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EFFECTS OF SCHOOL CLOSURES IN PORTUGAL

The termination of public-funded private school contracts

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Abstract

This study analyses the impact of the termination of public-funded private school contracts, known as *Contratos de Associação* (CA), on student performance and demand for different types of schools in Portugal. We use a difference-in-differences approach to measure this impact on high school national exam scores. Our findings suggest this policy had no effects on student achievement in the municipalities that had CA schools in that period. However, we find an increase in demand of both strictly private and public schools in response to the decrease in supply of CA classes caused by this policy, in the municipalities considered.

Keywords: education economics; public-funded private schools; educational achievement; difference-in-differences; national exams

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1. Introduction

In Portugal, certain private schools were awarded pluriannual contracts signed between the government and those private schools at the beginning of each school cycle, *Contratos de Associação*[1], to respond to a shortage of supply of public schools in certain geographic areas. This measure was implemented in the 80s, but the popularity of these contracts rose, as it provided an alternative to investing in the creation and expansion of public schools and was believed by some to be more cost-efficient. However, since 2016, due to educational reforms, the number of these type of contracts decreased significantly. A crucial legislation, that went in effect in the academic year of 2016/17, stated that if there was a public school in a 5km radius, private schools under these contracts would no longer have them. As many students were forced to enrol in different schools, it is natural to ask the question: were they affected? Did they move to public schools or continued in private schools paying tuition? What was the effect of this policy on student's high school exam grades in the municipalities affected?

The literature finds that public-funded private students in Portugal, for years prior to the policy, achieved better results in national exams than public school students (Rosado and Seabra, 2015). The results remain robust after adding individual-specific controls (Oliveira, 2018). Thus, it may be reasonable to expect a negative effect on exam grades from this policy, however broad observational findings in the US found no significant differences in test scores between the two types of students (CREDO, 2013).

The advantage of studying public-funded private schools in Portugal (CA schools), is that students in these schools are more similar in terms of income level to students in regular public-

[1] *"Contratos de Associação"* are pluriannual contracts signed between the government and private schools at the beginning of each school cycle, meaning that a new class established in the 10th grade in 2015/16 in a publicly funded private school will be financed for at least three years, until the 12th grade, even though the government may choose not to finance new 10th grade classes in 2016/17 in that publicly funded private school

school than to students in strictly private school, the reason being that the admission criteria to enrol in CA schools does not depend on family income.

The major difference between CA and public schools is in terms of the hiring decisions. They have freedom to hire staff as they see fit, while public schools are entirely dependent of centralized government decisions. In turn, studying the effect of losing CA schools allows to isolate the impact of public and private management systems on students' educational outcomes. With this study we intend to contribute with some valuable insights and policy recommendations regarding the aforementioned policy decision that resulted in the termination of many *Contratos de Associação.*

This study uses a group of municipalities that had CA schools in the period from 2015/2016 to 2018/19 and for which we observe the academic achievement of each municipality in terms of the mean grade for each high school national exam both in the year affected by the policy (treatment year) and the year unaffected, depending on whether the exam is an 11th or 12th grade exam. The years affected[2], in our group, for the 11th grade exams are 2017/18 and 2018/19 while for the 12th grade's only 2018/19 was affected. We then use different specifications of the difference-in-differences approach to estimate the effect of the policy, i.e., of terminating CA classes[3]. This estimation strategy exploits variation across municipalities differentially exposed to the policy. This empirical strategy allows to isolate the effect of a policy from broader time-trends or municipality specific characteristics.

We find evidence that the decrease in the supply of these contracts resulted in an increase in the demand of both strictly private education and public education. However, the size of this increase does not match the decrease in supply caused by this policy, which meant a net decrease in the number of students in the municipalities affected. Additionally, we find that

[2] Only true for students that did not fail 10th or 11th grade.
[3] Contratos de Associação are measured by number of classes.

losing CA classes has no statistically significant effect on the national exams' scores. The mean exam grades for the municipality, the outcome variable, was not affected by this policy.

This research is structured in the following manner: Section 2 is dedicated to Literature Review; Section 3 provides information on the Portuguese Education System and the nature of Contratos de Assosiação (CA); Section 4 describes the data, the variables used throughout the study and the group decomposition of the number of classes; Section 5 reports the methodology used; Section 6 is devoted to results obtained and robustness checks; and finally the last Section 7 is dedicated to our conclusion and policy implications.

2. Literature Review

The debate on whether schools should be able to manage their own resources, or a more centralized option is better is long-standing. Researchers have studied this topic by analysing the differences between public and private schools. However, the challenge of measuring school quality differences under private and public administrations, lies in separating students' achievement from differences in students' background (Hanushek et al., 2007).

The usual consensus is that students who attend strictly private schools tend to come from more privileged socioeconomic backgrounds, since the high tuition fees encountered in these schools impose a financial barrier to many families (Mancebón et al., 2010; Flaker, 2014), thereby generating a self-selection problem that may bias the results of private school attendees (upwards). Moreover, Hanushek et al. (2015) stated that early investments in education (pre-school), which are closely linked to family background, may explain differences in students' achievement further on. Their parents also tend to have more education and are likelier to enrol their children in private schools, and thus private schools will be upwardly biased.

Focusing now on the effectiveness of publicly funded private schools, Rosado and Seabra (2015) evaluate the relative performance of public versus private schools in Portugal. They use publicly funded private schools to isolate the impact of student's background from school's administration and funding. Employing cross-section data to compare students at the 9th grade in 2010, the authors find a positive effect of private ownership in students' performance in national exams. After controlling for students' individual characteristics e.g., age and gender, and background (the latter mainly district controls), belonging to a publicly funded private school increases the probability of passing the 9th grade national exam by 2.34% for Mathematics and by 2.06% for Portuguese, when compared to a public school. However, when considering the impact of school administration on students' consistency over academic years, being in a publicly funded private school decreases by 0.79% the probability of reaching 9th grade without any retention; increases by 0.68% the probability of being retained once; and increases by 0.11% the probability of being retained more than once when compared to public schools.

Moreover, João Oliveira (2018) also finds that CA students perform better, on average, than students that attend traditional public schools using a Value-Added approach to compare national exam scores from the 9th grade. Additionally, the results suggest that attending a publicly funded private class also increases national exam scores by 1 point in Portuguese subject and 3 points in Mathematics, when compared to strictly public class (0-100 scale).

Mancebón et al. (2010), perform a non-parametric efficiency analysis (Data Envelopment Analysis) on public and public-funded private schools in Spain, using microdata from PISA 2006 on science competencies. They controlled for students' background, school resources, and individual management inefficiencies. They found that public schools are more efficient than publicly funded private schools, i.e. students in public schools have better results than publicly funded private schools in science PISA scores, while the former use equal or fewer resources than the latter.

Some countries have Charter schools, another type of school choice program that differs mainly in school administration, given that they can be either privately or publicly owned and managed. Charter schools are publicly funded and foster student learning by promoting educational innovation while allowing more autonomy and freedom with regards to school governance (Robert Bifulco, 2006). On average, there are few differences in student test scores between charter and traditional public-school students in the United States when broad, nationwide comparisons are made. Using matching methods, a comparison between charter schools and traditional public schools on maths and reading tests across 27 states found very small differences (Center for Research on Education Outcomes [CREDO], 2013). Again, using matching methods, but for a sample of 36 charter management organizations (CMOs) across 19 states, each operating at least four schools, another study did not find statistically significant impacts on test scores or college enrolment (Furgeson, et al. 2012). Simultaneously, a lottery study of 33 middle schools across 15 states (Gleason et al., 2010; Clark et al., 2015), found, on average, no differences in test scores between charter and traditional public schools. Following the same students from college entrance to completion, no difference is found in outcomes between charter and traditional public schools (Place & Gleason, 2019). These latter studies, which are lottery based, conclude there are few differences in academic outcomes across charter and district schools coinciding with broad observational findings.

