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ABSTRACT

Exploring the impact of reducing instruction time has become a fundamental topic in education, particularly following the covid-19 outbreak. In this paper we consider an unexpected regulation change affecting the academic calendar of non-fee paying schools in the Madrid region (Spain) during the 2017-2018 school year. The availability of standard cognitive tests allows us to estimate the impact of this measure on academic performance by means of a difference in difference regression. Although this regulation change affected just two weeks of the school calendar, we found that it contributed to a significant deterioration of academic performance which was particularly evident in Spanish and English. We further explore non-linear (quantile) effects across the distribution of scores in the standardized exam finding that the disruption of the new normative affected more to students in the upper quartile of the distribution. Overall, we found a reduction of the gap across students in non-fee schools together with an increase of the gap between non-fee and fee paying schools. We discuss the implications of these results for a better understanding of the impact of school closures due to covid-19.

Keywords: *Instruction time, difference in difference, quantile regression, academic performance.*

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1. Introduction

Education is considered one of the fundamental factors of economic growth and social progress and instruction time the most obvious input in the production of education. Therefore, this issue has attracted the attention of researchers even in the years before the covid-19 outbreak (Lavy, 2015; Cattaneo et al. 2016; Heubener et al., 2017 and Aucejo and Romano, 2016). This interest has been multiplied with the emergency of the covid-19 crisis, which has led many countries to implement lockdown policies with drastic alterations of instruction time in many cases (DELVE Initiative, 2020; Burgess, 2020; Burgess and Sievertsen, 2020; Santibanez and Guarino, 2020; Kuhfeld et al., 2020; and Goulas and Magalokonomou, 2020). However, a precise estimate of its consequences on educational output will be only possible with the benefit of hindsight once international databases containing post covid-19 information will be available. Even then, in many cases it will be difficult to appraise the specific contribution of the decision to close school doors on academic performance as this information will be contaminated by changes in other social and economic variables related to covid-19 such as income and health.

An important difficulty in the identification of the impact of instruction time on academic performance is the fact that students' families, subject to regulatory constraints, self-select the total number of instructions hours (including extracurricular activities) they need to achieve a given academic outcome. Many papers have circumvented this issue by studying modifications of instruction time due to political decisions (Pischke , 2007, Bellei, 2009, Huebener et al, 2017). However, even if policy interventions were randomly allocated across schools, they are typically announced and discussed in advance by the media. Therefore, the implementation of these policies and their consequences could be largely anticipated by rational schools and students before making relevant decisions.

In this paper we consider an unexpected regulation change affecting 7th to 10th grade students in the Madrid region (Spain) during the 2017/18 school year. In particular, the new normative changed the ordinary final exam date from late to early June reducing the total instruction time of students who passed these exams by around two weeks. Thus, failed students received focused classes to pass resit exams during these two weeks while passed students did not received any formal teaching but, instead, they were involved in different cultural activities. The new regulation affected only public[1] and charter schools but not private schools. We use the fact that this region conducted standard cognitive tests in 10th grade students before and after the regulation change for schools affected and not affected by the new regulation to implement a difference in difference analysis. Despite the small size change in the school calendar, just two weeks, we found that this measure contributed to a significant deterioration of academic performance which was particularly evident in Spanish and English. In an extended analysis we explore non-linear (quantile) effects across the distribution of scores in the standardized exam finding that the disruption of the new normative affected more to the students in upper quartile of the distribution. Overall, we found a reduction of the gap across students in non-fee schools together with an increase of the gap between non-fee and fee paying schools.

This paper proceeds as follows. The next section discusses the related literature on the importance of instruction time. Section 3 describes the new normative. Section 4 presents the data considered in the analysis. The empirical strategy employed is discussed in Section 5, while Section 6 presents and discusses empirical results. The last section concludes.

2. Related Literature

It is remarkable that one of the most obvious investment in education, increasing instruction time, has not traditionally attracted the attention of early researchers. Goodman (2014), by quoting Berliner (1990), suggested this disinterest could be a consequence of the fact that instructional time has been deemed as an obvious and conceptually unexciting variable in

[1] Hereafter the term public school refers to state-owned non-fee schools.

determining educational output. An analysis of the impact of instruction time on academic performance can be only found in early works by Grogger (1996) and Eide and Showalter (1998) who observed an insignificant effect of the length of the school year in the US on academic outcome. Card and Krueger (1992) analysed the same issue considering state-level data in the US. They found a positive significant effect on earnings, although this effect vanished once they control for quality school variables.

However, it is the arrival of individual databases, especially the Programme for International Student Assessment (PISA), one of the main propellers of the recent burgeoning of this literature. Thus, Lavy (2012, 2015), Rivkin and Schiman (2015) and Cattaneo et al. (2017) achieve identification using the variation of instruction time in different subjects by the same student. This is at the cost of obtaining a single estimation of the causal effect for all the possible subjects. Alternative identification approaches include, for example, the use of exogenous political shocks (Pischke , 2007, Bellei, 2009, Huebener et al, 2017), natural experiments (Fitzpatrick et al., 2011 and Aucejo and Romano, 2016), instrumental variables (Goodman, 2014) and laboratory experiments (Banerjee et al. 2007).

A general conclusion of all this literature is that instruction time exerts a positive and significant effect, but of small magnitude, on educational outcome. When looking at the impact on score tests, papers that identify causal effects by subject provide inconclusive results. Thus, for example, while Aucejo and Romano (2011) found that instruction time exerts a higher impact on math compared to reading, results are not significantly different in Bellei (2009). Some of these papers also explore differences by ability which has a clear political relevance to apprise the contribution of instruction time to reduce differences in students' performances. However, results are also mixing in this respect. For example, Huebener et al (2017) found more sensitivity in low-performing students while the opposite is found in Aucejo and Romano (2016).

