The Effects of Changes in School Entry Age and Duration on Student Performance: Evidence from Brazilian Primary Schooling Reform

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Abstract

Despite important policy implications, there is limited evidence on the impact of lowering school entry age and increasing the length of primary education on student performance. This study examines these changes in the context of Brazil's 2006 compulsory schooling reform. We exploit differences in the years that schools adopted the policy as a plausibly exogenous source of variation and use students' birth months to predict school entry age. The overall impact of the policy package was to increase math and Portuguese test scores by approximately 0.10 and 0.03 standard deviations in students' fifth and ninth year of school. Among the students who entered primary school one year earlier, those without any prior education experienced larger increases in their fifth year test scores compared to students who gained an additional year of primary education at the expense of preschool. This advantage, however, disappeared by their ninth year. We discuss different mechanisms through which the policy may have influenced students' cognitive outcomes and conduct various robustness checks to verify the paper's interpretation.

Keywords: Compulsory Education, School Entry Age, Educational Outcomes

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1. Introduction

Lowering the minimum school entry age, which potentially increases the duration of compulsory education, could be a useful policy for improving student achievement. In countries where access to preschool education is limited, such a policy can provide more equal educational opportunities through earlier exposure to formal education (UNESCO (2004)). This is an alternative way to improve early childhood education in developing countries, which is the goal of many international organizations and developing country governments. In countries with better access to preschool education, a consequence of lowering the compulsory school attendance age could be the replacement of preschool with compulsory education. The effect of such a change on learning outcomes is unclear. While starting formal education earlier and increasing the duration of school can be beneficial components of early intervention programs (Cunha et al. (2006), Cunha and Heckman (2007)), there also exist advantages associated with starting school later. These include exposure to a playful learning environment at an earlier age, or the development of emotional stability or self-regulation in a later period (Hirsh-Pasek et al. (2009), Dee and Sievertsen (2016)).

The present study evaluates the impact of early exposure to primary school education on students' academic performance. To this end, we use Brazil's 2006 compulsory schooling reform, one of the only major country-level policy reforms that changed both school starting age and the length of education. The Brazilian government increased the duration of compulsory education from eight to nine years, and at the same time lowered the minimum age for some students by changing the cutoff date for school entry.[1] While all students attained an additional year of schooling, only those students who were born between the original and new cutoff dates began primary education one year earlier (we call them "early entrants"). Students born in other months began primary education at the same age as in the previous system, but took the exam when they were one year older (hereafter called "normal entrants"). Therefore, we first measure the overall effect of the policy package and then isolate the effect based on the age at school entry.

[1] Under the previous system, students needed to be seven years old by Dec. 31st to begin their primary education. After the reform, students had to be six by the beginning of the school year, which normally falls in February or March.

The major source of variation used in this paper is the differences in the years that schools adopted this policy. The Brazilian government gave schools up to four years to address the varying amounts of time they needed to prepare teachers, acquire resources, and alter their curriculum to accommodate the policy guidelines. Including school and year fixed effects, the adoption of the new educational system generated plausibly exogenous variation. We use this variation with data from school censuses and national standardized exams (Brazil Exam) taken by students in all public schools that had more than 20 students enrolled in the tested grades. These data sets cover the overwhelming majority of Brazilian public schools and include detailed information regarding school characteristics and students' socioeconomic status.

One concern regarding the validity of the empirical estimation is that each school decided which year to adopt the policy. As a consequence, inclusion of schools whose students would have performed well regardless of when the policy was implemented could potentially bias the results. Additionally, more involved parents could have enrolled their children in schools with 9-year systems when presented with the choice between schools under the new and old systems. To address this concern, we evaluate parallel trends, conduct a placebo test and event study, and investigate whether school or parental characteristics may have changed when the policy was adopted. There is little evidence that school-specific time trends, selectivity biases, or other factors influenced the results.

The overall impact of the compulsory schooling reform in Brazil was an approximately 0.10 standard deviation increase in both Portuguese and Mathematics test scores among 5th-year students. This is a sizable short-term impact that is comparable to the results of teacher bonus programs or school tracking systems evaluated in other studies (Duflo et al. (2011), Muralidharan and Sundararaman (2011))[2]. In the medium run—after 9 years—the effect of the policy faded out such that Portuguese and Mathematics scores increased by approximately 0.03 standard deviations. Beyond test scores, the present study finds that the policy significantly reduced the rates of grade

[2]Duflo et al. (2011) found a 0.14 standard deviation increase after 18 months of a tracking program. Muralidharan and Sundararaman (2011) found 0.27 and 0.17 standard deviation increases after 2 years of a teacher incentive program in India.

repetition in the short and medium run.

The policy impact on only the students who began their primary education one year younger (early entrants) showed a similar pattern, but the magnitudes were almost half the size of those seen in the entire sample. This result can be explained by different mechanisms through which the policy affects early and normal entrants. First, early entrants started their primary education and took the exam one year earlier than normal entrants. The difference between the effects of age at test and school starting age partially explains differential policy effects between the two groups.[3] Secondly, the impact of the policy on early entrants differed depending on whether these students were exposed to preschool education prior to school entry. This is due to the fact that early entrants with preschool education attained an additional year of schooling at the expense of their last year of preschool education whereas those without prior education attained an entire year of schooling with no such cost.[4]

The distinction for early entrants is critical to understanding the extent to which the implications of lowering school entry age differ in countries with universal versus insufficient availability of preschool education. We find that among the students who started their education a year earlier, those without preschool education attained a large increase only in short-term test scores.[5] Those who started their education a year earlier and had attended preschool, in contrast, experienced a smaller increase in their short-term test scores, but a more persistent increase in medium-term test scores.

This study contributes to the existing literature by providing policy relevant information on the impact of earlier (and longer) exposure to primary education. This contrasts with much of the literature on school reform that focuses on the impact of an additional year of compulsory education at relatively later ages. It also differs from studies that use birthday cutoff dates, which generally have had little to say about the impact of additional years of compulsory education. Further, by

[3] Black et al. (2011)and Crawford et al. (2010) showed that the effects of age at test on students' academic outcomes are significant and consistent while the effects of school starting age are smaller and fade out over time.

[4] The difference is smaller for normal entrants because normal entrants with preschool finish their entire preschool education before primary school entry.

[5] The positive effects no longer existed in their 9th year.

evaluating the extent to which the effects differed between students who did and did not attend preschool, and across the short- and medium run, this is the first study to examine the effect of lowering the school entry age in contexts of countries where preschool education is limited versus widespread. In addition, the present research utilizes exceptionally rich data in terms of the size and characteristics of the sample, which are particularly difficult to obtain in a developing country context. This enables us to examine important outcomes other than test scores, such as grade repetition, child labor and dropout rates.

The remainder of the paper is organized as follows: Section two reviews the previous literature related to school entrance age, age at test and duration of schooling. The third section provides an overview of the educational sector in Brazil and the compulsory schooling reform. The fourth and fifth sections explain the data and empirical strategy. Section six presents the results and section seven concludes.

2. Related Literature

The current study is related to a large international literature that examines the effects of age-related educational factors such as school starting age, duration of schooling, and testing age. Given that each factor has the potential to significantly affect students' outcomes, there is an extensive literature on the effects of single age-related factors. Previous studies measured the impact of a change in the length of schooling through compulsory schooling reforms,[6] unsafe weather conditions that cause temporary school closure,[7] or random variation of test dates for military service.[8] These studies mostly examine an exogenous change of an additional year of schooling at a later age (e.g., age 14-16) and found that more time spent in school with formal instruction has a positive effect on students' learning and labor market outcomes.

In recent periods, there has also been a growing literature on the impact of school starting age.

[6] Brinch and Galloway (2012); Banks and Mazzonna (2012); Dahmann (2017); Parinduri (2014); Krashinsky (2014); Eble and Hu (2017).

[7] Agúero and Beleche (2013).

[8] Carlsson et al. (2015).

One of the commonly used empirical strategies is based on cutoff dates. The major assumption behind this approach is that children born just before the cutoff date enter the school almost a year earlier than those born right after the cutoff while the other aspects of the students are arguably similar. The evidence from these studies is inconclusive. The majority of studies found a positive impact from starting school at an older age,[9] but there are a few studies that found no impact on academic performance or labor market outcomes, or even small negative impacts on IQ tests.[10] However, the major obstacles in these studies is to distinguish the effect of school starting age from the age at test effect. As older students are usually older when they are tested, studies often cannot completely separate these two effects.[11]

The current study is distinguished from most previous studies in that it measures the combined effects of age-related factors. It is essential to simultaneously examine the effects of altering school starting age and duration to provide policy-relevant information about the effects of lowering school starting age. This is because students start their formal education at earlier ages and spend more years in school when the entry age is lowered. Previous studies that relied on variation of a single factor, either in school entry age or duration, had limited policy implications given that all students either had the same school starting age or the same duration of education.

There are few studies that have measured the combined effect of age-related factors. Using the variation of school cutoff dates across local authorities in England, Cornelissen et al. (2018) and Crawford et al. (2010) showed that children exposed to compulsory education earlier (at the age of 4 instead of 5) obtained better test scores in earlier ages, although most of the cognitive benefits were no longer found from age 11. Based on multiple variations in the Dutch education system, Leuven et al. (2010) also showed that the availability of earlier exposure to compulsory education has positive effects, but only for disadvantaged students.

[9] Datar (2006); Bedard and Dhuey (2006); Fredriksson and Ockert (2014); McEwan and Shapiro (2008); Puhani and Weber (2007).

[10] Fertig and Kluve (2005); Dobkin and Ferreira (2010); Black et al. (2011).

[11] Fredriksson and Ockert (2014) and Black et al. (2011) used multiple variations to isolate the impact of one particular factor. Fredriksson and Ockert (2014)isolated the impact of school starting age from other factors using the cutoff date and minimum compulsory schooling law in Sweden, and Black et al. (2011) used school entry cutoff age in Norway and variation of the test date by birth month to isolate the impact of school starting age.