Pierre Lefebvre et al. (2011) measures the effects on achievement of students in state funded private schools in Quebec, Canada, by observing if the student moved from a public grade school to a private high school, arguing that this move is uncorrelated with the student's ability, but caused by the increase in supply of this school type. This identification strategy is a way of addressing endogeneity when trying to measure the effect of private schools on the academic performance of children. They find a positive effect on achievement at a low cost for the government.

On the topic of school closures, Deven Carlson and Stéphane Lavertu (2016) focus on estimating the effects of charter school closure on student achievement. Using data from Ohio, they conclude that the closure of charter schools that failed to meet the standard (due to the mandatory closure law for sub-standard schools) yielded achievement gains of around 0.2-0.3 standard deviations in reading and math for students attending these schools at the time they were identified for closure. This study looks at the exogenous variation produced by the automatic closure laws to measure the effects of low-performing charter schools on student achievement, as there is substantial heterogeneity in school quality.

Randall K. Filer and Daniel Munich (2012), on the other hand, study the responses of private and public schools to voucher funding. After the fall of the Soviet Union, the post-communist Czech Republic legalized private schools. They arose, specially, in areas where the public school was under-supplying or had sub-standard quality. They found that student achievement increased as a result of higher quality of education supplied by private schools as well as an increase in quality by public schools, as a response to competition from the private sector. However, the public sector also responded to private vouchers with manoeuvring from public officials to preserve the public schools' entrenched position.

Regarding supply and demand responses to schooling policies, Michael Dinerstein et al. (2021) studies how the policy effects differ under a fixed versus changing market structure in the context of a public-school funding reform in New York City. They find evidence that private schools decrease their supply in response to an increase in public funding of public schools. They further suggest in accordance with their findings that the private sector is likely to adjust to schooling policies of different sorts, i.e., the supply in the private sector is very elastic.

Similarly to the estimation strategy used in our study, Teresa Molina et al. (2021) employs a Difference-in-Differences (DiD) approach to estimate the effects of a policy that eliminated university tuition in Ecuador, which exploits variation across cohorts differentially exposed to the policy. This empirical strategy allows to isolate the effect of a policy from broader time-trends or cohort specific characteristics.

3. Background on Contratos de Associação

In Portugal, since 2009[4], education is mandatory until the 12th grade and is divided in two different stages: *Ensino Básico* and *Ensino Secundário*. The former comprises three school cycles: *1º Ciclo* – 1st to 4th grade; *2º Ciclo* – 5th and 6th grade; *3º Ciclo* – 7th to 9th grade. *Ensino Secundário,* high school, comprises 10th to 12th grade and corresponds to the last school cycle before higher education, which is not compulsory. In the scope period of this study (2015/16-2018/19), students were required to complete national exams for the Portuguese and, possibly[5], Mathematics subjects at the end of the 12th grade, as well as certain exams at the end of the 11th grade, which depend on the professional area of study that they chose in the beginning of high-school. We will use a group of national exams taken in this period and see how they vary at the municipality level in the treatment period.

Regarding ***C**ontratos de **A**ssociação*, these are pluriannual contracts established by the government with private schools to guarantee free education in areas with an undersupply of public education (or non-existent). The first law establishing these contracts dates back to 1980[6], when the law extended mandatory schooling until the 9th grade[7] and established public funding at the individual level which was consistent with the amount spent in public schools with the same level and equivalent degree of education. From 2015/16, the government decided to publicly fund private schools at the class level, allocating 80,500€ per class and per academic year.

[4] Portuguese Law nr.85/2009 of 27th August (Article nr.1 and nr.2)
[5] Depending on the student's high school track
[6] Portuguese Decree-Law nr.553/80 of 21st November (Article nr.14-16)
[7] Portuguese Law nr.45/86 of 14th October (Article nr.6)

Private school define the enrolment criteria for students, whilst in both public schools and CA schools, in order for students to apply they must rank their preferences[8]. The selection process is mainly based on their residential area (distance to the school) and their preferences.

Teachers are hired by private and CA schools entirely in accordance with their own criteria. From cleaning staff to teachers, public schools do not have the independence to make any hiring decisions. Teacher's hiring and allocation falls under the authority of the Ministry of Education's. Teachers are allocated based on their preferences, experience, and grades upon graduation (Ferreira, 2015).

Starting in the academic year 2016/17, a new policy went into effect and the Ministry of Science and Education started closing CA classes in areas where there was already a big enough supply of public schools, which was due to the construction of new public schools or a demographic decrease.

4. Data, Variables and Group Compositions

The data used in this study was provided by DGEEC[9] (Direção-Geral de Estatísticas da Educação e Ciência) from the Portuguese Ministry of Science and Education (MEC). This administrative dataset contains the entire population in mainland Portugal from the 1st grade up to the 12th grade with respect to students, teachers and schools from 2006/07 until the academic year of 2017/18. And, more specifically, contains a group of variables that include a set of school characteristics, such as school district, county and type of school or class (CA, strictly private or public), such that it is possible to retrieve information concerning the number

[8] Interestingly, students who apply to a certain school and whose sibling(s) are already studying there have priority; a possible explanation could be to facilitate transportation among the household.
[9] DGGEC is the entity responsible for collecting, monitoring, treating, producing and releasing statistics with regards to the Portuguese education system.

of classes in each grade and school, and district, by academic year, which will be utilised for both the descriptive and empirical analysis.

This study also uses data from AEEP[10], which contains the entire set of CA schools, their municipality, and the number of CA classes in each school per year. This data was used to construct the explanatory variable: **change in the share of classes under CA**, which is the difference in the number CA classes relative to the sum of public and CA classes in the 10th grade between 2016 and 2017 in each municipality, in percentage. Furthermore, the dummy variable **treatment** (equal to 1 if the change in the share of CA classes decreased in a given municipality) was also computed using this dataset.

Additionally, we use data of a set of high school national exams scores[11] at 11th and 12th grades from JNE[12] (Júri Nacional de Exames) to construct the measure of students' achievement with the difference-in-differences approach, analysing the difference before and after the policy was implemented. We consider this exams a fair proxy for student achievement as national exam scores determine 50% of college admittance in Portuguese universities, thus, having a determining impact on student's future professional careers.

The dataset starts at the academic year 2006/07 and continues until the academic year 2018/19. However, high school students first exposed to this policy, in 2016/17, were in the 10th grade and would only perform national exams in the 11th and 12th grade, the two following years. Therefore, we can only observe policy effects on exam scores from 2017/18 onwards. Thus,

[10] Associação de Estabelecimentos de Ensino Particular e Cooperativo represents private and cooperative (publicly-funded private contracts) establishments which are not upper education and is responsible for creating cooperation systems between schools and providing various types of support (legal, technical, etc.)
[11] These scores are comparable at the national level as the exams are equal for all students, graders are assigned at random at the national level and neither the student or the grader know each other's identity. The exams have two calls, done a few weeks apart.
[12] JNE is integrated into the DGE (Direção-Geral da Educação) and is responsible for coordinating, planning and executing final cycle exams, national final examinations, school-level examinations equivalent to national tests, equivalence tests for the 1st, 2nd and 3rd cycles of basic education (*Básico*) and high school (*Secundário*)

we decided to restrict the sample for exams performed in the academic year of 2012/13 onwards. Furthermore, the sample was restricted to exams performed in the first call as the second call may bias our analysis – students that repeated the exam and would be counted twice in our sample and they may not be comparable to the first call group (different population). After which we standardized the mean grade for each municipality and exam, by academic year[13].

