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Do 2 weeks of instruction time matter? Using a natural experiment to estimate the effect of a calendar change on students' performance

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Abstract

This paper investigates the effect on academic performance of an exogenous educational reform that reduced the school calendar of non-fee-paying schools in the Madrid region (Spain) by approximately two weeks, leaving the basic curriculum unchanged. To identify the consequences of such a measure, we exploit the fact that it did not affect private schools (control group) and the existence of an external cognitive test that measures academic performance before and after its application in the region. We find that the reform worsened students' educational outcomes by around 0.13 of a standard deviation. This effect was especially strong in the subjects of Spanish and Mathematics. We further explored quantile effects across the distribution of exam scores, finding that the disruption had a more negative effect on students in the upper quartile than those in the lower quartile. Overall, the analysis shows a reduction in the gap across non-fee-paying schools and an increase in the gap between non-fee- and fee-paying schools.

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1 | INTRODUCTION

How do political decisions on the length of the school year affect academic outcomes? This is an important question for a range of stakeholders in education, including policymakers and school principals. Instruction time is the primary input for the formation of human capital and, not without reason, 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. The term length for primary and secondary school students differs markedly across countries. For example, children in Spain, France, and the United Kingdom attend school on average of 175, 162, and 190 days in an academic year, respectively, compared to an OECD average of 184 days (OECD, 2021).

This paper considers an unexpected reform affecting the length of the school year of ninth-grade students in the Spanish region of Madrid (*Comunidad de Madrid*) during the 2017/2018 school year. More specifically, the reform changed the resit exams date from late to early June, reducing the total instruction time for students who had already passed these exams by around 2 weeks. Passing students did not receive formal teaching but were involved in cultural activities. Two characteristics of this reform are especially relevant to identify its consequences. First, it affected only public and charter schools (treatment group) but not private schools (control group), enabling a counterfactual analysis. A second important feature is that the region of Madrid conducted standardized external exams that measured the performance of tenth-grade students before and after being affected by the reform. Based on the previous arguments, we employ a difference-in-difference (DID) approach to estimate the differential effect of this reform on affected students. We rely on the parallel trend assumption, which states that in the absence of treatment, the difference in students' performance in schools affected and not affected by this measure would be constant over time.

The relevance of this analysis is twofold. First, the case study has a specific goal in seeking to evaluate an attempt to tackle persistent problems in the Spanish education system, such as excessive grade retention, which affects more than 30% of students in compulsory education in Spain (OECD, 2021). The reform has also intended to allow families to have more accessible summer holiday breaks by eliminating resit exams in September. The same measure was adopted in other Spanish regions such as País Vasco, Navarra, La Rioja, and Cantabria and was subsequently extended to the entire country. Therefore PISA 2018 (OECD, 2020) blamed this reform for the unexpected negative score of these communities in 2018. In particular, the report explicitly noted that this negative "impact is larger on the results of the five subnational entities with early high-stakes exams."[1] Thus, this paper is the first to attempt to appraise the relevance of this measure using standardized test results in the region before and after its application.

Second, and more importantly, this analysis has more general implications. Thus, although natural experiments are helpful tools to tackle the endogeneity of variations in instruction time associated with educational reforms, generally, these decisions are publicly known and anticipated before their implementation (Pischke, 2007). Moreover, assessing the impact of these policies is also challenging as the response variable is either an endogenous decision of the affected school (e.g., exam grades) or is only observed a long time after the policy implementation (e.g., future earnings or other employment outcomes).

A novel contribution of the present study is that it is particularly well-suited to deal with the internal validity concerns discussed in the previous paragraph. In particular, as we will discuss in Section 3, the reform was unexpected and enforced in all non-fee-paying schools (our treatment group). Moreover, it occurred at the end of the academic year, leaving little scope for subsequent reactions to alleviate the negative effect of this shock. Second, the response variable was an objective standard evaluation of academic performance for the treatment and control groups. In this respect, it is particularly relevant that the dependent variable is measured in March of the year after treatment, which alleviates history and maturation threats if compared with papers that focus on students' career

[1]The OECD concludes that "Rapid and patterned responses were not uniformly present in the Spanish sample, but observed predominantly in a small number of schools in some areas of Spain" (the anomalies mentioned in Annex A9 in the Initial Report). These schools are all late-testing schools, in regions where the high-stakes tests overlapped with the end of the PISA testing window.

outcomes. Finally, we employed a student-level sample with detailed information on students' socioeconomic characteristics. The data set also enabled control for unobserved school characteristics using fixed effects.

The present article provides evidence that even a small change in instruction time (just 2 weeks) can have substantial implications for students' academic outcomes (around 0.13 times a standard deviation). The effect was especially significant in the subjects of Spanish (about 0.19 times a standard deviation) and Mathematics (around 0.08 times a standard deviation). We also explored non-linear (quantile) effects across the distribution of scores in the standardized exam, finding that the effect of the new regulation was higher in the upper quartile of the distribution. Overall, we found a reduction in the gap across students in non-fee-paying schools (public and charter schools) and an increase in the gap between non-fee- and fee-paying schools (private schools).

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 describes the data used in the analysis. Section 5 presents and discusses the empirical results. Section 6 explores how the reform has affected different quantiles of the score distribution. Section 7 presents the conclusions of the study.

2 | RELATED LITERATURE

The previous empirical literature diverges according to, among other issues, the definition of instruction time and how this variable operationalizes. Therefore, papers have exploited different meanings of variations of instruction time, such as “more hours” per day (Battistin & Meroni, 2016; Bellei, 2009; Huebener et al., 2017), absenteeism (Aucejo & Romano, 2016; Kuhfeld et al., 2020; Santibanez & Guarino, 2020), school closing days for snowfalls (Marcotte & Hemelt, 2008), and length of the school year (Parinduri, 2014; Pischke, 2007). Each of the previous definitions implies distinct learning alterations and, therefore, could affect academic performance differently. Moreover, even if homogeneously defined, variations in instruction time, like any other economic resource, could disproportionately affect learning depending, for example, on when it occurred and how it was applied.

The present paper is explicitly related to a change in the length of the school year as it estimates the consequences of an unexpected reduction of the length of the school year by 2 weeks. We can distinguish at least two empirical strategies to deal with such a question. First, early papers assessed the importance of the length of the school year using state-, country-, or district-level data (Card & Krueger, 1992; Lee & Barro, 2001; Sims, 2008). Card and Krueger (1992) estimated the impact of average term length and other measures of school quality on state levels of return to education, finding positive and significant effects. Using country-level data, Lee and Barro (2001) found that, controlling for other school quality indicators, more time in school increased mathematics and science scores but lowered scores in reading. Sims (2008) examined the impact of moving school starting dates from September to August in Wisconsin, as requested by districts with low test scores. His results indicated a small positive effect of extra classroom days that were only evident in Math scores for fourth graders and in the upper proportion of the ability distribution for third graders.

A second approach exploits exogenous variation in the number of school days resulting from quasi-experimental settings. In turn, the random allocation of classroom test dates could be a consequence of weather conditions (Hansen, 2011; Marcotte & Hemelt, 2008), the timing of the tests (Aucejo & Romano, 2016; Fitzpatrick et al., 2011), or political reforms (Parinduri, 2014; Pischke, 2007). Marcotte and Hemelt (2008) estimated the impact on test scores of fewer days of class due to snow-related school closures. Their results indicate that each day lost reduced the pass rate for third-grade math and reading assessments by more than one-half of 1%. Hansen (2011) exploited two sources of variations in instructional days consisting of (1) weather-related cancellations in Colorado and Maryland and (2) state-mandated changes in test date administration in Minnesota. He found that more instructional days have a positive impact on student performance.

Fitzpatrick et al. (2011) exploited a state policy that provided exogenous variation in the number of school days before testing during kindergarten. According to their estimation, an additional day between tests increases children's math and reading scores by about 1.5 standard deviations with the passage of a school year. The identification strategy in Aucejo and Romano (2016) builds on quasi-randomness in the timing of standardized tests in North Carolina due to a state policy. A relevant aspect of their analysis is the joint estimation of the impact of absences and school calendars on test score performance. Their estimates suggest that extending the school calendar by 10 days increases math and reading scores by only 1.7% and 0.8% of a standard deviation, whereas absences have a higher impact.

Although our study focuses on a different type of instruction time interruption, it shares some similarities with the papers discussed in the preceding two paragraphs. In particular, treatment was also unexpected, and its effect on performance was measured from tests administered after students had been exposed to treatment using test scores. However, a relevant difference is that, rather than considering different interruption times for schools exposed to various weather events or testing dates, instruction time and the control groups are homogeneously defined in this paper. Furthermore, an interruption at the end of the school year could be helpful to deal with internal validity concerns as it is also more challenging to modify *ex-post* school resources to alleviate its negative consequences.

Among the papers that utilized educational reforms as their identification strategy, Pischke (2007) studied the West German short school years introduced in 1966–1967, which exposed some students to around two-thirds of a year less schooling during primary school. He found that the reform increased grade repetition and reduced the number of students attending higher secondary school. Parinduri (2014) considered a hastily implemented rule that assigned Indonesian students to a longer school year in 1978–1979, exposing many students to six additional months of schooling. His results show that this reform increased the probability of working in the formal sector and wages in later life. Like the previous two papers, our research estimates the consequences of an educational reform affecting the length of the school term. Furthermore, similar to Parinduri (2014), this reform was hastily implemented. However, unlike this literature, we focus on the short-term effect of instruction time variation on the scores from a standardized test. The nature of treatment is also different; in our case, it only lasted around 2 weeks and occurred at the end of the academic year.

3 | BACKGROUND

Although the overall framework and guidelines are defined nationally in Spain, most schooling decisions and funding are determined at the regional level. There are three main categories of schools: state-funded, charter, and private. Although the state funds the first two for compulsory education, the latter is privately funded. Here, we focus on ninth-grade students (aged 14–15 years) in the Madrid region. Many of these students were affected by the change in the school calendar analyzed in this paper. Furthermore, they took an external standardized test in 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 schools). In the 2017/2018 school year, the region of Madrid changed the timing of the second sitting of the final exam, leaving the curriculum unchanged. This exam is a chance to retake the exam for students who did not pass 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 in planning for the start of the school year, make family life easier during the summer break, and reduce family spending on private tuition (Madrid Regional Ministry of Education, 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, 2 weeks earlier than in previous academic years. For these 2 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 cultural activities such as museum visits, social volunteering, and debate tournaments. Regardless of whether they had passed their exams, all students still had to attend school. The school calendar must consist of a minimum of 175 school days for compulsory education by Spanish law (Organic Law on Education, LOE).