One of the most critical dimensions of these analyses regard the importance of the quality of instruction time. Banerjee et al. (2007) provide an interesting discussion on this issue. They explain that additional inputs in education will not affect educational performance unless they address specific unmet needs at the school. In their experiment, they found that specific programmes for remedial education and computer assisted learning exert a significant but not highly persistent effect on students' outcome. However, despite the importance of quality variables, they can only be imperfectly observed in most cases. For example, Rivkin and Schiman, (2015) use classroom environment as a proxy finding that the effect of instruction time on academic performance is higher the higher the quality of this instruction.

The interest in the impact of instruction time on education has been further intensified with the covid-19 outbreak which has produced unprecedented learning interruptions. Thus, recent papers have used these types of analyses to discuss the potential impact of covid-19 school closures (Santibanez and Guarino, 2020; Kuhfeld et al., 2020; and Goulas and Magalokonomou, 2020). In particular, Santibanez and Guarino (2020) estimate the effect of absenteeism on cognitive and social-emotional outcomes in the state of California while Kuhfeld et al. (2020) analyse the impact of missing schools due to absenteeism, regular summer breaks and school closures on learning outcomes in the US. These two papers found a significant effect of missing schools on learning outcomes. Goulas and Magalokonomou (2020) focus on the Swine flu pandemic in Greece. As a consequence of this, the Greek government eased its school attendance policy. Similar to our paper, the effect of this policy was not homogeneous across students affecting more those with higher prior performance, who took more absences while students with lower prior performance kept going to the school. They conducted a regression analysis to show a negative correlation between attendance and performance.

We contribute to all this literature by considering one of the few examples of a natural experiment that consists in an exogenous alteration of instruction time. More specifically, the new regulation implied a different treatment to failed students in non fee-paying and private schools. In the former (treatment group), students who must take resit exams, receive specific instruction for this exam during 15 additional days. This is provided by the teachers who are going to evaluate them. However, higher-performance state-school students, who already passed the exam, use this time for social and cultural activities. Private schools (control group) were unaffected by this measure.

Given that all students in the Madrid region must take a standard test in the following year, a difference in difference approach can be used to estimate the specific effect of this policy. In principle, we should not expect this political measure to have a significant impact on academic outcome. This is because the variation of the number of instruction days is very small compared to the literature discussed above, which already found effects of small magnitude. Moreover, the political shock affected both students regardless of whether or not they have to take the resit exam. However, a distinctive feature of this variation in instruction time is that is aiming to address specific unmet needs by students, who are supposed to be especially interested in receiving help to pass their resit exams.

3. Background

National and regional governments steer educational policies in Spain. Although the overall framework and guidelines are defined at the national level, most schooling decisions as well as funding are determined at the regional level. There are three main school categories: state-funded, charter and private schools. While the former two are funded by the state for compulsory education, the latter is privately funded. Here, we focus on students in 9th grade (around 14-15 years old) in the Madrid region as many of them were affected by the change in the school calendar we analyse. Moreover, they participated in the external and standardised test the following academic year; that is in 10th grade (final year of compulsory secondary school).

In the 2017/18 school year the Community of Madrid changed the timing for the extraordinary final evaluation for first time. This is a resit exam for those students who did not pass the ordinary final exam of the subject on the first attempt. Prior to this year, the final and the extraordinary exams took place in late June and September respectively. The aim of this change was to improve academic results, advance planning of the start of the course, more facilities for the reconciliation of family life during the summer period and the reduction of family spending intended for recovery activities, etc (Madrid, Regional Ministry of Education, 2018). Importantly, bringing forward the extraordinary final exams from September to the third week of June implied that the ordinary final exams were taken in the first week of June, two weeks earlier than in the previous academic year. During these two weeks, students with pending subjects attended regular classes with a lower student-to-teacher ratio, getting greater individual attention, support activities, reinforcement and tutoring. In contrast, students who passed all subjects in the ordinary final evaluation attended other types of cultural activities such as, for example, visits to museums, social volunteering activities or debate tournaments. All students, regardless of whether or not they have passed their exams, should continue attending school, as it is established that the school calendar must involve a minimum of 175 school days for compulsory education (fifth additional provision of the Educational Organic Law, LOE).

The modification of the extraordinary final evaluation from September 2018 to June 2018 was decided on 27 June 2017 (Madrid Regional Ministry of Education, 2018), when the previous course had already finished. Many schools did not realise of the calendar change until May 2018. The first evaluation of this measure by the Madrid Regional Education Council supports this conclusion. Firstly, attendance by students who passed all subjects from the first to third week of June was around 50%, since they did not want to participate in such activities. In fact, the Madrid Regional Education Council (2018) approved a Recommendation after this change pointing out that "Incentives to attend classes for all students should be improved, including those who have obtained positive grades in the ordinary final exams." Moreover, the Madrid Regional Education Council (2018) continued with the first evaluation of the measure suggesting some improvements for the next school year explicitly stating "What incentives will have students who have succeeded in all subjects with positive evaluation in the ordinary final call?". Secondly, in the first evaluation of the calendar change, the Madrid Regional Education Council concluded that the information about this new school calendar should reach the entire educational community in the 2018/19 school year. In fact, this information did not reach the entire community as the Madrid Regional Education Council suggested by asking “What actions will be carried out to ensure that the information of the new school calendar reaches the schools?”. Moreover, its report proposed some possible actions to follow such as “Information panels to the centres; institutional advertising; workshops especially aimed at families.” The document concluded that among the “Improvement Aspects” of the change in the calendar it should include “Attendance of students who passed all subjects in the ordinary exam” and “Information to families”. The absenteeism of students who have passed the ordinary exams in the last two weeks in the Region of Madrid was intensely covered by the national press[2].