Our study differs from these studies on earlier exposure to compulsory education in several ways. First, we demonstrate how the effects of early exposure to primary education differed by students' preschool education. We also examine other outcomes, such as grade repetition, dropout, and child labor, all of which are important measures in the context of many developing countries. In addition, using a nation-wide compulsory schooling reform in a developing country, we examine the effect of lowering school starting age from 7 to 6 years old, an age range that is relevant for many developing countries. Considering that students have different cognitive skills at each age, the policy to lower school starting age could have different effects on students' outcomes depending on which age range is affected. In our setting, we found more persistent effects of earlier exposure to primary education.

3. Context and compulsory education in Brazil

3.1 Context

The Brazilian educational system can be divided into early childhood education, compulsory education (primary school), and secondary school education. Compulsory education now covers ages 6 to 14, whereas before the policy reform it spanned ages 7 to 14. This is comparable to primary and middle school education in countries like the U.S. The local municipal government is largely responsible for primary education in Brazil, although a small portion of the schools are run by the state government, federal government, or private institutions. Private institutions provide approximately 10% of total primary education in Brazil and were also affected by the policy reform. However, this study was not able to measure the effect of the policy reform on the students enrolled in private institutions because they do not take the national exam used to measure test scores.

In 1996, the federal government began to allow the enrollment of 6-year-old students in a 9-year primary school system. The adoption of a new system was not mandatory at this time and depended on the decision of local governments or schools. Although it was possible that schools could have adopted the new system prior to 2004, we did not identify any schools that had done so.[12] In 2001, the federal government established nine years of primary education as a goal eventually to be achieved throughout the country (Law 10,172 of January 2001). The stated purpose was to provide every child with an equal opportunity of obtaining a quality education.

Five years later, Law 11,274 of February 2006 officially mandated an increase in the duration of compulsory education from 8 to 9 years and to lower the minimum school entry age from seven to six years old. The law required this change to be completed by 2010. The majority of schools adopted the new system after 2006, but there were two states--Minas Gerais and Goiás--and a few municipalities, that chose to adopt the new system in 2004. Minas Gerais implemented the policy much more rapidly than Goiás, as the state government mandated an immediate adoption of the new system. Goiás, in contrast, opted for a gradual implementation of the policy. Given that these municipalities and states, which adopted the policy earlier, could have had different characteristics than other regions and potentially contaminate our estimates, we conducted robustness checks to examine whether the same results held without including these schools. We did not find any evidence that these regions distorted the results.

3.2 Change in school cutoff date

Another important change made as part of this reform was the school entrance cutoff date. In Brazil, students normally started their primary education in the year they turned seven with the school year starting around February, and the cutoff date at the end of the year. The compulsory schooling reform mandated a change in the cutoff date to the beginning of the school year. However, ambiguity and the lack of enforcement led different states to choose different cutoff dates, with most states adopting between February and April. The new school cutoff date was officially set to March 31 in 2009 by the federal government, but several states appealed the decision and kept different cutoff dates. We checked every state's legislation to identify the school cutoff date in the year of

[12] In the school census, some schools indicated that they had a 9-year system in the early 2000s. However, when we matched with enrollment no schools had students enrolled in the first year of the new system. Therefore, we excluded these schools from the analysis.

policy implementation, and use it in the analysis.

The change in the cutoff date is a very important element of the present study because it determines which students began their primary education a year earlier than their peers who were attending schools under the previous system. Figure 1 shows one example of policy implementation. In this example, we compare the students who entered school in 2006 under the 8-year system (control group) with the students who entered school in 2005 when the 9-year system was adopted (treated group). All students took the exam in 2009, but there were variations in school starting age, duration, and test age due to the school reform.

A control group represents the schools under the previous system with the Dec. 31st cutoff date. Students in this system began their education in 2006 and took the first standardized exam in 2009 after 4 years of primary education. They all shared the same birth year (1999) because they had all turned 7 years old by December 31st of 2006. The age of these students in February, when the school year typically begins, ranged from 74 months (6.18 years) to 85 months (7.08 years).

A treated group in Fig 1 represents the schools that adopted the 9-year system in 2005 with the new cutoff date of March 31st. Since the school reform increased the number of years of education by one year, students who began their education in 2005 also took the exam in 2009. Unlike the previous system where students in the same cohort were born in the same year, students entering schools under the new system in 2005 had two birth years. First, students who were born in 1998, regardless of the month, turned 7 years old in 2005 and therefore began their primary education in that year. However, because the reform lowered the minimum age of school entry to 6 by the new cutoff date, additional younger students born between Jan 1st and March 31st in 1999 (marked "Early") also started their primary education in 2005. In February, the age of these additionally enrolled students ranged from 71 to 73 months.

Note that in February when the school year begins, the early group in the new system is exactly 12 months younger than the students born in the same months of 1999 but enrolled in previous system (i.e., 71-73 months vs 83-85 months). This comparison is demonstrated more clearly in Figure 2. The red group in treated schools represents the students who were additionally enrolled due to the change in cutoff dates. These students started their education exactly a year earlier in 2005 and received one more year of education than the students born in the same periods but enrolled in the 8-year system (control group).

It is also important to note that the students who were born between April 1st and Dec 31st in 1998 comprise an additional group of students in treated schools. These students have the same school entrance age (i.e., 74 – 82 months) as the students who were born in the same months of 1999 but enrolled in the 8-year system. However, the students in the treated group attained one more year of education and were one year older when they took the test in 2009. This comparison is shown in Figure 3. We distinguish the effect of the policy on these two treated sub-groups using students' birth months. This study hereafter deems early entrants in the 9-year system as those who entered primary school one year earlier than the students in the previous system, and refers to normal entrants as those who entered at the same age as students in the previous system.

4. Data and Descriptive Statistics

4.1 Brazil Exam and school census

The two primary data sources that are used for this study are the Brazil Exam (Prova Brasil) and the school census. The Brazil Exam is a standardized test taken nationally by 4th grade (5th year) students and 8th grade (9th year) students in all public schools that have more than 20 students in the tested grades. To distinguish the old and new systems, this study uses the terminology "grades" for the old system and "years" for the new system. There existed a total of 8 grades in primary school before the reform, but there are now 9 years under the new system.

The Brazil Exam data set consists of two components: students' test scores and socio-economic data. The test covers Portuguese (reading comprehension) and Mathematics (problem solving), and reports raw scores on a scale from 0 to 500 and standardized scores for both tests (4th grade/5th year and 8th grade/9th year). Using a method based on Item Response Theory (IRT), which is a technique widely used for large-scale educational assessments, both types of scores are designed to be comparable across time using the same scale. We use standardized test scores in the main analysis for comparison with other studies.

In addition to test scores, grade repetition, dropout, and child labor, the data set also provides a rich set of students' socio-economic information, such as age, sex, race, parent's education, household appliance ownership and many other predetermined characteristics before the policy reform. Students also reported when they began their education, including daycare, preschool, or primary school. This was a key question to identify students with preschool education prior to school entry. Teachers and school principals are also required to fill out questionnaires so that basic information about school infrastructure and teacher qualifications are provided. The data is available every two years starting in 2007. The current study used five years of data that span the years from 2007 to 2015.

The Brazil Exam data set provides a valuable source of information to examine the impact of the compulsory schooling reform in Brazil; however, the timing of when a certain school changed from the 8- to the 9-year system is missing. Therefore, we connect the Brazil Exam data with school census data, which is available starting in 1999. The school census data is collected annually from approximately 250,000 schools, from daycare to high school, and includes information about teachers, students, and the quality of school infrastructure. To measure the timing of the policy adoption, two pieces of school census information are used: the indicator for the 8- or 9-year system and the enrollment in each grade in both systems.

Relying on either the system duration or enrollment could generate measurement error because schools sometimes misreport one of these values. For example, in Rio de Janeiro, the duration indicator variable in the school census suggested that most schools were under a 9-year system before 2003, whereas there was no enrollment recorded in the first year of the 9-year system. Therefore, we checked the consistency of these two pieces of information and excluded schools in the following situations: (a) there were no students enrolled in the first year following the adoption of the 9-year system, (b) there were no students enrolled among 5th year students five years after the policy adoption, and (c) there were no students enrolled among 9th year students nine years after policy adoption.

4.2 Initial observations and descriptive statistics

Figure 4 illustrates the share of schools that adopted the 9-year system in each municipality over time. It shows that most of the schools adopted the 9-year system by 2009, and that the policy was implemented gradually over time.

Table 1 presents summary statistics on student and school characteristics for 5th and 9th year students. Student and school characteristics are all indicator variables, but some of the school characteristics, such as classroom, piped water system, electricity, illumination, and ventilation, indicate not only its existence, but also its quality. These variables are assigned to one if the items are reported to have good quality, and zero otherwise. Columns (1) to (4) show the average characteristics of the schools for 5th year students in the base year, 2007, depending on the year that the 9-year system was adopted. Column (5) to (7) show the same information for the 9th year students. There are fewer columns for 9th year students because students in the schools that adopted the policy after 2007 have not yet entered their 9th year. Column (7) includes the 9th-year students in all of these schools.

Compliant schools for 5th year students, which adopted the new system before the mandated year (2010), share similar values in most of the student characteristics except for race composition (see columns 1-3). There exists little variation for school characteristics across these schools, but we tested whether any of these school characteristics significantly changed with the policy adoption. As will be shown in robustness checks (Table 8), we did not find such evidence. Unlike compliant schools, the schools that adopted the new system from 2010 onward show inferior characteristics in many dimensions. This indicates that these schools might not have adopted the new system on time due to a lack of resources or other constraints. We did not exclude these schools from the main analysis because this difference in levels does not seem very problematic for parallel trends, which will be discussed in the next section. We did, however, conduct robustness check of the results without these schools in a later section. Lastly, schools for 9th year students share similar characteristics in general regardless of the year of policy adoption.

There are two additional points that are worthy of mention. First, in the Student Characteristics section, the average age at exam in 2007 is at least 0.7 years higher than the appropriate age, (10 and 14 years old for 5th and 9th year students, respectively). Note that these values are all calculated using the base year value in 2007, which is the year in which none of the 5th-year students had begun their education under the 9-year system or taken the exam. Therefore, this higher average age is not due to the policy reform. Instead, the main reason is due to the high rates of grade repetition in Brazil. According to Table 1, more than 30% of 5th-year students in compliant schools exhibited grade repetition. Therefore, we also evaluated the impact of the school reform on the repetition rate, which is an important dimension of educational challenges in Brazil.

