The "Quality of Quantity": Achievement Gains from Adding a Year to Brazilian Primary Schooling

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Abstract

This study is an impact evaluation of the policy that added an extra year in Brazilian compulsory education. We use a difference-in-difference-in differences (DDD) strategy to identify the effect of a one-year increase in instructional time on students' academic achievement by the end of the primary school level. The findings are that (1) the additional year of schooling increases mathematics and Portuguese language achievement in Brazilian primary schools by 0.22 - 0.26 standard deviations, controlling for student individual and family covariates and state fixed effects; and (2) schools with students from the highest average socioeconomic background and with the highest average test scores made larger gains from the extra year of education than schools with students from lower and middle socioeconomic backgrounds and lower and middle levels of test scores. We conclude that this policy has had positive effects on school achievement in Brazil, but that the policy may increase the gap between the highest SES students and the rest and between already higher achieving students and the rest. Our results also suggest that an additional year of schooling has an effect on achievement which is as large or larger than interventions attempting to raise learning in any given year of schooling.

Key words: impact evaluation; instructional time; difference-in-difference-in-differences; quantity of schooling; learning gains

Do Children Learn More When They Take an Additional Year of Primary Education? The Case of Brazil

Introduction

A major debate in educational policy has emerged around the educational and economic value of additional years of schooling versus the value of improving the quality of a given year of schooling (Behrman et al, 2008; Hanushek et al, 2013). In developing countries, where actual time spent on instruction in an academic year may be quite low, this debate is nested in the larger discussion about the value of adding instructional time in the form of more years of schooling (Barro, 2000; Barro and Lee, 2001), more hours to the school day (Stallings, 1980), reducing teacher and student absences (Chaudhury, et al, 201X; Benavot and Amadio, 2004; Abadzi, 2009), or increasing time on task in the classroom (Stallings, 1980; Bruns et al, 2014).

The main thrust of the argument for increasing the time that students spend on academic tasks is that, as in most activities, more exposure (and practice) produces greater familiarity with the task and, hence, greater skill. The main argument against it is that simply exposing students to more mathematics, reading, or science does not translate into higher levels of skills if the "quality" of exposure is low. According to the "quality better than quantity" advocates, increasing the number of years students stay in school has a particularly low payoff in learning gains in countries where students score relatively low on international tests (Hanushek et al, 2013).

Research that has examined the effect of instructional time on educational outputs can be divided into two approaches. The first approach examines the effects of policies that increase or reduce the amount of instructional time on student achievement, usually by comparing student achievement before or after the implementation of the policy, or by comparing the achievement of those students who were affected by the policy versus those who were not. These studies have assessed a broad range of interventions affecting instructional time, including hours within the school day, days within a school year, and years within an educational level (Andrade-Baena, G., 2014; Bellei, 2009; Cooper, Charlton, Valentine, Muhlenbruck, & Borman, 2000; Krashinsky, 2006).

The second approach takes advantage of "natural experiments" involving instructional time. This approach includes analyses that compare students of the same age with a year difference in schooling because of well-enforced school entry cut-off dates and analyses that compare similar students attending school differing numbers of school days per year because of severe winter conditions (Angrist & Krueger, 1990; Agüero & Beleche, 2013; Luyten, Peschar, & Coe, 2008; Marcotte & Hemelt, 2008).

In this study we combine elements of these two approaches. We estimate the effects on student achievement of a major educational policy intervention in Brazil that added one year to compulsory education for all Brazilian students by reducing the first grade entry age in public schools from 7 to 6 years old. A few states began implementing this policy in various municipalities as early as 2004, but the law did not come into effect nationally until 2007. In addition to this policy intervention at a defined period of time, we benefit from a natural experiment resulting from the way the policy was implemented. The law allowed municipalities to decide when to enforce the new entry age, even once the law went into effect in 2007. Municipalities only had to adopt the new policy in schools by 2010. Municipalities that were more "ready" to implement the new law may also have been those that would have made higher student test score gains had the law not been implemented. Our methodology allows us to deal with this possible endogeneity problem.

