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The Impact of Instruction Time and the School Calendar on Academic Performance: A Natural Experiment

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One of the most obvious and not sufficiently well understood political decisions in education regards the optimal amount of instruction time required to improve academic performance. This paper considers an unexpected, exogenous regulatory change that reduced the school calendar of non-fee-paying schools (public and charter schools) in the Madrid region (Spain) by two weeks during the 2017/2018 school year. Using difference-in-differences regression, we found that this regulatory change contributed to a significant deterioration in academic performance, particularly in Spanish and English. We further explored non-linear (quantile) effects across the distribution of scores in standardized exams, finding that the disruption due to the new regulations affected more students in the upper quartile of the distribution. Overall, we found a reduction in the gap across non-fee-paying schools and an increase in the gap between non-fee- and fee-paying schools (private schools).

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The Impact of Instruction Time and the School Calendar on Academic Performance: A Natural Experiment

Ismael Sanz[1] and J.D. Tena[2]

ABSTRACT

One of the most obvious and not sufficiently well understood political decisions in education regards the optimal amount of instruction time required to improve academic performance. This paper considers an unexpected, exogenous regulatory change that reduced the school calendar of non-fee-paying schools (public and charter schools) in the Madrid region (Spain) by two weeks during the 2017/2018 school year. Using difference-in-differences regression, we found that this regulatory change contributed to a significant deterioration in academic performance, particularly in Spanish and English. We further explored non-linear (quantile) effects across the distribution of scores in standardized exams, finding that the disruption due to the new regulations affected more students in the upper quartile of the distribution. Overall, we found a reduction in the gap across non-fee-paying schools and an increase in the gap between non-fee- and fee-paying schools (private schools).

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

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1. INTRODUCTION

How do political decisions on instruction time and the school calendar affect academic outcomes? This is an important question for a range of stakeholders in education, including policymakers and school principals. Instruction time has been a central element in many education reforms. OECD (2019) emphasizes the essential role of instruction time in improving the quality and quantity of education outcomes. As the most obvious input in education production, instruction time has also attracted considerable attention from academic researchers (Aucejo & Romano, 2016; Barrios-Fernandez & Bovini, 2021; Battistin & Meroni, 2016; Huebener, Huebener, & Marcus, 2017). Because identifying the impact of instruction time is difficult, many papers have addressed this issue by considering modifications of instruction time due to political intervention. However, in most cases, implementing these policies can be largely anticipated, if not endogenously chosen, by rational school managers and students before making relevant decisions. For example, the *Quality and Merit Project*, which provides additional hours of instruction to schools in Southern Italian regions, requires schools to apply in advance to be admitted. Moreover, treated classes are decided by schools principals (Battistin & Meroni, 2016). Another example, Chilean School Day, increased daily instruction time in all publicly subsidized Chilean primary and secondary schools. This scheme was gradually implemented, allowing for some degree of school autonomy (Barrios-Fernandez & Bovini, 2021; Bellei, 2009). In other cases, policy decisions prevent the sample from being divided into treatment and control groups (Huebener et al., 2017). For instance, the German G8 reform, which increased instruction time by two hours per week, was applied to all fifth grade to ninth grade students, so there was no control group for comparison.

This paper considers an unexpected regulatory change affecting seventh-grade to 10th-grade students in the Spanish region of Madrid (*Comunidad de Madrid*) during the 2017/2018 school year. The new regulations changed the ordinary final exam date from late to early June, reducing the total instruction time for students who passed these exams by around two weeks. Thus, students who failed received intensive classes throughout these two weeks to pass their repeat exams. Students who had already passed did not receive any formal teaching but were involved in cultural activities. The new regulations affected only public and charter schools, but not private schools. There are at least three desirable features of this political intervention for identification purposes. First, as we will discuss in Section 3, normative treatment was unexpected and enforced in all non-fee-paying schools (our treatment group). Second, the response variable was an objective, standard measure of academic performance for the treatment and control group. We used the fact that the region of Madrid set standard cognitive tests for 10th-grade students who were affected and not affected by the new regulations before and after the change to implement a difference-in-differences analysis. Finally, we considered a student-level sample with detailed information on students' socioeconomic traits. The data set also made it possible to control for unobserved school characteristics with fixed effects.

Using difference-in-differences regression, we found that this regulatory change contributed to a significant deterioration of academic performance, particularly in the subjects of Spanish and English. Moreover, we tested whether treated and non-treated schools' pre-trend evolution can explain this difference-in-performance. We further explored non-linear (quantile) effects across the distribution of scores in the standardized exam, finding that the disruption due to the new regulation affected more students in the upper quartile of the distribution. Overall, we found a reduction in the gap across non-fee-paying school (public and charter schools) students and an increase in the gap between non-fee- and fee-paying schools (private schools).

