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More time less time? The effect of lengthening the school day on learning trajectories

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"¿Más tiempo menos tiempo? Efectos de extender la jornada escolar en las trayectorias escolares"

Resumen

En este trabajo investigamos en qué medida extender la jornada diaria escolar afecta las trayectorias escolares. Usamos datos administrativos nacionales a nivel de escuela para estimar el impacto de más horas de escolaridad en la no promoción de grado en el nivel primario. Los microdatos de Argentina para el periodo 2011-2019, nos permiten explotar la variabilidad que ofrecen 1.297 escuelas que agregaron horas de escolaridad. El hecho de que haya escuelas que cambiaron de jornada simple (4 horas diarias) a jornada extendida (más de 4 horas, pero menos de 8 horas diarias) de forma progresiva y exógena, condicional en la capacidad edilicia preexistente, nos permite estimar su efecto por diferencias en diferencias. Los resultados muestran que extender la jornada escolar reduce la no promoción de grado en los estudiantes de la primaria en un 23,1%.

Palabras clave: No-promoción, aprendizajes, jornada extendida, tiempo escolar

“More time less time? The effect of lengthening the school day on learning trajectories”

Abstract

We investigate to what extent lengthening the primary school days affects learning trajectories. We use national administration reports at the school level to estimate the impact of more school hours on grade retention at the primary level. Using microdata available in Argentina from 2011 to 2019, we use the variation of 1,297 schools that added more hours of instructional time. The fact that the change from a simple regime (4 hours per day) to an extended regime (more than 4 hours but less than 8) was progressively and exogenous, conditional on infrastructure capacity, allows for estimating the effect through a difference-in-difference approach. We find that lengthening the school day reduces the grade retention of primary students by 23.1%.

Keywords: length day, school day, learning trajectories, time

Códigos JEL: I20; I21; I25.

1. Introduction[1]

At the time this document was being written, the National Minister of Education of Argentina announced an increase in school hours from a simple regime (4 hours) to an extended regimen (more than 4 hours but less than 8 hours). The aim was to recover learning lost in the pandemic of COVID-19[2].

But this was not new at all. In 2005, a law called "Ley de Financiamiento Educativo" (Educational Financing Law) established that within five years, at least 30% of primary students should attend schools with extended regimes (more than 4 hours but less than 8 hours per day) or complete regimes (8 hours per day)[3].

A year later, in 2006, a new law mandated that 100% of primary students must attend either extended regimes or complete regimes[4]. However, by 2019, only 14.1% of primary students attended an extended or complete regime, with a 5.2 percentage point increase between 2011 and 2019 (Catri *et al.*, 2022.b). Also, Hisse and Bottinelli (2019) shows a 9 pp increase in the number of schools with extended or complete regimes from 2011 to 2018, but the proportion of students enrolled in these schools grew by 6 pp.

As interesting as this non-compliance may be from a political economy point of view, the central focus of this paper is to assess to what extent lengthening school hours may lead to improvements in education results. Proponents of extending school time argue about the positive effects on learning and academic trajectory. Opponents suggest that increased time is not guaranteed to lead to more effective instructional time and suggest other costs (for a review of this topic, see Patall, Cooper and Allen (2010)).

There are three ways to increase instructional time. One option could be extending school calendars so that the number of hours per day remains equal but with more days of classes in a year. In Argentina, compulsory education must be of 180 days[5], a bit lower than the OECD average (186 days)[6]. Second, a combination of more hours per day and more days. A third approach could be to increase the hours per day fixing school days in a year. Although this work deals with the latter case, we will argue that there is a possibility that the number of school days in a year has not been completely fixed.

The causal evidence that more instructional time has positive effects on learning trajectories is scarce and weak (Meyer and Van Klaveren, 2013; Patall, Cooper and Allen, 2010).

Studies dealing with more days in year focus on its effect on two outcomes: repetition and test scores. Pischke (2007) studies the effects of a reduction in school days through a natural experiment in primary schools in Germany, finding that less days in school led to more repetition. Regarding test scores, the evidence shows that the effects of more school days on either math or reading are positive (Marcotte, 2007; Hansen, 2011; Fitzpatrick *et al.*, 2011; Beleche, 2013).

The evidence for increasing instructional time through more days in a year and more hours per day is much scarcer. To the best of our knowledge, the only available evidence finds that there were positive effects on reading and math skills (Robin, 2005).

The studies on extending school days are few and contradictory. Looking at test scores in math and

[1] I thank Diego Delic, Juan Pablo Romero and Victor Volman for their outstanding comments. Also, especially thanks to Maria Edo for guiding me in the investigation process.

[2] National Minister of Education announcement: https://www.argentina.gob.ar/noticias/perczyk-impulsa-que-haya-una-hora-mas-de-clase-por-dia-en-las-escuelas-primarias.

[3] Law 26075. Ley de Financiamiento Educativo (Educational Financing Law): https://siteal.iiep.unesco.org/sites/default/files/sit_accion_files/siteal_argentina_0848.pdf.

[4] Law 26206. Ley de Educación Nacional (National Education Law): https://www.argentina.gob.ar/sites/default/files/ley-de-educ-nac-58ac89392ea4c.pdf.

[5] Recently, in 2022, it was increased to 190 days: https://www.argentina.gob.ar/noticias/se-fijo-un-piso-de-190-dias-de-clase-para-el-ciclo-lectivo-2022-en-todo-el-pais.

[6] Country by country compulsory days of schooling: https://stats.oecd.org/Index.aspx?DataSetCode=EAG_IT_ALL.

reading, the evidence shows both positive effects (Bellei, 2009) and null effects (Meyer and Van Klaveren, 2013). More closely related to this paper, Llach, Adrogué and Gigaglia (2009) study the effects of a reform carried out in 1957 but that by 1971 was half implemented that extended school days from a simple regime to a full-time regime on secondary graduation rates in the city of Buenos Aires, Argentina. Using an ad-hoc survey with cross-section data for those cohorts that were exposed to the treatment more than 30 years later, and with kernel propensity score matching and OLS strategy, they show that more time in school led to positive results in graduation.