The additional year intervention is particularly interesting because students in Brazil score relatively low on international tests such as the OECD's Program in International Student Assessment (PISA), suggesting that if the "quality better than quantity" argument is correct, the value added of an additional year of schooling is likely to be small.

We also benefit from the existence of a national test, the *Prova Brasil*, that we can use as the outcome measure for our analysis. From 2005 on, the *Prova Brasil* has been administered every other year to all students in the last year of public primary and the last year of public middle schools.[1] The test assesses mathematics and Portuguese language and also surveys students, teachers, and principals with detailed questionnaires. Since the *Prova Brasil* is only administered to students at the end of primary and middle school, the 2009 *Prova Brasil* was the first to assess the cohort in those states where more than a few percent of students began schooling at six years old before 2007, and the 2011 *Prova*

[1] Before 2005, the national assessment of compulsory education—the SAEB—only assessed randomized samples of students enrolled in the 4th and 8th grades of both public and private schools in Brazil (the SAEB continues to be applied every two years along with the *Prova Brasil*). The 2005, 2007, and 2009 *Provas* tested students who had only been in primary school 4 years and 8 years, but the 2011 *Prova* tested cohorts of students who had been in school both 4 and 5 years at the end of primary school and 8 years, at the end of middle school. The 2015 *Prova* will be the first to test students completing middle school with the full 9 years of schooling. As a matter of nomenclature, the Brazilian government renamed the grades after 2007-08, so that *Prova Brasil* tested students in the "5th" and "9th" grades even though the first cohort to actually be in school 5 years when they were tested was the 2011 fifth graders, and the first cohort to actually be in school 9 years in the ninth grade will be the 2015 9th grade cohort.

Brasil was the first to assess the cohort starting primary school at the age of six in 2007.

Thus, we can compare the gain in academic achievement between 2007 and 2011 on the *Prova Brasil* in municipalities/schools that implemented the policy change in 2007 and those municipalities/schools that did not, and we can test for the effect of the policy change between 2007 and 2009 for municipalities/schools that did and did not implement the policy in states that had begun to implement the policy among a significant number of students by 2005. In 2007, all students taking the *Prova Brasil* in the final year of primary school were completing the 4th grade. In 2011, however, students taking the *Prova Brasil* in the municipalities/schools that implemented the new law in 2007 were completing 5 years of schooling; those students in non-implementing municipalities/schools taking the *Prova Brasil* were, as in 2007, completing only 4 years of schooling. That is, even if municipalities had implemented the reform in their schools in 2008, 2009, or 2010, their students tested at the end of primary school in the 2011 wave of *Prova Brasil* would still have entered at 7 years old and still been in 4th grade. Similarly, in those states that already had implemented the additional year basic education reform in a significant fraction of their municipalities/schools by 2005, students taking the *Prova Brazil* in 2009 at the end of primary school that were in such schools would have taken 5 years of schooling and those students in schools that had not implemented the reform would have taken the 2009 as 4th graders.

However, the schools in those municipalities that chose to implement the reform in 2005 or 2007 may have had greater increases in *Prova Brasil* scores at the end of primary school even if their students had not had the extra year of schooling. To test for this possibility, we control for the gains in achievement of 8th grade students (newly named 9th graders—see below) in the same school.

The manner of implementing the reform and the timing of the waves of the *Prova Brasil* in 2007, 2009, and 2011 allowed us to use a "difference-in-difference-in-differences" (DDD) empirical strategy to approach an unbiased estimate of the extra year of compulsory education on Brazilian students' academic achievement. Our study is able, therefore, to identify the causal effect on students' educational achievement of a policy that actually changes the required number of years of compulsory education.

We find that Brazilian students with the extra year of education performed about 0.22 standard deviations higher in mathematics and about 0.26 standard deviations in Portuguese than students not yet affected by the change in the legislation. This is a very large effect size in terms of educational interventions more generally. We also find that students in the highest quintile of socioeconomic status and highest quintile of test scores benefitted more from the extra year of instruction than students in the bottom 80 percent of socioeconomic status and bottom 80 percent of test scores.