4. Data

4.1. External exam

Our aim is to estimate the impact of the change in school calendar on students’ performance in the Madrid region. For this purpose we have an objective and independent measure of academic performance. Since the 2015/16 school year, the regional government of Madrid has conducted a standardized external exam for all 3rd and 6th grade students in the region. One year later, in 2016/17 the region extended this standardized external exam to students in 10th grade, who are in the final year of compulsory secondary school (15-16 years old). Here, we focus on the outcome of 10th grade exams as they include students affected by the change in the school calendar the previous academic year The exam measures basic knowledge in four competences: Spanish (*SPA*); foreign language, ,English, (*ENG*) ; maths (*MTH*) and history and geography (*H&G*). The results of this exam do not have academic consequences for students beyond the information they provide to the school (Anghel et al, 2016) but they are just used for a diagnostic purpose. Therefore, each student’s result is recorded in a report to be delivered to

2 Spanish newspapers explicitly stated “Early end of the year for Secondary students who have not failed”or “Generalized absenteeism after the advancement of the exams from September to June”
https://elpais.com/ccaa/2018/06/14/madrid/1528976557_605054.html
https://www.elmundo.es/madrid/2019/06/14/5d021b8cfc6c837e218b462b.html

his/her parents. This is informative for schools as well as teachers, parents, and students themselves. Furthermore, since the exams are compulsory for all students, our analysis is not just based on a sample but on the whole census of 10th grade students in the Madrid region.

Each of the four competence tests (mathematics, Spanish, foreign language, and history and geography) lasts 60 minutes. The exams take place in two consecutive days. In addition, all the principals, teachers of the subjects involved, families and students complete the context questionnaires in the days before and after the exam. These questionnaires provide information on the socio-economic and cultural conditions of the centres for the contextualization of the results obtained.

4.2. Description of the variables

The scores obtained in each of the four competences described above represent the dependent variable in our analysis. Like in the Programme for International Student Assessment (PISA) dataset, all scores have been standardized to have mean and standard deviation values equal to 500 and 100 respectively. There are also marginal differences in the number of observations for the different exams for reasons such as, for example, illness or not arriving on time to the exam. We provide specific estimation results for each competence. However, because our main interest is on the overall student performance, rather that specific competences, we also consider a principal component analysis in order to reduce the dimension of the problem. When this methodology is applied to our data, we found that the first principal component explained slightly over 60% of the variability of the four variables. Moreover, all the weights given to the different variables were positive and of a similar magnitude (around 0.5). This allows us to interpret this combination as an average of the score obtained in the four competences. We denote this combination by *pc1*.

Following the survey by Hanushek and Woessmann (2011) on the determinants of student achievement, we consider three groups of control variables in the analysis. The first one contains innate student's characteristics. In particular, we include in this group dichotomous variables that account for gender, immigrant and birth month. The second group of variables includes student's family background such as different indicators of the number of books at home, father's and mother's profession as well as different parent educational attainment levels. The third group of variables reflects institutional aspects such as tracking and repetition (captured by whether the student followed a strong math academic course or a more applied math course or if he/she has repeated in his/her primary and secondary education), age and attendance to 0-2 years early pre-primary education.

Using this information, we define the treatment and the control groups to be used in the difference in difference analysis in the following way. The treatment group before treatment contains 10th grade public and charter school students who took the Regional exam in the 2016/17 academic year. Students in the same types of schools who took the exam in 2018/19 belong to the treatment group after treatment. Likewise, the control groups before and after treatment are formed by students from private schools in the same periods.

The exclusion of the 2017/18 school year in our baseline analysis is because in that year the code used in the school, family level database is different to the code used in the student level database, and therefore the information for schools, families and students cannot be linked for all the control variables used in the analysis. In particular, in the 2017/18 school year, apart from the test scores, the only available information in is that provided by the students. Thus, we lose some variables such as students' age and birth month. We know if the student has repeated grades but not if the retention took place in the primary or secondary level. Furthermore, we only have very vague information on father's and mother's occupations that only indicates whether they are fully employed, partially employed, unemployed or inactive. However, as it will be shown later, we will study the robustness of our results to the inclusion of the 2017/18 exam information.

Table 1 shows descriptive statistics of all variables for the treatment and the control groups before and after the normative change. A description of these variables can be found in the Appendix. We can observe better academic performance in all competences for private schools compared to public and charter schools. However, students in these two types of schools are different in a number of individual and social characteristics. For example, a higher proportion of students in private schools attended 0-2 years early preliminary education. Immigrant population is lower in private schools. Moreover, on average, families of students in private schools have more books at home, wealthier occupations and a higher educational attainment. All these differences suggest that a proper analysis requires to control for these characteristics as well by the different schools.

Table 1: Descriptive Statistics

	Before treatment						After treatment					
	Public&Charter			Private			Public &Charter			Private		
	Obser	Mean	Std.Dev	Obser	Mean	Std.Dev	Obser	Mean	Std.Dev	Obser	Mean	Std.Dev
PC1	33,360	-0.13	1.50	3,376	1.10	1.45	37,352	-0.16	1.56	3,947	1.11	1.56
SPA	35,064	492.85	104.65	3,470	539.73	73.13	38,758	493.37	98.79	3,993	543.83	97.06
ENG	35,085	488.29	100.33	3,430	573.96	77.12	39,758	490.92	98.75	4,020	572.31	85.32
H&G	34,132	491.43	100.31	3,455	553.91	103.44	39,001	493.04	98.74	3,999	553.15	96.62
MTH	34,265	492.47	97.79	3,456	551.49	114.16	40,016	493.72	97.86	4,033	554.05	105.35
Gender	42,362	0.50	0.50	3,593	0.47	0.50	51,483	0.50	0.50	4,168	0.46	0.50
Immigrant	24,849	0.14	0.34	2,499	0.06	0.23	51,467	0.12	0.32	4,168	0.03	0.17
Pre-primary education	24,881	0.54	0.50	2,498	0.66	0.47	28,492	0.57	0.49	2,919	0.70	0.46
Age	24,992	16.13	0.67	2,508	15.89	0.45	28,616	16.14	0.68	2,939	15.89	0.44
January	25,000	0.08	0.27	2,510	0.08	0.27	28,630	0.08	0.27	2,940	0.07	0.26
February	25,000	0.07	0.26	2,510	0.07	0.26	28,630	0.08	0.26	2,940	0.08	0.27
March	25,000	0.08	0.28	2,510	0.10	0.30	28,630	0.08	0.27	2,940	0.09	0.28
April	25,000	0.08	0.28	2,510	0.09	0.28	28,630	0.09	0.28	2,940	0.10	0.30
May	25,000	0.09	0.29	2,510	0.09	0.29	28,630	0.09	0.29	2,940	0.09	0.29
June	25,000	0.08	0.27	2,510	0.08	0.27	28,630	0.08	0.27	2,940	0.09	0.28
July	25,000	0.09	0.28	2,510	0.08	0.27	28,630	0.09	0.28	2,940	0.08	0.27
August	25,000	0.08	0.28	2,510	0.07	0.26	28,630	0.08	0.28	2,940	0.08	0.27
September	25,000	0.08	0.27	2,510	0.08	0.28	28,630	0.08	0.28	2,940	0.08	0.27
October	25,000	0.09	0.28	2,510	0.09	0.29	28,630	0.08	0.28	2,940	0.08	0.28
November	25,000	0.09	0.28	2,510	0.08	0.28	28,630	0.08	0.28	2,940	0.07	0.26
December	25,000	0.08	0.27	2,510	0.08	0.27	28,630	0.08	0.28	2,940	0.09	0.28
Books-0-10	24,914	0.06	0.24	2,500	0.01	0.10	28,535	0.08	0.27	2,932	0.02	0.14

