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Too young to quit school? Increasing the compulsory schooling leaving age and students' educational paths

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Abstract

I study how a significant increase in the compulsory schooling leaving age, from 15 to 18 years old, can contribute to reducing early school leaving and changing students' educational paths. I analyse the Portuguese reform of 2009, exploiting the fact that grade retention in the 7th grade in this year provides quasi-experimental variation in exposure to the new policy. While effects for the overall student population are small or null, lower-achieving students significantly increase their schooling duration. Additionally, some sub-groups of lower-achieving students, particularly boys and those enrolling in upper-secondary school, increased their graduation probabilities. At the same time, I do not find that school quality decreased. These findings carry implications for research using compulsory schooling reforms as instruments for education, and inform policies aimed at supporting at-risk students.

Keywords: Compulsory schooling age, school dropout, early school leaving, difference-in-differences, grade retention

JEL Codes: I21, I22, I26, I28

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1 Introduction

Reducing early school leaving (ESL), defined as leaving education with at most lower-secondary schooling[1], is generally considered to be one of the top priorities of educational policy. ESL is associated with higher risks of unemployment and criminal activity, lower lifetime earnings, and poorer health (Brunello and Paola, 2014). By the year 2000, the ESL rate in the European Union (EU) averaged about 20%. Because of the high social and economic costs of ESL, the EU set as a Europe 2020 goal to bring this rate to under 10%, a target that has been met, with 2020 rates at 9.9%. Despite this positive trend, the prevailing consensus is that there is still considerable room for improvement, prompting countries worldwide to implement policies aimed at enhancing these rates.

A policy that has historically been applied with the objective of reducing ESL and dropout rates is to increase the compulsory schooling leaving age (CSL). By mandating that students stay longer in schools, we would expect that more of them eventually obtain an upper-secondary school diploma — specifically, the *marginal* students that change their educational path because of the policy (Harmon, 2017). However, it is still unclear how effective raising the CSL age is as a policy instrument to improve these rates. While it could increase graduation probabilities and reduce ESL rates by mitigating the perceived cost of additional years of education needed to graduate (Oreopoulos, 2009; Cabus and De Witte, 2011); it might also make students stay longer in school without influencing graduation probabilities, particularly if *marginal* students choose to drop out as soon as possible (Landis and Reschly, 2011; Adamecz, 2023).

In this paper, I provide new evidence on the effects of increasing the CSL age on students' educational paths. The recent and large Portuguese reform provides a unique setting to study such effects. In 2008, Portugal had the highest ESL rate in the EU, at 35%, before raising

[1] "Early leaver from education and training, previously named early school leaver, refers to a person aged 18 to 24 who has completed at most lower secondary education and is not involved in further education or training; the indicator '*early leavers from education and training*' is expressed as a percentage of the people aged 18 to 24 with such criteria out of the total population aged 18 to 24." Source: Eurostat

its CSL age from 15 to 18 in 2009. Recently, in 2020, it achieved a rate of 8.9%, meeting the Europe 2020 target of 10%. Furthermore, the availability of yearly data on the universe of students in the country, with comprehensive detail on both socioeconomic background and academic achievement, enables a clearer understanding of the specific *marginal* students affected by this policy. The law determined that all students enrolled in grades 1 to 7 from the academic year of 2009/10 onwards need to stay in school until they have turned 18 years old, or complete the 12^{th} grade of schooling. On the other hand, students enrolled in the 8^{th} grade or above in 2009/10 would still fall under the old CSL, and could leave school when they turned 15 years old, or completed the 9^{th} grade.

To identify the aforementioned effects, I focus on a group of students from the same grade-cohort for whom exposure to the new CSL was unexpectedly determined by their academic achievement in that year. Students enrolled in the 7^{th} grade in 2008/09 that were retained had to repeat this grade in the subsequent academic year and, as stipulated by the law, were thereby exposed to the new CSL. On the other hand, students that were promoted to the 8^{th} grade still fell under the old CSL. Retention in the 7^{th} grade in the academic year preceding the reform thus provides quasi-experimental variation in exposure to the reform.

Because of unobservable differences between retained and promoted students, I use data from previous 7^{th} grade cohorts for whom retention *vs* promotion did not determine exposure to different CSL ages, and utilize a Difference-in-Differences (DiD) design to isolate the effect of the reform from the confounding effect of retention on outcomes. Furthermore, to add credibility to the parallel-trends assumption, I compare students at the grade retention margin only, identified by school subject scores and retention rules. This methodology rests on the assumption that, during the period of analysis, the difference on educational outcomes between retention *vs* promotion at the margin remained constant.

A threat to the aforementioned identifying assumption would be if schools and teachers purposely changed 7^{th} grade retention practices in the academic year before the policy was implemented, or if students changed their effort levels in response to an anticipation of

different exposures to the policy. Still, I do not find evidence of such behaviour. Balance tests comparing retained with promoted students across academic years show that the composition of these groups did not change significantly in the years surrounding the reform. Moreover, retention rates in the 7th grade also remained stable around the period of analysis.

It is important to mention that retention levels in Portugal are high — the retention rate in the 7th grade between 2007 and 2011 was around 15%, and about 30% students in this grade were at least one year above the reference age, a strong proxy for having had previous grade retentions. Furthermore, experiencing at least one retention is a key predictor of early school dropout. 7th grade students in the analysis period with at least one retention were around 30% less likely to graduate, which implies this group is of particular policy interest for studying increases in the CSL.

I find that as a result of the higher CSL, students stayed longer in school. The probability to comply with the new CSL — defined as staying in school until 18 years old, or graduating upper-secondary school — increased by around 3 percentage points (p.p.) for the first affected cohort at the grade retention margin. However, graduation probabilities were, on average, not affected. There is still substantial heterogeneity within this margin, with positive treatment effects on both outcomes driven by relatively lower-achieving students. I find that the positive effect on compliance is driven by students above the 7th grade reference age (5 p.p.), who were likely retained in previous grades; while students at reference age experience no effect.

For students above the reference age, I estimate that this group was 3.2 p.p. more likely to graduate from upper-secondary school, although this effect is not statistically significant. Exploring further heterogeneity within this sub-group, I find positive effects for students that ever enrolled in upper-secondary school (6.1 p.p.), and boys (5.7 p.p.), that are statistically significant at the 10% level.

I complement the main DiD results for the grade retention margin, with those coming from event-study estimates comparing the outcomes of 7th grade cohorts with those of un-

treated 9^{th} grade cohorts around the years of the policy implementation. Identification rests on the assumption that unobservable cohort characteristics of 7^{th} and 9^{th} graders do not differentially drive outcomes across years. This analysis has the additional advantages of allowing us to understand who the likely *compliers* of the policy were — i.e., those changing educational choices because they were required to stay in school for longer, and would have dropped out earlier in the absence of the reform. Furthermore, it sheds light on whether the policy had spillover effects over the set of *compliers*, impacting education levels beyond those directly affected by the reform, as proposed by Lang and Kropp (1986). This is not entirely possible with the main identification strategy due to the fact that by construction, it captures effects for low-achieving students at the grade retention margin only. The event-study estimates corroborate the heterogeneity in treatment effects found in the main analysis: I find that low socioeconomic status, and low-achieving students increased their compliance probabilities by around 2 and 3 p.p., respectively. On the other hand, high socioeconomic status, and high-achieving students experienced no effects.

I contribute to the literature on compulsory schooling reforms in three main directions. First, I provide comprehensive evidence on the effectiveness of a recent reform, which is particularly relevant for policymakers considering raising their CSL. While only a minority of high-income countries currently mandate students to stay in school until at least 18 years old[2], there have been ongoing compulsory schooling reforms in various countries. For instance, Romania raised its compulsory schooling leaving age to 18 years in 2020, and Finland followed suit in 2021. Nevertheless, research on recent CSL reforms has failed to find clear evidence of their effectiveness in reducing ESL. Cabus and De Witte (2011) show that a one year increase in the CSL age in 2007, from 17 to 18, decreased dropout rates in the Netherlands by around 2.5 p.p. However, as noted by the authors, the observed effect comes entirely from the group non-liable to the policy reform — the control group more often left

[2]Eurydice (2023) indicates that among the education systems analysed in 37 European countries, only 7 mandate a compulsory schooling age of at least 18 years: Belgium, Finland, France, Germany, North Macedonia, Portugal, Romania.

school in the immediate period before the policy reform, which given the economic revival at that time, may reflect anticipation of labour market opportunities. On the other hand, the treatment group's dropout rates did not significantly change. Landis and Reschly (2011) find that higher CSL ages in the US had an effect on the timing of dropping out, but not on high school completion rates, by comparing states that changed the legal requirement from 2002 to 2006 with states that did not. Moreover, Adamecz (2023) finds that the Hungarian CSL increase from 16 to 18 years old in 1996 did not improve graduation probabilities. I show that the Portuguese reform increased the length of schooling for the more vulnerable segments of the student population, with no discernible effects for other groups. Moreover, I provide evidence that CSL increases can increase graduation rates for some at-risk sub-groups of the student population.

Second, I identify CSL *compliers* in a recent setting. The use of an administrative dataset covering the student population, with detailed yearly information on both socioeconomic background and achievement — an aspect previously absent in the literature — allows for such identification. This is important for two main reasons. On one hand, CSL increases are likely to be more effective if schools and policymakers can offer additional, well-targeted interventions to the set of *compliers* who would otherwise leave school (Oreopoulos, 2007, 2009). On the other hand, there is a vast literature using CSL reforms as instruments for education, documenting effects on wages[3], crime, civic participation, intergenerational transmission of education, teenage pregnancy, or health[4]. Still, it is expected that the composition of current *compliers* has changed when compared to that of the second half of the 20th century, which most of this literature explores[5]. I show that *compliers* in a recent

[3]Oreopoulos (2007), Devereux and Hart (2010), and Clay et al. (2021) find that increasing the CSL age had positive wage returns, while Pischke and Von Wachter (2008) find no returns. Moreover, Delaney and Devereux (2019) find that it lead to lower earnings volatility, and Brunello et al. (2009) find that it decreased conditional wage inequality.

[4]See Bell et al. (2016) for a study on CSL and crime; civic participation (Milligan et al., 2004); intergenerational transmission of education (Oreopoulos et al., 2006); teenage pregnancy (Black et al., 2008; Adamecz-Völgyi and Scharle, 2020); and health (Brunello et al., 2016; Malamud et al., 2023).

[5]In fact, Domnisoru (2021) shows that the use of parental background data identifies positive wage returns to a French CSL reform in 1967, previously shown to have generated no average effects.

setting primarily consisted of low-achieving students, while effects for the overall student population are small or null, a finding with implications for future research using CSL as instruments for education.

Lastly, I analyse how upper-secondary school classes changed in student and teacher composition with the arrival of the new CSL cohorts, as well as how school resources changed within the overall school system. Unlike Adamecz (2023), I do not find that school quality decreased. While average class sizes increased by approximately 2 students, spending per student and teacher experience also increased during this period.

This paper is organized as follows: Section 2 describes the Portuguese institutional setting. Section 3 outlines the identification strategy, and Section 4 details the analysis sample. Section 5 presents the main results, with additional results discussed in Section 6. Finally, Section 7 concludes and discusses the implications of the findings.

2 Institutional Setting

2.1 Compulsory schooling reform

Portugal increased its CSL age from 15 to 18 years old in 2009. The fact that the country had one of the EU's highest ESL rate during the 2000s was one of the main drivers of the reform. In 2008, this rate stood at 35%, the highest within the EU, significantly surpassing the EU average of 14.4%. 11 years later, Portugal was able to achieve a rate of 8.9%, meeting the Europe 2020 target of 10% (Figure 1). While this was a very substantial improvement, it remains to understand what contribution the CSL age policy exactly had, as the country's rate was already on a clear downwards trend.

The government first announced in 2007 its plans to increase the CSL age from 15 to 18 years old two years later. In April 2009, it enacted a legislative proposal stipulating that the new CSL would be applicable to all students enrolled in grades up to the 7^{th} in the subsequent academic year. The proposal was debated in parliamentary sessions during June

2009, approved by the parliament in July, and in August of that year the law was finally approved by the President and published[6]. Support for the policy was generally widespread, with political discussions primarily centering on the details of its implementation and the required resources. The law determined that students enrolled in grades 1 to 7 from the academic year of 2009/10 onward would be required to stay in school until they turned 18 years old, or finished the 12th grade of schooling, the last grade of upper-secondary schooling in either the academic or vocational tracks[7]. On the other hand, students enrolled in the 8th grade or above in 2009/10 would remain under the old CSL and could leave school when they turned 15 years old or finished the 9th grade, the last grade of lower-secondary schooling.

The academic year starts in September and finishes at the end of August of the following calendar year, with the summer holiday months going from July to September. The first cohort of students affected by the new CSL age reached 10th grade, the first grade of upper-secondary schooling, in the academic year of 2012/13, if they did not suffer any grade retentions since the implementation of the policy. In August of 2012, two laws were approved in light of the reform and the arrival of the first exposed cohort in upper-secondary school. The first[8] determined measures to prevent early school leaving and failure, such as better guidance and support to at-risk students, giving further emphasis to the enrollment in vocational and alternative tracks as a preventive measure. It also clarified the student's and their guardian's legal responsibilities in terms of school enrollment until the CSL requirements are met. The second altered the labour code to allow minors under 18 years old to work only if they had completed compulsory schooling (i.e., for students still under the old CSL, or for students graduating at 17 years old under the new CSL), or if they are enrolled in upper-secondary schooling[9] — serving therefore, as an additional way to enforce the

[6]Law "Lei n.º 85/2009"

[7]Students in Portugal can graduate from upper-secondary school at 17 years old if their birthday is after the end of the school year, in July; complied with the regular school starting age rule of starting school the year they turn 6 years old; and reached the 12th grade in the expected time (i.e., without retentions).

[8]Law "Decreto-Lei n.º 176/2012".

[9]Law "Lei n.º 47/2012". Before, minors under the age of 16 were only permitted to work if they had completed the previous CSL of 15 years old, and were engaged in light tasks. ("Lei n.º 7/2009").

new CSL. General enforcement was achieved through potential consequences for students' parents or legal guardians: specifically, by risking the loss of welfare support, implementing child protection policies, and imposing fines[10].

The Portuguese CSL reform differs in some aspects from other recent reforms. First, it does not allow for exceptions regarding the requirement that students stay in school until they reach 18 years old or graduate. On the contrary, many US states with CSL set at 17 or 18 years old allow for exceptions if students above a certain age (usually, 16) work, or have parental consent to drop out (Oreopoulos, 2009). Second, compulsory schooling in Portugal has to be completed exclusively in schools. Recent reforms from England and France, extended the age of compulsory schooling *or training* to 18 years old. This implies, for instance, that students in these countries can fulfill the law's requirements through apprenticeships.

2.2 Portuguese educational system

In Portugal, children start school the year they turn 6 years old. Private and public schools coexist in all levels of education[11]. Elementary schooling in Portugal goes from the 1st to the 6th grade and lower-secondary schooling goes from the 7th to the 9th grade. During these first two educational stages there is no ability-tracking and the general track has the vast majority of students enrolled, around 90%. Upper-secondary schooling in Portugal starts in the 10th grade. Students enrolling in this grade need to make the choice of whether to enroll in the vocational track, from which students can choose one of several different fields directed towards earlier integration in the labour market, or the academic track, targeting students who want to pursue a university degree. The academic track is composed of 4 different specialization sub-tracks: Sciences & Technologies; Socio-Economic Sciences; Humanities & Languages; and Arts. Although students are not tracked by ability into their upper-secondary education paths, student and parental selection make it more likely for stu-

[10] Articles 44 and 45 of law "Lei n.º 51/2012".

[11] The percentage of students in private schools is around 13% at all cycles of basic education and of around 20% in upper-secondary education.

dents coming from higher socioeconomic status or with higher prior achievement, to pursue the academic track, and within the academic track, to choose the Sciences & Technologies specialization.