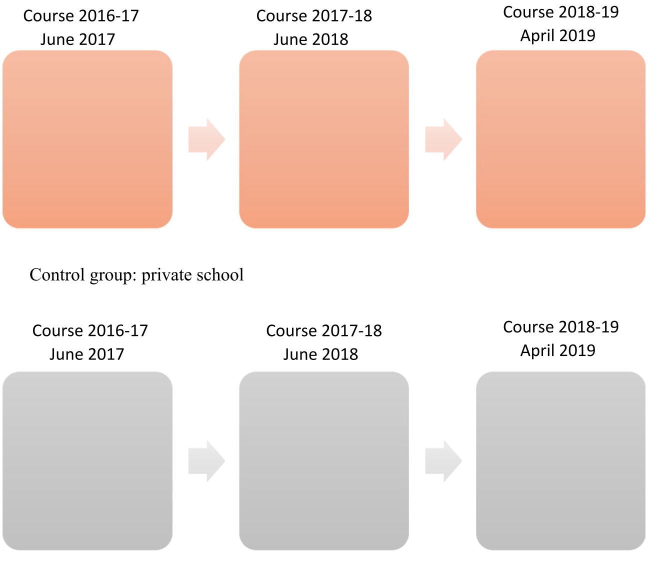

FIGURE 1 Effect of the regulatory change on different types of schools in the Madrid region. [Colour figure can be viewed at wileyonlinelibrary.com]

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. However, by that time, students could not move from one school to another as the application period to participate in the regular admission process in schools for the next academic year took place between April 19 and May 5, 2017 (i.e., before the regulatory decision).

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

Moreover, the Madrid Regional Education Council (2018) carried out 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. 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 2 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.[2] As a reaction to the problem of absenteeism, the Education Department of the Community of

[2]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.

Madrid approved an action plan from the General Inspection Sub-Directorate to ensure that educational centers were prepared with training activities in June of the 2018–2019 academic year.[3]

The previous discussion suggests that the reform negatively affected the length of instruction time for students who passed all the subjects. Although we cannot identify these students because exam grades are private information, they represent a slightly smaller proportion of the total sample than the students who failed at least one subject. In particular, before the reform, 43.6% of students passed the final exams in the first sitting. An additional 40.4% passed the final exams in the second sitting, giving a total of 84.0%. After it, there was an increase in the number of students who passed the 10th grade (85.0% in the 2017/2018 school year). We cannot identify the students who decided not to attend school or participated in different activities rather than receiving formal instruction. Based on the previous information, we anticipate that the reform had a negative effect on many students. We also anticipate this effect to be more prominent in those with higher cognitive abilities in the standardized tests as these students are more likely to have passed their exams in the first sitting.

4 | DATA

4.1 | External exam

We utilized 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 15–16 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) and to parents or guardians (who were sent a report of their children's scores) and to 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 min. 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 already described are the dependent variables in our analysis. We provide specific estimations for each competency. However, because our primary interest is overall student performance, not particular competencies, we also performed a principal component analysis of the dimension of the problem. We found that the first component explained slightly over 60% of the variability in the four variables. Moreover, all weights of the variables were positive and similar. These results suggest that this combination average the scores in the four competency tests.[4] We denote this combination *pc1*.

[3]See Official Bulletin of the Community of Madrid, 11 October 2018. (https://www.bocm.es/boletin/CM_Orden_BOCM/2018/10/11/BOCM-20181011-20.PDF): *7.3.1. Guidance for school management teams in the development of the work plan for the month of June and its inclusion in the Annual General Programming of the 2018–2019 school year, and verification of its level of implementation at the end of the school year*.

[4]See, for example, Lavy (2020) for an example of the use of score averages as response variable.

TABLE 1 Descriptive statistics. Individual characteristics of students in public and charter schools (treated group) and private schools (control group).

	Private schools			Public and charter schools		
	2016–17	2017–18	2018–19	2016–17	2017–18	2018–19
Total students	5038	4550	5401	51,138	47,968	57,593
Female students	49.2	48.1	47.5	49.9	50.2	50.2
Academic mathematics	99.5	99.7	99.2	85.7	89.1	82.7
Immigrant	6.2	8.7	3.3	13.4	10.5	11.3
Early childhood education	66.0	71.6	70.2	54.0	62.5	57.5
More than 200 books	45.6	49.4	41.9	26.4	32.0	22.7
Parents with high education	81.2	92.0	81.8	46.0	64.6	46.5

Note: Total students are the number of students in the sample. All the remaining variables are expressed in percentage terms.

Although our analysis controls for years and school-specific effects, we also include covariates available for the whole analysis period. It should be noted that there is a more extended set of variables only observed in two academic years (2016/2017 and 2018/2019). Thus, some variables such as students' age and birth month were missing. We could ascertain whether the student repeated the year. Furthermore, although we had an indication of each parent's labor force status (fully employed, partially employed, unemployed, or inactive), we had limited information on their occupation. These variables are defined in the appendix. Moreover, in Section 5.3, we study the robustness of our results by considering the additional variables defined for only 2 years in the analysis.

Focusing on the covariates available for the whole analysis period and 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 the innate characteristics of students. We employed dichotomous variables for female gender and immigration status. The second group of variables consisted of the student's family background, including indicators of the number of books at home and parents' educational attainment. The third group of variables consisted of institutional aspects such as Academic mathematics (captured by whether the student followed a strong math academic course or a more applied math course) and Early childhood education (0–2 years old).

We defined the treatment and control groups used in the DID analysis using the following information. Before treatment, the treatment group contained 10th-grade public and charter school students who took the standardized regional exam in the 2016/2017 and 2017/2018 academic years. 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.

Table 1 reports descriptive statistics for some relevant covariates in the treatment and control groups before and after the reform.[5] In the 2017/2018 school year, the code used in the school- and family-level database differed from that used in the student-level database. The only available information for 2017/2018 was the data provided by the students.[6] As we will show later in Section 5.3 and in Table A1, excluding the 2017–2018 academic year has no material effect on the conclusions.

[5]The only available information for 2017/2018 school year is the data provided by the students. Students might not have a precise idea of whether their parents finished their higher education studies, even when they started their university studies. Moreover, students might not know if they started early childhood education before they were 2 years old. Estimations using the information provided by students for the whole period (2016/2017, 2017/2018, and 2018/2019) in Table A2, show the same conclusions as in Section 5.

[6]The only available information for the 2017/18 school year is the data provided by the students. It should be noted that students may not have an accurate understanding of whether their parents completed higher education, even if they began their university studies. Additionally, students may not have precise knowledge of whether they started early childhood education before the age of 2. Estimations based on the information provided by students for the entire period (2016/17, 2017/18, and 2018/19) in Table A2 yield the same conclusions as in Section 5.

Students in private schools and public and charter schools differed in several individual and social characteristics. The immigrant population was lower in private schools. Moreover, on average, students' families in private schools had more books at home and higher educational attainment.

All these differences suggest a need to control for these characteristics and school characteristics.

5 | EMPIRICAL ANALYSIS

5.1 | Methodology

We employed a DID approach to investigate the reform described in Section 3 on students' performance in the Spanish region of Madrid. The underlying assumption under this method is that, once we control for individual characteristics and fixed school effects, the change in score differences between students in public and charter schools (treatment group) and private schools (control group) in 2017/2018 was due to the effect of the reform.

We used least-squares estimation of the following two-way fixed effects model:

$$Y_{i,j,t} = \beta_0 + \beta_1 \left(D_{PC} * T_{2018/2019}\right) + \sum\nolimits_{k=1}^{K} \gamma_k X_{i,j,t} + \eta_j + \delta_t + \varepsilon_{i,j,t}, \tag{1}$$

where the response variable, $Y_{i,j,t}$, is the score in the standardized exam by student i, school j, and academic year t; D_{PC} is a dummy variable that takes the value of 1 for schools affected by the reform (i.e., public and charter schools) and 0 otherwise; $T_{2018/2019}$ takes the value of 1 if the observation belongs to the 2018/2019 academic year and 0 otherwise; $X_{i,j,t}$ are observed individual characteristics, school characteristics, family background, and institutional aspects; β_0, β_1, and γ_k for $k = 1$ to K are parameters to be estimated; η_j is a school fixed effect; δ_t is a time effect for the academic year; 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 included the composite outcome index described in the previous section (*pc1*). In all cases, our focus parameter is β_1, which represents the differential effect of the reform on students attending non-fee-paying schools conditional to the controls included in the specification.

5.2 | Effect of the reform

Table 2 shows the estimation results of Model 1. Although the econometric specification includes school-specific effects (776 schools), these indicators are fixed throughout the analysis period. Thus, to account for the impact of community changes, we also include each individual variable aggregated at the school level in the group of covariates. Therefore, the estimated specification allows academic performance to react not only to individual changes but to changes in community circumstances. Moreover, in a nonreported experiment, we tested the importance of students and family characteristics by estimating model 1 without control variables but including fixed school effects with no material change on our conclusions about the impact of the reform. Generally, the estimated effect of the control variables is in line with the findings in the literature. Specifically, girls perform slightly worse because their positive relative results in Spanish and English are more than offset by the negative comparative results in math and geography and history. The results by gender in Spanish and mathematics, which are also assessed by the OECD under the PISA framework, are aligned with the results found in international tests (OECD, 2019). Academic mathematics positively affects all the competencies except mathematics. The probable explanation is that these students face a more demanding mathematics exam.

TABLE 2 Estimated effect of the regulatory change on academic scores in different competencies.