Books-11-50	24,914	0.22	0.41	2,500	0.08	0.27	28,535	0.24	0.43	2,932	0.10	0.30
Books-51-100	24,914	0.25	0.43	2,500	0.19	0.39	28,535	0.26	0.44	2,932	0.21	0.40
Books-101-200	24,914	0.21	0.41	2,500	0.26	0.44	28,535	0.20	0.40	2,932	0.26	0.44
Books>200	24,914	0.26	0.44	2,500	0.46	0.50	28,535	0.22	0.42	2,932	0.41	0.49
Mother no occupied	24,093	0.03	0.16	2,461	0.02	0.14	26,992	0.03	0.17	2,848	0.02	0.13
Mother elementary occupations	24,093	0.17	0.38	2,461	0.02	0.16	26,992	0.10	0.30	2,848	0.01	0.10
Mother craft & related trades workers	24,093	0.02	0.14	2,461	0.00	0.06	26,992	0.02	0.14	2,848	0.01	0.09
Mother skilled agricultural & forestry	24,093	0.00	0.06	2,461	0.00	0.03	26,992	0.00	0.06	2,848	0.00	0.05
Mother plant & machine operators	24,093	0.02	0.13	2,461	0.00	0.05	26,992	0.02	0.13	2,848	0.01	0.07
Mother retail and & services & personal care	24,093	0.05	0.22	2,461	0.10	0.30	26,992	0.13	0.33	2,848	0.07	0.25
Mother armed forces. Protection & security	24,093	0.07	0.25	2,461	0.03	0.18	26,992	0.00	0.05	2,848	0.00	0.07
Mother clerical support workers	24,093	0.19	0.40	2,461	0.18	0.38	26,992	0.27	0.44	2,848	0.31	0.46
Mother technicians & professionals.	24,093	0.15	0.36	2,461	0.31	0.46	26,992	0.03	0.18	2,848	0.06	0.24
Father no occupied	23,566	0.01	0.08	2,443	0.00	0.04	26,746	0.01	0.09	2,854	0.00	0.06
Father elementary occupations	23,566	0.05	0.22	2,443	0.01	0.08	26,746	0.08	0.27	2,854	0.00	0.06
Father craft & related trades workers	23,566	0.15	0.35	2,443	0.02	0.13	26,746	0.10	0.31	2,854	0.02	0.15
Father skilled agricultural & forestry	23,566	0.01	0.10	2,443	0.00	0.06	26,746	0.01	0.11	2,854	0.01	0.08
Father plant & machine operators	23,566	0.08	0.28	2,443	0.01	0.11	26,746	0.11	0.31	2,854	0.02	0.14
Father retail and & services & personal care	23,566	0.09	0.28	2,443	0.12	0.32	26,746	0.03	0.16	2,854	0.03	0.16
Father armed forces. Protection & security	23,566	0.07	0.25	2,443	0.03	0.18	26,746	0.01	0.11	2,854	0.02	0.14
Father clerical support workers	23,566	0.07	0.26	2,443	0.06	0.24	26,746	0.16	0.36	2,854	0.20	0.40
Father technicians & professionals.	23,566	0.16	0.37	2,443	0.31	0.46	26,746	0.07	0.25	2,854	0.09	0.28
Parents-education1	43,097	0.09	0.29	3,612	0.02	0.14	52,244	0.08	0.28	4,188	0.02	0.14
Parents-education2	43,097	0.21	0.41	3,612	0.09	0.28	52,244	0.20	0.40	4,188	0.09	0.28
Parents-education3	43,097	0.26	0.44	3,612	0.57	0.50	52,244	0.24	0.43	4,188	0.57	0.50
Repeat in secondary once	22,691	0.08	0.27	2,446	0.02	0.15	25,944	0.08	0.27	2,862	0.02	0.15

Repeat in secondary more than once	22,691	0.00	0.06	2,446	0.00	0.03	25,944	0.00	0.07	2,862	0.00	0.03
Repeat in primary once	24,358	0.17	0.38	2,471	0.05	0.21	27,859	0.17	0.38	2,898	0.04	0.21
Repeat in primary more than once	24,358	0.04	0.20	2,471	0.01	0.09	27,859	0.04	0.20	2,898	0.01	0.08

