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It's time to learn: School institutions and returns to instruction time

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ABSTRACT

This paper investigates whether the effects of a reform that substantially increased daily instruction time in Chilean primary schools vary depending on school institutions. Focusing on *incumbent* students and exploiting an IV strategy, we find that longer daily schedules increase reading scores at the end of fourth grade and that the benefits are greater for pupils who began primary education in no-fee charter schools rather than in public schools. We provide evidence that these two types of publicly subsidized establishments, which cater to similar students but differ in their degree of autonomy, expand the teaching input in different ways: in order to provide the additional instruction time, no-fee charter schools rely more on hiring new teachers and less on increasing teachers' working hours than public schools do.

1. Introduction

Given the important role played by schools in the formation of human capital, scholars and policymakers have long been interested in understanding what makes a school effective. The literature focuses on the role of school inputs and, more recently, of school institutions and governance (Woessmann, 2016).

This paper lies at the intersection of these two strands of research. We study how two different types of school —Chilean public and charter schools— adjust to a nationwide expansion of a specific school input —instruction time. We therefore investigate whether the way in which schools are managed and organized affects how they implement educational reforms and whether this has an impact on the returns that these reforms yield in terms of students' achievement. This question is also interesting from a policy perspective. In many countries there is a lively debate about the benefits of granting more autonomy to schools, a distinctive feature of charter schools; moreover, many countries are also considering devoting, or have already allocated, substantial funds to increasing the amount of time that pupils spend at school.[1]

Our analysis takes advantage of two features of the Chilean educational system. First, we leverage the fact that in Chile public schools and charter schools coexist. Both types of schools are publicly subsidized through a voucher system, but they differ substantially in terms of autonomy: charter schools have more autonomy over personnel decisions and over the course offer and content.

Second, we exploit the roll-out of a reform that substantially expanded a specific school input. Passed in 1997, the Full School Day (*Jornada Escolar Completa* or FSD, henceforth) reform markedly increased daily instruction time in all publicly subsidized primary and secondary schools, while leaving the term length and the national curriculum unchanged. The increment was sizable, ranging from 4 to 9 additional instruction hours per week depending on the grade. In grades 1 to 4 it translated into a 26.7 per cent increase in weekly instruction time. Schools could decide when to adopt the longer school day and how to allocate the additional time across subjects. Thus, the introduction of the FSD provides an example of a large scale expansion of an important school input that required schools to adjust in several respects. They needed to decide how to allot the additional instruction time across the various subjects and educational activities, and how to increase teaching hours to provide longer schedules.

In this paper, after estimating the effect of the FSD on achievement at the end of grade 4 on the entire sample of pupils, we divide them according to the type of school they enrolled into in grade 1: public school or no-fee charter school. We focus on these two types of schools because

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[1] For instance, since 2003 Germany has begun phasing in all-day schooling and the percentage of pupils attending all-day primary schools has increased from 7.9 per cent in 2005 to 24.2 per cent 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. 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 and Learning (NCTL) advocates extended school schedules.

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they cater to students from similar backgrounds. Fee-charging charter schools, on the other hand, serve pupils with a higher socio-economic status. This restriction, therefore, attenuates concerns that the comparison also captures differences in students' characteristics. We document that returns are heterogeneous across the two groups of students and investigate whether this can be explained by differences in the implementation of the longer school day in public and no-fee charter schools.

Our estimation strategy exploits within-school variation in years of exposure to the FSD across several cohorts of pupils who started grade 1 between 2002 and 2010, and who later took standardized reading and mathematics tests at the end of grade 4. As the availability of longer schedules may affect the composition of pupil intake, we restrict our attention to cohorts of *incumbent* students. We focus on those children who started primary education in schools that had not yet adopted the FSD in first grade or in any later grade. We further deal with potentially endogenous mobility across schools by instrumenting actual exposure with the exposure a student would have accumulated if she/he had remained in the school where she/he initially enrolled.

Our preferred linear specification shows that an additional year of exposure to the FSD raises fourth grade reading scores by 0.024σ. The effect on mathematics scores is smaller (0.007-0.008σ) and not statistically significant.

We then document that the benefits of the FSD are greater for students enrolled in no-fee charter schools than for those enrolled in public schools. The difference is large for both subjects, but it is only statistically significant for reading.

What is the reason for these differences? We provide evidence that they do not seem to stem from children in no-fee charter schools having characteristics that make them benefit more from longer schedules. While we do not observe how each school in our sample allotted the additional instruction time across subjects, the available survey evidence suggests that the extra-time is used in a similar way in both public and charter schools: differences in the use of additional instruction hours, at least in term of allocation across subjects, therefore do not seem to explain the difference either.

We then turn to the other dimension where public and no-fee charter schools can make different choices: how to increase the teaching input to provide additional instruction hours. We uncover a significant difference. In an event study setting, we show that no-fee charter schools rely more on hiring new teachers and less on increasing work hours per teacher than public schools. It therefore appears that, thanks to their higher degree of autonomy, charter schools could adjust the teaching input in a different way. Moreover, we show that public school teachers display a lower degree of satisfaction with the FSD scheme than their colleagues in charter schools. If extended teachers' working hours translate into a lower quality of time use at school, this could be one mechanism behind the documented heterogeneity.

Our paper contributes to two major strands of recent research. Firstly, it relates to the literature that examines the effect of school institutions on students' performance. This literature has shown that attending a charter school in the US or an academy in the UK typically improves academic performance. Studies on oversubscribed charter schools that exploit admission lotteries have documented a positive effect both on academic (Abdulkadiroğlu, Angrist, Dynarski, Kane, & Pathak, 2011; Dobbie, Fryer, & Fryer Jr, 2011) and non-academic outcomes (Dobbie & Fryer Jr, 2015). Abdulkadiroğlu, Angrist, Hull, and Pathak (2016) analyze charter takeovers (i.e. formerly public schools converted into charter schools) and also report positive effects on the achievement of *incumbent* students. Eyles, Machin, and McNally (2017) and Eyles and Machin (2018) examine how the conversion of some English community schools into academies —autonomous, state-funded education establishments not subject to local authority control— affects the achievement of incumbent pupils. The former studies several post-2010 episodes of conversion involving already high-performing primary schools and does not find significant effects on achievement. The latter assesses the first round of conversion of mostly under-performing secondary schools in the 2000s, and finds a positive impact on test scores.[2]

The Chilean setting is different from the US and UK. While Chile has been one of the fastest growing Latin American economies in recent decades, GDP per capita in 2018 was still around one fourth of that of the US and one third of that of the UK. It is therefore interesting to study whether school institutions also matter in countries at different stages of economic development. Our findings suggest that this is the case. We show that in the context of a large-scale expansion of a school input (i.e. instruction time) schools with greater levels of autonomy experience larger improvements; we provide evidence that this appears to be related to the ability to adjust other inputs (i.e. teaching hours) in a more effective way.

Secondly, our findings contribute to the literature exploring the nexus between instruction time and academic achievement, and in particular to the branch of this literature focusing on the length of the school day.[3] This literature provides mixed evidence.

Huebener, Kuger, and Marcus (2017) and Lavy (2020) examine reforms that, as in our setting, increase weekly instruction hours in Germany and Israel, respectively. They both find a positive effect on achievement. The former documents a larger gain for high-performing students, while the latter does not find evidence of differential benefits across pupils from different socio-economic backgrounds. Battistin and Meroni (2016) and Meroni and Abbiati (2016) study an expansion of mathematics and reading instruction time in lower secondary schools in southern Italy, documenting positive effects on mathematics test scores, concentrated among high-achieving disadvantaged pupils. Figlio, Holden, and Ozek (2018) show that extending the school day and providing additional literacy instruction time in low-performing schools in Florida have a positive effect on reading test scores. On the other hand, using randomized control trials Holmes and McConnell (1990) find no positive effects of full-day versus half-day instruction in kindergartens in California; in some domains, children attending half-day schools performed better. Similarly, Meyer and Van Klaveren (2013) report that extending instruction time for 5th, 6th and 7th grade Dutch students did not significantly improve their performance in mathematics or reading.

As in the case of Chile, several other Latin American countries have switched from a two-shift scheme —where some grades are taught in the morning and some in the afternoon— to a one-shift scheme, substantially lengthening the school day. The impact has been evaluated in a series of reports.[4] Their findings are mixed, suggesting that how the reform of school schedules is implemented and how schools adjust to offer the longer school day are important in shaping returns.

Two papers study the effect of the Chilean FSD reform on achievement. Bellei (2009) focuses on test scores at grade 10 in 2001 and 2003, adopting a difference-in-differences approach. Berthelon, Kruger, and Vienne (2016) explore the effect on early literacy skills at grade 2; based on one cross-section of students observed in 2012, they instrument exposure to the FSD with the local availability of schools offering longer schedules. Both papers find positive and significant effects on academic performance. Although their findings are related to our work, the focus of this paper is different, as our main goal is to understand how schools' characteristics —including their ability to manage the various inputs—

[2] A related literature has studied which features characterize successful charter schools. See for instance Dobbie and Fryer Jr (2013), Angrist, Pathak, and Walters (2013) and Baude, Casey, Hanushek, Phelan, and Rivkin (2020).

[3] Instruction time can be modified by extending the term length, by redistributing time across subjects, or by increasing the length of the school day. Here we discuss the literature investigating the last type of changes. Appendix A presents a detailed discussion of other related work.

[4] Cerdan-Infantes and Vermeersch (2007) on Uruguay, Almeida, Bresolin, Pugialli Da Silva Borges, Mendes, and Menezes-Filho (2016) on Brazil and Hincapie (2016) on Colombia.

affect the way in which they adjust when large scale educational reforms – the lengthening of the school day in our setting - take place. We also propose a different identification strategy to assuage concerns about students endogenously sorting into schools, and focus on a different and larger sample of students (those who start primary education between 2002 and 2010 and take a standardized test at the end of grade 4 in 2005-2013).[5]

The rest of the paper is organized as follows. Section 2 describes the Chilean education system and the FSD reform. Section 3 presents the identification strategy. Section 4 describes the data and the sample. Section 5 discusses the main findings. Section 6 concludes.

2. Institutional setting

2.1. The chilean school system: public and charter schools

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 core subjects at the end of various grades. These tests are administered at the end of the school year.

Education is provided by three types of schools: public schools, charter schools and non-subsidized private schools. Public schools are free and are funded through student vouchers.[6] Charter schools are private, but they are publicly subsidized through the voucher system as well. Since 1994 they have also been able to charge tuition fees, but the amount of the voucher decreases as tuition fees increase. Non-subsidized private schools rely on tuition fees only and are usually much more expensive. The FSD reform applies to public and charter schools, which serve more than 90 per cent of the students attending regular programs in the school system.[7]

Public and charter schools are subject to different regulations. The first important difference lies in the governance structure. In public schools the highest governing authority is either the Municipal Department of Education (*DAEM*) or a non-profit municipal corporation.[8] Decisions on the allocation of resources and the recruiting or dismissal of teachers are taken at the municipality level, and school principals are not necessarily involved. On the other hand, charter schools are private organizations and these choices are made by the school authorities.

The higher degree of autonomy of charter schools is reflected in surveys conducted among school principals during PISA tests. Appendix Table Appendix F.1 shows that respondents in charter schools report much more often that the principal, the teachers or the governing body of the school have considerable responsibility over the design of the curricula, as they can decide the course offer and the course content more frequently. They are also more likely to be responsible for: the budget formulation and allocation; personnel decisions, in terms of recruitment, promotions and dismissals.[9]

In regard to personnel decisions, another relevant difference is that teachers' working conditions in public schools are regulated by a labor code specific to education professions (*Estatuto de los Profesionales de la Educación)*, while in charter schools they are regulated by the private sector labor code (*Código del trabajo*). In public schools teachers are appointed by a commission formed by the Mayor, the director of the DAEM or of the education corporation, and one randomly selected teacher working in the municipality. Dismissals are subject to many restrictions, are only possible under specific circumstances,[10] and must be approved by the Provincial Office of the Ministry of Education. The salary of public school teachers is based on a national scale that takes into account experience, training and specific difficult situations or responsibilities (e.g. teaching in rural, remote or deprived areas). A system of performance assessment is in place, but very few teachers receive poor evaluations. Charter schools are, on the other hand, free to set their own recruitment, incentives and dismissal criteria. Wages and other working conditions are subject to the same regulations that apply to private firms.

To further explore how working conditions differ between public and charter schools, we rely on information from the Teacher Longitudinal Survey (*Encuesta Longitudinal Docente*).[11] Teachers in public schools earn a higher wage (Appendix Table F.2, column 1). The difference decreases substantially, but remains positive and statistically significant, when controlling for teachers' observable characteristics (column 2): this reflects the fact that in public schools the wage is a function of experience and that teachers are older and, therefore, more experienced (columns 4-5). Charter school teachers are more likely to receive additional benefits on top of their salary (column 6), but at the same time, they are also more likely to be fired (column 7).[12]

2.2. The full school day reform

In 1997 the Chilean government decided to increase daily instruction time in all publicly subsidized primary and secondary schools (i.e. public schools and charter schools), while leaving the term length and the national curriculum unchanged.[13] The Full School Day (*Jornada Escolar Completa*, or FSD henceforth) reform aimed to improve the quality of education and reducing inequality in learning outcomes.[14] The reform envisaged a substantial increase in instruction hours (an instruction hour lasts 45 minutes). Specifically, in primary schools 8 instruction hours per week were added in grades 1 to 6 and 5 hours per week in grades 7 and 8; in secondary schools, grades 9 and 10 experienced a 9-hour increase in instruction time per week.[15] In grades 1 to 4, this translated into a 26.7 per cent increase in weekly instruction time. As a result, in 2015 the length of school days in Chilean primary schools was

[5] Other papers have investigated the effect of the FSD on different outcomes. Berthelon and Kruger (2011) show that the FSD reduces the incidence of teenage pregnancy among girls and of youth crime, with the effects concentrated among poorer families; Contreras and Sepúlveda (2016) report a positive effect of the FSD on labor force participation and employment of single mothers whose youngest child is eligible to attend longer schedules. Finally, in a recent paper Dominguez and Ruffini (2018) study the effect of the FSD on longer-term outcomes, focusing on educational attainment and earnings when individuals are in their 20s and 30s.

[6] During the 1980s the Chilean school system experienced a major transformation that transferred the administration of public schools from the Ministry of Education to the municipalities. Furthermore, the funding system was changed and a voucher system was introduced.

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

[8] 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 mayor of the municipality.

[9] PISA tests are taken by pupils when they are 15 years old. As we focus on primary schools, we restrict our attention to secondary schools that also offer primary education; this explains the small sample size.

[10] They are: i) a decline in school enrollment; ii) a change in the national curricula; iii) schools' merges; iv) protracted poor performance.

[11] This survey was implemented by the Microdata Center of the University of Chile among a representative sample of teachers over the period 2005-2009.

[12] The probability of being dismissed in any given year is 2.3 percentage point higher for teachers working in charter schools, amounting to a 74 per cent increase over the baseline probability.

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

[14] The Law 19494 that introduced the FSD was enacted on January 25, 1997.

[15] In grades 11 and 12, 6 instruction hours per week were added in the scientific-humanities track, while 4 hours per week were added in the vocational track.

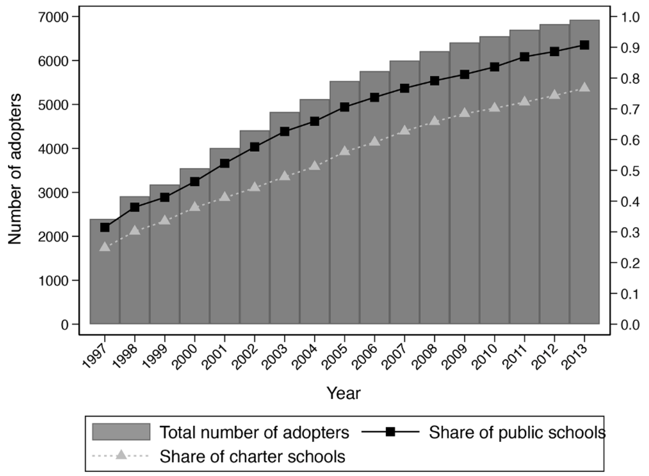


Fig. 1. FSD Adoption over the Period 1997-2013 Notes: The figure illustrates the pattern of adoption of the FSD in publicly subsidized primary schools over the period 1997-2013. On the left axis it plots the number of schools that had adopted the policy by a given year; on the right axis it displays the share of public and charter schools that had implemented the FSD by a given year.

the highest among OECD countries, when considering total compulsory instruction time (OECD, 2016b).