In this paper, we aim to contribute over and above these previous findings by studying the effect of a reform that extended school days on the students' trajectories in Argentina. Following the Educational Financing Law of 2005 and the subsequent National Education Law in 2006, schools in Argentina started to extend their school hours. Most schools in Argentina share buildings to maximize the usage of infrastructure capacity. Typically, this implies that two schools with simple regimes (four hours) share the same building: while one operates during the morning, the other does so in the afternoon. This implies that only some schools have enough space to increase school hours. Therefore, this expansion was subject to pre-infrastructure capacity and thus provides an exogenous source of variation that may be used to estimate the causal effect on education results.

Our results show that increasing instructional time in primary schools by extending school hours per day reduces the grade retention of primary students[7] by 23.1%, holding constant other factors that could affect grade retention.

We will discuss if these findings are due to learning gains or if some other mechanisms are operating. Even though, our results could be led by learning gains, grade retention is an important output *per se*. Grade retention has negative effects on children's school trajectories, increasing dropouts (Manacorda, 2012; Lefgren, 2009).

Also, we studied the effects of intra-annual dropouts, but the identification is not completely neat, so we relegate the results to the appendix. We encourage further study of possible effects in this line of research for future research.

We make several contributions to the literature. First, our investigation pretends to contribute to the causal evidence of more instructional time and its effects, especially extending school hours per day. Second, our research contributes to the scarce literature of more hours of school in developing countries. Specifically, it provides more evidence for Argentina, complementing previous results focused on the City of Buenos Aires with findings only for secondary levels (Llach, Adrogué and Gigaglia, 2009) and relying on an identification strategy that allows to control for unobservable variables that remain constant over time, while covering the whole country. Finally, we contribute to the very scarce literature that focus on the link between extending school time and students' trajectories.

2. Literature review

To understand more about the causal evidence of instructional time and its effects on educational outcomes, we explore different papers with credible identification strategies (Angrist and Pischke, 2010). As we said before, the causal evidence for the impact of instructional time on educational outcomes is weak and scarce (Patall, Cooper and Allen, 2010).

We are interested in two strands of the literature. First, we will analyze the effects of more instructional

7 Grade retention refers to the decision made by the school at the end of the school year regarding whether a student should promote to the following grade. At that point, the student (and his family) may decide either to continue at the school in the same grade the following year or to dropout. Given that our data regarding dropout is less solid, we focus on grade retention.

time, either more days, more years, or both, on educational trajectories. Second, we want to understand the effects of grade retention on students' trajectories.

Regarding the first strand of the literature, Pischke (2007) studies the effects of a reduction in school days through a natural experiment in primary schools in Germany. Some cohorts were exposed to fewer days of schooling in 1966–7 due to the unification of starting day in all states of Germany. He finds that shorter school years increased grade repetition in primary school and had negative effects on higher secondary attendance.

Using variation in the number of instructional days due to weather cancellations in schools across the United States and controlling for school-fixed effects and year-fixed effects, Hansen (2011) discovers that adding 5 days to the school year raises test scores by 0.05 to 0.15 standard deviations. With a similar identification strategy and also for the USA, Marcotte (2007) shows that substantial snowfalls are related to lower test scores.

In the same line, Fitzpatrick *et al.* (2011) test the hypothesis that more school days affect children's performance positively in the USA. They exploit variation in the timing of test-taking in the Early Childhood Longitudinal Study-Kindergarten Class of 1998 (ECLS-K). Children in the study obtained more conservative results than previous authors, showing that an additional year of education in kindergarten increases achievements in math and reading by about 0.9 to 1.2 standard deviations.

Exploiting differences in the number of days between standardized tests in Mexico, in particular the start date of the school year, and the date when the national standardized exam, Evaluación Nacional del Logro Académico en Centros Escolares (ENLACE), is administered, Agüero and Beleche (2013) finds that having more days of school can translate into better reading and math exams.

There is only one rigorous study that tackles more days and more hours. Through a randomized controlled trial in the USA, NIEER (National Institute for Early Education research) compared "85 children assigned to an 8-hour program for 45 weeks to 254 children assigned to a 2.5- to 3-hour program for 41 weeks" finding positive results in reading and math skills (Robin, 2005).

Last but not least, there is less evidence that extended regimes improve students' trajectories. Llach, Adrogué and Gigaglia (2009) presents evidence for the Autonomous City of Buenos Aires (Argentina) that increasing school hours per day has effects on secondary graduation rates. They argue that the implementation in the schools of the city was as if random. They use cross-sectional data, collected by an ad-hoc survey 35 years later, from those who went to the school after the implementation of the 8-hours regime in 1971.

Through a field experiment in seven Dutch elementary schools, Meyer and Van Klaveren (2013) argue that increasing instructional time with an extended day program has no effect on math and reading. Contrary, using a similar strategy to us, Bellei (2009) shows that increasing instructional time per day in secondary in Chile's secondary schools improves language test scores by 0.05-0.07 standard deviations and math test scores by 0.00 to 0.12. He uses a difference-in-difference specification very similar to ours, where he exploits data at individual level from SIMCE-2001 and SIMCE-2003.

Moreover, the improvement in learning trajectories is a topic on its own. We present literature that finds negative effects of grade retention and repetition on students' trajectories. Manacorda (2012) uses a regression discontinuity identification strategy to show that grade failure induces dropout at the end of the school year in Uruguay. In line with that but for the USA, with a similar identification strategy, Jacob and Lefgren (2009) finds that eighth-grade students in elementary school substantially increase the probability that these students will drop out of high school, although there are no effects among sixth-grade students.