When performing the analysis, observations of students with previous retentions were excluded from the sample – students that were over 17 years old when performing a 11th grade exam or over 18 years old when performing a 12th grade exam. This allows avoiding the cases where students failed 10th grade or 11th grade and, thus, were not affected by the policy. Moreover, using the data from AEEP, we determined the municipalities with CA schools in the academic year 2015/16, the year before the policy went into effect. Only these municipalities were considered (restricted sample, henceforth). Finally, with DiD approaches, we estimate the impact of the **change in the share of classes under CA** and the impact of the policy, using the interaction term between the time and treatment dummy variables.

4.1 Descriptive Statistics

In this subsection, we compare different types of classes in the restricted sample, in which we only consider municipalities with CA schools in the academic year before the policy implementation, 2015/16.

In our restricted sample, according to the DGEEC's report for the academic year 2015/16, from the 5th grade to the 12th grade, the number of classes was 5949, 15% were CA classes, 7% were private and the remaining 78% were public. In the academic year 2017/18, the number of CA classes had decreased by 349, 38%, and the number of private classes decreased by 5.

[13] All the grades were standardized by exam and year.

Whilst the number of public classes only increased by 70. Thus, there was a net decrease 284 classes (5%).

In our restricted sample, in the 10th grade, in 2015/16, there are 1002 classes, 11% are CA, 19% private and 70% public. In the following year all types of classes suffered a decrease. The total decrease was 22%.

From **Table 1**, we can conclude that, in the periods considered, for the 10th grade and overall (5th to 12th grade) the decrease in supply of publicly funded private schools does not appear to have been followed by an increase of the same number in the demand for public or private schools in the selected municipalities, which meant a net decrease in classes. However, the same effect is not as pronounced in the 7th grade.

Table 1

Group Decomposition – Number of Classes (per type of school), Restricted Sample

Type of School	CA	Private	Public	Total
5th to 12th GRADE				
N(2016)	914	415	4620	5949
(% Of Classes)	(15%)	(7%)	(78%)	(100%)
ΔN (2016-2018)	-349	-5	70	-284
(ΔN %)	(-38%)	(-1%)	(2%)	(-5%)
7th GRADE				
N(2016)	124	7	600	731
(% Of Classes)	(17%)	(1%)	(82%)	(100%)
ΔN (2016-2017)	-58	20	15	-23
(ΔN %)	(-47%)	(286%)	(3%)	(-3%)
10th GRADE				
N(2016)	112	186	704	1002
(% Of Classes)	(11%)	(19%)	(70%)	(100%)
ΔN (2016-2017)	-37	-45	-136	-218
(ΔN %)	(-33%)	(-24%)	(-19%)	(-22%)

Furthermore, we standardized the number of classes in the restricted sample by school, grade, and type of class. We then plotted the standardized number of classes for each type across time.

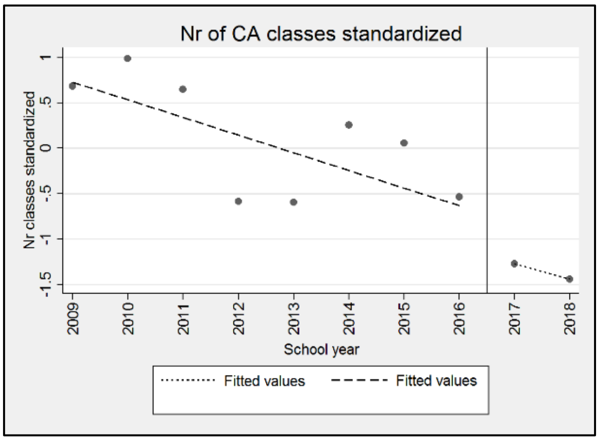

Figure 1

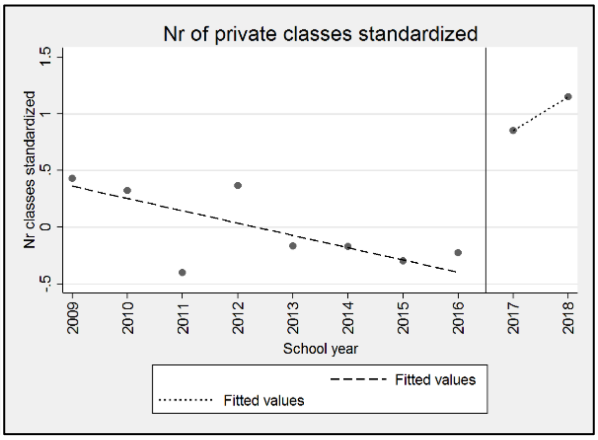

Figure 2

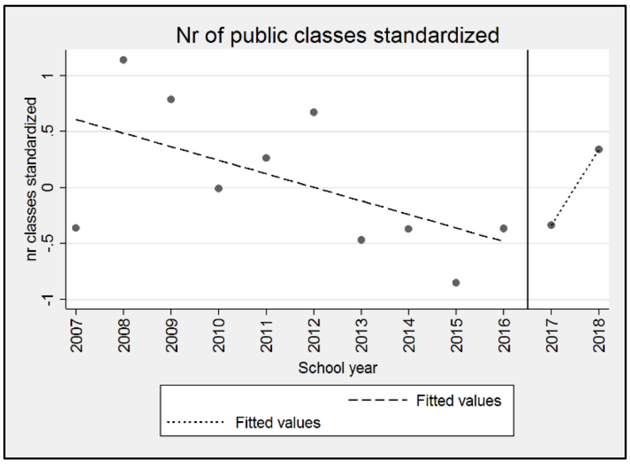

Figure 3

From **Figure 1**, we observe a decrease in CA classes, in the 10th grade, in the years following the policy implementation. Inversely, there was an increase in the standardized number of private and public classes, **Figure 2** and **Figure 3** (in the 10th grade), which supports the idea there was an increase in demand for both public and private education as a response to the decrease in supply of CA classes due to the policy. However, the effect was greater for private schools where we can see a clear increase in the year following the policy compared to a steady behaviour in the years prior[14].

[14] **Figure 8-10 of the Appendix** show the evolution of the three different types of classes similarly to the figures above but, instead, uses the absolute values of the number of classes, which does not control for the overall trends that occur, i.e., the decrease in net classes in the municipalities (restricted sample) in the selected period. The figures present a similar behaviour to the one shown above.

To define the treatment group, the criteria used was if the municipality had lost CA classes in the first year under the policy in the 10th grade. In the complete sample, which all 275 municipalities in Portugal, 244 municipalities are in the control group and 31 are in the treatment group. For the restricted sample, however, 31 are in the treatment group and only 17 are in the control group, which adds to 48 municipalities in total.

Moreover, **Figure 4**, presents how the treatment group, the 31 municipalities that lost CA classes, is distributed in terms of losses. **The share of classes under CA** between 2015/16 and 2016/17 decrease less than 20% for more than half of the municipalities in the treatment group.

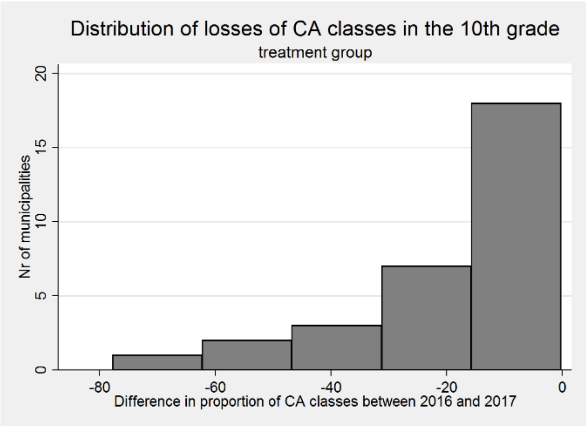

Figure 4

5. Analytical framework

In this section we will present the two approaches used to estimate the policy impacts on student achievement. The difference-in-differences strategy allows to estimate the impact of the policy on student achievement and the second approach measures the intensity of this effect.