Students in the Portuguese school system are evaluated through teacher assessment and national exams. National exams in Portuguese Language and Mathematics are performed by every student in the system, by the end of the 4^{th} and 6^{th} grades (until 2015), and 9^{th} grade. Students take these exams at exactly the same time, facing the same questions. Exams are then evaluated by a randomly allocated evaluator teacher, from schools other than the school in which the student is enrolled, in an anonymous fashion. In order to complete the general academic track of upper-secondary education, students must also sit through national exams — typically completing two track-specific exams in the 11^{th} grade, and two others in the 12^{th} grade, in most cases, Portuguese language and Mathematics exams. Students can only gain admission to tertiary education if they have a passing grade in both 12^{th} grade exams. Students in the vocational track have the option to take 12^{th} grade exams, enabling them to apply to tertiary education institutes, despite the fact that courses in this track are not designed to prepare for these exams.

Teacher-assigned school grades are based on several coursework elements, that include in-class tests but also homework, oral presentations, class participation and student behavior. From the 1^{st} to the 9^{th} grade, grades are given on 1–5 scale, where 3–5 are passing grades; while in upper-secondary education the 1–20 scale is used, where the passing grades range from 10–20. During lower-secondary school, grades assigned by teachers at the end of each academic year, along with national exam scores in the grades they are taken, are used to determine whether a student is promoted to the next grade or retained, in which case the student has to repeat the same school-grade in the following academic year. Retention is decided by the student's teachers and a class committee at the end of the school year in June. However, as determined by national law, it is only considered when a student has at least two or three subjects (depending on the school-grade and subjects) with failing scores.

Retention levels in Portugal are high and a common practice (Eurydice, 2011). Although this practice has been steadily falling since around 2013, the retention rate in the 7th grade between 2007 and 2011 was around 15%. Moreover, about 30% of students in this grade were at least one year above the reference age, a strong proxy for having had previous grade retentions.

2.3 Data

To analyse the question at hand, I use an administrative dataset[12] containing detailed information on the universe of students enrolled in public and private schools in continental Portugal from the academic year 2006/07 to 2017/18. It contains relevant data on personal and socioeconomic characteristics of each student — such as their gender, birthday, home neighborhood and school attended, country of origin, parents' education, parents' employment status, whether they receive socioeconomic support[13], and have access to computer or internet at home — with minimum measurement error or missing information. These data are used to construct the control variables described in Appendix Table A1. Moreover, the dataset includes information of students' teachers and school subject grades. A unique student identifier allows tracking students throughout grades and gathering additional information about their educational pathway. We thus have a panel dataset of students since they are first observed in the Portuguese education system from 2006/07. A student's track is lost when the students moves abroad, drops from the education system altogether, or dies. The data is merged with two other datasets (ENEB and ENES) containing comprehensive information on student achievement in the standardized national exams of the 4th, 6th, 9th, 11th, and 12th grades.

[12]The administrative data is collected and maintained by the Directorate General of Education and Science Statistics (*Direcção-Geral de Estatísticas da Educação e Ciência*), a department under the administration of the Ministry of Education in Portugal.

[13]This support, named ASE (*Ação Social Escolar*) in the Portuguese school system, is aimed at providing financial assistance to students from low-income families to help cover education-related expenses. This support is intended to ensure equal access to education and reduce disparities caused by economic disadvantages. Eligibility for full or partial ASE support, described in Appendix Table A1, is determined based on the household's income level.

3 Identification Strategy

3.1 Event-Study

Exposure to the new CSL was determined by the school-grade in which a student enrolled in the academic year of 2009/2010: students enrolled in grades 1 to 7 from the academic year of 2009/10 were required to stay in school until they turned 18 years old or finished the 12^{th} grade of schooling; while students enrolled in the 8^{th} grade or above in 2009/10 remained under the old CSL, and could leave school when they turned 15 years old or completed the 9^{th} grade.

A straightforward, yet naïve, approach to analyse the effect of the policy change would simply compare educational outcomes of older cohorts (in the 7^{th} grade in 2008/09, or before) that were under the old CSL, with those of the younger cohorts under the new CSL (in the 7^{th} grade in 2009/10, or later):

$$Y_{ist} = \sum_{\substack{d=2007/08 \\ d\neq 2008/09}}^{t} \gamma_d \times \mathbb{1}\{t = d\} + X'_{it}\beta + \delta_s + \epsilon_{ist} \tag{1}$$

where Y_{ist} is an educational outcome (e.g., graduation) of student i, enrolled in school s in the 7^{th} grade of academic year t; the parameters of interest γ_d capture differences in outcomes of each cohort t relative to the last untreated cohort of 2008/09 (the reference group); X_{it} is a vector of student background characteristics, including indicators for gender, migrant status, socioeconomic support, being above the grade's reference age, resources at home, parental education, and parental employment status; and δ_s are school fixed effects. A student's cohort is defined by the last academic year in which the student enrolled in the 7^{th} grade. In terms of identification, this approach cannot account for unobservable cohort effects driving educational outcomes and as such, $\gamma_{d\geq 2009/10}$ are likely to be biased estimates of the CSL reform. Examples of such unobservable cohort differences could include student skills not captured by data on school subject scores, or parental valuation of education.

Nevertheless, it is possible to use data from cohorts of subsequent grades, which were not exposed to the new CSL until a later period, in an attempt to account for these unobservable cohort effects. Specifically, 9$^{\text{th}}$ grade cohorts did not become exposed until 2011/12 (i.e., when the first exposed 7$^{\text{th}}$ grade cohorts of 2009/10 would reach the 9$^{\text{th}}$ grade two years later). As such, I extend equation (1) in the following way:

$$Y_{isgt} = \sum_{\substack{d=2007/08 \\ d\neq 2008/09}}^{2009/10} \gamma_{dg} \times \mathbb{1}\{t = d, g = 7\} + X'_{it}\beta + \delta_{sg} + \epsilon_{isgt} \tag{2}$$

where the parameters of interest γ_{dg} provide event-study estimates of the policy change using 9$^{\text{th}}$ grade cohorts as a control group, conditional on the student background characteristics X_{it} described above, and school-by-grade fixed-effects δ_{sg}. Identification rests on the assumption that unobservable cohort characteristics of 7$^{\text{th}}$ and 9$^{\text{th}}$ graders do not differentially drive outcomes across years — i.e., in absence of the reform, outcomes of 7$^{\text{th}}$ grade cohorts from 2008/09 to 2009/10 would have followed the same trend as those of 9$^{\text{th}}$ grade cohorts.

A student's grade cohort is defined by the last academic year in which the student enrolled in that grade. The use of 9$^{\text{th}}$ instead of 8$^{\text{th}}$ grade cohorts is justified by the fact that most students in 7$^{\text{th}}$ grade cohort t also belong to 8$^{\text{th}}$ grade cohort in $t+1$ (e.g., students that were promoted in the 7$^{\text{th}}$ grade in 2008/09, and were again promoted in the 8$^{\text{th}}$ grade in 2009/10). To avoid the issue of duplicate student observations[14], I use 9$^{\text{th}}$ grade cohorts, which do not pose this problem when focusing on an event-study window between 2007/08 and 2009/10 — i.e., when estimating one lead $\gamma_{2007/08}$, and one lag $\gamma_{2009/10}$ parameter.

[14]To see this, notice that the basic 2x2 DiD estimate of the CSL treatment effect using 8$^{\text{th}}$ grade cohorts as control group would be given by $\widehat{\gamma}_{2009/10,DiD} = (\overline{Y}_{7g,2009/10} - \overline{Y}_{7g,2008/09}) - (\overline{Y}_{8g,2009/10} - \overline{Y}_{8g,2008/09})$. However, most students in $\overline{Y}_{7g,2008/09}$ would also be included in $\overline{Y}_{8g,2009/10}$, in which case there would be a perfect correlation between their outcomes.

3.2 Local Difference-in-Differences

The main analysis of this paper is based on a group of students from the same grade-cohort for whom exposure to the new CSL was unexpectedly determined by their academic achievement in that year. Students enrolled in the 7th grade in 2008/09 that were retained had to repeat this grade in the subsequent academic year and, as stipulated by the law, were thereby exposed to the new CSL. On the other hand, students in the 7th grade in 2008/09 that were promoted to the 8th grade still fell under the old CSL. A student's cohort is now defined by the first academic year in which the student enrolled in the 7th grade[15].

Retention in the 7th grade in the year preceding the reform thus provides quasi-experimental variation in treatment exposure. Because of unobservable differences between retained and promoted students, I use data from previous 7th grade cohorts for whom retention *vs* promotion did not determine exposure to different CSL ages, and utilize a Difference-in-Differences (DiD) design to isolate the effect of the reform from the confounding effect of retention on outcomes. Furthermore, to add credibility to the parallel-trends assumption that is needed for DiD estimates, I compare students at the grade retention margin only, identified by school subject scores and retention rules. Estimates have a local interpretation, as they compare students just above the retention threshold with those just below it. If the identifying assumptions discussed below hold, it is possible to identify the average effect of the reform for the group of retained students at the grade retention margin (the Average Treatment effect on the Treated, or ATT). Nevertheless, being at this margin is of particular policy-relevance, as it also indicates low-achievement — a group that CSL policies specifically target, and for whom the literature identifies potentially larger effects.

Figure 2, panel A, illustrates the timing of exposure to the new CSL of different 7th grade cohorts and the identification strategy. For the *Pre-Intervention* cohorts, students always

[15]This contrasts with the event-study's definition, described in the previous subsection, where a student's grade cohort is defined by the last academic year in which they enrolled in that grade. These different definitions guarantee exposure to the new CSL in each setting.

fell under the old CSL, irrespective of retention status[16]. However, for the *Mid-Intervention* cohort, retained students became exposed to the new CSL due to enrollment in the 7th grade in 2009/10, while promoted students still fell under the old CSL, as they will have enrolled in the 8th grade in 2009/10. Finally, in the *Post-Intervention* cohorts, all students were exposed to the new CSL, regardless of retention status. The identification of the CSL treatment effect comes from comparing the evolution of outcomes of the retained group from 07/08 to 08/09, for whom their treatment status changed in the latter academic year, with that of the promoted group, who remained under the old CSL in both 7th grade academic year cohorts.

The estimation of the CSL treatment effect is, in practice, implemented through the use of repeated cross-sections of 7th grade students at the retention margin, in a 2x2 DiD specification (i.e., the canonical DiD with 2 time periods and 2 groups):

$$Y_{ist} = \alpha Retain_i + \lambda Post\text{-}period_t + \gamma Retain_i \times Post\text{-}period_t + X'_{ist}\beta + \epsilon_{ist} \tag{3}$$

where Y_{ist} is an educational outcome of student i, enrolled in school s in the 7th grade of academic year t; $Retain_i$ indicates retention in the 7th grade; $Post\text{-}period_t$ indicates that the observation comes from the 08/09 instead of the 07/08 7th grade cohort; and X_{ist} is a vector of student background characteristics listed above, now including school-by-year averages of these same variables to control for school composition effects, and indicators for failed subjects to control for student achievement in this grade. The parameter of interest is therefore γ, the parameter on the interaction term of $Retain_i$ with $Post\text{-}period_t$, which under the identifying assumptions discussed below, identifies the effect of the new CSL on these students' educational paths. The main analysis focuses on two educational outcomes: compliance with the new CSL, i.e., staying in school until at least 18 years old or graduating upper-secondary school; and graduation from upper-secondary school. In sub-section 6.2 I

[16]To guarantee that retained 7th grade students in the 2006/07 and 2007/08 (the *Pre-Intervention* cohorts) do not end up exposed to the new CSL because of additional retentions, I focus only on students with one 7th grade retention at most in all cohorts, excluding around 1% of the sample.

analyse additional outcomes such as graduation from lower-secondary school, enrollment in upper-secondary school, upper-secondary school track choices, and alternative measures for the length of schooling.

The setting illustrated in Figure 2 indicates variation in treatment timing of the different retained and promoted groups. While the retained group change their treatment status from 07/08 to 08/09 and the promoted group remain unexposed, the promoted group change their treatment status from 08/09 to 09/10 while the retained group remain exposed. In principle, we could then estimate a second set of treatment effects, comparing the evolution of outcomes of the promoted group from 08/09 to 09/10 with that of the retained group — i.e., comparing the *late* switchers with the *early* switchers. However, as discussed by the recent literature on DiD with variation in treatment timing (also known as staggered-adoption), comparisons of *late* with *early* switchers are biased in the presence of treatment effect heterogeneity across time (Goodman-Bacon, 2021). In this setting, this would mean that the treatment effect experienced by the retained group differs across 7^{th} grade cohorts from 08/09 onward — this could happen if, for instance, upper-secondary schools became better prepared to receive later cohorts of retained students, positively affecting their outcomes. Under that scenario, comparing what happens when the promoted group change their treatment status with what happened with the retained group leads to bias. One solution proposed by Callaway and Sant'Anna (2021) is to consider a "*never-treated*" group that remains unexposed to the policy in the period of analysis and therefore, can be used to estimate the second set of treatment effects for when the promoted group becomes treated. However, because there is no such group in this setting, I focus only on the treatment effect arising from the change of the treatment status of the retained group from 07/08 to 08/09.

3.3 Validity of the identification strategy

The change in outcomes of the promoted group from the academic year of 07/08 to 08/09 constitutes a valid counterfactual to that of the retained group, as long as the parallel-trends

assumption holds. This assumption implies that the trend in outcomes of the retained group from one cohort to the next would not be different from that experienced by the promoted group, had the retained group remained unexposed to the higher CSL. This further implies that, during the period of analysis, the difference on educational outcomes between retention *vs* promotion at the margin remained constant, allowing us to isolate the causal effect of the reform from the confounding effect of retention on outcomes.

If, for instance, schools and teachers purposely changed retention practices in the academic year before the policy was implemented in the 7^{th} grade, or if these students changed their effort levels in response to an anticipation of different exposures to the policy, this would suggest changes on relevant unobservables, and a violation of the identifying assumptions. Although the reform was officially passed in August 2009, after grade retention decisions are made in June, the government's proposal — outlining the planned grade-level exposures as specified in the final law — had been publicly known since April of the same year. Hence, I cannot rule out the possibility of strategic behavior from teachers, students, and their parents, in anticipation of the proposed exposures to the new CSL. Specifically, certain teachers might have attempted to retain students at the retention margin to ensure exposure to the new CSL, while others might have sought to promote students to avoid such exposure. These diverse actions could be justified, for example, by teachers' well-intentioned beliefs that students would benefit differently from each type of exposure. On the other hand, students could have increased their effort to avoid retention, or parents could attempt to exert pressure on teachers to prevent retention and, consequently, exposure to the new CSL.

Despite these concerns, I do not find evidence in their support. The comparison of retained and promoted students across years, in subsection 4.2, provides good indication that the composition of these groups did not change significantly in the years surrounding the reform. Note that this also lends support to the identifying assumptions of the event-study design, as described in subsection 3.1. One potential scenario in which 7^{th} grade

cohorts (defined by the *last* academic year of enrollment) could have changed from 08/09 to 09/10 would be if the group of students retained in their *first* 7^{th} grade in 08/09 differed in terms of unobservables, due to this potential strategic behavior.

To test the plausibility of the parallel-trends assumption, I consider two other sets of effects illustrated in panel B of Figure 2, as well as a placebo test with 8^{th} grade cohorts. The first inspects if pre-treatment trends are parallel — that is, if from 06/07 to 07/08 the outcomes of the retained and promoted groups followed the same trend, when both groups remained untreated. While verifying this does not guarantee that the parallel-trends assumption is satisfied in the periods for which the treatment effect is estimated (as this is related with a potential and unobservable outcome for the retained group in 08/09), it makes it more plausible. However, because data on school subject scores are not available in the first year of the dataset, 2006/07, it is not possible to define the analysis sample at the retention margin for this year. When analyzing the *full* group of retained and promoted students, I find that these groups were on different trends regarding their compliance outcomes (Appendix Figure A1). This difference could be attributed, in part, to the considerable heterogeneity within the two groups. While compliance rates remain relatively stable for the promoted group, the retained group exhibits a notable upward trend throughout the analysis period. However, when narrowing the focus to a more homogeneous group of low-achieving students, identified as those above the reference age, I find that trends were indeed parallel, as shown in Appendix Figure A2.

The second validity test lies in inspecting the evolution of outcomes for the retained and promoted groups at the retention margin after 2009/10, i.e., when both groups were exposed to the new CSL. This falsification test is valid under the assumption that treatment effect dynamics of both groups are homogeneous from 2009/10 onward. Thus, while not verifying this homogeneity does not constitute a potential violation of the parallel-trends assumption, it provides an important description of treatment effect dynamics in any case.