3. The program

Argentina is a federal country, and this has significant implications for education. To begin with, financial resources for education are provided mainly by provinces, not by the National Administration[8].
Also, in Argentina, there are 14 years of mandated education. Two years of kindergarten and twelve years of primary and secondary school. But the number of years of schooling in primary and secondary schools differs from province to province. Twelve provinces have six years of primary school and six years of secondary school, while the other twelve have seven years of primary school and five years of secondary school (see Table A.1 in the Appendix).
Even though there are a number of resolutions and laws at different state levels that try to increase time in school and implement extended regimes, there are two national laws that are essential to our investigation.
On the one hand, the Educational Financing Law enacted in 2005 established that within five years at least 30% of primary students should attend schools with extended regimes (more than 4 hours but less than 8 hours per day) or complete regimes (8 hours per day). In 2006, the law 26,206, active now, mandated that 100% of primary students must attend either extended regimes or complete regimes.
Led by the National Ministry of Education but implemented by the different provinces, the law established some relevant aspects of the implementation by the name of "National Model for the Extension of the School Day". First, school days' length was to be extended by an additional 3 hours in primary schools. Second, attendance to these extended school days was compulsory for all primary students. Third, new curricular spaces were subject to a comprehensive evaluation and were registered in the reference curricular space. Fourth, provinces and schools as instances of curricular definition, framed in current curricular agreements (Veleda, 2013).
The specific objectives of the program were: i) guarantee access to Priority Learning Nuclei (NAP); ii) strengthen school trajectories; iii) expand the cultural universe of students; iv) favor other forms of institutional organization; and v) renew teaching strategies. There was an intention not to modify the curriculum component (Veleda, 2013).
The program of lengthening school days was implemented all around the country. Even though there are a number of resolutions and laws at different state levels, our results do not seem to be influenced by a particular province. To understand if our results are led by a particular province, Table 1 shows that some provinces have more representation some years but not others in the total of the variability for each year.
Although the National Law of Education established that either extended or complete regimes were mandated for primary students, Table A.2 from the Appendix shows how the extended regime was increasing through the years by province.
The main constraint on extending school hours was given by infrastructure capacity (Veleda, 2013). This is because schools usually share buildings with other schools in order to maximize their usage, or the same school could have two turns, one in the morning and the other at the afternoon. Then, if a particular school has two turns with simple regimes, it will be able to enroll more students.
Using the fact that the pre-infrastructure capacity was the main reason why a school shifted from a simple regime to an extended one, we will exploit that variation. Also, in this paper, we assess to what extent the second objective of the program was achieved by exploring the impact of the program on grade retention. If the program reduces grade retention, as we argue, then we can say that the program led to better school trajectories.

[8] Around 75% of the budget is provided by the provinces and 25% by the National Administration.

Table 1. Proportion of primary schools that changed from a simple regime to an extended one in each year, by province. Expressed as a percentage.

Province	2012	2013	2014	2015	2016	2017	2018	2019
Buenos Aires	0.0	0.2	0.3	0.2	0.2	0.2	1.4	2.8
Catamarca	0.0	0.0	1.2	1.8	2.3	2.2	2.3	2.4
Chaco	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Chubut	0.4	0.4	0.5	0.7	0.6	0.6	0.5	0.5
Ciudad de Buenos Aires	0.0	0.2	0.9	1.0	1.3	1.3	2.4	4.7
Corrientes	0.4	2.6	3.9	3.8	3.5	3.3	3.0	2.6
Córdoba	62.1	65.6	56.0	40.0	36.9	36.9	37.9	36.9
Entre Ríos	1.8	1.1	2.6	3.2	3.5	4.6	5.4	7.3
Formosa	0.0	0.2	1.2	2.0	2.3	2.4	2.2	2.1
Jujuy	0.0	0.0	1.0	0.8	1.7	2.0	1.8	1.6
La Pampa	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
La Rioja	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.1
Mendoza	30.4	19.9	22.8	18.0	16.3	15.9	14.7	13.1
Misiones	1.8	5.5	4.6	3.8	5.1	5.3	4.8	4.4
Neuquén	0.0	0.0	0.5	0.8	1.0	1.1	1.1	1.0
Rio Negro	1.8	1.1	0.9	0.6	0.5	0.5	0.4	0.4
Salta	0.0	2.0	2.2	2.5	2.7	2.8	2.6	2.3
San Juan	0.0	0.4	0.3	0.2	0.2	0.2	0.2	0.3
San Luis	0.0	0.0	0.0	0.0	0.1	0.1	0.1	0.2
Santa Cruz	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Santa Fe	0.0	0.0	0.3	18.9	18.8	17.8	16.5	15.0
Santiago del Estero	0.0	0.0	0.0	1.0	2.1	2.0	1.9	1.7
Tierra del Fuego	1.3	0.7	0.7	0.7	0.8	0.8	0.7	0.6
Tucumán	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.2
Total	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**

Source: Annual Survey (RA). 2011-2019.

Note: This table express the number of extended regime primary schools divided by the total of schools in primary level year-by-year. We removed three groups of schools from our database: those that adopted extended regimes prior to 2011 (always treated), those that had or changed to a full-time regime, and schools that did not exist during the entire period of analysis, either because it was a new school or because it closed during those years. See section 4 for a discussion.

4. Data and Empirical Strategy

To investigate the effect of lengthening school days on grade retention, we use mainly one source of data for ten years (2011 to 2019): the Annual Survey (RA, because of the initials in Spanish "Relevamiento Anual"). The RA is a national annual census at the school level, which contains information about each school in Argentina for all levels. In 1996, the government began to implement a Federal System of Educational Information that guarantees comparative data at the school level for all the schools in the

country. Since 2011, a new web system for data loading and querying has been in place[9]. Because of this, there is only available data for the period 2011 to 2020, until now.
We have administrative data for more than twenty thousand schools each year, with more than five hundred characteristics of the school (like number of classes, number of students, etc.) and the students (like nationality, number of repeaters, etc.). We use data about the number of repeaters, number of students with more years than in theoretical trajectories (over-aged), number of students in the school, mean of class size of the school, number of grade-retained students, number of students enrolled in school with extended regimes, province of the school (see Table 2 for definitions).

Table 2. Definitions for key variables.

Grade-retained (output)	Number of students enrolled on the last day of class who have not met the learning requirements corresponding to a cycle or year of studies and cannot take the cycle or year of studies in any of the instances provided for it (exams of year-end or supplementary period).
Intra-year dropouts	Number of students who definitely stopped attending the establishment during the school year and were dropped from the course records. In addition, they leave the school they attended without enrolling (or passing) in another school.
Extended regime (treatment)	A school has an extended regime if it has at least one student who attends more hours than the simple day (4 hours per day) but less than the full day (8 hours per day).
Grade-repeaters	Refers to students who are studying for the second time or more for the degree/year of study, regardless of whether they have repeated it previously during their trajectory.
Over-aged students	Number of students who attend a year or degree of studies above their theoretical age in that course.