Difference-in-Differences Approach

In this study, we observe municipalities that were affected differently by the policy. We use a difference-in-differences strategy that explores variation across groups in two periods, the year before the policy and the year after, which depend on the grade at which the exam is taken. In turn, this strategy allows us to deal with endogeneity created by municipality-specific unobservable characteristics that are fixed through time.

Our identification strategy consists in estimating the effect of losing CA schools, in the first year of the policy, on the mean grade of a certain exam or a set of exams:

$$meangrade_{mt} = \alpha YearT_t + \gamma treatment_m + \beta YearT_t * treatment_m + \varepsilon_{mt} \ (1)$$

$$m = 1, \dots, N; \ t = 2017, 2018 \text{ or } t = 2018, 2019$$

Where the $\boldsymbol{meangrade_{mt}}$ is the mean exam grade in municipality *m* and year *t*. $\boldsymbol{treatment_m}$ is a dummy variable equal to 1 if municipality *m*'s **share of classes under CA** in the 10th grade suffered a decrease between 2015/16 and 2016/17, i.e., if the municipality is in the treatment group. Finally, $\boldsymbol{YearT_t}$ is a dummy variable equal to 1 if year *t* is under the new policy. N is the number of municipalities in the sample. The coefficient **β** of the $YearT_t * treatment_m$ interaction term, captures the impact of the policy on the exam grades. The validity of the difference-in-differences approach relies on the parallel-trends assumption that differences between treatment and control groups are constant over time.

Intensity of the effect

Our modelling strategy for the second specification was constructed to observe the intensity of the effect, i.e., if municipalities that lost a greater share of classes under CA experienced greater effects in terms of exam grades. We consider the following regression:

$$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt} \ (2)$$

$$m = 1, \dots, N; \ t = 2018 \text{ or } t = 2019$$[15]

[15] If the exam is done in the 11th grade t=2018 and if the exam is done in the 12th grade t=2019, which correspond to the treatment year. Exams are always done in the end of the academic year, an exam performed in 2018/19 corresponds to t=2019.

In this specification, we consider the mean exam grade of each municipality of our restricted sample for the first year affected by the policy, $\boldsymbol{meangrade_{mt}}$ and for the year before, $\boldsymbol{meangrade_{m,t-1}}$. The mean grade in year *t* is regressed on the mean grade in the year before, *t*-1, and on our variable of interest, the difference in the number of CA classes relative to the sum of public and CA classes in the 10th grade between 2015/16 and 2016/17, i.e., $\boldsymbol{DproportionCA_m}$. Finally, $\boldsymbol{\varepsilon_{mt}}$ is the error term. N is the number of municipalities in the sample.

This specification relies on the assumption that the explanatory variable, the share of classes under CA in a municipality, is not correlated with an unobservable variable that also influences the dependent variable, the mean grade in the same municipality. If this assumption fails, there is omitted variable bias. This model also assumes the intensity of the effect is linear[16].

6. Results

In this section we start with validity tests to justify our regression results, after which we present our estimations of the effects of school closures on achievement and, finally, the robustness tests.

6.1 Validity tests

In this subsection we test the validity of our difference-in-difference strategy. We address the parallel-trends assumption and examine if it holds before the treatment year. We start by comparing the characteristics between the treatment and control groups (in the restricted and full sample).

Difference between treatment and control municipalities

[16]A quadratic model was tested in Stata and yielded no statistically significant results.

In order to compare the characteristics between the treatment and control groups, we selected three characteristics, the mean standardized grades of the municipalities in each group, the share of public students and the share of male students. The results are presented below.

Table 2
Summary statistics comparing the treatment and control groups of the restricted and full samples.

Exams

		Treatment group		Control group – restricted sample		Control group – full sample	
	Year	**2018**	**2019**	**2018**	**2019**	**2018**	**2019**
Maths	Mean grade	0.143	0.158	0.063	0.055	0.066	0.059
	Male students (%)	45	55	50	21	51	50
	Public students (%)	61	81	100	93	98	98
Portuguese	Mean grade	0.177	0.163	0.103	0.107	0.103	0.108
	Male students (%)	45	39	50	29	41	38
	Public students (%)	81	87	93	100	98	97
	Year	**2017**	**2018**	**2017**	**2018**	**2017**	**2018**
BG	Mean grade	0.180	0.195	0.087	0.087	0.089	0.088
	Male students (%)	42	39	29	64	35	44
	Public students (%)	81	74	100	93	98	97
GD	Mean grade	0.159	0.219	0.130	0.109	0.129	0.113
	Male students (%)	38	65	40	45	41	40
	Public students (%)	65	69	92	100	96	96
Econ	Mean grade	0.221	0.158	0.163	0.160	0.162	0.159
	Male students (%)	56	50	46	50	46	47
	Public students (%)	81	83	100	100	94	94
Philosophy	Mean grade	0.161	0.197	0.092	0.082	0.091	0.083
	Male students (%)	23	26	38	31	36	36
	Public students (%)	84	87	100	100	97	97
FQ	Mean grade	0.163	0.159	0.060	0.058	0.061	0.060
	Male students (%)	32	55	50	36	45	49
	Public students (%)	77	77	100	100	95	98
Geography	Mean grade	0.175	0.125	0.083	0.086	0.087	0.088
	Male students (%)	32	39	36	43	39	32
	Public students (%)	74	68	100	100	97	98
History	Mean grade	0.309	0.288	0.254	0.200	0.253	0.199
	Male students (%)	40	32	50	50	28	30
	Public students (%)	73	65	100	100	98	97

Mean grades are standardized by exam and year.

We can observe small differences for the mean grades within each group across years (year before and after the policy affected those exams). The mean grades of the two control groups are very similar . Also, the share of public students increased for most exams in the treatment group. There are also smaller variations in the share of public students in the control group of the restricted sample, which might be explained by the decrease in net number of classes[17]. Furthermore, the share of male students presents significant variation across groups and years.

Parallel-Trends Analysis

The validity of the difference-in-differences approach relies on the parallel-trends assumption, or rather the assumption that no time-varying differences exist between the treatment and control groups. Thus, we performed a visual analysis to check for validity. The figures below expose the parallel behaviour of both the treatment and control group in the restricted sample, which supports the validity of our DiD approach.