Another characteristic of schools, which is not shown in summary statistic, is that there are three types of primary schools: those providing only the first 5 years, the last 4 years, or the entire 9 years of education. In the case of schools providing only the second half of primary education, it is unknown whether students accumulated 4 or 5 years of education prior to enrollment. Although the government mandated that students choose schools under the same educational system when they transferred, we do not have the data to confirm whether this happened. Thus, for our analysis of 9th-year students, we included only municipalities in which all of the schools simultaneously adopted the 9-year system.[13] Therefore, we expect that 8 years after the policy was adopted, the majority of 9th-year students in these municipalities had begun their education under the new system, regardless of the types of primary schools they were currently enrolled in. Our approach did not significantly reduce the sample size of 9th-year students, and it provides a valid comparison of the policy effect between 5th and 9th-year students.

[13]This was the case for the majority of municipalities in Brazil. Approximately 10% of the municipalities showed some variation in the timing that their schools adopted the policy and consequently were excluded from the anlaysis.

5. Empirical estimation

The identification strategy used in this paper relies on the variation in the year that schools adopted the 9-year system. The variation enables the comparison between schools that adopted the new system and those still under the old system. The key assumption of this approach is that individual students in treated and control schools would have similar test score trends without the compulsory schooling reform.[14] To examine the overall impact of the compulsory schooling reform in the short-run (5th year students) and medium-run (9th year students), this study estimates the following two equations:

$$Y_{ist} = \alpha_0 + \alpha_1 treated_{st-j} + \theta_s + \mu_t + \rho X_{ist} + \tau^s_{st} + \varepsilon_{ist} \quad (j = 4, 8) \tag{1}$$

where Y_{ist} is the test score of student i in school s in year t. The main coefficients of interest is α_1 on the variables $treated_{st-j}$, which represent the impacts of the policy on 5_{th} year students ($treated_{st-4}$) and 9_{th} year students ($treated_{st-8}$). These treatment variables are lagged relative to the dependent variables by 4 and 8 years because the students who started primary school education under the new system would take 4 and 8 years to take the test. X_{ist} includes student specific characteristics, such as student's race, gender, age, working status, parents' education, ownership of TV and refrigerator as proxies for household wealth, preschool attendance, and having a single parent. Grade repetition and dropped out status could be affected by the policy reform, and as a result they are not used as control variables. A variable measuring age is also not used for the same reason.[15] Lastly, we also include school specific linear time trends, τ^s_{st}, to control for the possibility that schools with temporary negative shock might have been more likely to adopt the 9-year system, or that schools with low initial test scores might have been more likely to experience improved test

[14]We also assume that students who began their education under the new system completed their education in the same school or system.

[15]We also have information about school characteristics such as the quality of the classrooms, piped water system, electricity, light brightness, ventilation, and computers; the existence of a library, and policies to prevent violence/robbery. However, this information was not available for many schools every period. Therefore, we did not use this information in a main specification to construct a balanced panel of schools. Instead, we used the school characteristics for robustness checks.

scores over time. Standard errors are clustered at the school level for 5th-year students and at the municipality level for 9th-year students because the treated variable is assigned at the municipality level for 9th-year students.[16]

Another important focus of this study is the evaluation of how the policy impacted early entrants. Given that the school reform changed school entry age only for these students, we also estimate a model that separately captures the policy effect for early and normal entrants. Students' age at entry could not be used directly in the estimation because the decision was potentially endogenous. For example, more mature students who were born after the cutoff date might successfully petition to enter one year earlier. At the same time, less developed students might have postponed their school entry. Therefore, we used students' birth months to predict the school entry age after the reform.

The new cutoff date changed from Dec. 31 to the beginning of the school year, from February to April in most of the states.[17] As explained in section 3, because students born between the original and the new cutoff dates began primary education a year earlier, we use the following equation to compare the effect of the policy on early and normal entrants:

$$\begin{aligned} Y_{ist} = \delta_0 + \delta_1 treated_{st-j} + \delta_2 [bmonth < cutoff]_{ist} + \delta_3 treated_{st-j} * [bmonth < cutoff]_{ist} \\ + \theta_s + \mu_t + \rho X_{ist} + \tau^s_{st} + \varepsilon_{ist} \quad (j = 4, 8) \end{aligned} \tag{2}$$

where the birth month indicator, $[bmonth < cutoff]_{ist}$, represents whether a student has a birth month between Jan 1 and the new state-specific cutoff date. Note that δ_1 measures the effects of the policy on normal entrants. This captures the combined effects of an additional year of schooling and age at test. The additional effect of the policy on the students entering primary school a year earlier is shown by the coefficient δ_3 on the interaction term between the birth month indicator and the treatment variable. Since both early and normal entrants attained an additional year of

[16]Note that we also included fixed effects and linear time trends at the municipality level for 9th-year students.

[17]The start date of the school year in Brazil is different every year because of the Christian calendar and carnival. It moves around depending on when carnival is each year.

schooling, the coefficient captures the difference between the effect of age at test and the effect of school starting age.[18]

Lastly, we separately evaluated equation (1) based on preschool education status for each of the early and normal entrant samples. Note that in treated schools, early entrants with preschool education attained an additional year of schooling at the expense of preschool education, whereas early entrants without prior education attained an entire year of schooling at an earlier age and at no cost in terms of foregone preschool. Therefore, the comparison of the policy effects on these two groups can shed light on how the implications of lowering school entry age differs in contexts with universal versus limited access to preschool education.

It is less clear how preschool education may have impacted normal entrants. Unlike the early entrants, normal entrants who attended preschool did not attain an additional year of schooling at the expense of preschool education. Furthermore, all of the normal entrants attained an additional year of schooling and took the exam one year later, which provided additional maturity at the exam. It is possible that both age at test and an additional year of schooling dominate any differences in the treatment effect that may exist based on preschool education status.

6. Estimation Results

6.1 Parallel trends

Before examining the main results, we examine the parallel trends hypothesis and conduct event studies to check for possible test scores pre-trends. Figures 5 and 6 show the parallel trends of test scores for 5th and 9th year students by the years that schools adopted the policy. Schools are divided into groups based on the year that students who began their compulsory education under

[18] In the year of policy implementation, there is the possibility of introducing some measurement error by combining the oldest and youngest students in a treated group when we use the birth month indicator (i.e., students who were born between Jan 1st and Mar 31st in 1998 and 1999 in the example introduced in Figure 1). However, this is not a big concern for two reasons. First, the oldest students born in the year of policy implementation usually used the option to skip the first year of primary education, whereas those born later in the year had no such options. Also, the main results (Table 4) held even in a separate analysis that excluded the year of policy implementation.

the 9-year system took the exam, and each point represents the average test score of the year that the test scores were available.[19]

Figure 5 shows that each group of schools exhibited an increase in test scores exactly when students who started their education under the 9-year system took the exam. However, this test score gap disappeared as other schools also have the treated students taking the exam 4 years after the policy implementation. The only difference is reflected in the timing of the test scores increase, but there is a convergence of test scores over time. These patterns are observed regardless of the tested subject, with the left Figure showing Portuguese scores and the right Figure showing math scores. Except for the schools that adopted the policy in 2010 and 2011,[20] which was later than the deadline set by the government, the level of test scores are also very similar across schools. Inclusion of the schools that adopted the policy after 2010 did not change the estimation results, as will be shown in the section on robustness checks.

Figure 6 shows the results for 9th year students. We have a smaller number of treated groups because there are some schools with students who began their education under the new system but had not yet taken the exam. Given that treated students only take the exam 8 years after the new system is adopted, the first treated group is in 2012/13. Before the treatment, we observe reasonably parallel trends of the test scores over time periods. Another notable pattern in Figure 6 is that the policy effect seems to be observed only in schools that were treated in 2015, not in the schools treated in 2013. However, we found that the estimates for the medium-run effect of the policy change only slightly even without the schools treated in 2013. This result is shown in robustness checks (Table 10).

6.2 Event Study

Another way of testing parallel trends is to assign pre-and post-policy implementation periods for every treated schools and use an event study specification. We did this with the following

[19] Conventional parallel trends showing patterns of pre-test scores before 2009 cannot be constructed because the data is only available from 2007.

[20] Treated students in these schools took the exam in 2015.

specifications:

$$Y_{ist} = \alpha + \sum_{k=-3}^{2} \beta_k E_{sk} + \theta_s + \mu_t + \rho X_{ist} + \varepsilon_{ist} \quad (5th\ year\ student) \tag{3}$$

$$Y_{ist} = \alpha + \sum_{k=-4}^{0} \beta_k E_{mk} + \theta_m + \mu_t + \rho X_{imt} + \varepsilon_{imt} \quad (9th\ year\ student) \tag{4}$$

Individual student characteristics, X_{ist}, school (municipality) specific time trends, $\theta_s(\theta_m)$ and μ_t, were used as in the main specification. The coefficients of interest are β_k, where E_{sk} represents the indicator for each school's pre- and post-treatment periods (k). Given that each school's test scores were available every two years, we combined two policy-adoption years and assigned one indicator dummy for pre- and post-implementation periods. For example, schools in one and two years prior to policy implementation were pooled together and assigned one for the indicator of one-year pre-period (E_{s-1}). Had we not done so, we would have had observations for completely different sets of schools every year. Given this structure, treatment began when the event time indicator became zero (E_{s0}), as this is when 5th-and 9th- year students who started their primary education under the 9-year system first took the Brazil Exam. The indicator for one-year pre-period (E_{s-1}) was dropped in the event-study specification, and therefore each event-time indicator was identified from the comparison with the omitted time period.[21]

Figure 7 shows the event study results for 5th-year students. In pre-periods, Portuguese test scores showed a slight increase, but in general there were no significant pre-trends in either subject. The test scores in both subjects exhibited a substantial and statistically significant increase when the event-time indicator became zero (E_{s0}), as this is the first year that the 5th-year students who started their education under the 9-year system took the exam. Figure 8 shows the results for 9th-year students. There were relatively longer pre-periods, but a shorter post-period for 9th-year students as it took 8 years to see the effect of the policy on these students. The results show that unlike the outcome among 5th-year students, there was only a very small impact of the policy

[21] Unfortunately, we cannot have the same set of the schools to identify each event-time indicator because every school had five years of test scores with different policy adoption years.

when 9th-year students who began their education under the 9-year system took the exam (E_{s0}). This outcome is also confirmed in the next sections. The policy effect among 9th-year students faded out in the medium-run.