Finally, in a placebo test in sub-section 5.4, I give further credibility to the identification

strategy by showing that outcomes for 8^{th} grade-cohorts evolved parallelly between retained and promoted students at the retention margin throughout all years, as expected by the absence of any policy differentially treating the two groups.

4 Sample

4.1 Sample selection

Data on school subject scores are used to define the grade retention margin, central to the main identification strategy. These data are available for public schools only[17], and from the academic year of 2007/08 on. For students in public schools, we have subject scores data for around 86% of 7^{th} grade students.

Furthermore, I restrict my sample to students who were at most one year above the reference age in their first 7^{th} grade. The reference age at the end of the 7^{th} grade, assuming a regular school starting age and no accumulated retentions, is 13 years old for students with birthdays between January 1^{st} and August 31^{st}. For students born after August 31^{st}, the reference age is 12 years old, as they turn 13 by the end of the calendar year. Under the old CSL, students could drop out of school at the end of the academic year they turned 15 years old. As such, to ensure that retention in the 7^{th} grade results in exposure to the reform for the 2008/09 cohort, the analysis sample includes students that were at most 14 years old in their first enrollment in the 7^{th} grade.

Finally, to ensure comparability between more recent and older 7^{th} grade cohorts, the educational outcomes analysed are measured using a consistent time horizon of 7 years later. Since the latest available data is from 2017/18, the last cohort analysed is the 2010/11 7^{th} grade cohort.

[17] As such, I restrict the main analysis to students who attended a public school in the 7^{th} grade. Students may have enrolled in a private school before or after their first enrollment in 7^{th} grade. The only condition of inclusion is therefore, that the enrollment in 7^{th} grade was in a public school.

4.2 Retained and promoted students

During the period of analysis, students in the 7th grade across all Portuguese schools took 10 subjects: Portuguese Language, Mathematics, Natural Sciences, Physics & Chemistry, History, Geography, English Language, a second Foreign Language (usually French), Visual Arts, and Physical Education. Grade retention in the 7th grade is decided by the student's teachers and the class committee but, as determined by national law, it is only considered when a student has at least 3 failing subject scores out of the 10 subjects taken in this grade. However, in practice, grade retention is much more commonly applied when a student fails at least 4 subjects: only 5.6% of students with 3 failed subjects were retained, compared with 69.2% of those with 4 failed subjects, and 93.5% of those with 5 failed subjects. Figure 3 shows this distribution separately for each academic year.

I define being at the grade retention margin as failing between 3 and 5 subjects. The first four columns of Table 1 presents summary statistics comparing the retained and promoted groups at the retention margin, across the academic years of 2007/08 and 2008/09. Differences between groups within a year are statistically significant for approximately half of the predetermined variables analysed (columns 5 and 6). Differences in subject score variables in the last three rows are all statistically significant. Such differences are expected, as subject scores directly determine retention decisions.

What is crucial to the DiD identification strategy is ensuring comparability between the groups of retained and promoted students across different years. Columns 7 to 9 of Table 1 provide evidence that, based on observed predetermined variables and subject score variables, there were no significant changes in the composition of these groups, and the likelihood of changes stemming from strategic behaviour appears low. Columns 7 and 8 compare retained and promoted groups across different academic years, respectively. While some statistically significant differences persist, when examining the change in the composition of retained versus promoted students from 2007/08 to 2008/09 in column 9, these differences essentially disappear. This is equivalent to subtracting the differences in columns 7 and 8, or reporting

the interaction term, γ, in equation 3 without controls. A consistent pattern of findings persists when comparing retained and promoted students in subsequent years following the policy change, as shown in Appendix Table A2.

Moreover, Appendix Figure A3 shows that retention rates in the 7^{th} grade, both for the overall population and for the retention margin, remained stable around 15% and 40%, respectively. This provides additional evidence that, on average, there was no apparent anticipatory behavior that would be reflected in the composition of retained students across years.

5 Results

5.1 Who are main *compliers* of the new CSL?

The literature on compulsory schooling laws suggests that the effects from these policies tend to be concentrated on specific groups of the student population, and are far from being homogeneous. To understand who the likely *compliers* of the policy were — i.e., those changing educational choices because they were required to stay in school for longer, and would have dropped out earlier in the absence of the reform — I look at dimensions which the literature identifies as being key predictors of school dropout: namely socioeconomic status, and academic achievement (Cratty, 2012).

Figure 4 plots event-study estimates from equation 2 for both the compliance and graduation outcomes, across different socioeconomic status (SES) and achievement levels, comparing these with estimates for the overall student population. I classify students as low-SES if their parents or legal guardians have at most lower-secondary education or if they are beneficiaries of socioeconomic support. Students are classified as low-achieving if they failed at least two subjects in their first enrollment in that grade or if they are above the reference age for that grade. High-SES and high-achieving status are determined by negating these

conditions[18].

Estimates indicate that disadvantaged students were more likely to comply with the new CSL following the reform. This probability increased by approximately 3 and 2 p.p. for low-achieving and low-SES students, respectively. In contrast, the estimated increase for the overall population is of 1 p.p. Conversely, compliance probabilities remained unchanged for high-achieving and high-SES students. Before the reform, all 7^{th} grade groups exhibited parallel trends when compared with their 9^{th} grade counterparts, thereby providing support to the validity of the identification strategy. On the other hand, estimates indicate that, on average, graduation probabilities were not affected for any of these groups following the reform.

5.2 Main Difference-in-Differences Results

This subsection presents the main results of the local DiD identification strategy, as described in subsection 3.2, for the compliance and graduation outcomes. The results shown in Figure 5 are presented in two ways: firstly, by illustrating the trend in mean outcomes for both the retained and promoted groups at the retention margin across 7^{th} grade cohorts (panels A and B), and secondly, by plotting estimates from equation 3, which consider the covariates outlined in Appendix Table A1, in panels C and D. Table 2 complements the analysis by presenting point estimates, both when covariates are not considered and when they are considered.

I estimate that the probability of complying with the new CSL increased by 2.4 p.p. when no covariates are considered (column 1 of Table 2), and by 2.8 p.p. when covariates are taken into account (column 2 of Table 2, and panel C of Figure 5), an effect that is statistically significant at the 5% level.

On the other hand, I find that the probability to graduate upper-secondary school was, on average, not affected by the reform. Columns 3 and 4 of Table 2 show that the estimated

[18] Appendix Figure A4 plots event-study estimates separately for each SES and achievement sub-group.

treatment effect is very close to zero, regardless of the inclusion of covariates.

Furthermore, Figure 5 shows that compliance and graduation outcomes evolved parallelly for the retained and promoted groups from the 09/10 to the 10/11 cohorts. DiD estimates for these cohorts, where both retained and promoted groups are treated, are not statistically significant. This suggests homogeneity of treatment effect dynamics from 09/10 to 10/11, and that post-trend estimates serve as a valid falsification test.

5.3 Heterogeneity analysis

The results of the previous subsection suggest that the CSL reform induced the first treated cohort of students at the retention margin to stay longer in school, but did not significantly alter their probability to graduate from upper-secondary school. However, significant heterogeneity exists across student groups even within the retention margin. I explore treatment effect heterogeneity across socioeconomic status, gender (as presented in Appendix Table A3), and prior academic achievement, finding meaningful differences in the latter dimension. To analyse this dimension, I focus on students' age relative to the reference age in the 7^{th} grade. Being older than the reference age serves as a strong proxy for having already experienced grade retention, indicating lower prior achievement[19].

Table 3 reveals that the positive compliance treatment effect is driven by students above the reference age. I estimate that this group increased their compliance probabilities by approximately 5 p.p., while students at the reference age experienced no change. Moreover, I estimate that the reform increased graduation probabilities by around 3 p.p. for students above the reference age; however, this effect is imprecisely estimated due to the smaller sample sizes, and not statistically significant. These results suggest that, even within the retention margin — a group of students already characterized by low achievement — those with even lower levels of prior achievement may benefit more from CSL policies.

[19]Students may also be above the reference age because of a late school start. Late school starting is not observable for 7^{th} grade cohorts in the analysis period. Nevertheless, removing students eligible for late school starting (i.e., those born between the 16^{th} of September and the 31^{st} of December of a given calendar year) from the above-reference-age group does not change the interpretation of the results.

As positive treatment effects are driven by students above the reference age, I further investigate whether certain sub-groups of these lower-achieving students could have benefited in terms of graduation probabilities. Once again, I examine treatment effect heterogeneity based on socioeconomic status, gender, and additionally, whether students ever enrolled in upper-secondary school. This last condition may be of particular relevance for different reasons. First, students that enrolled in upper-secondary at some point (73.4% of this sample, as seen in column 3 and 4 of Table 4) might have perceived the cost of the additional years of education needed to graduate as lower, given that they were legally required to stay in school until 18 years old. Moreover, upper-secondary schools may have also been better prepared to receive and engage students from 2012/13 onwards, the first academic year when students under the new CSL could have started this stage of education. As described in Section 2, in August of 2012, a law was approved determining measures to prevent early school leaving and failure, such as better guidance and support to at-risk students, giving further emphasis to the enrollment in vocational and alternative tracks as a preventive measure.

Table 4 shows larger graduation treatment effects for lower-achieving students (i.e., above the 7^{th} grade reference age) that enrolled in upper-secondary school (5.8 p.p., p-value: 0.062), and lower-achieving boys (5.7 p.p., p-value: 0.058). When combining these two conditions in column 10, I estimate that lower-achieving boys that enrolled in upper-secondary school increased their graduation probabilities by 8.8 p.p (p-value: 0.024). These results lend support to the hypothesis that upper-secondary schools policies targeting at-risk students may have significant impacts in reducing early school leaving.

The fact that the estimated treatment effects are only statistically significant for boys may be related to their lower baseline graduation probabilities compared to girls (a 7.2 p.p difference, as seen from columns 5 and 7), or it could indicate that policies targeting at-risk students were more successful in engaging boys. Differences across socioeconomic status are not meaningful, as the high-SES group constitutes only 10% of the sample above reference age.

5.4 Placebo test

The identification strategy assumes that, in the absence of a policy differentially treating retained and promoted students in the 7^{th} grade in 2008/09, the outcomes for these two groups would have evolved in parallel. To give credibility to this assumption, a useful test is to compare the evolution of outcomes for 8^{th} grade students at the retention margin, where retention rules are the same as those applied in the 7^{th} grade. As defined by the reform, all students enrolled in the 8^{th} grade or above in 2009/10 would remain under the old CSL, irrespective of retention status.

In a similar fashion to the main results presented in Figure 5, Figure 6 shows compliance and graduation outcomes for 8^{th} grade cohorts at the retention margin. We verify that both outcomes evolved parallelly between retained and promoted students, as expected by the absence of any policy differentially treating the two groups in the 8^{th} grade. The placebo treatment effect is not statistically different from zero for either outcome, and furthermore, all post-trends placebo treatment effects are not statistically significant.

6 Additional results

6.1 Contrasting Event-Study and local Difference-in-Differences estimates

The findings from the local DiD methodology indicate that certain sub-groups of lower-achieving students saw improved graduation probabilities following the reform (sub-section 5.3). Conversely, the event-study approach indicates that while students from more disadvantaged backgrounds showed increased compliance probabilities, there were no significant effects on their graduation probabilities (sub-section 3.1). An exception is the sub-group of students above reference age, for whom a probability of 2 p.p is estimated, as seen in Appendix Figure A4.

To reconcile these divergent findings, I analyse event-study estimates using a comparable sample to that of the local DiD analysis. Specifically, I focus on students at the 7^{th} grade retention margin, contrasting their outcomes with those at the 9^{th} grade retention margin[20]. This analysis is performed on the same groups explored in the local DiD methodology — i.e., those above the grade's reference age, and sub-groups of students above the the grade's reference age (boys, students that enrolled in upper-secondary school, and boys that enrolled in upper-secondary school). Appendix Figure A5 shows that these estimates are largely in line with those found with the local DiD analysis.

6.2 Additional outcomes

In addition to examining the reform's impact on compliance and graduation probabilities, I investigate its effects on other outcomes to gain a more comprehensive understanding of how students' educational paths were affected. Estimates are conducted using both the event-study approach, focusing on key groups (i.e., different SES and achievement levels, compared to the overall student population, as seen in Appendix Figure A6); and the local DiD approach for students at the grade retention margin, and students at the grade retention margin above the reference age, as seen in Appendix Table A4. The first main finding is that, similar to its effects on compliance and graduation probabilities, the reform predominantly affected other educational outcomes for more disadvantaged and lower-achieving students, while other groups exhibited only small or statistically insignificant changes. These additional outcomes are described in Appendix Table A1.

Findings from both approaches show that the length of schooling increased by around 1.4 months for lower-achieving groups, consistent with the estimates for the compliance outcome. Moreover, despite some discrepancies between approaches, there is some evidence that the reform affected levels of educational attainment lower than those predicted by the reform. Local DiD estimates indicate that lower-achieving students increased their probability to

[20]Students are classified as being at the grade retention margin if they failed between 3 and 5 subjects in their *first* enrollment in that grade.

graduate from lower-secondary schooling by 6.2 p.p.[21]; while event-study estimates show that the probability to enroll in upper-secondary schooling increased by around 2 p.p for the low-achieving group. Nevertheless, I do not find a clear effect on upper-secondary school track choices[22].

Additionally, I return to the main compliance outcome to more carefully understand which margin of educational attainment was affected by the reform. Appendix Figure A7 presents local DiD estimates for the probability of staying in school at most until age 15 (the old CSL), and at least until age 16, 17, 18 or graduation (i.e., compliance with the new CSL), and 19 or graduation. Estimates are provided both for students at the grade retention margin, and those at the grade retention margin above the reference age. I find positive effects only for the two highest levels of attainment, suggesting that the reform primarily encouraged these students to remain enrolled in school for one or two additional years beyond age 17.

Finally, I explore why the reform might have had a limited effect on upper-secondary school graduation probabilities. A plausible explanation is that a significant portion of lower-achieving students, the main compliers of the reform, experienced additional grade retentions before reaching the 12^{th} grade and dropped out before completing it. Local DiD estimates show that the probability of obtaining at least 5 additional years of education — sufficient for graduation — increased by 3.6 p.p. for lower-achieving students, although this effect is not statistically significant. Additionally, lower-achieving students who enrolled in upper-secondary education experienced an increase in the duration of their upper-secondary schooling by 1.7 months.

[21]The event-study estimate for the low-achieving group is approximately 2 p.p. However, pre-reform estimates for this group are notably higher, around 4 p.p. This discrepancy might be attributed to the fact that graduation from lower-secondary is measured only for the main track (which enrolls over 90% of students), and related shifts in enrollment shares between the two tracks based on achievement levels.

[22]Although the event-study treatment effect estimates are not statistically significant for any group, the pre-trend estimates are positive and statistically significant for the probability of enrolling in the academic track. This discrepancy may be explained by changes in the supply and demand for different tracks across enrollment years.

6.3 School composition & resources

Lastly, I analyse how 10th grade upper-secondary school classes changed in student and teacher composition with the arrival of the new CSL cohorts, focusing on differences between academic and vocational track classes (Appendix Table A5). This analysis also examines how school resources changed within the overall school system, and specifically in upper-secondary schools during this period (Appendix Table A6).

The main findings indicate that average class sizes increased by approximately 2 students, rising from 22.65 before the 2012/13 academic year to 24.73 afterwards. Notably, vocational track classes experienced a more significant increase, with an average addition of 3 students per class. Based on recent findings from the literature, it is unlikely that class size increases of this magnitude significantly worsened student outcomes[23]. Moreover, there is no clear evidence that the share of disadvantaged students changed. At the same time, there were notable shifts in teacher demographics[24]: since 2012/13, there has been a 2 p.p increase in male teachers, who are now slightly older and more experienced. Finally, we observe a 10% increase in spending per student in upper-secondary schools and a 15% increase across the overall school system in the years immediately following the arrival of the CSL cohorts in upper-secondary schools (2013 and 2014).