Source: Glossary documentation of Annual Survey (RA).

We are able to identify schools with extended regimes by assessing the number of students enrolled in schools with this program. Our output is the number of retained children for each school at primary level.
Additionally, we use two sources of data. First, to control for school days in a year, we use data on days of teacher strikes in primary schools in Argentina, an open data source provided by Jaume and Willén (2019). Second, we use Aprender-2016 and Aprender-2018, two waves of the national standardized tests of math and reading comprehension for primary schools, to explore mechanisms.
In Table 3 we show summary statistics of the different dimensions that we are using. On average, there are 5.48 grade-retained students per school in all primary schools per year. This is 3% of the students enrolled.

[9] Glossary documentation of Relevamiento Anual: https://www.argentina.gob.ar/sites/default/files/documento_metodologico.pdf

Table 3. Summary statistics.

	N	Mean	Standard deviation
Grade-retention rate	149,283	0.03	0.078
Grade-retained	149,782	5.48	11.849
Intra-year dropouts' rate	149,283	0.01	0.037
Intra-year dropouts	149,782	0.60	3.149
Grade-repeaters	149,922	6.35	12.815
Over-aged	149,922	26.22	42.520
Class size mean of the school	84,112	22.99	6.647
Teacher strikes per year	149,922	10.25	10.404
Enrollment	149,922	218.40	239.858

Source: own estimation based on Annual Survey (RA). 2011-2020.

As we discussed before, we exploit the fact that the reason a particular school changes its regime from a simple one to an extended one is the previous infrastructure capacity, which we can control for fixed effects, giving us an exogenous source of variation. We will show evidence that the treatment is orthogonal to the output, although it is not possible to test this assumption. Table 4 reports the number of schools changing from a simple regime to an extended one year-by-year.

Table 4. Incorporation of schools to the extended regime year-by-year.

	Primary schools with extended regimes and simple regimes		
Year	Simple regimes	Extended regimes (accumulated)	New extended regime schools
2011	16.658	0	0
2012	16.431	227	227
2013	16.205	453	226
2014	16.074	584	131
2015	15.773	885	301
2016	15.673	985	100
2017	15.620	1.038	53
2018	15.522	1.136	98
2019	15.361	1.297	161

Source: own estimation based on Annual Survey (RA). 2011-2019.

Note 1: Simple regime means 4-hours per day of schooling. Extended regime refers to more than 4-hours but less than 8-hours per day. Column (2) contains the number of primary schools with simple regimes per year. Column (3) contains the total number of primary schools with extended regimes per year. Column (4) shows the number of new schools with extended regimes per year.

Note 2: We removed three groups of schools from our database: those that adopted extended regimes prior to 2011 (always treated), those that had or changed to a full-time regime, and schools that did not exist during the entire period of analysis, either because it was a new school or because it closed during those years. See section 4 for a discussion.

We removed from our database three groups of schools. First, we drop schools that adopted extended regimes before 2011. Given that we have no data before 2011, we are not able to assess the effect of the change of regime on any outcome.

Second, we exclude those schools that had -or changed to- a full-time regime. Including schools that had changed to a full-time regime in the control group biases the estimates downward. This makes complete sense because if more instructional time causes less grade retention, then including 8-hour schools in the control group led our coefficient down. Nevertheless, our results are robust to this, results are

presented Table A.3 in the Appendix.
Third, we drop schools that did not exist during the whole period of analysis, either because of being a new school or because it closed during those years. We have no reason to think that there is a correlation between extended regimes and the opening or closing of schools.
Assuming that the pre-infrastructure capacity was the reason a school shifted from a simple regime to an extended one, we use a difference-in-difference empirical strategy to estimate the effect of lengthening the school day on grade retention.
This identification strategy allows us to control for fixed effects. This means we can control for all characteristics of the school that do not change from year to year, for example, the location, whether it is private or public, urban or rural, and so on. Also, we can add a set of controls that may change over time and could be related to grade retention and the treatment.
As for the difference-in-difference model, we estimate the following equation,

$$Y_{it} = \beta * T_{it} + \alpha_i + \mu_t + \delta * X_{it} + e_{it} \quad (1)$$

Where Y_{it} is the number of primary grade-retained students for the school *i* at year *t*, T_{it} is a dummy variable that takes 1 if the school is treated by lengthening the school days and 0 otherwise. α_i captures the school's fixed effects, μ_t captures year-fixed effects (2011 to 2019). Also, we include a set of controls that possible change from year to year like number of over-aged students, enrolment, repeaters at the beginning of the year, mean class size of the school, teacher strikes, included in X_{it}, and e_{it} is the typical error term. The parameter of interest is β which gives us the estimation of the causal effect of lengthening the school day on grade retention.

5. Results

Table 5 shows the results of the estimation of the four specifications of equation (1). All specifications are statistically significant at 99% of confidence, and standard errors are clustered at the result level (Bertrand, Duflo, and Mullainathan, 2004). Column (1) presents the effect of lengthening the school day on the number of students that were retained, controlling for fixed effects at a school level and time-year fixed effects. Column (3) has the same specification as column (1) but adds a set of covariates that the literature explains could affect our output and could change over time.
To see the percentual change in grade retention after introducing more hours to the school, we take the logarithm of the grade retained. Again, column (2) contains the estimation controlling for fixed effects (variables that do not vary across time for a particular school) and year-fixed effects (variables that affect everyone in a particular year). Column (4) is identical to column (2) but with extra controls that could vary across time for a particular school, like the number of repeaters from a period before, the number of students over-aged, the number of students enrolled, the mean class size of the school, and the number of teachers' strikes in the province for each year.
Columns (3) and (4) contain the more conservative results because we control for a more covariates that could be related with our output and the treatment. Column (3) indicates an average reduction of 1.307 grade-retained students per year in those schools that extended school days. The average number of grade-retained primary students per year, for the period studied, is 5.48 (see Table 3). Relative to this,

it means that the treatment led to a reduction of grade retention by 23.8 percent[10]. Column (4) indicates that the reduction was by a 23.1 percent[11].
Our most conservative findings suggest that lengthening the school day in primary schools in Argentina led to a reduction in grade retention of 23.1%. Our results are in line with the literature which states that more time in school improves students' trajectories (Pischke, 2007).