6.3 Main results

Given that the parallel trends are satisfied, we start the analysis in Table 2 with results that represent the overall effect of compulsory schooling reform on test scores of 5th and 9th year students (Eq.1). The "overall" impact of compulsory schooling reform analyzes the reform package as a whole, and thus ignores differences in school entry age. The main coefficient of interest is the coefficient on the variable "Treated". This dummy variable equals one when students who began their primary education under the 9-year system took the exam in their 5th and 9th years.

The results for the test scores of 5th-year students indicate that the overall policy reform had a large and significant effect on both math and Portuguese test scores. Portuguese and math test scores increased by approximately 0.08 and 0.10 standard deviations, respectively. These results are robust to the inclusion of control variables representing students' socioeconomic status. When school (municipality) specific time trends were included in the preferred specification (Columns 3 and 6), the policy effect for both subjects increased by about an additional 0.01 in each subject. The preferred specifications show increases of 0.09 and 0.11 in Portuguese and math.

Despite the substantial increase of test scores in the short run, the policy impact faded in the medium run. After 9 years, the gain in Portuguese and Math scores decreased to approximately 0.03 and 0.02 standard deviations in the first two specifications, which we estimated without school specific time trends. When school (municipality) specific time trends are included, the policy effect on test scores increased slightly for math test scores. Considering all the results for 9th year students, we conclude that the policy generated a smaller--but still statistically significant--effect on students' academic performance in the medium run.

6.4 Effects on other outcomes

One of the concerns that arose during the schooling reform was that the policy may exacerbate grade repetition or dropout rates by enrolling students who were not prepared for primary school education. Such outcomes are not only costly for the individual students, but also for schools as they incur these costs when they have to take additional students and use extra inputs (Koppensteiner (2014)). Therefore, the potential benefits of the reform also depend on the effect that it has on these outcomes. In addition, we examined the impact of the policy on child labor, which is quite common in Brazil.[22] Several studies have shown its negative impact on Brazilian students' academic outcomes and earning (Emerson and Souza (2011), Emerson et al. (2017)). Because investment in human capital is often considered an alternative option to child labor, and early childhood investment could be complementary to those in later life (Cunha and Heckman (2007)), we expect that child labor might decrease after the reform. Table 3 presents the results for the effect of schooling reform on these outcomes.

About 26% of the 5th-year students in the sample had repeated their grade at least once in the base year (2007) prior to when any of the treated students took the exam. Column (1) shows that in the short run the policy decreased grade repetition by a significant amount for 5th year students. The coefficient on the treated variable indicates that the policy decreased the repetition rate by about 4 percentage points, which is close to a 15% reduction from the base value. The dropout rate and child labor in columns (2) and (3) also decreased by 0.8 (13%) and 0.5 (3%) percentage points, respectively.[23] The estimate for child labor indicates that although statistically significant, the compulsory schooling reform had only a very modest effect on child labor.

In the medium run, the policy still had an impact on the grade repetition rate, but did not affect dropout and child labor. In 2007, about 31% of the 9th-year students had repeated their grade at least once during school years. After the policy was implemented, grade repetition was decreased

[22] According to Table 1, about 16 % of 5th year and 25% of 9th year students were engaged in child labor in 2007.

[23] The effect on dropout rates is comparable to the effect of another program implemented in Brazil. For example, Glewwe and Kassouf (2012) showed that Brazil's Bolsa Escola/Familia program, which provided families monetary incentives for their children's school enrollments, decreased dropout rates by approximately 0.5 percentage points for students in grades 1-4.

by 2.3 percentage points, which is approximately an 8% decrease from the base value. This result indicates that the policy had a persistent effect on the grade repetition rate. The estimates for dropout and child labor, in contrast, are close to zero and not statistically significant. Given that grade repetition is prevalent in developing countries and incurs costs for both schools and students, it is important to note that the school reform had a significant effect on this variable.

6.5 Heterogeneous effects

Heterogeneous effects of school entry age and the length of education

The previous sections evaluated the overall impact of the compulsory schooling reform. They did not shed light on whether the effects were mainly driven by students who began primary education at age 6 or age 7. In this section we distinguish the effect of the policy based on school entry age. As described in section 5, we use students' birth months to differentiate between early and normal entrants.

Table 4 reports the effect of compulsory school reform on early and normal entrants. The variable, Early age, refers to the birth month indicator in equation 2. The main coefficient of interest is the coefficient on the interaction term; it represents the additional effect of the school reform on early entrants. In the short run, 5th-year students who started their primary education one year earlier and attained one more year of primary education gained a smaller increase in Portuguese test scores—by about 0.055 standard deviations—compared to the students who also had one more year of education but took the exam when they were one year older. For mathematics test scores, a similar outcome was observed. Compared to the coefficients reported in Table 2, which measured the overall impact of the policy, normal entrants had slightly higher test score increases while early entrants had test score gains that were about half the size. In the medium-term (after 9 years), the early entrants again experienced a smaller increase in test scores. While early entrants experienced a 0.031 standard deviations smaller increase in Portuguese test scores, the gain was 0.024 standard deviations smaller for mathematics scores.[24]

[24]There is a possibility that some municipal governments enforced their own cutoff dates instead of the state-specific

We observe that the policy still has positive effects on early entrants' test scores, but the effect is smaller than for the normal entrants. This could be explained by the different mechanisms through which the policy affected the students. First, normal entrants took the exam a year later (at age 11 or 15) than the students in the previous system because the new school system has a longer curriculum regardless of school entry age. On the other hand, early entrants took the exam at the same time as students in the previous system, but started their primary education one year earlier. Therefore, age at test seems to have a much larger effect on test scores than school entry age. In addition, the early entrants who were enrolled in preschool previously attained an additional year of schooling at the expense of their last year of preschool education whereas the normal entrants who attended preschool started their primary education after completing their entire preschool education. For this reason, the policy may have had a larger impact on the test scores of normal entrants. Nonetheless, early entrants gained a significant amount in the short run and, although smaller, the medium-term effects were still significant. This suggests that having early exposure to formal education can have a positive effect on students' academic performance.

Heterogeneous effects by school entry age and preschool experience

Tables 5 and 6 show the extent to which the policy impacts differed between early and normal entrants who did and did not receive a preschool education. Because the policy also had a significant impact on rates of grade repetition (as shown in Table 3), we examined test scores as well as grade repetition rates as the outcome variables. Table 5 focuses on the early entrants. Columns (1) and (2) indicate that the increases in test scores among early entrants without preschool education were approximately one-third to two-thirds larger in the short-run than those of early entrants who attended preschool. The difference is particularly pronounced for Portuguese test scores because

cutoff dates. Also, there were a few cases where the state legislation did not specifically mention the exact cutoff dates, and these might have chosen dates other than March 1st. We test the sensitivity of our results by only using the students born in January and February. Given that municipalities or states that might have chosen alternative dates were much more likely to set the cutoff date beyond March 1st, the students born in these two months should always be a valid comparison groups to analyze the impact of lowering school entry age and increasing the duration accordingly. The results in (Table 4) changed slightly when we used only the students born in January and February, but early entrants still experienced a smaller increase in test scores in both subjects.

students learn the basic materials of Portuguese during the last year of preschool. The policy also had a substantially larger effect on the rate of grade repetition among early entrants without preschool education. The grade repetition rates were reduced by approximately 17% for this group, whereas the decrease was only 8% among students who had attended preschool.

Despite the significant short-term effects for early entrants without preschool education, we observe no significant effects of early exposure to primary education in the medium run. Most estimates are close to zero and statistically insignificant. In contrast, those with preschool education still exhibited a statistically significant increase in both test scores and decrease in grade repetition rates. While it is beyond the scope of this study to determine the exact cause of these results, it is important to note that the reform did not replace the entire 3 years of preschool education for those who attended preschool. Instead, the reform replaced only the last year of preschool education with the first year of compulsory education. Therefore, it is possible that the additional two years of preschool education at an earlier age generated the difference in educational outcomes at later stages.[25]

Lastly, Table 6 shows the extent to which the policy effects differed between normal entrants who did and did not attend preschool. Unlike the results in Table 5, the short run policy impacts among normal entrants were not substantially different based on their preschool education. Mathematics test scores increased a bit more among normal entrants without preschool education, and the increase in Portuguese scores was slightly larger among those with preschool education. In the medium run, the increase in test scores for both subjects was higher among normal entrants without preschool education. In addition, grade repetition rates decreased by a similar magnitude in both groups. In the case of normal entrants, it is likely that the additional year of schooling and the effect of being one year older at the time of the test exert a dominant effect in both groups. Therefore, we observe smaller differences in the effect of the policy depending on preschool education.

[25] Subha Mani (2012) also demonstrated the importance of earlier schooling investment on later outcomes in rural Ethiopia. They found that school enrollment and grade repetition in later periods were significantly affected by the schooling investment made in earlier stages.

6.6 Robustness checks

In this section, we perform several robustness checks to show that the results of the study were not driven by other factors such as school-specific time varying unobservables, other school investments, changes in student composition, or the inclusion of certain types of schools. Overall, we did not find evidence that our results were affected by such factors.

Placebo test for time-varying unobservables

In this section, we conduct a test to check whether any time varying unobservable school-level factors might threaten the validity of the empirical strategy. To do so, we first picked the schools that have both primary and middle school education, and then erroneously assigned the treatment variable for 5th year students to 9th year students. Note that the correct treatment variable for 9th year students has to be lagged for four additional years compared to the 5th year treatment variable. The idea is that if there are unobservable factors that affect student's test scores over time in treated schools differently than in the non-treated schools, these unobservables could affect both 5th year and 9th year student's test scores in treated schools even before the policy actually had time to have an impact. Schools in which 9th year students were treated in 2013 or 2015 are not included in this test.