7 Conclusion

This paper studies the effect of a large and recent CSL increase, from 15 to 18 years old, on students' educational paths — namely, on the probabilities of compliance with the new CSL, and graduation from upper-secondary school. The Portuguese 2009 reform provides a unique opportunity to study such effects, given the country's initially high rates of early school

[23] Angrist et al. (2019) and Leuven and Løkken (2020) both find null class size effects for Israeli primary schools, and Norwegian primary to lower-secondary schools, respectively. However, a limitation of this literature is the lack of causal studies on class size effects in upper-secondary schools.

[24] I analyse characteristics of Portuguese class teachers, a key subject that is common across all upper-secondary school tracks.

leaving. Furthermore, the availability of student-level population data, with comprehensive detail on both socioeconomic background and academic achievement, allows for a clearer understanding of the specific *marginal* students affected by this policy.

Identification is based on the fact that grade retention in the 7^{th} grade in the year preceding the reform unexpectedly determined exposure to the reform. As grade retention only lead to different treatment exposure in 2008/09, I leverage data from students at the retention margin from previous grade-cohorts to isolate the causal effect of the reform from the effect of retention on outcomes, in a local Difference-in-Differences design. This identification strategy is complemented with event-study estimates that compare the outcomes of 7^{th} grade cohorts with those of untreated 9^{th} grade cohorts around the years of the policy implementation. I find that the Portuguese reform increased the length of schooling for the more vulnerable segments of the student population, with no discernible effects for other groups. Moreover, I provide evidence that CSL increases can increase graduation rates for some at-risk sub-groups of the student population: namely lower-achieving boys, and lower-achieving students enrolling in upper-secondary school.

The findings presented in this paper reveal that the group of *compliers* mainly comprised low-achieving students. Existing literature, utilizing CSL changes as instruments for educational attainment, has extensively demonstrated positive impacts on wages and labour market outcomes, as well as various non-pecuniary benefits. These include reduced crime rates, increased civic participation, reduced teenage pregnancy, and improved health. Therefore, central questions will be to assess whether these pecuniary and non-pecuniary benefits are also observed among the set of *compliers* in this specific context, and whether there are spillover effects to other groups — questions which are not yet possible to address due to data limitations.

In the absence of market failures, CSL polices constrain behavior concerning schooling decisions, that would otherwise be both individually and socially optimal. However, the presence of positive externalities, stemming from the many pecuniary and non-pecuniary

benefits of education mentioned above; imperfect information about the returns to schooling, such as the tendency for students from disadvantaged backgrounds to overestimate the costs and underestimate the benefits of education (Oreopoulos, 2007); or agency problems, where parents may not select the optimal level of schooling for their children, provide a rationale for CSL policies. Still, it is highly probable that these policies adversely impact the welfare of some families, such as through foregone income during schooling — an aspect which also merits further investigation to understand its full extent.

At the societal level, it is essential to weigh the benefits of CSL against their associated costs. These include the direct costs of implementation, encompassing the need for additional teaching capacity in terms of staff and physical infrastructure. On the other hand, CSL policies may also have unintended adverse consequences, including a decrease in the effort exerted by teachers in the classroom (Green and Navarro Paniagua, 2012), or an increase in criminal behavior among students within schools (Anderson et al., 2013).

Despite the potential for school quality to deteriorate, I find that spending per student and teacher experience increased in the years immediately following the arrival of the CSL cohorts in upper-secondary schools. As shown by Clark (2023), the relation between school quality and CSL increases is crucial in determining labour market returns. While a limitation of this study is that I can only estimate the effects of the CSL reform for the first affected cohort, it is possible that upper-secondary schools became better prepared to receive subsequent cohorts, further positively affecting their graduation probabilities and, ultimately, their returns to additional schooling.

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Tables

Table 1: Descriptive statistics and balance

	Groups				Differences within each year		Differences across years		DiD
	Retained 2007/08 (1)	Promoted 2007/08 (2)	Retained 2008/09 (3)	Promoted 2008/09 (4)	2007/08 (1)-(2) (5)	2008/09 (3)-(4) (6)	Retained (3)-(1) (7)	Promoted (4)-(2) (8)	Diff. (7)-(8) (9)
Pre-determined variables:									
Male	0.58	0.56	0.60	0.55	0.02	0.05***	0.03*	-0.01	0.03*
Parent Ed: Below Upper-Sec.	0.70	0.72	0.69	0.72	-0.02*	-0.04***	-0.02	0.00	-0.02
Parent Ed: Upper-Sec.	0.17	0.13	0.17	0.13	0.04***	0.04***	0.00	0.00	0.00
Parent Ed: Tertiary	0.03	0.04	0.04	0.03	0.00	0.00	0.00	0.00	0.00
1st Gen Immigrant	0.05	0.04	0.05	0.04	0.01*	0.01*	0.00	0.00	0.00
2nd Gen Immigrant	0.04	0.03	0.04	0.03	0.01**	0.01**	0.00	0.00	0.00
Socioeconomic support: full	0.31	0.34	0.29	0.32	-0.03**	-0.03**	-0.02*	-0.02**	0.00
Socioeconomic support: partial	0.10	0.12	0.12	0.13	-0.02***	0.00	0.03***	0.01	0.02
Father unemployed	0.05	0.07	0.05	0.06	-0.02***	0.00	0.00	-0.02***	0.02*
Mother unemployed	0.11	0.11	0.11	0.11	-0.01	0.01	0.00	-0.01	0.01
Computer at home	0.60	0.59	0.52	0.49	0.02	0.02*	-0.09***	-0.09***	0.01
Internet at home	0.38	0.32	0.33	0.27	0.06***	0.05***	-0.05***	-0.04***	-0.01
Above reference age	0.41	0.42	0.37	0.39	-0.01	-0.01	-0.04***	-0.03***	0.00
7th grade scores (1-5):									
Mathematics score	2.18	2.37	2.17	2.34	-0.19***	-0.17***	-0.01	-0.03**	0.02
	(0.41)	(0.51)	(0.40)	(0.50)					
Portuguese score	2.36	2.63	2.34	2.64	-0.28***	-0.30***	-0.02*	0.00	-0.03
	(0.49)	(0.49)	(0.48)	(0.49)					
GPA	2.62	2.78	2.62	2.78	-0.16***	-0.16***	0.00	0.00	0.00
	(0.11)	(0.11)	(0.11)	(0.10)					
N	2 561	3 947	2 684	4 253	6 508	6 937	5 245	8 200	13 445

Columns 1 and 2 present summary statistics for retained and promoted groups at the 7th grade retention margin in the 2007/08 academic year. Columns 3 and 4 display the same statistics for these groups in the 2008/09 academic year. Standard deviations are presented in parentheses for subject score variables. Columns 5 and 6 present differences between retained and promoted groups within each academic year. Columns 7 and 8 present differences between retained and promoted groups within each academic year, respectively. Column 9 presents the change in the composition of retained versus promoted students from 2007/08 to 2008/09 by reporting the interaction term, γ, in equation 3 without controls, equivalent to subtracting the differences in columns 7 and 8. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Table 2: Main local Difference-in-Differences estimates

	Compliance		Graduation	
	(1)	(2)	(3)	(4)
Post-period x Retained	0.024*	0.028**	-0.003	0.002
	(0.014)	(0.014)	(0.017)	(0.017)
Post-period	0.019**	0.006	0.016	-0.028
	(0.009)	(0.015)	(0.012)	(0.018)
Retained	-0.005	0.010	-0.073***	-0.011
	(0.012)	(0.015)	(0.013)	(0.018)
Constant	0.782***	0.583***	0.352***	0.009
	(0.008)	(0.186)	(0.009)	(0.201)
Controls	No	Yes	No	Yes
N	13,445	13,445	13,445	13,445
R-squared	0.001	0.059	0.006	0.054

Each column shows estimates of the local Difference-in-Differences specification of equation 3. The sample of analysis considers 7^{th} grade cohorts at the grade retention margin. Columns 1 and 2 show results for the compliance with the new compulsory schooling law outcome, defined as staying in school until 18 years old or graduating upper-secondary school. Columns 3 and 4 display results for the upper-secondary school graduation outcome. Columns 2 and 4 take into the covariates described in Appendix Table A1. Standard Errors are clustered at the school level. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Table 3: Treatment effect heterogeneity based on reference age

	Compliance		Graduation	
	(1)	(2)	(3)	(4)
A. Above reference age				
Post-period x Retained	0.054**	0.051*	0.030	0.032
	(0.027)	(0.026)	(0.024)	(0.024)
Post-period	0.023	0.023	0.004	-0.029
	(0.015)	(0.024)	(0.015)	(0.022)
Retained	-0.033*	-0.020	-0.069***	-0.019
	(0.019)	(0.023)	(0.017)	(0.023)
Constant	0.726***	0.283	0.284***	0.066
	(0.013)	(0.316)	(0.012)	(0.268)
Controls	No	Yes	No	Yes
N	5,369	5,369	5,369	5,369
R-squared	0.003	0.046	0.004	0.036
B. At reference age				
Post-period x Retained	0.002	0.010	-0.024	-0.018
	(0.017)	(0.017)	(0.022)	(0.022)
Post-period	0.011	-0.004	0.017	-0.027
	(0.011)	(0.015)	(0.014)	(0.020)
Retained	0.013	0.033**	-0.077***	-0.001
	(0.012)	(0.017)	(0.016)	(0.022)
Constant	0.823***	0.721***	0.402***	-0.078
	(0.008)	(0.188)	(0.010)	(0.246)
Controls	No	Yes	No	Yes
N	8,076	8,076	8,076	8,076
R-squared	0.001	0.047	0.008	0.039

Each column shows estimates of the local Difference-in-Differences specification of equation 3 but considering only students above the 7^{th} grade reference age in Panel A, and students at the 7^{th} grade reference age in Panel B. The reference age at the end of the 7^{th} grade — i.e., under a regular school starting age and with no accumulated retentions — is 13 years old, if a student's birthday is between the 1^{st} of January and the 31^{st} of August, or 12 years old if the birthday if after that date. Columns 1 and 2 show results for the compliance with the new compulsory schooling law outcome, defined as staying in school until 18 years old or graduating upper-secondary school. Columns 3 and 4 show results for the upper-secondary school graduation. Even numbered columns take into the covariates described in Appendix Table A1. Standard Errors are clustered at the school level. Significance levels: ***p < 0.01, **p < 0.05, *p < 0.1.

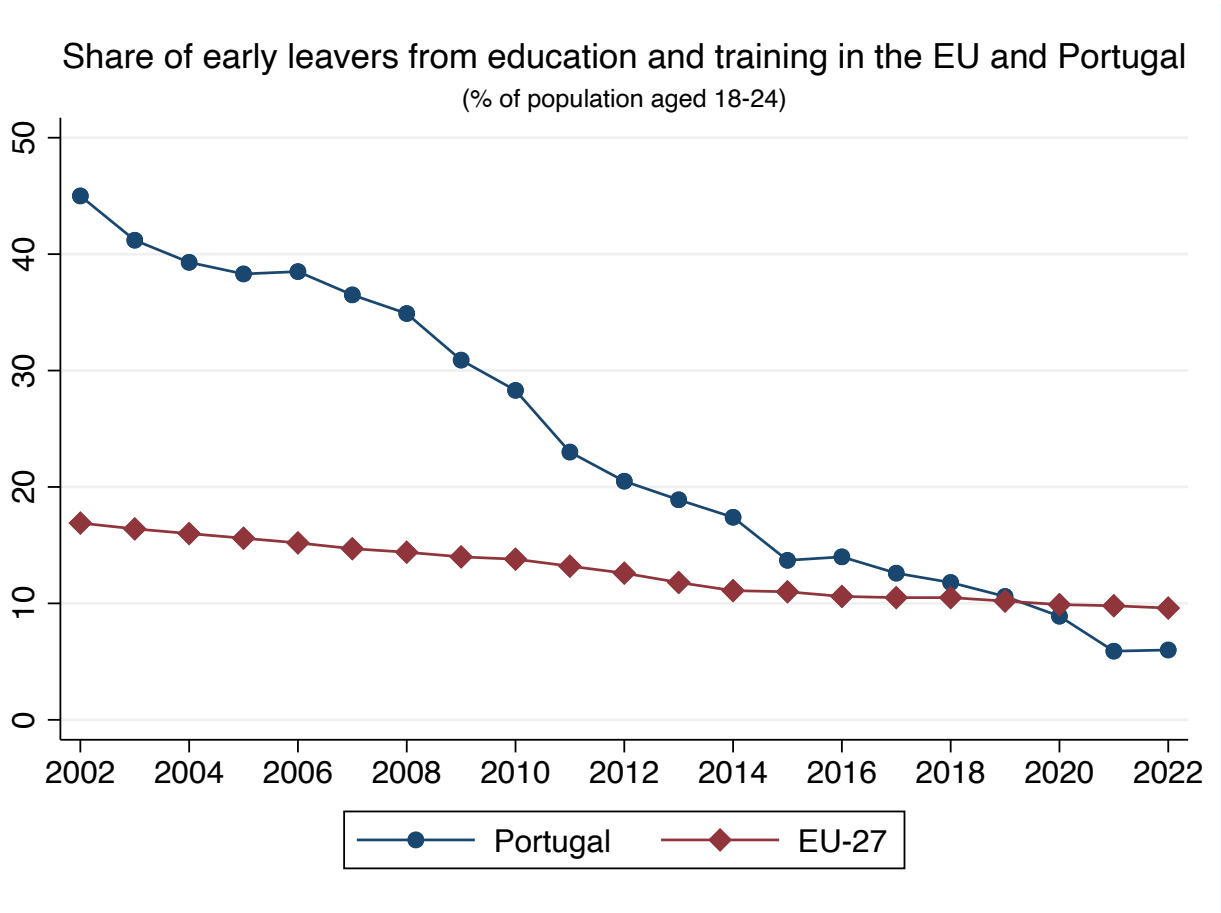
Table 4: Graduation treatment effect heterogeneity, above reference age sub-groups

	All Above reference age		Enroll Upper-secondary school		Boys		Girls		Enroll Upp-Sec. School Boys		Enroll Upp-Sec. School Girls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-period x Retained	0.030	0.032	0.047	0.058*	0.053*	0.057*	0	-0.002	0.073*	0.088**	0.014	0.021
	(0.024)	(0.024)	(0.031)	(0.031)	(0.030)	(0.030)	(0.041)	(0.040)	(0.039)	(0.039)	(0.052)	(0.051)
Post-period	0.004	-0.029	-0.009	-0.042	-0.009	-0.044	0.021	-0.004	-0.024	-0.063*	0.011	-0.011
	(0.015)	(0.022)	(0.019)	(0.027)	(0.019)	(0.027)	(0.026)	(0.037)	(0.023)	(0.033)	(0.032)	(0.046)
Retained	-0.069***	-0.019	-0.070***	-0.018	-0.065***	-0.036	-0.073**	0.005	-0.070**	-0.037	-0.070*	0.002
	(0.017)	(0.023)	(0.022)	(0.029)	(0.021)	(0.028)	(0.028)	(0.038)	(0.028)	(0.036)	(0.036)	(0.049)
Constant	0.284***	0.066	0.383***	0.385	0.254***	0.120	0.326***	-0.232	0.346***	0.212	0.434***	0.375
	(0.012)	(0.268)	(0.015)	(0.342)	(0.015)	(0.336)	(0.019)	(0.466)	(0.018)	(0.433)	(0.023)	(0.575)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	5,369	5,369	3,939	3,939	3,191	3,191	2,178	2,178	2,323	2,323	1,616	1,616
% of Above Ref. Age Sample	100%	100%	73.4%	73.4%	59.4%	59.4%	40.6%	40.6%	43.3%	43.3%	30.1%	30.1%
R-squared	0.004	0.044	0.003	0.049	0.003	0.036	0.006	0.057	0.003	0.045	0.004	0.058

Each column shows estimates of the local Difference-in-Differences specification of equation 3 for the graduation outcome, but considering only students above the 7^{th} grade reference age — i.e., that were 14 years old at the end of the year. Columns 1 and 2 show estimates for all students above the reference age. Columns 3 to 12 show estimates for different sub-groups of these lower-achieving students: those that ever enrolled in upper-secondary school (columns 3 and 4), boys (columns 5 and 6), and girls (columns 7 and 8). Columns 9 and 10 show estimates for lower-achieving boys that enrolled in upper-secondary school, while columns 11 and 12 shows these for girls. Even numbered columns take into the covariates described in Appendix Table A1. Standard Errors are clustered at the school level. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

