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It's Time to Learn: Understanding the Differences in Returns to Instruction Time

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Abstract
As hours per day are inherently a limited resource, increasing daily instruction time reduces the amount of time pupils can dedicate to other activities outside school. We study how the effect of longer school days on achievement varies across students and schools. We exploit a large-scale reform of school schedules that substantially increased daily instruction time in Chilean primary schools. We show that the average effect of one additional year of exposure to the longer school day on reading and on mathematics test scores at the end of grade 4 masks substantial heterogeneity. Students from disadvantaged backgrounds benefit more from longer schedules, indicating that returns to time spent at school are larger the scarcer the learning opportunities available at home. Added instruction time yields higher gains in charter than in public schools, suggesting that more autonomy on administrative and pedagogical decisions may increase the effectiveness of other school inputs.

Keywords: instruction time, education reform, heterogeneous effects, charter schools
JEL: I28; I24; I20

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1 Introduction

Instruction time is among the school inputs that are more recurrently discussed in public debate, as many countries are considering or have devoted conspicuous funds to increase the amount of time that pupils spend at school. For instance, in response to a disappointing performance in PISA tests, since 2003 Germany has begun phasing in all-day schooling and the percentage of pupils attending all-day primary schools has risen from 7.9% in 2005 to 24.2% in 2013 (OECD, 2016a). Several Latin-American countries have recently transitioned from two-shift schemes, where some grades are taught in the morning and some in the afternoon, to one-shift schemes that feature a longer school day (see Section 2).[1] Assessing the effect of added instruction hours on achievement in such heterogeneous settings is a complex exercise.

First, as hours per day are inherently a limited resource, increasing instruction hours reduces the amount of time students can devote to other activities. Therefore, the returns hinge upon both the absolute quality of time use at school and its relative quality with respect to time use outside school, which varies across students and schools. On the one hand, indeed, for a given student the benefits of increasing instruction time can vary across schools, depending on how effectively they make use of the additional time. On the other hand, the benefits can vary across pupils attending the same school, depending on the learning opportunities and the broader environment that they have access to outside school, which may be very heterogeneous across households from different socio-economic backgrounds. Furthermore, the effects on the distribution of achievement can also be different based on whether the additional time is devoted to explaining the same teaching material at a slower pace or to expanding it by introducing new topics.

Second, an additional aspect to take into consideration when increasing instruction time is that, depending on the size of the change, it may require a substantial re-organization of school schedules and the overall daily routine: schools and pupils may have to adjust to very different start and end times of the school day and re-arrange their activities accordingly. This may entail initial adaptation costs.

The aim of the paper is to study how schools' and students' circumstances shape the academic returns to instruction time in a unified setting. It also provides some suggestive evidence about re-organization issues associated with large scale increases of instruction hours. It therefore

[1] As other examples, President Obama in 2009 and Chancellor Osborne in 2016 advocated for longer school days in the US and UK respectively. In the US, the National Center on Time Learning (NCTL) has continued advocating in favor of longer school days.

speaks to the renewed interest in understanding whether instruction time has heterogeneous effects and what the drivers of such heterogeneity are (Hanushek, 2015). Despite the relevance of these questions, the empirical evidence remains relatively limited, as most previous work has focused on the estimation of average effects.

In order to achieve this we take advantage of unique features of the Chilean educational system, and we exploit a large-scale country-level reform of daily schedules in primary schools. In 1997 the Chilean government implemented the Full School Day (FSD henceforth) reform, which increased daily instruction time in all publicly subsidized schools (i.e. public and charter schools) whilst leaving the term length and the national curriculum unchanged. The increment was sizable, ranging from 45 to 120 minutes per day depending on the grade and translating into a 26.7% increase of total yearly instruction time in grades 1 to 4. Schools could decide when to adopt the longer school day and how to allocate the additional time across subjects. We provide some evidence that a sizable fraction was devoted to core subjects.

We leverage within-school, cohort-to-cohort variation in years of exposure to the FSD by the end of grade 4, when pupils take a standardized test, and we assess the effect of additional instruction time on reading and mathematics scores. As the availability of longer schedules may affect the composition of pupils' intake, making adjacent cohorts not comparable, we restrict our attention to cohorts of *incumbent* students, i.e. students who start primary education in schools that had not adopted the FSD yet and may unexpectedly become exposed to it at different stages of their education. We further deal with potentially endogenous mobility across schools by either focusing on students who never transfer between grade 1 and 4 or by instrumenting actual exposure with the exposure a student would accumulate if remaining in the school where she initially enrolled.

Linear specifications show that an additional year of exposure to the FSD raises reading scores by 0.017-0.020σ. The effect on mathematics score is smaller (0.003-0.006σ) and non significant. Fully non-parametric specifications highlight that the gains from longer schedules increase more than linearly with years of exposure, suggesting that more instruction time in earlier grades also boosts achievement in later grades. Average effects mask substantial heterogeneity by students' and schools' characteristics.

Additional instruction time and longer school days benefit pupils from disadvantaged backgrounds to a greater extent. For example, the effect of an additional year of exposure on reading scores for students who live in poor households is between five to six times larger

(0.022-0.024σ) than the effect for more affluent peers (0.004σ). This is consistent with the idea that returns to spending more time at school are larger the scarcer the learning resources and opportunities available at home. We document that the frequency of homework assignments is lower when classroom instruction time increases, therefore partly entailing a replacement between self-study at home with supervised study at school. This may especially benefit students with limited support outside school.

Whilst both publicly subsidized by the student voucher system, public and charter schools differ in the degree of autonomy they enjoy: charter schools have more autonomy over staff and budgetary decisions and they have more freedom in designing the course offer and the course content. We compare the effect of longer schedules in public schools and in charter schools that do not charge fees. They serve students from similar backgrounds and have similar resources. We document that the benefit is much larger in charter schools and is not driven by observable differences in pupils' and teachers' characteristics. We suggest that a higher degree of autonomy may allow charter schools to better and faster adapt the curriculum to reap the learning opportunities that longer schedules offer. This also highlights that important complementarities between school inputs may exist: large school input expansion programs may need well-functioning school institutions to fully reap the benefits.

The large increase of instruction time requires the transition from a two-shift scheme, where some grades are taught in the morning and some in the afternoon, to a one-shift scheme, where all students attend school from the morning to mid-afternoon. This entails a substantial change in the times of the school day and in the overall daily routine, which is more radical for pupils previously attending the afternoon shift. We show that, although imprecisely estimated, the benefit from longer schedules is greater for students previously attending the morning shift. This finding highlights that adaptation costs may exist when sizable changes to school schedules are made.

The remainder of the paper is organized as follows. Section 2 reviews the related literature; Section 3 describes the Chilean education system and the FSD reform; Section 4 sets out the identification strategy; Section 5 describes the data and the sample; Section 6 discusses the main findings; Section 7 presents several robustness checks; Section 8 concludes.

2 Related literature

Interest in providing sound empirical estimates of the relationship between instruction time and achievement in quasi-experimental settings has recently rekindled.[2] A number of papers study the effect of the number of school days prior to standardized tests on performance. Marcotte (2007); Marcotte and Hemelt (2008); Hansen (2011) and Goodman (2014) rely on changes induced by unplanned school closures due to extreme weather conditions, whereas Agüero and Beleche (2013) and Aucejo and Romano (2016) exploit changes in term dates and/or test dates. These studies document positive effects. While they leverage small variations in the number of school days, we focus on substantial and permanent changes to the length and organization of the school day. Varying the length rather than the number of school days may have different consequences on students' achievement. For example, while the former entails a re-organization of daily routines, the latter does not.

Starting from Lavy (2015), recent studies use cross-country PISA data and exploit within-pupil variation in subject-specific instruction time to shed light on the effect of instruction time and, similarly to our study, on the drivers of its productivity. Lavy (2015) finds that a one-hour increase of weekly subject-specific instruction time raises scores by 0.06σ and that schools' and students' circumstances matter: the effect is larger for schools that enjoy more autonomy and for pupils from disadvantaged backgrounds. Rivkin and Schiman (2015) further highlight that productivity of instruction time depends positively on the quality of the classroom environment, as captured by student disruption and student-teacher interactions. Cattaneo et al. (2017) focus their attention on Switzerland and document that students in more demanding school-tracks enjoy greater benefits. Also in this case, the source of variation leveraged in these studies is different from ours. Different allocations of weekly instruction time across subjects do not necessarily entail a change in the length of the school day. Students do not have to re-arrange their daily routine or reduce the time for activities carried out outside schools, nor do schools need to operate for more hours. Moreover, we explore several schools' and students' determinants of instruction time heterogeneity in a unified setting.

A number of papers leverage reform-induced variation in instruction time. Pischke (2007) and Parinduri (2014) exploit the existence of exceptionally short or long school years due to

[2]The early studies focus on term length and report modestly positive to insignificant effects. These studies rely either on variation in term length between U.S states (Rizzuto and Wachtel, 1980; Card and Krueger, 1992; Betts and Johnson, 1998) and within US states over time (Grogger, 1996; Eide and Showalter, 1998) or on cross-country differences (Lee and Barro, 2001; Wößmann, 2003).

country-level reforms of school calendars that leave the curriculum unchanged.[3] Similarly to us, Lavy (2012) and Huebener et al. (2017) study reforms that increase daily instruction time in Israel and Germany, respectively. They both find a positive effect on achievement. The former documents no differential benefits for pupils from low socio-economic status, whereas the latter documents a larger gain for high performing students. Similarly to Chile, several Latin American countries have switched from a two-shift to a one-shift scheme, substantially lengthening the school day. Their effects have been evaluated in a series of recent reports, i.e. Cerdan-Infantes and Vermeersch (2007) on Uruguay, Almeida et al. (2016) on Brazil and Hincapie (2016) on Colombia. Results in achievement and for students who benefit most are mixed, suggesting that how the reform is implemented and how additional instruction time is used play important roles in shaping returns.