Our paper contributes to the analysis of educational policy decisions in at least two ways. First, it provides evidence that even a tiny change in instruction time (just two weeks) can have substantial implications for students' academic outcomes (around 0.14 times standard deviation). Second, the study contributes to an incipient strand of literature on the potential impact of drastic reductions of instruction time due to lockdown policies in many countries (Burgess, 2020; Burgess & Sievertsen, 2020; DELVE Initiative, 2020; Goulas & Magalokonomou, 2020; Kuhfeld et al., 2020; Santibanez & Guarino, 2020). A precise estimate of the consequences of the lockdown on scholarly output will only be possible with the benefit of hindsight once international post-COVID-19 data become available. In many cases, it will be difficult to assess the impact of the decision to close school doors on academic performance because this effect will be influenced by other social and economic changes due to COVID-19, such as income and health. The natural experiment studied in this paper constitutes an example of an instruction time shock that is not associated with any other macroeconomic confounding variable.

This paper proceeds as follows. The next section discusses the literature on the importance of instruction time. Section 3 describes the new regulation. Section 4 presents the data used in the analysis. The empirical strategy is discussed in Section 5.

Section 6 presents and discusses the empirical results. Section 7 presents the conclusions of the study.

2. RELATED LITERATURE

An analysis of the impact of instruction time on academic performance is found in early works by Grogger (1996) and Eide and Showalter (1998), who observed a non-significant effect of the length of the school year on academic outcomes in the United States. Card and Krueger (1992) analyzed the same issue, using state-level data for the United States. They found a significant positive effect on earnings, which vanished once they controlled for school quality variables.

For a KIPP school in Massachusetts, Angrist et al. (2012) analyzed a highly standardized and widely replicated charter model with features including a long school day, an extended school year, selective teacher hiring, strict behavioral norms, and emphasis on traditional reading and math skills. KIPP students spend extended time in school, with both a longer school day and a longer academic year than traditional public school students. Much of the additional time is used for instruction in basic math and reading skills. Using applicant lotteries, those authors showed average achievement gains of 0.36 standard deviations in math and 0.12 standard deviations in reading for each year spent at KIPP, with the largest gains observed in those with limited English proficiency and low baseline scores or in special education.

Heinrich et al. (2014) reported that U.S. school districts are spending millions of dollars on tutoring outside regular school day hours for economically and academically disadvantaged students who need extra academic assistance. Under *No Child Left Behind* (NCLB), parents of children in persistently low performing schools were allowed to choose their child's tutoring provider. Those authors found that many students were not receiving enough hours of high-quality, differentiated instruction to produce significant gains in their learning. Recent K–12 educational reform activity suggests that out of school tutoring remains an important source of supplementary instruction for students in need.

However, the arrival of individual databases, especially the Programme for International Student Assessment (PISA), has driven the growth in this literature. For example, Cattaneo et al. (2017), Lavy (2012, 2015), and Rivkin and Schiman (2015) achieved identification using the variation of instruction time in different subjects for the same student. This analysis was at the cost of obtaining a single estimate of the causal effect for all possible subjects. Alternative identification approaches include, for example, the use of exogenous political shocks (Barrios-Fernandez & Bovini, 2021; Bellei, 2009; Huebener et al., 2017; Pischke, 2007), natural experiments (Aucejo & Romano, 2016; Fitzpatrick et al., 2011), instrumental variables (Goodman, 2014), and laboratory experiments (Banerjee et al., 2007).

A general conclusion throughout the literature is that, although instruction time exerts a positive and significant effect on educational outcome, this effect is small. When examining the impact on test scores, papers that report causal effects by subject provide inconclusive results. For example, although Aucejo and Romano (2011) found that instruction time exerts a greater impact on math than on reading, the results were not reported as being significantly different by Bellei (2009). Some of these papers also explore differences by ability. This approach is politically relevant to assess the role of instruction time in reducing differences in student performance. However, these results are also mixed. For example, Huebener et al. (2017) found greater sensitivity in low-performing students, whereas the opposite was reported by Aucejo and Romano (2016). One of the most important dimensions of these analyses is the quality of instruction time. Banerjee et al. (2007) provided an interesting discussion on this issue, explaining that additional educational inputs would not affect educational performance unless they addressed specific unmet needs at the school. Their experiment showed that specific programs for remedial education and computer-assisted learning exerted a significant but not highly persistent effect on student outcomes. However, despite the importance of quality variables, they can only be imperfectly observed in most cases. For example, Rivkin and Schiman (2015) used the classroom environment as a proxy, finding that the effect of instruction time on academic performance is higher when the quality of this instruction is also higher.

Interest in the impact of instruction time on education has further intensified following the COVID-19 pandemic, which has caused unprecedented learning interruptions. Recent studies have used these types of analyses to discuss the potential impact of COVID-19 school closures (Goulas & Magalokonomou, 2020; Kuhfeld et al., 2020; Santibanez & Guarino, 2020). Notably, Santibanez and Guarino (2020) estimated the effect of absenteeism on cognitive and social-emotional outcomes in California. Kuhfeld et al. (2020) analyzed the impact of missing school due to absenteeism, regular summer breaks, and school closures on learning outcomes in the United States. These two papers report a significant effect of missing school on learning outcomes.

In a similar study, Goulas and Magalokonomou (2020) focused on the swine flu pandemic in Greece. Consequently, the Greek government relaxed its school attendance policy. As in the case examined in our paper, the effect of this policy was not the same across all students, with a greater effect on those with higher prior performance, who took more absences. In comparison, students with lower previous performance kept going to school. They conducted regression analysis to show a negative correlation between attendance and performance.