Table 5. Estimate of the impact of extended school days on grade retention.

	(1) Grade retention	(2) Ln (Grade retention)	(3) Grade retention	(4) Ln (Grade retention)
extended regime	-1.839***	-0.250***	-1.307***	-0.263***
	(0.000)	(0.000)	(0.000)	(0.000)
Year and school fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Number of observations	118,840	118,840	82,225	82,225

Source: own estimations based on Annual Survey (RA). 2011-2020.
Note: The sample covers the years 2011 to 2019. The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets.
* p < 0.10, ** p < 0.05, *** p < 0.01

5.2 Parallel trends assumption

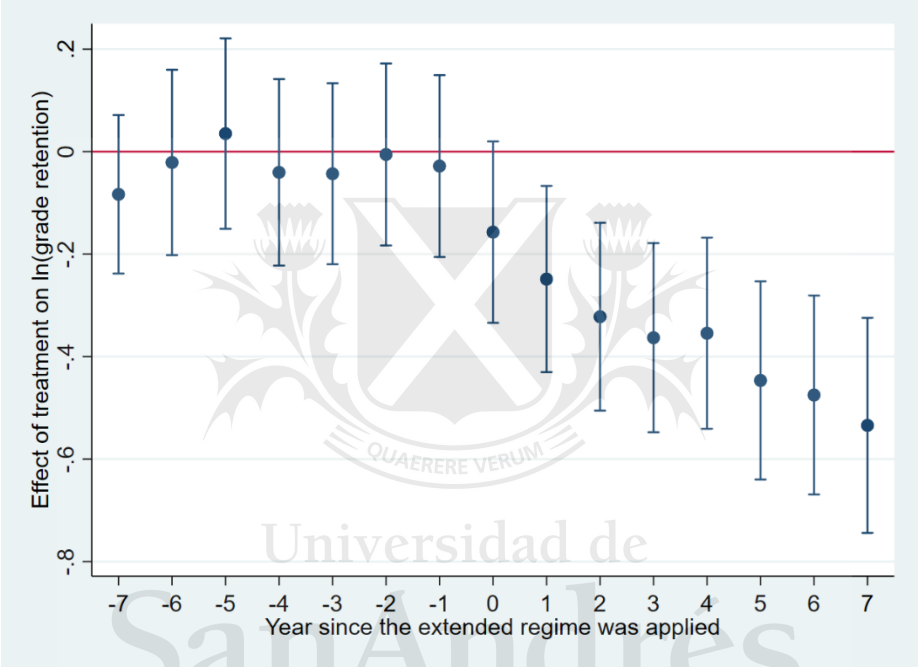
Even though we cannot test the identification assumption, we can provide evidence that supports our hypothesis by assessing the differences in trends of our outcome of interest for both treatment and control groups before and after the extension of the regime took place. Parallel trends before the treatment would give support to our identification assumption. This establishes that the trends between the treatment group (those who switched from a simple regime to an extended one) and the control group (those who remained on a simple regime) were the same prior to the treatment. If there were events or policies that happened at the same time as the implementation of the extended regime and were correlated with the treatment (extended regime) and the output (grade retention) and we were not controlling for them, then our results could be biased.
We should keep in mind that we lack real contrafactual and cannot prove the assumption of parallel trends. But what we can do is test whether the causes happen before the consequences and not vice versa and give evidence in favor of the parallel-trend assumption. To do this, we estimate the "leads and lags" specification (Angrist and Pischke, 2008; Cunningham, S., 2021),

[10] Our estimation of the beta coefficient for column (3) is -1.307 which is the marginal effect of the treatment on the treated. In order to understand the magnitude of it, we reconvert this number relative to the general mean of grade-retained students. We calculate this by doing: [(5.48-1.307)/5.48 – 1] *100 = 23.8%.
[11] Because we took logarithm to know the percentual change when the treatment was applied, we have to reconvert the number by doing the following: $(e^{-0.263} - 1) * 100 = 23.1\%$.

$$Y_{it} = \sum_{k=q-}^{q+} \beta^k T_{it}{}^k + \alpha_i + \mu_t + \epsilon_{it} \quad (2)$$

Where $T_{it}{}^k$ is a dummy variable that takes 1 if treatment took place *k* periods ago, and $q-$ is the pre period furthest back. $q+$ is the post period furthest after the lengthening school program. Therefore, β^k shows the effect of *k* periods after treatment was implemented.

As we can see in graph 1, the effect begins in the period that a school adds more hours, and it continues over time (time since shock equal to zero). This makes sense because we are taking grade-retainers for all primary schools. Cohorts that did not have extended regimes in early grades and were treated in the last years of primary school biased down the effect. But over time, there will be cohorts that have extended regimes throughout the whole primary school, accumulating human capital for more years.



Graph 1. Leads and lags estimate for grade retention.
Source: own estimation based on Annual Survey (RA). 2011-2020.
Note: Estimated impact of extended regime on logarithm of grade retention. Negative years in the x-axis refer to years before the treatment was applied. Year zero is the first year of extended regime adoption.

Our identification strategy allows us to control for all factors that remain constant over time, regardless of whether they are observable or unobservable, and we control for a set of observable characteristics that change over time. This means that our results could be biased if we do not control for variables that change over time and are correlated with the treatment and the output.