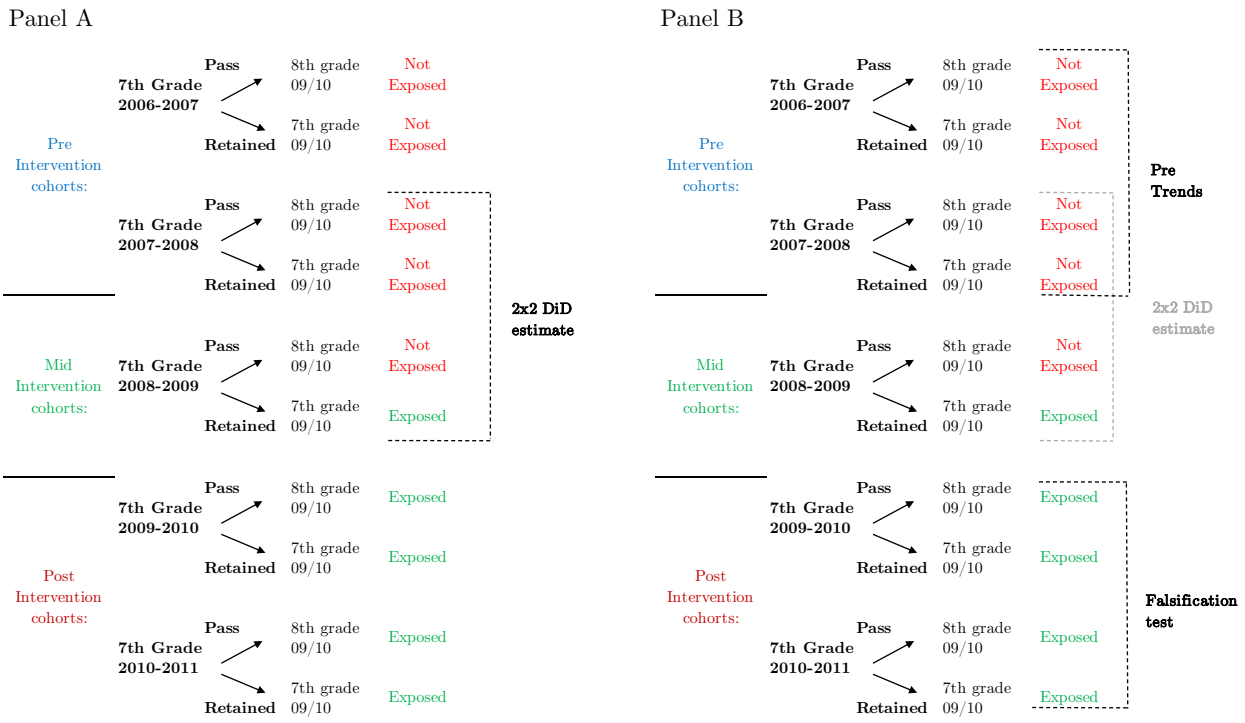
Figures

Figure 1



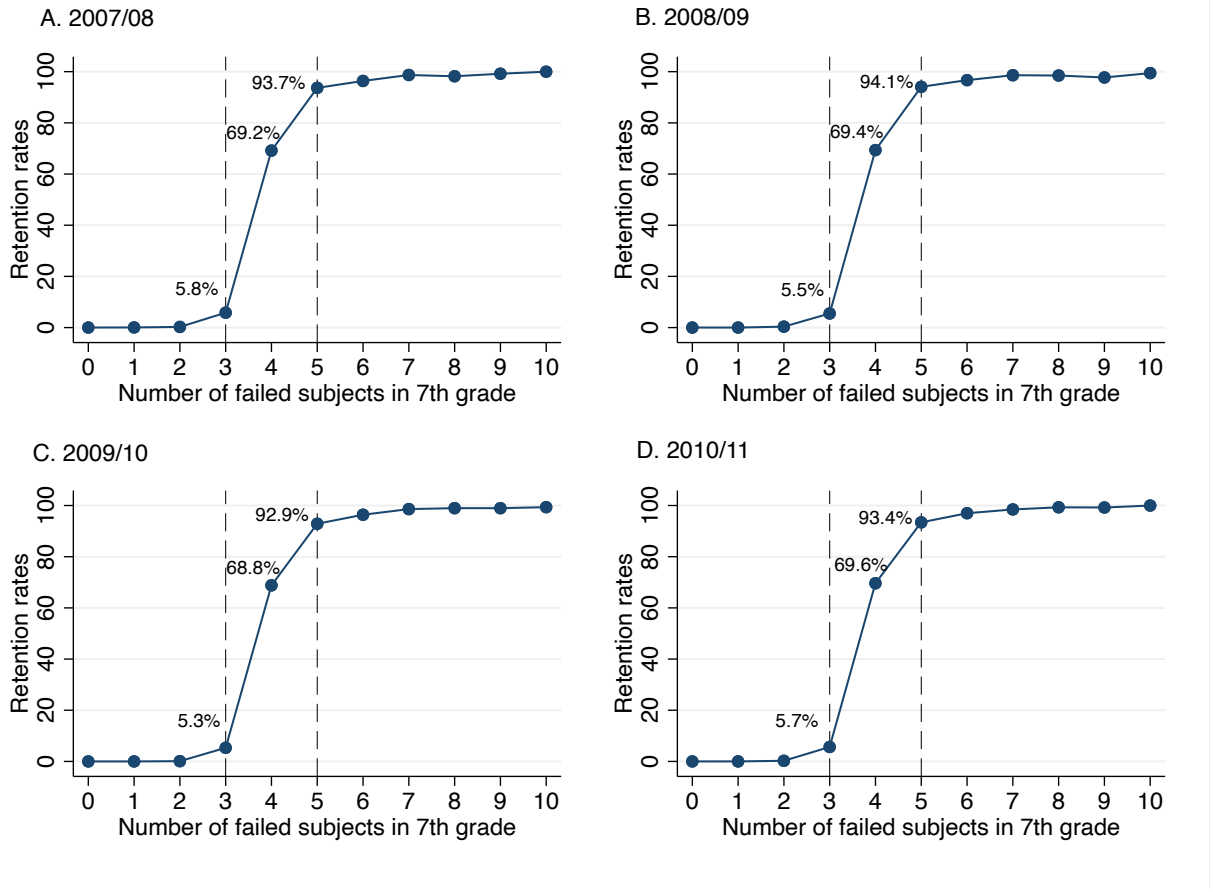
Share of early leavers from education and training in the EU-27 and Portugal. According to the Eurostat definition, an "early leaver from education and training, previously named early school leaver, refers to a person aged 18 to 24 who has completed at most lower secondary education and is not involved in further education or training; the indicator '*early leavers from education and training*' is expressed as a percentage of the people aged 18 to 24 with such criteria out of the total population aged 18 to 24.". Data source: Eurostat.

Figure 2: Identification Strategy



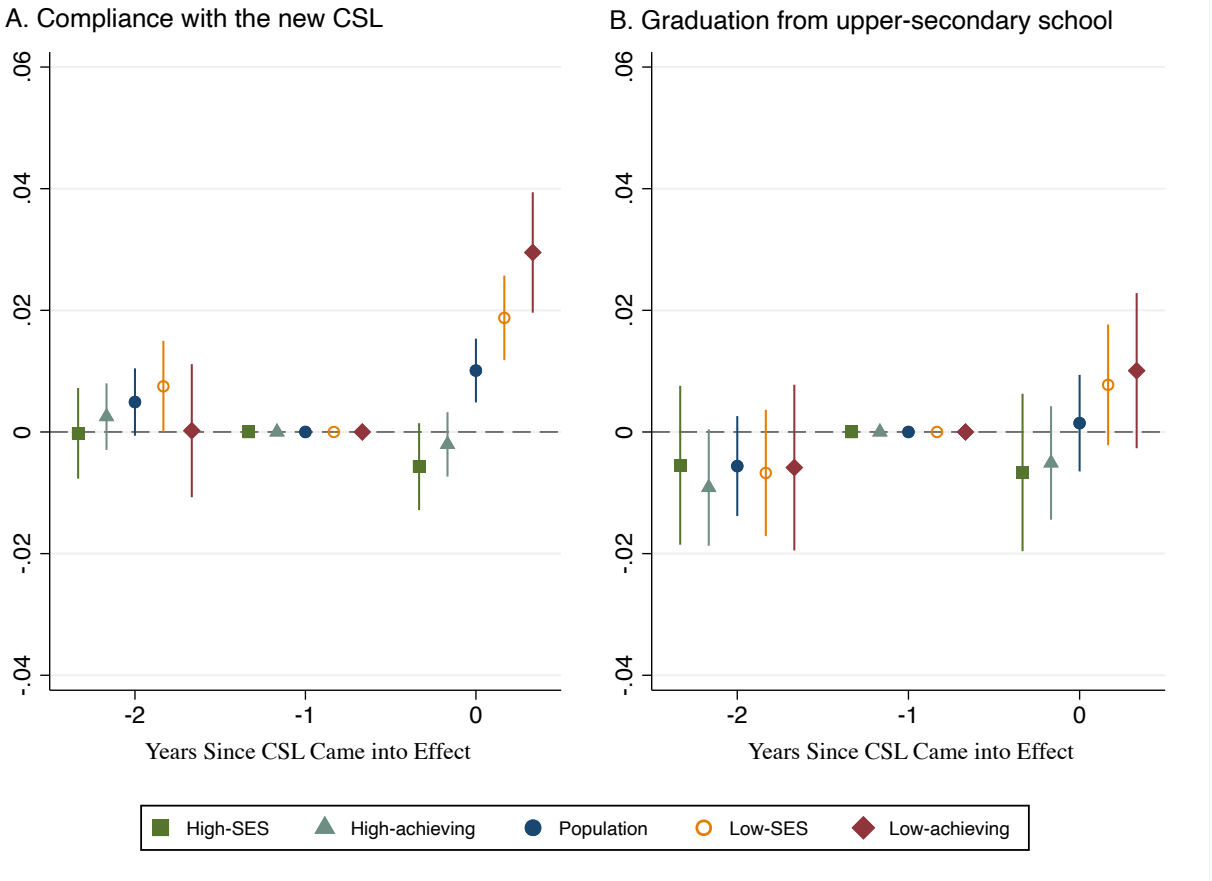
Timing of exposure to the new compulsory schooling law and the difference-in-differences identification strategy. Panel A illustrates where the identification of the reform effect comes from, while Panel B shows two possible tests of the validity of the identification strategy.

Figure 3: Retention rates and number of failed subjects



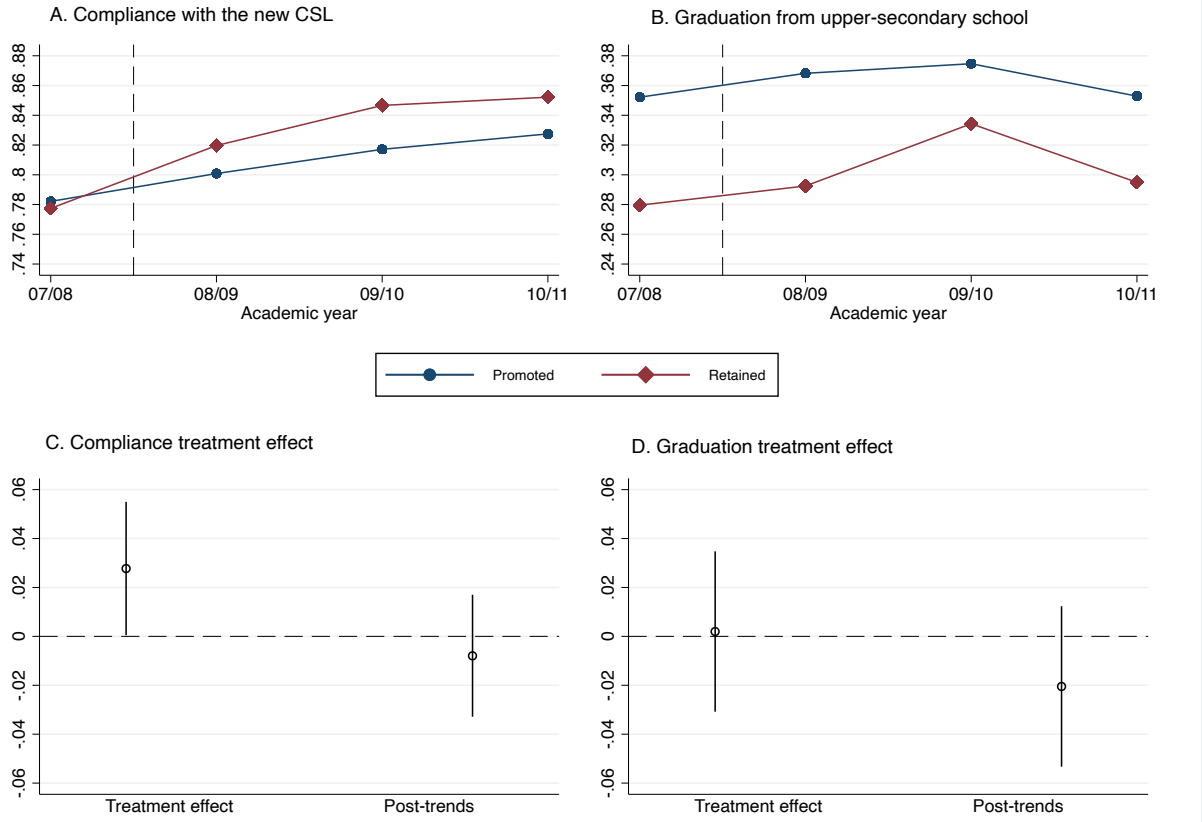
Retention rates by number of failed subjects out of the 10 school subjects taken in the 7^{th} grade, across 7^{th} grade cohorts. The grade retention margin sample consists of students who failed between 3 and 5 subjects.

Figure 4: Event-Study estimates



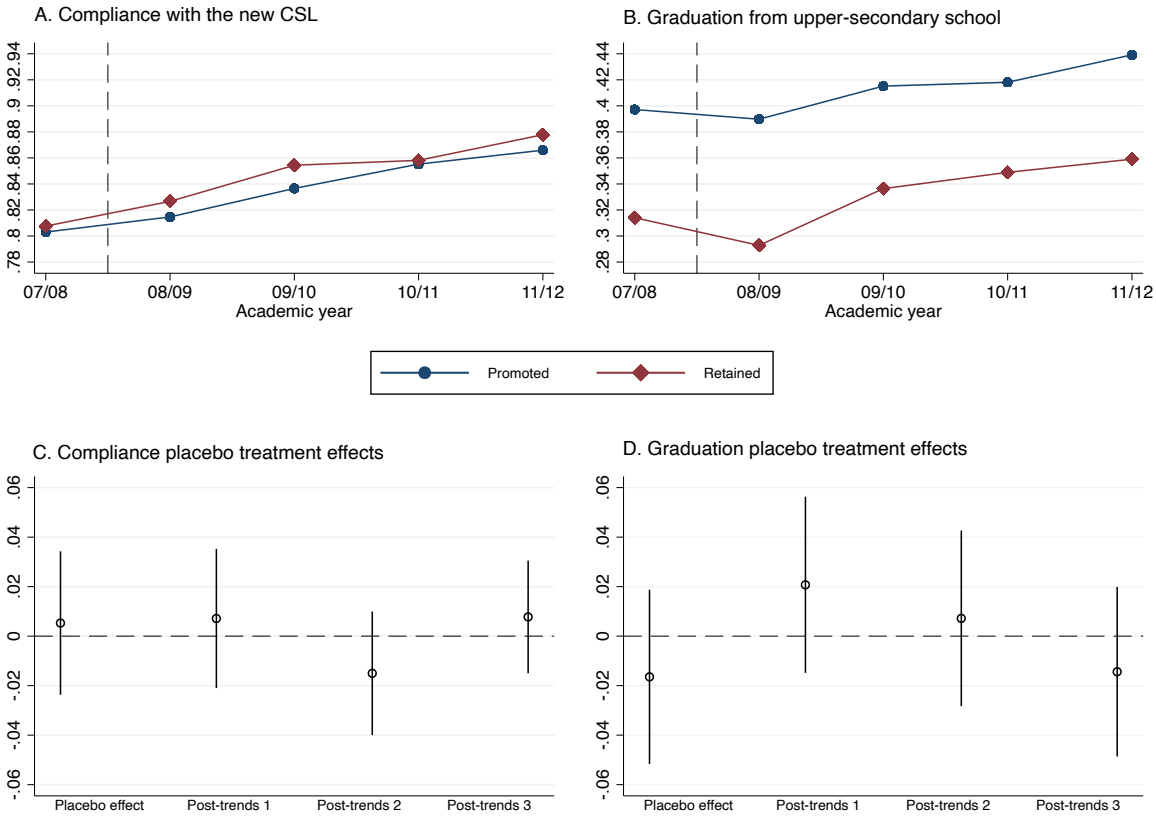
Event-study estimates based on equation 2 for the outcomes of compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school), and graduation from upper-secondary school, for different socioeconomic status and achievement groups, in comparison with the overall student population. Students are classified as low-SES if their parents or legal guardians have at most lower-secondary education or if they are beneficiaries of socioeconomic support. Students are classified as low-achieving if they failed at least two subjects in their first enrollment in that grade or if they are above the reference age for that grade. High-SES and high-achievement are determined by negating these conditions. Estimates take into account all pre-determined covariates described in Appendix Table A1. Lines display 95% confidence intervals around the point estimates. Standard errors are clustered at the classroom level.