Schools could choose when to implement the FSD.[16] The deadline was initially set in 2002. However, it was later extended and differentiated by type of school and student: 2007 for all public schools and for charter schools catering to disadvantaged pupils, 2010 for the remaining charter schools. Yet, by 2013 —the last year of data available to us— there were still schools operating under the old scheme. Fig. 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, as well as the share of public and charter schools, that had adopted the policy by that year. The two types of schools display similar patterns of adoption, although a larger share of public schools had implemented the FSD by 2013.[17]

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 5 hours per day at school. The typical morning shift runs from 8.00 to 13.00, while the typical afternoon shift runs from 14.00 to 19.00. 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.[18]

[16] Schools could also adopt the FSD in 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 to it in all subsequent grades.

[17] By 2013 around 12 per cent of primary schools were still operating without the FSD.

[18] 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 full school day and the morning shift start at 8.00, while the afternoon shift starts at 14.00.

Table 1
Daily Schedules with and without the FSD.

	FSD	No FSD
	(1)	(2)
Minimum number of hours per day	7.08	5.00
Example of daily schedule	5 days under FSD: 08:00-15.05 4 days under FSD: 08.00 - 15.45	Morning shift: 08:00-13:00 Afternoon shift: 14:00-19:00

Notes: The table reports the minimum number of hours students spend at school every day 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 full school day and the morning shift start at 8.00, while the afternoon shift starts at 14.00.

Table 2
Hours of Instruction per Week and Student Voucher with and without the FSD.

Subject/Grades	1st - 4th	
	FSD (1)	No FSD (2)
Mathematics	6	6
Spanish	6	6
Natural and Social Sciences	6	6
Physical Education	3	3
Arts and Music	4	4
Technology	3	3
Others	2	2
School Free Choice	8	0
Total	38	30
Student Voucher (U.S.E.)	1.99	1.45

Notes: The table reports weekly subject-specific and total instruction time with and without the FSD, for grades 1 to 4. The information comes from the Decree 625 of the Ministry of Education enacted in 2003 (http://bcn.cl/253tx). It also reports the amount of the student voucher with and without the FSD, expressed in educational subsidy units (U.S.E). This information comes from the version of the DFL2/1996 of the Ministry of Education enacted in May, 2003 (http://bcn.cl/1uy40). These units have undergone some modifications since the implementation of the FSD reform.

The passage from a two-shift to a single-shift scheme implies that pupils have lunch at school. During the period under study children's malnutrition rates were very low[19] and students from disadvantaged backgrounds could already have lunch at school under the short school day scheme. Nonetheless, eating a meal at school may change the composition of the nutritional intake, as well as social and interaction patterns among pupils. While we cannot asses the relevance of this change, we argue that beneficial effects are likely to be stronger for poorer children; as those students are more prevalent in public than in no-fee charter establishments, having lunch at school can not be the driver of the larger gains from the FSD that we document for pupils in no-fee charter schools.

The main difference between the short and the long school day is the increase in instruction time, which requires to adjust the teaching input.[20] Table 2 reports weekly instruction hours per subject with and without the FSD for grades 1 to 4. It shows that the legislated increase in instruction time was not allotted to specific subjects, but rather allocated to the so-called "Free Choice Time". Schools could therefore decide how to allocate the "Free Choice Time" across the various subjects and educational activities. While having considerable freedom over the

[19] In 2000 only 2.9 per cent of children aged 0-6 suffered from malnutrition and only 0.3 per cent suffered from moderate or serious malnutrition (Mönckeberg B., 2003).

[20] As prescribed by the law, recess time was also increased.

Table 3
Use of Time under the FSD in Primary Schools (Hours per Week).

Subject	All Schools		Public Schools		Charter Schools	
	Core Time (1)	Free Choice Time (2)	Core Time (3)	Free Choice Time (4)	Core Time (5)	Free Choice Time (6)
Spanish	5.47	2.39	5.39*	2.49	5.59*	2.24
	(0.98)	(1.64)	(0.81)	(1.59)	(1.18)	(1.71)
Mathematics	5.19	1.48	5.14	1.55	5.25	1.37
	(0.94)	(1.31)	(0.78)	(1.34)	(1.13)	(1.26)
Social Sciences	3.83	0.17	3.84	0.15	3.81	0.19
	(0.81)	(0.56)	(0.74)	(0.56)	(0.91)	(0.55)
Natural Sciences	3.89	0.49	3.91	0.47	3.85	0.51
	(0.73)	(0.94)	(0.70)	(0.93)	(0.77)	(0.96)
Foreign Languages	2.03	0.27	1.90***	0.16***	2.22***	0.43***
	(0.70)	(0.75)	(0.59)	(0.57)	(0.80)	(0.93)
Technology	2.03	0.01	2.00	0.004	2.05	0.02
	(0.53)	(0.12)	(0.52)	(0.07)	(0.54)	(0.18)
Art	3.12	0.06	3.09	0.07	3.17	0.06
	(0.81)	(0.35)	(0.77)	(0.33)	(0.86)	(0.38)
Sports	2.10	0.04	2.04**	0.028	2.19**	0.06
	(0.61)	(0.27)	(0.50)	(0.21)	(0.74)	(0.34)
Religion	1.92	0.04	1.89	0.00***	1.97	0.10***
	(0.47)	(0.28)	(0.51)	(0.00)	(0.38)	(0.43)
Number of Schools	387		229		158	

Notes: The table reports hours per week allocated to different subjects in 5th grade for a representative sample of urban schools that adopted the FSD by 2005 and were surveyed by the Studies Directorate of the Sociology Faculty at the Catholic University of Chile (*DESUC*). "Core Time" excludes "Free Choice Time". *, **, *** indicate that the number of hours allocated to a given subject is significantly different between public and charter schools at the 10, 5 and 1 per cent level, respectively. Standard deviations are in parentheses.

organization of the FSD, they had to submit a pedagogical plan to the Ministry of Education detailing the use of the additional instruction time.

We do not observe how each school allocates the additional time across subjects. However, we can provide some evidence based on a survey conducted in 2005 to investigate the use of time in 5th grade at 387 urban primary schools that had already implemented the FSD at that point.[21] Drawing on this, Table 3 reports the allocation of weekly instruction time across curricular subjects, both for all schools (columns 1-2) and distinguishing between public (columns 3-4) and charter (columns 5-6) schools. "Core Time" excludes "Free Choice Time". The table confirms that schools allocated their "Free Choice Time" to increase the teaching of various subjects. A substantial portion of it was devoted to core subjects: among those, more hours are allocated to Spanish than to mathematics.[22] A small fraction of additional instruction time is dedicated to other subjects; the remaining portion of "Free Choice time" is distributed among other various educational activities (not reported in the table for brevity). Charter schools devote slightly less additional time to Spanish and mathematics. However, the allocation of additional instruction time across subjects is similar in public and charter schools. The only significant differences emerge with regards to foreign languages and religion, to which charter schools devote more of the additional instruction time.

To further investigate the effect of the FSD on schools' time use, we rely on data that reports, at the school-class-year level, the list of subjects taught.[23] Appendix Fig. F.1 shows, in an event study framework that collapses information at the school-grade-year level, the evolution of the total number of subjects, as well as subjects related to specific disciplinary areas, around the adoption of the FSD.[24]Following the introduction of a longer school day in a given grade, the number of subjects taught in that grade increases by a small but statistically significant amount, up to almost 0.1 four years after the implementation of the policy. The increase is driven by the fact that there are more subjects related to foreign languages as well as more tutorials and workshops. While the number of subjects is an imperfect measure of how schools use the additional time, as they could simply increase the hours devoted to each subject, this provides further evidence that the longer school day translated into an increase in instruction time.

Augmenting instruction time and lengthening the school day generated additional operational costs, which were funded through an increase in the baseline vouchers, by 25-50 per cent depending on the grade (Table 2).[25] 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, which were allocated through public tenders organized by the Ministry of Education and its regional offices. Priority was usually granted to schools catering for students from lower socio-economic backgrounds.[26] In Appendix Section Appendix C we show that our estimates of the impact of the FSD on pupils' achievement do not simply capture the effect of infrastructure

[21] The survey was administered by the Studies Directorate of the Sociology Faculty at the Catholic University of Chile (*DESUC*) and a report based on it was written by Ruz Pérez and Madrid Valenzuela (2005).

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

[23] This data is available at http://datos.mineduc.cl/dashboards/19923/bases-de-datos-de-planes-y-programas-de-estudios-anos-2002-al-2016/.

[24] The event study specification reads:

$$Y_{gst} = \gamma_g + \eta_s + \theta_t + \lambda_{sg} + \phi_{st} + \mu_{gt} + \sum_{\rho=-5}^{-2} \beta^\rho 1(p_{gst} = \rho) + \sum_{\rho=0}^{4} \beta^\rho 1(p_{gst} = \rho) + \varepsilon_{gst} \quad (1)$$

g, s and t index the grade, the school and the year, respectively. $p_{gst} = t - E_{gs}$ is the distance (in years) from the event, which is the introduction of the FSD in grade g of school s. Controls consist of grade (γ_g), school (η_s) and year (θ_t) fixed effects, as well as their interactions. The FSD is adopted in event-year 0 and coefficients β^ρ show how different the number of subjects taught is in event-year ρ relative to event-year -1, which is taken as the reference year. Standard errors are clustered at the school-grade level.

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

[26] Schools serving pupils from higher socio-economic backgrounds were less likely to need infrastructure-related investments.

investments.

3. Empirical strategy

In order to study whether increased instruction time and a longer school day affect achievement, we exploit the fact that we observe several cohorts of pupils starting primary education in a given school in the 2002-2010 period and then taking a standardized test at the end of grade 4 –possibly in a different establishment– over the period 2005-2013. Since we can follow the entire school career of each one of these students, we can compute actual years of exposure to the FSD by the end of grade 4 as $ExpFSD4_i = \sum_{j=1}^{4} d_i^j$, where d^j is a dummy that takes value 1 if the pupil is ever exposed during grade j to the FSD.[27]

We then estimate the following specification:

$$Y_{ist} = \eta_s + \theta_t + \beta ExpFSD4_{ist} + \gamma X_{ist} + \delta Z_{st} + \varepsilon_{ist} \tag{2}$$

Y_{ist} is the test score of student i who starts primary school in school s in year t and then takes the standardized test at the end of grade 4. η_s is a set of school fixed effects that account for time-invariant heterogeneity across schools; θ_t is a set of year fixed effects that control for common unobserved year-specific shocks. In the richest specifications, we also include a set of controls at the student and at the school level. X_{ist} is a vector of student characteristics; specifically, it includes the pupils' gender and three characteristics measured when they attend first grade: age, attendance rate and end-of-year status (i.e. promotion to second grade or retention in first grade). Z_{st} averages student characteristics contained in X_{ist} at the school level. It also includes enrollment and average class size in first grade. Standard errors are clustered at the school level.

By including first-grade school fixed effects, specification (2) leverages variation in exposure to the FSD by the end of grade 4 across cohorts of students who enrolled in the same establishment. It therefore exploits the fact that, depending on whether the school adopted longer schedules within our sample period (i.e. by 2013) and on the exact year of adoption, adjacent cohorts of enrollees could experience a different exposure to the FSD before taking the test. This source of variation can be used to estimate the causal effect of the FSD on learning outcomes if cohorts of pupils are not systematically different along characteristics that are not taken into account in specification (2) and that correlate both with years of exposure to increased instruction time and with achievement.

Given the staggered adoption of the FSD across schools, a first concern is that parents would factor the availability of the longer school day into their preferences about the school in which to enroll their children. This could affect the composition of pupil intake, possibly along dimensions that our set of controls cannot account for. According to parent surveys administered alongside the test in 2005, the FSD was the most important reason for enrolling their child in a given school for only 2.46 per cent of parents. Proximity to home (27.61 per cent), the presence of a relative in the school (15.66 per cent) and the school's prestige (14.89 per cent) were cited as the most important determinants of school choices.[28] In line with this survey evidence, we find that 98.43 per cent of the students starting grade 1 in a school without FSD had alternative schools that had already adopted the policy in their home municipality.

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 before this school adopts the FSD in first grade or in any other grade. As an example, if a school adopts the longer school day in at least one grade in 2007, we discard students who start primary education in that school in 2007 or later. Cohorts that enrolled before 2007, on the other hand, made their decision before the introduction of the FSD and possibly became exposed to it at some point in their school career.

For *incumbent* students who never repeat a grade the range of the treatment variable ($ExpFSD4_{ist}$) is 0-3, as exposure to the FSD can start as early as in grade 2. For repeaters, on the other hand, the range is 0-4; the variable takes value 4 in cases where the school adopts the FSD in the year when the pupil repeats first grade. Furthermore, restricting the sample to *incumbent* students implies that the first-grade controls included in specification (2) are observed before the adoption of the FSD and hence are pre-determined with respect to the treatment. The focus on *incumbent* cohorts also characterizes recent studies on the effects of charter takeovers in the US and of academy conversions in England (Abdulkadiroğlu et al., 2016; Eyles & Machin, 2018; Eyles et al., 2017). This restriction attenuates threats to identification related to unobserved changes in pupil intake, the more so the less parents can anticipate the exact year in which a school will adopt the FSD.

Students can move across schools and in Chile school transfers are indeed a common phenomenon; in our estimation sample (described in Section 4) around 35 per cent of students change school between grades 1 and 4. Pupils who enroll in first grade in the same establishment and in the same year can therefore experience a different exposure to the FSD by the time they reach grade 4. Furthermore, if mobility across schools is influenced by the availability of longer schedules, student-level actual exposure to the FSD could be correlated with other unobserved determinants of achievement. To mitigate this concern, in our preferred specification we instrument actual exposure to the FSD with the exposure a student would have accumulated had she/he never transferred from the school where she/he attended first grade. The instrumental variable is therefore $PotExpFSD4_i = \sum_{j=1}^{4} d_s^j$, where d_s^j is a dummy variable that takes value 1 if student i would have ever been exposed to the FSD in grade j, had she/he remained in school s, where she/he started first grade.[29]

When discussing the results in Section 5, we show that the instrument is relevant, as there is a positive and statistically significant relationship between "potential" exposure and actual exposure to the FSD by grade 4. By relying on this instrument we aim to isolate and exploit the variation in actual exposure that is not affected by the possibly endogenous mobility decisions of *incumbent* pupils after first grade. Moreover, we assume that "potential" exposure is not systematically correlated with unobserved determinants of achievement and affects fourth grade test scores only through its impact on actual exposure.

A remaining concern is that the timing of adoption may depend on past performance. For example, if schools switch to the longer school day after they observe a cohort of pupils faring particularly poorly in the test, our estimates may simply capture mean-reversion effects. In general, there can be concerns about the confounding effects of underlying school-specific trends in test scores. We show in Section 4 that there are no visible clear trends in reading and mathematics scores in the years preceding the switch to longer schedules. Another concern is that other

[27] The treatment is therefore more precisely defined as the number of grades attended at least once under the FSD scheme by the end of grade 4. Throughout the paper, we use the term years of exposure to the FSD for brevity. Moreover, the two definitions are exactly equivalent for non repeaters, who constitute the largest majority of the sample (88 per cent).

[28] Ethical values (8.47 per cent) and the cost (7.34 per cent) of the school follow in the ranking. The presence of the FSD is ranked seventh out of 15 options.

[29] To construct this "potential" measure of exposure we also assume that the student would never have repeated, as we do not observe the pattern of repetitions in this counterfactual school career. Therefore, the range of the instrumental variable for all *incumbent* pupils is 0-3. The first stage regression specification reads:

$$ExpFSD4_{ist} = \eta_{0,s} + \theta_{0,t} + \beta_0 PotExpFSD4_{ist} + \gamma_0 X_{ist} + \delta_0 Z_{st} + \epsilon_{ist} \tag{3}$$

events may take place at the school around the time of FSD adoption, which could also affect learning outcomes in the subsequent years. We discuss and address these additional issues in Appendix C.