5. Methodology

We use a difference in difference econometric methodology to investigate the impact of the calendar change described in Section 3 on students' performance in the Madrid region. More specifically, we estimate whether the change of school calendar in 2017/18 significantly impacted academic results of students in public and charter schools (affected by this measure) relative to students in private schools, who were not concerned by that change. Our response variable, $Y_{i,j,t}$, represents the relevant score in the standardized exam by student i, school j and academic year t. In our baseline specification, we consider a least square estimation of the following model

$$Y_{i,j,t} = \beta_0 + \beta_1 D_{PC} + \beta_2 T_{2018/19} + \beta_3 (D_{PC} * T_{2018/19}) + \sum_{k=1}^{K} \gamma_k X_{i,j,t} + \eta_j + \varepsilon_{i,j,t}, \quad (1)$$

where D_{PC} is a dummy variable that takes value 1 for schools affected by the normative change, i.e. public and charter schools, and zero otherwise; $T_{2018/19}$ takes values 1 if the observation belongs to the 2018/19 academic year and zero otherwise; $X_{i,j,t}$ are observed individual characteristics; β_0 to β_3 and γ_k for $k = 1$ to K are parameters to be estimated; η_j is a school fixed effect; and $\varepsilon_{i,j,t}$ is an error component.

This analysis is carried out by considering four different response variables that correspond to score in mathematics, Spanish, foreign language and history and geography as well as the composite outcome index described in the previous section (*pc1*). In all cases, our focus parameter is β_3 which measures the impact of the normative change on student's performance. The hypothesis is that, in general, students will be negatively affected by a measure which reduces instruction time for most of them. However, the effect of this policy is not homogeneous across students as it is likely to affect more negatively to those who do not have to resit any exam. Therefore, in an extended analysis, we explore non-linear (quantile) effects across the distribution of scores in the standardized exam (Battistin and Meroni, 2016 ; Huebener et al, 2017). By doing this, we model treatment intensity across the score distribution. More specifically, we hypothesise that treatment students at the bottom of the performance distribution will not be so negatively affected by the policy as those in the top of the distribution. A possible reason is that some of the students who must take resit exams receive classes to prepare them. Thus, we know that before the calendar change in the 2016/17 school year, 43.6% of students passed the ordinary final exams and an additional 40.4% passed the extraordinary final exams, making a total of 84.0%. After the normative change, there was an increase in the number of students who passed the 10^{th} grade (up to 85.0% in the 2017/18 school year)

6. Analysis of results

6.1. Control variables

Estimation of model (1) in the previous section involves the inclusion of many covariates that are not the main foci of our analysis. However, this information is interesting by itself as they inform of the determinants of students' academic scores. Table 2 shows the estimated impact of these control variables on scores in each competence as well as the general index *pc1* under our baseline specification (control school year 2016/17). The table also reports relevant information on the econometric specifications, such as the inclusion of school fixed effects. Generally, the effects of control variables are in line with the previous literature. More specifically, there is a small negative effect for girls because their positive relative results in Spanish and English are more than offset with the negative relative results in Math and Geography and History. The results by gender in Spanish and Math, competences also assessed by the OECD in PISA, are in line with the results found in international tests (OECD, 2019). Students who have attended early school from 0-2 years score significantly better, which is consistent with previous papers on the importance of early years in the acquisition of both cognitive and non-cognitive skills (Felfe et al., 2015 and Heckman et al. 2010). Those students that have repeated, either in primary or in previous course of secondary, have a significant worse performance as has been pointed out by PISA (PISA, 2018). Even when including retention, age has a significant and negative effect which can be explained as some students might be in a lagged academic course than their corresponding age without having being retained (if they delay school entry or if they come from other education systems). We also find evidence of a relative age effect as, ceteris paribus, students born in the first months of the year tend to perform better than those born at the end of the year (Dhuey and Bedard, 2006 and Berniell and Estrada 2020). It can be reported a positive monotonic relationship between the number of books at home and the student outcome. Hanushek and Woessmann (2011) show that number of books in the students' home is used as a proxy for socio-economic background finding that the number of books at home is the most correlated variable with academic achievement. Parents professions also matter. For example, being technicians, associate professionals, professionals, chief executives and senior officials increase the expected students' outcome. In contrast, some mothers' or parents' professions are significantly and negatively correlated with students' output. This is the case of never been occupied, have elementary occupations, craft and related trades workers, skilled agricultural and forestry workers, plant and machine operators and assemblers or working for the armed forced.

Table 2. Impact of control variables in academic scores for different competences. Difference-in-difference estimation using OLS. School years 2016/17 and 2018/19.