We contribute to this literature by presenting one of the few examples of a natural experiment that consists of an exogenous and unexpected alteration of instruction time. The new regulation discussed in this study meant a different treatment for failing students in non-fee-paying (public and charter schools) and private schools. In the former (treatment) group, students who had to repeat their exams, received specific instruction for these exams for 15 additional days. This instruction was provided by the teachers who would then evaluate them. However, higher-performing state-school students, who had already passed the exam, used this time for social and cultural activities. Private schools (control group) were unaffected by this measure.

Given that all students in the region of Madrid had to take a standard test the following year, a difference-in-differences approach could be used to estimate the specific effects of this policy. In theory, we should not expect this political measure to have a significant impact on academic outcomes because variation in the number of instruction days is minimal compared to the cases described in the literature, which reports only small effects. This political shock affected both groups of students, regardless of whether they had to repeat their exams. However, a distinctive feature of this variation in instruction time is that it aimed to address specific unmet needs of students who were supposed to be especially interested in receiving help to pass their repeat exams.

3. BACKGROUND

National and regional governments are responsible for educational policy in Spain. Although the overall framework and guidelines are defined nationally, most schooling decisions and funding are determined at the regional level. There are three main school categories: state-funded, charter, and private. While the state funds the first two for compulsory education, the latter is privately funded. Here, we focus on ninth-grade students (aged 14 to 15 years old) in the Madrid region. Many of these students were affected by the change in the school calendar analyzed in this paper. Moreover, they took an external standardized test the following academic year (i.e., in the 10th grade, which is the final year of compulsory secondary school).

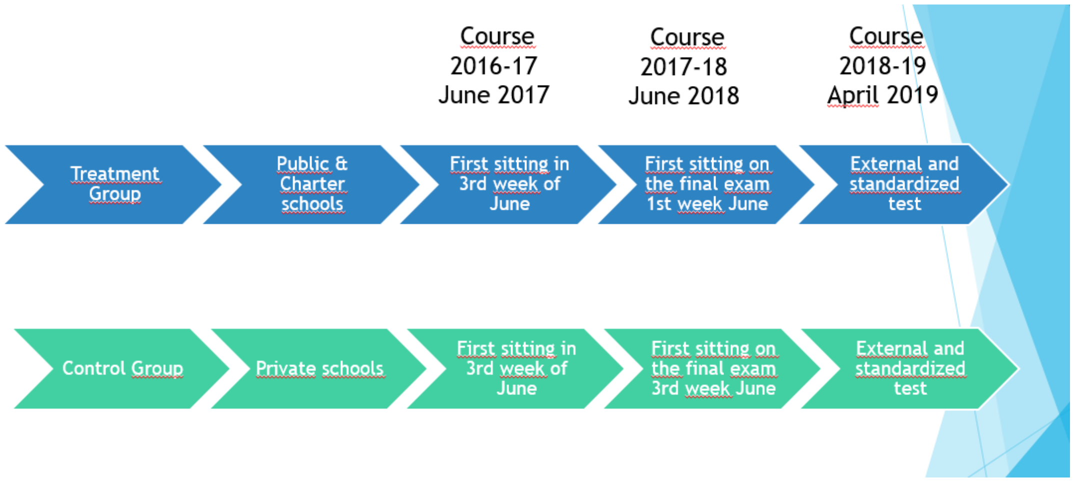
Figure 1 represents the effect of the regulatory change on the treatment group (public and charter schools) and control group (private school). In the 2017/2018 school year, the region of Madrid changed the timing of the second sitting of the final exam for the first time. This exam is a chance to retake the exam for students who did not pass the first sitting of the final exam on the first attempt. Previously, the first and second sittings of the exams took place in late June and early September, respectively.

This change aimed to improve academic results, aid planning for the start of the school year, make family life easier during the summer break, and reduce family spending on private tuition (Regional Ministry of Education for Madrid, 2018). Crucially, bringing forward the second sitting of the final exams from September to the third week of June meant that the first sitting of the final exams took place in the first week of June, two weeks earlier than in previous academic years. For these two weeks in between, students with failed subjects attended regular classes with a lower student-to-teacher ratio. They were given greater individual attention, support activities, reinforcement, and tutoring. In contrast, students who passed all subjects in the first sitting attended other types of cultural activities such as visits to museums, social volunteering activities, and debate tournaments. All students, regardless of whether they had passed their exams, still had to attend school. The school calendar must consist of a minimum of 175 school days for compulsory education by Spanish law (Organic Education Organic Law, LOE).

The decision to bring the second sitting forward from September 2018 to June 2018 was made on June 27, 2017 (Madrid Regional Ministry of Education, 2018), when the previous school year had already finished. The period for applications to participate in the ordinary process of admission in schools of students for the next academic year took place between the 19th of April and the 5th of May, 2017. So, when the change in the school examination calendar was made in June 27, 2017, students were not already able to move from one school to the other.