Two papers study the effect of the FSD reform in Chile on achievement. Bellei (2009) focuses on performance at grade 10 over the period 2000-2003, adopting a difference-in-difference approach. Berthelon et al. (2016) explore the effect on early literacy skills at grade 2. Based on one year of observations (2012), they instrument exposure to the FSD with the local availability of schools offering longer schedules. Both papers find positive and significant effects on performance in reading and mathematics. However, they do not exploit all the unique features offered by the Chilean educational system to thoroughly study which schools' and students' circumstances shape heterogeneous returns to instruction time, and why they do so, which is the aim of our paper. Furthermore, we focus on a different grade (grade 4), we also study cumulative effects and we propose a different identification strategy that attenuates concerns related to endogenous student sorting. We collect and combine previously unexploited data from multiple sources, which allows us to rule out the confounding effects of concurring infrastructure investment or changes in schools' leadership. We can also provide some evidence on how the longer school day affects the use of time at school and outside it, as well as studying heterogeneous effects from the shift (morning or afternoon) previously attended.

[3]The former studies the short 1966-67 German school year and documents an increase in repetition rates in primary school as well as a reduction in enrolment to higher secondary school tracks, without long-lasting effects on earnings and employment. The latter studies the long 1978-1979 Indonesian school year and reports a reduction in repetition rates and improved educational attainment, with positive effects also on wages and the probability of working in the formal sector.

3 Institutional setting

3.1 The Chilean school system

The Chilean school system features two education cycles: primary education (grades 1-8) and secondary education (grades 9-12). Standardized tests called SIMCE assess pupils' knowledge and skills in various core subjects at the end of grades 2, 4, 8 and 10. The testing frequency varies across grades. It is highest at grade 4, with tests taking place every year since 2005.[4] We use pupil-level scores in the reading and mathematics sections of the SIMCE test administered at the end of grade 4 as our measures of achievement.

Education is provided by three types of schools: public schools, charter schools and private schools. Public schools are free and are funded through student vouchers.[5] Charter schools are private, but they are publicly subsidized through the voucher system as well. Since 1994 charter schools can also charge tuition fees, but the size of the voucher decreases as tuition fees increase. Private schools are funded only through tuition fees and are usually substantially more expensive than charter schools.

The FSD reform applies to public and charter schools, which serve around 90% of the students attending regular programs in the school system.[6] Despite both being publicly subsidized, they are different in how they are managed and regulated. Public schools are either managed by the Municipal Department of Education (DAEM) or by private non-profit corporations.[7] The working conditions are regulated by a labor code specific for education professions.[8] Charter schools are private organizations and, accordingly, the working conditions of teachers are regulated by the private sector labor code.[9] Different regulations translate into charter schools having greater autonomy and flexibility in the management of the teaching staff, in terms of recruiting, dismissal and compensation policies. Importantly, they also enjoy more responsibility and freedom over the design of the curriculum. In Appendix A.1 we discuss in more detail the main regulatory differences between public and charter schools. We provide further evidence when exploring the differential effect of the FSD by type of school in Section 6.

[4]Fourth graders were also tested in 1999 and 2002.

[5]The voucher system was introduced in 1981, when a major reform of the educational system took place. Following this reform, the administration of public schools was also decentralized from the Ministry of Education to Municipalities.

[6]This excludes education for adults, education for students with specific disabilities and other types of special programs.

[7]While the director of the DAEM is usually a teacher appointed by the Municipality, corporations are led by a board of directors who do not need to be teachers and whose president is the major of the Municipality.

[8]This is called "Estatuto de los Profesionales de la Educación".

[9]This is called "Código del trabajo".

3.2 The FSD reform

In 1997 the Chilean government decided to increase daily instruction time in all publicly subsidized schools (i.e. public schools and charter schools) and across primary and secondary education, whilst leaving the term length and the national curriculum unchanged.[10] The aim of the policy was to improve the quality of education and reduce inequality in learning outcomes. The increment in instruction time was sizable. It ranged from 45 to 120 minutes per day depending on the grade. In grades 1 to 4, it translated into a 26.7% increase of total yearly instruction time. As a result, Chilean primary schools feature the longest school day among OECD countries, when considering total compulsory instruction time (OECD, 2016b).

Schools could choose when to implement the FSD.[11] The deadline was initially set to 2002. However, it was later extended and differentiated by type of school and students: 2007 for all public schools and for charter schools catering for disadvantaged pupils, 2010 for the rest of the charter schools. Yet, by 2013 there were still schools operating under the old scheme. Figure 1 illustrates the pattern of adoption of the FSD between 1997 and 2013 for primary schools. For every year, it shows the number of schools operating under the FSD, as well as the share of public and charter schools. They display similar patterns of adoption, although a larger share of public schools had implemented the FSD by 2013.[12]

By the time the reform was announced many schools were operating a two-shift scheme: some grades were taught in the morning and some in the afternoon. The increased instruction time and the longer school day required a change to a one-shift scheme, where all pupils attend school from the morning to mid-afternoon. Table 1 illustrates the daily school schedules with and without the FSD, inclusive of time devoted to breaks. Without the FSD pupils spend at least 4.88 hours per day at school. The typical morning shift runs from 8.00 to 12.55, while the typical afternoon shift runs from 14.00 to 18.55. Under the FSD students spend at least 7.08 hours per day at school. If the school adopts the FSD from Monday to Friday, the typical school day starts at 08.00 and ends at 15.05. If the school adopts the FSD on 4 days and the shorter school day on the remaining one, the typical longer school day starts at 8.00 and ends at 15.45.[13]

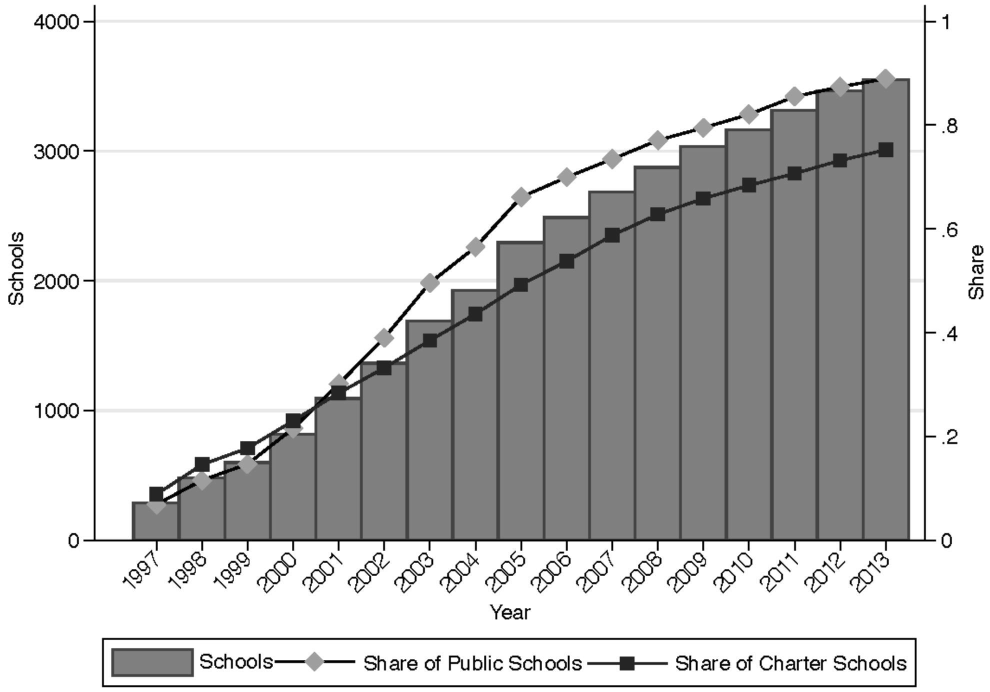
[10] Increasing daily instruction time is not mandatory in grades 1 and 2.

[11] Schools could also adopt the FSD for different grades at different times, but they were mandated to ensure that pupils who started attending the longer school day in a given grade would then also be exposed in all following grades.

[12] By 2013 around 20% of primary schools were still operating without the FSD.

[13] The minimum hours of daily instruction are prescribed by the law. Schools can freely choose the time at which the school day starts. The daily schedules in Table 1 are built assuming that the longer school day and the morning shift start at 8.00, while the afternoon shift starts at 14.00. These are the most common choices.

Figure 1: FSD adoption over the period 1997-2013



Note: The figure illustrates the pattern of adoption of the FSD in primary schools over the period 1997-2013. Despite the fact that 2010 was the last deadline to adopt the FSD, by 2013 there are still schools that have not adopted the policy.

Table 1: Daily schedules with and without FSD

	FSD	No FSD
Minimum number of hours	7.08	4.88
Day schedule	5 days under FSD: 08:00-15.05 4 days under FSD: 08.00 - 15.45	Morning shift: 08:00-12:55 Afternoon shift: 14:00-18:55

Notes: The table reports the minimum number of hours students spend at school daily and the daily schedule with and without the FSD in place, inclusive of time devoted to breaks. The minimum number of hours is prescribed in the law. Schools can freely choose the time at which the school day starts. The daily schedules are built assuming that the longer school day and the morning shift start at 8.00, while the afternoon shift starts at 14.00. These are the most common choices.

Table 2 reports yearly instruction hours per subject with and without the FSD for grades 1 to 4. It shows that most of the legislated increase in instruction time was not allocated to specific subjects, but rather allocated to the so-called "Free Choice time", which schools could decide how to use.[14] Therefore, schools had considerable freedom over the organization of the FSD, the only constraint being the approval by the Ministry of Education of a pedagogical plan that describes the use of the additional time. We do not observe how each school allocates the additional time across subjects. However, we can provide some evidence based on a survey administered in 2005 to investigate the use of time at 387 urban primary schools that had already implemented the FSD at that point, with grade 5 as a reference.[15] Drawing on this, Table 3 reports the allocation of weekly instruction time across curricular subjects, distinguishing between public and charter schools. "Core Time" excludes "Free Choice Time". It shows that schools allocated a sizable portion of their "Free Choice Time" to core subjects, with Spanish being allocated more of the additional instruction time than mathematics.[16] The instruction time devoted to other subjects was increased by a small amount. The remaining additional time was devoted to various extra-curricular activities (not reported in the table for brevity). The allocation of "Free Choice Time" across public and charter schools is similar. Charter schools devote slightly less additional time to Spanish and mathematics. Differences in subject-specific time use are significant only for foreign languages and religion, to which charter schools devote more of the additional time.