While the identification strategy underlying the regression specification (2) is designed to study the effect of individual exposure to the FSD on students' performance, it is not suited to investigate whether the FSD triggers changes at the school level. To examine the impact of the FSD on school-level outcomes, we rely on an event study analysis. The baseline specification reads:

$$Y_{st} = \eta_s + \theta_t + \sum_{\rho=-5}^{-2} \beta^\rho 1(p_{st} = \rho) + \sum_{\rho=0}^{4} \beta^\rho 1(p_{st} = \rho) + \varepsilon_{st} \quad (4)$$

Y_{st} is the outcome of interest in school s in year t. $p_{st} = t - E_s$ is the distance (in years) from the event, which is the introduction of the FSD in at least one grade. η_s and θ_t are school and year fixed effects, respectively. The FSD is implemented in event-year 0 and the coefficients β^ρ show how different the outcomes observed in event-year ρ are in comparison to event-year -1 (i.e. the year before the adoption of the policy), which is taken as the reference year. Schools are observed up to 5 years before and after the introduction of the longer school day. This analysis allows us to study how a given school input, such as the number of teachers, change around the adoption of the FSD.

4. Data and sample

We link several administrative and survey datasets taking advantage of unique schools, students and teachers identifiers.

Data on achievement in fourth grade comes from a nationwide standardized low-stakes test (SIMCE test) designed by the Education Quality Agency (*Agencia de Calidad de la Educación*).[30] It is administered at the end of the school year and is marked by external examiners, therefore leaving little room for test score manipulation. The testing frequency is highest in fourth grade: individual records on performance in the test are available for fourth grade students in 1999, 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 we can follow students' school careers only for cohorts who start primary school from 2002 onward; this is necessary both to correctly identify *incumbent* students (i.e. pupils who enroll in first grade in a school that has not yet adopted the FSD) and to compute actual exposure to the FSD for students who move between schools between grades 1 and 4. 2013 is the last year of data available to us. We use pupil-level test scores in the reading and mathematics sections of the test as our measure of achievement. Scores are standardized by year and subject to have a mean equal to 0 and a standard deviation equal to 1. Alongside the test, surveys are administered to students and their parents, as well as to teachers. Based on questions that are consistent across all waves of the parent survey, we recover a rich set of information on pupils' backgrounds as of grade 4.

The second source of information is the register of pupils enrolled in the school system over the period 2002-2013, maintained by the Ministry of Education. Besides gender and date of birth, for every school year it records information about the school that the student attends, the attendance rate and the end-of-year status (i.e. promotion to the next grade or retention in the same grade). We also have access to the register of educational establishments, from which we recover the administrative status of the school (public, charter or non subsidized private). 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 from grade 1 to grade 4 of every student who started primary school between 2002 and 2010 and took the fourth grade test between 2005 and 2013; we then compute the actual years of exposure to the FSD by the end of grade 4, as well as the exposure a student would have experienced had she/he never transferred from her/his first grade school. We also retrieve the set of first grade student- and school-level characteristics that we include in the richest regression specification. In order to distinguish charter schools with and without tuition fees, we rely on a dataset maintained by the Ministry of Education that records all the subsidies that schools received from the government over the 2005-2013 period. Since charter schools that charge tuition fees receive reduced subsidies, we can distinguish them from schools that do not charge tuition fees.[31]

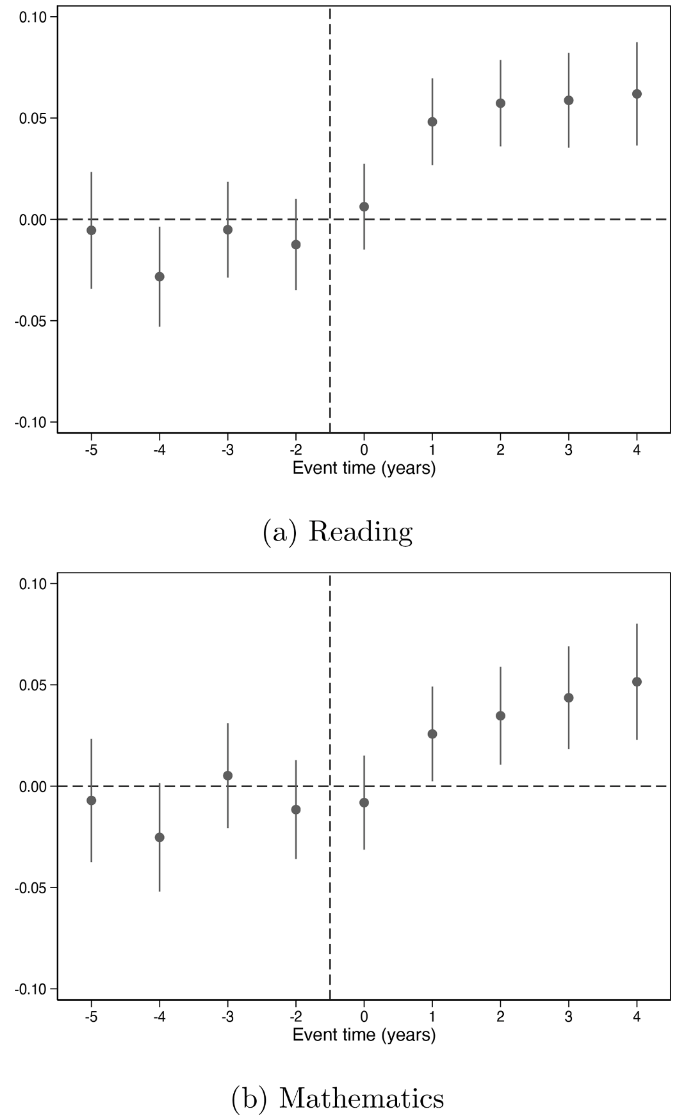


Fig. 2. Evolution of Test Scores relative to 1 Year before the FSD Adoption Notes: Panels (a) and (b) plot coefficients, alongside 95 per cent confidence intervals, from the event study specification (5). The FSD is adopted in event-year 0 and the coefficients show how different the reading and mathematics test scores are in event-year ρ relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level.

[30] While the stakes are low because the test does not impact a student' final evaluation, schools care about it because school-level average scores are publicly available for consultation.

[31] We classify a charter school as a no-fee charter school if it never charged fees between 2005 and 2013. For 2.39 per cent of charter schools attended in first grade by students belonging to the master sample we do not find information about the tuition fees in the dataset.

Table 4
Summary Statistics.

	All	Public	Charter	
			No Tuition Fees	Tuition Fees
	(1)	(2)	(3)	(4)
Students demographics				
Female	0.49	0.50	0.49	0.49
Age at school entry	6.60	6.60	6.60	6.59
Parental education				
Less than university	0.87	0.92	0.91	0.78
Books at home				
At most 50	0.85	0.89	0.88	0.79
Other resources at home				
Computer	0.55	0.42	0.50	0.72
Internet	0.31	0.20	0.27	0.46
Schools Characteristics				
First grade average class size	34.68	33.69	35.37	35.53
First grade enrollment	82.33	83.09	72.39	86.56
Academic performance				
Reading test score	-0.04	-0.17	-0.12	0.18
Mathematics test score	-0.04	-0.17	-0.15	0.19
1st-grade attendance rate	94.23	93.88	94.83	94.33
End of 1st-grade status (1=repeat)	0.03	0.03	0.04	0.02
N. of students	604532	270417	114074	218495

Notes: The table reports summary statistics for the sample of 4th graders who start primary school between 2002 and 2010 in publicly subsidized schools that had not yet adopted the FSD. Parental education refers to the highest educational attainment among the mother and the father; in case the information is missing for one parent, it refers to the education level of the other parent. All figures are expressed as fractions, except for averages referring to the age of pupils, class size, enrollment, test scores and the attendance rate. Test scores are standardized by year and subject (including also pupils who are not in the master sample) to have mean equal to 0 and standard deviation equal to 1. The number of observations in columns (2) to (4) does not sum to the number of observations in column (1) because for 2.39 per cent of charter schools we could not find information about the tuition fees.

We also exploit the information contained in the register of teachers, which is available for the period 2003-2013. We draw on this dataset to study how no-fee charter and public schools adjust the number of teachers and their working hours after the adoption of the FSD. We also rely on the 2005 Longitudinal Teachers Survey (*Encuesta Longitudinal Docente*) to investigate differences in teachers' opinions on the FSD.

Finally, we digitized from primary sources the list of schools that received additional funds to expand their infrastructure when lengthening the school day; we parsed 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 infrastructures were assigned. 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. Since 2008, students from disadvantaged backgrounds are granted additional subsidies (PSS) on top of the vouchers. We obtain the list of beneficiaries from the Ministry of Education. This information is used to perform robustness checks described in Appendix C.

In order to create the estimation sample, we restrict our attention to *incumbent* pupils, i.e. students who started first grade in a given school when the FSD had not been yet introduced (see Section 3). Furthermore, we discard students that attended non-subsidized private schools at some point between grades 1 and 4. This is motivated by the fact that the FSD reform only applies to publicly subsidized schools. Moreover, we do not know whether a given non-subsidized private school was already offering a longer school day or started providing it at some point after it became compulsory for other types of schools. Therefore, students attending non-subsidized private schools cannot serve as a control group.

The estimation sample consists of around 600,000 4th-grade test takers; they started primary school between 2002 and 2010 in schools that had not yet adopted the FSD and took the test between 2005 and 2013. It follows that schools attended by pupils in the master sample had not switched to the longer school day by 2002. While schools could start to offer the FSD in 1997, Fig. 1 shows that many of them had not yet done so by 2002. Focusing on this sub-sample of schools does not threaten the internal validity of our results. However, to assess the external validity of our analysis, it is important to examine whether schools in our sample are different from those that adopted the FSD earlier. As we show in Appendix B, we find in many cases statistically significant differences, but they are small in size. This suggests that the schools in our sample are representative of Chilean subsidized primary schools and our identification strategy does not appear to impair the external validity of our analysis within the Chilean context.

As discussed in Section 3, a threat to identification could arise if schools adopted the FSD based on the trend or transitory component of test scores. Fig. 2 plots coefficients from a slightly modified version of the event study exercise based on regression specification (4), where the outcome Y_{ist} is the reading or mathematics score of student i who takes the SIMCE test in school s in year t.[32] For both subjects, there appear not to be evident trends in the pre-adoption period, suggesting that test scores were not trending either downward or upward before schools decided to implement longer daily schedules. Furthermore, there are no evident spikes or dips in test scores just before the introduction of the FSD. On the other hand, from event-year 1 scores start increasing. This pattern suggests both a positive effect of the FSD on achievement and the presence of initial adaptation costs (as improvements start to become significant when the FSD has been in place at the school for two years). We provide a formal estimation based on our identification strategy in Section 5, where we also explore in a non parametric way how the effects of the FSD grow with each additional year of exposure.

Table 4 reports summary statistics for pupils in the master sample. Column (1) pools all students together, whereas columns (2) to (4) split schoolchildren according to the type of school (public, charter without tuition fees and charter with tuition fees) they attended in first grade.

In the vast majority of households (87 per cent) parents do not have university education.[33] Only 15 per cent of students have more than 50 books at home; 55 per cent of the households have a computer at home and slightly less than one third also have a connection to the Internet.[34] The first grade attendance rate is very high (94 per cent) and 3 per cent of pupils repeat first grade. On average, there are 35 students in a first grade class.

When splitting students according to the type of establishment they

[32] The regression specification therefore reads:

$$Y_{ist} = \eta_s + \theta_t + \sum_{\rho=-5}^{-2} \beta^\rho 1(p_{st} = \rho) + \sum_{\rho=0}^{4} \beta^\rho 1(p_{st} = \rho) + \varepsilon_{ist} \quad (5)$$

The sample consists of all schools where students in the master sample enrolled in first grade. The sample is unbalanced, meaning that not all schools are observed in every event-year. Given the calendar of SIMCE tests, using a balanced sample would significantly reduce the number of event-years that we can observe. For this exercise, we also use the 1999 and 2002 waves of the SIMCE test.

[33] We construct a variable that measures parental education by setting $ParentalEd = \max(MotherEd, FatherEd)$, where $MotherEd$ and $FatherEd$ are the highest academic attainment of the mother and the father, respectively; if the information for either one of the two parents is missing, we rely on the level of education achieved by the other parent.

[34] Information about students' backgrounds, i.e. parental education and resources at home, is drawn from parent surveys. Since these variables are observed at the end of grade 4, they could be affected by a student's exposure to the FSD (for example, if longer school days have an effect on parents' labour supply); for this reason, they are not included in the regression specifications, which only feature pre-determined controls. Furthermore this information is missing for around 15 per cent of schoolchildren in the sample.

Table 5
Effect of the FSD on Test Scores.

	Linear specification				Non parametric specification	
	FE1 (1)	FE2 (2)	FE-IV1 (3)	FE-IV2 (4)	FE-IV1 (5)	FE-IV2 (6)
			A. Reading			
ExpFSD4	0.002	0.011***	0.024***	0.024***		
	(0.003)	(0.003)	(0.006)	(0.006)		
ExpFSD4 = 1					0.023	0.022
					(0.015)	(0.015)
ExpFSD4 = 2					0.029**	0.030**
					(0.015)	(0.014)
ExpFSD4 = 3					0.116***	0.114***
					(0.021)	(0.021)
First stage coefficient			0.720***	0.720***		
			(0.005)	(0.005)		
Kleibergen-Paap rk Wald F statistic			23614.36	24416.41	5136.66	5265.46
N. of students	596108	596108	596108	596108	596108	596108
			B. Mathematics			
ExpFSD4	-0.007**	0.005	0.007	0.008		
	(0.003)	(0.003)	(0.007)	(0.007)		
ExpFSD4 = 1					-0.014	-0.015
					(0.016)	(0.016)
ExpFSD4 = 2					-0.003	-0.002
					(0.017)	(0.016)
ExpFSD4 = 3					0.057**	0.058**
					(0.023)	(0.023)
First stage coefficient			0.719***	0.720***		
			(0.005)	(0.005)		
Kleibergen-Paap rk Wald F statistic			23460.87	24294.13	5140.07	5278.70
N. of students	596281	596281	596281	596281	596281	596281
Student-level controls	No	Yes	No	Yes	No	Yes
School-level controls	No	Yes	No	Yes	No	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the effect of the FSD, measured in terms of exposure by grade 4 (*ExpFSD4*) on reading and mathematics test scores. Estimates in columns (1), (3) and (5) are based on a parsimonious specification that only includes as controls school fixed effects and year fixed effects. Estimates in other columns are based on a richer specification that features an additional set of controls. Specifically, student-level controls include: gender, age at school entry, as well as the attendance rate and the status (pass or repeat) at the end of grade 1. School-level controls include averages of the students' characteristics at the school level, as well as enrollment and average class size in first grade. The effect of the FSD is assumed to be linear in exposure in columns (1) to (4), whereas it is allowed to vary in a fully non-parametric way in columns (5) and (6). In specifications FE-IV1 and FE-IV2 the treatment (i.e. actual years of exposure to the FSD by the end of grade 4) is instrumented with the exposure a student would experience had she never transferred out of her first grade school. Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

started primary school in, it emerges that public schools and charter schools without tuition fees cater for relatively similar students. On the other hand, schoolchildren attending charter schools that charge tuition fees come from more affluent households. Test scores are lowest in public schools and highest in charter schools with tuition fees.

5. Results

This section starts by documenting the average effect of the FSD on the academic performance of all the pupils in the estimation sample. It then shows that these effects are substantially different in public schools and in no-fee charter schools. It concludes by investigating the reason behind such difference.

5.1. Effect of the FSD on achievement

Table 5 reports results from regression specification (2). We start by discussing coefficients when we estimate the most parsimonious specification, which only includes school and year fixed effects, and we do not instrument actual years of exposure to the FSD (specification FE1, column 1). These estimates point to a virtually null effect on reading and a negative impact on mathematics. Including the pre-determined controls listed in Section 3, however, changes the picture significantly (specification FE2, column 2): the effect of an additional year of exposure to the FSD is positive for both subjects, although it is only statistically significant for reading (0.011σ). This indicates that controlling for first grade status (pass or repeat) is important because repeaters, who are low performers, spend more years at school and are therefore more likely to be exposed to the FSD at some point.