Table 7 shows the results of the placebo test. Compared to the estimates for 9th-year students, which were reported in Table 2, we found smaller and statistically insignificant estimates for both Portuguese and math subjects. The estimates for both subjects become very close to zero. If the results of 9th year students were driven by unobservable time varying factors at the school level, we might observe a policy impact here. Both in terms of statistical significance and magnitude, the existence of such a factor is not supported.

Change of school characteristics

Given that the Brazilian government increased investments in education and implemented a number of policy changes in recent decades, it is possible that the impact of the compulsory schooling reform is overestimated by other investments or reforms. One way to check this possibility is to examine whether other school characteristics changed at the same time as the compulsory schooling reform. A related concern might be that class sizes could have changed following policy implementation. In the first year of policy implementation, class sizes could be bigger than in other years as there are additional younger students enrolled with the regular cohort. We expect this one-time effect of cohort size to be minimized since we measure the average effect over time, nonetheless it is important to examine. The school census and Brazil exam data provide information on school infrastructure, frequency with which teachers check their student's homework, and average class sizes. Using these variables as outcomes, we ran the main regression for both 5th and 9th year students at the school level. Table 8 reports the results.

Overall, we do not find evidence that school characteristics change significantly with the policy adoption. For 5th year students, the change in classroom quality is statistically significant, but negative, which runs against finding a positive effect of the policy. In addition, the magnitudes of these changes from the base values were quite small. For example, the policy adoption decreased the probability that schools report that they have good quality classrooms by 1.9 percentage points, but this is a negligible change considering the mean value of 60% in the base year (2007). We find similar results for 9th-year students. The change of some school characteristics, such as ventilation quality, or the tendency that teachers check Portuguese homework regularly are statistically significant, but either the signs are negative or the magnitudes of the changes from the base values were very small.

Change of student characteristics

If the students who are more motivated or have more involved parents self-select into schools in the 9-year system, the policy impact might have been overestimated. This is less likely to happen in the current study because schools in the same municipality tended to adopt the 9-year system simultaneously, and students in Brazil usually attend schools that are closest to their residence.[26]

[26]Under this circumstance, parents had to change the municipality of residence to self-select into schools in the 9-year system, which is much less likely to happen.

Nonetheless, we examined whether student characteristics, such as race, gender, the ownership of household appliances that potentially represent household wealth, or students' parental characteristics changed with the policy adoption.

Table 9 shows whether student characteristics changed with the policy adoption. Among 5th-year students, several student characteristics changed by a small but statistically significant amount. For example, among 5th-year students, less white and more female students tend to enter the schools under the 9-year system. However, the magnitudes of these changes are quite small considering the mean value in the base year (2007). For example, approximately 49% of the students were female in 2007. The policy increased the enrollment of female students by one percentage point, but this is only a 2% increase in relation to the base year. In addition, the signs of many estimates that show statistically significant changes are negative, which is not what we would expect if these were the variables contributing to a positive effect of school reform. Among 9th-year students, we have a smaller number of student characteristics that show statistically significant changes. But again, the magnitudes of all these changes are small relative to their base values.

Overall policy impact with restricted samples

This section tests the overall policy impacts with a number of restricted samples. First, schools that adopted the policy too early or too late could possibly have different unobservable characteristics compared to the schools that adopted the policy on time. To address this issue, we first dropped the schools that adopted the policy before 2006 when the Brazilian government implemented the policy. Next, we dropped the schools for 5th year students if they adopted the policy after 2009. As shown in the summary statistics, schools that adopted the policy after 2009 generally had worse characteristics than those that adopted it before 2009. Finally, we use the same restricted sample that we used previously to conduct a placebo test (i.e., schools that provided the entire 9 years of primary school education). This differs from the main analysis that also includes the schools that provided only the first 4 (old) or 5 (new) years of primary school education, or schools that had only the last 4 years of education.

Table 10 shows the results for each restricted sample. The magnitudes of all policy impacts are quite similar to those reported in Table 2. The estimates vary slightly in each restricted sample, but the differences are in the range of 0.01 to 0.03 standard deviations. Thus, the main results of the current study were not driven by certain type of schools.[27]

7. Conclusion

Altering the starting age of compulsory schooling in a way that potentially changes its duration has attracted the attention of policy makers, researchers, and parents due to its implications for the educational system and student' development. Despite the importance of this topic, there have been very few opportunities to evaluate an actual policy that mandated changes in school entry age and duration. This study examines the impact of changes in these factors through Brazil's 2006 compulsory schooling reform, which lowered the minimum age of entry to primary school and increased the duration of compulsory education by one year.

We found strong and robust evidence that the compulsory schooling reform in Brazil influenced academic performance. In the short run (5 years), the overall impact of the reform was a 0.09 SD increase in Portuguese test scores and a 0.11 SD increase in mathematics test scores. These benefits persist in the medium run (after 9 years) with a smaller magnitude. In addition, the policy reduced grade repetition both in the short run and medium run. Considering that grade repetition is an important issue in many developing countries, this finding is quite encouraging.

Furthermore, this study used students' birth months to identify the students who began their primary education a year earlier and attained an additional year of schooling compared to the students enrolled in schools under the previous system. The impact of schooling reform on these students was still positive, but smaller than the overall policy effect because age at test exerted a stronger influence than school entry age, and because most of the early-entrant students attained an additional year of schooling at the expense of their last year of preschool education. We also found

[27] we observed an increase of the policy effect in the third restricted sample. This is because schools that provide the entire 9 years of education are governed mostly by state governments, which consists of approximately 30% of the primary schools for 5th-year students. These schools tended to experience a larger increase in test scores through the reform.

that the policy of lowering school entry age and increasing the duration accordingly had significant short-term effects on both math and Portuguese test scores among early entrants without preschool education, although the effects faded out in the medium run. Those who had attended preschool experienced a smaller increase in their short-term test scores, but the effect was more persistent in the 9th year of school. This finding has important policy implications: earlier exposure to compulsory education could be an effective tool for enhancing learning outcomes not only in developing countries, where students have limited access to preschool education, but also in countries where preschool education is more prevalent.

Test scores are surely one of the important measures to assess the impact of compulsory schooling reforms. However, non-cognitive outcomes, such as mental health or social behavior are also important elements for child development. This study, unfortunately, does not have information on these outcomes. A recent study by Dee and Sievertsen (2016) shows that delaying school entry age can reduce inattention or hyperactivity. Therefore, without additional information of this type, we are unable to conclude that the reform had positive effects on students in all dimensions. Future research should examine the effect of this policy on long-run outcomes, such as college entrance rate or labor market outcomes. Non-cognitive outcomes, such as the crime rate, should also be evaluated.

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Figure 1: The example of policy implementation

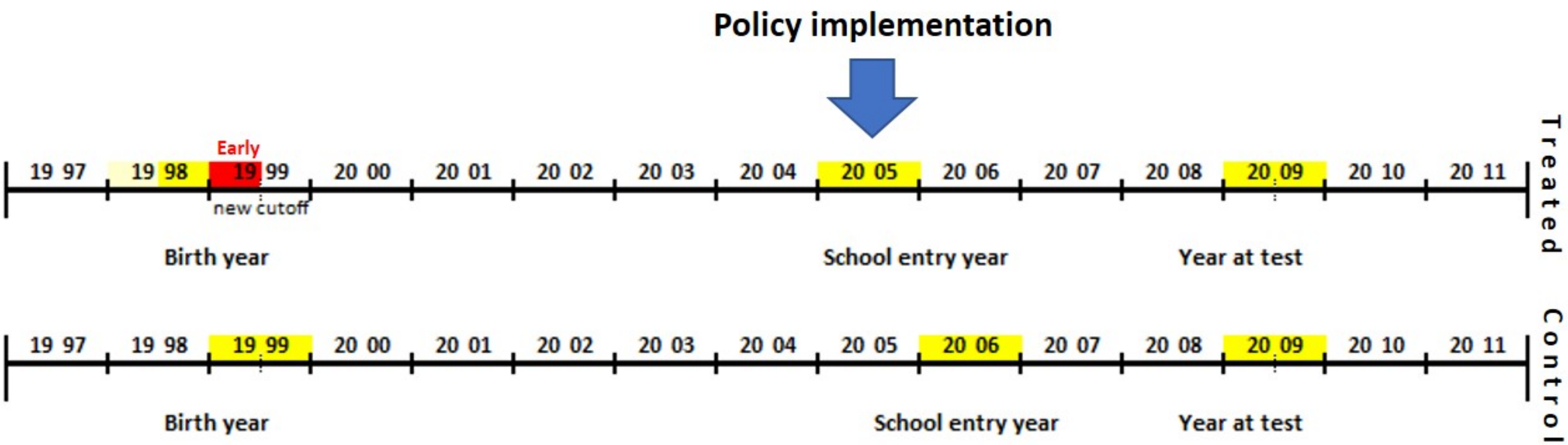


Figure 2: The comparison groups for early exposure to compulsory education (Early Entrants)