Augmenting daily instruction time and lengthening the school day generates additional operational costs, which were funded through an increase in the baseline vouchers, by 25%-50% depending on the grade (Table 2).[17] Some schools also had to expand their infrastructure in order to switch to the single-shift scheme. Infrastructure-related costs were funded through *ad-hoc* additional resources. They were allocated through public tenders organized by the Ministry of Education and its regional offices, which usually accorded priority to school catering for students from lower socio-economic backgrounds.[18]

[14]Technology and Physical Education are the only two subjects for which there is a mandated increase in instruction time.

[15]The survey was administered by the Studies Directorate of the Sociology Faculty at the Catholic University of Chile (DESUC). The report based on the survey is available at: `www.opech.cl/bibliografico/Participacion_Cultura_Escolar/Informe_final_jec.pdf`

[16]Spanish features more instruction time also under the shorter school day.

[17]The final amount that a school receives through student vouchers also depends on the its location, size, and other characteristics. We focus on the increase in the baseline, because this was the change common to all schools.

[18]Yet schools serving students from higher socio-economic backgrounds usually had lower infrastructure requirements.

Table 2: Hours of instruction per year and voucher baseline with and without the FSD

Subject/Grades	1st - 4th	
	FSD	No FSD
Mathematics	304	304
Spanish	228	228
Natural Sciences	114	114
Social Sciences	114	114
Physical Education	152	114
Arts and Music	152	152
Technology	38	19
Others	95	95
School Free Choice	247	0
Total (hours)	1444	1140
Voucher Baseline (U.S.E.)	2.77	1.99

Notes: The table reports yearly subject-specific and total instruction time with and without the FSD, for grades 1 to 4. The information comes from the Ministry of Education website (`www.mineduc.cl`). It also reports the amount of the student voucher with and without the FSD, expressed in educational subsidy units (U.S.E). These units underwent some modifications since the implementation of the FSD reform. The figures presented in the table refer to year 2013.

Table 3: Use of time under the FSD in primary schools

Subject	Public Schools		Charter Schools	
	Core Time	Free Choice Time	Core Time	Free Choice Time
Spanish	5.39*	2.49	5.59*	2.24
	(0.81)	(1.59)	(1.18)	(1.71)
Maths	5.14	1.55	5.25	1.37
	(0.78)	(1.34)	(1.13)	(1.26)
Social Sciences	3.84	0.15	3.81	0.19
	(0.74)	(0.56)	(0.91)	(0.55)
Natural Sciences	3.91	0.47	3.85	0.51
	(0.70)	(0.93)	(0.77)	(0.96)
Foreign Languages	1.90***	0.16***	2.22***	0.43***
	(0.59)	(0.57)	(0.80)	(0.93)
Technology	2.00	0.004	2.05	0.02
	(0.52)	(0.07)	(0.54)	(0.18)
Art	3.09	0.07	3.17	0.06
	(0.77)	(0.33)	(0.86)	(0.38)
Sports	2.04**	0.028	2.19**	0.06
	(0.50)	(0.21)	(0.74)	(0.34)
Religion	1.89	0.00***	1.97	0.10***
	(0.51)	(0.00)	(0.38)	(0.43)
Number of Schools	229		158	

Notes: The table presents the allocation of time across curricular subjects for a representative sample of 5th graders in urban schools that adopted the FSD before 2005 and were surveyed by the Studies Directorate of the Sociology Faculty at the Catholic University of Chile. “Core Time" excludes “Free Choice” time. The stars indicate that the number of hours allocated to a given subject is significantly different between public and charter schools (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). XX in parentheses.

4 Empirical strategy

In order to study whether increased instruction time and a longer school day affect achievement, we exploit the fact that we observe multiple cohorts of fourth graders taking a standardized test at the end of the school year. Specifically, we leverage within-school, cohort-to-cohort variation in years of exposure to the FSD by the end of grade 4. Depending on the year in which a school adopts the FSD, adjacent cohorts of pupils vary in the number of years of exposure before they are tested.

The identifying variation provides unbiased estimates of the causal effect of the FSD on learning outcomes as long as there is no cohort-to-cohort changes in unobservable characteristics that correlate both with achievement and years of exposure to increased instruction time. Given the staggered adoption of the FSD across schools, the main concern is that parents would factor the availability of the longer school day into their preferences about the school in which to enrol their children. This could affect the composition of pupil intake, possibly along dimensions that our rich set of controls (which, notably, include parental education and household income) cannot account for. According to parent surveys administered alongside the test, the FSD features among the main three drivers of school preferences for only 10% of households.[19] Proximity to home (50%), teacher quality (40%) and the level of tuition fees (35%) are the most relevant criteria.[20]

Nonetheless, we address this concern by restricting our analysis to *incumbent* pupils. This means that we only consider pupils who enroll in first grade in a given school prior to the adoption of the FSD. As an example, if a school adopts the longer school day in 2007, we discard students who start primary education in that school in 2007 or later. Cohorts who enrolled before 2007, on the other hand, made their decision before the implementation of the longer school day and possibly became exposed to it at some point in their school career. The variation in the treatment that comes only from legacy enrollment cohorts has been exploited in recent work by Abdulkadiroğlu et al. (2016) and Eyles et al. (2017), who analyze the effect of charter takeovers in the US and academy conversions in England, respectively. This restriction attenuates identification issues related to unobserved changes in pupil intake; the less parents can anticipate the exact year in which a school will increase instruction time and lengthen the

[19] The question about drivers of parental preferences is present in the parent survey administered in 2002. For reasons that we describe in Section 5, we do not use this wave of the test when evaluating the impact of the FSD on student performance.

[20] The presence of a relative attending the same school, the school's academic performance and ethical values follow in the ranking, each of them being cited by around 30% of parents.

school day, the more convincing the strategy will be.

Cohort-to-cohort compositional changes that correlate with exposure to increased instruction time may still exist because of pupil mobility across schools. The provision of longer schedules may influence the decision to transfer a student from one school to another. Transfers in the Chilean school system are not negligible: in our master sample (described in Section 5) 23% of students change schools between grades 1 and 4. The existence of transferring students also potentially generates variation in the treatment not only across cohorts in a given school, but also within them. As we can follow pupils along their school career up to grade 4, we can compute actual years of exposure at the time of the test for students who move across schools. Moreover, in our preferred regression specifications we either: *i*) restrict the sample to pupils who never transfer between grade 1 and 4; or *ii*) instrument actual exposure to the FSD with the exposure a student would accumulate had she never transferred from the school where she attended first grade.

The baseline regression specification reads:

$$Y_{ist} = \alpha + \beta FSD_{ist} + \gamma X_{ist} + \delta Z_{st} + \eta_s + \theta_t + \varepsilon_{ist} \tag{1}$$

Y_{ist} is the test score of student i attending school s when she takes the standardized test at the end of school year t. FSD_{ist} measures actual years of exposure to the FSD by year t. X_{ist} is a vector of student characteristics as of year t, which include: gender, age, parental education, monthly household income, the number of books available at home, as well as the availability of a PC and a connection to the Internet at home.[21] Z_{st} is a vector of time-varying school characteristics as of year t, which consists of: the share of female pupils, the average monthly household income, the share of students whose most educated parent received at least some higher education, the share of students with more than 10 books at home, as well as the share of students who have a PC or an Internet connection at home. Since 2008, pupils from disadvantaged backgrounds have been granted additional subsidies (SEP) on top of the vouchers. We therefore also include the share of students who benefit from SEP subsidies. Further school-level controls are: average class size, the share of female teachers, average teacher experience

[21] Monthly household income is a categorical variable featuring 13 classes. The first class is less than 100.000 CLP and the last class is more than 1.800.000 CLP. The width of the class is 100.000 CLP up to 600.000 CLP and 200.000 CLP after that. We make such a variable continuous by attributing the midpoint of the interval to each close category, and 4/3 of the lower bound to the last open category. Parental education is captured by a set of dummies that take value equal to 1 if the highest educational attainment of the most educated parent is: no education, some primary education, some secondary education, some vocational higher education, some academic higher education, respectively. The number of books at home is a categorical variable and features the following categories: 0-10, 11-50, 51-100, and more than 100.

and the share of teachers holding an education degree.[22] η_s is a set of school fixed effects that account for time-invariant heterogeneity across schools, so that we leverage only within-school, cohort-to-cohort variation; θ_t is a set of year fixed effects that control for common unobservable year-specific shocks.

In our IV specification we instrument FSD_{ist} with FSD_{is_1t}, i.e the exposure a pupil would accumulate had she never transferred from the school where she attended first grade. When we explore heterogeneity by school and student circumstances, we augment the regression specification in (1) by interacting every right-hand side variable with a dummy capturing heterogeneity along the dimension of interest (for example, a dummy D_s that takes value 1 if the school is public, or a dummy D_i that takes value 1 if the most educated parent of the pupil does not have some higher education). This is equivalent to running the regression separately for each group of schools or pupils, but has the advantage that it is straightforward to assess the statistical significance of the differential effects captured by the interaction term $FSD_{ist} \times D_s$ or $FSD_{ist} \times D_i$.[23]

A second concern is that the timing of adoption may depend on past performance. For example, if schools switch to the longer school days after they observe a cohort of pupils faring particularly poorly at the test, our estimates may also capture mean-reversion effects. In general, one could worry about the confounding effect of rising or falling underlying school-specific trends in test scores. We show in Section 5 that there are no visible trends in reading and mathematics scores in the years preceding the switch. Another concern is that other events may take place at the school around the time of FSD adoption, which may affect learning outcomes in the following years as well. We discuss and address these further issues in Section 7, where we show that we obtain very similar findings when restricting our attention to schools that do not receive public funds for expanding infrastructure when lengthening the school day and to schools that do not change the school principal around the time of FSD adoption.