As mentioned in Section 3, a non negligible fraction of students transfer from one school to another between grades 1 and 4. Furthermore, the availability of longer daily schedules appears to influence mobility across schools. Relying on the event study analysis outlined in regression specification (4), Appendix Figure F.2 shows the evolution of transfers of pupils attending grades 1 to 4 at the school-year level, in a 5-year window around the implementation of the FSD. Following the introduction of longer schedules schools experience a decline in the outflow of pupils; at the same time, although the pre-adoption pattern is more scattered, inflows of students appear to increase, with a spike in the year of adoption. As a result, net transfers (i.e. the difference between transfers into and transfers out of a given school) grow, by up to 5 pupils per year. Appendix Table F.3 further shows that, among schoolchildren who belong to the master sample, those who transfer are negatively selected, as they have a slightly lower attendance rate in grade 1 (93 per cent versus 95 per cent) and are more than twice as likely to repeat first grade.[35] Moreover, it emerges that pupils tend to transfer

[35] first grade attendance rates have a very low dispersion, so that a 1 percentage point difference amounts to almost one fifth of a standard deviation.

Table 6
Heterogeneous Effects of the FSD on Test Scores by School Type.

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A - Reading			
*ExpFSD*4	0.019**	0.017*	0.033*	0.031*	0.035*	0.037*
	(0.009)	(0.009)	(0.018)	(0.018)	(0.019)	(0.019)
*ExpFSD*4 × no-fee charter	0.042**	0.041**	0.043**	0.048**	0.041*	0.043*
	(0.019)	(0.020)	(0.020)	(0.020)	(0.024)	(0.024)
Kleibergen-Paap rk Wald F statistic	828.30	222.42	175.01	154.79	125.50	122.72
N. of students	377856	297759	297759	297171	297171	297171
			Panel B - Mathematics			
*ExpFSD*4	0.012	0.015	0.022	0.020	0.023	0.025
	(0.009)	(0.010)	(0.018)	(0.018)	(0.019)	(0.020)
*ExpFSD*4 × no-fee charter	0.017	0.012	0.015	0.025	0.021	0.017
	(0.021)	(0.022)	(0.021)	(0.021)	(0.024)	(0.025)
Kleibergen-Paap rk Wald F statistic	839.28	221.87	174.06	149.25	121.80	121.19
N. of students	377719	298573	298573	298573	298573	298573
Interactions with students' characteristics (grade 1)	No	Yes	Yes	Yes	Yes	Yes
Interactions with students' characteristics (grade 4)	No	No	Yes	Yes	Yes	Yes
Interactions with schools' characteristics	No	No	No	Yes	Yes	Yes
Interactions with teachers' characteristics (grade 1)	No	No	No	No	Yes	Yes
Interactions with teachers' characteristics (grade 4)	No	No	No	No	No	Yes
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher-level controls	No	No	No	No	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effects of exposure to the FSD (ExpFSD4) on reading and mathematics test scores by the type of school that the student attended in grade 1. The sample is restricted to pupils who enrolled in public or no-fee charter schools. The estimates in column (1) are based on an enriched version of specification (2), where the treatment and all controls listed in the notes to Table 5, including year fixed effects, are also interacted with a dummy taking value 1 if the school is a charter establishment without tuition fees. In column (2) additional interactions between the treatment and students' characteristics observed in grade 1 are added. The characteristics are: gender, age, end-of-year status (pass or repeat) and the attendance rate, both at the individual level and averaged at the school level. Column (3) includes additional interactions between the treatment and students characteristics observed when they reach grade 4: a dummy that takes value 1 if no parent has university education, a dummy that takes value 1 if there are at most 50 books at home and a dummy that takes value 1 if there is no computer or Internet connection at home. These characteristics are also transformed into shares at the school level. Column (4) also includes interactions between the treatment and school characteristics observed when students were in grade 1 (class size, enrollment) or observed before our sample period (parental satisfaction with the school and its teachers in 2002; an index of school value added based on performance in the SIMCE in 2002). Columns (5) further includes interaction between the treatment and teachers' characteristics in the school where the pupil attends grade 1, observed when the student is in grade 1 (share of female teachers, average teachers' age and share of teachers with an education degree). Column (6) further adds the interaction between the treatment and the same teachers' characteristics working in the first grade school, but observed when the student is in grade 4. Actual years of exposure to the FSD are instrumented with years of exposure a student would accumulate had she never transferred from the school where she attended grade 1. In columns (2) to (6) the sample is restricted to observations where the covariates have no missing value, to keep the sample size constant. Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

towards schools that offer the FSD; while transferring and non transferring students have a very similar "potential" exposure (i.e. the exposure they would have experienced had they remained in their 1st-grade schools), the former end up with a much higher actual exposure.[36] Partly because of fewer transfers out and more transfers in, the number of students per class in grades 1 to 4 increases after the adoption of the FSD (Appendix Figure F.3) by an amount that is however modest (at most around 1.50 more pupils) when compared to the average class size in primary schools.

These patterns motivate the decision to instrument actual exposure (*ExpFSD*4) with the exposure a student would have experienced had she/he never transferred (*PotExpFSD*4). When adopting the IV approach, the estimates are remarkably stable across the most parsimonious specification (FE-IV1, column 3) and the specification featuring all controls (FE-IV2, column 4). An additional year of exposure to the FSD significantly raises reading test scores by 0.024σ. The effect on mathematics test scores lies in the narrow range 0.007-0.008σ, but is not statistically significant. As shown in the same table, the instrumental variable displays a positive and strong relationship with the treatment, as the first stage coefficient is statistically significant and equal to 0.72, implying that for slightly less than 30 per cent of pupils the real exposure and the "potential" exposure do not coincide.

In columns (5) and (6) of Table 5, we relax the assumption that every additional year of exposure has the same effect on achievement. We estimate the preferred IV specification in a fully non parametric way, by introducing a set of dummies for every possible level of exposure to the FSD and setting 0 years of exposure as the reference category.[37]

The non parametric specification reveals that the effect of longer schedules increases more than linearly with exposure. Three years of exposure are associated with a 0.114-0.116σ increase of reading test

[36] This also holds true when restricting the comparison to students who never repeat between grade 1 and grade 4.

[37] The non-parametric specification therefore reads:

$$Y_{ist} = \eta_s + \theta_t + \sum_{k=1}^{2} \beta_k 1(ExpFSD4_{ist} = k) + \beta_3 1(ExpFSD4_{ist} >= 3) + \gamma X_{ist} + \delta Z_{st} + \varepsilon_{ist} \quad (6)$$

Specification (6) highlights that we collapse 3 and 4 years of actual exposure into a unique category, as only very few pupils (i.e. students who repeat first grade in the year when the school adopts the longer schedules) attend all 4 grades under the FSD scheme. In the IV specification, the set of dummies that capture every possible level of actual exposure to the FSD are instrumented by a set of dummies that capture every possible level of exposure a student would have experienced had she never transferred out of her first grade school.

Table B.1
Comparison between schools in our sample and other schools.

	β_0	β_1
	A. Characteristics of students who attend grade 1 in 2002	
Female = 1	0.497***	-0.007***
	(0.002)	(0.002)
Age in grade 1	6.622***	-0.011***
	(0.002)	(0.003)
Attendance rate in grade 1	97.454***	0.222***
	(0.011)	(0.013)
Repeat grade 1 = 1	0.097***	-0.009***
	(0.001)	(0.001)
Parental education < university	0.872***	0.010***
	(0.001)	(0.002)
Books at home ≤ 50	0.869***	-0.003*
	(0.001)	(0.002)
No computer or Internet at home	0.878***	0.003*
	(0.001)	(0.002)
	B. Characteristics of teachers in 2002	
Female = 1	0.761***	0.007
	(0.004)	(0.005)
Age	45.299***	0.438**
	(0.172)	(0.218)
Experience	17.485***	0.133
	(0.202)	(0.256)
Hold education degree = 1	0.928***	-0.007*
	(0.003)	(0.004)
	C. Characteristics of schools in 2002	
Cohort size in grade 1	60.670***	2.06
	(1.211)	(1.522)
Number of classes in grade 1	1.744***	0.107***
	(0.028)	(0.035)
Class size in grade 1	33.225***	-1.067***
	(0.275)	(0.346)
SIMCE reading score	-0.073***	-0.016
	(0.014)	(0.018)
SIMCE mathematics score	-0.093***	0.016
	(0.014)	(0.017)

Notes: The table reports estimates from regression specification (B.1). The coefficient β_0 is the average value of the various characteristics listed in column (1) among schools not in our sample (i.e. those that adopted the FSD by 2002); the coefficient β_1 measures how different schools in our sample are. Panel A focuses on the characteristics of pupils who started grade 1 in 2002; the characteristics of the households they live in (parental education, number of books at home, availability of a computer and Internet) are measured when pupils attend grade 4. Panel B reports teachers' characteristics in 2002. Panel C displays schools' characteristics in 2002, including the performance of students in the fourth grade SIMCE reading and mathematics tests.

scores, significant at the 1 per cent level, and a 0.057-0.058σ increase of mathematics test scores, significant at the 5 per cent level.[38]

The IV estimates therefore show that the FSD has a positive effect on learning outcomes, which increases more than linearly with exposure and is stronger for reading than for mathematics. The stronger impact on reading may in part depend on the fact that a larger fraction of additional instruction time is devoted to Spanish than to mathematics (Table 3). If the effect of extra instruction hours is the same across subjects this could not be the only explanation: mathematics scores should have increased by 0.015σ, a value within the confidence interval but almost twice as large as the point estimate. The production function of mathematics skills may, however, be different from that of reading skills (e.g. Aucejo & James, 2019): it therefore may be the case that the effect of additional instruction time is not the same across these two subjects.

The pattern of coefficients in the fully non parametric specification is consistent with added instruction time in earlier grades having a positive effect on achievement in later grades. Moreover, as the passage from a two-shift to a one-shift scheme implies a re-organization of daily routines, it may also be explained by the presence of adaptation costs that eventually fade away over time.

A possible remaining concern is that other events may happen in a school around the adoption of the FSD and affect learning outcomes in the following years. In Appendix C we show that our estimates are robust to: *i)* restricting the attention to pupils who started primary schools in establishments that most likely did not expand their infrastructure at the same time when the FSD was adopted; *ii)* controlling for a policy granting further subsidies to disadvantaged schoolchildren since 2008.

5.2. Heterogeneity by school type

We compare pupils who started primary school in a public school to those who enrolled in a charter school that does not charge tuition fees. This choice is motivated by the fact that, as shown in Table 4, charter schools with tuition fees cater for more affluent pupils, whereas public schools and charter schools without tuition fees both serve children of lower socio-economic status. This attenuates the concern that the comparison between different types of schools is confounded by differences in students' characteristics.

We estimate a richer version of specification (2), whereby we also interact the treatment and all controls with a dummy variable that takes value 1 if the pupil started primary school in a charter school.[39] The coefficient of interest is the interaction term "ExpFSD4 × no-fee charter". We report estimates coming from the preferred linear IV specification that includes the full set of controls (FE-IV2).[40]

Table 6, column (1) shows that returns to additional instruction time are higher for students starting primary school in no-fee charter schools. The difference, captured by the interaction term "ExpFSD4 × no-fee charter" is sizable for both subjects and statistically significant with regards to reading. The effect of an additional year of exposure to the FSD on reading test scores is more than three times larger for students starting primary school in no-fee charter schools (0.061σ) as opposed to in public schools (0.019σ). Mathematics scores are raised by a statistically insignificant 0.012σ for pupils attending grade 1 in public schools; the coefficient more than doubles to 0.029σ for enrollees of no-fee charter schools, although the associated p-value is slightly above 0.1.

To assess the magnitude of the gains in reading proficiency, we benchmark them to some important results in the literature on school inputs. Chetty, Friedman, and Rockoff (2014) estimate that a 1σ improvement in teachers' value added raises end-of-grade reading test scores by approximately 0.1σ. It takes 1.6 years of exposure to the FSD in no-fee charter schools and 5 years of exposure in public schools to produce an equivalent improvement.[41] Krueger and Whitmore (2000) find that a one-unit decrease in class size boosts test scores[42] by an amount (0.048σ) that can be achieved by less than 1 year (resp. 2 years and a half)

[38] The fact that the benefits of longer school schedules grow with individuals exposure is not in contrast with the relative flat profile that emerges after event-year 1 in the event study displayed in Fig. 2. In the event studies pupils' test scores are not regressed on individual exposure to the FSD (as we do in our main regression specification) but on how long the FSD has been in place in at least one grade at the school.

[39] A fully interacted specification yields estimates that are equivalent to those obtained from separately estimating two regressions on the two sub-samples of students.

[40] "ExpFSD4" and "ExpFSD4 × no-fee charter" are instrumented with "PotExpFSD4" and "PotExpFSD4 × no-fee charter", respectively.

[41] In this and the following comparisons, we assume –as in our main regression specification– that the effect of the FSD on achievement is linear in years of exposure; moreover, it is important to remember that not all additional instruction hours were devoted to teaching Spanish (Table 3).

[42] The measure of proficiency used in the paper is the average score on the Stanford achievement test.

of exposure to the FSD in no-fee charter schools (resp. public schools).[43]

Why are returns to the FSD larger for pupils who enrolled in no-fee charter schools? A first hypothesis is that the gains could be higher for students with certain characteristics and that such pupils simply happen to be more prevalent in no-fee charter schools. We argue that this appears not to be the case. First, students in the two types of schools are similar in terms of observable attributes; those attending no-fee charter schools are slightly more affluent, but in Appendix D we provide suggestive evidence that, if anything, longer schedules benefit students from advantaged backgrounds less. Second, we do not find support for this explanation when we directly test it. To this end, in columns (2) and (3) of Table 6 we further add the interaction terms "ExpFSD4 $\times$ $\overline{X}$", where $\overline{X}$ is a vector of students' characteristics.[44] If the hypothesis was true, controlling for the fact that the FSD can affect different students heterogeneously should shrink the size of "ExpFSD4 $\times$ no-fee charter". However, this does not happen.

Another explanation could be that no-fee charter schools and public schools are systematically different in terms of quality, and that returns to the FSD vary with school quality. In column (4) of Table 6 we test this hypothesis by further adding the interaction terms "ExpFSD4 $\times$ $\overline{Z}$", where $\overline{Z}$ is a set of variables that proxy for school quality.[45] This does not appear to be the main explanation either, as "ExpFSD4 $\times$ no-fee charter" remains large and statistically significant.

In columns (5) and (6) we also control for the possibility that returns to the FSD are different depending on teachers' demographic characteristics.[46] Teachers in no-fee charter schools are younger (Appendix Table F.2), so one could posit that the heterogeneous effect documented in column (1) could perhaps be explained by younger teachers being more capable of adapting to longer schedules. Even in this very rich specification, however, the larger effect documented for students who attend no-fee charter schools does not vanish, meaning that this is not the explanation either.[47]

Having ruled out those explanations, we examine whether there are differences in how public and no-fee charter schools adjusted to provide longer schedules. Schools could choose how to allot the additional time across subjects and charter schools have more autonomy over the design of the course offer. However, according to survey evidence in Subsection 2.2, public and charter schools allocate the additional instruction time across subjects in a similar way. The only significant differences we find are in the time devoted to foreign languages and religion. If allocating more time to foreign languages positively affects students' performance in Spanish, this could partly explain the larger returns found in no-fee charter schools. However, when considering the total increase in time devoted to Spanish and foreign languages, the difference is very small: 1.2 minutes per week in favor of charter schools.[48]

Longer school schedules require schools to adjust the teaching input and charter schools also have more autonomy over recruiting, compensation and dismissal policies. Figs. 3 and 4 plot coefficients from the event study specification outlined in (4), where the outcomes are various measures of teaching inputs at the school-year level. Consistent with the need to provide more instruction hours, Fig. 3 shows that total teachers' contract hours and teaching hours increase after the adoption of the FSD (top panels). Moreover, the pattern of coefficients is similar across public and no-fee charter schools and the confidence intervals overlap. Indeed, Appendix Table F.5 shows that the difference between public and no-fee charter schools is significant only in the final event-year. When total contract and teaching hours are divided by the number of classes (bottom panels), the differences between the two types of schools are never significant.[49]

An increase in the number of total contract and teaching hours can be achieved by adjusting both the number of teachers and the number of contract/teaching hours per teacher. Fig. 4 shows that the number of teachers increases both in public and no-fee charter schools, but the increase is significantly higher in no-fee charter schools. In contrast, contract hours per teacher increase significantly more in public schools. Appendix Table F.5 confirms that these differences are statistically significant in most post-adoption event-years. Teaching hours per teacher also increase more in no-fee charter schools than in public establishments, with the difference being slightly smaller. Therefore no-fee charter schools rely more on expanding the number of teachers and less on increasing teachers' workload than public schools do.