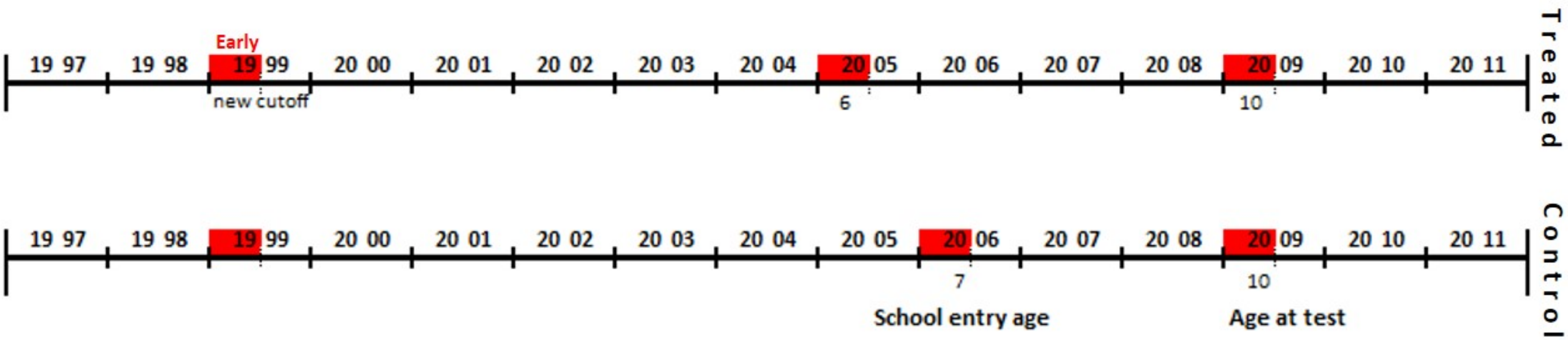


Figure 3: The comparison groups for an additional year of schooling and being one year older at test (Normal Entrants)

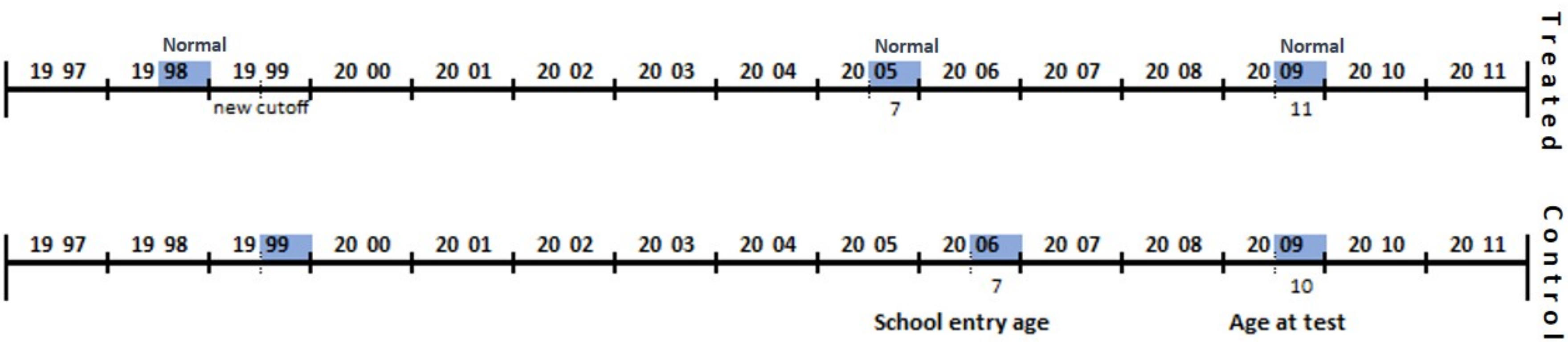
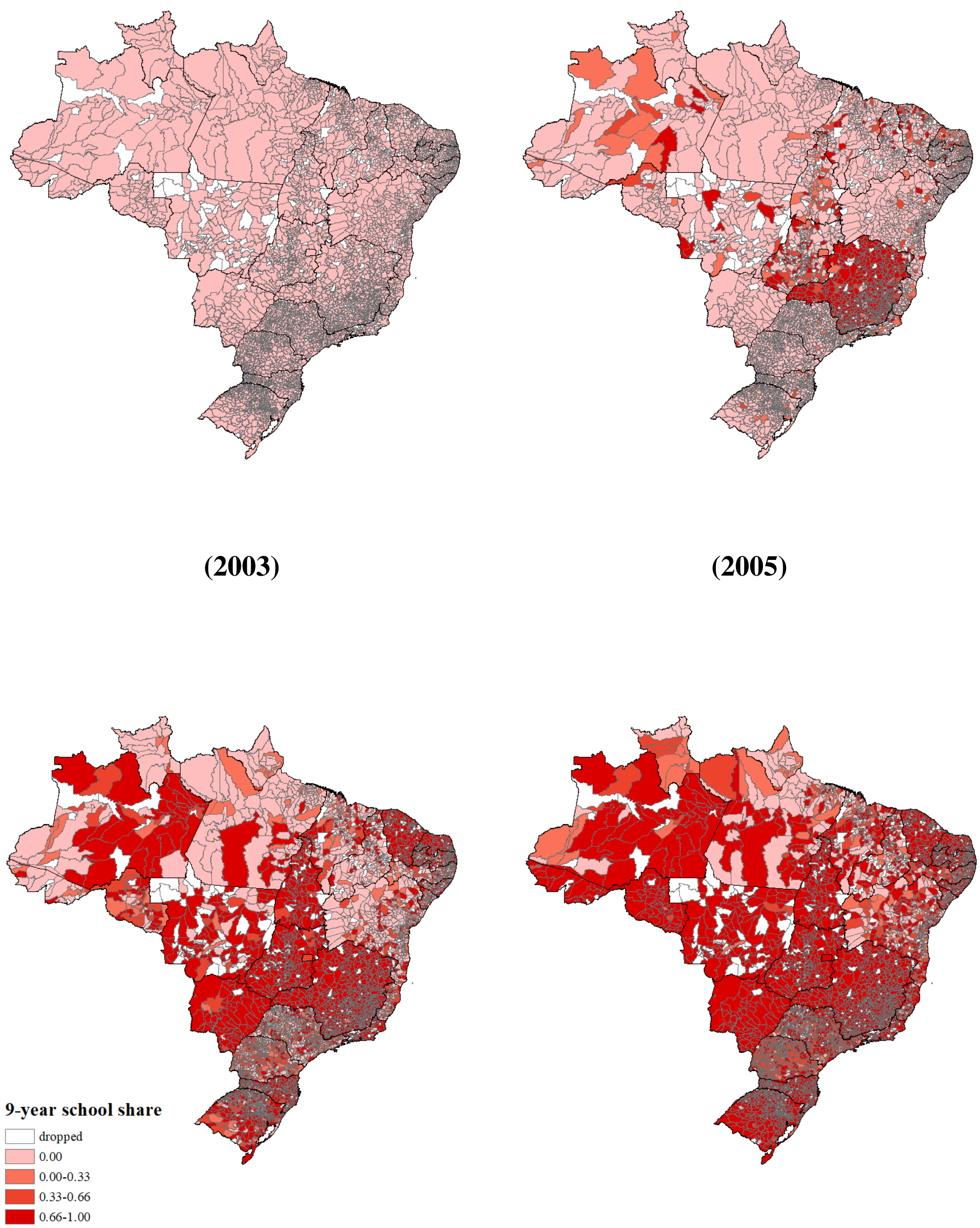


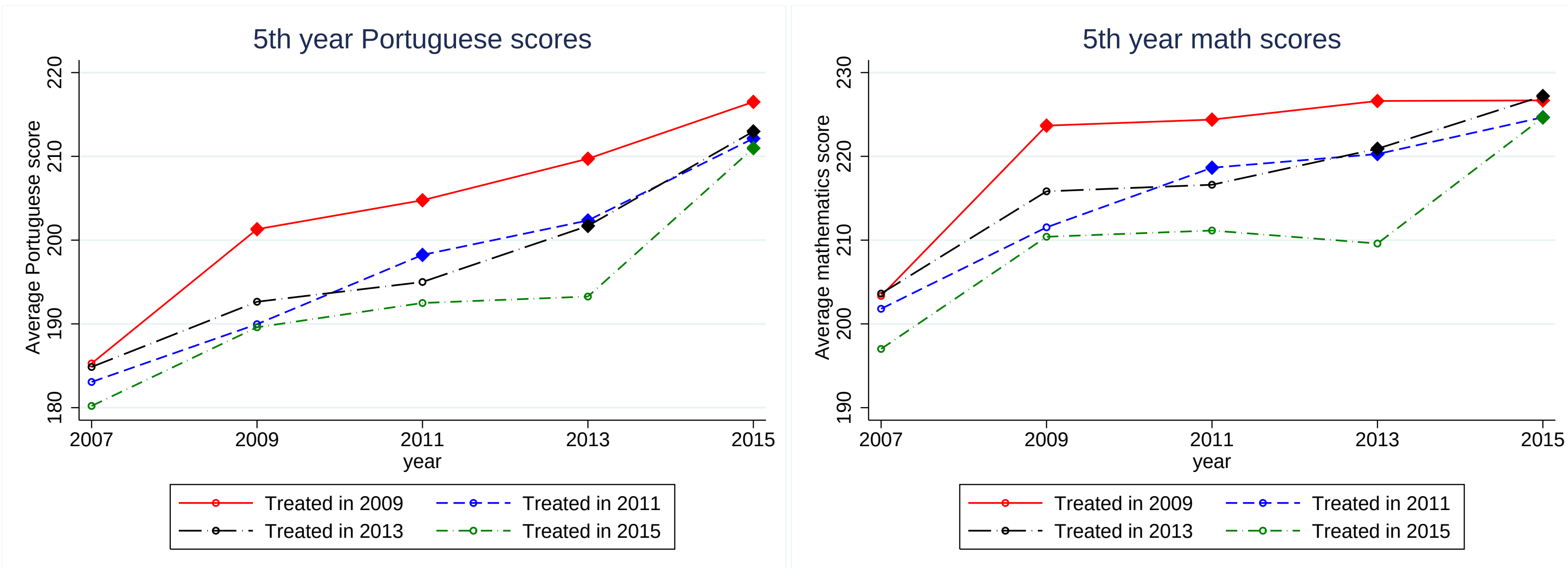
Figure 4: Share of schools with the 9-year system from 2003 to 2009



(2007) (2009)