VARIABLES	PC1	SPA	ENG	H&G	MTH	VARIABLES	PC1	SPA	ENG	H&G	MTH
Gender	-0.0539***	15.22***	13.62***	-18.66***	-27.03***	**Mother plant operators**	-0.062	-4.862	-4.035	1.655	-6.757*
	(0.012)	(0.770)	(0.719)	(0.812)	(0.857)		(0.050)	(3.253)	(3.046)	(3.436)	(3.681)
Immigrant	-0.015	-5.106***	6.919***	0.763	-5.287***	**Mother retail and services**	-0.109***	-6.139***	-4.534***	-6.452***	-4.132**
	(0.023)	(1.481)	(1.382)	(1.562)	(1.648)		(0.023)	(1.471)	(1.373)	(1.551)	(1.632)
Pre-primary education	0.0690***	2.562***	3.193***	3.229***	4.691***	**Mother security & armed forces**	-0.069**	-2.031	-4.650**	-4.292*	-2.506
	(0.0123)	(0.806)	(0.752)	(0.851)	(0.896)		(0.035)	(2.271)	(2.117)	(2.404)	(2.536)
Age	-0.352***	-22.16***	-23.61***	-16.10***	-4.795***	**Mother technician & professionals**	0.062***	0.959	3.527**	3.180**	4.7665***
	(0.030)	(1.939)	(1.796)	(2.050)	(2.145)		(0.022)	(1.448)	(1.357)	(1.530)	(1.617)
February	-0.024	0.678	-0.681	-2.211	-1.330	**Father no occupied**	-0.294***	-12.84**	-21.81***	-8.332	-10.62*
	(0.029)	(1.939)	(1.791)	(2.027)	(2.137)		(0.079)	(5.220)	(4.856)	(5.536)	(5.761)
March	-0.123***	-3.562*	-6.880***	-5.225***	-5.613***	**Father elementary occupations**	-0.095***	-2.628	-9.208***	-0.714	-4.986*
	(0.029)	(1.873)	(1.748)	(1.977)	(2.085)		(0.029)	(1.866)	(1.739)	(1.971)	(2.074)
April	-0.114***	-4.550**	-5.512***	-5.361***	-5.144**	**Father craft**	-0.044**	-0.520	-7.202***	-0.483	0.345
	(0.029)	(1.897)	(1.769)	(2.002)	(2.111)		(0.021)	(1.396)	(1.304)	(1.474)	(1.556)
May	-0.147***	-7.084***	-7.779***	-7.662***	-4.234***	**Father skilled agricultural**	-0.207***	-11.58***	-15.54***	-6.377	0.150
	(0.029)	(1.919)	(1.788)	(2.025)	(2.135)		(0.060)	(3.898)	(3.649)	(4.111)	(4.336)
June	-0.176***	-8.370***	-11.03***	-7.057***	-7.442***	**Father plant operator**	-0.077***	-0.190	-4.731***	-3.645**	-4.801***
	(0.031)	(2.025)	(1.889)	(2.140)	(2.254)		(0.023)	(1.526)	(1.422)	(1.611)	(1.694)
July	-0.216***	-10.60***	-12.92***	-9.973***	-6.321***	**Father retail & services**	0.059**	3.131*	-1.577	4.889***	5.544***
	(0.032)	(2.060)	(1.920)	(2.174)	(2.291)		(0.027)	(1.759)	(1.645)	(1.861)	(1.961)
August	-0.285***	-15.60***	-17.84***	-13.46***	-6.607***	**Father security & armed forces**	0.024	2.567	-1.179	3.829*	-0.635
	(0.033)	(2.167)	(2.022)	(2.289)	(2.411)		(0.032)	(2.114)	(1.973)	(3.232)	(2.356)
September	-0.317***	-16.49***	-19.39***	-16.50***	-5.417**	**Father technician & professionals**	0.100***	5.292***	5.265***	4.027***	5.070***

	(0.035)	(2.247)	(2.092)	(2.372)	(2.495)
October	-0.333***	-17.84***	-19.93***	-18.18***	-4.994*
	(0.036)	(2.312)	(2.154)	(2.442)	(2.569)
November	-0.341***	-17.99***	-20.57***	-20.06***	-3.995
	(0.037)	(2.414)	(2.244)	(2.549)	(2.680)
December	-0.444***	-24.49***	-26.25***	-21.49***	-8.558***
	(0.039)	(2.536)	(2.361)	(2.677)	(2.815)
Books-11-50	0.360***	18.87***	21.57***	18.95***	13.41***
	(0.031)	(2.007)	(1.870)	(2.115)	(2.234)
Books-51-100	0.514***	25.78***	31.03***	27.09***	19.05***
	(0.031)	(2.023)	(1.884)	(2.132)	(2.249)
Books-101-200	0.708***	33.27***	41.01***	37.07***	28.68***
	(0.032)	(2.066)	(1.926)	(2.178)	(2.294)
Books>200	0.872***	38.78***	47.04***	47.42***	38.97***
	(0.032)	(2.068)	(1.926)	(2.180)	(2.299)
Mother no occupied	-0.030	-2.996	-3.331	2.013	-2.549
	(0.039)	(2.548)	(2.379)	(2.690)	(2.830)
Mother elementary occupations	-0.036	-0.285	-3.232***	-1.683	-2.676*
	(0.022)	(1.429)	(1.333)	(1.508)	(1.590)
Mother craft	-0.072	-6.943**	-2.376	-4.389	-0.389
	(0.046)	(2.987)	(2.793)	(3.160)	(3.326)
Mother skilled agricultural	-0.140	-10.86*	-15.78**	-6.727	-2.107
	(0.108)	(6.916)	(6.360)	(7.400)	(7.576)
	(0.020)	(1.302)	(1.217)	(1.375)	(1.451)
Parents-education-2	0.130***	4.768***	8.998***	7.363***	3.950***
	(0.019)	(1.265)	(1.179)	(1.336)	(1.405)
Parents-education-3	0.415***	15.05***	27.90***	21.56***	16.06***
	(0.021)	(1.349)	(1.257)	(1.424)	(1.499)
Repeat in Secondary	-0.284***	-10.68***	-23.67***	-16.82***	-4.378
	(0.040)	(2.560)	(2.406)	(2.738)	(2.871)
Repeat in primary	-0.367***	-12.21***	-24.45***	-12.70***	-18.25***
	(0.040)	(2.688)	(2.372)	(2.706)	(2.830)
Constant	5.052***	829.8***	842.9***	725.5***	560.9***
	(0.505)	(32.49)	(30.09)	(34.33)	(35.94)
Number of schools	755	757	758	756	759
Observations	43,414	44,457	44,730	44,336	44,647
School fixed effects	YES	YES	YES	YES	YES
R-squared	0.138	0.082	0.155	0.085	0.061

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

6.2. Impact of the regulatory change

Table 3 reports the estimation of our foci parameters. Two econometric specifications were considered. The first one corresponds to the estimation shown in table 3 as it contains the 2016/17 & 2018/19 academic years. The second econometric specification also adds the 2017/18 academic year to the estimation sample. In this case we include a specific set of dummy variables for father and mother occupations in 2017/18 as these variables a differently defined that year. Regardless of the econometric specification, for the composite index we find that non-fee-paying schools dummies are positive and significant indicating that after controlling for socio-economic status students from these school do not perform worse than those from private schools. The 2018/19 academic year dummy is positive and significant pointing to a better performance that year. The interaction term between academic year 2018/19 and non-fee-paying school is negative and significant which clearly indicates that the impact of the change in the normative regulation had a negative effect on students' performance. More specifically, the new normative reduce the expected score by around 0.14 standard deviation units (-0,212 divided by the 1.5 standard deviation of *pc1*) . It is a remarkable result given that the regulation change only affected two weeks of classes at the end of the academic year. The normative change had a negative impact across in each of the academic competences but it was only significant in Spanish and English.