5.3 SUTVA

We are under the scope of Rubin's model of potential outcomes and causal effects. Because we want to discuss causal effects, we should determine if our subsequent assumptions are adequate. We have discussed other assumptions, like exogeneity. Now we want to challenge SUTVA's assumption. SUTVA means "Stable Unit Treatment Value Assumption" (Rubin, 1980). Imbens and Rubin (2015) defines this assumption as: "the potential outcomes for any unit do not vary with the treatments assigned to other units, and, for each unit, there are no different forms or versions of each treatment level, which lead to

different potential outcomes".
So, the STUVA assumption implies two conditions. First, there is no interference or spillovers. A treatment applied to one unit must not affect the outcome of other units. If we have spillover effects, it leads us to bias because we are counting the effect through the treatment and the effect through the spillover (Imbens and Rubin, 2015). We have no reason to think that lengthening the school days in a particular school could affect other schools.
The second assumption requires that an individual receiving a specific treatment cannot receive different forms of the treatment. This implies that extended school day programs in all provinces or, even more, in all schools are the same. We cannot guarantee it. Furthermore, there are numerous reasons to believe that extended school day programs across provinces are not the same. Not only in the implementation but in the number of hours per day. We have no data on the number of hours per day in each school. This could bias our results, and we have no information about the sign of that bias if it exists.

6. Robust test and discussion

6.1 Fixed days in a year?

Despite controlling for teachers' strikes, we do not have data on effective days of schools. In Argentina, all provinces must prepare a school calendar with 180 days of schooling, but some of them do not achieve it. Catri *et al.* (2022.a) shows that the provinces have slightly increased the number of days in recent times. If this trend was the same in the past, we cannot be assured that the number of school days over the years was the same and equally distributed by province. We have data of teachers' strikes at the provincial level but not at the school level, so we do not know if schools had more or less days of school due to other motives such as infrastructure problems, climate cancelations, or other reasons that could interrupt classes. Moreover, if the number of days of school in a year at a particular school level is correlated with our treatment, then our results could be biased.

6.2 Same population?

As we said before, we use data at the school level. This implies that we cannot be sure that the population was the same across time. Therefore, it could be argued that the population might not be the same and that when a school changes from a simple regime to an extended one, it attracts people from higher socioeconomic levels.
To face this problem, we have used data from Aprender 2016 and Aprender 2018[12], both standardized tests for primary schools in the country. In addition to the standardized test, students have to complete a supplementary questionnaire that collects extra information about each child. This provides information about the socioeconomic status of each child, like the number of books in the house, the parent's level of education, the household's conditions of life, home crowding and if he or she has access to technological devices, etc. Also, the data provides a socioeconomic index for each student that condenses some of the variables related to socioeconomic status mentioned before.
We are able to merge data from Aprender 2016, Aprender 2018 and RA. Unfortunately, we only have

12 Aprender is the national learning evaluation operation that, since 2016 (CFE Resolution 280/16), is carried out annually in Argentina for the different levels. They mainly assess mathematics and reading comprehension in a standardized way.

data for 2016 and 2018. To identify if there is a change in the socioeconomic status of the students in a school, we estimate the following equation,

$$SEL_{it} = \beta * T_{it} + \alpha_i + \mu_t + \delta * X_{it} + e_{it} \quad (3)$$

If there is an effect of lengthening the school days on the mean of socioeconomic status of the school, we could say that, regardless of whether it is the same school for all years, the population changes when the school extends hours of schooling. And the reduction in grade retention could occur because of higher socioeconomic status and not because of an extension of instructional time.
Our data does not support this hypothesis. Table 6 presents evidence that this does not seem to be happening, showing that the effect of the extended regime on socioeconomic level is zero.

Table 6. Estimate of the impact of extended school days on socioeconomic level of primary students.

	(1) Socioeconomic level of primary students
extended regime	0.00339
	(0.872)
Number of observations	28,646
Year and school fixed effects	Yes
Controls	Yes

Source: own estimations based on Annual Survey (RA) from 2016 to 2019 and Aprender 2016 and 2018.
Note: The sample covers the years 2016 and 2018. The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets.
* p < 0.10, ** p < 0.05, *** p < 0.01

6.3 Migration effect?

Another argument that could challenge our identification strategy is that, for some reason, there is a migration effect. If the results are driven by a selection process and not because of the treatment effect, our investigation could not have any sense.
Imagine a family's deciding to change schools for their son or daughter because it was too challenging or because longer hours implied too much effort. If this happens, then our reduction in grade retention could be due to a selection process where the school only keeps those with a lower chance of being retained.
Fortunately, we are able to test if this is happening. During the school year, when a student leaves the school, they can do it in two ways: by dropping the school or by changing to another school. To be accepted to another school, you must take with you a pass that allows you to change. This pass is given by the school that the student wants to leave.
If the student leaves without the pass, we can affirm that a student dropped out of school. We have data about the number of students who left the school with a pass. So, we do know how many students migrate to another school during the year.
To test if there is a migration effect when a school changes from a simple regime to an extended one,

we estimate the following equation,

$$LWP_{it} = \beta * T_{it} + \alpha_i + \mu_t + \delta * X_{it} + e_{it} \quad (4)$$

Where β give us the effect of the treatment (extended regime) in the number of students left with pass. Table 7 shows evidence that there is no migration effect. The coefficients for the four specifications are very small, and we can't reject the null hypothesis of being equal to zero. By doing this, we present evidence that there is no migration effect. So, our results seem not to be driven by a selection process.

Table 7. Estimate of the impact of extended school days on migration to another school during the year.

	(1) left with a pass	(2) Ln(left with a pass)	(3) left with a pass	(4) Ln(left with a pass)
extended regime	-0.192	-0.000332	0.201	0.0208
	(0.277)	(0.984)	(0.324)	(0.233)
Year and school fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Number of observations	118,840	118,840	82,225	82,225

Source: own estimations based on Annual Survey (RA). 2011-2020.
Note: The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets
* p < 0.10, ** p < 0.05, *** p < 0.01

6.4 Effect for each grade

It is important to check if all the effects do not come from one grade, given that in Table 5, we showed aggregate data for primary school. As a policy, it is less relevant if the effect is shown only in a few or one grade. To check if this is the case, we estimate the following equations,

$$Y_{jit} = \beta * T_{jit} + \alpha_{ji} + \mu_{jt} + \delta * X_{jit} + e_{jit} \qquad \forall j = 1,2,3,4,5,6 \text{ and } 7 \quad (5)$$

Table 8 provides evidence that in 6 of 7 grades, the effect of the treatment on grade retention is negative. Also, the coefficients of 5 of those 6 grades are significantly different from zero. In grade 7, the coefficient is positive but not statistically significant from zero.
As one can observe, the number of observations in grade 7 is much lower than the others. This happens because in Argentina, 12 provinces have a primary level of 6 grades and another 12 have 7 grades (see Table A.1 in the Appendix). That reduces our variation for that grade.