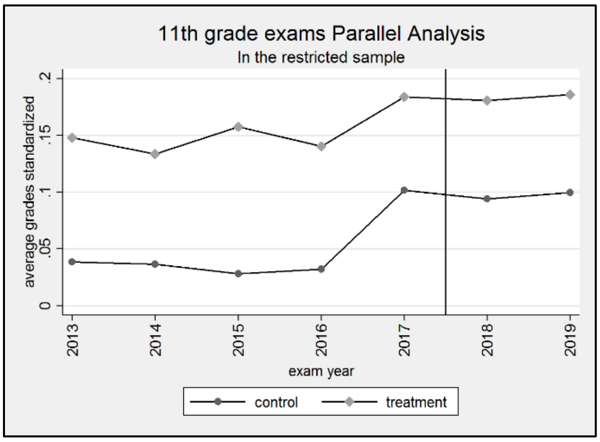

Figure 5

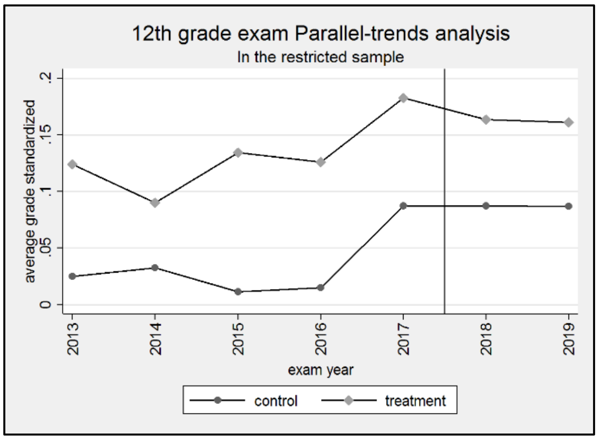

Figure 6

[17] Described previously in subsection 4.1 Descriptive Analysis

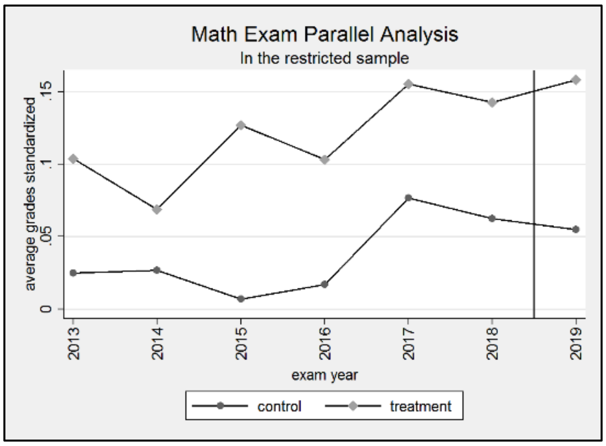

Figure 7

This visual analysis also provides an indication of the estimated treatment effects. For the set of 11th grade exams in the restricted sample, **Figure 5**, it appears the effect of treatment on treated is not significant. Which is further supported by our regression results for both samples. For the set of 12th grade exams, **Figure 6**, we can observe a more significant effect of treatment on treated, which appears to be negative. However, we still cannot be sure about its level of significance. The regression results suggest it is not significant. In **Figure 7**, the effect of treatment on treated appears to be positive and possibly significant. However, the regression results do confirm a positive result but not statistically significant[18].

6.2 Effects of school closures on achievement

In this subsection two estimation strategies are used, the first is a difference-in-difference approach which estimates the impact of the termination of the contracts on the mean grades of the municipalities that lost CA classes. The second measures the intensity of the effects using a continuous explanatory variable, the share of CA classes in each municipality.

[18] When extending our sample to all municipalities (Figures 11-13 of the Appendix), instead of our restricted sample, we are increasing our control group while maintaining our treatment group the same. This allows us to see if the parallel trends hold better for this group. The figures are very similar. However, the restricted sample is better for the analysis given that the control group's characteristics, in the restricted sample, are more similar to the treatment group than the control group in the unrestricted sample, which can be seen in Table 2.

DiD Approach

The estimation results of the DiD approach are shown in Column 1 of **Table 2**.[19]

Regarding the results, in all individual exams and the exams combined, no significant effects were found for the coefficient of interest, **β**. This suggests that the policy had no effect on the mean exam grades of students. These findings are consistent with broad observational findings on charter schools in the US[20].

Intensity of the effect

Next, we estimate the impact of a **change in the share of classes under CA**. The estimation results are shown in Column 2 of **Table 3** below. Each row corresponds to a different exam, and the last to all the exams combined.

Looking at the results, no coefficient for $DproportionCA_m$ is statistically significant, at any level, which suggests that there is no impact of a change in the share of classes under CA in the high school exam grades. Furthermore, the control variable, mean grade in the year before in that municipality, explains most of the variation in the outcome variable as expected.

[19] Estimations were done in Stata using the xtreg command.
[20] Discussed in section 2: Literature Review.

Table 3 - Estimated effects of losing CA on secondary education exam's standard deviation scores

Exams	$meangrade_{mt} = \alpha YearT_t + \gamma treatment_m + \beta YearT_t * treatment_m + \varepsilon_{mt}$ (1)	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}$ (2)	
Maths			$R^2 = 0.08$
$DproportionCA_m$	—	0.001(0.002)	
$\alpha meangrade_{m,t-1}$	—	0.362 (0.133)***	
$YearT_t$	0.051(0.062)	—	
$YearT_t * treatment_m$	-0.019(0.078)	—	
N	294(49)	48	
Portuguese			$R^2 = 0.21$
$DproportionCA_m$	—	-0.001(0.002)	
$\alpha meangrade_{m,t-1}$	—	0.299(0.132)**	
$YearT_t$	0.058(0.045)	—	
$YearT_t * treatment_m$	-0.024(0.056)	—	
N	293(49)	48	
BG			$R^2 = 0.4$
$DproportionCA_m$	—	-0.001(0.002)	
$\alpha meangrade_{m,t-1}$	—	0.657(0.118)***	
$YearT_t$	-0.007(0.047)	—	
$YearT_t * treatment_m$	-0.011(0.058)	—	
N	96(48)	48	
GD			$R^2 = 0.52$
$DproportionCA_m$	—	-0.002(0.003)	
$\alpha meangrade_{m,t-1}$	—	0.611(0.101)***	
$YearT_t$	0.067(0.086)	—	
$YearT_t * treatment_m$	-0.109(0.106)	—	
N	79(41)	39	
Econ			$R^2 = 0.75$
$DproportionCA_m$	—	-0.002(0.004)	
$\alpha meangrade_{m,t-1}$	—	0.973(0.107)***	
$YearT_t$	-0.055(0.062)	—	
$YearT_t * treatment_m$	0.011(0.078)	—	
N	88(46)	43	
Philosophy			$R^2 = 0.25$
$DproportionCA_m$	—	0.003(0.003)	
$\alpha meangrade_{m,t-1}$	—	0.494(0.116)***	
$YearT_t$	0.126(0.098)	—	
$YearT_t * treatment_m$	-0.157(0.121)	—	
N	95(48)	47	
FQ			$R^2 = 0.28$
$DproportionCA_m$	—	-0.001(0.001)	
$\alpha meangrade_{m,t-1}$	—	0.713(0.111)***	
$YearT_t$	0.049(0.047)	—	
$YearT_t * treatment_m$	-0.035(0.059)	—	
N	96(48)	48	
Geography			$R^2 = 0.36$
$DproportionCA_m$	—	-0.002(0.002)	
$\alpha meangrade_{m,t-1}$	—	0.578(0.130)***	
$YearT_t$	0.047(0.058)	—	
$YearT_t * treatment_m$	-0.028(0.072)	—	
N	96(48)	48	
History A			$R^2 = 0.24$
$DproportionCA_m$	—	0.004(0.003)	
$\alpha meangrade_{m,t-1}$	—	0.369(0.112)***	
$YearT_t$	0.023(0.101)	—	
$YearT_t * treatment_m$	-0.109(0.126)	—	
N	95(48)	47	
All exams			$R^2 = 0.37$
$DproportionCA_m$	—	-0.000(0.000)	
$\alpha meangrade_{m,t-1}$	—	0.097(0.039)***	
$YearT_t$	0.043(0.036)	—	
$YearT_t * treatment_m$	-0.060(0.045)	—	
N	837	368	

Statistical significance: *p < 0.10; **p < 0.05; ***p < 0.01. Standard errors are in parenthesis. N is the number of observations, in parenthesis is the number of municipalities[21].

[21] The number of municipalities may differ according to the estimation model used. When using a classical DiD model Stata considers municipalities that have only one observation instead of an observation for each time period, which is required for the intensity effect estimation strategy.

Robustness Tests

In this subsection, we introduce different samples and different specifications of the main equations. A trimmed sample analysis is performed by splitting the restricted sample in two sub-samples of either private and CA or public students to verify whether the policy had different effects on each type of student. A placebo analysis is also performed, using the year before the treatment year as the new treatment year, as a further robustness test. Lastly, different specifications derived from the two main equations are computed for the same reason.