By competences, the inclusion of the 2017/2018 academic year matters. The effect of the two weeks reduction is negative and significant for each of the disciples once the whole sample is taken into consideration.

Table 3. Impact of the regulatory change on academic scores in different competences. Difference-in-difference estimation using OLS.

	School years included 2016/17 & 2018/19				
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2018/19	0.146*** (0.042)	17.48*** (2.723)	2.864 (2.560)	-0.144 (2.872)	6.012** (3.034)
Public & Charter school	0.254 (**) (0.114)	4.571 (3.845)	9.545 (3.625)	22.13*** (4.501)	8.215 (8.380)
Test in 2018/19 interacted with Public & Charter school	-0.212*** (0.044)	-20.74*** (2.774)	-5.463** (2.614)	-3.114 (2.900)	-2.949 (3.100)
Observations	43,414	44,457	44,730	44,336	44,647
R-squared	0.138	0.082	0.155	0.085	0.061
	School years included: 2016/17, 2017/18 & 2018/19				
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2018/19	0.200*** (0.0341)	15.34*** (2.086)	5.552*** (2.020)	7.233*** (2.226)	9.543*** (2.345)

Public & Charter school	0.295**	4.618	10.41	25.63***	10.63
	(0.115)	(7.077)	(6.854)	(7.563)	(7.960)
Test in 2018/19 interacted	-0.230***	-16.62***	-5.778***	-8.488***	-6.604***
with Public & Charter school	(0.0352)	(2.151)	(2.082)	(2.295)	(2.417)
Observations	62,062	63,297	63,728	63,223	63,839
R-squared	0.136	0.094	0.156	0.092	0.063

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

6.3. Extended analysis

A relevant political question regards the estimation of the impact of this measure across the score distribution. In order to do this, we follow previous contributions in the literature and perform this analysis by means of quantile regressions (Battistin and Meroni, 2016 and Heubener et al, 2017). We use the quantile regressions with fixed effects estimation following the method recently proposed by Machado and Silva (2019). These authors propose an estimator that provides information on how the regressors affect the entire conditional distribution in a panel data setting. Table 4 shows the estimation results in the aggregate component *pc1* for quantiles 0.01, 0.25, 0.5, 0.75 and 0.99. The estimated coefficient associated to our focus variable, test in 2018/19 interacted with Public and Charter school, shows that the intervention becomes more negative the upper the quantile is. At least two plausible explanations can be used to explain this heterogeneous effect. Firstly, the change in the school calendar has a more negative effect for those students who are not lagging behind and do not receive the remedial education (Goodman, 2017) during the weeks between the ordinary and extraordinary final exam. A second reason is that high performing students make a better use of instruction time and, therefore, could be more affected by its reduction (Battistin and Meroni, 2016 and Heubener et l, 2017).

Table 4. Impact of the regulatory change on academic scores in different quantiles. Difference-in-difference estimation for the composite index PC1.

	School years included 2016/17 & 2018/19				
Variables	Q1	Q25	Q50	Q75	Q99
Test in 2018/19	-0.331***	0.012	0.144***	0.282***	0.623***
	(0.128)	(0.053)	(0.041)	(0.053)	(0.127)
Public & Charter school	0.209	0.241	0.253**	0.266	0.298
	(0.395)	(0.164)	(0.126)	(0.164)	(0.393)
Test in 2018/19 interacted	-0.081	-0.170***	-0.204***	-0.240***	-0.329**
with Public & Charter school	(0.135)	(0.056)	(0.043)	(0.056)	(0.134)

	School years included: 2016/17, 2017/18 & 2018/19				
Variables	Q1	Q25	Q50	Q75	Q99
Test in 2018/19	-0.254**	0.0767*	0.200***	0.327***	0.638***
	(0.111)	(0.045)	(0.0342)	(0.0443)	(0.105)
Public & Charter school	0.460	0.340**	0.295**	0.249	0.135

	(0.397)	(0.162)	(0.123)	(0.159)	(0.379)
Test in 2018/19 interacted with Public & Charter school	-0.142	-0.206***	-0.230***	-0.254***	-0.314***
	(0.114)	(0.047)	(0.035)	(0.046)	(0.109)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

In another extended analysis we explored if the estimated heterogeneity across students could be explained by some specific variable. In particular, in three separate models we considered a triple difference in difference specification where the focus variable interacts the 2018/19 test with non-fee paying school and 1) low parent education; 2) repeat in secondary; and 3) students who chose a strong mathematical course. However, results were not statistically significant in any case and we do not report them.

Our last extended exercise concerns a pre-testing analysis. In particular, the underlying assumption in a difference in difference estimation is that the difference between the control and treatment group is due to the treatment policy. In order to validate this hypothesis, researchers are increasingly testing for trend differences between these two groups prior to the intervention (Kahn and Lang, 2019). Thus, we conduct a placebo test in our sample to estimate the effect of a hypothetical intervention only affecting private schools in the 2017/18 (instead of 2018/19) academic year. Estimation results are shown in Table 5. It can be observed that the effect of this intervention would be positive. Furthermore, we looked at trend differences between private and non-fee paying schools before our estimation sample started in the 2016/17 school year. This type of analysis is possible as the region of Madrid participates in the international test of PISA of the OECD with an oversample. A naïve comparison of total scores in the three PISA areas of knowledge, Reading, Science and Mathematics, between PISA 2012 and PISA 2015 does not show any significant performance trend difference in private compared to non-fee paying schools. More specifically, during this period, the overall score across the three areas of knowledge increased by 4.7 and 3.6 points in non-fee paying schools and private schools respectively (OECD, 2013; and OECD 2016). Neither this trend nor specific trends for each area of knowledge were significant at the conventional levels. If at all, these results indicate that non-fee paying schools were improving even more than private schools providing further evidence that our estimation results are not due to any previous pre-trend.