Variables	Composite index[a]	Spanish	English	Mathematics	History and geography
Female student	−0.05***	13.20***	11.99***	−22.19***	−16.56***
	(−0.009)	(−0.542)	(−0.538)	(−0.632)	(−0.596)
Academic mathematics	0.93***	59.63***	70.08***	−25.70***	56.21***
	(−0.018)	(−1.062)	(−1.056)	(−1.24)	(−1.17)
Immigrant	−0.15***	−9.51***	−2.78***	−9.72***	−5.10***
	(−0.016)	(−0.954)	(−0.948)	(−1.114)	(−1.05)
Early childhood education	0.10***	3.88***	5.06***	6.65***	4.00***
	(−0.010)	(−0.566)	(−0.563)	(−0.661)	(−0.624)
Books 11–50	0.37***	16.39***	18.56***	17.66***	15.81***
	(−0.026)	(−1.513)	(−1.504)	(−1.766)	(−1.666)
Books 51–100	0.57***	24.39***	30.21***	25.24***	25.52***
	(−0.026)	(−1.526)	(−1.517)	(−1.782)	(−1.681)
Books 101–200	0.77***	32.26***	40.81***	34.96***	35.13***
	(−0.027)	(−1.557)	(−1.548)	(−1.819)	(−1.72)
Books >200	0.95***	39.05***	48.60***	44.00***	44.84***
	(−0.027)	(−1.556)	(−1.547)	(−1.817)	(−1.713)
Parents' education2[b]	0.17***	5.758***	11.85***	6.71***	7.43***
	(−0.015)	(−0.893)	(−0.888)	(−1.042)	(−0.983)
Parents' education3[b]	0.53***	17.97***	35.04***	21.89***	23.62***
	(−0.016)	(−0.918)	(−0.913)	(−1.072)	(−1.011)
Prop Female students school[c]	−0.12	−7.23	−8.82*	−7.94	2.01
	(−0.092)	(−5.376)	(−5.345)	(−6.278)	(−5.922)
Prop Academic mathematics school[c]	−0.38***	1.496	−39.01***	−25.75***	−9.40
	(−0.103)	(−6.03)	(−6.00)	(−7.042)	(−6.642)
Prop Immigrants school[c]	−0.23**	7.05	−8.73	−29.47***	−15.41**
	(−0.100)	(−5.857)	(−5.824)	(−6.839)	(−6.451)
Prop Early childhood education school[c]	−0.11	−11.72***	4.76	−5.21	−6.34
	(−0.067)	(−3.961)	(−3.938)	(−4.625)	(−4.363)
Prop Books 11–50 school[c]	0.45***	6.53	3.38	23.79**	50.67***
	(−0.156)	(−9.145)	(−9.093)	(−10.680)	(−10.07)
Prop Books 51–100 school[c]	0.43***	13.38	10.69	34.87***	22.54**
	(−0.161)	(−9.440)	(−9.386)	(−11.020)	(−10.4)
Prop Books 101–200[c]	0.54***	9.09	19.84**	34.02***	39.65***
	(−0.164)	(−9.632)	(−9.576)	(−11.250)	(−10.61)
Prop Books >200[c]	0.72***	27.81***	22.62**	31.39***	52.08***
	(−0.163)	(−9.554)	(−9.499)	(−11.160)	(−10.52)
Prop Parents' education2 schools[c]	0.14	22.93***	11.61*	−13.23*	−1.07
	(−0.109)	(−6.408)	(−6.372)	(−7.483)	(−7.058)
Prop Parents' education3 school[c]	−0.007	7.09	−0.060	−10.68	0.18
	(−0.109)	(−6.426)	(−6.389)	(−7.504)	(−7.078)

TABLE 2 (Continued)

Variables	Composite index[a]	Spanish	English	Mathematics	History and geography
2018/2019 Interacted with public and charter schools	−0.19*** (−0.031)	−19.46*** (−1.802)	−3.29* (−1.792)	−7.18*** (−2.105)	−3.40* (−1.985)
Observations	76,757	76,757	76,757	76,757	76,757
R^2	0.131	0.102	0.15	0.059	0.092

Note: Difference-in-difference regression. School years 2016/2017, 2017/2018, and 2018/2019. The model specification includes 775 school fixed effects and two academic year effects corresponding to 2017/2018 and 2018/2019. The following categories were omitted to avoid perfect collinearity: 2016/2017 school year, Books 0–10; and Parents' education1 (father or mother achieved qualification up to compulsory secondary school.

[a] The composite index is an average of scores in Spanish, English, Mathematics, and History & Geography using principal components.

[b] Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and undergraduate studies, respectively.

[c] Variables are averaged at the school level.

***p < .01, **p < .05, and *p < .1. Standard errors in parentheses.

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). There is a positive 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. Also, as expected, indicators of parents' education are positively correlated with students' scores.

Variables aggregated at the school level also significantly impact performance in some cases. Thus, the proportion of students with Academic mathematics and the proportion of books at the school level exerts a positive spillover on individual student performance. On the contrary, the proportion of school immigrants is associated with lower individual performance.

When we turn our attention to the interaction term between the academic year 2018/2019 and non-fee-paying schools, its associated parameter is negative and significant at conventional levels. More specifically, the reform reduced the expected score by around 0.13 times a standard deviation (−0.19 divided by 1.5 times the standard deviation of the composite index). This finding is remarkable given that the regulatory change only affected 2 weeks of classes at the end of the academic year. The calendar change negatively affected all academic competencies in a significant way. By competency, the effect was negative and significant for each discipline. In particular, because the standard deviation of the scores in each competence is 100, the estimated negative effects range from a 3% decrease to a 19% decrease in a standard deviation in English and Spanish, respectively.

The underlying assumption behind our difference in difference design is that the observed trend of the score variable is the same for private and charter schools and public schools before the reform. Therefore, we first visually explore whether the trajectory of the response variable for the two types of schools was parallel before the implementation of the reform. Figure A1 shows the observed score means and the results of the linear-trends model. For each subject and for the composite index, the figure has two diagnostic plots for assessing the parallel-trend assumption, which is required for consistent estimation of the average treatment effect on the treated. The first plot in each subject consists of two lines showing the mean of the outcome over time for students in public and charter schools (treated group) and for students in the private (control group). The second plot augments the DID model to include interactions of time with an indicator of treatment and plots the predicted values of this augmented model for students in the public and charter schools and for students in the control group. Both plots include a vertical line when the treatment took place, that is between the 2017–2018 and 2018–2019 academic years.

TABLE 3 Parallel-trend test (pre-treatment period) and Granger causality test for no effect in anticipation of treatment.

	PC1[a]	Spanish	English	Mathematics	History and geography
F(1,1)[b]	3.65	6.24	3.10	11330.22	2.52
p-value[b]	0.307	0.243	0.329	0.01	0.358

[a]The composite index is an average of scores in Spanish, English, Mathematics, and History & Geography using principal components.
[b]Wald tests applied to linear-trends models that estimate a coefficient for the differences in linear trends before the reform. This model contains the set of covariates considered in the DID model.

Consistently with Section 4, although mean scores are higher in private schools for the four competencies and the composite index, the pre-trend evolution of treatment and control groups seem to be parallel in most cases. Therefore, using our set of confounders, we conduct a more formal test of parallel-trend assumption following Angrist and Pischke (2009, pp. 238–239).[7] Estimation results are shown in Table 3. With the sole exception of mathematics, the parallel-trend assumption is not rejected at the conventional values for the different competencies and the composite index. However, as observed in Figure A1, even if there is any trend difference at the time of the reform, it favors the treatment group. This difference is reversed in the post-treatment period.

We further investigate the possible presence of a pre-trend effect by looking 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. See Lavy (2020) and Battistin and Meroni (2016) for examples in the recent literature of considering a different sample to explore a pre-trend effect. 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).

Furthermore, we do not observe relevant changes in the student-to-teacher ratios before the treatment. In particular, according to the Ministry of Education, these ratios in public schools in the Region of Madrid were 13.6, 13.5, and 13.0 in the 2016/2017, 2017/2018, and 2018/2019 school years, respectively. In contrast, in private schools, these ratios were 14.5, 14.4, and 14.2 for the same years.[8] Again results indicate that non-fee-paying schools were improving even more than private schools, suggesting that our estimation results are not likely to be due to any previous pre-trend.

5.3 | Robustness exercises and transmission channels

5.3.1 | Omitted variables

An essential characteristic of the estimation results reported in the previous section is that they control for unobserved school and time heterogeneity, employing school-, and time-specific effects as well as a set of individual confounders. However, this could not be enough as estimation results could still be biased due to omitted confounders affecting the composition of treated and control groups. We focus on an additional set of variables that

[7]The test for no anticipatory effects is also advocated in Angrist and Pischke (2009). It consists of augmenting the model with a dummy for future treatment status and testing if the coefficient on this dummy is equal to 0. However, in a model with only two periods prior to treatment, these results do not differ from a parallel-trend test.
[8]*Source*: Spanish Ministry of Education and Vocational Training. https:ww.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/profesorado/estadistica/series.html

cannot be considered in the previous analysis as they are only available for the 2016–2017 and 2018–2019 academic years. As an alternative, we follow the suggestion of Pei et al. (2019) to include the less informative confounders on the left-hand side of the regression to conduct the balance test. We take such advice given that bias due to omitted variables is only present when they are correlated with both the outcome and the causal variable of interest.

More specifically, we estimate the following specification using the 2016–2017 and 2018–2019 academic years[9]:

$$O_{i,j,t} = \beta_0' + \beta_1'\left(D_{PC} * T_{2018/19}\right) + \sum_{k=1}^{K} \gamma_k{}' X_{i,j,t} + \eta_j{}' + \delta_t{}' + \varepsilon_{i,j,t}', \tag{2}$$

where $O_{i,j,t}$ is an omitted variable in Regression (1). Our focus parameter is $\beta_1{}'$, which indicates a compositional change in treatment due to the omitted variable.

Table 4 reports the estimation results of Model (2) using indicators of repetition in previous years and whether the student's father or mother was unemployed. None of the foci estimated parameters are significantly different from 0 at the 5% significant level.

An alternative approach to test the influence of the omitted variables is to include them on the right-hand side of a DID specification defined for the 2016–2017 and 2018–2019 academic years, the so-called right-hand side test in Pei et al. (2019). We conducted this estimation, including in the regression all the variables defined in the appendix as well as fixed school effects (Table A1). Still, the estimated effect for the composite index is −0.18 (with 0.04 standard error), which is qualitatively and even quantitatively similar to the estimation results in the previous section. Therefore, overall, we also find a negative effect of the reform on students' performance, even after controlling for all the variables in the analysis.