5 Data and Sample

We link several administrative and survey datasets on account of unique school, student and teacher identifiers.

Data on achievement at grade 4 come from a nationwide standardized test (SIMCE test) de-

[22] We also estimate an alternative specification where we replace school-level time-varying controls with school-specific linear time trends. Estimates are very similar and are available upon request.

[23] In our IV specification, we instrument the interaction term $FSD_{ist} \times D_j$ with $FSD_{is_1t} \times D_j$.

signed by the Education Quality Agency at the end of the school year. The test was administered for the first time in 1999 and in 2002, and then with a yearly frequency from 2005 onward. We restrict our attention to the 2005-2013 waves of the test. The reason is that the cohort who reached 4th grade in 2005 is the first for which we can track their entire school career, which is necessary both to correctly identify *incumbent* students (i.e. students who enroll in first grade in a school that had not adopted the FSD yet) and to compute actual exposure to the FSD for students who move across schools between grades 1 and 4. We use pupil-level test scores in the reading and mathematics sessions of the test as our measure of achievement. Alongside the test, students and their guardians, as well as teachers, are surveyed about several dimensions of their life inside and outside school. Based on questions that are consistent across all waves of the parent survey, we recover a rich set of information about pupils' backgrounds that we use as controls in regression specification (1). Based on teacher surveys, we provide evidence on the frequency of homework assignments without and with the FSD. Since 2008, students from disadvantaged backgrounds are granted additional subsidies (SEP) on top of the vouchers. We obtain the list of beneficiaries from the Ministry of Education. The Education Census Database records information about teachers and school principals working in the school system over the period 2003-2013. We draw information about teachers' gender, education and experience, as well as about principal turnover.

The Ministry of Education maintains the register of pupils enrolled in the school system, over the period 2002-2013. Besides gender and date of birth, for every school year it records information about the school that the student attends and their end-of-year status (i.e. promotion or retention). It also provides the register of educational establishments, from which we recover the location and the administrative status of the school (i.e. public, charter or private), as well as the level of enrollment and tuition fees. A companion dataset records the year of adoption of the FSD at the school-grade level over the period 1997-2013. Based on these sources, we reconstruct the school career of every student from grade 1 to 4 and we compute the actual years of exposure to the FSD by the end of grade 4.

The Ministry of Education also maintains a dataset that records the shift (morning or afternoon) that students attend in schools operating double shifts, from 2004 onward. Based on this source, we recover information about the shift attended in the year before the school adopts the FSD.[24] Finally, we compile from primary sources the list of schools that received additional

[24]Because the dataset records information from 2004 onward, we do not observe the previous shift arrangements prevailing in schools that adopt the longer school day across all grades in 2002 or 2003. We therefore exclude these schools when we perform the heterogeneity analysis by the shift previously attended. For schools

funds to expand their infrastructure when lengthening the school day: we consulted the releases of the Official Journal (*Diario Oficial*) published by the Interior Ministry over the period 1997-2004 and searched for the outcomes of all public tenders through which *ad-hoc* resources for infrastructure were assigned.[25] Based on this, we create a dataset that records, for every school, the year in which resources were disbursed and the amount received, if any.

We impose a set of restrictions to create the master sample for our analysis. First, we discard private schools, because the FSD reform only applies to public and charter schools. Private schools cater for roughly 10% of students across the different educational stages. Second, we drop schools located in rural areas, because they are also targeted by other specific interventions that we do not fully observe and that may confound our estimates.[26] Moreover, many of these schools were already operating a full day scheme before the FSD reform.[27] Schools in rural areas account for roughly 10% of enrollment.

Our master sample consists of 430,026 fourth graders who take the standardized test between 2005 and 2013 and who started first grade in schools that had not yet adopted the FSD.[28] As our treatment is the the length of exposure (in years) to the FSD by grade 4, we discard students who repeat a grade at least once between grade 1 and 4.[29] Because the first cohort of fourth graders in the sample (i.e. the one that takes the test in 2005) started primary education in 2002, it follows that all schools in our sample had not adopted the FSD by 2002. Given that first switches to the single-shift occurred in 1997, our sample of schools consists of mid to late adopters.

Figure 2 provides the first piece of evidence on the evolution of test scores around the time of FSD adoption. It plots average raw reading and mathematics test scores - net of school and

that start adopting the policy only across a subset of grades in 2002 and 2003, we attribute to students attending the other unreformed grades in 2003 and 2003 the same shift that later same-grade cohorts attend in 2004.

[25] The last tender took place in 2004.

[26] In 1992 the government developed a program aimed at improving the quality of education in rural areas (i.e. "Programa de Mejoramiento de la Calidad y Equidad de la Educación Rural"). It consisted of a wide set of interventions, including: providing rural schools with sufficient resources; adjusting the curriculum to acknowledge the local culture and traditions; training teachers to improve their teaching practices. Moreover, since 1996 the learning of native languages has been included in the curricula of rural schools that cater for indigenous populations and since 2003 teachers in rural schools located in remote areas and serving very vulnerable pupils have received bonuses. The law that introduces the SEP subsidy for poor children prescribes additional funding if they attend schools in remote areas.

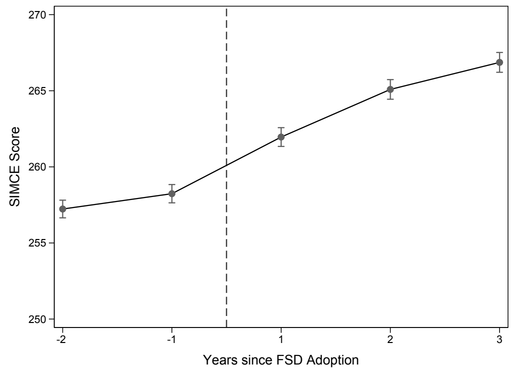
[27] See De Andraca and Gaiardo (1987) and Leyton (2013).

[28] This figure refers to the number of students for which we do not have missing covariates.

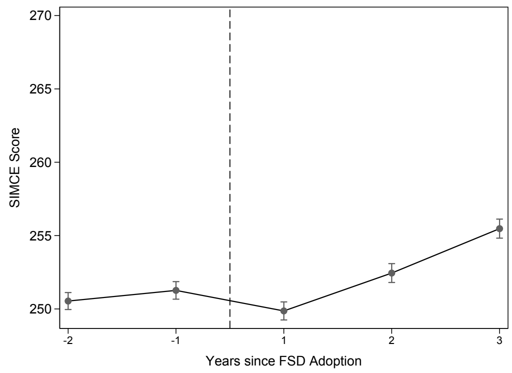
[29] In Table A4 in Appendix A.3 we study if the adoption of the FSD is associated with a change in the probability of repeating a grade. We do not find statistical evidence supporting this hypothesis. We also document a positive association with the GPA, which is consistent with the finding that a longer exposure to the FSD has a positive impact on standardized test scores. On the other hand, no positive association with attendance rates emerges.

Figure 2: The evolution of test scores with respect to the timing of FSD adoption

(a) Reading



(b) Mathematics



Notes: The sample consists in the balanced panel of schools that we observe over the period spanning from two years before the adoption of the FSD to three years after (i.e. schools that adopt the FSD between 2007 and 2010). Figures (a) and (b) plot the average reading and mathematics test scores in any given year, after controlling for school fixed effects and year fixed effects, along with the 90% confidence interval. As the tests are administered at the end of the academic year, we flag the year of adoption with 1 on the x-axis, to highlight that pupils accumulate one year of exposure to the longer school day by the end of that scholastic year. Standard errors are clustered at the school level.

year fixed effects - in the period spanning two years prior to the lengthening of the school day to three years after. To avoid compositional effects, it is based on the subset of schools that we observe every year during the period (i.e. schools that increase instruction time between 2007 and 2010). Reassuringly, the pre-adoption period does not display visible trends. This suggests that the estimates we will discuss in Section 6 should not capture underlying pre-existing trends. Post-adoption coefficients indicate a positive effect of additional instruction time, greater for reading and increasing over time. We will provide a formal estimation based on our identification strategy in Section 6.

Table 4 reports characteristics of pupils, teachers and schools, as well as test scores. Column

[1] pools all schools together, whereas columns [2] to [4] split the sample according to the type of school (public or charter), further distinguishing between charter schools that charge fees and charter schools who do not. It shows that public schools and charter schools that do not charge fees cater for students from similar backgrounds. Charter schools that charge fees serve significantly more affluent pupils, who live in households where the monthly income is 70% higher. They are almost three times as likely to have at least one parent with some academic higher education and roughly two times as likely to have more than 100 books at home. The average class size is slightly larger in fee-charging charter schools and the proportion of students attending the afternoon shift is higher in charter schools. Teachers are disproportionately females and virtually all of them hold an education degree. Public school teachers have far more experience, as they are older. Test scores are standardized by subject and year to have mean 0 and standard deviation 1. They are lowest in public schools and highest in fee-charging charter schools.

Table 4: Summary Statistics

	All	Public	Charter No Tuition Fees	Charter Tuition Fees
Students demographics				
Female	50.56	50.70	50.57	50.45
Age	9.61	9.62	9.60	9.60
Household income				
Monthly income (thousands of CLP)	334	239	251	431
Parental Education				
None	0.52	0.65	0.77	0.36
Primary	9.15	14.92	12.37	3.74
Secondary	54.60	62.12	63.35	46.59
Higher education - vocational	19.89	13.69	14.97	26.00
Higher education - academic	15.85	8.62	8.54	23.31
Books at Home				
< 10	39.43	49.12	46.87	29.92
11-50	43.72	38.85	41.34	48.22
51-100	10.67	7.87	8.02	13.52
> 100	6.18	4.17	3.78	8.34
Other Resources at Home				
Computer at home	61.48	49.18	53.14	73.31
Internet at home	37.62	25.27	28.57	49.64
Schools Characteristics				
Class size (avg.)	31.60	30.36	30.83	32.78
Proportion benefiting from SEP	18.50	25.73	30.21	10.09
Tuition fees (thousands of pesos)	14.66	0.00	0.00	29.78
Enrollment fees (thousands of pesos)	1.46	0.00	0.00	2.83
Students attending afternoon shift	46.67	42.30	49.95	49.16
Teachers Characteristics				
Female	77.03	77.65	76.30	76.69
Experience	17.75	23.05	13.90	14.24
Education degree	95.58	97.18	94.69	94.46
SIMCE test scores				
Mathematics test score (SD)	0.00	-0.19	-0.16	0.18
Reading test score (SD)	0.00	-0.18	-0.11	0.17
N. of students	430,026	172,924	45,394	211,708

Notes: The sample consists of all pupils who took the SIMCE test at grade 4 between 2005 and 2013, never repeated a grade and started primary education in a school that had not yet adopted the FSD. The information about students demographics comes from the students register. The information about household characteristics comes from the parent surveys administered alongside the SIMCE tests. The information about school fees is recorded in register of education establishment. All figures are expressed as percentages, except from those referring to the average: age of the pupil, household monthly income, class size, tuition and enrollment fees, and SIMCE test scores.