Figure 5: Local Difference-in-Difference estimates



Panels A and B display compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) and upper-secondary school graduation rates, respectively, across 7th grade cohorts at the retention margin. Promoted students are shown in blue and retained students in red. Panels C and D plot Difference-in-Difference estimates from equation 3 for the same outcome variables and sample. Lines display 95% confidence intervals around the point estimates. The *Treatment effect* estimate compares retained with promoted students in the academic years of 2008/09 vs 2007/08, while the *Post-Trends* estimates compares these groups in 2010/11 vs 2009/10. Estimates take into account all covariates described in Appendix Table A1. Standard errors are clustered at the school level.

Figure 6: Placebo estimates, 8th grade cohorts



Panels A and B display compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) and upper-secondary school graduation rates, respectively, across 8th grade cohorts at the retention margin. Promoted students are shown in blue and retained students in red. Panels C and D plot Difference-in-Difference estimates from equation 3 for the same outcome variables. Lines display 95% confidence intervals around the point estimates. The *Placebo effect* estimate compares retained with promoted students in the academic years of 2008/09 vs 2007/08, while the *Post-Trends* estimates compare these groups in 2009/10 vs 2008/09, 2010/11 vs 2009/10, and 2012/11 vs 2010/11, respectively for *Post-Trends* 1 to 3. Estimates take into account all covariates described in Appendix Table A1. Standard errors are clustered at the school level.

Appendix

Additional tables

Table A1: List of variables used

Variable	Description	Type
	Outcomes	
Compliance	Staying in school until 18 years old, or graduating upper-secondary school	Binary
Graduation	Graduating upper-secondary school, in either the academic or vocational tracks, measured 7 years after the relevant grade-cohort	Binary
Years of education	Difference in years between the relevant grade-cohort academic year and that when the student is last observed in the dataset	Discrete
Graduation from lower secondary-school	Graduating in lower-secondary school from the main general track	Binary
Enroll in upper-secondary school	Enrolling in upper-secondary school, in either the academic or vocational tracks	Binary
Enroll in academic track	Ever enrolling in the academic track of upper-secondary school	Binary
	Pre-determined variables	
Male	Student's sex is male	Binary
1st generation immigrant	Student was born outside of Portugal and parents were also born outside of Portugal	Binary
2nd generation immigrant	Student was born in Portugal but parents were born outside of Portugal	Binary
Socioeconomic support: full	Student receives free lunch at school, and economic support for school material and transportation costs (support level varies by year)	Binary
Socioeconomic support: partial	Student receives lunch at school at half-price, and economic support for school material and transportation costs (support level varies by year)	Binary
Father unemployed	Father is unemployed	Binary
Mother unemployed	Mother is unemployed	Binary
Computer at home	Student has access to a computer at home	Binary
Internet at home	Student has access to an internet connection home	Binary
Parental education: below upper-secondary	Highest parental education is below upper-secondary school level (reference category)	Binary
Parental education: upper-secondary	Highest parental education is at most upper-secondary school level	Binary
Parental education: tertiary	Highest parental education is at tertiary level	Binary
Above reference age	Student's age is above the reference age for the relevant grade	Binary
Missing indicators	Missing indicators for migrants status, parental unemployment, and parental education. Other variables do not have missing information.	Binary
	School subject scores	
GPA	Grade point average for all subjects taken in the relevant grade	Continuous (1-5)
Failed subject indicators	Indicators for failing each of the subjects taken in the relevant grade	Binary
	School-level variables	
Retention rate	School-level retention rate	Continuous (0-1)
School-level student background	Proportion of male students, students with any immigrant background, any socioeconomic support, unemployed father, unemployed mother, computer at home, internet at home, and parents with tertiary education at the school level	Continuous (0-1)
Classroom-level background	Average of 6th grade Portuguese and Mathematics exams at the classroom level	Continuous (1-5)

Table A2: Descriptive statistics and balance, post-reform cohorts

	Groups				Differences within each year		Differences across years		DiD
	Retained 2009/10	Promoted 2009/10	Retained 2010/11	Promoted 2010/11	2007/08	2008/09	Retained	Promoted	Diff.
	(1)	(2)	(3)	(4)	(1)-(2) (5)	(3)-(4) (6)	(3)-(1) (7)	(4)-(2) (8)	(7)-(8) (9)
Pre-determined variables:									
Male	0.62	0.54	0.59	0.56	0.08***	0.03**	-0.03**	0.02*	-0.05***
Parent Ed: Below Upper-Sec.	0.67	0.71	0.67	0.70	-0.04***	-0.03***	0.00	-0.01	0.01
Parent Ed: Upper-Sec.	0.18	0.15	0.18	0.15	0.04***	0.03***	0.00	0.00	0.00
Parent Ed: Tertiary	0.03	0.03	0.04	0.04	0.00	0.01*	0.01*	0.00	0.01
1st Gen Immigrant	0.06	0.05	0.06	0.05	0.01**	0.01*	0.00	0.00	-0.01
2nd Gen Immigrant	0.04	0.03	0.03	0.03	0.01**	0.00	0.00	0.00	-0.01
Socioeconomic support: full	0.34	0.37	0.34	0.37	-0.03***	-0.03**	0.01	0.00	0.00
Socioeconomic support: partial	0.19	0.19	0.20	0.20	0.00	0.00	0.01	0.02*	-0.01
Father unemployed	0.05	0.07	0.07	0.07	-0.01**	0.00	0.01**	0.00	0.01
Mother unemployed	0.10	0.12	0.12	0.11	-0.01*	0.01	0.02**	0.00	0.02*
Computer at home	0.56	0.53	0.53	0.48	0.03***	0.05***	-0.03**	-0.05***	0.02
Internet at home	0.36	0.32	0.33	0.29	0.04***	0.04***	-0.04***	-0.03***	0.00
Above reference age	0.36	0.39	0.37	0.38	-0.04***	-0.01	0.01	-0.02	0.03
7th grade scores (1-5):									
Mathematics score	2.17	2.33	2.16	2.31	-0.16***	-0.15***	-0.02	-0.02**	0.01
	(0.40)	(0.50)	(0.39)	(0.48)					
Portuguese score	2.37	2.66	2.37	2.63	-0.29***	-0.26***	0.00	-0.03***	0.03
	(0.50)	(0.49)	(0.50)	(0.49)					
GPA	2.63	2.78	2.63	2.79	-0.03***	-0.16***	0.00	0.01***	-0.01
	(0.11)	(0.11)	(0.11)	(0.11)					
N	2 680	4 265	2 733	4 435	6 945	7 168	5 413	8 700	14 113

Columns 1 and 2 present summary statistics for retained and promoted groups at the 7th grade retention margin in the 2009/10 academic year. Columns 3 and 4 display the same statistics for these groups in the 2010/11 academic year. Standard deviations are presented in parentheses for subject score variables. Columns 5 and 6 present differences between retained and promoted groups within each academic year. Columns 7 and 8 present differences between retained and promoted groups within each academic year, respectively. Column 9 presents the change in the composition of retained versus promoted students from 2009/10 to 2010/11 by reporting the interaction term, γ, in equation 3 without controls, equivalent to subtracting the differences in columns 7 and 8. Significance levels: ***p < 0.01, **p < 0.05, *p < 0.1.

Table A3: Additional treatment effect heterogeneity by gender and socioeconomic status

	Male	Female	Low SES	High SES
	(1)	(2)	(3)	(4)
A. Compliance outcome				
Post-period x Retained	0.018	0.033	0.024	0.024
	(0.019)	(0.021)	(0.016)	(0.030)
Post-period	0.018	0.018	0.024**	-0.009
	(0.012)	(0.013)	(0.010)	(0.020)
Retained	0.010	-0.024	-0.005	-0.022
	(0.014)	(0.015)	(0.012)	(0.021)
Constant	0.765***	0.804***	0.767***	0.892***
	(0.009)	(0.009)	(0.007)	(0.014)
Controls	No	No	No	No
N	7,656	5,789	10,804	1,883
R-squared	0.002	0.002	0.002	0.001
B. Graduation outcome				
Post-period x Retained	0.019	-0.027	-0.003	-0.001
	(0.021)	(0.026)	(0.019)	(0.044)
Post-period	0.006	0.027*	0.012	0.031
	(0.014)	(0.016)	(0.011)	(0.029)
Retained	-0.070***	-0.072***	-0.071***	-0.082***
	(0.015)	(0.019)	(0.013)	(0.031)
Constant	0.318***	0.396***	0.354***	0.361***
	(0.010)	(0.012)	(0.008)	(0.021)
Controls	No	No	No	No
N	7,656	5,789	10,804	1,883
R-squared	0.005	0.008	0.006	0.009

Each column shows estimates of the Difference-in-Differences specification of equation 3 for the outcomes of compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) in Panel A, and upper-secondary school graduation in Panel B, for different student groups. Students are classified as low-SES (socioeconomic status) if their parents or legal guardians have at most lower-secondary education or if they are beneficiaries of socioeconomic support. High-SES is determined by negating this condition. Standard Errors are clustered at the school level. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Table A4: Local Difference-in-Differences estimates for additional outcomes

	Years of education	Graduation lower-secondary school	Enroll upper-secondary school	Enroll academic track	At least 5 years of education	Years in upper-secondary school
	(1)	(2)	(3)	(4)	(5)	(6)
A. Retention margin						
Post-period x Retained	0.064	0.019	-0.014	0.025	0.011	0.081*
	(0.062)	(0.018)	(0.014)	(0.020)	(0.014)	(0.047)
Post-period	0.061	-0.018*	0.031***	-0.026**	0.024***	-0.136***
	(0.039)	(0.011)	(0.009)	(0.012)	(0.009)	(0.028)
Retained	0.257***	-0.083***	-0.032***	-0.022	-0.006	-0.196***
	(0.045)	(0.013)	(0.010)	(0.016)	(0.012)	(0.036)
Constant	5.380***	0.570***	0.818***	0.311***	0.766***	3.204***
	(0.028)	(0.008)	(0.006)	(0.011)	(0.009)	(0.023)
Controls	No	No	No	No	No	No
N	13,445	13,445	13,445	11,003	13,445	11,003
R-squared	0.007	0.005	0.004	0.001	0.001	0.007
B. Retention margin & Above reference age						
Post-period x Retained	0.119	0.062**	-0.005	0.055**	0.036	0.143*
	(0.102)	(0.027)	(0.025)	(0.027)	(0.028)	(0.079)
Post-period	0.082	-0.048***	0.028*	-0.048***	0.017	-0.160***
	(0.063)	(0.017)	(0.015)	(0.016)	(0.016)	(0.044)
Retained	0.076	-0.135***	-0.054***	-0.041**	-0.039*	-0.208***
	(0.072)	(0.019)	(0.017)	(0.020)	(0.021)	(0.057)
Constant	4.857***	0.438***	0.741***	0.218***	0.654***	2.984***
	(0.045)	(0.012)	(0.011)	(0.013)	(0.014)	(0.035)
Controls	No	No	No	No	No	No
N	5,369	5,369	5,369	3,939	5,369	3,939
R-squared	0.003	0.013	0.005	0.003	0.002	0.006

Each column shows estimates of the Difference-in-Differences specification of equation 3 for additional students' educational path outcomes. These outcomes are defined in Appendix Table A1. Estimates shown in columns 4 to 6 are conditional on upper-secondary school enrollment. Panel A shows estimates for the grade retention margin sample, while Panel B shows these for the sub-group of students above the grade reference age. The grade retention margin is defined as failing between 3 and 5 subjects, out of the 10 subjects taken in this grade. The reference age in the 7^{th} grade is 13 years old. Standard Errors are clustered at the school level. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1.

Table A5: Student and teacher composition in 10^{th} grade school classes

Academic year	Share under new CSL	Share Low-achieving	Share Low-SES	Number of students	Average class size	Share female teachers	Average teacher age	Average experience teachers
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. All classes								
2008/09	0.0%	61.2%	64.1%	112 104	23.11	85.4%	45.89	20.67
2009/10	0.0%	64.1%	68.4%	109 858	22.43	88.1%	45.02	19.75
2010/11	0.0%	68.2%	68.0%	108 072	22.76	85.9%	46.55	21.48
2011/12	0.0%	75.0%	67.6%	109 139	22.27	87.5%	47.61	22.44
2012/13	73.7%	66.9%	65.4%	111 063	24.12	85.1%	49.02	24.22
2013/14	93.2%	74.6%	64.4%	110 355	24.65	85.7%	49.72	25.15
2014/15	100.0%	65.1%	62.4%	108 383	24.04	86.2%	50.38	25.63
2015/16	100.0%	64.9%	60.6%	112 107	24.98	85.6%	50.85	26.08
2016/17	100.0%	67.4%	60.4%	112 086	25.43	86.0%	51.10	25.58
2017/18	100.0%	62.4%	59.7%	109 328	25.15	86.4%	51.47	25.64
B. Academic classes								
2008/09	0.0%	48.9%	56.6%	66 764	24.19	84.2%	46.65	21.55
2009/10	0.0%	48.3%	59.5%	64 837	23.68	87.6%	45.91	20.92
2010/11	0.0%	53.8%	58.5%	64 567	23.91	85.3%	47.64	22.83
2011/12	0.0%	62.9%	57.8%	64 154	23.21	86.6%	48.28	23.38
2012/13	84.1%	53.7%	56.7%	67 508	24.35	85.5%	49.25	24.66
2013/14	97.3%	63.6%	55.0%	68 417	25.44	86.6%	50.05	25.73
2014/15	100.0%	52.6%	53.7%	69 646	25.00	87.4%	50.81	26.32
2015/16	100.0%	52.4%	52.2%	70 760	25.58	85.9%	51.47	27.10
2016/17	100.0%	55.6%	52.1%	69 916	25.94	87.0%	51.71	26.62
2017/18	100.0%	48.8%	50.8%	67 640	25.87	87.2%	52.08	26.77
C. Vocational classes								
2008/09	0.0%	92.9%	84.4%	45 340	20.17	88.3%	43.91	18.31
2009/10	0.0%	93.2%	85.9%	45 021	19.94	88.8%	43.28	17.42
2010/11	0.0%	95.3%	86.9%	43 505	20.39	87.4%	44.37	18.93
2011/12	0.0%	97.6%	86.5%	44 985	20.55	88.9%	46.35	20.67
2012/13	55.8%	95.3%	84.8%	43 555	23.66	84.0%	48.41	23.17
2013/14	85.9%	97.8%	85.0%	41 938	23.12	83.9%	49.12	24.04
2014/15	100.0%	94.4%	83.5%	38 737	21.93	83.4%	49.43	24.12
2015/16	100.0%	94.7%	81.2%	41 347	23.68	84.9%	49.41	23.73
2016/17	100.0%	95.9%	81.2%	42 170	24.30	83.7%	49.63	23.25
2017/18	100.0%	93.6%	80.7%	41 688	23.58	84.7%	50.08	23.13

Student and teacher composition in all 10^{th} grade school classes, across academic years. Student CSL exposure, in column 2, is as determined by law: students enrolled in grades 1 to 7 from the academic year of 2009/10 onwards are under the new CSL, while those enrolled in the 8^{th} grade or above in 2009/10 fall under the old CSL. 10^{th} grade students whose exposure is undetermined because they are not observed in all years of the dataset are assumed to be under the new CSL if first observed in the 10^{th} grade in 2012/13 or later. Students are classified as low-achieving (column 3) if they failed either the Portuguese or Mathematics 9^{th} grade exam, or are above the 10^{th} grade reference age. Students are classified as low-SES (column 4) if their parents or legal guardians have at most lower-secondary education or if they are beneficiaries of socioeconomic support. Columns 7 to 9 consider characteristics of the Portuguese class teacher, a subject that is common across all upper-secondary school tracks. Teachers' age and experience are measured in years. Additionally, there is data on teachers' skipped classes, education levels, type of contract, and wages. These are not report for the sake of simplicity and because no clear pattern is found.