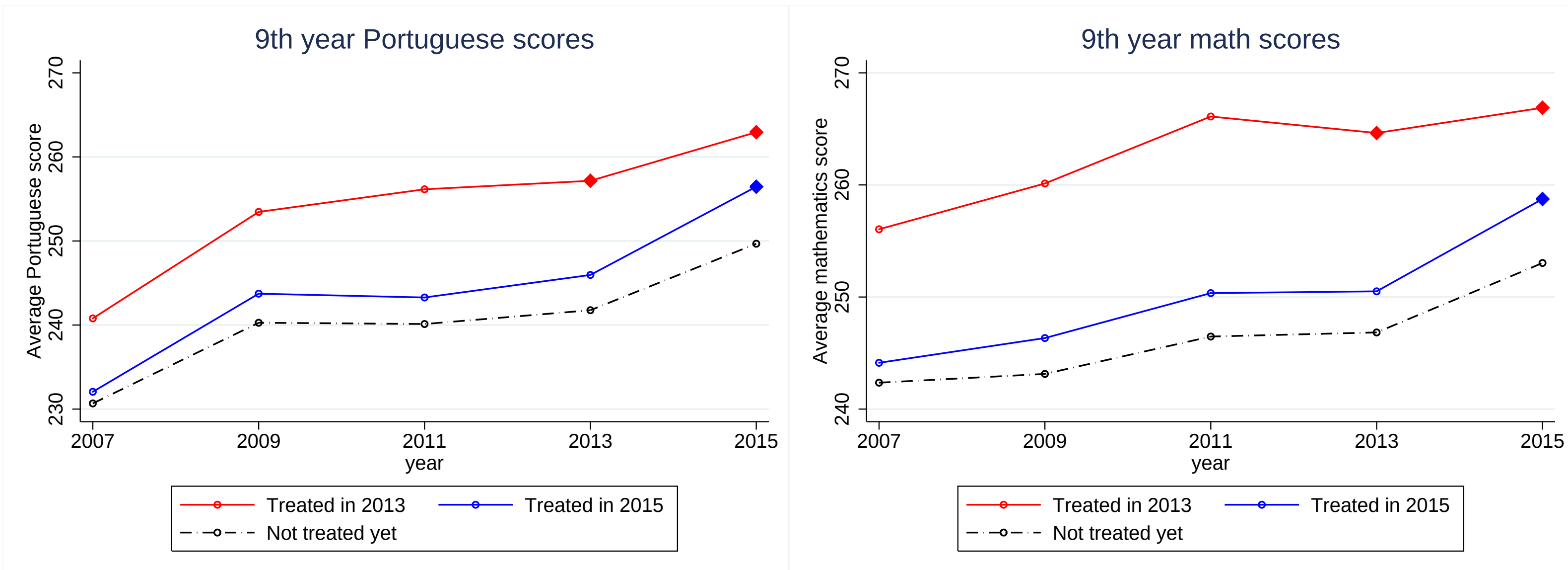
Notes: Municipalities shown in white are excluded in the main analysis because every school in these municipalities had inconsistency between the school duration indicator and enrollment in grade/year.

Figure 5: Parallel trend of test scores (5th year students)



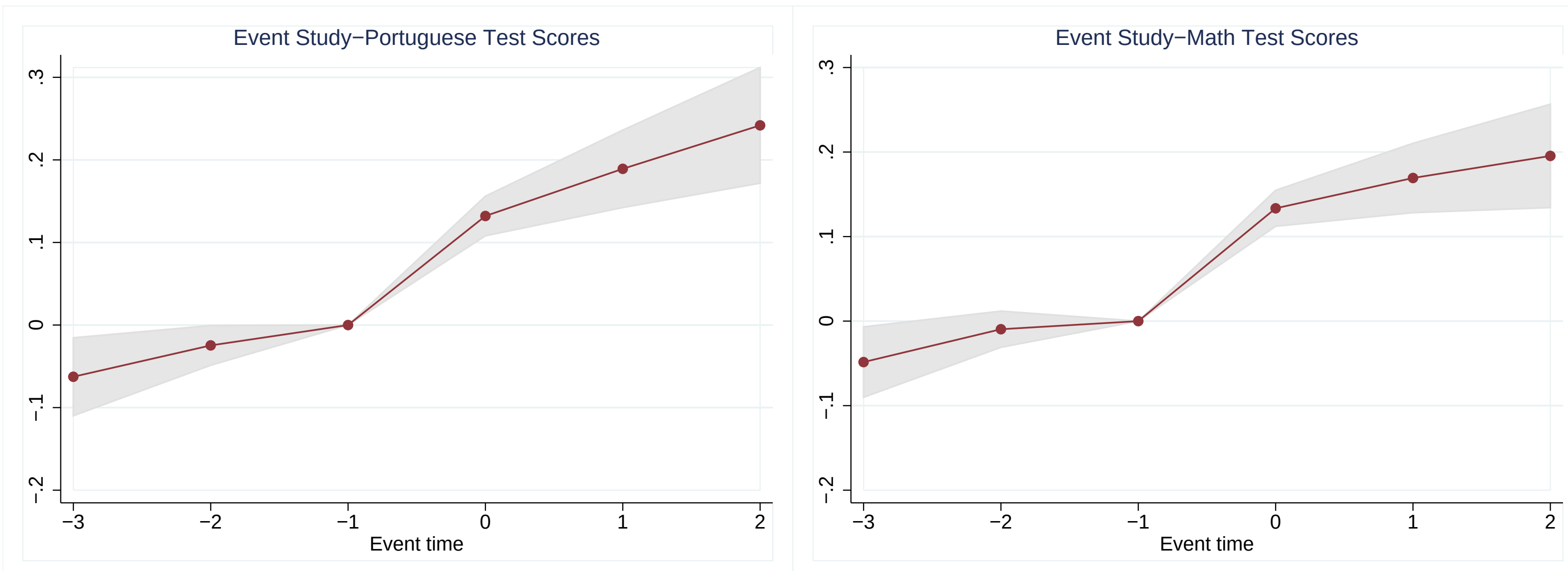
Notes: Schools are divided into groups based on the year that students under the 9-year system took the exam. For example, students who entered primary schools under the 9-year system in 2004/2005 took the 5th year exam in 2009. These groups are denoted as "treated in 2009." Test score data is not available in even numbered years. Each point represents the average test score of the year that the test scores were available. Dots indicate pre-treatment scores, and diamonds indicate post-treatment scores.

Figure 6: parallel trend of test scores (9th year students)



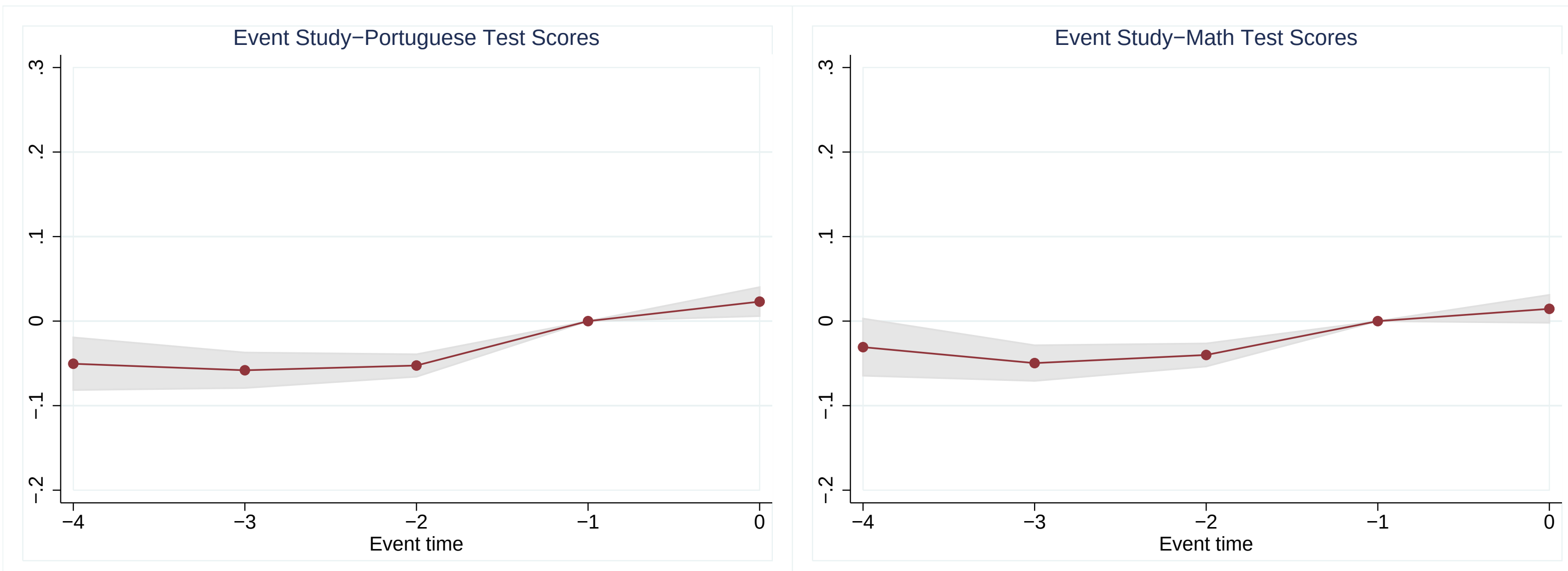
Notes: Schools are divided into groups based on the year that students under the 9-year system took the exam. For example, students who entered primary schools under the 9-year system in 2004/2005 took the 9th year exam in 2013. These groups are denoted as "treated in 2013." Test score data is not available in even numbered years. Each point represents the average test score of the year that the test scores were available. Dots indicate pre-treatment scores, and diamonds indicate post-treatment scores.

Figure 7: Event study results - test scores for 5th year students



Notes: The grey shaded area represents the 95% confidence interval. Standard errors are clustered at the school level.

Figure 8: Event study results - test scores for 9th year students



Notes: The grey shaded area represents the 95% confidence interval. Standard errors are clustered at the school level.