6 Results

6.1 The effect of the FSD on achievement

Table 5 reports coefficients from regression specification (1). Estimates in columns [1] and [4] are based on all students in the master sample (FE1), whereas estimates in columns [2] and [5] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4 (FE2). Estimates in columns [3] and [6] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade (IV). In the top panel we impose a linear specification of the treatment, whereas in the bottom panel we allow for a fully flexible, non-parametric specification by using a complete set of dummies.[30]

The table shows that increased instruction time and a longer school day have a positive and modest effect on achievement, which is stronger with regards to reading. The impact on reading is very stable across specifications and always significant at the 1% level: an additional year of exposure to the FSD by grade 4 raises test scores by 0.017-0.020σ. On the other hand, the impact on mathematics varies across specifications. In particular, it drops and loses statistical significance in our preferred specifications, when we restrict the sample to students who never transfer or adopt an instrumental variable approach: an additional year of exposure to the FSD improves performance by only 0.003-0.006σ and the effect is not statistically different from 0. [31]

Non-linearities emerge from our preferred specifications in the bottom panel of Table 5. Since we exploit variation in exposure to the FSD among incumbent students only, maximum exposure by grade 4 in our sample is equal to 3 years. The impact of the third year of exposure is far greater than the impact of the first and second years of exposure: while 2 years under the FSD raise reading test scores by 0.026-0.029σ, 3 years of exposure boost them by 0.094-0.104σ. This pattern also holds true for mathematics. The first and (only in column [6]) second year of exposure appear detrimental to learning, although the coefficients are small and never significant. The third year of exposure has instead a positive effect, in the range 0.043-0.051σ

[30] In the IV estimate of the fully flexible non - parametric specification we instrument the set of dummies $\{FSD^k_{ist}\}^{k=3}_{k=0}$ with the set of dummies $\{FSD^k_{is_1t}\}^{k=3}_{k=0}$. FSD^k_{ist} is a dummy that takes value 1 if the pupil has been exposed to the FSD for k years by the time of the test; $FSD^k_{is_1t}$ is a dummy that takes value 1 if the pupil had been exposed to the FSD for k years by the time of the test, had she remained in the first-grade school.

[31] The first stage coefficient of the IV specification is 0.804 and is highly statistically significant. As discussed before, the magnitude of the coefficient highlights the non-negligible share of pupils who change school between grade 1 and 4. First stage coefficients of other specifications are not reported for brevity, but are available upon request.

and statistically significant at the 10% level in column [5].

The stronger impact of the FSD on reading may depend on the fact that a larger fraction of additional instruction time is devoted to Spanish than to mathematics (Table 3), both in public and charter schools. The pattern of coefficients in the non parametric specification shows that the effect of longer schedules accumulates and compounds over time. This is consistent with added instruction time in earlier grades having a positive effect on achievement in later grades. The passage from a two-shift to a one-shift time scheme implied quite a radical re-organization of the school day. The pattern of coefficients may therefore also be explained by the presence of adaptation and learning costs that fade away over time - both for pupils and teachers - and that could have had a more negative effect on mathematics performance.

Table 5: Effect of the FSD on test scores

	Reading			Mathematics		
	FE1	FE2	FE-IV	FE1	FE2	FE-IV
			A. Linear Specification			
Years under FSD	0.019***	0.017***	0.020***	0.014***	0.006	0.003
	(0.003)	(0.006)	(0.007)	(0.004)	(0.007)	(0.008)
First stage coefficient			0.804 ***			0.804 ***
			(0.004)			(0.004)
Kleibergen-Paap statistic			32024.78			31962.72
			B. Non parametric specification			
Years under FSD = 1	0.018**	0.011	0.016	0.006	-0.017	-0.016
	(0.008)	(0.013)	(0.015)	(0.009)	(0.015)	(0.016)
Years under FSD = 2	0.043***	0.026*	0.029**	0.034***	0.002	-0.006
	(0.007)	(0.014)	(0.015)	(0.009)	(0.016)	(0.017)
Years under FSD = 3	0.031**	0.094***	0.104***	0.011	0.051*	0.043
	(0.015)	(0.025)	(0.026)	(0.016)	(0.029)	(0.028)
Kleibergen-Paap statistic			7050.17			7088.32
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
N. of students	421671	312510	421671	422837	313382	422837

Notes: Estimates in columns [1] and [4] are based on all students in the master sample, whereas estimates in columns [2] and [5] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [3] and [6] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.

Student-level controls consist of: gender; age; household monthly income; parental education; the number of books at home; a dummy that takes value 1 if the pupil has access to a PC at home; a dummy that takes value 1 if the pupil has access to the Internet at home. Footnote 21 describes how these variable are created. School-level controls consist of: the average class size; the share of female pupils; the average household monthly income; the share of students whose most educated parents received at least some higher education; the share of pupils with more than 10 books at home; the share of students with access to a computer and the share of students with access to the Internet at home; the share of students receiving the SEP subsidies. They also include: the share of female teachers; average teachers' experience; the share of teachers with and education degree. All specifications include school fixed effects and year fixed effects.

Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, **$p < 0.05$, *** $p < 0.01$.

6.2 Heterogeneous effects

As hours per day are an inherently limited resource, increasing the amount of time pupils spend at school reduces the amount of time they can devote to other activities outside school. The return to longer school schedules therefore depends on the absolute quality of time use at school and its relative quality with respect to time use outside school. Since this can vary both across students and schools, we study heterogeneous effects of the FSD by student background, school autonomy and previous organization of the school day. For brevity, we only report estimates coming from our two preferred specifications, FE2 and FE-IV.

6.2.1 Heterogeneity by student background

We explore whether returns to longer school schedules vary depending on the characteristics of the environment students are exposed to when not in school. We focus our analysis on the role of household resources, as reflected by household income and parental education. Panel A of Table 6 shows that the FSD has a far greater effect on students who live in poorer households (i.e. households with a monthly income below 400,000 CLP). This holds true both for reading and mathematics, although the coefficient of the interaction is significant at the 10% level only for the former. An additional year under the FSD boosts reading scores of less affluent pupils by 0.022-0.024σ, which is between five and six times bigger than the improvement that more affluent peers obtain (0.004σ and not significant). Although imprecisely estimated, it also seems that the small overall impact on mathematics scores masks a slightly negative effect for wealthier students and a positive effect for others. Panel B of Table 6 shows a very similar picture when studying differential effects by parental education: additional instruction time benefits more pupils whose most educated parent does not have some higher education. The differential effect is always positive and is significant at the 10% level in columns [1] and [4].

Our findings support the idea that the return to an extra-hour of instruction time and, more broadly, to longer school days does not depend only on the absolute quality of time use during that extra-hour. It also depends on its relative quality, i.e. on how students would make use of that time and on the inputs they would be exposed to were they not at school. The sign of the interaction terms in Table 6 suggests that pupils from disadvantaged backgrounds have fewer and/or worse learning opportunities and resources available at home and in the broader environment that surrounds them, thus benefiting to a larger extent from spending more time learning at school.

Children in primary school may seek the help of their parents when doing homework. Table

7 draws information from teacher surveys administered alongside the test. It shows that the longer school day is associated with a reduction in the frequency of homework assignments, both in public schools and charter schools. For example, the percentage of teachers assigning homework after every mathematics class is roughly 19% in schools where the FSD is not in place, while it drops to 13.51% in public schools and to 9.57% in charter schools that feature longer schedules. If the productivity of homework is greater for pupils who live in more affluent households, because they receive more support from their families, the partial replacement of self-study with teacher-led and supervised instruction may be one of the mechanisms that explains why the returns to the FSD are larger for pupils with fewer resources at home.

At an older age students from disadvantaged backgrounds may also be more at risk of engaging in behavior, when out of school, that is detrimental both to learning and to their overall current and future well-being. Berthelon and Kruger (2011) document that the FSD reduces the rate of teenage motherhood as well as youth crime, with the effect concentrated among poorer families. This suggests that additional instruction time and a longer school day may continue to benefit the learning of underprivileged pupils more as they grow older.

Overall, these findings highlight that the amount of time spent at school may play an important role in providing a level playing field and in reducing inequality in learning opportunities and outcomes. As pupils from different backgrounds are exposed to the same school inputs for a larger fraction of the day, the role of household inputs - whose quality varies greatly - may become less important. This is likely to be especially true if longer schedules allow the partial replacement of self-study at home with supervised study at school.

Our findings are in line with Lavy (2015). When studying the distributional effect of a reform that increased weekly instruction hours by two hours in Germany, Huebener et al. (2017) document that it widens the gap between high- and low-performing pupils. In the German setting, the increase in instruction time was accompanied by an expansion of the national curriculum, while this is not the case in the setting we study. This suggests that the use of additional instruction time - whether it is used to cover new learning material or to explain the same material at a slower pace - plays an important role in explaining these different findings.