The previous analysis does not study the role of unobserved omitted variables. Note that omitted variables can only affect the DID estimand if they explain the treatment decision even after controlling by observable covariates (Corollary 3 in Diegert et al., 2022). This is a strong assumption in our setting as the treatment variable is a political decision defined at the regional level (and implemented in other Spanish regions), whereas observable controls (defined at the student level) include school dummies (776 schools), gender, parents' education, attendance of pre-primary education, and the number of books at home.

Despite the previous arguments, to further assess the role of omitted variables, we use the method proposed by Diegert et al. (2022) on account of its generality, as it allows omitted variables and observable controls to be correlated. Thus, Diegert et al. (2022)'s test is not based on a universal threshold for what is or is not a robust result. Instead, it allows the research to evaluate results in terms of their interpretation of the correlation between omitted and observable variables. In this study, an obvious omitted variable is student ability, which we assume to be correlated with parents' education and the different indicators of the number of books at home. Based on these hypotheses, the beta parameter is robust (corrected delta of 96.4 with $R^2{}_{max} = 1$). Following Diegert et al. (2022), we also report the estimated selection of unobservables relative to observables needed to overturn the findings. This is 99.4%, well above the 50% rule of thumb (Diegert et al., 2022).

5.3.2 | Separate effects for charter and public schools

Although both public and charter schools were affected by the reform, whether the magnitude of the impact was different for the two types of schools constitutes a relevant empirical question. To study this issue, we extend Model (1), allowing for two separate interactive effects for charter and public schools. As discussed in Section 3, although

[9]We thank Jorn-Steffen Pischke for providing very helpful advice on how to conduct the test in our specific case.

TABLE 4 Left-hand side test in Pei et al. (2019).

Variables	Repeat in primary education	Repeat in secondary education	Mother unemployed	Father unemployed
Female students	−0.000871 (0.00218)	−0.0447*** (0.00349)	−0.00301** (0.00133)	−0.00110 (0.000669)
Academic mathematics	−0.287*** (0.00416)	−0.651*** (0.00616)	−0.00386* (0.00234)	−0.00691*** (0.00118)
Immigrant	0.0730*** (0.00391)	0.100*** (0.00614)	0.00767*** (0.00232)	0.00667*** (0.00118)
Pre-primary education	−0.0159*** (0.00226)	−0.0230*** (0.00363)	−0.0184*** (0.00138)	−0.000739 (0.000696)
Books 11–50	−0.0246*** (0.00533)	−0.00785 (0.00827)	−0.0177*** (0.00314)	−0.00629*** (0.00160)
Books 51–100	−0.0404*** (0.00540)	−0.0345*** (0.00840)	−0.0248*** (0.00319)	−0.0109*** (0.00162)
Books 101–200	−0.0436*** (0.00555)	−0.0531*** (0.00866)	−0.0267*** (0.00329)	−0.00968*** (0.00167)
Books >200	−0.0472*** (0.00555)	−0.0577*** (0.00868)	−0.0256*** (0.00330)	−0.0100*** (0.00167)
Parents' education2[a]	−0.0588*** (0.00342)	−0.0404*** (0.00538)	−0.0328*** (0.00204)	−0.00965*** (0.00104)
Parents' education3[a]	−0.0867*** (0.00355)	−0.118*** (0.00565)	−0.0366*** (0.00215)	−0.0104*** (0.00109)
Prop Female students school[b]	0.0238 (0.0258)	−0.0374 (0.0415)	−0.0146 (0.0157)	0.0115 (0.00794)
Prop Academic mathematics school[b]	0.118*** (0.0286)	0.266*** (0.0455)	−0.0194 (0.0173)	0.00928 (0.00875)
Prop Immigrants school[b]	−0.0262 (0.0305)	0.0468 (0.0485)	0.0274 (0.0184)	0.00364 (0.00933)
Prop Early childhood education school[b]	0.0222 (0.0201)	−0.00642 (0.0324)	0.0267** (0.0124)	1.26e-05 (0.00624)
Prop Books 11–50 school[b]	0.0477 (0.0465)	−0.0525 (0.0740)	−0.0760*** (0.0281)	−0.0485*** (0.0143)
Prop Books 51–100 school[b]	0.0145 (0.0450)	−0.0397 (0.0716)	−0.0334 (0.0273)	−0.0611*** (0.0138)
Prop Books 101–200 school[b]	0.00705 (0.0456)	−0.167** (0.0727)	−0.0752*** (0.0277)	−0.0499*** (0.0140)
Prop Books >200[b]	0.0132 (0.0446)	−0.141** (0.0712)	−0.0669** (0.0272)	−0.0447*** (0.0137)
Prop Parents' education2 schools[b]	−0.0923*** (0.0302)	−0.0210 (0.0481)	0.0354* (0.0184)	−0.00106 (0.00927)
Prop Parents' education3 school[b]	−0.0633** (0.0301)	−0.0500 (0.0482)	0.0369** (0.0183)	−0.00870 (0.00927)
2018/2019 Interacted with public and charter school	−0.00139 (0.00735)	−0.00666 (0.0121)	0.00725 (0.00459)	−0.000607 (0.00230)
Constant	1.365*** (0.0475)	1.861*** (0.0754)	0.127*** (0.0286)	0.0681*** (0.0145)
Observations	59,356	63,185	62,742	61,888
(Overall) R^2	0.117	0.194	0.014	0.007

Note: This regression only includes the 2016–2017 and 2018–2019 academic years.
[a]Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and undergraduate studies, respectively. The model specification includes 770 school fixed effects and academic year effects corresponding to 2018/2019. The following categories have been omitted to avoid perfect collinearity: 2016/2017 academic year, Books 0–10, and Parents' education1.
[b]Variables are averaged at the school level.
$^{***}p < .01$, $^{**}p < .05$, and $^{*}p < .1$. Standard errors in parentheses.

the reform was hastily implemented in all cases, we hypothesize that charter schools are guided by private incentives and could react to the new situation in a more flexible manner. Consistently with this hypothesis, for the composite index, the reform had an impact of −0.315 (significant at a 1% level) and −0.069 (significant at a 1% level) for public and charter schools, respectively. Moreover, in public schools, the harmful effect of the reform was significant at the conventional levels in the four competencies. In contrast, in charter schools, it only had a significantly negative impact in Spanish (see Table A3).

5.3.3 | Heterogenous effects across students' attitude

The main result that stands out from the analysis in the previous section is the negative impact of the reform on academic performance. However, we can hypothesize that students with a worse attitude are less adaptable and, therefore, will be especially harmed by the reform. To better explore this idea, we estimated how unjustified absences and time devoted to homework explain the heterogeneity of treatment impacts by means of a triple DID specification (see Table A4). In the baseline case, the focus variable is the interaction between "2018/2019" and "non-fee-paying schools." In the triple DID, this term itself also interacted with (1) more than 6 days of unjustified absences throughout the quarter and (2) less than 3 h of homework in a week. These two variables can be deemed as proxies for students' (or families') attitudes. Consistently with our intuition, the reform affected more negatively the less engaged students. In particular, for the composite index, we found that the impact of the triple interaction is significant at a 1% level and of magnitudes of −0.246 and −0.065 for those with less than 2 days of unjustified absences and less than 3 h of homework per week, respectively.

6 | QUANTILE TREATMENT EFFECTS

Given the characteristics of the reform, it is expected to have affected students who did not have to repeat any exams more negatively. However, although our database does not contain information on school exam results, it is still possible to explore non-linear (quantile) effects across the distribution of scores in the standardized exam (Battistin & Meroni, 2016; Huebener et al., 2017). Therefore, we hypothesize that treatment students at the bottom of the performance distribution would be less negatively affected by the reform than those at the top. A possible reason for this difference is that students who had to repeat their exams are more likely to receive low scores on standardized tests. Thus, in the first approach, we estimate quantile treatment effects under the assumption that changes in scores at a particular quantile would have been the same in public, charter, and private schools without the calendar change (Athey & Imbens, 2017). Therefore, we estimate the effect of the reform at quantile τ using the following model:

$$Q_{Y_{i,j,t}}\left(\tau \mid D_{PC} * T_{2018/2019}, X_{i,j,t}, \delta_t, \eta_j\right) = \beta_0(\tau) + \beta_3(\tau)\left(D_{PC} * T_{2018/2019}\right) + \sum\nolimits_{k=1}^{K} \gamma_k(\tau) X_{i,j,t} + \delta_t(\tau) + \eta_j(\tau). \quad (3)$$

Note that the presence of school fixed effects could be problematic in this setting as quantile regressions suffer from the incidental parameter problem (Lancaster, 2000; Neyman & Scott, 1948). Machado and Santos Silva (2019) proposed a way to circumvent this concern by applying the method of moments–quantile regression to a location-scale model for panel data. They show that estimation bias can be removed using a jackknife and present two illustrations of the application of this approach to real data. For robustness, we also estimated a standard quantile regression model without school-fixed effects but with standard errors clustered by schools.

Given that Model (3) hypothesizes that a particular quantile would be the same in the absence of the reform, we consider an alternative approach with a less restrictive identification assumption. In particular, we applied the method of recentered influence function (RIF) developed by Firpo et al. (2009) and used it in a difference in difference context (RIF-DID) by Havnes and Mogstad (2015) and Huebener et al. (2017). Rather than estimating the quantile of the dependent variable, the RIF-DID looks at the fraction of students whose score is below a given quantile and compares this fraction in the control and treatment groups before and after the reform took place. Huebener et al. (2017) explained that RIF-DID estimates can be obtained in a two-step procedure. The first step dichotomizes the outcome variable at each quantile according to

$$RIF\{Y_{i,j,t},\tau\} = Q_\tau + \frac{\tau - 1\{Y_{i,j,t} < Q_\tau\}}{f_Y(Q_\tau)}, \tag{4}$$

where $RIF\{Y_{i,j,t},\tau\}$ is the transformed score of subject $Y_{i,j,t}$ at quantile τ, Q_τ is the unconditional score at quantile τ, and $1\{\}$ indicates if the score is below quantile τ. Finally, $f_Y(Q_\tau)$ denotes the density of Y around Q_τ. The second step consists of a DID style regression using $RIF\{Y_{i,j,t},\tau\}$ as a dependent variable.[10]

Note that the RIF-DID also relies on a strong assumption such as, in the absence of the reform, the fraction of students who score below a certain quantile τ is the same in the control and treatment groups. However, Havnes and Mogstad (2015) argued that identification assumptions in the RIF-DID model are less restrictive, especially if there are pretreatment differences between the treatment and the control groups and changes in the outcome are a function of the outcome level.