Why do public and no-fee charter schools adjust the teaching input differently? We argue that charter schools' regulations likely make it easier to quickly hire new teachers when teaching hours need to be increased. First, charter schools can recruit teachers directly, rather than after setting up a commission as in public schools. Furthermore, if the match with the new teacher turns out to be unsatisfactory, the school faces fewer constraints in deciding to terminate the relationship. In addition to this, as public schools pay on average higher wages (Appendix Table F.2,

[43] The literature on the effects of class size is rich and there is variation in the magnitude of the estimates. Another well-known example, Angrist and Lavy (1999) reports that a one-unit decrease in class size increases the reading scores of Israeli fourth graders by 0.017-0.019σ. An equivalent gain can be achieved by 1 year of exposure to the FSD (and even by less than 1 year of exposure when focusing on pupils who enrolled in no-fee charter schools). Angrist, Lavy, Leder-Luis, and Shany (2019) no longer find evidence of class size effects when looking at a large sample of Israeli schoolchildren over the period 2002-2011.

[44] In column (3) $\overline{X}$ contains: pupil's gender, age in grade 1, attendance rate in grade 1 and end-of-year status in grade 1. These controls are included both at the individual level and as averages at the school level. In column (4) $\overline{X}$ further includes three characteristics that are measured in grade 4, when the student takes the test: a dummy that takes value 1 if no parent in the household has university education, a dummy that takes value 1 if there are no more than 50 books at home, and a dummy that takes value 1 if there is no computer or Internet connection at home. These controls are also included as school-level shares. It has to be kept in mind that specifications including interactions of the treatment with attributes observed in grade 4 could be problematic if such characteristics can be influenced by the treatment.

[45] We proxy quality with a set of school characteristics measured either when the student attends grade 1 - average class size and enrollment - or before our sample period - parents' satisfaction with the school and with the teachers in 2002 and a value added index based on schools' performance in the 2002 wave of the SIMCE. The value-added index for schools is computed independently for reading and mathematics. It comes from regressions in which students' scores in the 2002 wave of the SIMCE are regressed on a vector of individual characteristics (pupil's gender and age, parental education, number of books at home and availability of computer and Internet at home) and a school fixed effect. The school fixed effect captures the variation in pupils' scores not explained by the characteristics X included in the specification:

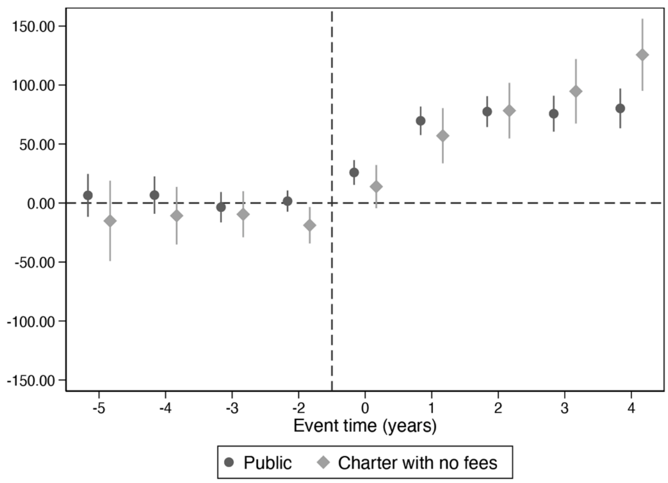
$$Y_{is} = \eta_s + \Sigma_{j=1}^{J} \beta_j X_{jis} + \varepsilon_{ist}$$

[46] In column (5), the vector $\overline{T}$ consists of teachers' demographic characteristics measured at the school level when the student attends grade 1 (share of female teachers, average teachers' age, and share of teachers with an education degree). In column (6) $\overline{T}$ also includes the demographics characteristics of teachers working in the first grade school when the student attends grade 4. If this information is missing for that school-year, we impute it with the value observed in the closest school-year.

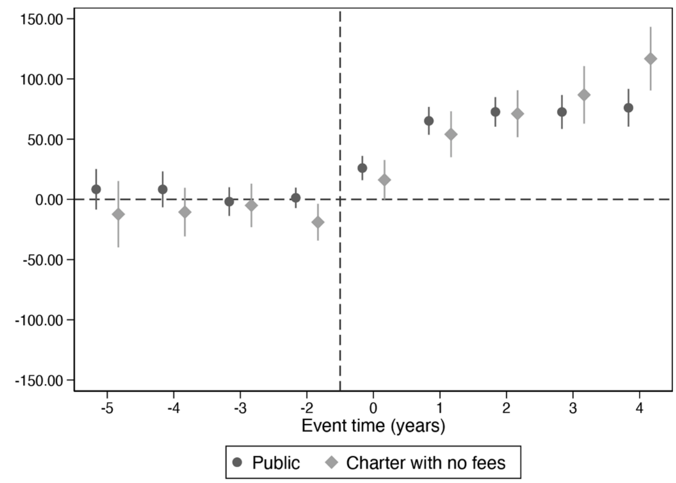
[47] In columns (2) to (6), the variables contained in the vectors $\overline{X}$, $\overline{Z}$ and $\overline{T}$ are: centered around their mean value in the sample when they are continuous; also included in the regression as main terms; and interacted also with all controls. The regressions are estimated on the sub-sample of pupils for whom all the variables have no missing value. Appendix Table Appendix F.4 presents similar results using all the observations available for every different regression.

[48] After considering the "Free Choice Time", the difference in the time allocated to Spanish and foreign languages between public and charter schools continues being around 30 minutes per week.

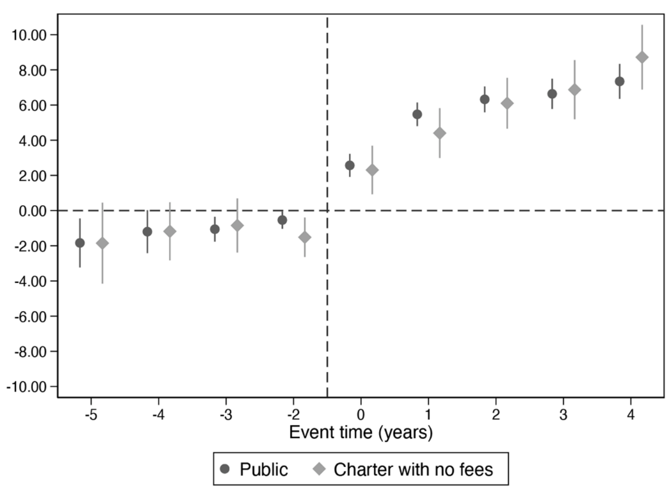
[49] The coefficients presented in Table Appendix F.5 come from a richer version of specification (4), where event year dummy and calendar year fixed effects are also interacted with a dummy (D_s) taking value 1 if the school is a no-fee charter school. This specification makes it possible to test whether differences between public and charter schools are statistically significant.



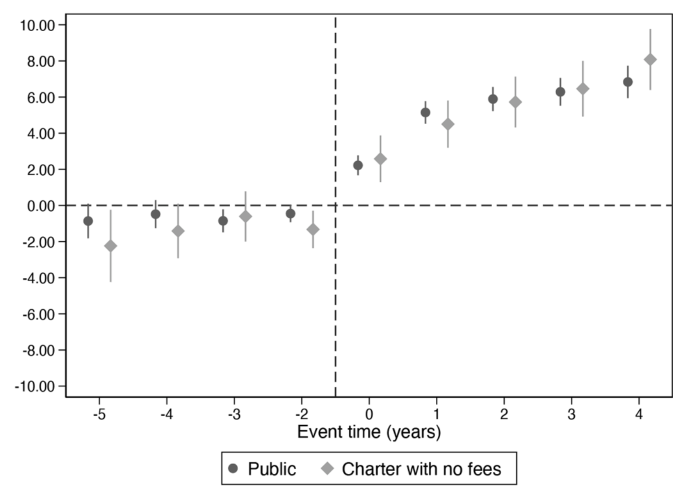
(a) Contract hours



(b) Teaching hours

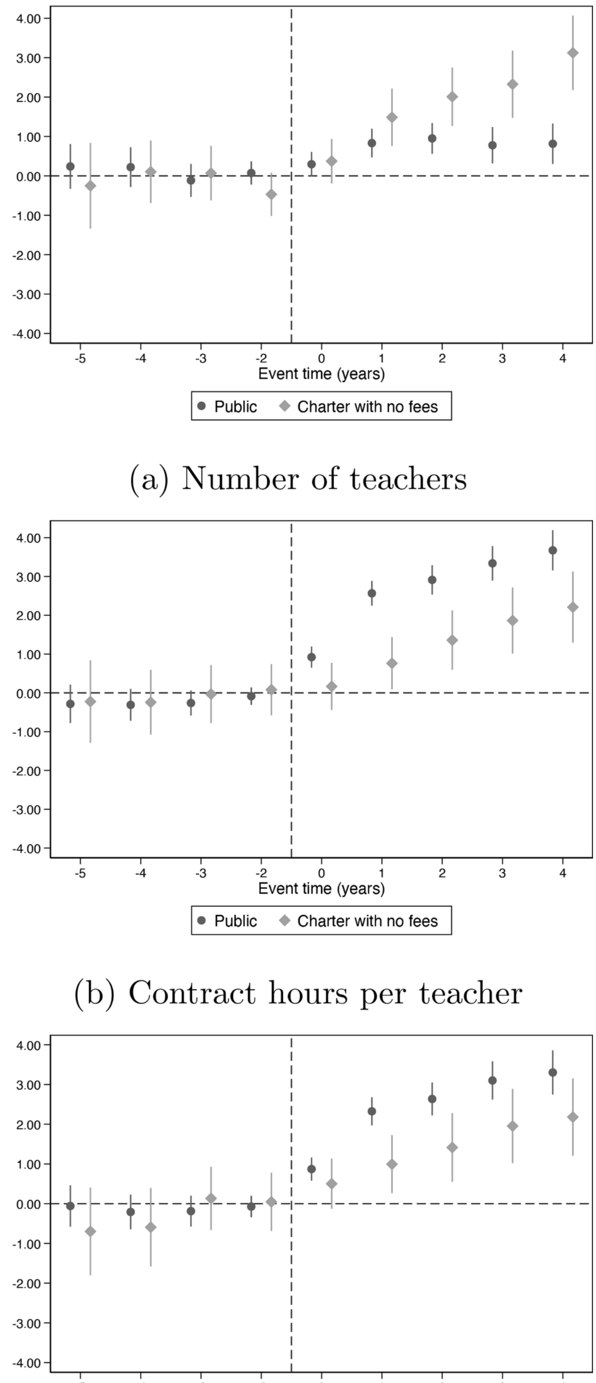


(c) Contract hours/n. of classes



(d) Teaching hours/n. of classes

Fig. 3. Evolution of Contract and Teaching Hours relative to 1 Year before the FSD Adoption Notes: Panels (a) to (d) plot coefficients, alongside 95 per cent confidence intervals, from the event study specification outlined in (4). The FSD is adopted in event-year 0 and the coefficients show how different contract and teaching hours are in event-year ρ relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level.



(a) Number of teachers

(b) Contract hours per teacher

(c) Teaching hours per teacher

Fig. 4. Evolution of Number of Teachers, Contract Hours per Teacher and Teaching Hours per Teacher relative to 1 Year before the FSD Adoption Notes: Panels (a) to (c) plot coefficients, alongside 95 per cent confidence intervals, from the event study specification outlined in (4). The FSD is adopted in event-year 0 and coefficients show how different the number of teachers, contract hours per teacher and teaching hours per teacher are in event-year ρ relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level.

column 1), they may also face greater budgetary constraints that prevent them from recruiting new teachers in the aftermath of the FSD adoption.

With the data available to us, we cannot examine whether, due to their recruiting and compensation policies, no-fee charter schools were also able to attract better quality teachers, even by poaching some of them from public schools. We can however compare teachers' opinions about the FSD in public and charter schools. Based on answers to a survey carried out in 2005, Appendix Table F.6 reports that public school teachers display a lower degree of satisfaction with longer daily schedules. Only 45 per cent of them judge the FSD to be "good or very good", compared to 54 per cent of charter school teachers. This may signal that the workload of teachers in public schools increases excessively following the introduction of the FSD.[50] This could in turn negatively affect the absolute quality of additional time use in public schools, contributing to explaining the lower returns to longer schedules.

Bellei (2009) and Berthelon et al. (2016) find that the effect of the FSD on achievement is larger in public schools. As discussed in Section 1, they focus on pupils attending grades 10 and 2, respectively, rather than grade 4 as in our setting. Besides the differences in the sample of students and the period under analysis, they do not restrict the comparison to public schools and no-fee charter schools. As fee-charging charter schools cater for more affluent pupils, it may be difficult to disentangle the role of school institutions from that of differences in students' characteristics (which matter, as shown in Appendix D). In Appendix E we adopt an IV strategy similar to that used by Berthelon et al. (2016): when comparing children in public and no-fee charter schools we find results that are in line with ours.

6. Conclusions

With the goal of improving pupils' academic achievement, many countries undertake costly educational reforms that expand the resources available to schools. This paper studies whether the way in which schools are organized and managed makes a difference on how they implement these educational reforms and, ultimately, on the gains that they generate in terms of their students' academic performance.

We address this question by examining how public and no-fee charter schools adjust during the roll-out of the Full School Day (FSD) reform, a large scale program that substantially increased weekly instruction time in Chile. While public and no-fee charter schools cater for similar students and are publicly funded through a voucher system, no-fee charter schools enjoy greater levels of autonomy. We find that exposure to the FSD has a positive effect on fourth grade test scores and that the benefits are larger for pupils enrolled in no-fee charter schools.

What is behind this difference? Although schools could choose how to allocate the additional instruction time across subjects, survey evidence indicates that public and charter schools made similar choices along this dimension. Furthermore, the heterogeneous impact of the FSD across the two types of school persists even when allowing longer schedules to have differential effects based on students', schools' and teachers' observable characteristics.

A dimension in which we uncover a significant difference is in how public and no-fee charter schools adjusted the teaching input to provide the additional instruction hours. No-fee charter schools relied more on hiring additional teachers and less on increasing working hours per teacher than public schools did. The greater autonomy that charter schools enjoy over recruiting and other personnel policies seems to have facilitated them to expand the teaching input by quickly hiring new teachers in the aftermath of the FSD adoption.

Our findings are in line with those of Lavy (2015), who documents that the productivity of instructional time is larger in schools that have more autonomy over staff and budget decisions. We find a similar result and provide suggestive evidence that autonomy over personnel

[50] In an event-study setting we however found no evidence of increasing teachers' turnover in public schools. This results is available upon request.