We find no statistically significant effects using the full sample (instead of the restricted sample) for **equation 1** as can be seen in the results of **Table 4** of the Appendix, which further suggests the policy has no significant effects on exam scores and confirms the robustness of the results obtained in column 1 of **Table 2**.

The trimmed sample analysis considers two different sub-samples. Students in the restricted sample that were in private or CA classes and students that were in public classes. We then apply the same methodology as before in regard to **equation 2**. The results are shown in **Table 5** of the Appendix.

From **Table 5**, we can observe a statistically significant effect for the history exam for the subsample of private students only: If the share of classes under CA decrease by 1%, students in public schools would have increased their grades by 1.2% standard deviations, on average, ceteris paribus. However, this effect is not statistically significant for any other individual exam, which suggests the effect in question was not caused by the policy.

The placebo analysis, in **Table 6**, consists in estimating the specifications of both the trimmed sample analysis and the parallel-trends analysis using the year prior to treatment, *t*-1 instead of *t*. We expect to find no significant effects in our coefficient of interest, as in the year prior to treatment we should not observe any effects on treated. We find the coefficient for the Portuguese exam to be statistically significant (at 10% but not at 5%) using the parallel-trends specification, column 1, however given that no other exam has a significant coefficient, the adopted approach is valid. In the trim analysis, column 2, however, some coefficients are statistically significant which might invalidate the approach used.

Furthermore, a different specification of **equation 1** is estimated, not controlling for the mean exam grade for the year before, *t*-1, and instead using a different outcome variable, the variation in the mean grade between year *t* and *t*-1. The results are shown in Column 1 of **Table 7** of the Appendix and seem to be similar in most cases, which supports the robustness of our results. In the cases they differ, it might be explained by some unobserved time-effects.

Column 3 and 4 of **Table 7** considers a pool of years between 2015 and 2019, instead of only 2 periods. Where column 3 corresponds to a fixed effects regression estimated using **equation 1**, and column 4 corresponds to the standard **equation 1**. Only three exams are statistically significant which further suggests the policy had no effect on exam grades. If a municipality lost CA classes due to the policy, students had **math scores** 17.6% of a standard deviation higher, on average, ceteris paribus. For **geometry** and **physics and chemistry** exam scores the results were -28.3% and 9.4% of a standard deviation, respectively. The results for maths are not consistent with the findings by Rosado and Seabra (2015) and Flacker (2014), while the results for physics and chemistry are consistent with the findings by Mancebón et al. (2010).

Table 8 uses a dummy variable on **equation 2** instead of a continuous variable to check for robustness. The results seem to be unchanged. All tables in the Appendix have different specifications of the main equations for the same reasons explained previously. The results seem to be unchanged.

7. Conclusion

The purpose of this study was to evaluate the effect of the policy which terminated a significant portion of public-funded private classes in Portugal and resulted in school closures, by using a DiD strategy on a sample of exam grades and looking at the variation within the municipalities considered. We find evidence this policy increased the supply of both public and private schools, the latter suffering the greatest increase which may be evidence that private schools rapidly adjust their supply in response to an increase in demand caused by the schooling policy (Dinerstein et al., 2021).

Considering the existing literature on the performance of publicly funded private school students, the results obtained, regarding the policy effects on student achievement, are consistent with the main findings on charter school performance in the US (CREDO, 2013). Test scores did not differ significantly from public-funded schools to traditional public schools in the US and average municipality scores on high school national exams in Portugal were unaffected by public-funded school closures.

Although our research suggests the policy did not cause an effect on student achievement, to fully understand the impact on welfare of this policy, other outcome variables should be considered. One example are dislocation costs for students that changed schools. CA students might have had greater dislocation costs before the policy, as they preferred CA schools to public schools and were willing to travel further. Thus, the policy would cause these costs to decrease, which would positively contribute to the student's welfare, ceteris paribus. However, the opposite may be true. After the policy, students affected could have had greater dislocation costs as near public schools could be, on average, further away than their previous CA school.

Another variable necessary to understand the full impact on welfare is the change in public spending caused by this policy. Did costs increase due to the influx of students that changed to public schools (hiring more teachers, more spending on infrastructure, etc.)? Or did it decrease in costs due to the termination of contracts? Which of these two effects was stronger, i.e., are CA students cheaper than public students in terms of public spending? If so, then this policy would decrease net social welfare, ceteris paribus.

Furthermore, it is safe to assume there was an increase in education costs for students that moved from CA classes to private classes (either in the same school or to another private school), as they started paying tuition fees[22], which negatively impacts their welfare, ceteris paribus. Additionally, another factor to consider in terms of education costs incurred by students is the spending on tutoring caused by this policy. Did parents decide to invest more in private tutoring as their children lost access to private education? However, the impact on net welfare is not clear, as private tutoring may have a positive effect on student achievement.

Finally, it is important to consider the welfare effects of this policy on schools that possessed these contracts and lost them. Were they forced to shut down? Were other private classes shut down? How many teachers were unemployed?

In conclusion, the results obtained suggest the policy that terminated a significant amount of CA classes did not affect student achievement. However, further research is required to determine the welfare impact of this policy. Such research is important to evaluate the legitimacy of a policy that raised concerns for many.

[22] Except in cases where students earned a scholarship.

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Appendix

Table 4

Estimated effects of losing CA on secondary education exam's standard deviation scores **considering an unrestricted sample – Parallel-trends Analysis (two periods)**

Exams	$(FE) meangrade_{mt} = \alpha YearT_t + \beta YearT_t * treatment_m + \varepsilon_{mt}$	$meangrade_{mt} = \alpha YearT\_t + \gamma treatment_m + \beta YearT_t * treatment_m + \varepsilon_{mt}$
Maths		
$YearT_t * treatment_m$	0.067(0.066)	0.066(0.066)
N	548	548
Portuguese		
$YearT_t * treatment_m$	0.030(0.057)	0.031(0.058)
N	549	549
BG		
$YearT_t * treatment_m$	0.006(0.050)	0.007(0.050)
N	549	549
GD		
$YearT_t * treatment_m$	-0.080(0.087)	-0.076(0.086)
N	305	305
Econ		
$YearT_t * treatment_m$	-0.017(0.090)	-0.019(0.089)
N	383	383
Philosophy		
$YearT_t * treatment_m$	-0.023(0.097)	-0.024(0.096)
N	529	529
FQ		
$YearT_t * treatment_m$	-0.011(0.056)	-0.010(0.056)
N	548	548
Geography		
$YearT_t * treatment_m$	0.003(0.085)	-0.003(0.085)
N	537	537
History A		
$YearT_t * treatment_m$	-0.079(0.101)	-0.091(0.100)
N	523	523
All exams		
$YearT_t * treatment_m$	-0.013(0.032)	-0.013(0.036)
N	3,774	3,774

Standard errors are in parentheses. Statistical significance: *p < 0.10; **p < 0.05; ***p < 0.01. N is the number of observations.

Table 5

Estimated effects of losing CA on secondary education exam's standard deviation scores **for private and CA students and for public students – trim analysis**

Exams	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}$ *if* $pub = 1$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}$ *if* $pub = 0$
Maths		
$DproportionCA_m$	-0.003(0.002)	-0.005(0.005)
N	47	25
Portuguese		
$DproportionCA_m$	0.002(0.002)	-0.002(0.004)
N	47	25
BG		
$DproportionCA_m$	-0.002(0.002)	-0.000(0.003)
N	47	27
GD		
$DproportionCA_m$	-0.000(0.003)	0.008(0.017)
N	38	16
Econ		
$DproportionCA_m$	-0.003(0.005)	0.010(0.016)
N	42	16
Philosophy		
$DproportionCA_m$	0.003(0.003)	-0.004(0.006)
N	46	25
FQ		
$DproportionCA_m$	-0.002(0.001)	-0.000(0.003)
N	47	27
Geography		
$DproportionCA_m$	-0.003(0.002)	-0.002(0.007)
N	47	20
History A		
$DproportionCA_m$	0.004(0.003)	0.012(0.006)*
N	46	22
All exams		
$DproportionCA_m$	-0.001(0.001)	0.001(0.002)
N	461	297

Standard errors are in parentheses. Statistical significance: *p < 0.10; **p < 0.05; ***p < 0.01. N is the number of observations.