Table 6: Heterogeneous effect of the FSD on test scores by socio-economic background

	Reading		Mathematics	
	FE2	FE-IV	FE2	FE-IV
	A. Household Income			
Years under FSD	0.004	0.004	-0.001	-0.009
	(0.009)	(0.009)	(0.010)	(0.011)
Years under FSD × Household Income (≤ 400,000 CLP)	0.018*	0.020*	0.008	0.015
	(0.010)	(0.010)	(0.011)	(0.011)
Kleibergen-Paap statistic		4505.80		4478.83
	B. Parental Education			
Years under FSD	0.008	0.010	-0.004	-0.012
	(0.008)	(0.009)	(0.009)	(0.010)
Years under FSD × Parents Education (No HE)	0.015*	0.013	0.014	0.019*
	(0.009)	(0.009)	(0.010)	(0.010)
Kleibergen-Paap statistic		6470.18		6498.55
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
N. of students	312510	421671	313382	422837

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.
Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with either a dummy taking value 1 if the monthly income of the household is below 400,000 CLP (Panel A) or a dummy taking value 1 if the most educated parent does not have some higher education (Panel B).
Standard errors are clustered at the school-income or school-parental education level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Homework Frequency

	Public Schools		Charter Schools	
	No FSD	FSD	No FSD	FSD
Mathematics homework is assigned after:				
Every class	19.51%	13.51%	19.80%	9.57%
Almost every class	50.04%	42.30%	50.82%	35.14%
Some classes	29.30%	42.40%	28.25%	51.46%
Never	1.15%	1.78%	1.13%	3.83%

Notes: Figures come from the teachers surveys administered alongside the 2011, 2012 and 2013 waves of the test.

6.2.2 Heterogeneity by school autonomy

The absolute quality of time use is likely to be the primary driver of additional instruction time's effectiveness. It is therefore important to also study the contribution of school circumstances in shaping returns to longer schedules. The Chilean school system provides an attractive setting to study the role of school autonomy: as explained in Section 3, whilst being both publicly subsidized, charter schools enjoy more autonomy than public schools over the management of school resources and the design of the curricula. We study whether the FSD has a differential effect in public and charter schools. We exclude charter schools that charge tuition fees from the analysis. Table 4 shows that fee-charging charter schools cater for more affluent pupils, whereas public schools and charter schools that do not charge tuition fees serve pupils from similarly more disadvantaged backgrounds. As we aim to uncover the role of school autonomy, we do not want to capture differences related to students' characteristics and the amount of funds available.

Table 8 shows that returns to additional instruction time are bigger in charter schools. The differential effect is large in size compared to the main effect for both subjects, but is significant at the 1% level only for reading test scores: depending on the specification, an additional year of exposure to the FSD raises reading scores by 0.055-0.066σ in charter schools and by 0.018σ only in public schools. Although imprecisely estimated, the effect on mathematics test scores is roughly one and a half times larger in charter schools as well.

Despite the fact that public schools and charter schools that do not charge fees serve students from similar backgrounds, we further check that the differential effect shown in Table 8 does not stem from differences in pupils' observable characteristics. Table 9 reports estimates from specifications that feature, beyond the interaction between years of exposure to the FSD and school type, the interactions between years of exposure to the FSD and: *i*) a dummy that takes value 1 if the pupil lives in a household with a monthly income below 400,000 CLP; *ii)* a dummy that takes value 1 if the most educated parent does not have any higher education. The inclusion of these interactions does not reduce the differential effect by school type, which remains identical in size and significance. Table A2 in the Appendix shows that the heterogeneous effect also does not reflect differences in teacher characteristics (teachers are substantially younger and less experienced in charter schools) or differences in the previous organization of the FSD (charter schools cater for slightly more pupils in the afternoon shift under the double-shift scheme).[32]

[32]We add to the set of controls underlying estimates reported in Table A2 the interaction between years of exposure to the FSD and: average teacher experience; the share of teachers who hold an education degree; the share of pupils attending the afternoon shift the year before the adoption of the FSD.

Table 8: Heterogeneous effects of the FSD on test scores by school type

	Reading		Mathematics	
	FE2	FE-IV	FE2	FE-IV
Years under FSD	0.055***	0.066***	0.027	0.037*
	(0.017)	(0.017)	(0.020)	(0.020)
Years under FSD × Public	-0.037*	-0.048**	-0.016	-0.025
	(0.019)	(0.020)	(0.022)	(0.023)
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Kleibergen-Paap statistic		642.29		652.88
N. of students	163314	212053	163809	212684

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.

Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with a dummy taking value 1 if school is public.

Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Heterogeneous effects of the FSD on test scores by school type, controlling for students' socio-economic background

	Reading		Mathematics	
	FE1	FE-IV	FE1	FE-IV
Years under FSD	0.050***	0.063***	0.020	0.032
	(0.019)	(0.020)	(0.022)	(0.023)
Years under FSD × Public	-0.037*	-0.048**	-0.016	-0.025
	(0.019)	(0.020)	(0.022)	(0.023)
	(0.007)	(0.008)	(0.008)	(0.008)
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Kleibergen-Paap statistic		318.42		323.81
N. of students	163314	212053	163809	212684

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.
Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with a dummy taking value 1 if school is public.
Furthermore, we include the interaction between years of exposure to the FSD and: *i)* a dummy that takes value 1 if the pupil lives in a household where the household monthly income is $\leq$ 400,000 CLP; *ii)* a dummy that takes value 1 if the most educated parent does not have some higher education.
Standard errors are clustered at the school level and are reported in parenthesis. We also include the interactions between years of exposure to the FSD and: *i*) a dummy that takes value 1 if the pupil lives in a household with monthly income below 400,000; *ii)* a dummy that takes value 1 if the most educated parent does not have higher education.
Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Our results show that returns to longer school days are larger in charter schools and that this is not driven by differences in observable students' and teachers' characteristics. The main difference between public and charter schools consists of the degree of autonomy they enjoy. Table 10 reports answers to school principal surveys administered alongside the 2006, 2009 and 2012 waves of PISA tests, which ask about the tasks over which head-teachers have a considerable responsibility. The sample consists of all principals of public and charter schools that offer primary education.[33] It emerges that principals in charter schools do indeed have greater autonomy in designing the curricula, as more often they can decide the course offer and the course content. Moreover, they are more likely to be responsible for the budget formulation and allocation. They are also in charge of taking personnel decisions, in terms of recruitment, promotions and dismissals. We therefore speculate that our findings are likely to be driven by school autonomy: as charter schools have more freedom in tailoring the curriculum, they may be more able and quick to adjust the school day to reap the benefits from longer schedules. Since the allocation of additional instruction time across subjects is similar in public and charter schools (Table 3), the heterogeneous effect is likely to reflect differences in subject-specific content.

Our findings are consistent with those of Lavy (2015), who also documents that additional instruction time yields larger benefits in schools that feature more autonomy and accountability. They are also in line with the growing literature showing that granting autonomy to schools improves pupils' performance.[34] Charter schools in the US typically have a longer school day than public schools. Dobbie and Fryer Jr (2013) find that a 25% (or more) increase of instruction time raises achievement in mathematics (reading) by 0.059σ (0.015σ), making it one of the most successful features of charter schools. In a different setting, our results indicate that these schools may perform better not only because students log longer school days, but also because autonomy allows them to use the additional time in an effective way.[35] Our findings

[33]PISA tests are administered to pupils aged 15. We therefore restrict our attention to secondary schools that also offer primary education, which explains the very small sample size.

[34]Several papers focus on newly founded or converted charter schools in US. Studies on high-performing oversubscribed charter schools exploit the fact that admission depends on a lottery and document a positive effect both on school performance (Abdulkadiroğlu et al., 2011; Dobbie et al., 2011; Dobbie and Fryer Jr, 2013) and medium-term non-academic outcomes (Dobbie and Fryer Jr, 2015), larger in urban schools and for disadvantaged students (Angrist et al., 2013). Abdulkadiroğlu et al. (2016) analyze school takeovers and adopt an identification strategy similar to ours. They also report positive effects on achievement. The recent work of Eyles and Machin (2015) and Eyles et al. (2017) analyze the consequences of converting English community schools into academies - autonomous, state-funded education establishments not subject to local authority control. They also uncover a positive impact on performance.

[35]Bellei (2009) and Berthelon et al. (2016) find that the effect of the FSD on achievement is larger in public schools. They both include rural schools in the analysis, which are mostly public and typically cater for very

Table 10: Differences in school autonomy between public and charter schools

	Public schools	Charter schools
Textbook use	95	100
Courses content	32	56
Courses offered	73	96
Formulate budget	16	96
Allocate budget	52	98
Hire teachers	26	99
Fire teachers	11	98
Set starting salaries	2	91
Increase salaries	2	91
Observations	62	85

Notes: The table reports the percentage of school principals who claim to have a considerable responsibility over the listed tasks. Data come from the 2006, 2009 and 2012 principal surveys administered along PISA tests. The sample consists of all school principals that manage schools also offering primary education.

also suggest the existence of complementarities between school inputs. This is important, as it implies that large, and costly, school input expansion programs may require well-functioning school institutions to fully reap the benefits.

6.2.3 Heterogeneity by previous organization of the school day

For many schools the adoption of the FSD required the transition from a two-shift scheme, where some students attended in the morning and some students in the afternoon, to a one-shift scheme, where all pupils attend school from the morning to mid-afternoon. As described in Table 1, this leads to a substantial adjustment in the times of the school day. This change is arguably more significant for students previously attending the afternoon shift, because their daily routine suffers a more radical transformation.

We further explore this topic, which is under-investigated in the literature, and check whether the benefits of the FSD are heterogeneous depending on the shift attended in the year before the switch to the single-shift scheme. Table 11 shows that the gain from longer schedules is smaller for pupils who previously attended the afternoon shift. Although the differential effect is never

vulnerable pupils. As explained in Section 5 we exclude these schools because they are targeted by other educational policies as well, which may confound our estimates, and because many of them were working under a one-shift scheme before the FSD reform. Berthelon et al. (2016) focus on the effect of the FSD at grade 2. FSD adoption is not mandatory in the first two grades of primary school and funds for this purpose are only accorded to schools catering for vulnerable students. Public schools offering longer schedules at grade 2 are therefore a selected sample. In both cases, the greater effect found in public schools may therefore reflect very large differences in the composition of students attending public and charter schools and does not contrast with our findings.