Table 5 shows the estimation result of the focus parameter (test in 2018/2019 interacted with public and charter schools) using the approaches described in the previous paragraphs for the quantiles 0.01, 0.05, 0.10, 0.25, 0.5, 0.75, 0.90, 0.95, and 0.99. For the sake of brevity, we only report results for the aggregate component *pc1*. Results indicate that the reform had a more negative effect on higher performing students and that the reform did not even have a positive effect on the scores of students at the bottom of the distribution. This is relevant because this suggests that, even for this group, the remedial education they received in June did not compensate for the loss of time over the summer preparing for a September exam. However, the reform affected more negatively those students in the upper quantiles who were more likely to be affected by the reduction in the length of instruction time.

7 | DISCUSSION AND CONCLUDING REMARKS

The fact that instruction time is usually an endogenously selected variable in education creates a fundamental difficulty in assessing its role 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 2 weeks for 42.8% of non-fee-paying school students in compulsory secondary education who had already passed their final exams. The instruction time of the remaining students was unaffected by this policy. We conclude that this intervention diminished students' skills by around 0.13 of a standard deviation, as measured by an external

[10]This regression analysis was conducted using the command uqreg in Rios-Avila (2020). We applied bootstrapped standard errors to allow for clustering at the school level.

TABLE 5 Estimated effect of the regulatory change on academic scores in different quantiles.

	Quantile DID[a]	Quantile DID[b]	RIF-DID[c]
Q1	−0.144 (0.105)	0.277 (0.302)	0.458 (0.480)
Q5	−0.159* (0,088)	−0.182 (0.278)	0.003 (0.247)
Q10	−0.166** (0,074)	−0.346** (0.172)	−0.153 (0.218)
Q25	−0.177** (0.071)	−0.448*** (0.120)	−0.289*** (0.140)
Q50	−0.189*** (0.066)	−0.513*** (0.123)	−0.360*** (0.087)
Q75	−0.202*** (0.075)	−0.597*** (0.125)	−0.334*** (0.100)
Q90	−0.213** (0.083)	−0.591*** (0.126)	−0.361*** (0.100)
Q95	−0.220** (0.091)	−0.567*** (0.181)	−0.383*** (0.088)
Q99	−0.233** (0.103)	−0.948*** (0.260)	−0.388 (0.121)

Note: Difference-in-differences estimation for the composite index PC1. School years 2016/2017, 2017/2018, and 2018/2019.
[a]Quantile DID models were estimated using the approach proposed in Machado and Santos Silva (2019) with school fixed effects and clustered standard errors.
[b]Quantile DID models were estimated using standard quantile regression (without fixed effect) but clustering standard errors by schools.
[c]RIF-DID was estimated using the command uqreg in Rios-Avila (2020) using bootstrap standard errors to allow for clustering at the school level. The three models consider all the available observations and covariates for the academic years 2016/2017 to 2018/2019.
***$p < .01$, **$p < .05$, and *$p < .1$. Standard errors in parentheses.

standardized test. The impact of regulatory changes in public and charter schools has further increased the gap between these schools and private schools by one-sixth. As of 2019, the difference between non-paying (public and charter) schools and private schools is higher than would be expected in the absence of the reform.

Although affected students could not be identified, results are consistent with the hypothesis that the reform exerted a higher effect on high-performing students, that is, those more likely to be affected by the reduction in instruction time.

Of course, the proposed research is not without limitations. The first one concerns the ambiguous meaning of the treatment, which is different for failing and passing students. We have dealt with this issue by exploring non-linear (quantile) effects across the distribution of scores in the standardized exam, given that passing students (those affected by the reduction in instruction time) are more likely to belong in the upper quartile of this distribution. Consistently with this hypothesis, our results indicated that the negative effects of the new regulation were more pronounced in the upper quartile of the score distribution compared to the lower. However, our analysis in Section 5.3 also shows that other students' characteristics are also relevant to understand the effect of the reform. In particular, it affected more negatively those students with a worse attitude (i.e., those who devoted less time to homework and with more unjustified absences). Secondly, the reform could affect schools' and students' strategies in ways that observable variables cannot capture. For example, one could argue that students may have reacted to it by migrating to private schools. However, this appears not to have been the case, according to the information provided by The

Spanish Ministry of Education.[11] In particular, in 2016–2017, the region had 11.0% of the ninth- and 10th-grade students attending private schools, 10.9% in 2017–2018, and 10.9% in 2018–2019. Therefore, there was no relevant movement of students migrating from public and charter schools to private schools or vice versa. Furthermore, as previously discussed, we do not find evidence of significant changes in student-to-teacher ratios or the percentage of previous students.

Schools could also adjust to the reform in the long run by, for example, changing their teaching strategies or their examination scheme. Students could also react by changing their levels of effort. Although the analysis of such reactions is an interesting research topic, we study a hastily implemented policy, and therefore, we cannot provide an estimation of the long-run effect. It should be noted that the standardized test was suspended during the COVID period. However, the analysis of the long-run impact of the reform warrants future research when this information is available.

However, other papers in the literature, which studied exogenous variations in instruction time that were anticipated by the students and the schools, can provide a better insight into the long-term impact of instruction time. For example, Pischke (2007) found that the reduction of schooling in Germany in 1966 increased grade repetition by about 0.9 to 1.1 percentage points, whereas it had no adverse effect on earnings and employment later in life. Lavy (2020) evaluated an exogenous change in funding that reduced instructional resources for some schools and increased them for others in Israel. He found that increasing instruction time by approximately 22% raises the average score by 0.04 standard deviation. This result is significant but substantially smaller than the one found in the present research.

To assess the relevance of the results in this research, we compare it with an alternative education policy such as class size changes. Krueger (1999) analyzed the STAR project that randomly assigned 11,571 Tennessee students between 1985 and 1989 to small classes (15 students on average) or large classes (22 students on average). This author concludes that a reduction of seven students (from 22 to 15) improved the results by 22% of one standard deviation. Fredriksson et al. (2013) estimate that a class reduction of seven students in the last 3 years of primary school (age 10–13 years) improves cognitive skills by 23% of a standard deviation. That would mean more than 3% of the standard deviation for each student. Therefore, the effect of the change in instruction time that we estimate (around 15% of the standard deviation) is similar to the impact of reducing the class size by five students, from 22 to 17.

Topics to explore in future research include, for example, the complementarities between face-to-face and online instruction time. For instance, regarding the school lockdown due to COVID-19, it seems unlikely that online education will have adequately replaced 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. Another issue is how to use instruction time to tackle the 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. The example provided in this paper shows that even focusing instruction time on low-performing students did not generate a positive effect. Another line of research is the analysis of the learning loss produced by the COVID lockdown and the school closures. The international external and standardized tests of PISA, TIMSS, and PIRLS will provide relevant information about the possible reduction in the learning of students at the secondary and primary levels in different countries between the cohorts affected by the pandemic and previous cohorts. It will be interesting to examine whether the effect of the school closures has been more negative for lagging behind students and those coming from disadvantaged households. Future articles should address more efficient ways to reduce differences in student performance.

[11]This information can be found at the following url: https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/indicadores/cifras-educacion-espana.html

DATA AVAILABILITY STATEMENT

The database belongs to the Regional Ministry of Education, and we cannot make it accessible to the public. However, we can provide the codes and instructions on how interested parties can readily replicate our results.

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APPENDIX A: DEFINITIONS OF CONTROL VARIABLES

Variables observed in the whole analysis period

Female takes the value of 1 if the student is female and 0 otherwise. Academic mathematics indicates if the student followed a strong math academic course or a more applied math course. Immigrant indicates immigrant student. Early childhood education: indicates pre-primary education attendance (aged 0 to 2 years). Books 0–10: dummy variable indicating that the total number of books in a 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. Parents' education1: Mother or father achieved qualification up to compulsory secondary school. Parents' education2: Mother or father achieved qualification at post-compulsory secondary school. Parents' education3: Mother or parent achieved qualification in undergraduate, graduate, or upper vocational training.

Variables only observed in the 2016/2017 and 2018/2019 academic years

Age: student's age. January: dummy variable indicating that student was born in January. Other months are defined similarly. Mother does not work: Mother has never worked. Mother basic occupation: Mother has a 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. Repeat in secondary once, Repeat in secondary more than once, 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.

TABLE A1 Impact of control variables on academic scores in different competencies.