Table 6

Estimated effects of losing CA on secondary education exam's standard deviation scores considering the previous year as the treatment year – **placebo effect**

Exams	$meangrade_{mt} = \alpha YearT\_t + \gamma treatment_m + \beta YearT_t * treatment_m + \varepsilon_{mt}$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}\ if\ pub = 1$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}\ if\ pub = 0$
Maths			
β	-.022(0.057)	0.003(0.002)	0.004(0.003)
N	547(274)	47	27
Portuguese			
β	-0.104(0.063)*	0.005(0.002)**	0.003(0.003)
N	547(275)	47	27
BG			
β	-0.075(0.054)	0.002(0.002)	0.001(0.004)
N	546(275)	47	27
GD			
β	-0.110(0.091)	-0.004(0.007)	-0.001(0.006)
N	285(154)	36	17
Econ			
β	-0.115(0.094)	0.009(0.004)**	0.008(.0140136
N	332(191)	43	15
Philosophy			
β	0.078(0.109)	-0.002(0.003)	0.010(0.004)**
N	524(273)	46	24
FQ			
β	-0.050(0.067)	0.001(0.001)	0.004(0.003)
N	545(275)	47	26
Geography			
β	-0.036(0.089)	0.002(0.003)	0.016(0.005)***
N	528(268)	47	22
History A			
β	-0.034(0.111)	0.002(0.004)	-0.000(0.006)
N	510(265)	46	21
All exams			
β	-0.034(0.037)	0.002(0.001)**	0.005(0.001)***
N	4,634	455	298

Standard errors are in parentheses. Statistical significance: *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. N is the number of observations, in parenthesis is the number of municipalities.

Table 7

Estimated effects of losing CA on secondary education exam's standard deviation scores

Exams	$Dmeangrade_m = \beta DproportionCA_m + \varepsilon_m$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}$	$(FE) meangrade_{mt} = \alpha YearT_t + \beta YearT_t * treatment_m + \varepsilon_{mt}$	$meangrade_{mt} = \alpha YearT_t + \gamma treatment_m + \beta YearT_t * treatment_m + \varepsilon_{mt}$
Maths				
$DproportionCA_m$	-0.005(0.002)**	0.001(0.002)	—	—
$\alpha meangrade_{m,t-1}$	—	0.362 (0.133)***	—	—
$YearT_t$	—	—	0.034(0.036)	0.034(0.036)
$YearT_t * treatment_m$	—	—	0.176(0.064)***	0.176(0.064)***
N	48	48	49 (294)	49 (294)
Portuguese				
$DproportionCA_m$	-0.001(0.002)	-0.001(0.002)	—	—
$\alpha meangrade_{m,t-1}$	—	0.299(0.132)**	—	—
$YearT_t$	—	—	.017(0.035)	.017(0.035)
$YearT_t * treatment_m$	—	—	.004(0.064)	.004(0.064)
N	48	48	49 (293)	49 (293)
BG				
$DproportionCA_m$	-0.001(0.002)	-0.001(0.002)	—	—
$\alpha meangrade_{m,t-1}$	—	0.657(0.118)***	—	—
$YearT_t$	—	—	0.026(0.026)	0.026(0.026)
$YearT_t * treatment_m$	—	—	0.061(0.047)	0.061(0.047)
N	48	48	49 (294)	49 (294)
GD				
$DproportionCA_m$	-0.004(0.004)	-0.002(0.003)	—	—
$\alpha meangrade_{m,t-1}$	—	0.611(0.101)***	—	—
$YearT_t$	—	—	.070(0.046)	.071(0.046)
$YearT_t * treatment_m$	—	—	-0.283(0.093)***	-0.283(0.093)***
N	39	39	40 (216)	40 (216)
Econ				
$DproportionCA_m$	-0.002(0.003)	-0.002(0.004)	—	—
$\alpha meangrade_{m,t-1}$	—	0.973(0.107)***	—	—
$YearT_t$	—	—	0.063(0.045)	0.0473(0.045)
$YearT_t * treatment_m$	—	—	-0.045(0.093)	-0.066(0.091)
N	43	43	46 (222)	46 (222)
Philosophy				
$DproportionCA_m$	0.002(0.003)	0.003(0.003)	—	—
$\alpha meangrade_{m,t-1}$	—	0.494(0.116)***	—	—
$YearT_t$	—	—	0.113(0.058)*	0.114(0.059)*
$YearT_t * treatment_m$	—	—	-0.025(0.106)	-0.033(0.106)
N	47	47	49 (286)	49 (286)
FQ				
$DproportionCA_m$	-0.001(0.001)	-0.001(0.001)	—	—
$\alpha meangrade_{m,t-1}$	—	0.713(0.111)***	—	—
$YearT_t$	—	—	-0.004(0.027)	-0.004(0.027)
$YearT_t * treatment_m$	—	—	0.094(0.049)*	0.094(0.049)*
N	48	48	49 (294)	49 (294)
Geography				
$DproportionCA_m$	-0.005(0.002)**	-0.002(0.002)	—	—
$\alpha meangrade_{m,t-1}$	—	0.578(0.130)***	—	—
$YearT_t$	—	—	0.029(0.029)	0.030(0.029)
$YearT_t * treatment_m$	—	—	0.018(0.054)	0.012(0.054)
N	48	48	49 (286)	49 (286)
History A				
$DproportionCA_m$	0.004(0.003)	0.004(0.003)	—	—
$\alpha meangrade_{m,t-1}$	—	0.369(0.112)***	—	—
$YearT_t$	—	—	0.079(0.046)*	0.077(0.045)*
$YearT_t * treatment_m$	—	—	-0.065(0.082)	-0.073(0.083)
N	47	47	49 (283)	49 (283)
All exams				
$DproportionCA_m$	-0.001(0.001)	-0.000(0.000)	—	—
$\alpha meangrade_{m,t-1}$	—	.0967(0.039)***	—	—
$YearT_t$	—	—	0.042(0.016)***	0.040(0.018)**
$YearT_t * treatment_m$	—	—	-0.033(0.029)	-0.034(0.032)
N	368	368	2,058	2,058

Standard errors are in parentheses. Statistical significance: *p < 0.10; **p < 0.05; ***p < 0.01. N corresponds to the number of municipalities, in parenthesis is the number of observations.