Table 8. Estimate of the impact of extended school days on grade retention by grade.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Ln (Grade retention 1)	Ln (Grade retention 2)	Ln (Grade retention 3)	Ln (Grade retention 4)	Ln (Grade retention 5)	Ln (Grade retention 6)	Ln (Grade retention 7)
extended regime	-0.0324	-0.0921***	-0.0991***	-0.111***	-0.0781***	-0.0252*	0.0217
	(0.163)	(0.000)	(0.000)	(0.000)	(0.000)	(0.092)	(0.225)
Year and school fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	44,114	63,088	68,474	69,923	70,344	71,776	28,791

Source: own estimations based on Annual Survey (RA). 2011-2020.
Note: The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets: * p < 0.10, ** p < 0.05, *** p < 0.01

7. Mechanisms

Based on a qualitative assessment that explores the experiences of teachers, directors, supervisors, and other relevant actors in the school system (Veleda, 2013) we theorize about the underlying mechanism of the reduction in grade retention.

The interviews with the different actors in the school system offer us potential subsequent mechanisms that could explain our results. We identify five potential explanations: (i) a reduction in students' absenteeism; (ii) students' motivational effects; (iii) better relationships between students and teachers; (iv) the attraction of more qualified teachers; and (v) an increase in learning achievements. Given data restrictions, we can only provide limited evidence about one potential mechanism: learning improvement[13].

Different papers find evidence that more instructional time enhances school performance, in particular better results on standardized tests (Bellei, 2009; Robin, 2005; Agüero and Beleche, 2013; Fitzpatrick et al., 2011; Hansen, 2011; Marcotte, 2007).

To show evidence that more instructional time has effects on students' performance through test scores, we should have comparative standardized tests year-by-year for the whole period. In that case, we could have reproduced our identification strategy and tested whether there were effects on standardized tests.

Unfortunately, we do not have data for the whole period of interest, but we do have data for two years. Even though we cannot replicate our analysis, if our results are driven by the improvement in school

13 The interviews from Veleda (2013) reveal that the motivation factor could be a gain from this policy. More activities like school workshops could be attractive to students, boosting motivation. Moreover, mechanisms could be correlated with each other. For example, less student absenteeism could lead to more learning or an increase in student motivation could lead to reduced absenteeism and increased learning, which reduces grade retention rates.

performance, then we should test the necessary conditions for that. In concrete, we should see that the implementation of the extended regime increases test scores, or at least they do not get worse.
We are not able to present evidence of standardized tests for all years. Having data for only two years for primary standardized tests, within our investigation period, we can present a replication preliminary result on this dimension. We do not have enough variability to draw robust conclusions in inference terms in favor or against improving students' performance.
But we do present necessary conditions for the hypothesis of improving learning outcomes. Table 9 shows the results of the effects of extended regimes on standardized tests in both math and reading comprehension. On the other hand, column 1 presents a substantial increase in math results but is not statistically significant from zero. The reading comprehension test shows zero effect statistically and in punctual estimation.

Table 9. Estimate of the impact of extended school days on standardized test.

	(1) Math	(2) Reading comprehension
extended regime	4.022	-0.00975
	(0.425)	(0.998)
Constant	521.1***	508.5***
	(0.000)	(0.000)
Number of observations	28,425	28,351
Year and school fixed effects and Controls	Yes	Yes

Source: own estimations based on Annual Survey (RA) from 2016 to 2019 and Aprender 2016 and 2018.
Note: The sample covers the years 2016 and 2018. The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets
* p < 0.10, ** p < 0.05, *** p < 0.01

8. Conclusion

Exploiting panel data from Argentina between 2011 and 2019, we show evidence that lengthening the school days substantially reduces grade retention in primary schools. The implementation of extended school regimes reduces the number of grade-retained students by 23.1%, and the result is statistically significant at 99% of confidence, using those schools with simple regimes as a control group, controlling for fixed effects and a set of controls that might change across time.
We present robust evidence for our findings. Our "lead and lags" estimation is sharp, showing that the effect happens at the moment of treatment implementation and not before. Moreover, it is consistent with the idea of a growing effect due to the accumulating hours of learning in all the school trajectory. Also, we exhibit results for all school grades where we obtain the expected effect except for the last

year, which has considerably less variability because only a few provinces have 7 years of primary school. Additionally, our exogeneity assumption is supported by qualitative evidence that only schools with existing infrastructure conditions were able to change from a simple regime to an extended one.

We have discussed causality issues. Some caveats must be taken into consideration. First, we cannot assure that school days in each school year were fixed in spite of the fact that we controlled for teachers' strikes at the provincial level. Second, we present evidence that socioeconomic students' status at school level remains constant, at least for the period 2016 to 2018, but we can't assure this for other years. Third, the STUVA assumption could be questioned. We do not know how comparable, especially in instructional time per day, schools are in different provinces.

Following the existing literature, grade retention reduction by itself, maintaining standards as we tried due to the quasi experiment, is a relevant outcome. It has positive effects on dropout reduction. However, we investigate various mechanisms that contribute to our main finding. The most plausible explanation for this decrease in grade retention is an increase in learning.

We alert and encourage further investigation about the reduction in the number of intra-years dropout. Our estimations show that extended school regimes reduce it by 15,6% in those schools that used to have intra-year dropouts (see Table A.4 from the Appendix). Unfortunately, we cannot be certain that our parallel trend assumption is adequate (see graph 2 from the Appendix). Finally, we contribute to the existing and growing literature about increasing instructional time effects, in particular more hours of school per day.

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10. Appendix

Table A.1. Educational structure in Argentina.