Many schools were not aware of the change until May 2018. The first evaluation assessment of this measure by the Madrid Regional Education Council supports this conclusion. First, from the first to third week of June, attendance by students who passed all subjects was around 50 per cent because they did not want to participate in cultural activities. In fact, the Madrid Regional Education Council (2018) approved legislation after this change, pointing out that "Incentives to attend classes for all students should be improved, including those who have obtained positive grades in their final exams."

Figure 1. Effect of the regulatory change on different types of schools in the Madrid region



Moreover, the Madrid Regional Education Council (2018) continued with the first evaluation assessment of the measure, suggesting some improvements for the next school year and explicitly asking, "What incentives will students who have already passed all subjects have?" Second, in the first assessment of the calendar change, the Madrid Regional Education Council concluded that information regarding this new school calendar should be shared with the entire educational community in the 2018/2019 school year. However, this information did not reach the community as a whole as suggested by the Madrid Regional Education Council, leading the council to ask, "What actions will we carry out to ensure that the information about the new school calendar reaches the schools?" Moreover, the report proposed actions such as "information panels for schools; institutional advertising; workshops especially aimed at families.” The document concluded that the “areas for improvement” of the change to the calendar include “attendance of students who had already passed all subjects” and “information for families”. In the last two weeks of term, the absenteeism of students in Madrid who had already passed the exams in the first sitting received heavy coverage in the national press.[1]

4. DATA

4.1 External exam

We aim to estimate the impact of these changes to the school calendar on students’ performance in the region of Madrid. For this purpose, we used an objective, independent measure of academic performance. Since the 2015/2016 school year, Madrid’s regional government has set a standardized external exam for all third- and sixth-grade students in the region. One year later, in 2016/2017, the region extended this standardized external exam to 10th-grade students in their final year of compulsory secondary school (aged 15 to 16 years). We focus on the outcome of the 10th-grade exams because they include students affected by the change in the school calendar in the previous academic year. The exam measured basic knowledge in four competencies: Spanish (*SPA*), English (*ENG*), math (*MTH*), and history and geography (*H&G*). The exam results did not have consequences for the students’ academic endeavors but merely provided information to the school (Anghel et al., 2016). These exams were just

[1] Consult the following headlines in Spanish newspapers: “Early end of the year for secondary students who have not failed” or “Generalized absenteeism after bringing forward 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

used for measurement purposes. Therefore, the results for each student were recorded in a report sent to parents or guardians. This report was informative for schools as well as teachers, parents, and the students themselves. Furthermore, since the exams were compulsory for all students, our analysis is based on the full population of 10th-grade students in the region of Madrid instead of just a sample.

Each of the four competency tests (mathematics, Spanish, foreign language, and history and geography) lasted 60 minutes. The exams took place on two consecutive days. Principals, subject teachers, families, and students completed context questionnaires before and after the exam. These questionnaires provide information on the socioeconomic and cultural conditions of the families and schools to contextualize the results.

4.2 Description of variables

The scores in each of the four competency tests described above are the dependent variable in our analysis. Like in the PISA data set, all scores were standardized, with mean and standard deviation of 500 and 100, respectively. There were also marginal differences in the number of observations for each exam for reasons such as illness or arriving late to the exam. We provide specific estimations for each competency. However, because our main interest is overall student performance not particular competencies, we also performed principal component analysis to the dimension of the problem. We found that the first principal component explained slightly over 60 percent of variability in the four variables. Moreover, all weights of the variables were positive and similar (around 0.5). These results suggest that this combination offers an average of the scores in the four competency tests. We denote this combination *pc1*.

Following the study by Hanushek and Woessmann (2011) on the determinants of student achievement, we considered three groups of control variables in the analysis. The first consisted of innate characteristics of students. We considered dichotomous variables for gender, immigration status, and each birth month. The second group of variables consisted of the student's family background, including indicators of the number of books at home, father's and mother's profession, and parents' educational attainment. The third group of variables consisted of institutional aspects such as tracking (captured by whether the student followed a strong math academic course or a more applied math course) and repetition (whether the student repeated primary and secondary education), age and early childhood education (0 to 2 years old).

Using this information, we defined the treatment and control groups used in the difference-in-differences analysis. Before treatment, the treatment group contained 10th-grade public and charter school students who took the standardized regional exam in the 2016/2017 academic year. Students in the same schools who took the exam in 2018/2019 belonged to the treatment group after treatment. Likewise, the control groups before and after treatment were formed of students from private schools in the same periods.

The 2017/2018 school year was excluded from our baseline analysis because the code used in the school and family-level database differed from the code used in the student-level database. Therefore the information for schools, families, and students could not be linked for all control variables in the analysis. In the 2017/2018 school year, apart from the test scores, the only available information was the data provided by the students. Thus, some variables such as students' age and birth month were missing. We could ascertain whether the student repeated the year but not whether it took place at the primary or secondary level. Furthermore, we only had vague information on father's and mother's occupations, which only indicated whether they were fully employed, partially employed, unemployed, or inactive. However, as described later in the paper, we studied the robustness of our results by including the 2017/2018 exam data.