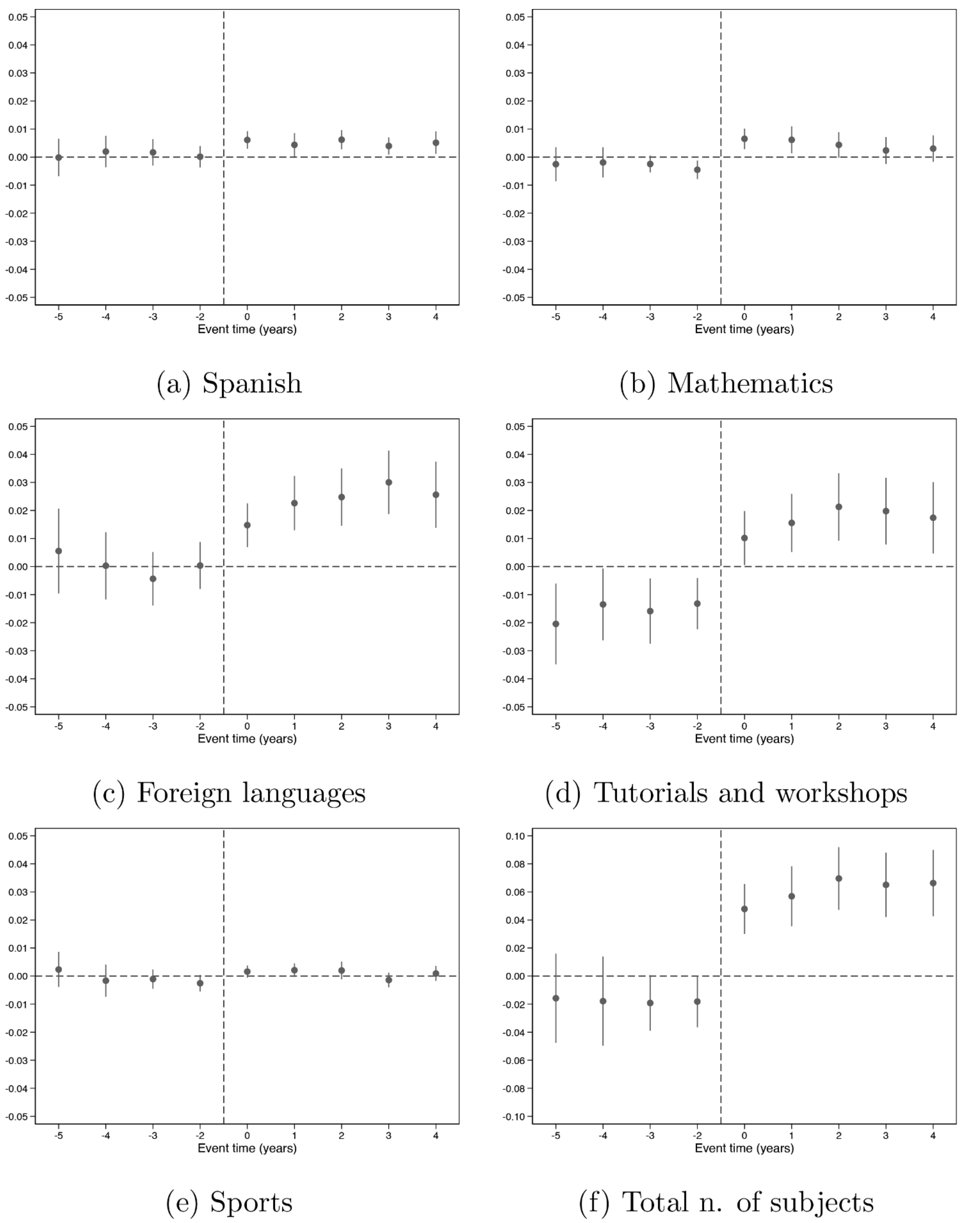
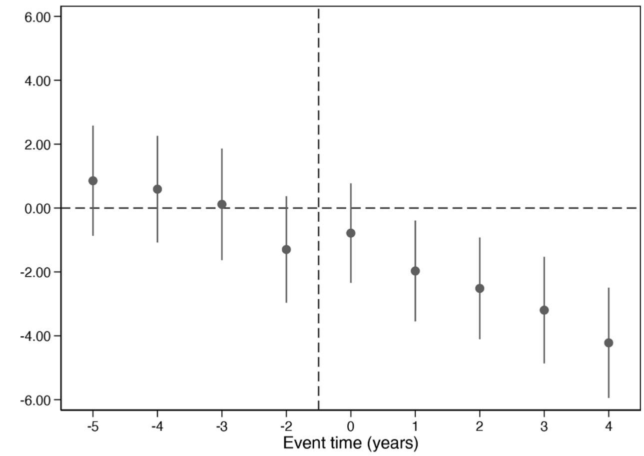
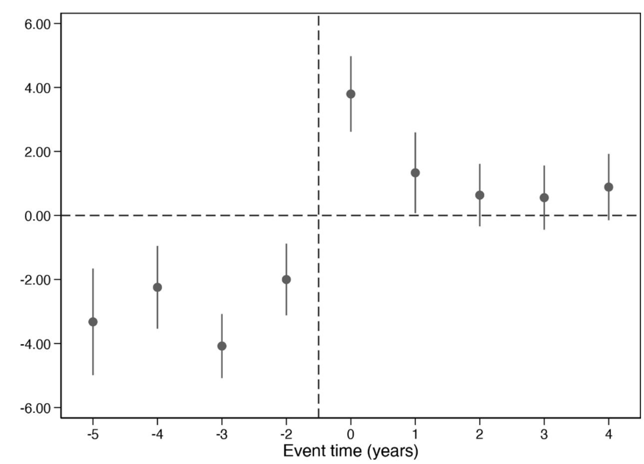


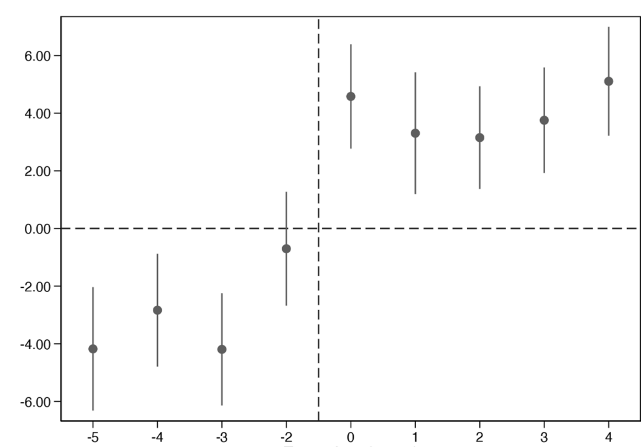
Fig. F.1. Evolution of the Number of Subjects Relative to 1 Year before the FSD Adoption Notes: Panels (a) to (f) plot coefficients, alongside 95 per cent confidence intervals, from the event study specification outlined in (1). The FSD is adopted in event-year 0 and coefficients show how different the number of subjects taught is in event-year *p* relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school, grade and year fixed effects, as well as their interactions. Standard errors are clustered at the school-grade level.



(a) Transfers out of the school



(b) Transfers into the school



(c) Net transfers (In - Out)

Fig. F.2. Evolution of Transfers relative to 1 Year before the FSD Adoption Notes: Panels (a), (b) and (c) plot coefficients, alongside 95 per cent confidence intervals, from the event study specification outlined in (4). The FSD is adopted in event-year 0 and coefficients show how different the number of transfers in grades 1 to 4 is in event-year p relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level.

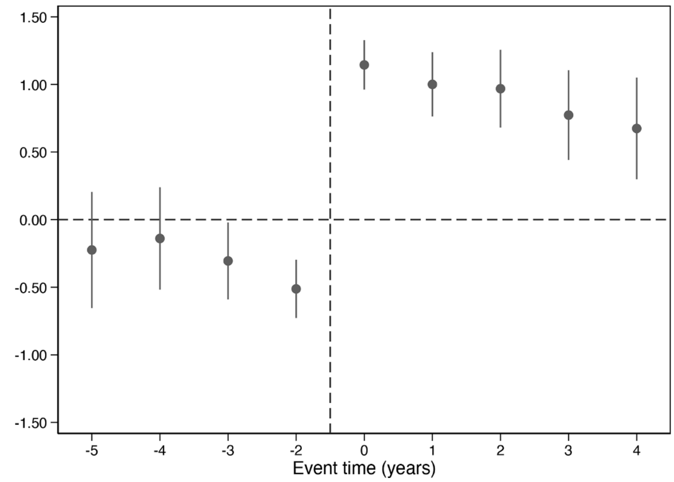


Fig. F.3. Evolution of Class Size relative to 1 Year before the FSD Adoption Notes: The figure plots coefficients, alongside 95 per cent confidence intervals, from the event study specification outlined in (4). The FSD is adopted in event-year 0 and coefficients show how different the average class size in grades 1 to 4 is in event-year ρ relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level.

decisions is especially important when increasing instruction time, as providing longer schedules requires expanding the teaching input as well. In general, our results suggest that school institutions and governance can matter for the effectiveness of various education policies. Further analysis on complementarities between school inputs and institutions could be a promising avenue for future research.

CRediT authorship contribution statement

Andrés Barrios-Fernández: Conceptualization, Methodology, Software, Validation, Formal analysis, Data curation, Writing - original draft, Writing - review & editing, Visualization. **Giulia Bovini:** Conceptualization, Methodology, Software, Validation, Formal analysis, Data curation, Writing - original draft, Writing - review & editing, Visualization.

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Appendix A. Additional Related Literature

Instruction time can be modified by extending the term length, by redistributing time across subjects, or by increasing the length of the school day. In Section 1 we have reviewed the work on the length of the school day. In this section we discuss some of the existing evidence about term length and subject-specific instruction time.

The early studies investigating the effect of instruction time on academic performance mostly focus on term length and report modestly positive to insignificant effects. These studies rely either on variation in term length between and within US states over time (Betts & Johnson, 1998; Card & Krueger, 1992; Eide & Showalter, 1998; Grogger, 1996; Rizzuto & Wachtel, 1980) or on cross-country differences (Lee & Barro, 2001; Wößmann, 2003). Patall, Cooper, and Allen (2010) presents a comprehensive review of studies conducted in the 1985-2009 period.

A set of more recent papers study the effect of the number of school days prior to standardized tests on performance, by exploiting either unplanned school closures due to adverse weather conditions (Goodman, 2014; Hansen, 2011; Marcotte, 2007; Marcotte & Hemelt, 2008) or changes in term dates and/or test dates (Agüero & Beleche, 2013; Aucejo & Romano, 2016; Sims, 2008). These studies find positive, although in some cases modest, effects. Pischke (2007) and Parinduri (2014) study the effects of exceptionally short or long school years due to country-level reforms of school calendars that leave the curriculum unchanged. The former studies the short 1966-67 West German school year and documents an increase in repetition rates in primary school as well as a reduction in enrollment to higher secondary school tracks, but no effects on earnings and employment. The latter examines the long 1978-79 Indonesian school year and reports a reduction in repetition rates and improved educational attainment, with positive effects also on wages and on the probability of working in the formal sector. Exploiting a randomized trial Zvoch and Stevens (2013) finds that kindergartners and first grade students substantially increase their literacy performance after participating in a 5 weeks summer school program in which students received 14 hours of tutoring per week.

Starting from Lavy (2015), recent studies examine the effect of instruction time on achievement and the drivers of its effectiveness by using cross-country PISA data and exploiting within-pupil variation in subject-specific classroom hours. Lavy (2015) finds that a one-hour increase of weekly subject-specific instruction time raises scores by 0.06σ and that schools' characteristics matter: the effect is larger for schools that enjoy more autonomy. 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, Oggenfuss, and Wolter (2017) focus their attention on Switzerland and document that students in more demanding school-tracks enjoy greater benefits.

Appendix B. Characterizing Schools in the Master Sample

Our identification strategy requires us to focus on schools that had not adopted the FSD by 2002. Although focusing on "late" instead of on "early" adopters does not pose threats to the internal validity of our results, to assess their external validity it is important to examine how the schools in our sample compare to others. We investigate this by dividing schools in two groups, based on whether they belong to our sample or not.

We want to examine whether schools in the two different groups serve different pupils. Ideally, we would like to study the characteristics of pupils who started grade 1 in 1996, before any school had adopted the FSD. Yet, this is not possible, because official students' registers are only available for cohorts who started primary school in 2002 or later. Hence, with this caveat in mind, we focus on schoolchildren who entered grade 1 in 2002 and we estimate the following specification:

$$y_{is} = \beta_0 + \beta_1 \times 1(\text{School}_s \in \text{sample}) \tag{B.1}$$

i indexes the student and s indexes the school. Appendix Table Appendix B.1 presents the results of this exercise. β_0 captures the average of variable y in the schools that adopted the FSD in 2002 or before and, hence, are not in our sample; β_1 measures the difference between these early adopters and schools that are in our sample.

Differences are in many cases statistically significant, but they are always very small in size: this indicates that schools in our sample are very similar to other schools. While our identification strategy requires us to focus on schools that had not adopted the FSD by 2002, it seems not to impair the external validity of our results.

Specifically, in panel A we look at various students' characteristics. Schools in our sample cater to students who are only slightly less likely to be female and a bit younger. The attendance rate in grade 1 is marginally higher, the difference being 0.2 against an average of 97.5, and the probability of repeating first grade is somewhat smaller. Differences remain very small also when looking at the characteristics of students' households, measured when they attend grade 4. Schools in our sample serve schoolchildren that are marginally more likely to have parents with no university education and to live in households without a computer or Internet (by 1.2 and 0.3 per cent, respectively).

We also estimate specification (B.1) using as dependent variables a set of characteristics of the schools and teachers in 2002. We find no statistically significant differences across the two groups of schools in regards to teachers' gender. Teachers working in our sample of schools are slightly older and more experienced; they are only 0.7 per cent less likely to have and education degree (Panel B). Finally, schools in our sample catered to somewhat more students (2, against an average of 61), in somewhat smaller classes. Notably, when comparing performances on the 2002 SIMCE test we find no significant differences in both reading and mathematics test scores (Panel C).

Appendix C. Robustness Checks

As a first robustness check, we show that specification (2) delivers similar estimates if we also control for a set of characteristics of teachers in the school s where the student started first grade in year t. Specifically, we include as controls the share of female teachers, the share of teachers with an education degree and teachers' average age.[51] Appendix Table C.1 shows that results are virtually unchanged.

A possible concern not addressed by specification (2) is that other events may happen in a school around the time of FSD adoption and affect

[51] These controls are not included in the baseline specification because they are not available for the year 2002 and are missing for some schools in other years. In this regression specification, we assume that the teaching staff in 2002 is the same as that observed in 2003, so as not to drop one year of observations.

Table C.1
Effect of the FSD on Test Scores including Teacher Controls.

	Linear specification				Non parametric specification	
	FE2 (1)	FE2 (2)	FE-IV2 (3)	FE-IV2 (4)	FE-IV2 (5)	FE-IV2 (6)
A. Reading						
Years under FSD	0.011***	0.011***	0.024***	0.024***		
	(0.003)	(0.003)	(0.006)	(0.006)		
Years under FSD = 1					0.022	0.023
					(0.015)	(0.015)
Years under FSD = 2					0.030**	0.032**
					(0.014)	(0.014)
Years under FSD = 3					0.114***	0.115***
					(0.021)	(0.021)
First stage coefficient			0.720***	0.723***		
			(0.005)	(0.005)		
Kleibergen-Paap rk Wald F statistic			24416.41	24689.20	5265.46	5244.84
N. of students	596108	578112	596108	578112	596108	578112
B. Mathematics						
Years under FSD	0.005	0.004	0.008	0.009		
	(0.003)	(0.003)	(0.007)	(0.007)		
Years under FSD = 1					-0.015	-0.012
					(0.016)	(0.016)
Years under FSD = 2					-0.002	0.001
					(0.016)	(0.016)
Years under FSD = 3					0.058**	0.060***
					(0.023)	(0.023)
First stage coefficient			0.720***	0.723***		
			(0.005)	(0.005)		
Kleibergen-Paap rk Wald F statistic			24294.13	24506.65	5278.70	5253.62
N. of students	596281	578281	596281	578281	596281	578281
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher-level controls	No	Yes	No	Yes	No	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the effect of the FSD on reading and mathematics test scores. Estimates in columns (1), (3) and (5) are based on the specification with baseline controls. Student-level controls include: gender, age at school entry, as well as the attendance rate and the status (pass or repeat) at the end of grade 1. School-level controls include averages of the students' characteristics at the school level, as well as enrollment and average class size. Estimates in columns (2), (4) and (6) include also controls referring to teachers' characteristics when the students attend grade 1. Specifically, they are the share of female teachers, teachers' average age and the share of teachers with an education degree. The treatment in specifications FE2 is actual years of exposure to the FSD by the end of grade 4, while in specifications FE-IV2 is instrumented with the exposure a student would experience had she never transferred out of her first grade school. Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

Table C.2
Effect of the FSD on Test Scores - Robustness Checks.