Variables	Composite index	Spanish	English	Mathematics	History and geography
Female students	−0.0595***	15.15***	13.38***	−25.97***	−18.79***
	(0.0114)	(0.684)	(0.656)	(0.807)	(0.758)
Academic mathematics	0.538***	43.95***	45.71***	−42.91***	41.52***
	(0.0271)	(1.626)	(1.558)	(1.916)	(1.800)
Immigrant	−0.0596***	−7.181***	2.482*	−5.499***	−0.559
	(0.0221)	(1.326)	(1.271)	(1.563)	(1.468)
Pre-primary education	0.0697***	2.671***	3.086***	4.920***	2.472***
	(0.0119)	(0.717)	(0.687)	(0.845)	(0.794)
Books 11–50	0.356***	15.60***	18.40***	17.40***	14.49***
	(0.0306)	(1.836)	(1.760)	(2.165)	(2.033)
Books 51–100	0.515***	21.50***	27.04***	23.70***	23.00***
	(0.0308)	(1.848)	(1.771)	(2.178)	(2.045)
Books 101–200	0.716***	29.48***	37.08***	33.63***	32.37***
	(0.0314)	(1.883)	(1.805)	(2.220)	(2.085)
Books >200	0.884***	35.12***	43.55***	43.67***	41.80***
	(0.0314)	(1.884)	(1.805)	(2.220)	(2.085)
Parents' education2[a]	0.113***	3.185***	7.024***	5.442***	5.462***
	(0.0191)	(1.147)	(1.100)	(1.352)	(1.270)
Parents' education3[a]	0.408***	12.87***	25.30***	18.20***	19.59***
	(0.0203)	(1.216)	(1.166)	(1.434)	(1.347)
Prop female students school[b]	−0.270**	−10.46	−16.60**	−8.689	−13.88
	(0.135)	(8.133)	(7.795)	(9.587)	(9.004)
Prop Academic mathematics school[b]	−0.407***	−7.195	−20.30**	−39.73***	−12.07
	(0.153)	(9.168)	(8.787)	(10.81)	(10.15)
Prop Immigrants school[b]	−4.73e-05	4.624	17.97*	−30.54***	3.500
	(0.165)	(9.888)	(9.477)	(11.65)	(10.95)
Prop Early childhood education school[b]	−0.0638	−12.73**	5.708	−5.441	1.654
	(0.106)	(6.338)	(6.075)	(7.471)	(7.017)
Prop Books 11–50 school[b]	0.750***	44.89***	31.52**	−7.475	62.92***
	(0.252)	(15.16)	(14.53)	(17.87)	(16.78)

(Continues)

TABLE A1 (Continued)

Variables	Composite index	Spanish	English	Mathematics	History and geography
Prop Books 51–100 school[b]	0.386	15.03	26.32*	0.393	27.65*
	(0.242)	(14.56)	(13.95)	(17.16)	(16.12)
Prop Books 101–200[b]	0.475*	18.00	42.59***	−11.98	35.31**
	(0.246)	(14.75)	(14.14)	(17.39)	(16.33)
Prop Books >200[b]	0.814***	43.23***	50.22***	2.128	49.34***
	(0.240)	(14.40)	(13.81)	(16.98)	(15.95)
Prop Parents' education2 schools[b]	0.236	31.25***	3.926	−3.269	7.162
	(0.163)	(9.770)	(9.364)	(11.52)	(10.82)
Prop Parents' education3 school[b]	0.0755	23.00**	−5.652	−3.734	−3.135
	(0.160)	(9.631)	(9.231)	(11.35)	(10.66)
Repeat year in primary	−0.593***	−23.77***	−36.85***	−30.11***	−19.42***
	(0.0302)	(1.814)	(1.739)	(2.138)	(2.008)
Repeat year in secondary	−0.575***	−22.64***	−37.56***	−21.17***	−24.45***
	(0.0219)	(1.317)	(1.262)	(1.553)	(1.458)
Born in February	0.0128	1.897	0.892	0.303	−0.942
	(0.0282)	(1.693)	(1.623)	(1.996)	(1.875)
Born in March	−0.0677**	−1.777	−2.494	−4.407**	−4.145**
	(0.0273)	(1.638)	(1.570)	(1.931)	(1.814)
Born in April	−0.0211	−0.0441	0.994	−3.759*	−1.558
	(0.0272)	(1.631)	(1.563)	(1.922)	(1.805)
Born in May	−0.0391	−1.689	−0.318	−1.872	−3.355*
	(0.0267)	(1.603)	(1.536)	(1.889)	(1.774)
Born in June	−0.0341	−1.006	−0.788	−3.931**	−0.927
	(0.0274)	(1.647)	(1.579)	(1.941)	(1.823)
Born in July	−0.0351	−0.704	−1.073	−2.854	−2.115
	(0.0270)	(1.622)	(1.555)	(1.912)	(1.796)
Born in August	−0.0873***	−4.451***	−4.140***	−3.083	−4.267**
	(0.0277)	(1.662)	(1.593)	(1.960)	(1.840)
Born in September	−0.0931***	−3.768**	−3.593**	−2.843	−6.869***
	(0.0275)	(1.651)	(1.583)	(1.946)	(1.828)
Born in October	−0.0786***	−3.416**	−2.295	−1.639	−6.940***
	(0.0272)	(1.632)	(1.565)	(1.924)	(1.807)
Born in November	−0.0628**	−2.329	−1.819	0.00287	−7.164***
	(0.0274)	(1.642)	(1.574)	(1.936)	(1.818)
Born in December	−0.138***	−7.565***	−6.085***	−3.671*	−7.575***
	(0.0277)	(1.664)	(1.595)	(1.962)	(1.842)
Mother does not work	−0.0459	−3.049	−3.300	−2.728	0.651
	(0.0384)	(2.304)	(2.208)	(2.716)	(2.550)
Mother basic occupations occupations	−0.0473**	−0.433	−3.942***	−2.527*	−2.098
	(0.0213)	(1.281)	(1.228)	(1.510)	(1.419)
Mother craft and related trades worker	−0.0915**	−5.685**	−3.442	−3.380	−4.075
	(0.0445)	(2.671)	(2.560)	(3.149)	(2.957)

TABLE A1 (Continued)

Variables	Composite index	Spanish	English	Mathematics	History and geography
Mother skilled agricultural and forestry	−0.0960	−7.572	−10.37*	3.760	−2.090
	(0.106)	(6.374)	(6.110)	(7.514)	(7.057)
Mother plant and machine operators	−0.0553	−3.889	−4.215	−3.591	1.543
	(0.0486)	(2.920)	(2.799)	(3.442)	(3.233)
Mother retail, services, and personal care	−0.0977***	−5.287***	−3.788***	−3.404**	−5.314***
	(0.0218)	(1.309)	(1.255)	(1.543)	(1.449)
Mother armed forces/protection and security	−0.0675**	−1.397	−3.087	−3.232	−4.979**
	(0.0331)	(1.988)	(1.905)	(2.343)	(2.201)
Mother clerical support workers	0.0274*	0.445	1.145	1.313	2.268**
	(0.0150)	(0.903)	(0.865)	(1.064)	(1.000)
Mother technician and professionals	0.0643***	1.508	2.837**	3.841***	3.973***
	(0.0209)	(1.257)	(1.205)	(1.482)	(1.392)
Father does not work	−0.218***	−8.816*	−17.52***	−7.973	−5.813
	(0.0793)	(4.763)	(4.565)	(5.614)	(5.272)
Father basic occupations occupations	−0.0933***	−2.327	−9.675***	−4.197**	−1.257
	(0.0282)	(1.694)	(1.624)	(1.997)	(1.876)
Father craft and related trades worker	−0.0321	0.998	−7.263***	0.0782	0.137
	(0.0209)	(1.253)	(1.201)	(1.477)	(1.387)
Father skilled agricultural and forestry	−0.210***	−9.180***	−17.12***	−4.271	−7.507*
	(0.0588)	(3.532)	(3.386)	(4.164)	(3.911)
Father plant and machine operators	−0.0833***	−1.098	−6.191***	−3.955**	−4.487***
	(0.0229)	(1.373)	(1.316)	(1.619)	(1.520)
Father retail, services, and personal care	0.0343	1.738	−1.953	3.206*	3.526**
	(0.0256)	(1.535)	(1.471)	(1.809)	(1.699)
Father armed forces/protection and security	0.0212	2.026	−1.883	0.134	3.395*
	(0.0308)	(1.850)	(1.773)	(2.181)	(2.048)
Father clerical support workers	0.0199	2.315**	−0.0921	0.0898	1.068
	(0.0187)	(1.120)	(1.074)	(1.320)	(1.240)
Father technician and professionals	0.110***	4.795***	5.605***	6.242***	3.882***
	(0.0190)	(1.138)	(1.091)	(1.341)	(1.260)
2018/2019 test	0.142***	19.24***	−0.478	8.881***	−2.737
	(0.0355)	(2.131)	(2.043)	(2.512)	(2.359)
2018/2019 Interacted with public and charter school	−0.186***	−23.94***	−2.928	−7.278***	2.050
	(0.0369)	(2.216)	(2.124)	(2.612)	(2.453)
Constant	−1.155***	407.9***	418.8***	578.5***	424.8***
	(0.258)	(15.49)	(14.85)	(18.26)	(17.15)
R^2	0.142	0.098	0.169	0.071	0.092
Number of COD_FINAL	766	766	766	766	766

Note: Difference-in-differences estimation using OLS. School years 2016/2017 and 2018/2019 (without 2017/2018).
[a]Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and undergraduate studies, respectively. The model specification includes 766 school fixed effects and academic year effects corresponding to 2018/2019. The following categories have been omitted to avoid perfect collinearity: 2016/2017 academic year, Books 0–10, and Parents' education1, born in January, profession not applicable.
[b]Variables are averaged at the school level.

Table A1 shows the DID estimation, including school fixed effects, of the composite index *pc1* and each competency using only the 2016/2017 and 2018/2019 school years. Results in Table A1 are very similar to those of Table 2 in the main paper confirming the robustness of our findings. We find the same impact on female students, academic mathematics, immigrants, early childhood education, the number of books at home, and parents' education. As for the additional variables available for the 2016/2017 and 2018/2019 academic years, we find that students who repeated years either in primary or secondary school had significantly worse performance, as noted in PISA reports (OECD, 2019). Ceteris paribus, students born in the first half of the year tended to perform better than those born toward the end of the year (Dhuey et al., 2019). Parents' professions also matter. For example, having parents who are 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 are parents not working or having professions such as basic occupations, crafts and related trades, skilled agriculture and forestry, and plant and machine operation and assembly. The interaction between the 2018/2019 academic year and the treatment group is negative, of a similar magnitude, and significant for the composite index, Spanish and Mathematics. The interaction term is not negative any more for English and History and Geography. The negative impact of the 2 weeks reduction in instruction time is robust for the composite index, Spanish and Mathematics, when using the full available variables for 2016/2017 and 2018/2019.

TABLE A2 Estimated effect of the regulatory change on academic scores in different competencies.