Provinces	K-12	
	Primary level	Secondary level
Buenos Aires, Córdoba, Entre Ríos, Corrientes, Tucumán, Formosa, Catamarca, San Juan, San Luis, Chubut, La Pampa y Tierra del Fuego	6 years	6 years
Santa Fe, Santiago del Estero, Misiones, Chaco, Ciudad de Buenos Aires, Mendoza, Salta, Jujuy, Río Negro, Neuquén, La Rioja, Santa Cruz	7 years	5 years

Source: CFE and National Ministry of Education of Argentina.
Note: All grades K-12 are required by law.

Table A.2. Number of schools with extended regimes by province, by year.

Province	2012	2013	2014	2015	2016	2017	2018	2019
Buenos Aires	0	1	2	2	2	2	16	36
Catamarca	0	0	7	16	23	23	26	31
Chaco	0	0	0	0	0	0	0	0
Chubut	1	2	3	6	6	6	6	6
Ciudad de Buenos Aires	0	1	5	9	13	14	27	61
Corrientes	1	12	23	34	34	34	34	34
Córdoba	141	297	327	354	363	383	431	478
Entre Ríos	4	5	15	28	34	48	61	95
Formosa	0	1	7	18	23	25	25	27
Jujuy	0	0	6	7	17	21	21	21
La Pampa	0	0	0	0	0	0	0	0
La Rioja	0	0	0	0	0	0	0	1
Mendoza	69	90	133	159	161	165	167	170
Misiones	4	25	27	34	50	55	55	57
Neuquén	0	0	3	7	10	11	12	13
Rio Negro	4	5	5	5	5	5	5	5
Salta	0	9	13	22	27	29	30	30
San Juan	0	2	2	2	2	2	2	4
San Luis	0	0	0	0	1	1	1	2
Santa Cruz	0	0	0	0	0	0	0	0
Santa Fe	0	0	2	167	185	185	187	194
Santiago del Estero	0	0	0	9	21	21	22	22
Tierra del Fuego	3	3	4	6	8	8	8	8
Tucumán	0	0	0	0	0	0	0	2
Total	**227**	**453**	**584**	**885**	**985**	**1,038**	**1,136**	**1,297**

Source: own estimations based on Annual Survey (RA). 2011-2019.
Note: We removed three groups of schools from our database: those that adopted extended regimes prior to 2011 (always treated), those that had or changed to a full-time regime, and schools that did not exist during the entire period of analysis, either because it was a new school or because it closed during those years. See section 4 for a discussion. After this, the total number of schools, both simple and extended regimes, is 16,658.

Table A.3. Estimate of the impact of extended school days on grade retention. Including 8-hours regime in the control group.

	(1) Grade retention	(2) Ln (Grade retention)	(3) Grade retention	(4) Ln (Grade retention)
extended regime	-1.630***	-0.221***	-1.129***	-0.228***
	(0.000)	(0.000)	(0.000)	(0.000)
Year and school fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Number of observations	128,605	128,605	88,830	88,830

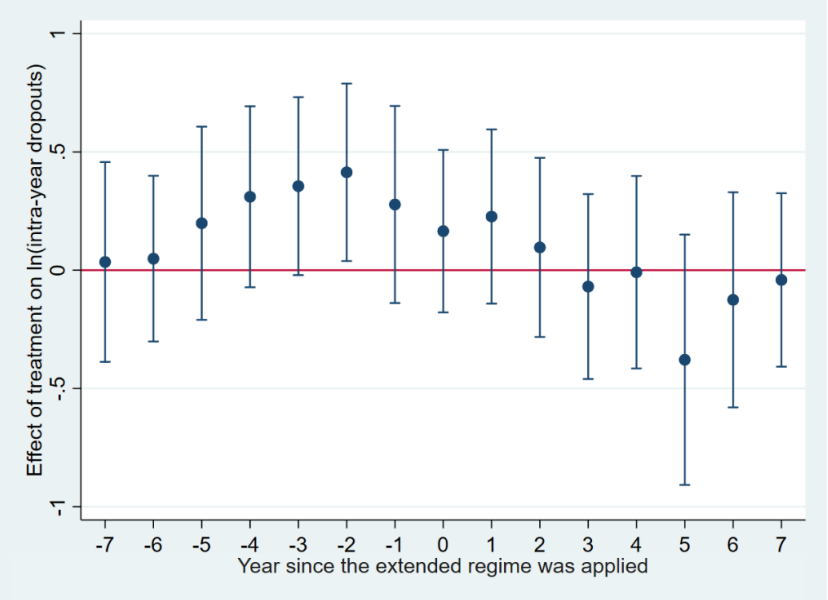
Source: own estimations based on Annual Survey (RA). 2011-2020.
Note: The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has 4 hours or 8 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets
* p < 0.10, ** p < 0.05, *** p < 0.01

Table A.4. Estimate of the impact of extended school days on intra-year dropouts.

	(1) Intra-year dropouts	(2) Ln(intra-year dropouts)	(3) Intra-year dropouts	(4) Ln(intra-year dropouts)
extended regime	-1.591**	-0.167***	-1.602**	-0.170***
	(0.029)	(0.002)	(0.038)	(0.002)
Year and school fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	19,701	19,701	14,650	14,650

Source: own estimations based on Annual Survey (RA). 2011-2020.
Note: The treatment variable is an extended day (more than 4 hours but less than 8 per day) that takes 1 if the school has more than 4 hours per day and 0 if the school has only 4 hours per day.
All standard errors are clustered at the school level.
The control variables are: the number of students who repeats the course (grade-retained the period before), the number of students with more years than in theoretical trajectories (over-aged), the number of students in the school, the mean class size of the school, and the number of teachers' strikes in the province for each year.
P-values in brackets.
* p < 0.10, ** p < 0.05, *** p < 0.01

Lead and lags estimation for intra year dropouts.



Graph 2. Leads and lags estimate for intra-year dropouts.
Source: own estimation based on Annual Survey (RA). 2011-2020.
Note: Estimated impact of extended regime on logarithm of intra-year dropouts. Negative years in the x-axis refer to years before the treatment was applied. Year zero is the first year of extended regime adoption.