Table 1 shows the descriptive statistics for all treatment variables and control groups before and after the regulatory change. A description of these variables can be found in the Appendix. There was better academic performance in all competencies in private schools than in public and charter schools. However, students in these two schools differed in several individual and social characteristics. For example, a higher proportion of students in private schools attended early childhood education (0 to 2 years). The immigrant population was lower in private schools. Moreover, on average, families of students in private schools had more books at home, wealthier occupations, and higher educational attainment. All these differences suggest a need to control for these characteristics as well as by school.

Table 1: Descriptive Statistics

	Before treatment						After treatment					
	Public and charter			Private			Public and charter			Private		
	Obs.	**Mean**	**SD**	**Obs.**	**Mean**	**SD**	**Obs.**	**Mean**	**SD**	**Obs.**	**Mean**	**SD**
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 does not work	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 basic 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 and 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 agriculture and 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 and 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, services, and 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 and 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 and 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 does not work	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 basic 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 and 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 agriculture and 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 and 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, services, and 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 and 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 and 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 only compulsory education	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 post-secondary education	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 higher education	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 year 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 year 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 year 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 year 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 used a difference-in-differences econometric method to investigate the school calendar change described in Section 3 on students' performance in the Spanish region of Madrid. More specifically, we estimated whether this change in 2017/2018 significantly affected students' academic results in public and charter schools, which were affected by this measure, differently from students in private schools, which were not affected by this change. The response variable, $Y_{i,j,t}$, was the score in the standardized exam by student i, school j, and academic year t. In our baseline specification, we used least squares estimation of the following model:

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

(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 0 otherwise; $T_{2018/19}$ takes values 1 if the observation belongs to the 2018/2019 academic year and 0 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 was carried out considering four different response variables corresponding to scores in mathematics, Spanish, foreign language, and history and geography tests. We also considered the composite outcome index described in the previous section (*pc1*). In all cases, our focal parameter was β_3, which captured the impact of the regulatory change on student performance. The general hypothesis was that students would be negatively affected by a measure that reduces instruction time for most of them. However, the effect of this policy was not expected to be the same for all students. It was likely to affect those who did not have to repeat any exams more negatively. Therefore, in an extended analysis, we explored non-linear (quantile) effects across the distribution of scores in the standardized exam (Battistin and Meroni, 2016; Huebener et al., 2017). We thus modeled treatment intensity across the score distribution. More specifically, we hypothesized that treatment students at the bottom of the performance distribution would be less negatively affected by the policy than those at the top of the distribution. A possible reason for this difference is that some of the students who had to repeat their exams received classes to prepare for them. Before the school calendar change in the 2016/2017 school year, 43.6 percent of students passed the final exams in the first sitting. An additional 40.4 percent passed the final exams in the second sitting, giving a total of 84.0 percent. After the regulatory change, there was an increase in the number of students who passed the 10th grade (85.0 percent in the 2017/2018 school year).

6. ANALYSIS OF RESULTS

6.1. Control variables

Estimation of Model 1 in the previous section involves many covariates that are not the focus of our analysis. However, this information is interesting in itself because they explain what determines students' academic results. Table 2 shows the estimated impact of these control variables on scores in each competency test as well as the general index *pc1* under our baseline specification (control school year 2016/2017). The table also reports relevant information on the econometric specifications, such as the inclusion of school fixed effects. Generally, the impact of the control variables is in line with the findings in the literature. Specifically, girls have a small negative impact because their positive relative results in Spanish and English are more than offset with the negative comparative results in math and geography and history. The results by gender in Spanish and math, which are also assessed by the OECD under the PISA framework, are aligned with the results found in international tests (OECD, 2019). Students who attended pre-primary education (from 0 to 2 years) scored significantly better. This finding is consistent with previous papers on the importance of early years in acquiring both cognitive and non-cognitive skills (Felfe et al., 2015; Heckman et al., 2010). Students who repeated years either in primary or secondary school had significantly worse performance, as noted in PISA reports (PISA, 2018). Even when grade retention was included, age had a significant negative effect. This effect can be explained by the fact that some students might be behind an academic year for their age without having been retained (if school entry is delayed or if they come from other education systems). We also find evidence of a relative age effect. Ceteris paribus, students born in the first half of the year tended to perform better than those born toward the end of the year (Berniell & Estrada, 2020; Dhuey & Bedard, 2006). There is a positive monotonic relationship between the number of books at home and student performance. Hanushek and Woessmann (2011) showed that the number of books at home is a proxy for socioeconomic background, finding that the number of books at home is the variable that is most strongly correlated with academic achievement. Parents' professions also matter. For example, having parents who were technicians, associate professionals, professionals, chief executives, and senior officials increased expected academic performance. In contrast, some specific professions were significantly negatively correlated with student performance. Such situations were unemployment, basic occupations, crafts and related trades, skilled agriculture and forestry, plant and machine operation and assembly, and armed forces professions.

