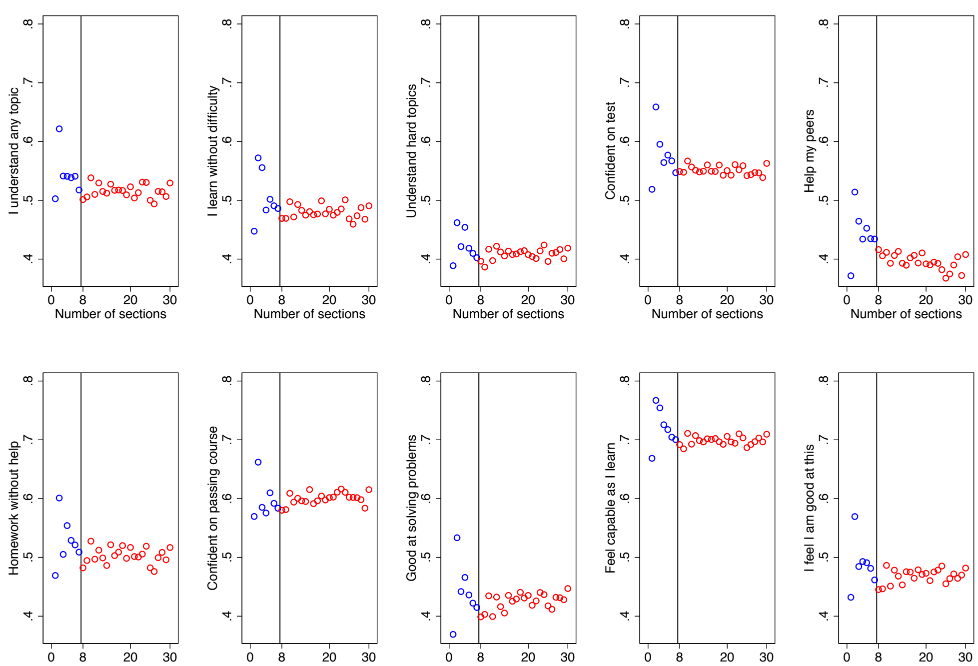
Note: Each symbol represents the sample average by section. Sample is restricted to public schools in urban areas. Data source: 2015 ECE-S.

Figure A5: Students' behaviour (reduced form)

Panel A. Attitudes towards reading

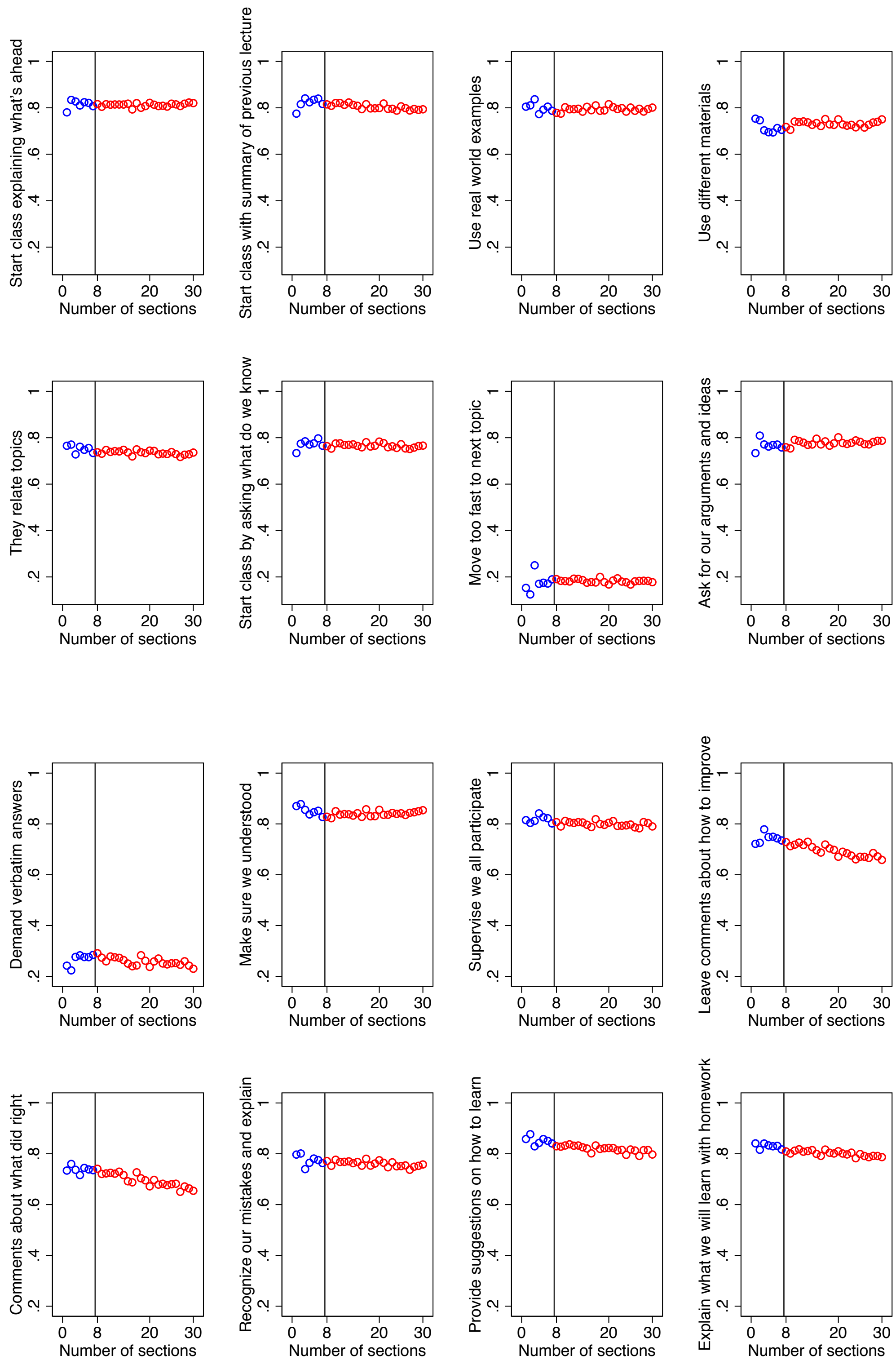


Panel B. Attitudes towards math



Note: Each symbol represents the sample average by section. Sample is restricted to public schools in urban areas. Data source: 2015 ECE-S.

Figure A6: Teachers' behaviour (reduced form)



Note: Each symbol represents the sample average by section. Sample is restricted to public schools in urban areas. Data source: 2015 ECE-S.