Table 5. Pre-trend test for the 2017/18 academic year. Difference-in-difference estimation using OLS.

	School years included 2016/17 & 2017/18				
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2018/19	-0.514***	-10.38***	-33.23***	-31.79***	-19.57***
	(0.058)	(3.392)	(3.491)	(3.797)	(4.037)
Public & Charter school	0.552**	18.22	12.60	39.25***	28.57*
	(0.228)	(13.45)	(13.77)	(15.03)	(16.03)
Test in 2018/19 interacted with Public & Charter school	0.151***	0.858	9.454***	16.69***	6.819**
	(0.043)	(2.503)	(2.594)	(2.804)	(2.995)
Observations	39,687	40,387	40,548	40,224	40,544
R-squared	0.129	0.087	0.150	0.086	0.057
	School years included: 2016/17, 2017/18 & 2018/19				
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**

Test in 2018/19	-0.543*** (0.053)	-16.65*** (3.239)	-29.22*** (3.124)	-31.74*** (3.448)	-22.52*** (3.614)
Public & Charter school	0.197* (0.114)	-2.552 (7.014)	8.061 (6.787)	22.11*** (7.490)	8.329 (7.886)
Test in 2018/19 interacted with Public & Charter school	0.176*** (0.036)	5.564** (2.213)	6.858*** (2.148)	14.96*** (2.358)	7.313*** (2.485)
Observations	62,062	63,297	63,728	63,223	63,839
R-squared	0.137	0.094	0.158	0.093	0.064

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

7. Discussion and concluding remarks

In this paper we consider a quasi-natural experiment consisting in a modification of the school calendar in the Region of Madrid. This change reduced learning time by two weeks for 42.8% of non-fee paying school students in compulsory secondary education who passed their ordinary exams. The remaining students were not affected by this policy. We find that this intervention diminished the skills of students as measured by an external and standardized test. Moreover, students in the 75th percentile of the score distribution were relatively more affected than those in the 25th percentile by 4.4% of a standard deviation (baseline estimation in Table 4).

Whilst the policy intervention considered in this paper took place before the COVID-19 outbreak, it can be used to draw lessons for the possible impact of the school closures triggered by this crisis. By the summer of 2020 most countries have closed schools for around 12 weeks of face-to-face learning, a third of the 2019-20 academic year. It is commonly accepted that this decision is causing major interruption in students' learning. In particular, based on previous interruptions in schools, the Royal Society DELVE Initiative (2020) estimates an impact of between 6% SD to 10% standard deviations for the learning loss of the 12-week lockdown. Kuhfeld et al. (2020) project the potential impacts of COVID-19 school closures on academic achievement. They estimate that students are likely to return in fall 2020 with approximately 63-68% of the learning gains in reading relative to a typical school year and with 37-50% of the learning gains in math. In order to properly interpret these findings, it can be considered that learning gains in most national and international tests during one year are equal to between one-quarter and one-third of a standard deviation (Woessman, 2016). Therefore, Kuhfeld's et al. projection of learning loses due to covid-19 varies from a minimum of an 8% of the standard deviation in reading to a maximum of a 22% in math . It is remarkable that despite the relatively small reduction of instruction time considered in this paper, we estimate a higher effect on academic outcome than the Royal Society DELVE Initiative (2020) and in line with those of Kuhfeld et al. (2020). Thus, our baseline estimates are also significant with an overall effect of 14% of the s.d. affecting particularly learning areas such as Spanish (-20.7% s.d) or English (-

5.5% s.d.) while they are insignificant for Maths and History and Geography.

Students with a higher test scores were more affected by the policy. The calendar change studied in this paper may differ from the school lockdown produced by COVID-19. This lockdown is expected to exert an heterogeneous impact with a limited effect among students from favoured backgrounds but high among lagging and disadvantaged students. It seems very unlikely that that online education will on average replace the learning lost from school (Bettinger et al., 2017). Wide variation in the quantity and quality of remote schooling and home learning support between pupils and schools underlies much of the variation in learning loss over this period. There will likely be substantial disparities between families in the extent to which they can help their children learn. Key differences include the amount of time available to devote to teaching, the non-cognitive skills of the parents, resources, and also the amount of knowledge. These are relevant issues to explore in future research.

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Appendix: definitions of control variables

Gender: takes value 1 if student is female and 0 otherwise, *Pre-primary education*: indicates attendance to 0-2 years early pre-primary education, *Age*: student's age Immigrant: Immigrant student, *January*: dummy variable indicating that the student was born in January. Other months of birth are defined similarly. *Books-0-10*: dummy variable indicating that the total number books in the student's home was in the [0,10] range. Variables *Books-11-50*, *Books-51-100*, *Books-101-200* and *Books>200* are similarly defined. *Mother no occupied*: Mother has never been occupied, *Mother elementary occupations*: Mother performs elementary occupations, *Mother craft*: Mother works in the craft sector or as a related trade worker, *Mother skilled agricultural*: Mother works as a skilled agricultural or forestry worker, *Mother plant operators:* Mother is plant and machine operator or assembler; *Mother retail and services*: Mother works in hospitality, retail or other related services, *Mother security & armed forces*: Mother works in protection, security or armed forces, *Mother technician & professionals*: Mother is a technician, associate professional, chief executive or senior official, *Mother clerical support workers*: Mother is a clerical support worker. Father's occupations are defined likewise. *Parents-education1*: Mother or father achieved up to compulsory Secondary school, *Parents-education2*: Mother or father achieved post- compulsory Secondary school, *Parents-education3*: Mother or parent achieved Undergraduate, Graduate or Upper Vocational Training. *Repeat in secondary once*, *Repeat in secondary more than one*, *Repeat in primary once* and *Repeat in primary more than one* are 0-1 dichotomous variables indicating whether the student has repeated once or more than one in his/her previous primary or secondary education.