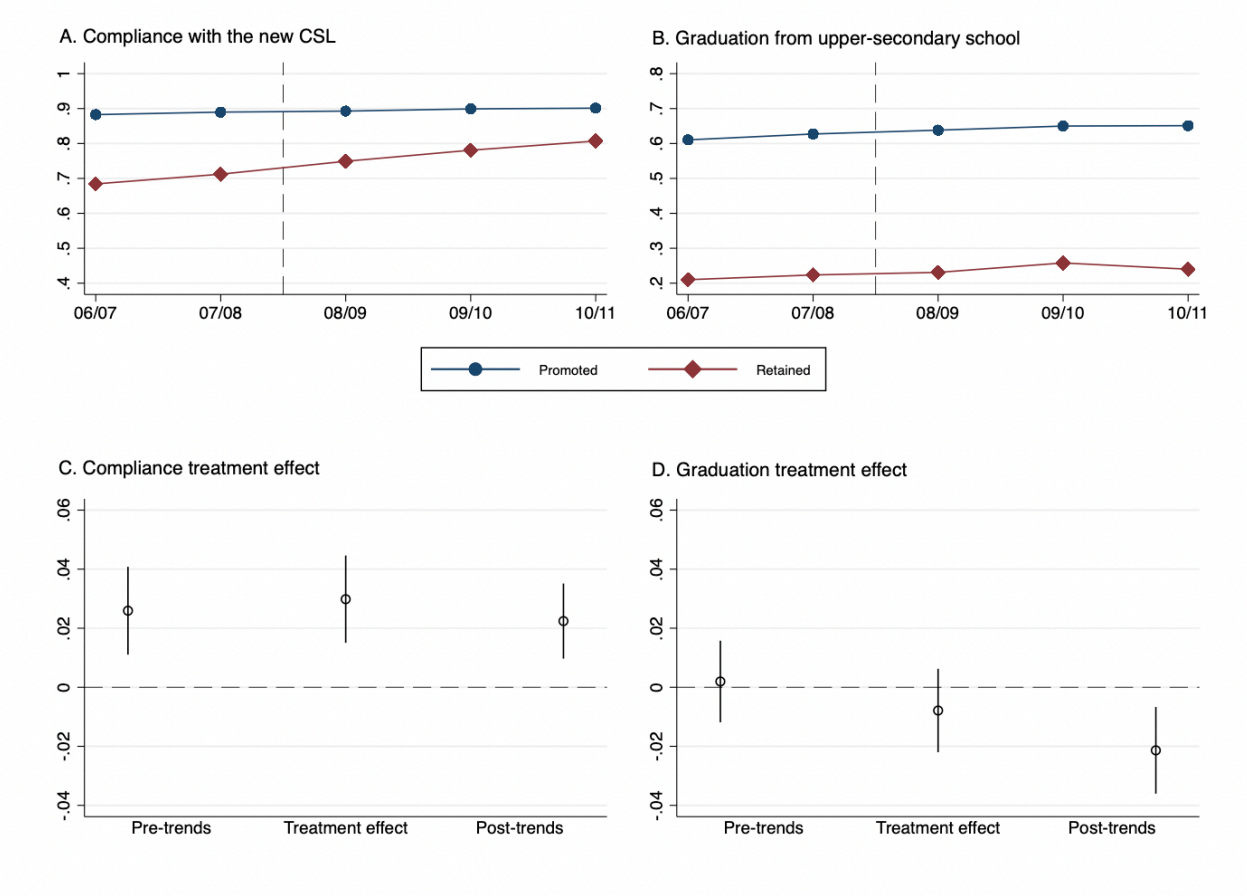
Table A6: School resources

Year	Enrollment	Number of estabilishements	Spending % of GDP	Spending per student US dollars
(1)	(2)	(3)	(4)	(5)
A. Primary to Post-Secondary Non-Tertiary				
2008	1 541 796	3 573	3.27	5 985
	(97.6)	(98.2)	(72.4)	(72.7)
2009	1 788 035	3 601	3.83	6 814
	(113.2)	(99.0)	(84.8)	(82.8)
2010	1 748 084	3 632	3.74	-
	(110.7)	(99.8)	(82.9)	
2011	1 657 008	3 623	3.56	-
	(104.9)	(99.6)	(79.0)	
2012	1 578 936	3 638	4.51	8 232
(base=100)	(100)	(100)	(100)	(100)
2013	1 502 311	3 628	4.72	9 514
	(95.1)	(99.7)	(104.6)	(115.6)
2014	1 454 213	3 628	4.46	9 411
	(92.1)	(99.7)	(99.0)	(114.3)
2015	1 447 495	3 643	3.94	8 575
	(91.7)	(100.1)	(87.4)	(104.2)
2016	1 411 234	3 658	3.89	9 108
	(89.4)	(100.5)	(86.2)	(110.6)
2017	1 404 592	3 641	3.99	9 823
	(89.0)	(100.1)	(88.5)	(119.3)
2018	1 393 495	3 627	3.82	10012
	(88.3)	(99.7)	(84.6)	(121.6)
B. Upper-Secondary				
2008	349 477	917	-	-
	(85.0)	(96.8)		
2009	498 327	927	-	-
	(121.2)	(97.9)		
2010	483 982	937	-	-
	(117.7)	(98.9)		
2011	440 895	937	-	-
	(107.2)	(98.9)		
2012	411 238	947	2.87	9 800
(base=100)	(100)	(100)	(100)	(100)
2013	398 447	953	2.90	11 081
	(96.9)	(100.6)	(101.0)	(113.1)
2014	385 210	958	2.70	10 747
	(93.7)	(101.2)	(94.1)	(109.7)
2015	393 618	962	2.37	9 565
	(95.7)	(101.6)	(82.8)	(97.6)
2016	391 538	963	2.36	10 181
	(95.2)	(101.7)	(82.4)	(103.9)
2017	399 775	965	2.38	10 708
	(97.2)	(101.9)	(83.1)	(109.3)
2018	401 050	960	2.30	11 000
	(97.5)	(101.4)	(80.3)	(112.2)

School resources in the overall school system, in Panel A, and in upper-secondary schools, in Panel B. Data used in columns 2 and 3 comes from PORDATA, a certified statistical database about Portugal. Data used in columns 4 and 5 comes from OECD Data Explorer. Both databases report statistics for calendar years and not academic years. OECD reports expenditure in the national currency converted into equivalent USD by dividing the national currency figure by the purchasing power parity (PPP) index for GDP. Expenditure per student on educational institutions at a particular level of education is calculated by dividing total expenditure on educational institutions at that level by the corresponding full-time equivalent enrolment. The - symbol indicates that data is missing.

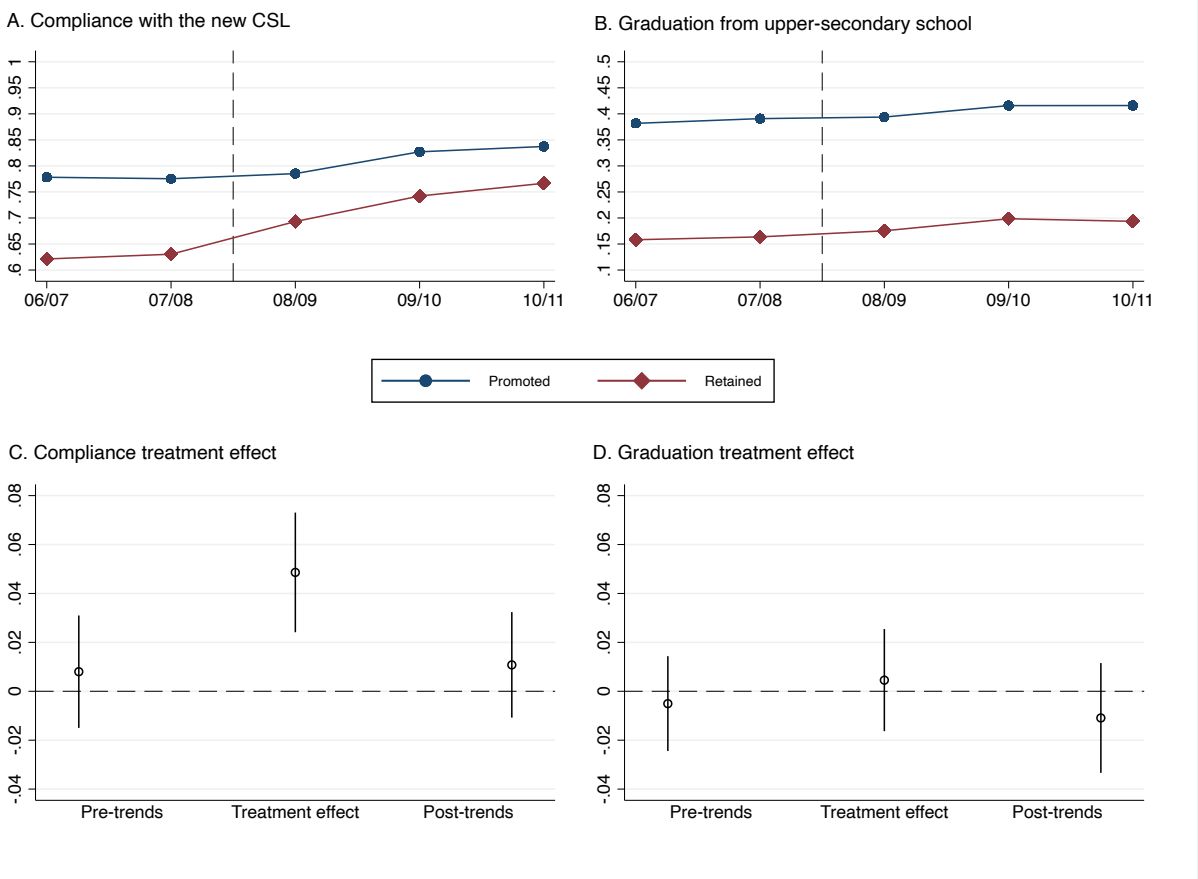
Additional figures

Figure A1: Difference-in-Difference estimates, full population



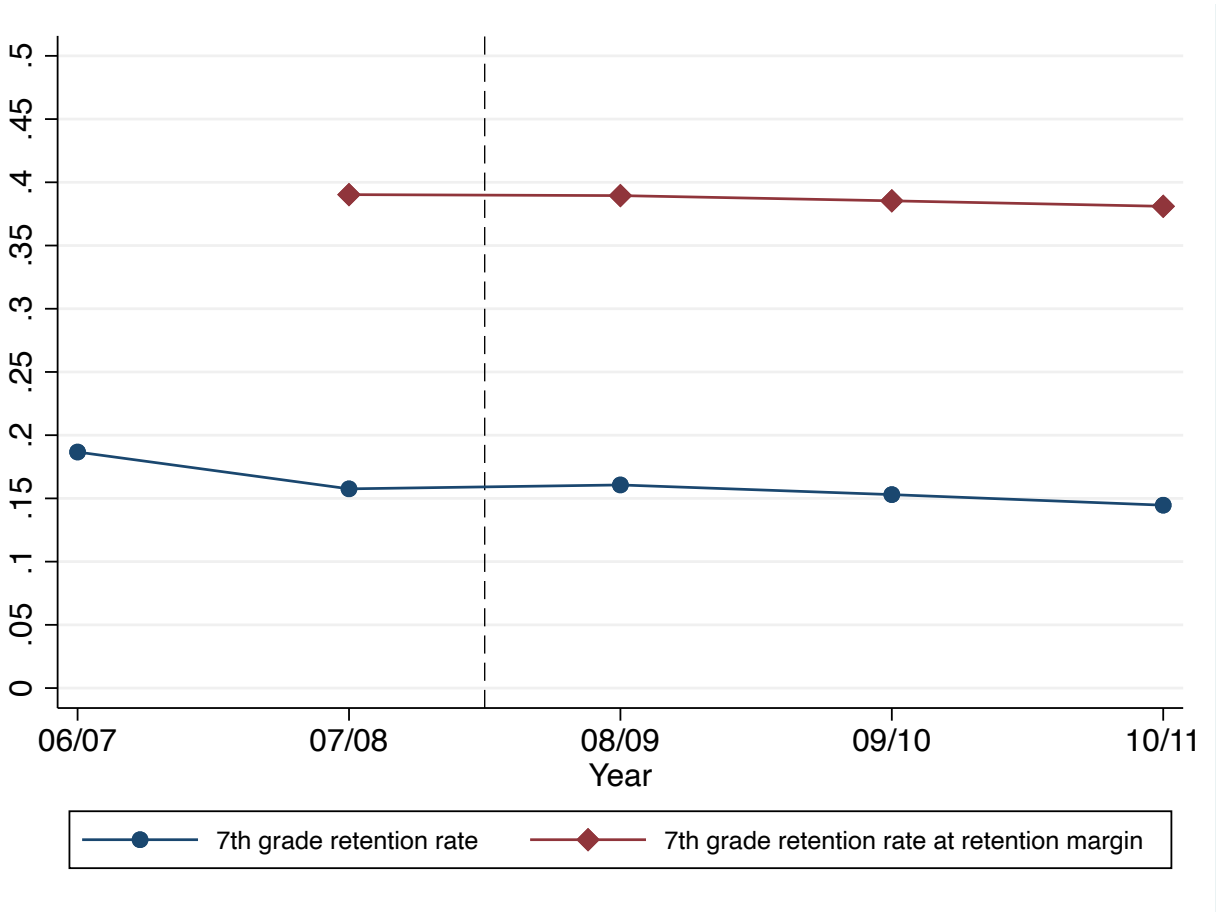
Panels A and B display compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) and upper-secondary school graduation rates, respectively, across 7^{th} grade cohorts. Promoted students are shown in blue and retained students in red. Panels C and D plot Difference-in-Difference estimates from equation 3 for the same outcome variables. Lines display 95% confidence intervals around the point estimates. The *Treatment effect* estimate compares retained with promoted students in the academic years of 2008/09 vs 2007/08; while the *Pre-Trends* estimates compares these groups in 2010/11 vs 2009/10, and *Post-Trends* estimates compares these groups in 2010/11 vs 2009/10. Estimates take into account all covariates described in Appendix Table A1. Standard Errors are clustered at the school level.