Table 11: Heterogeneous effects of the FSD on test scores by previous shift

	Reading		Mathematics	
	FE2	FE-IV	FE2	FE-IV
Years under FSD	0.027**	0.033**	0.024	0.023
	(0.013)	(0.013)	(0.016)	(0.015)
Years under FSD × Afternoon Shift	-0.007	-0.017	-0.015	-0.023
	(0.016)	(0.016)	(0.019)	(0.018)
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Kleibergen-Paap statistic		3482.38		3484.74
N. of students	269505	342220	270253	343158

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.
Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with a dummy taking value 1 if the pupil attended the afternoon shift in the year before the adoption of the FSD.
Standard errors are clustered at the school-shift level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

significant at the 10% level, it is large in size compared to the main effect.[36] In the FE-IV specification, for example, an additional year of exposure improves reading test scores of pupils attending the morning shift by 0.033σ, almost twice as much as the gain for pupils attending the afternoon shift (0.016σ). The differential effect is larger on mathematics test scores: the FSD has a null impact on the latter, while it has a positive but imprecisely estimated effect (0.023σ) on the former. This is consistent with the pattern of coefficient in Table 5, which also suggests that adaptation costs may have a greater effect on mathematics.[37]

Overall, our findings provide suggestive evidence that substantial changes to the organization and length of the school day may be associated with initial adaptation costs, which likely fade away as pupils, as well as teachers, get used to the new schedules. Short-term benefits may therefore be smaller than longer-term ones. According to a growing strand of literature, there are time-of-day effects on productivity. Pope (2016) and Lusher and Yasenov (2016) document

[36]The large standard errors also stem from a smaller sample size. As explained in Section 5, due to data constraints we could not assign a shift to pupils in schools that adopted the FSD in all grades in 2002 or 2003.

[37]To address the concern that these results may be driven by differences in the characteristics of students attending the morning and afternoon shifts we present in Appendix A.3 (Table A3) a specification in which, in addition to the interaction between FSD and the shifts, we interact the FSD with parental education and household income. The results are robust to the inclusion of these additional interactions.

that morning classes have a more positive impact on performance than afternoon classes. This could also partly explain the smaller benefits enjoyed by students previously attending afternoon shifts: not only because they arguably face a more radical change of their daily routine, but also because they may have been slightly less prepared than pupils previously attending the morning shift.

6.3 A cost-benefit analysis

To gauge the cost effectiveness of the policy, we weight average improvements on the tests against operational costs associated with the provision of an additional year of exposure to the FSD.[38] According to the cost-effectiveness index that we describe in Appendix A.2, providing an additional year of exposure to the FSD is as costly as contracting an additional teacher every 51 students.[39] Under a certain set of assumptions (detailed in Appendix A.2 as well), this would allow reducing average class size from 31 to 19. Borrowing estimates of returns to smaller class size in grade 4 from Angrist and Lavy (1999) and assuming that effects are linear, reading scores would have risen by 0.12σ and mathematics scores by 0.04σ.[40] By comparison, the effect of the FSD on achievement appears modest.[41]

Yet, our estimates highlight that it takes time to adapt to a radical change in the times of the school day and that benefits increase more than linearly with exposure. Hence, the learning gains may become larger as students grow older. Moreover, the documented heterogeneity by student characteristics shows that longer schedules may help to reduce achievement gaps by background, while the bigger impact of additional instruction time in charter schools suggests that giving more autonomy to schools may boost the benefits. Finally, this cost-benefit analysis does not take into account the effect that the longer school day may also have on non-academic outcomes and on mothers' working choices. As mentioned above, Berthelon and Kruger (2011) document a reduction of teenage pregnancy rates and youth crime as a result of this intervention. Berthelon et al. (2015) also find a positive effect on mothers' participation and attachment to

[38] Operational costs are measured by the increase in the vouchers received by schools that implement the FSD (see Table 2). We neglect one-time costs related to infrastructure funding. Benefits are captured by the coefficients in the linear specification (Panel A, Table 5).

[39] These figures assume away general equilibrium effects on teachers wages.

[40] The reference to the work of Angrist and Lavy (1999) is motivated by the fact that average class size in Chilean schools (31) is very close to the average class size in Israeli schools (29) and larger than the average class size in the US and most European countries. The numbers used here correspond to the 2sls estimates for grade 4 transformed to standard deviation units following the procedure they propose for comparing their results with other works.

[41] In the case of mathematics this comparison should be taken with caution. In Angrist and Lavy (1999) this effect is not significant; in addition the sign of the coefficient varies across alternative specifications.

the labor force.

7 Robustness checks

In this section we discuss the concern that other events may happen in a school around the time of the adoption of the FSD and affect learning outcomes in the following years. If these changes to the broader school environment also have a positive effect on performance, we would overestimate the benefit of additional instruction time.

Some schools had to expand their infrastructure prior to switching to a single-shift scheme. One therefore could worry that our estimates also capture the effect of infrastructure investment. Funds disbursed for this purpose covered costs related to replicating the existing infrastructure on a larger scale, not to improving it. Nonetheless, to address this issue, we replicate our analysis on the sample of schools that did not receive public funds for expanding infrastructure and thus are unlikely to have made substantial changes to their facilities prior to lengthening the school day. Panel A of Table 12 reports estimates that are similar to those coming from the full sample of schools. Added instructional time and the longer school day have a positive effect on reading scores (0.015-0.024σ). The impact on mathematics scores is not significant as in the full sample.

Schools had to submit a pedagogical plan for the Ministry of Education regional offices' approval prior to adopting the FSD. This might raise the concern that the decision to draft such plan is contemporaneous to other improvements in the school environment, like the appointment of a more motivated and engaged school principal. As a check, we also replicate our analysis on the sub-sample of schools that do not change school principals in a 2-year time window around the year of adoption of the FSD. Panel B of Table 12 reports the estimates from this exercise. Also in this case, coefficients are similar to those coming from the full sample. The effect on reading scores is in the range 0.014-0.020σ. The effect on mathematics scores is not significant as in the full sample and is smaller in size.

Finally, we also present estimates based on an alternative identification strategy. Chilean primary schools do not have defined catchment areas and parents can in principle enroll their pupils wherever they prefer. Yet, as discussed in Section 4, proximity to home is the main driver of parental preferences. 88% of 4th graders attend a school located in the same municipality where they live. Because of this, we also estimate an over-identified regression specification

Table 12: Effect of the FSD on test scores - Schools not receiving public funds for expanding their infrastructure and not changing principal around FSD adoption

	Reading		Mathematics	
	FE-2	FE-IV	FE-2	FE-IV
A. Schools not receiving public funds for infrastructure				
Years under FSD	0.015*	0.024**	-0.001	0.000
	(0.009)	(0.009)	(0.011)	(0.011)
Number of students	201243	274808	201749	275508
Kleibergen-Paap statistic		5770.46		5742.00
B. Schools not changing principal				
Years under FSD	0.014	0.020**	0.000	0.002
	(0.010)	(0.010)	(0.013)	(0.011)
N. of students	85648	112869	85858	113165
Kleibergen-Paap statistic		12918.60		13001.78
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.
The analysis is performed on the sub-sample of schools that did not receive public funds for expanding their infrastructure (Panel A) or over the sub-sample of schools that did not change principal between 2 years before and 2 years after adopting the FSD (Panel B).
Controls are the same as those listed in the notes to Table 5. All specifications include school fixed effects and year fixed effects.
Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Effect of the FSD on test score - Municipality-level IV

	Reading	Mathematics
Years under FSD	0.025**	0.030**
	(0.012)	(0.014)
Student-level controls	Yes	Yes
School-level controls	Yes	Yes
Year fixed effects	Yes	Yes
School fixed effects	Yes	Yes
Number of Students	1074169	1077354
Kleibergen-Paap statistic	283.66	283.32

Notes: All specifications include school fixed effects and year fixed effects. School- and student-level controls are the same as those listed in the notes to Table 5. Instruments for individual exposure to FSD consist in the average exposure to the FSD and the share of students attending schools with FSD in the municipality where the student lives. Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

that instruments student-level exposure to the FSD with the share of fourth graders attending schools that offer the FSD and the average number of years under the FSD by grade 4 in the municipality where the student lives. The instruments capture the local availability of longer schedules. They are likely to be correlated with the treatment, since the decision on whether to enroll the pupil in schools offering the FSD depends on the local offer. As long as the exclusion restriction is satisfied (i.e. as long as the intensity of FSD diffusion at the municipality level affects individual scores only through its effect on individual-level exposure to the FSD), they are valid instruments. We estimate this specification on all cohorts of pupils (i.e. not only on *incumbent pupils*) and we also use the 1999 and 2002 waves of the test. Table 13 reports coefficients that are similar, and if anything larger, than those coming from our main strategy.

8 Conclusions

As hours per day are an inherently limited resource, increasing daily instruction time reduces the amount of time pupils dedicate to other activities outside school. The returns to longer school days therefore hinge upon the absolute quality of time use at school and its relative quality with respect to time use outside school, which vary across students and schools. We study how the effect of increased instruction time and longer schedules on achievement varies depending on students' and schools' circumstances. We exploit a large-scale reform that substantially increases daily instruction time in Chilean primary schools, requiring the transition from half-day to full-day instruction (FSD). Our findings highlight that the average effect of an additional year of exposure to the FSD on reading (0.017-0.020σ) and mathematics (0.003-0.006σ) scores in a test taken at the end of grade 4 masks substantial heterogeneity depending on students' and schools' circumstances.

Returns to longer school schedules are bigger for pupils from disadvantaged backgrounds, as captured by parental education and household income. For example, the benefit associated with an additional year of exposure to the FSD is between five and six times larger for pupils living in poorer households (0.022-0.024σ) than for pupils living in more affluent ones (0.004σ). This suggests that returns to spending more time at school are larger for students who have fewer learning resources and opportunities available at home. We further document that longer school days are associated with a lower frequency of homework assignments. The partial replacement of self-study at home with supervised study at school is likely to be an important driver of our findings, as it should benefit more pupils with less support at home.