Variables	Composite index	Spanish	English	Mathematics	History and geography
Female students	−0.0454***	13.01***	12.12***	−21.68***	−16.26***
	(0.00927)	(0.544)	(0.542)	(0.635)	(0.599)
Academic mathematics	0.933***	59.91***	70.47***	−25.67***	56.60***
	(0.0181)	(1.064)	(1.060)	(1.243)	(1.173)
Immigrant	−0.187***	−11.62***	−5.204***	−12.30***	−5.497***
	(0.0155)	(0.910)	(0.907)	(1.063)	(1.003)
Early childhood education	0.103***	3.807***	4.983***	6.425***	4.102***
	(0.00968)	(0.568)	(0.566)	(0.663)	(0.626)
Books 11–50	0.341***	14.82***	16.84***	16.50***	15.05***
	(0.0255)	(1.496)	(1.491)	(1.749)	(1.649)
Books 51–100	0.545***	22.93***	28.72***	24.16***	24.93***
	(0.0257)	(1.509)	(1.504)	(1.764)	(1.664)
Books 101–200	0.753***	30.91***	39.60***	33.95***	34.82***
	(0.0263)	(1.541)	(1.535)	(1.801)	(1.698)
Books >200	0.946***	38.25***	48.27***	43.51***	45.13***
	(0.0262)	(1.538)	(1.533)	(1.797)	(1.695)
Parents' education2[a]	0.163***	5.903***	11.22***	5.857***	7.180***
	(0.0160)	(0.936)	(0.932)	(1.093)	(1.031)
Parents' education3[a]	0.452***	15.09***	30.18***	18.73***	19.89***
	(0.0152)	(0.891)	(0.888)	(1.042)	(0.983)
Prop female students school[b]	−0.0803	−0.666	−9.677**	−7.281	1.968
	(0.0746)	(4.377)	(4.361)	(5.115)	(4.825)

TABLE A2 (Continued)

Variables	Composite index	Spanish	English	Mathematics	History and geography
Prop academic mathematics school[b]	−0.358***	0.0984	−38.15***	−22.70***	−8.260
	(0.102)	(5.995)	(5.974)	(7.006)	(6.609)
Prop immigrants school[b]	−0.106	17.64***	−11.69**	−13.40*	−16.05**
	(0.102)	(5.959)	(5.938)	(6.964)	(6.569)
Prop early childhood education school[b]	−0.101	−11.49***	4.931	−4.507	−6.579
	(0.0677)	(3.972)	(3.958)	(4.642)	(4.378)
Prop Books 11–50 school[b]	0.439***	11.05	−2.284	23.74**	50.09***
	(0.155)	(9.065)	(9.033)	(10.59)	(9.993)
Prop Books 51–100 school[b]	0.425***	18.42*	3.904	35.62***	22.58**
	(0.161)	(9.445)	(9.412)	(11.04)	(10.41)
Prop Books 101–200 school[b]	0.534***	14.11	12.39	34.34***	40.23***
	(0.164)	(9.631)	(9.597)	(11.26)	(10.62)
Prop Books >200[b]	0.700***	32.07***	14.26	30.90***	51.86***
	(0.163)	(9.554)	(9.520)	(11.16)	(10.53)
Prop Parents' education2 schools[b]	0.143	21.34***	13.19**	−12.88*	0.398
	(0.112)	(6.571)	(6.547)	(7.679)	(7.243)
Prop parents education3 school[b]	0.0825	13.67**	5.854	−7.071	0.108
	(0.109)	(6.383)	(6.360)	(7.459)	(7.036)
2017/18 academic course dummy	0.137***	5.261***	5.794***	10.50***	4.431***
	(0.0151)	(0.887)	(0.884)	(1.037)	(0.978)
2018/2019 academic course dummy	0.123***	12.52***	−1.971	8.971***	2.628
	(0.0293)	(1.720)	(1.714)	(2.010)	(1.896)
2018/2019 Interacted with public and charter schools	−0.206***	−19.81***	−3.882**	−8.599***	−4.166**
	(0.0308)	(1.804)	(1.797)	(2.108)	(1.988)
Observations	76,503	76,503	76,503	76,503	76,503
R^2	0.126	0.100	0.145	0.057	0.089

Note: The model specification includes 775 school fixed effects and two academic year effects corresponding to 2017/2018 and 2018/2019. The following categories have been omitted to avoid perfect collinearity: Books 11–50 and Parents' education1 (father or mother achieved qualification up to compulsory secondary school. Difference-in-differences estimation. Control variables drawn from student questionnaires for 2016–2017, 2017–2018, and 2018–2019.

[a]Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and higher education studies, respectively.

[b]Variables are averaged at the school level.

****p* < .01, ***p* < .05, and **p* < .1. Standard errors in parentheses.

TABLE A3 Estimated effect of the regulatory change on academic scores in different competencies.

Variables	Composite index	Spanish	English	Mathematics	History and geography
Female student	−0.0493***	13.11***	12.03***	−22.18***	−16.58***
	(0.00923)	(0.542)	(0.539)	(0.633)	(0.597)
Academic mathematics	0.929***	59.79***	70.15***	−25.62***	56.30***
	(0.0181)	(1.061)	(1.056)	(1.240)	(1.169)
Immigrant	−0.148***	−9.575***	−2.679***	−10.04***	−5.073***
	(0.0162)	(0.950)	(0.946)	(1.110)	(1.047)
Pre-primary education	0.104***	3.888***	5.066***	6.628***	4.002***
	(0.00963)	(0.566)	(0.563)	(0.661)	(0.624)
Books 11–50	0.371***	16.30***	18.70***	17.80***	15.81***
	(0.0257)	(1.510)	(1.503)	(1.765)	(1.664)
Books 51–100	0.572***	24.33***	30.38***	25.37***	25.52***
	(0.0259)	(1.524)	(1.516)	(1.780)	(1.679)
Books 101–200	0.776***	32.22***	41.00***	35.11***	35.15***
	(0.0265)	(1.555)	(1.547)	(1.817)	(1.713)
Books >200	0.955***	39.01***	48.80***	44.14***	44.85***
	(0.0264)	(1.553)	(1.545)	(1.815)	(1.711)
Parents' education2[a]	0.173***	5.871***	11.91***	6.738***	7.450***
	(0.0152)	(0.892)	(0.887)	(1.042)	(0.982)
Parents' education3[a]	0.530***	17.84***	34.96***	21.81***	23.62***
	(0.0156)	(0.917)	(0.912)	(1.071)	(1.010)
Prop Female students school[b]	−0.0577	0.0926	−8.496*	−5.771	2.784
	(0.0743)	(4.366)	(4.344)	(5.101)	(4.811)
Prop Academic mathematics school[b]	−0.538***	−10.27*	−43.31***	−32.22***	−16.18**
	(0.103)	(6.032)	(6.002)	(7.046)	(6.646)
Prop Immigrants school[b]	−0.221**	10.78*	−15.47***	−20.35***	−19.77***
	(0.101)	(5.942)	(5.912)	(6.941)	(6.547)
Prop Early childhood education school[b]	−0.0645	−9.088**	5.473	−2.727	−4.730
	(0.0675)	(3.968)	(3.948)	(4.635)	(4.372)
Prop Books 11–50 school[b]	0.364**	8.421	−5.468	18.65*	46.85***
	(0.154)	(9.055)	(9.010)	(10.58)	(9.978)
Prop Books 51–100 school[b]	0.331**	14.72	−0.0659	29.53***	18.71*
	(0.161)	(9.433)	(9.386)	(11.02)	(10.39)
Prop Books 101–200 school[b]	0.452***	11.13	9.020	29.15***	36.50***
	(0.164)	(9.616)	(9.568)	(11.23)	(10.60)
Prop Books >200[b]	0.628***	29.48***	11.40	25.73**	48.80***
	(0.162)	(9.540)	(9.493)	(11.15)	(10.51)
Prop Parents' education2 school[b]	0.111	20.83***	9.997	−14.77*	−0.136
	(0.111)	(6.541)	(6.509)	(7.642)	(7.208)
Prop Parents' education3 school[b]	0.0135	9.905	2.088	−9.716	−2.286
	(0.108)	(6.364)	(6.332)	(7.434)	(7.012)

TABLE A3 (Continued)

Variables	Composite index	Spanish	English	Mathematics	History and geography
2017/2018 test	0.108***	4.275***	3.593***	9.483***	3.143***
	(0.0151)	(0.889)	(0.884)	(1.038)	(0.979)
2018/2019 test	0.0871***	9.833***	−2.914*	7.147***	1.631
	(0.0294)	(1.726)	(1.717)	(2.016)	(1.902)
2018/2019 Interacted with public school	−0.315***	−26.09***	−6.399***	−14.35***	−9.719***
	(0.0330)	(1.937)	(1.928)	(2.263)	(2.135)
2018/2019 Interacted with charter school	−0.0689**	−10.58***	−0.855	−1.628	1.513
	(0.0321)	(1.886)	(1.876)	(2.203)	(2.078)
Constant	−1.610***	399.7***	420.0***	515.7***	403.8***
	(0.175)	(10.25)	(10.20)	(11.98)	(11.30)
Observations	76,757	76,757	76,757	76,757	76,757
R^2	0.132	0.103	0.150	0.059	0.093

Note: Impact differentiated between public and charter schools. Difference-in-differences regression. School years 2016/2017, 2017/2018, and 2018/2019.
[a]Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and higher education studies, respectively.
[b]Variables are averaged at the school level.

TABLE A4 Heterogenous effects across students' attitudes.