The paper is organized as follows: in Section 2, we briefly describe the legislation that increased the length of compulsory schooling in Brazil; in Section 3, we review the results of the extant research on the achievement effects of additional time in school; Section 4 describes our empirical strategy; Section 5 reports the results; Section 6 provides robustness checks, and Section 7 discusses the results and concludes.

2. The Change in Brazil's Compulsory Education Law

Beginning in 2004, some Brazilian states began promoting schools to add an extra year to basic education—increasing the number of grades in primary education from 4 to

5 and expanding basic education from 8 to 9 years. The mechanism used was to begin admitting students into first grade at age six rather than seven. In the first year that schools implemented this change, they had two different first grades, one with six year-olds and the other with seven year-olds. The six year-olds were defined as being in a nine-year cycle of basic education and the seven year-olds, in an eight-year cycle.[2] Table 1 shows official data for the percentage of students by state that were in the nine-year cycle in the years 2005-2007.

[Table 1 about here]

By 2006, Brazil's national legislature enacted a law requiring schools to implement gradually a nine-years cycle of compulsory education in all states, with the intention to have all children in Brazil starting school at the age of six (Law Number 11,274) by 2010.

Figure 1 depicts the Brazilian education system levels before and after the change. The main reasons the Brazilian Ministry of Education gave for increasing the number of years of primary and middle schooling were to guarantee greater opportunity to learn for all children, to improve students' academic performance and to promote an earlier educational start for children from lower socioeconomic class families, since middle and

[2] In those schools that had implemented the nine-year cycle in 2004 or 2005, the *Prova Brasil* test was only applied in 2009 to those students in the nine-year cycle. They would have been in the 5th grade in 2009, even if there were 4th graders in the same school. In those schools that had not implemented the nine-year cycle (even in the same state), the *Prova Brasil* in 2009 was applied to 4th graders. Thus, a number of students were not tested in 2009 in those states—specifically, those who had entered at seven years old in schools that had switched over to the nine-year cycle. Similarly, in 2011, the Prova Brazil was only applied to 5th graders in schools that had switched over to the nine-year cycle, with the same caveat that a number of students were not tested with the 5th grade test because they were still in 4th grade.

upper class families already tended to enroll their six year olds in (private) school *(Ministério da Educação do Brazil, Secretaria de Educação Básica,* 2007*)*.[3]

The new national law began to be implemented in 2007, with municipalities and states[4] responsible for progressively implementing this change in schools under their control. By 2010, all schools had to be ready to provide nine years of public schooling for every Brazilian child. This legislation added roughly 3.3 million students per year to the Brazilian primary school system (Ministério da Educação do Brazil, Secretaria de Educação Básica, 2007), bringing the total enrolled to about 16 million. It is also important to note that even though the Ministry of Education provided pedagogical guidelines for municipalities on what to teach in this extra year of schooling, each municipality had the autonomy to decide the curriculum to be taught during this additional instructional time and how much time would be dedicated to each subject.

[Figure 1 about here]

Another important feature of the legislation is that it permitted schools and school districts (municipalities) to decide when to implement the new compulsory education period. As mentioned earlier, schools in some states already had a high percentage of six-year-old students enrolled in school before 2007. As a result, in 2007, the first year of

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[3] The Ministry meant by this that children from middle and upper class families took advantage of private kindergarten for children age 6, thereby giving their children a head start in preparing for school. Of course, with the early enrollment reform in place, higher social class families could begin enrolling their children at 5 years old in private kindergartens.

[4] In Brazil, basic (grades 1-9 under the new system) and secondary (grades10-12) education is managed in parallel state and municipal system, with a very small minority of schools also run by the federal government. Beginning in the 1990s, states began to shift their basic education to municipalities, so that in 2013, about 80 percent of public primary school (grades 1-5) enrollment and about 50 percent of public middle school (grades 6-9) enrollment was in municipal schools.

national implementation, there were schools that had six-year-old students enrolled in first grade classrooms, and schools that had not implemented the new compulsory period yet and had only seven-year-old students enrolled in first grade classrooms. About 15 thousand schools (62 percent of the total in Brazil) had implemented the compulsory enrollment of six-year olds by 2007. In the three years that followed, the remaining 9.2 thousand schools did the same *(Ministério da Educação do Brazil, Secretaria de Educação Básica,* 2007*)*.