Table 2. Impact of control variables on academic scores in different competencies. Difference-in-differences estimation using OLS. School years 2016/2017 and 2018/19

VARIABLE	PC1	SPA	ENG	H&G	MTH
Gender	-0.054***	15.22***	13.62***	-18.66***	-27.03***
	(0.012)	(0.770)	(0.719)	(0.812)	(0.857)
Immigrant	-0.015	-5.106***	6.919***	0.763	-5.287***
	(0.023)	(1.481)	(1.382)	(1.562)	(1.648)
Pre-primary education	0.0690***	2.562***	3.193***	3.229***	4.691***
	(0.0123)	(0.806)	(0.752)	(0.851)	(0.896)
Age	-0.352***	-22.16***	-23.61***	-16.10***	-4.795***
	(0.030)	(1.939)	(1.796)	(2.050)	(2.145)
February	-0.024	0.678	-0.681	-2.211	-1.330
	(0.029)	(1.939)	(1.791)	(2.027)	(2.137)
March	-0.123***	-3.562*	-6.880***	-5.225***	-5.613***
	(0.029)	(1.873)	(1.748)	(1.977)	(2.085)
April	-0.114***	-4.550**	-5.512***	-5.361***	-5.144**
	(0.029)	(1.897)	(1.769)	(2.002)	(2.111)
May	-0.147***	-7.084***	-7.779***	-7.662***	-4.234***
	(0.029)	(1.919)	(1.788)	(2.025)	(2.135)
June	-0.176***	-8.370***	-11.03***	-7.057***	-7.442***
	(0.031)	(2.025)	(1.889)	(2.140)	(2.254)
July	-0.216***	-10.60***	-12.92***	-9.973***	-6.321***
	(0.032)	(2.060)	(1.920)	(2.174)	(2.291)
August	-0.285***	-15.60***	-17.84***	-13.46***	-6.607***
	(0.033)	(2.167)	(2.022)	(2.289)	(2.411)
September	-0.317***	-16.49***	-19.39***	-16.50***	-5.417**
	(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 does not work					
	-0.030	-2.996	-3.331	2.013	-2.549

	(0.039)	(2.548)	(2.379)	(2.690)	(2.830)
Mother basic occupations occupations	-0.036	-0.285	-3.232***	-1.683	-2.676*
	(0.022)	(1.429)	(1.333)	(1.508)	(1.590)
Mother craft and related trades	-0.072	-6.943**	-2.376	-4.389	-0.389
	(0.046)	(2.987)	(2.793)	(3.160)	(3.326)
Mother skilled agricultural and	-0.140	-10.86*	-15.78**	-6.727	-2.107
	(0.108)	(6.916)	(6.360)	(7.400)	(7.576)
Mother plant and machine operators	-0.062	-4.862	-4.035	1.655	-6.757*
	(0.050)	(3.253)	(3.046)	(3.436)	(3.681)
Mother retail, services and personal	-0.109***	-6.139***	-4.534***	-6.452***	-4.132**
	(0.023)	(1.471)	(1.373)	(1.551)	(1.632)
Mother armed forces/protection and	-0.069**	-2.031	-4.650**	-4.292*	-2.506
	(0.035)	(2.271)	(2.117)	(2.404)	(2.536)
Mother technician and professionals	0.062***	0.959	3.527**	3.180**	4.7665***
	(0.022)	(1.448)	(1.357)	(1.530)	(1.617)
Father does not work	-0.294***	-12.84**	-21.81***	-8.332	-10.62*
	(0.079)	(5.220)	(4.856)	(5.536)	(5.761)
Father basic occupations	-0.095***	-2.628	-9.208***	-0.714	-4.986*
	(0.029)	(1.866)	(1.739)	(1.971)	(2.074)
Father craft and related trades worker	-0.044**	-0.520	-7.202***	-0.483	0.345
	(0.021)	(1.396)	(1.304)	(1.474)	(1.556)
Father skilled agriculture and forestry	-0.207***	-11.58***	-15.54***	-6.377	0.150
	(0.060)	(3.898)	(3.649)	(4.111)	(4.336)
Father plant and machine operators	-0.077***	-0.190	-4.731***	-3.645**	-4.801***
	(0.023)	(1.526)	(1.422)	(1.611)	(1.694)
Father reta, services, and personal	0.059**	3.131*	-1.577	4.889***	5.544***
	(0.027)	(1.759)	(1.645)	(1.861)	(1.961)
Father armed forces/ protection and	0.024	2.567	-1.179	3.829*	-0.635
	(0.032)	(2.114)	(1.973)	(3.232)	(2.356)
Father technician and professionals	0.100***	5.292***	5.265***	4.027***	5.070***
	(0.020)	(1.302)	(1.217)	(1.375)	(1.451)
Parents post-secondary education	0.130***	4.768***	8.998***	7.363***	3.950***
	(0.019)	(1.265)	(1.179)	(1.336)	(1.405)
Parents higher education	0.415***	15.05***	27.90***	21.56***	16.06***
	(0.021)	(1.349)	(1.257)	(1.424)	(1.499)
Repeat year in secondary once	-0.284***	-10.68***	-23.67***	-16.82***	-4.378
	(0.040)	(2.560)	(2.406)	(2.738)	(2.871)
Repeat year in primary once	-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 regulatory change

Table 3 reports the estimate of the focal parameters. Two econometric specifications were considered. The first corresponded to the analysis shown in Table 3, covering the 2016/2017 and 2018/2019 academic years. The second also covered the 2017/2018 academic year. In this case, we included a specific set of dummy variables for occupations of the mother and father in 2017/2018 because these variables were defined differently that year. Regardless of the econometric specification, for the composite index, non-fee-paying school dummies were positive and significant, indicating that students from these schools did not perform worse after controlling for socioeconomic status. The 2018/2019 academic year dummy was positive and significant, indicating better performance that year. The interaction term between the academic year 2018/2019 and non-fee-paying schools was negative and significant, indicating that the regulatory change negatively affected student performance. The new regulation reduced the expected score by around 0.14 times the standard deviation (-0.212 divided by 1.5 times the standard deviation of *pc1*). This finding is remarkable given that the regulatory change only affected two weeks of classes at the end of the academic year. The regulatory change negatively affected all academic competencies, but it was only significant in Spanish and English.