Public schools and charter schools that do not charge tuition fees serve students from similar backgrounds and have similar resources. Charter schools have more autonomy over staff and budgetary decisions, as well as over the design of the course offer and content. We show that the benefits from longer schedules are larger in charter schools and are not explained by observable differences in students' and teachers' characteristics. We therefore suggest that school autonomy plays an important role in shaping the effectiveness of other school inputs, giving rise to interesting complementarities. Autonomy is likely to enable charter schools to adapt the curriculum to reap the benefits from longer schedules quicker and better than public schools. A topic for future research is the reasons why school institutions and school inputs appear to be complements to the education production function and on whether some specific features of school management/governance matter more than others.

The transition from a two-shift scheme, where some grades are taught in the morning and some in the afternoon, to a one shift-scheme, where all students attend school from the morning to mid-afternoon, entails a substantial re-organization of times of the school day. Our analysis indicates that initial adaptation costs may exist. First, we note that benefits from longer schedules increase more than linearly with exposure. Second, we show that, although the differential effect is not precisely estimated, gains are smaller for pupils previously attending the afternoon shift, who face a more radical change of their daily routine. We are among the first to discuss and test for the existence of adaptation costs when there are significants changes to the length of the school day.

When presenting these findings we thoroughly discuss how they relate to the existing relevant literature. We also show that they are robust to a battery of robustness checks and an alternative identification strategy.

The amount of daily instruction time displays substantial heterogeneity across OECD countries (OECD, 2016b). Our work indicates that the optimal level may be different across countries, as returns to an additional hour of instruction vary depending on the characteristics of the school system and on learning resources available to students outside school. Our work also suggests that effects might take time to build up given the initial re-organization costs.

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A Appendix

A.1 Public and charter schools

Public and charter schools are subject to different regulations. This translates into charter schools enjoying more autonomy and flexibility over budgetary and personnel decisions.

Public schools are either managed by the Municipal Department of Education (DAEM) or by private non-profit education corporations. While the director of the DAEM is a teacher, corporations are led by a board of directors who do not need to be teachers and whose president is the major of the Municipality. Under both management schemes decisions related to the allocation of resources and to hiring/firing school staff are taken at the Municipality level and school principals are not necessarily involved. Charter schools are instead private organizations and all relevant decisions are taken by the school authorities.

The working conditions of the employees of public schools are regulated by the "Estatuto de los Profesionales de la Educación". The relevant regulation for charter schools is the "Código del Trabajo", the labor code that applies to all firms in Chile. Appointments of public school teachers are decided by a commission that is formed by the Major, the director of the DAEM or the education corporations, as well as one randomly selected teacher from the schools in the municipality. Priority is given to spouses of teachers already working in the municipality. The salary of public school teachers is fixed according to a national scale that takes into account experience, training, specific difficult situations (such as teaching in rural, remote or deprived areas) and responsibilities. Firing is subject to many restrictions. It is possible only if one of the following conditions are met: *i)* school enrolment decreases; *ii)* the national curriculum undergoes changes that justify the decision; *iii)* schools' merges; *iv)* protracted poor performance (see below). Teachers having tenured positions enjoy a greater job security.[42] In any case, firings have to be justified in the Annual Plan of Educational Development that needs the approval of the Provincial Office of the Ministry of Education. Charter schools are instead free to set their own recruitment and dismissal criteria. Wages and the other working conditions are subject to the same regulations that apply to private firms.

There are also differences in the evaluation of teachers. The "Estatuto de los Profesionales de la Educación" originally set some criteria for assessing teachers performance, but they were never implemented properly due to teachers' unions opposition. In 2003 a new evaluation

[42] The "Estatuto de los Profesionales de la Educación" contemplates two type of contracts,"titular" and "contratado". The first type of contract affords a greater job security, as it offers a tenured position.

system was agreed. Nevertheless it is quite lax and in practice very few teachers receive poor evaluations. In principle, teachers could be fired if they fare unsatisfactorily in two or three consecutive evaluations. School principals are not accountable based on the school performance and they can be fired only in case of a grave fault, while poor evaluations can result in assigning them to smaller schools. Charter schools can instead set their own evaluation systems and the consequences in case of poor performance.

A.2 An index of cost-effectiveness of the FSD

We construct a simple cost-effectiveness index that captures the average benefit of an additional year of exposure to the FSD by grade 4 for every 100USD invested per student. The index is built in the following way. We first compute the average difference in the voucher received by schools with and without the FSD over the period 2002-2013 and we convert it in 2015 USD. We assume the operational costs of the FSD are captured by this difference and we neglect infrastructure-related expenses. We compute the present value of the operational costs of catering for a student under the FSD until grade 4, using the social discount rate defined by the Chilean government for evaluating its investment projects (i.e. 6%). The index is then built as the ratio between the estimated benefits of full exposure to the FSD by grade 4 and the present value operational costs (divided by 100). Table A1 shows that for every \$100 invested on the FSD, students improve their performance by $0.15\%\sigma$ in mathematics and by $0.97\%\sigma$ in reading. The third and fourth rows of the same table display the present value of the operational costs of full exposure to the FSD by grade 4 and the present value of the cost of contracting an additional teacher over 4 years, respectively.[43] The comparison shows that providing full exposure to the FSD is as costly as contracting an additional teacher over the same period for around every 51 students. Under certain assumptions, implementing the FSD would then be as costly as significantly reducing average class size, from around 31 to 19 students. Specifically, the assumptions are: *i)* there is one teacher per class;[44] *ii)* contracting new teachers does not produce general equilibrium effects on teachers wage and quality; *iii)* the existing infrastructure has enough capacity to accommodate smaller classes, so that there are no additional infrastructure-related costs.

[43]In order to compute the cost of contracting an additional teacher, we refer to the average wages reported in the Teachers Longitudinal Survey implemented by the Centro de Microdatos of Universidad de Chile.

[44]This assumption is reasonable for grade 4, whereas at later grades there are different teachers specialized in different subjects.

Table A1: Costs and Benefits of the FSD

Index	Value
(1) Mathematics (% σ increase/100 USD - Student)	0.15%
(2) Reading (% σ increase/100 USD - Student)	0.97%
(3) Annual Operational Costs (USD per student)	206
(4) Annual Cost of Contracting a Teacher (USD)	10,608
(5) Ratio $\frac{(4)}{(3)}$	51.49

Notes: The figures presented in rows (1) and (2) indicate the % improvement in SIMCE test scores for every 100 USD invested per student. Row (3) displays the annual operational costs for providing longer schedules during 1 year, while row (4) displays the operational cost of contracting an additional teacher for 1 year. The ratio presented in the last row illustrates the relation between the PV cost of the FSD and the PV cost of a teacher. It says that providing the FSD for 4 years is as costly as contracting one additional teacher every 50 students.

A.3 Additional Tables

Table A2: Heterogeneous effect of the FSD on test scores by school type, students' background, previous shift and schools' characteristics

	Reading		Mathematics	
	FE1	FE-IV	FE1	LE
Years under FSD	0.015	0.082	-0.215	-0.200
	(0.130)	(0.137)	(0.153)	(0.152)
Years under FSD × Public	-0.036	-0.051**	-0.014	-0.032
	(0.023)	(0.024)	(0.026)	(0.026)
N. of students	161683	209951	162169	210576
Kleibergen-Paap statistic		380.44		391.51
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade. Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with a dummy taking value 1 if school is public.

Furthermore, we include the interactions between years of exposure to the FSD and: *i)* a dummy that takes value 1 if the pupil lives in a household with monthly income below 400,000 CLP; *ii)* a dummy that takes value 1 if the most educated parent does not have higher education; *iii)* average teachers experience; *iv)* share of teachers who hold an education degree; *v)* share of pupils attending the afternoon shift the year before the adoption of the FSD. Standard errors are clustered at the school level and are reported in parenthesis.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Heterogeneous effect of the FSD on test scores by previous shift attended and students' characteristics

	Reading		Mathematics	
	FE2	FE-IV	FE2	FE-IV
Years under FSD	0.024*	0.032**	0.025	0.025
	(0.013)	(0.014)	(0.016)	(0.016)
Years under FSD × Afternoon Shift	-0.007	-0.018	-0.015	-0.023
	(0.016)	(0.016)	(0.019)	(0.018)
Student-level controls	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes
School fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Kleibergen-Paap statistic		2295.63		2298.22
N. of students	269505	342220	270253	343158

Notes: Estimates in columns [1] and [3] come from the subset of pupils in the master sample who do not transfer between grades 1 and 4. Estimates in columns [2] and [4] are based on all students in the master sample and true exposure to the FSD is instrumented with the exposure a student would accumulate had she never transferred from the school where she attended first grade.

Controls are the same as the ones listed in the notes to Table 5. We also interact each control, as well as the treatment, with a dummy taking value 1 if the pupil attended the afternoon shift in the year before the adoption of the FSD.

Furthermore, we include interactions between years of exposure under the FSD and: a dummy that takes value 1 if the pupil lives in a household with monthly income below 400,000 CLP; *ii)* a dummy that takes value 1 if the most educated parent does not have higher education.

Standard errors are clustered at the school-shift level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: FSD adoption and repetition, school GPA and attendance (Grade 4)

	Repetition	GPA	Attendance
Years since FSD Adoption	-0.0001	0.004***	0.039
	(0.0002)	(0.001)	(0.030)
Student level controls	Yes	Yes	Yes
School level controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes
Mean of Dep.Var.	0.017	5.84	93.65
N. of students	912,069	912,069	912,069

Notes: The table presents the results for models studying changes in repetition probabilities, gpa and attendance associated with years since the adoption of the FSD. The sample consists of all pupils (including repeaters) attending grade 4 between 2005 and 2013 in schools had not adopted the FSD before 2003. The last sample restriction is imposed to make the sample as similar as possible to the master sample on which the main analysis is carried out. Controls are the same as the ones listed in the notes to Table 5. All specifications include school and year fixed effects. Standard errors are clustered at the school level and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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