	No infrastructure funds		PSS			
	Reading (1)	Mathematics (2)	Reading (3)	Mathematics (4)	Reading (5)	Mathematics (6)
Years under FSD	0.020**	-0.001	0.027***	0.011	0.026**	0.002
	(0.010)	(0.011)	(0.006)	(0.007)	(0.012)	(0.013)
Number of students	379449	379691	596020	596190	291057	291085
Kleibergen-Paap rk Wald F statistic	9202.99	9259.78	21528.75	21474.33	11287.84	11198.08
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the results from a set of specifications that check the robustness of the main estimates of the effects of the FSD on test scores. All specifications include school and year fixed effects, and actual 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. In columns (1) and (2), specification (2) is estimated on the sub-sample of pupils in the master sample who start first grade in schools that did not receive public funds for expanding their infrastructure. In columns (3) and (4) specification (2) is enriched with two additional controls, on top of those listed in the notes to Table 5: individual exposure to the Preferential Subsidy Scheme (PSS) policy by grade 4 and the average share of pupils benefiting from the PSS in the schools attended by the student in grades 1 to 4. In columns (5) and (6), specification (2) is estimated on the sub-sample of cohorts never exposed to the Preferential Subsidy Scheme (i.e. cohorts starting primary education between 2002 and 2004). Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

learning outcomes in the following years. Our estimates would then also capture the effects of other changes to the school environment.

The first potential confounder to check is infrastructure investment, as some schools had to expand their infrastructure prior to switching to a single-shift scheme. 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 pupils who started first grade in schools that did not receive public funds for expanding infrastructure. These establishments are unlikely to have made substantial changes to their facilities prior to lengthening the school day. Columns (1) and (2) of Appendix Table C.2 report estimates that are in a similar range as those coming from the full sample of schools. An additional year of exposure to the FSD raises reading test scores by 0.020σ. The effect on mathematics test scores is virtually 0. According to this exercise, infrastructure investment does not appear to be an important alternative driver of our estimates.

In 2008 Chile introduced a Preferential School Subsidy scheme (*Subvención Escolar Preferencial*, or PSS henceforth) which grants schools an additional subsidy for each disadvantaged student they cater to.[52] To check whether our estimates are also capturing the roll-out of the subsidy, we implement two exercises. First, we enrich specification (2) with controls for the individual exposure to the PSS scheme (i.e. the number of grades during which the student received the subsidy) by grade 4 and the average share of pupils benefiting from the PSS scheme in the schools attended by a student in grades 1 to 4. Second, we estimate specification (2) on the sub-sample of cohorts never exposed to the PSS (i.e. those starting primary education before 2005). In both cases, coefficients are similar to those coming from the main specification and, if anything, in the case of reading they are slightly larger (Table Appendix C.2, columns 3 and 5). The second exercise, where we restrict our attention to cohorts of students who were at most two years apart, provides further support to the assumption that the effect of the FSD we estimate is not confounded by other unobservable changes: major changes in students' or schools' characteristics are unlikely to take place in such a short period of time.

Appendix D. Heterogeneity by Students' Backgrounds

In this section we explore whether the effect of the FSD varies depending on the characteristics of the environment students are exposed to when they are not in school. We focus our analysis on the role of household resources, as reflected by parental education and the availability of books and ICT technologies at home.[53] We rely on this information to distinguish schoolchildren from a more privileged background from others.

Appendix Table Appendix D.1 shows that longer schedules appear not to benefit in a significant way pupils from more advantaged backgrounds. An additional year of exposure to the FSD does not raise by a statistically significant amount reading and mathematics scores for children living in households where at least one parent has some university education (columns 1 and 4), there are more than 50 books (columns 2 and 5), or both a computer and a connection to the Internet are available (columns 3 and 6). On the other hand, reading scores increase by a significant amount for pupils living in households where neither parent has any university education (0.022σ), there are at most 50 books at home (0.022σ), and either a computer or a connection to the Internet is not available (0.024σ).[54] Also mathematics scores increase by a larger amount, which however never becomes significantly different from 0. It has to be noted, however, that the documented difference, as captured by the interaction term $ExpFSD4 \times D$, although large in size, is not statistically significant. With this caveat concerning the precision of the estimates in mind, the analysis provides suggestive evidence that returns to an additional hour of instruction time tend to be larger for students who have fewer resources and opportunities available at home.

We also examine whether the FSD boosts academic resilience, which is the ability of students from worse off backgrounds to obtain good or even excellent learning outcomes. To this end, we estimate regression specification (2) using as dependent variable the probability of scoring above a given percentile of the test score distribution. Table Appendix D.2 reports the results of this exercise. The first four columns focus on reading scores, while the last four on mathematics scores. We restrict our attention to students who: have parents with no higher education (Panel A); live in households where there are no more than 10 books (Panel B); live in households where a computer or an Internet connection are not available. Consistently with the main results of the paper, the probability of scoring above percentiles 60, 70, 80 and 90 increases in reading, but not in mathematics. According to these results, the probability of being in the top 60 per cent of the distribution of reading scores increases by around 1 percentage point with each year of exposure to the FSD for disadvantaged individuals. The same figure when focusing on the top 90 per cent of the distribution is 0.4 percentage points.

Why do we find these results? A longer school day increases the time that pupils spend at school, where all students in the same class are exposed to the same input. At the same time, it may reduce the time that pupils spend in education-related activities at home, where the amount and quality of inputs available varies greatly. Therefore, it may benefits the most children who have fewer resources and opportunities available outside the school.

Drawing on information coming from the 2015 Chilean Time-Use Survey, Appendix Table Appendix D.3 shows that pupils from privileged backgrounds indeed receive more support outside of school. We restrict our attention to households where there is at least one child aged 5-18 and we divide them into two groups, depending on whether either the head of the household or the head's spouse has any university education ($U_{hh} = 1$) or not ($U_{hh} = 0$). In households where $U_{hh} = 1$, the percentage of heads of household and heads' spouses who declare that they help their children with their homework is 48 per cent, whereas this percentage drops to 33 per cent in households where $U_{hh} = 0$ (column 1). Summing up the minutes that they dedicate to helping with homework on a given day of the working week and on a given day at the weekend, there is a 14-minute difference in favor of households where $U_{hh} = 1$ (column 2). Assuming a uniform distribution of help across the days of the week, this would translate into a difference of around 50 minutes per week. It is also interesting to look at support by other providers, in the form of tutoring outside of school. 5 per cent of pupils aged 12-18 and living in households where $U_{hh} = 0$ receive some tutoring, as opposed to 12 per cent of students living in households where $U_{hh} = 1$ (column 3). In terms of minutes per day, the former receives tutoring for less than half the time than the latter (column 4).

Appendix Table Appendix D.4 further suggests that the introduction of the FSD likely reduces the need of helps from parents or tutors outside the school. It reports information about the frequency of mathematics homework from the teacher surveys administered alongside the SIMCE test in 2011, 2012 and 2013. The limited period for which this information is available does not allow us to study the evolution of homework's frequency around the adoption of the FSD in an event study framework such as the one in (4). Panel A considers all schools and shows that the frequency of homework is lower in schools with the FSD than in establishments without it. For example, the percentage of teachers assigning homework after every class is roughly 20 per cent in schools where the FSD is not in place, while it drops to about 12 per cent in schools that feature longer schedules. In panel B we restrict our attention to schools that had not adopted longer schedules by 2011 and we compare the frequency of homework between the years 2011 and 2013. In establishments that did not adopted the FSD in 2012 or 2013 (column 2), the frequency is very similar in the the two years. On the other

[52] The receipt of the subsidy is conditional upon schools developing a pedagogical plan that outlines how additional funds are used to improve learning outcomes and upon allowing for an external evaluation of the results achieved. See Santiago, Benavides, Danielson, Goe, and Nusche (2013) for more info.

[53] This information is drawn from parent surveys administered alongside the test. The non-response rate is similar across the variables and is around 15 per cent. This explains the smaller sample size.

[54] These figures are the sum of coefficients related to the main term $ExpFSD4$ and the interaction term $ExpFSD4 \times D$. They are significant at the 1 per cent level.

Table D.1
Heterogeneous Effects of the FSD on Test Scores by Students' Socio-economic Background.

	Reading			Mathematics		
	$D = 1(NoUni.)$	$D = 1(Books \leq 50)$	$D = 1(NoICT)$	$D = 1(NoUni.)$	$D = 1(Books \leq 50)$	$D = 1(NoICT)$
	(1)	(2)	(3)	(4)	(5)	(6)
ExpFSD4	0.007	0.012	0.012	-0.003	0.003	0.004
	(0.012)	(0.011)	(0.009)	(0.012)	(0.012)	(0.010)
ExpFSD4 × D	0.015	0.010	0.012	0.011	0.005	0.004
	(0.013)	(0.011)	(0.010)	(0.012)	(0.011)	(0.011)
Kleibergen-Paap rk Wald F statistic	2781.84	11502.69	5644.29	2797.02	11528.94	5643.12
N. of students	532970	529879	517249	534473	531369	518676
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effect of the FSD on reading and mathematics test scores by different measures of students' socio-economic background. Every column shows an enriched version of specification (2) where the treatment and all controls listed in the notes to Table 5, including school and year fixed effects, are also interacted with a dummy D, capturing a relevant dimension of heterogeneity. In columns (1) and (4) D takes value 1 if no parent in the household has some university education, and 0 otherwise. In columns (2) and (5) D takes value 1 if there are at most 50 books at home, and 0 otherwise. In columns (3) and (6) D takes value 1 if there is not a computer or an Internet connection at home, and 0 otherwise. Actual years of exposure to the FSD are instrumented with years of exposure a student would accumulate had she never transferred from the school where she attended first grade. Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

Table D.2
Effect of the FSD on Academic Resilience.

	Reading				Mathematics			
	Probability of being above percentile:							
	60	70	80	90	60	70	80	90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A - Parental Ed. ≤High School				
ExpFSD4	0.009***	0.009***	0.006***	0.004***	-0.000	0.003	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Outcome mean	0.332	0.235	0.146	0.066	0.327	0.230	0.141	0.062
Kleibergen-Paap rk Wald F statistic	22190.21	22190.21	22190.21	22190.21	22127.40	22127.40	22127.40	22127.40
N. of students	368292	368292	368292	368292	369415	369415	369415	369415
				Panel B - Books at Home ≤10				
ExpFSD4	0.008**	0.009***	0.006**	0.004***	-0.002	-0.000	0.000	0.001
	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)
Outcome mean	0.316	0.221	0.136	0.060	0.310	0.216	0.130	0.057
Kleibergen-Paap rk Wald F statistic	19312.03	19312.03	19312.03	19312.03	19420.26	19420.26	19420.26	19420.26
N. of students	232067	232067	232067	232067	232696	232696	232696	232696
				Panel C - No ICT at Home = 1				
Years under FSD	0.010***	0.010***	0.007***	0.004***	-0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Kleibergen-Paap rk Wald F statistic	22175.04	22175.04	22175.04	22175.04	22189.21	22189.21	22189.21	22189.21
Outcome mean	0.358	0.258	0.164	0.076	0.355	0.255	0.160	0.074
N. of students	360134	360134	360134	360134	361119	361119	361119	361119

Notes: The table reports the effect of the FSD on the probability of scoring above percentiles 60, 70, 80 or 90 of the distribution of reading and mathematics test scores, computed based on all test-takers in the same year. The sample is restricted to disadvantaged students who: have parents with no higher education (Panel A); live in households with at most 10 books (Panel B); live in households where the computer or a connection to the Internet are not available (Panel C). Student-level controls include: gender, age at school entry, as well as the attendance rate and the status (pass or repeat) at the end of grade 1. School-level controls include averages of the students' characteristics at the school level, as well as enrollment and average class size in first grade. In addition, all specifications include as controls school fixed effects and year fixed effects. Standard errors are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

hand, homework is assigned much less frequently in 2013 than in 2011 in establishments that switched to the FSD by that year (column 3).[55] Overall, there is therefore suggestive evidence that longer school schedules are associated with less homework. If the productivity of homework is higher for schoolchildren from advantaged backgrounds, because they have more support at home, the reduction of its frequency that seems to be associated with longer school schedules could be one of the mechanisms that explains the documented heterogeneity.

The findings discussed in this section are in line with the results of Lavy (2015).[56] If also confirmed in other settings, they would suggest that the amount of time spent at school may play a role in reducing inequality in learning opportunities. As pupils from different backgrounds are exposed to the same school inputs for a larger part of the day, the role of household inputs —the quality of which varies greatly— may become less important. This is likely to be especially true if, as in the Chilean setting, the additional instruction time does not entail an expansion of the curriculum. Indeed, in a

[55] As an example, in 2011 around 52 per cent of teachers working in schools that had not adopted the policy declared that they had assigned homework after almost every class. This figure remained the same in schools that had not adopted the FSD by 2013, while it fell to 31.82 per cent in schools that adopted it in 2012 or 2013.

[56] When restricting the analysis to a sub set of developing countries that include Chile, Lavy (2015) finds a stronger effect among schoolchildren from highly educated families. However, he does not provide country-specific estimates that allow to verify what is the estimated effect for Chile.

Table D.3
Support Received by Students Outside of School.

	Help with homework from household head and head' spouse		Tutoring	
	1 = Yes (1)	Hours (2)	1 = Yes (3)	Hours (4)
No university	0.328	0.462	0.050	0.123
University	0.484	0.717	0.119	0.280
Observations	6205	6205	2671	2671

Notes: The table shows the amount of support that students receive outside of school, depending on whether they live in a household where one among the household head and the head' spouse has some university education (row "University") or not (row "No university"). The units of observation are the households heads and their spouses (in households where these is at least one child aged 5-18) when the question is whether they provide help with homework. The units of observations are pupils aged 12-18 (younger children are not interviewed) when the question is whether they receive tutoring outside of school. Information is drawn from the 2015 Chilean Time-Use Survey (*Encuesta nacional sobre uso del tiempo*).

Table D.4
Frequency of Mathematics Homework.

	A. All schools		
	No FSD (1)	FSD (2)	(3)
Every class	20.04%	11.80%	
Almost every class	50.39%	39.35%	
Some classes	28.42%	46.33%	
Never	1.15%	2.51%	
N. of Teachers	3294	18494	
	B. Schools that had not adopted the FSD by 2011		
	2011	2013	
		No FSD	FSD
Every class	22.51%	18.81%	7.95%
Almost every class	51.88%	52.10%	31.82%
Some classes	24.50%	28.34%	59.09%
Never	1.11%	0.74%	1.14%
N. of Teachers	902	808	88

Notes: The table reports information about the frequency of mathematics homework, drawn from the 2011, 2012 and 2013 waves of the teacher surveys administered alongside the SIMCE test. Panel A compares the frequency of homework in schools with and without the FSD. Panel B focuses on schools that had not adopted the FSD by 2011 and compare homework frequency in 2011 and 2013. In 2013, schools are divided according to whether they switched to longer schedules by that year (column 3) or not (column 2).

setting in which increased weekly instruction hours are accompanied by an expansion of the curriculum, Huebener et al. (2017) document a widening gap between high- and low-performing German pupils.

Appendix E. An Alternative Instrumental Variable

Berthelon et al. (2016) study the effect of the FSD on early literacy skills of a cross-section of pupils taking the SIMCE test at the end of grade 2 in 2012. They estimate the following specification:

$$y_{ism} = \gamma' X_i + \beta' S_s + \rho FSD_{ism} + \varepsilon_{ism} \quad (E.1)$$

i indexes the student, s the school and m the municipality. FSD_{ism} is a dummy variable that takes value 1 if individual i from municipality m enrolls in school s offering the FSD. The instrument for attending a school offering the FSD is the share of schools offering the FSD in grades 1 and 2 in the municipality where the student lives, in the year before she/he enrolls in grade 1.

We adapt the instrument proposed by Berthelon et al. (2016) to our setting. Table Appendix E.1 reports the first stage coefficient from Berthelon et al. (2016), taken from Table 4 of their paper. Columns (2) and (3) present the coefficients that come from a similar specification estimated in our sample. As in our analysis, we focus on schools that had not adopted the FSD by 2002. We slightly modify regression specification (E.1) because we rely on a different sample: several cohorts of 4th grade students taking the SIMCE test in 2005-2013 rather than a cross-section of 2nd grade pupils being examined in 2012. Specifically, we use as the endogenous treatment variable a dummy that takes value 1 if the student attends a school where the FSD is in place in at least one grade between grade 1 and grade 4. The instrumental variable is the share of schools located in the municipality where the pupil lives that offer the FSD in at least one grade between grades 1 and 4. With respect to controls, we include year fixed effects and the few observable students' characteristics that are pre-determined with respect to the treatment (gender and age at school entry). In column (2) we also control for school-level average 4th grade reading and mathematics scores in the 2002 SIMCE test, while in column (3) we include school fixed effects.