By competency, the inclusion of the 2017/2018 academic year in the analysis matters. In particular, the two-week reduction effect was negative and significant for each of the disciplines once the whole sample had been considered.

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

School years included: 2016/2017 and 2018/2019

VARIABLE	PC1	SPA	ENG	H&G	MTH
Test in 2018/2019	0.146*** (0.042)	17.48*** (2.723)	2.864 (2.560)	-0.144 (2.872)	6.012** (3.034)
Public and charter school	0.254 (**) (0.114)	4.571 (3.845)	9.545 (3.625)	22.13*** (4.501)	8.215 (8.380)
Test in 2018/2019 interacted with public and 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: 2016/2017, 2017/2018, and 2018/2019					
VARIABLE	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2018/2019	0.200*** (0.0341)	15.34*** (2.086)	5.552*** (2.020)	7.233*** (2.226)	9.543*** (2.345)
Public and charter school	0.295** (0.115)	4.618 (7.077)	10.41 (6.854)	25.63*** (7.563)	10.63 (7.960)
Test in 2018/2019 interacted with public and charter school	-0.230*** (0.0352)	-16.62*** (2.151)	-5.778*** (2.082)	-8.488*** (2.295)	-6.604*** (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

An important political question regards estimation of the impact of this measure across the distribution of scores. To examine this question, we followed the approach of previous studies and performed the analysis using quantile regressions (Battistin & Meroni, 2016; Heubener et al., 2017). We used quantile regressions with fixed effects estimation following the method proposed by Machado and Silva (2019). These authors proposed an estimator that provides information on how the regressors affect the entire conditional distribution in a panel data set. Table 4 shows the estimation results for the aggregate component *pc1* for the quantiles 0.01, 0.25, 0.5, 0.75 and 0.99. The estimated coefficient associated with our focal variable, namely the test in 2018/2019 interacted with public and charter schools, shows that the effect of the intervention is more strongly negative the higher the quantile becomes. There are at least two plausible explanations for this heterogeneous effect. First, the school calendar change was expected to have the biggest effect on students who did not receive remedial education (Goodman, 2017) during the weeks between the first and second sittings of the final exam. Second, high-performing students make better use of instruction time and may therefore be more affected by a reduction in this time (Battistin & Meroni, 2016; Heubener et al., 2017).

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

School years included: 2016/2017 and 2018/2019					
Variable	Q1	Q25	Q50	Q75	Q99
Test in 2018/2019	-0.331***	0.012	0.144***	0.282***	0.623***
	(0.128)	(0.053)	(0.041)	(0.053)	(0.127)
Public and charter school	0.209	0.241	0.253**	0.266	0.298
	(0.395)	(0.164)	(0.126)	(0.164)	(0.393)
Test in 2018/2019 interacted with public and charter school	-0.081	-0.170***	-0.204***	-0.240***	-0.329**
	(0.135)	(0.056)	(0.043)	(0.056)	(0.134)
School years included: 2016/2017, 2017/2018, and 2018/2019					
Variable	Q1	Q25	Q50	Q75	Q99
Test in 2018/2019	-0.254**	0.0767*	0.200***	0.327***	0.638***
	(0.111)	(0.045)	(0.0342)	(0.0443)	(0.105)
Public and charter school	0.460	0.340**	0.295**	0.249	0.135
	(0.397)	(0.162)	(0.123)	(0.159)	(0.379)
Test in 2018/2019 interacted with public and 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 whether some specific variables could explain the estimated heterogeneity across students. In three separate models, we applied a triple difference-in-differences specification. The focal variable interacted the 2018/2019 test with non-fee-paying schools and 1) low parent education, 2) repeating a year in secondary school, and 3) choosing a strong mathematical course. However, the results were not statistically significant and are not reported.

Our last extended analysis concerned a pre-trend analysis. The underlying assumption in difference-in-differences estimation is that the difference between the control and treatment groups is due to the treatment policy. Researchers are increasingly testing trend differences between these two groups before the intervention (Kahn & Lang, 2019). We conducted a placebo test in our sample to estimate the effect of a hypothetical intervention only affecting private schools in the 2017/2018 (instead of the 2018/2019) academic year. The estimation results are shown in Table 5. The effect of this hypothetical intervention would be positive. We looked at trend differences between private and non-fee-paying schools before our estimation sample started in the 2016/2017 school year. This type of analysis was possible because Madrid participates in the international PISA scheme of the OECD with an oversample. A basic comparison of total scores in the three PISA areas of knowledge (reading, science, and mathematics) between PISA 2012 and PISA 2015 shows no significant performance trend differences in private and non-fee-paying schools. 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, 2016). Neither this trend nor the 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. The students-to-teacher ratio has had similar evolution in public and private schools in the Region of Madrid in the period analyzed. If anything, the evolution is even slightly better for the public schools with a steeper drop. The Ministry of Education shows that the students-to-teacher ratio in public schools in the Region of Madrid was of 13.6 in the 2016-17 academic year, 13.5 in 2017-18 and 13.0 in 2018-19 whereas in private schools was 14.5 in 2016-17, 14.4 in 2017-18 and 14.2 in 2018-19[1].