The 2007 *Prova Brasil* was administered to all public school students in Brazil in their last year of primary school (4th grade) and last year of middle school (8th grade). Unless they had repeated a year, none of the public primary school students tested in those two years had had more than 4 years of schooling. In 2009, the *Prova Brazil* was administered to 5th graders in schools in states that had admitted six year-olds in 2005 and administered to 4th graders otherwise. In 2011, the *Prova Brasil* assessed cohorts of students in schools that had implemented entry for students into primary school at the age of six in 2007 or before. Those students had had five years of primary schooling and were tested at the end of 5th grade rather than 4th grade. In 2011, in those schools that had not implemented earlier age entry, *Prova Brasil* also assessed cohorts of students that were exposed to only four years of primary education, still entering school at age seven under the 1971 law.

3. Research on Additional Instructional Time

3.1. Instructional time and achievement through the lens of natural experiments

Random assignment, the gold standard of evaluation methods, for ethical or operational reasons,[5] often cannot be used to assess the impact of educational inputs on student outcomes. In the absence of random assignment, natural experiment research designs are powerful alternatives to estimate the unbiased effects of these inputs. For example, Marcotte & Hemelt (2008) investigated the difference in math and reading test scores between Maryland students that attend different numbers of school days due to unscheduled closures related to snowstorms. The authors found that for third grade students, five unscheduled closures decreased math and reading test scores by 3 percent compared to the scores of students that attended school uninterrupted by closures.

In a similar study, Agüero & Beleche (2013) examined the differences in academic achievement resulting from exogenous variation in the number of days students attended school across Mexican states from the first day of school to the day of the assessment test was administered. The authors found positive but diminishing marginal effects on academic achievement for students that attended more days. They also found that students attending schools in higher income areas had greater benefits from the extra school days than students in lower income areas.

Using a regression discontinuity approach first developed by Angrist and Krueger (1990) and taking advantage of strict compliance to a school entry cutoff date on school age to enter kindergarten, Luyten et al (2008) estimated the effect of an extra year of schooling on the PISA reading test performance of 15 year-old U.K. students. They show that an additional year of secondary schooling results in a 0.12 increase in reading

[5] Denying control group children access to an educational service in order to create ideal conditions for academic research can be considered unethical. Additionally, for certain types of educational interventions, random assignment is not feasible.

achievement. In this case, disadvantaged students benefited more from the extra year of schooling (0.15) than their advantaged counterparts (0.044). Similarly, Luyten & Veldkamp (2011) used TIMMS-95 data, which relate student achievement to both mathematics and science in 15 different countries and found that the cross national effect size on student achievement of an extra year of education is 0.46 for math and 0.35 for science.

The problem with the regression discontinuity strategy used to measure the effect of the extra year of schooling is that it does not take into account peer and relative age effects. It turns out that the students that had access to an extra year of schooling in the U.K. also were the younger students in their cohort, whereas the students that had access to fewer years were also the older students in their cohorts. As a result, peer and relative age effects might be confounding the estimations found in this study. Given these shortcomings, our approach in Brazil is better able to estimate the effects of the extra year of education because it compares whole cohorts of students that have similar age distributions, although those that take an extra year of education in Brazil entered school a year younger. Nevertheless, in the last year of primary school, when they are tested, Brazilian students in our treatment and control groups are of the same average age with similar age distributions in the fifth grade (treatment schools) or fourth grade (control schools).