Variables	Composite index	Composite index
Female student	−0.0912***	−0.0534***
	(0.0115)	(0.0114)
Academic mathematics	0.504***	0.511***
	(0.0271)	(0.0271)
Immigrant	−0.0544**	−0.0456**
	(0.0220)	(0.0221)
Pre-primary education	0.0669***	0.0653***
	(0.0119)	(0.0119)
Books 11–50	0.333***	0.345***
	(0.0306)	(0.0306)
Books 51–100	0.477***	0.493***
	(0.0308)	(0.0308)
Books 101–200	0.670***	0.692***
	(0.0314)	(0.0314)
Books >200	0.834***	0.859***
	(0.0314)	(0.0314)
Parents' education2[a]	0.104***	0.107***
	(0.0190)	(0.0191)
Parents' education3[a]	0.396***	0.400***
	(0.0202)	(0.0203)

(Continues)

TABLE A4 (Continued)

Variables	Composite index	Composite index
Prop female students school[b]	−0.277**	−0.232*
	(0.136)	(0.137)
Prop academic mathematics school[b]	−0.406***	−0.424***
	(0.153)	(0.153)
Prop immigrants school[b]	0.0293	0.218
	(0.165)	(0.167)
Prop early childhood education school[b]	−0.0735	−0.136
	(0.105)	(0.107)
Prop Books 11–50 school[b]	0.640**	0.697***
	(0.257)	(0.256)
Prop Books 51–100 school[b]	0.317	0.308
	(0.247)	(0.247)
Prop Books 101–200 school[b]	0.399	0.290
	(0.250)	(0.251)
Prop Books >200[b]	0.653***	0.586**
	(0.248)	(0.246)
Prop Parents' education2 schools[b]	0.245	0.0686
	(0.163)	(0.166)
Prop Parents' education3 schools[b]	0.0316	−0.0765
	(0.162)	(0.162)
Repeat year in primary	−0.586***	−0.585***
	(0.0301)	(0.0302)
Repeat year in secondary	−0.544***	−0.524***
	(0.0219)	(0.0221)
Born in February	0.0103	0.0102
	(0.0281)	(0.0282)
Born in March	−0.0673**	−0.0687**
	(0.0272)	(0.0273)
Born in April	−0.0237	−0.0201
	(0.0271)	(0.0272)
Born in May	−0.0405	−0.0419
	(0.0266)	(0.0267)
Born in June	−0.0369	−0.0327
	(0.0273)	(0.0274)
Born in July	−0.0399	−0.0380
	(0.0269)	(0.0270)
Born in august	−0.0933***	−0.0862***
	(0.0276)	(0.0277)
Born in September	−0.100***	−0.0942***
	(0.0274)	(0.0275)

TABLE A4 (Continued)

Variables	Composite index	Composite index
Born in October	−0.0819***	−0.0805***
	(0.0271)	(0.0272)
Born in November	−0.0685**	−0.0673**
	(0.0273)	(0.0274)
Born in December	−0.144***	−0.144***
	(0.0276)	(0.0277)
Mother does not work	−0.0513	−0.0407
	(0.0382)	(0.0384)
Mother basic occupations occupations	−0.0458**	−0.0417*
	(0.0213)	(0.0214)
Mother craft and related trades worker	−0.0848*	−0.0952**
	(0.0443)	(0.0444)
Mother skilled agricultural and forestry	−0.103	−0.110
	(0.106)	(0.107)
Mother plant and machine operators	−0.0424	−0.0433
	(0.0485)	(0.0486)
Mother retail, services, and personal care	−0.0961***	−0.0883***
	(0.0217)	(0.0218)
Mother armed forces/protection and security	−0.0599*	−0.0608*
	(0.0330)	(0.0331)
Mother clerical support workers	0.0255*	0.0235
	(0.0150)	(0.0150)
Mother technician and professionals	0.0597***	0.0597***
	(0.0209)	(0.0209)
Father does not work	−0.201**	−0.184**
	(0.0790)	(0.0794)
Father basic occupations occupations	−0.0878***	−0.0908***
	(0.0281)	(0.0283)
Father craft and related trades worker	−0.0359*	−0.0314
	(0.0208)	(0.0209)
Father skilled agricultural and forestry	−0.208***	−0.197***
	(0.0586)	(0.0589)
Father plant and machine operators	−0.0813***	−0.0860***
	(0.0228)	(0.0229)
Father retail, services, and personal care	0.0340	0.0368
	(0.0255)	(0.0256)
Father armed forces/protection and security	0.0148	0.0223
	(0.0307)	(0.0309)
Father clerical support workers	0.0215	0.0139
	(0.0186)	(0.0187)

(Continues)

TABLE A4 (Continued)

Variables	Composite index	Composite index
Father technician and professionals	0.107***	0.107***
	(0.0189)	(0.0190)
2018/2019 test	0.186***	0.154***
	(0.0363)	(0.0355)
2018/2019 Interacted with public and charter school	−0.192***	−0.168***
	(0.0374)	(0.0370)
2018/2019 Interacted with public and charter schools and students devoting less than 3 h of homework per week	−0.0649**	
	(0.0317)	
2018/2019 Interacted with public and charter schools with more than 6 days of unjustified absences throughout the quarter.		−0.246***
		(0.0590)
Students devoting less than 3 h of homework per week	−0.339***	
	(0.0343)	
Students devoting between 3 and 6 h of homework per week	−0.345***	
	(0.0291)	
Students devoting between 6 and 9 h of homework per week	−0.222***	
	(0.0302)	
Students devoting between 9 and 12 h of homework per week	−0.217***	
	(0.0306)	
Students devoting between 12 and 15 h of homework per week	−0.170***	
	(0.0312)	
Students devoting between 15 and 18 h of homework per week	−0.0801**	
	(0.0326)	
Students devoting between 18 and 21 h of homework per week	−0.0263	
	(0.0341)	
Prop number of students devoting less than 3 h of homework per week. School averages	−0.700***	
	(0.251)	
Prop number of students devoting between 3 and 6 h of homework per week. School averages	−0.817***	
	(0.252)	
Prop number of students devoting between 6 and 9 h of homework per week. School averages	−0.890***	
	(0.265)	
Prop number of students devoting between 9 and 12 h of homework per week. School averages	−0.199	
	(0.278)	
Prop number of students devoting between 12 and 15 h of homework per week. School averages	−0.777***	
	(0.276)	
Prop number of students devoting between 15 and 18 h of homework per week. School averages	−0.453	
	(0.314)	
Prop number of students devoting between 18 and 21 h of homework per week. School averages	−0.881***	
	(0.335)	

TABLE A4 (Continued)

Variables	Composite index	Composite index
Between 2 and 4 days of unjustified absences throughout the quarter.		−0.220*** (0.0160)
Between 4 and 6 days of unjustified absences throughout the quarter.		−0.339*** (0.0246)
More than 6 days of unjustified absences throughout the quarter.		−0.247*** (0.0405)
Prop Students with 2–4 days of unjustified absences throughout the quarter. School averages		−0.822*** (0.136)
Prop Students with 4–6 days of unjustified absences throughout the quarter. School averages		−0.218 (0.214)
Prop Students with more than 6 days of unjustified absences throughout the quarter. School averages		−0.619*** (0.230)
Constant	−0.0527 (0.356)	−0.585** (0.272)
Observations	49,650	49,215
R^2	0.149	0.151

Note: Estimated effect of the regulatory change on academic scores in students with more than 6 days of unjustified absences throughout the quarter and students with less than 3 h of homework in a week. Triple DID regression. School years 2016/2017 and 2018/2019 (without 2017/2018).

[a]Parents' education2 and Parents'education3 indicate that the mother or father has completed post-compulsory secondary school and higher education studies, respectively.

[b]Variables are averaged at the school level.

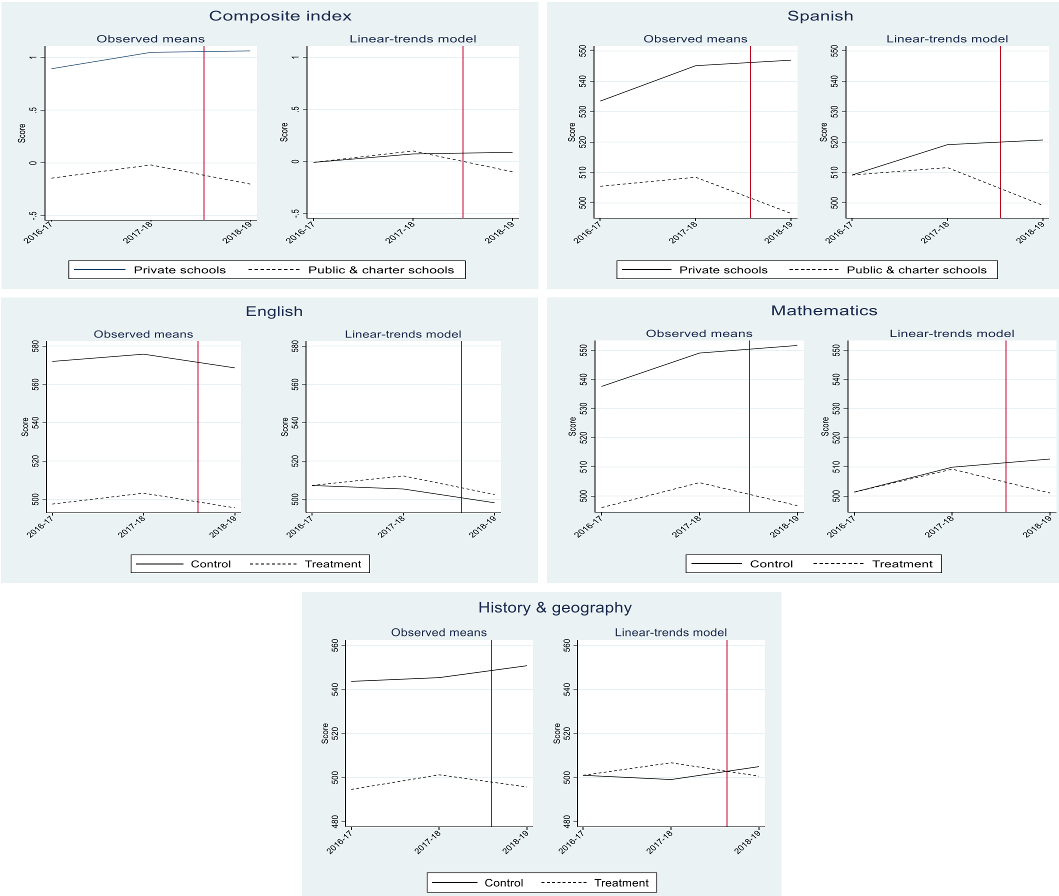

FIGURE A1 Parallel trends for each competence and the composite index in the external and standardized test. Academic years 2016–2017, 2017–2018, and 2018–2019. *Note:* Private schools and public schools are represented with continuous and dashed lines, respectively. The linear trend model augments the two-way fixed effect model to allow for interactions between the dummy variable for public and charter schools and the pre-treatment and post-treatment time periods. [Colour figure can be viewed at wileyonlinelibrary.com]