Table E.1
First Stage of Berthelon et al. (2016) in our Setting.

	Berthelon et al. (2016)	Our setting	
		2002 Scores	School Fixed Effects
	(1)	(2)	(3)
FSD Available in Municipality = 1	0.41***	0.647***	0.025
	(0.04)	(0.041)	(0.024)
Students	99,211	865,432	1,024,852
F-Statistic	105.2	246.62	0.39
Grade 1 controls - students	Yes	Yes	Yes
Grade 1 controls - schools	Yes	No	No
Grade 2/4 controls - students	Yes	No	No
Grade 2/4 controls - schools	Yes	No	No
Lagged scores of the school	Yes	Yes	No
Current scores of the school	Yes	No	No
School fixed effects	No	No	Yes
Years fixed effects	No	Yes	Yes

Notes: Column (1) reports the first stage of Berthelon et al. (2016). Columns (2) and (3) present specifications that replicate their analysis in our setting. As in our preferred regression specification, our sample consists of schools that had not adopted the FSD by 2002. As in Berthelon et al. (2016), the endogenous treatment variable is a dummy indicating if students were exposed to the FSD before taking the test. The instrumental variable is the share of schools located in the municipality where the student lives that had adopted the FSD one year before the student enrolls in grade 1. Controls in our specifications are year fixed effects and students' gender and age in grade 1. In column (2) we also also control for school-level average reading and mathematics scores in the 2002 SIMCE test. In column (3) we also include school fixed effects among controls. Both specifications include year fixed effects. Standard errors clustered at grade 1 school level are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

Table E.2
Estimates of the Effect of the FSD on Reading and Mathematics Scores - Berthelon et al. (2016) IV.

	Reading		Mathematics	
	Avg. Effects	By Type of School	Avg. Effects	By Type of School
	(1)	(2)	(3)	(4)
FSD = 1	0.072**	0.127	-0.002	0.030
	(0.036)	(0.085)	(0.043)	(0.103)
FSD = 1 × Public School = 1		-0.049		-0.043
		(0.095)		(0.114)
First Stage	0.647***		0.647***	
	(0.041)		(0.041)	
Students	865,432	567,359	865,668	567,360
F-Statistic	246.62	25.94	246.50	25.85
Grade 1 controls - students	Yes	Yes	Yes	Yes
Scores of the school in 2002	Yes	Yes	Yes	Yes
Years fixed effects	Yes	Yes	Yes	Yes

Notes: This table reports results based on a specification that closely resembles that adopted by Berthelon et al. (2016). As in our preferred regression specification, our sample consists of schools that had not adopted the FSD by 2002. As in Berthelon et al. (2016), the endogenous treatment variable is a dummy indicating if students were exposed to the FSD before taking the test. The instrumental variable is the share of schools located in the municipality where the student lives that had adopted the FSD one year before the student enrolls in grade 1. Controls in our specifications are: year fixed effects, students' gender and age in grade 1, school-level average reading and mathematics scores in the 2002 SIMCE test. Standard errors clustered at grade 1 school level are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

The specification estimated in column (2) delivers a positive and statistically significant first stage coefficient, and the F statistics associated to the first stage equation is high. However, when we include school fixed effects the first stage coefficient becomes virtually 0 and is no longer statistically significant. This may cast doubt on the use of such an instrumental variable in our setting.

In Table Appendix E.2 we report the second stage coefficients coming from the regression specification that includes the controls used in column (2) of Table Appendix E.1. The results are consistent with the ones we present in the main body of the paper. Also this approach delivers the findings that the longer school day impacts are stronger on reading than on mathematics scores. In addition, although the difference is not statistically significant at conventional levels, the effect on reading scores in public schools is about one third lower than the effect we find in no-fee charter schools. In regards of mathematics scores, we also find a lower, yet imprecisely estimated, effect for students attending public schools. When comparing estimates across specifications, it is important to remember that these coefficients measure the effect of being exposed to the FSD, whereas the ones in our main regression specification measure the effect of an extra year of exposure to the FSD.

Appendix F. Additional Figures and Tables

Table F.1
Differences in School Autonomy between Public and Charter Schools.

	Public schools (1)	Charter schools (2)
Textbook use	98	100
Courses content	30	63
Courses offer	70	97
Formulate budget	18	96
Allocate budget	52	97
Hire teachers	28	98
Fire teachers	11	97
Set starting salaries	2	88
Increase salaries	2	91
Observations	62	85

Notes: The table reports the percentage of schools in which the principal or the governing body have a considerable responsibility over the listed tasks. Information comes from the 2006 and 2009 school surveys administered alongside PISA tests. The sample consists of all public or charter schools in the Chilean PISA sample that also offer primary education.

Table F.2
Teachers' Characteristics and Working Conditions in Public and Charter Schools.

	ln(wage)		Female	Age	Experience	Additional Benefits	Was Fired
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Charter	-0.160***	-0.036***	0.049***	-8.569***	-9.646***	0.093***	0.023***
	(0.012)	(0.012)	(0.011)	(0.257)	(0.305)	(0.011)	(0.006)
Constant	8.058***	7.873***	0.769***	49.025***	23.632***	0.148***	0.031***
	(0.014)	(0.051)	(0.014)	(0.291)	(0.352)	(0.012)	(0.006)
Experience	No	Yes	No	No	No	No	No
Education	No	Yes	No	No	No	No	No
Demographic characteristics	No	Yes	No	No	No	No	No
Years fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of observations	6263.00	6263.00	7623.00	7623.00	7623.00	. 7623.00	7623.00

Notes: The table shows how teachers' working conditions differ in private and charter schools, in terms of salary (columns 1 and 2) and other benefits (column 6), as well as job security (column 7). In columns 3-5 the dependent variables are teachers' observable characteristics: gender, age and experience. Charter is a dummy variable that takes value 1 if the teacher works in a charter school. The information comes from the Teacher Longitudinal Survey (*Encuesta Longitudinal Docente*), administered to a representative sample of teachers over the period 2005-2009.

Table F.3
Characteristics of Students in the Master Sample who do and do not Transfer.

	First grade Academic Performance		FSD Exposure	
	Attendance (1)	Repetition (2)	Real (3)	Potential (4)
Do not transfer between grades 1 and 4	94.64	0.02	0.52	0.51
Transfer between grades 1 and 4	93.44	0.05	1.35	0.42

Notes: Columns (1) and (2) show the average attendance rate in grade 1 for students in the master sample and the fraction of them who repeat grade 1, distinguishing pupils who never transfer between grades 1 and 4 from those who transfer. Columns (3) and (4) display their average actual exposure to the FSD by grade 4 as well their average "potential" exposure, i.e. the years of exposure a student would experience had she never transferred out of her first-grade school.

Table F.4
Heterogeneous Effects of the FSD on Test Scores by School Type.

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A - Reading			
ExpFSD4	0.019**	0.011	0.034*	0.030*	0.035*	0.037*
	(0.009)	(0.009)	(0.018)	(0.018)	(0.019)	(0.019)
ExpFSD4 × no-fee charter	0.042**	0.042**	0.046**	0.047**	0.041*	0.043*
	(0.019)	(0.019)	(0.019)	(0.020)	(0.024)	(0.024)
Kleibergen-Paap rk Wald F statistic	828.30	201.42	152.49	152.41	125.50	122.72
N. of students	377856	377856	319578	299155	297171	297171
			Panel B - Mathematics			
ExpFSD4	0.012	0.007	0.023	0.020	0.023	0.025
	(0.009)	(0.009)	(0.018)	(0.018)	(0.019)	(0.020)
ExpFSD4 × no-fee charter	0.017	0.019	0.019	0.025	0.021	0.017
	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)	(0.025)
Kleibergen-Paap rk Wald F statistic	839.28	202.98	151.59	146.85	121.80	121.19
N. of students	377719	377719	320452	299980	298573	298573
Interactions with students' characteristics (grade 1)	No	Yes	Yes	Yes	Yes	Yes
Interactions with students' characteristics (grade 4)	No	No	Yes	Yes	Yes	Yes
Interactions with schools' characteristics	No	No	No	Yes	Yes	Yes
Interactions with teachers' characteristics (grade 1)	No	No	No	No	Yes	Yes
Interactions with teachers' characteristics (grade 4)	No	No	No	No	No	Yes
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
School-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher-level controls	No	No	No	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effects of exposure to the FSD (ExpFSD4) on reading and mathematics test scores by the type of school that the student attended in grade 1. The sample is restricted to pupils who enrolled in public or no-fee charter schools. Estimates in column (1) are based on an enriched version of specification (2), where the treatment and all controls listed in the notes to Table 5, including year fixed effects, are also interacted with a dummy taking value 1 if the school is a charter establishment without tuition fees. In column (2) additional interactions between the treatment and students' characteristics observed in grade 1 are added. The characteristics are: gender, age, end-of-year status (pass or repeat) and the attendance rate, both at the individual level and averaged at the school level. Column (3) includes additional interactions between the treatment and students characteristics observed when they reach grade 4: a dummy that takes value 1 if no parent has university education, a dummy that takes value 1 if there are at most 50 books at home and a dummy that takes value 1 if there is no computer or Internet connection at home. These characteristics are also transformed in shares at the school level. Column (4) also includes interactions between the treatment and school characteristics observed when students were in grade 1 (class size, enrollment) or observed before our sample period (parental satisfaction with the school and its teachers in 2002; an index of school value added based on performance in the SIMCE in 2002). Columns (5) further includes interaction between the treatment and teachers' characteristics in the school where the pupil attends grade 1, observed when the student is in grade 1 (share of female teachers, average teachers' age and share of teachers with an education degree). Columns (6) further adds the interaction between the treatment and the same teachers' characteristics working in the first grade school, but observed when the student is in grade 4. Actual years of exposure to the FSD are instrumented with years of exposure a student would accumulate had she never transferred from the school where she attended grade 1. In columns (2) to (6) the sample is restricted to observations where the covariates have no missing value, to keep the sample size constant. Standard errors are clustered at the school level and are reported in parenthesis. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

Table F.5
Evolution of Teacher related Inputs relative to 1 Year before the FSD Adoption.

	Contract HH. (1)	Teaching HH. (2)	$\frac{ContractHH.}{N.ofClasses}$ (3)	$\frac{TeachingHH.}{N.ofClasses}$ (4)	N. of Teachers (5)	$\frac{ContractHH.}{N.ofTeachers}$ (6)	$\frac{TeachingHH.}{N.ofClasses}$ (7)
Event-year -5	6.507	8.355	-1.838***	-0.861*	0.239	-0.285	-0.058
	(9.269)	(8.588)	(0.710)	(0.489)	(0.289)	(0.253)	(0.266)
Event-year -4	6.712	8.316	-1.198*	-0.481	0.222	-0.309	-0.209
	(8.060)	(7.596)	(0.622)	(0.396)	(0.257)	(0.209)	(0.223)
Event-year -3	-3.556	-1.857	-1.058***	-0.850***	-0.115	-0.262	-0.187
	(6.566)	(6.063)	(0.360)	(0.325)	(0.214)	(0.165)	(0.198)
Event-year -2	1.630	1.301	-0.539**	-0.451*	0.073	-0.085	-0.072
	(4.568)	(4.302)	(0.252)	(0.242)	(0.150)	(0.115)	(0.137)
Event-year 0	25.898***	26.016***	2.570***	2.222***	0.297*	0.921***	0.871***
	(5.359)	(5.119)	(0.334)	(0.280)	(0.160)	(0.139)	(0.149)
Event-year 1	69.697***	65.191***	5.473***	5.148***	0.832***	2.567***	2.325***
	(6.157)	(5.886)	(0.342)	(0.318)	(0.186)	(0.161)	(0.180)
Event-year 2	77.471***	72.651***	6.323***	5.891***	0.951***	2.912***	2.636***
	(6.671)	(6.254)	(0.375)	(0.340)	(0.199)	(0.192)	(0.212)
Event-year 3	75.693***	72.580***	6.638***	6.289***	0.777***	3.339***	3.102***
	(7.754)	(7.170)	(0.437)	(0.392)	(0.235)	(0.226)	(0.245)
Event-year 4	80.204***	76.055***	7.345***	6.837***	0.815***	3.674***	3.303***
	(8.578)	(7.968)	(0.507)	(0.456)	(0.261)	(0.264)	(0.284)
Event-year -5 × no-fee charter	-21.642	-20.688	-0.014	-1.378	-0.490	0.060	-0.640
	(19.645)	(16.428)	(1.369)	(1.127)	(0.624)	(0.598)	(0.621)
Event-year -4 × no-fee charter	-17.450	-18.870	0.019	-0.933	-0.118	0.066	-0.384
	(14.774)	(12.779)	(1.046)	(0.865)	(0.478)	(0.473)	(0.550)
Event-year -3 × no-fee charter	-5.987	-3.150	0.213	0.244	0.184	0.229	0.320

(continued on next page)

Table F.5 (*continued*)

	Contract HH. (1)	Teaching HH. (2)	$\frac{ContractHH.}{N.ofClasses}$ (3)	$\frac{TeachingHH.}{N.ofClasses}$ (4)	N. of Teachers (5)	$\frac{ContractHH.}{N.ofTeachers}$ (6)	$\frac{TeachingHH.}{N.ofClasses}$ (7)
	(11.892)	(11.005)	(0.861)	(0.777)	(0.411)	(0.415)	(0.450)
Event-year -2 × no-fee charter	-20.465**	-20.247**	-0.978	-0.876	-0.543*	0.167	0.119
	(9.075)	(8.880)	(0.624)	(0.580)	(0.316)	(0.354)	(0.397)
Event-year 0 × no-fee charter	-11.967	-9.823	-0.261	0.359	0.077	-0.754**	-0.370
	(10.717)	(9.832)	(0.778)	(0.714)	(0.328)	(0.338)	(0.353)
Event-year 1 × no-fee charter	-12.675	-11.155	-1.068	-0.645	0.654	-1.804***	-1.331***
	(13.391)	(11.319)	(0.798)	(0.736)	(0.415)	(0.379)	(0.413)
Event-year 2 × no-fee charter	0.834	-1.541	-0.223	-0.166	1.058**	-1.553***	-1.221**
	(13.714)	(11.724)	(0.826)	(0.791)	(0.426)	(0.435)	(0.488)
Event-year 3 × no-fee charter	19.009	14.212	0.236	0.174	1.549***	-1.475***	-1.147**
	(15.902)	(14.086)	(0.961)	(0.877)	(0.493)	(0.489)	(0.536)
Event-year 4 × no-fee charter	45.435**	40.769***	1.375	1.244	2.308***	-1.464***	-1.122**
	(17.756)	(15.615)	(1.065)	(0.973)	(0.547)	(0.536)	(0.571)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of school-years	18765	18765	18538	18538	18765	18765	18765

Notes: The table reports coefficients from a richer version of the event study specification outlined in (4) where calendar year fixed effects and event years are also interacted with a dummy D_s taking value 1 if the school is a no-fee charter school, and 0 otherwise. The FSD is adopted in event-year 0 and coefficients show how different total contract hours, teaching hours, contract hours per class, teaching hour per class, total number of teachers, contract hours per teacher and teaching hours per teacher are in event-year ρ relative to event-year -1, which is taken as the reference year. The sample consists of all schools where students in the master sample enrolled in first grade. All specifications include school and year fixed effects. Standard errors are clustered at the school level. *, ** and *** denote significance at the 10, 5 and 1 per cent level, respectively.

Table F.6
Teachers opinion about the FSD.

	Public Schools (1)	Charter Schools (2)
Good or very good	44.99%	54.44%
Not bad, not good	32.99%	29.89%
Bad or very bad	22.02%	15.67%

Notes: The table reports the opinion of teachers about the FSD, dividing them according to the school (public or charter) in which they teach. Information is drawn from the 2005 wave of the *Encuesta Longitudinal Docente* implemented by the *Centro de Microdatos* of the *Universidad de Chile*.

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