3.2. Estimates of the effects of policy-driven increases in instructional time.

Instructional time is an educational input just like class size or textbooks. When faced with the dilemma of where to invest to increase educational outputs, countries or school districts can opt to increase instructional time among a number of options

(Krashinsky, 2006). Research has examined policies that changed the number of hours, days and years of instructional time offered to students. For example, Cooper et al.'s (2000) review of the literature of summer school programs in the United States found evidence that these programs have positive effects on student achievement. Using meta-analysis, they concluded that students who completed summer programs score about 0.2 standard deviations higher on outcome measures than students who did not attend summer programs. They also concluded that summer school programs have more positive effects on the achievement of middle-class children (effect sizes of 0.44 - 0.56 standard deviations) than on their disadvantaged counterparts (0 .20 to 0.24 standard deviations).

In 1996, the Chilean government implemented a policy that increased the school day by two hours. Similar to the Brazilian policy that added an extra year to primary schooling, Chile’s full day reform was not implemented in all schools in the same year. Bellei (2009) used this exogenous variation to evaluate the impact of the Chilean longer day program. Using a difference-in-differences approach, he found that the program had a significant positive and effect on students’ academic achievement of 0.05 - 0.07 standard deviations in Spanish and of 0.00 - 0.12 in mathematics. Bellei found that students in the upper part of the achievement distribution and rural public school students benefitted most from the longer school day, but found no evidence of heterogeneous program effects across student socioeconomic groups.

Some school systems have reduced instructional time. In 1999, Ontario, Canada, eliminated the fifth year of education from its high school level. This policy change created two cohorts of students who finished high school together, but with different number of years of high school education. Using several different econometric approaches, Krashinsky (2006), examined university level academic achievement of these two cohorts and concluded that students with one year less of high school performed worse than their counterparts in all subjects examined by the study (effect sizes of 0.53 -1.24 SDs). However, there are some important threats to this study's external validity. First, since the study only compared the performance of students that entered university after finishing high school, its estimates do not account for the students that did not enroll in university. Second, the students who completed five years of high school were one year older than four-year high school completers. Since academic achievement can be related to age (Smith, 2009), Krashinsky's estimates could include an age effect. Older students may have chosen to enroll in classes more aligned with their tastes and abilities. Unless we assume that part of the positive effect of staying in high school a year longer is growing older and wiser, this would have resulted in upward biased estimates of the effect of the additional year of schooling.

3.3. Possible effects of starting school early

Also relevant to our Brazilian study is a series of studies addressing the question of whether students benefit from starting school earlier. Using U.S. data and relying on a regression discontinuity approach, Mayer & Knutson (1997) estimated differences in math and reading achievement of children who were in the same grades but started school roughly one year apart from each other. The authors find that children who started school earlier had higher achievement. Starting school a year younger results in a reading score increase of 0.403 SDs and a math score increase of 0.261 SDs.

Leuven, et al. (2004) study the effects of being exposed to primary school at an earlier age in the Netherlands. Dutch parents have the option of enrolling their children in regular school between the first day after their fourth birthday and the day before they turn five, at which point children must be enrolled in school. The results of this study show that enrolling children one month earlier increases test scores on average by 0.6 SDs.

James Heckman has led a line of research that finds evidence that disadvantaged children have greater benefits from earlier intervention than their advantaged counterparts (Heckman, 2006). However, other studies have reached the opposite conclusion: children starting school at an older age achieve at higher levels. Analyzing data from the U.S. 1999 Early Childhood Longitudinal Study, Datar (2006) finds that entering kindergarten a year older increases test scores for the first two years of regular school. He also finds that this benefit is greater for at-risk children. Analyses of Swedish (Fredriksson & Ockert, 2005) and German (Puhani & Weber, 2005) data show that entering school at an older age increases pupil test scores and longterm school attainment. Fredriksson & Ockert (2005) find that starting school one year later increases school performance by 0.2 SDs, whereas Puhani & Weber (2005) find that entering school at 7 instead of 6 years old increases test scores at the end of primary school by 0.42 SDs.

Note that the amount of schooling taken by both the treatment and control groups in these studies were held constant. The variable of interest was the age that children started school. However, students in these studies were also tested when they were at different ages. As a result, the research design used in these studies is not able to disentangle the joint effect of entry age, test age and relative age. They fail to explain whether the students who start school older perform better because they enter school later or because they are in a favorable position compared to their younger peers.