Table 1: Baseline summary statistics for 5th and 9th year students by year of adoption

		5th year students				9th year students		
		2004&2005	2006&2007	2008&2009	2010&2011	2004&2005	2006&2007	Pending
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of schools	2231	4179	2854	1512	2367	2005	944
	Number of municipalities	623	1400	878	377	759	767	501
Student characteristics								
	White	0.32	0.40	0.38	0.29	0.35	0.37	0.36
	Female	0.49	0.49	0.50	0.50	0.56	0.56	0.56
	Color TV	0.95	0.94	0.95	0.93	0.95	0.95	0.93
	Refrigerator	0.91	0.92	0.92	0.86	0.93	0.92	0.89
	Repeat grade	0.31	0.33	0.32	0.42	0.33	0.36	0.34
	Drop out	0.07	0.07	0.07	0.10	0.07	0.08	0.08
	No preschool education	0.18	0.20	0.19	0.23	0.19	0.21	0.22
	Mother w/o primary edu	0.51	0.53	0.53	0.54	0.62	0.59	0.61
	Father w/o primary edu	0.54	0.54	0.54	0.56	0.66	0.64	0.65
	Work	0.16	0.16	0.16	0.19	0.27	0.25	0.25
	Average age at exam	10.70	10.71	10.75	11.17	14.96	15.00	15.13
	Single parent	0.23	0.20	0.21	0.25	0.19	0.20	0.21
School characteristics								
	Classroom quality	0.58	0.61	0.61	0.46	0.53	0.57	0.53
	Piped water system	0.42	0.49	0.48	0.36	0.39	0.42	0.42
	Electricity quality	0.46	0.53	0.53	0.40	0.41	0.45	0.43
	Illumination quality	0.87	0.87	0.89	0.80	0.86	0.88	0.86
	Ventilation quality	0.81	0.78	0.79	0.70	0.83	0.79	0.77
	Policy exists for violence	0.20	0.25	0.34	0.18	0.24	0.26	0.29
	Number of computers	9.49	10.12	10.78	10.19	11.74	11.11	10.80
	Internet access	0.62	0.61	0.60	0.33	0.71	0.66	0.64
	Library	0.74	0.60	0.55	0.36	0.90	0.79	0.71
	Teacher frequently checks Portuguese homework	0.83	0.83	0.83	0.81	0.88	0.86	0.84
	Teacher frequently checks math homework	0.85	0.85	0.85	0.82	0.89	0.88	0.86
	Teacher-student ratio	23.67	24.23	26.12	27.31	20.64	22.71	22.59
	Class size	26.09	25.29	26.63	27.67	28.59	28.20	30.41

Notes: The table shows average baseline characteristics of the schools depending on the year that the policy was adopted. The columns are first divided by schools for 5th- and 9th-year students. In the next row, schools are divided further based on policy adoption year. For 5th-year students, there are about 46 schools that adopted the policy after 2011. They were excluded from this analysis. For 9th-year students, schools that adopted the policy after 2007 are all aggregated under the column of 'pending' because their students have not yet taken the exam.

Table 2: The overall effect of compulsory schooling reform on standardized test scores

	Portuguese (1)	Portuguese (2)	Portuguese (3)	Math (4)	Math (5)	Math (6)
Panel A: 5th year students						
Treated	0.086***	0.082***	0.095***	0.101***	0.100***	0.112***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	1,196,726	1,196,726	1,196,726	1,196,726	1,196,726	1,196,726
Panel B: 9th year students						
Treated	0.031***	0.034***	0.031***	0.019**	0.021**	0.040***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
N	1,043,128	1,043,128	1,043,128	1,043,128	1,043,128	1,043,128
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	No	Yes	Yes
School (municipality) specific time trend	No	No	Yes	No	No	Yes

Notes: Year fixed effects for exam years and school(municipality) fixed effects are included in every specificication. Second specifications for each test add control variables for student characteristics, such as household appliance ownership, parent's education, sex, race, etc.. The last specifications for each test add school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01. Test scores are all standardized in a comparable scale over time using Item Response Theory.

Table 3: The overall effect of compulsory schooling reform on grade repetition, dropout, and child labor

	5th year students			9th year students		
	Grade Repetition	Dropout	Work	Grade Repetition	Dropout	Work
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.040***	-0.008***	-0.005***	-0.023***	-0.001	0.002
	(0.002)	(0.001)	(0.001)	(0.005)	(0.002)	(0.003)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
School (municipality) specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Share in base year	0.26	0.06	0.13	0.31	0.07	0.23
N	1,196,726	1,196,726	1,196,726	1,043,128	1,043,128	1,043,128

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 4: The effect of the compulsory schooling reform on the test scores of early and normal entrants

	5th year students		9th year students	
	Portuguese (1)	Math (2)	Portuguese (3)	Math (4)
Treated*Early age	-0.055***	-0.056***	-0.031***	-0.024***
	(0.003)	(0.003)	(0.005)	(0.005)
Treated	0.115***	0.136***	0.042***	0.048***
	(0.004)	(0.005)	(0.009)	(0.009)
Early age	0.024***	0.026***	0.012***	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
School (municipality) specific time trend	Yes	Yes	Yes	Yes
N	1,038,406	1,038,406	1,042,837	1,042,837

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 5: Heterogeneous effect of the reform on the test scores of early entrants

	5th year students			9th year students		
	Portuguese	Math	Grade Repetition	Portuguese	Math	Grade Repetition
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without preschool						
Treated	0.094***	0.095***	-0.065***	-0.002	0.027	-0.007
	(0.018)	(0.018)	(0.011)	(0.024)	(0.023)	(0.012)
N	47,797	47,797	47,797	50,318	50,318	50,318
Share in base year			0.37			0.43
Panel B: With preschool						
Treated	0.049***	0.071***	-0.017***	0.027**	0.028**	-0.021***
	(0.007)	(0.007)	(0.003)	(0.011)	(0.012)	(0.005)
N	254,721	254,721	254,721	305,002	305,002	305,002
Share in base year			0.22			0.27
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
School (municipality) specific time trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 6: Heterogeneous effect of the reform on the test scores of normal entrants

	5th year students			9th year students		
	Portuguese	Math	Grade Repetition	Portuguese	Math	Grade Repetition
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without preschool						
Treated	0.105***	0.140***	-0.051***	0.051***	0.058***	-0.032***
	(0.01)	(0.01)	(0.01)	(0.016)	(0.017)	(0.011)
N	123,091	123,091	123,091	112,541	112,541	112,541
Share in base year			0.39			0.43
Panel B: With preschool						
Treated	0.116***	0.131***	-0.037***	0.029***	0.041***	-0.022***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.006)
N	582,307	582,307	582,307	570,463	570,463	570,463
Share in base year			0.24			0.29
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
School (municipality) specific time trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 7: Placebo test based on false treatement years (9th year)

	Portuguese (1)	Math (2)
Treated	0.007	0.014
	(0.016)	(0.014)
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School (municipality) specific time trend	Yes	Yes
N	241,597	241,597

Notes: Year fixed effects for exam years and school(municipality) fixed effects are included in every specificication (baseline specification). The second specifications for each test adds control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.). The last specifications for each test add school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01. Test scores are all standardized in a comparable scale over time using Item Response Theory.

Table 8: Test of change in school characteristics with the policy adoption

	5th year students	9th year students
Piped water system	-0.007	0.012
	(0.009)	(0.017)
(share)	0.46	0.41
Electricity quality	0.006	-0.023
	(0.01)	(0.018)
(share)	0.49	0.43
Illumination quality	-0.006	0.005
	(0.006)	(0.008)
(share)	0.89	0.88
Ventilation quality	0.006	-0.028**
	(0.008)	(0.012)
(share)	0.79	0.82
Policy exists for	0.01	-0.018
Violence	(0.012)	(0.023)
(share)	0.36	0.29
Classroom quality	-0.019*	0.032
	(0.011)	(0.02)
(share)	0.60	0.55
Teachers regularly checking	0.000	-0.011*
Portuguese homework	(0.004)	(0.006)
(share)	0.82	0.86
Teachers regularly checking	0.001	-0.004
math homework	(0.003)	(0.005)
(share)	0.85	0.87
Average class size	0.158	-0.462
	(0.131)	(0.292)
(share)	28.42	31.01
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School (municipality) specific time trend	Yes	Yes
N	26,270	15,415

Notes: There is a difference in the number of schools for 5th and 9th year students because many schools for 9th year students have missing values for one of the school characteristics. Every specification includes year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 9: Test of change in student characteristics with the policy adoption

	5th year students	9th year students
White students	-0.017***	0.007**
	(0.002)	(0.003)
(share)	0.37	0.37
Female	0.010***	0.000
	(0.002)	(0.003)
(share)	0.49	0.55
Have a car	0.004	-0.001
	(0.003)	(0.005)
(share)	0.61	0.54
Have a computer	0.006***	-0.005
	(0.002)	(0.005)
(share)	0.36	0.34
Mother less than	-0.001	0.002
high school degree	(0.002)	(0.002)
(share)	0.44	0.53
Father less than	-0.006***	-0.005*
high school degree	(0.002)	(0.003)
(share)	0.44	0.54
Single parent	-0.001	0.004
	(0.002)	(0.003)
(share)	0.22	0.22
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School (municipality) specific time trend	Yes	Yes
N	1,196,726	1,043,128

Notes: The variable in each row was used as a dependent variable. Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), which were not used as dependent variable. We also include year and school(municipality) fixed effects, and school(municipality) specific time trends. Each coefficient represents the coefficient on being treated. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.

Table 10: The overall effect of compulsory schooling reform from restricted samples

		5th year students		9th year students	
		Portuguese	Math	Portuguese	Math
		(1)	(2)	(3)	(4)
Restricted samples 1: Drop schools that adopted the policy before 2006					
	Treated	0.094***	0.113***	0.026	0.024
		(0.005)	(0.005)	(0.019)	(0.018)
	N	966,780	966,780	466,366	466,366
Restricted samples 2: drop schools that adopted the policy after 2009 for 5th year students					
	Treated	0.088***	0.098***		
		(0.004)	(0.005)		
	N	998,133	998,133		
Restricted samples 3: Schools that have 9 years of primary education					
	Treated	0.116***	0.144***		
		(0.007)	(0.008)		
	N	334,903	334,903		
School FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes	Yes
School (municipality) specific time trend		Yes	Yes	Yes	Yes

Notes: The coefficient on being treated is measured again with restricted sample in each row. We used main specification including control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school(municipality) fixed effects, and school(municipality) specific time trends. Standard errors are reported in parentheses and clustered at the school level for 5th-year students and at the municipality level for 9th-year students. *p<0.1, **p<0.05, ***p<0.01.