Table 5. Pre-trend test for the 2017/2018 academic year. Difference-in-differences estimation using OLS

School years included 2016/2017 and 2017/2018					
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2017/18	-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 2017/18 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: 2016/2017, 2017/2018 and 2018/2019					
VARIABLES	**PC1**	**SPA**	**ENG**	**H&G**	**MTH**
Test in 2017/18	-0.543***	-16.65***	-29.22***	-31.74***	-22.52***
	(0.053)	(3.239)	(3.124)	(3.448)	(3.614)
Public & Charter school	0.197*	-2.552	8.061	22.11***	8.329
	(0.114)	(7.014)	(6.787)	(7.490)	(7.886)

[1] Source: Spanish Ministry of Education and Vocational Training. https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/profesorado/estadistica/series.html

Test in 2017/18 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

The fact that instruction time is usually an endogenously selected variable in education creates a fundamental difficulty in assessing the role of instruction time in academic outcomes. In this paper, we consider a quasi-natural experiment consisting of an unexpected modification of the school calendar in the Spanish region of Madrid. This change reduced learning time by two weeks for 42.8 percent of non-fee-paying school students in compulsory secondary education who had already passed their final exams. The remaining students were unaffected by this policy. This intervention diminished the skills of students, as measured on an external standardized test. Moreover, students in the 75th percentile of the score distribution were more affected than those in the 25th percentile by 4.4 percent of the standard deviation (baseline estimate in Table 4).

Although the results presented in this paper are of general interest to education stakeholders, they may be particularly relevant for assessing the possible impact of the school closures triggered by the COVID-19 crisis. By the summer of 2020, most countries had kept schools closed for around 12 weeks of face-to-face learning, a third of the 2019/2020 academic year. It is widely accepted that this decision is causing significant disruption to students' learning. Based on previous interruption experiences, the Royal Society DELVE Initiative (2020) estimated an impact of between 6 and 10 percent of the standard deviation due to the learning loss during the 12-week lockdown. Kuhfeld et al. (2020) projected the potential effects of COVID-19 school closures on academic achievement. They estimated that students were likely to return in fall 2020 with approximately 63 to 68 percent of the learning gains in reading relative to a typical school year and 37 to 50 percent of math learning gains. To correctly interpret these findings, it should be recalled that learning gains in most national and international tests over one year are equal to between one quarter and one third of a standard deviation (Woessman, 2016). Therefore, the projection by Kuhfeld et al. (2020) of learning loses due to COVID-19 varies from a minimum of 8 percent of the standard deviation in reading to a maximum of a 22 percent in math. Despite the relatively small reduction of instruction time considered in this paper, we estimate a higher effect on academic outcomes than the effect estimated by the Royal Society DELVE Initiative (2020). Our estimates are more in line with those of Kuhfeld et al. (2020). Our baseline estimates are also significant, with an overall effect of 14 percent of the standard deviation, particularly in learning areas such as Spanish (-20.7 percent of the standard deviation) and English (-5.5 percent of the standard deviation). The effects are non-significant for math and history and geography.

Students with higher test scores were more affected by the policy. The calendar change studied in this paper may differ from the school lockdown due to COVID-19. This lockdown is expected to have a minor effect among students from advantaged backgrounds but a major effect among lagging and disadvantaged students. It seems unlikely that online education will replace the learning lost from face-to-face school classes (Bettinger et al., 2017). The wide variation in the quantity and quality of remote schooling and home learning support 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 for teaching and parents' non-cognitive skills, resources, and knowledge. These are key 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 pre-primary education attendance (aged 0 to 2 years). *Age*: student's age. Immigrant: Immigrant student. *January*: dummy variable indicating that student was born in January. Other months are defined similarly. *Books 0–10*: dummy variable indicating that total number books in student's home is in the range [0,10]. Variables *Books 11–50*, *Books 51–100*, *Books 101–200*, and *Books >200* are defined similarly. *Mother does not work*: Mother has never worked. *Mother basic occupation*: Mother has basic occupation. *Mother craft*: Mother works in the craft sector or similar. *Mother skilled agriculture*: Mother works as a skilled agricultural or forestry worker. *Mother plant operator:* Mother is a plant and machine operator or assembler. *Mother retail and services*: Mother works in hospitality, retail, or related services. *Mother security and armed forces*: Mother works in protection, security, or armed forces. *Mother technician and professionals*: Mother is a technician, associate professional, chief executive, or senior official. *Mother clerical support worker*: 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 once* are dichotomous variables indicating whether the student has repeated once or more than once in primary or secondary education.