Table 8

Estimated effects of losing CA on secondary education exam's standard deviation scores **using dummy variables**

Exams	$Dmeangrade_m = \beta DproportionC_{A} + \varepsilon_m$	$Dmeangrade_m = \beta treatment_m + \varepsilon_m$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta DproportionCA_m + \varepsilon_{mt}$	$meangrade_{mt} = \alpha meangrade_{m,t-1} + \beta treatment_m + \varepsilon_{mt}$
Maths				
$DproportionCA_m \mid z_m$	-0.005(0.002)**	0.188(0.086)**	0.001(0.002)	0.095(0.074)
$\alpha meangrade_{m,t-1}$	—	—	0.362 (0.133)***	0.363(0.135)
N	48	48	48	48
Portuguese				
$DproportionCA_m \mid z_m$	-0.001(0.002)	0.078(0.070)	-0.001(0.002)	0.040(0.061)
$\alpha meangrade_{m,t-1}$	—	—	0.299(0.132)**	0.404(0.146)***
N	48	48	48	48
BG				
$DproportionCA_m \mid z_m$	-0.001(0.002)	0.073(0.060)	-0.001(0.002)	0.071(0.057)
$\alpha meangrade_{m,t-1}$	—	—	0.657(0.118)***	0.717(0.117)***
N	48	48	48	48
GD				
$DproportionCA_m \mid z_m$	-0.004(0.004)	-0.150(0.122)	-0.002(0.003)	-0.123(0.103)
$\alpha meangrade_{m,t-1}$	—	—	0.611(0.101)***	0.607(0.098)***
N	39	39	39	39
Econ				
$DproportionCA_m \mid z_m$	-0.002(0.003)	0.052(0.088)	-0.002(0.004)	0.045(0.092)
$\alpha meangrade_{m,t-1}$	—	—	0.973(0.107)***	0.967(0.105)
N	43	43	43	43
Philosophy				
$DproportionCA_m \mid z_m$	0.002(0.003)	-0.072(0.122)	0.003(0.003)	-0.111(0.099)
$\alpha meangrade_{m,t-1}$	—	—	0.494(0.116)***	0.421(0.115)
N	47	47	47	47
FQ				
$DproportionCA_m \mid z_m$	-0.001(0.001)	0.082(0.047)*	-0.001(0.001)	0.072(0.047)
$\alpha meangrade_{m,t-1}$	—	—	0.713(0.111)***	0.815(0.114)***
N	48	48	48	48
Geography				
$DproportionCA_m \mid z_m$	-0.005(0.002)**	0.130(0.081)	-0.002(0.002)	0.078(0.080)
$\alpha meangrade_{m,t-1}$	—	—	0.578(0.130)***	0.673(0.134)***
N	48	48	48	48
History A				
$DproportionCA_m \mid z_m$	0.004(0.003)	-0.203(0.123)	0.004(0.003)	-0.186(0.095)*
$\alpha meangrade_{m,t-1}$	—	—	0.369(0.112)***	0.378(0.110)***
N	47	47	47	47
All exams				
$DproportionCA_m \mid z_m$	-0.001(0.001)	0.020(0.033)	-0.000(0.001)	-0.009(0.029)
$\alpha meangrade_{m,t-1}$	—	—	.0590(0.039)***	0.592(0.040)***
N	368	368	368	368

Standard errors are in parentheses. Statistical significance: *p < 0.10; **p < 0.05; ***p < 0.01. N is the number of observations.

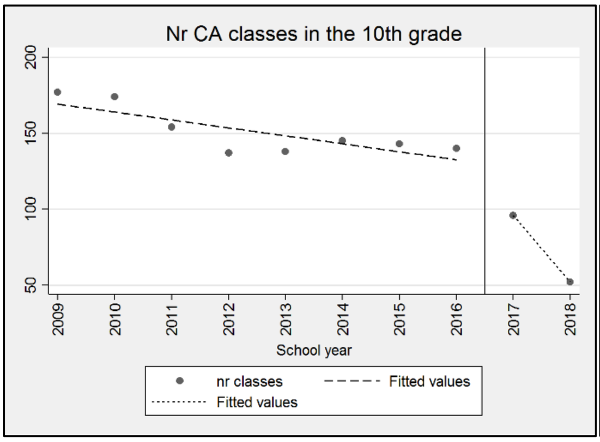

Figure 8

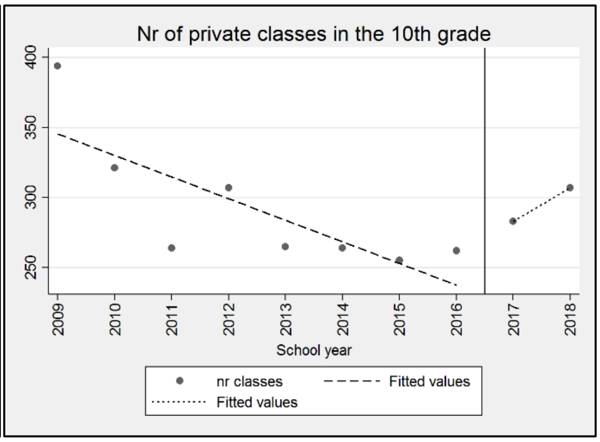

Figure 9

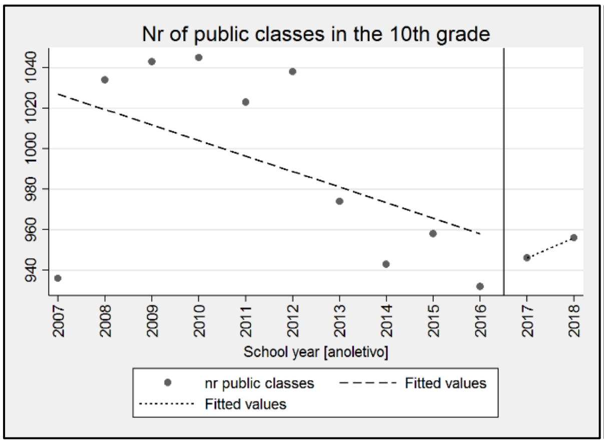

Figure 10

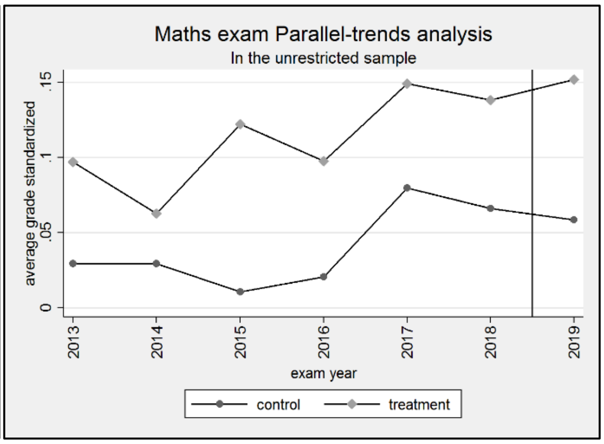

Figure 11

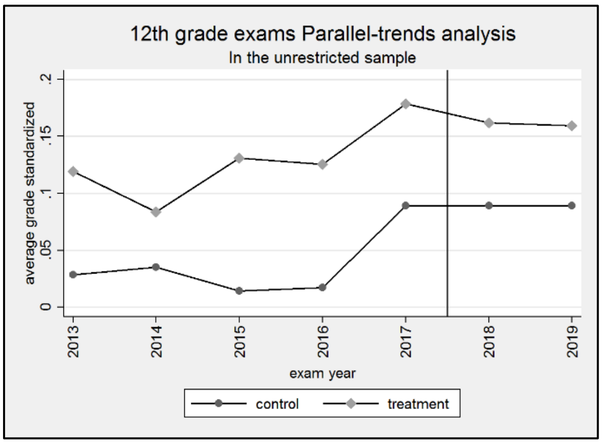

Figure 12

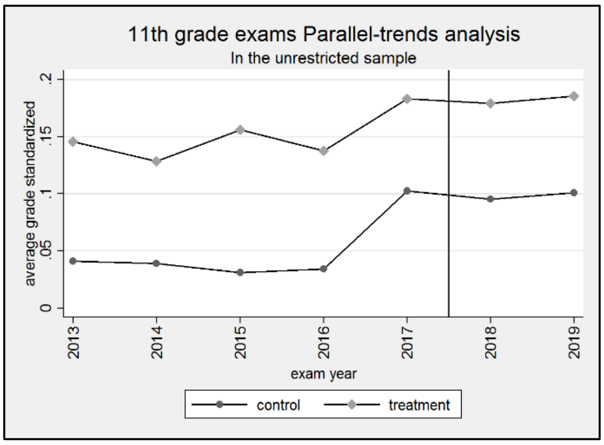

Figure 13