Figure A2: Difference-in-Difference estimates, full population above reference age



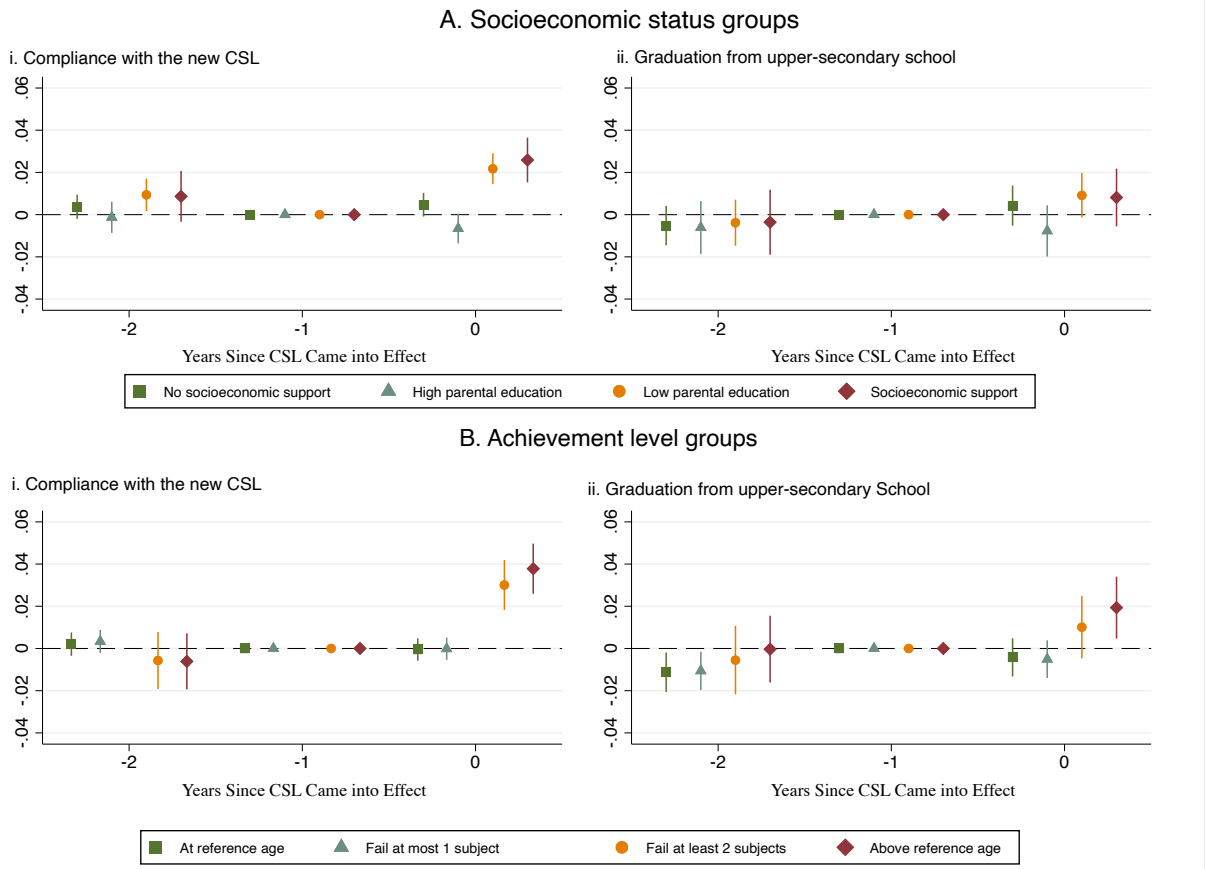
Panels A and B display compliance with new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) and upper-secondary school graduation rates, respectively, across 7th grade cohorts above the grade's reference age — i.e., considering students who were 14 years old at the end of the year. Promoted students are shown in blue and retained students in red. Panels C and D plot Difference-in-Difference estimates from equation 3 for the same outcome variables. Lines display 95% confidence intervals around the point estimates. The *Treatment effect* estimate compares retained with promoted students in the academic years of 2008/09 vs 2007/08; while the *Pre-Trends* estimates compares these groups in 2010/11 vs 2009/10, and *Post-Trends* estimates compares these groups in 2010/11 vs 2009/10. Estimates take into account all covariates described in Appendix Table A1. Standard Errors are clustered at the school level.

Figure A3: Retention rates in the 7th grade



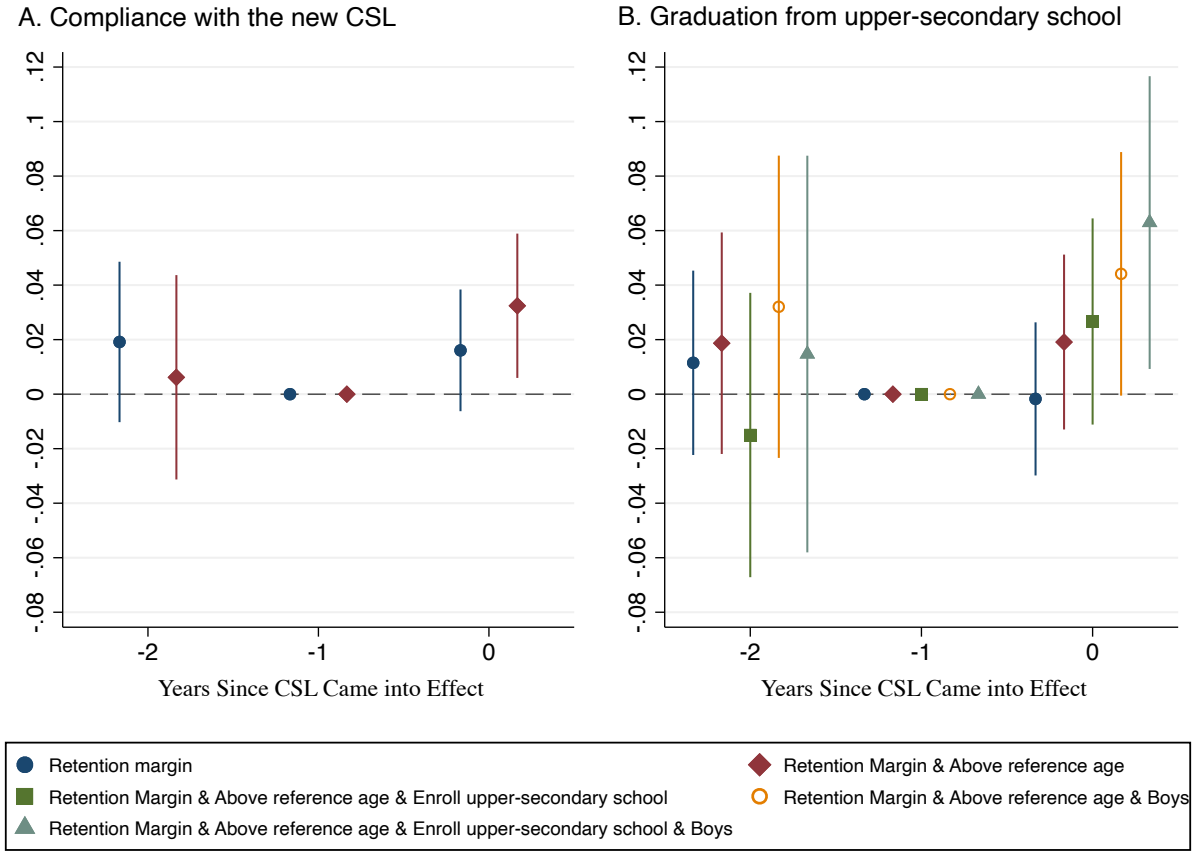
Grade retention rates across 7th grade cohorts for the full student cohort in blue, and for students at the grade retention margin in red. The grade retention margin is defined as failing between 3 and 5 subjects, out of the 10 subjects taken in this grade.

Figure A4: Event-Study estimates, socioeconomic status and achievement level groups



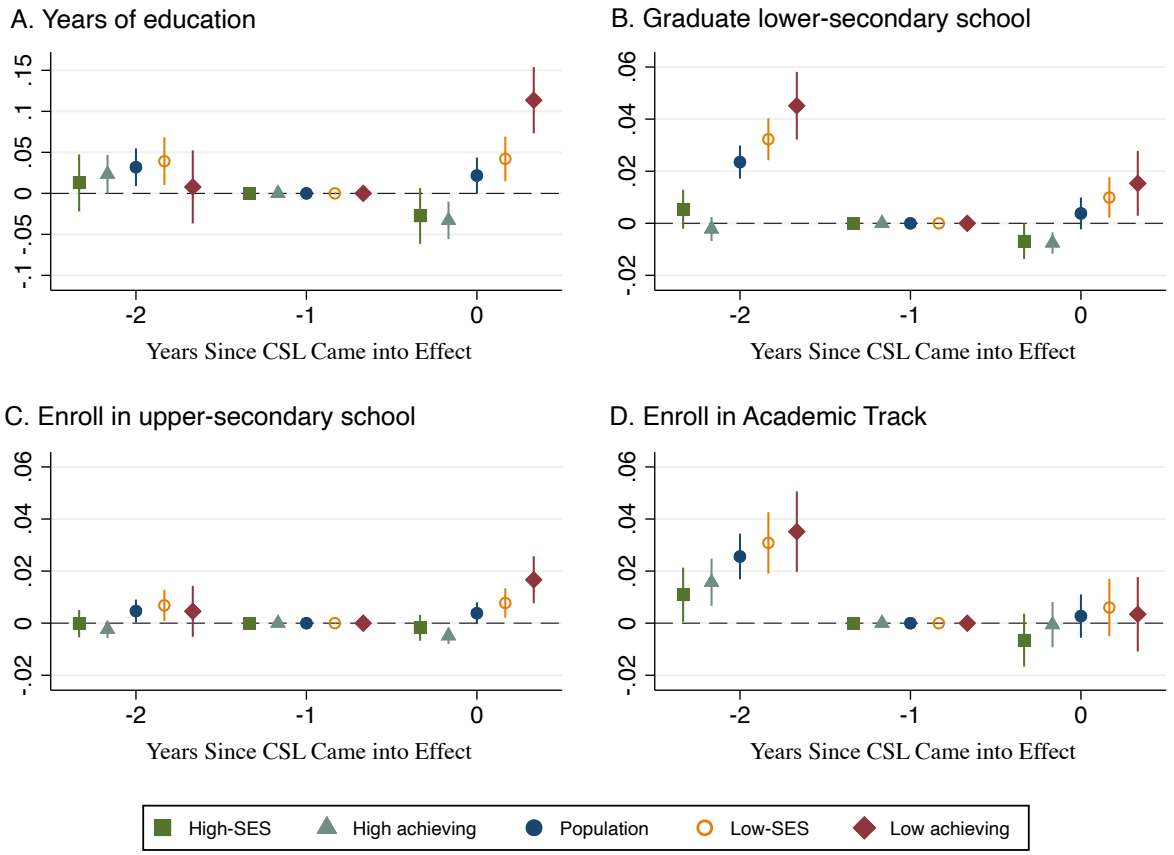
Event-study estimates based on equation 2 for the outcomes of compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school), and graduation from upper-secondary school, for different socioeconomic status and achievement groups. Socioeconomic support (named *ASE* in Portugal) is determined based on the household's income level. The group of students receiving this support considers both *full* and *partial* support, described in Appendix Table A1. Low parental education is defined as having parents or legal guardians with a maximum educational attainment of lower-secondary education. The reference age is 13 years old in the 7^{th} grade, and 15 years old in the 9^{th} grade. The number of failed subjects takes into account the 10 school subjects taken in both grades. Estimates take into account all pre-determined covariates described in Appendix Table A1. Lines display 95% confidence intervals around the point estimates. Standard errors are clustered at the classroom level.

Figure A5: Event-Study estimates, grade retention margin sub-groups



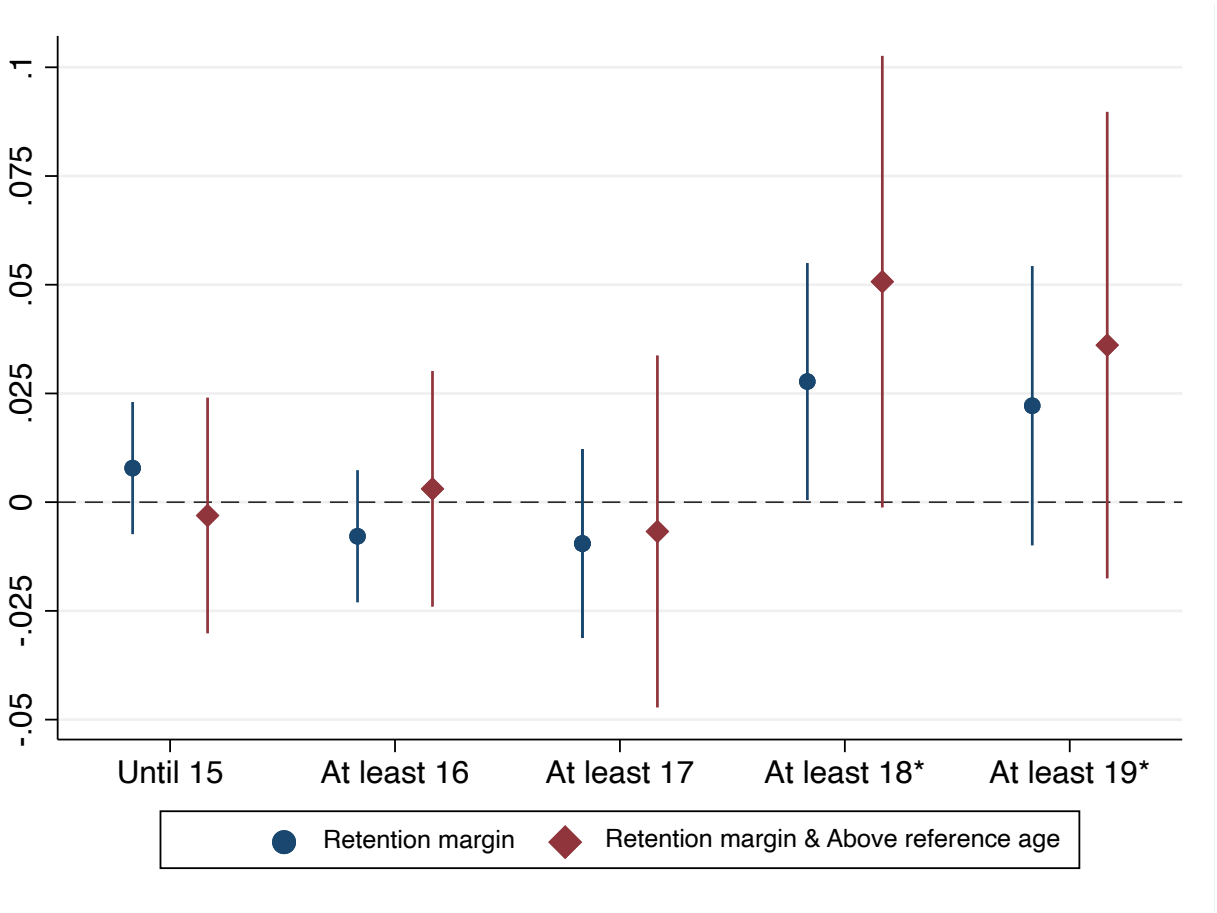
Event-study estimates based on equation 2 for the outcomes of compliance with the new Compulsory Schooling Law (defined as staying in school until 18 years old or graduating upper-secondary school) in Panel A, and graduation from upper-secondary school in Panel B, for students at the grade retention margin and subgroups at this margin. Students are classified as being at the grade retention margin if they failed between 3 and 5 subjects in their *first* enrollment in that grade. The reference age is 13 years old in the 7^{th} grade, and 15 years old in the 9^{th} grade. Estimates take into account all pre-determined covariates described in Appendix Table A1. Lines display 95% confidence intervals around the point estimates. Standard errors are clustered at the classroom level.

Figure A6: Event-Study estimates, additional outcomes



Event-study estimates based on equation 2 for additional educational path outcomes, for different socioeconomic status and achievement groups, in comparison with the overall student population. These outcomes are defined in Appendix Table A1. I classify students as low-SES if their parents or legal guardians have at most lower-secondary education or if they are beneficiaries of socioeconomic support. Students are classified as low-achieving if they failed at least two subjects in their first enrollment in that grade or if they are above the reference age for that grade. The reference age is 13 years old in the 7^{th} grade, and 15 years old in the 9^{th} grade. High-SES and high-achievement are determined by negating these conditions. Estimates take into account all pre-determined covariates described in Table A1. Lines display 95% confidence intervals around the point estimates. Standard errors are clustered at the classroom level.

Figure A7: Local Difference-in-Difference estimates, educational attainment



Local Difference-in-Differences estimates from equation 3 for the probability to stay in school until age: $\leq$ 15; $\geq$ 16; $\geq$ 17; $\geq$ 18 or graduation from upper-secondary school (i.e., compliance with the new compulsory schooling law); and $\geq$ 19 or graduation from upper-secondary school. The * symbol indicates that the two highest levels of attainment include graduation from upper-secondary school. Lines display 95% confidence intervals around the point estimates. Students at the grade retention margin are shown in blue and students at the grade retention margin above the reference age are shown in red. Estimates take into account all covariates described in Appendix Table A1. Standard errors are clustered at the school level.