In our Brazil study, we avoid these pitfalls. Students starting first grade under the reform are younger than students who start first grade a year older under the previous law, but all these students are placed in classes with students of the same age. Furthermore, our measure of student achievement is a test given in the last year of primary school, when students who started a year earlier under the new law and students who started a year older under the previous law are the same age. The only difference is that those entering a year younger took a additional year of schooling (five rather than four years). It is true that the effect of an additional year may be reduced because students starting school a year younger at 6 years-old may not make as large gains in the first year as students starting first grade at 7 years-old. Nevertheless, this way of adding a year of schooling may be more "efficient" than having students begin primary school at 7 years-old and staying in high school until 19.

4. Empirical Strategy

The 2006 entry-age legislation gave schools and school districts (municipalities) in Brazil the prerogative to decide when to implement the new entry rule. As a result, in the academic year 2007 (which corresponds in Brazil to the calendar year) there were two categories of schools: schools that had six-year-old students enrolled in first grade classrooms, and schools that had not implemented the reform and had only seven-year-old students enrolled in its old-system first grade classrooms. The 2011 *Prova Brasil* therefore assessed two groups of students in the last year of primary school: those who had access to five years of education starting at the age of six—the "treatment" group—and students who had access to only four years of basic education starting at the age of seven—the "control" group.

For states that had already begun implementing the nine-year basic education cycle by 2005 among a "significant" proportion of schools (and students), we were able to estimate the extra year effect comparing the 2009 test results with the results in 2007, when tested students in these same schools had only attended 4 years of primary schooling.

In addition, in both 2011 and 2009, the *Prova Brasil* tested students in the newly named 9th grade of these same schools.[6] Ninth grade students in the same schools as younger "treated" and "control" students in all three test years—2007, 2009, and 2011—had not yet been subject to the extra year treatment, so provide an additional level of control. The 9th grade variable controls for the possibility that students in "extra year treatment" schools made larger gains than students in control schools independent of the extra year taken by 5th graders.

Brazil has 5,565 municipalities and, since the federal law did not dictate any criteria for deciding when to implement the new five-year primary education policy, each municipality used its own criteria to decide when each school would put the reform into effect. However, independent of the existence of an improbable coincident criterion for the implementation of the law, from the students' perspective, the timing of program implementation was completely arbitrary, which rules out selection bias on the individual level (Bellei, 2009). Therefore, we anticipate that any historical trend that differentiates pre- and post-implementation cohorts will impact students in treatment and control schools in a similar way. In addition, we control for the gains of untreated 9th graders in

[6] Even though they are called "9th graders" in 2009 and 2011, the students in 9th grade had started school at seven years old and had therefore only been in school eight years.

the same schools. This set of conditions allows us to approach unbiased estimates of the effect of the additional year of instruction on students' academic achievement.

To estimate this effect, we use a difference-in-difference-in-differences strategy (de Carvalho Filho, 2012; Authors, 2015) to investigate whether schools that offered five years of primary education had higher academic achievement than comparable schools that offered only four years of primary school. We did this for students in all states, comparing 2007-2011 gains for 5th graders in schools that had converted to 9-year basic education in 2007 or before with 2007-2011 gains for students in schools that had not and gains on the *Prova Brasil* for students in the 8th/9th grade in the same schools in 2007-2011. Additionally, we did a similar difference-in-difference-in-differences for 2009 versus 2007 for states that had implemented the 9-year basic education reform in a major way by 2005.

4.1. Data

We use three datasets to evaluate the impact on student achievement of an additional year of schooling. The first is the three waves of the *Prova Brasil* from 2007 to 2011. This assessment includes questionnaires for schools, principals, teachers and students, as well as a test of student proficiency in mathematics and Portuguese. The results of this test are considered by the Brazilian government to be the official measure of quality of the educational system. The test is vertically integrated and comparable from test wave to test wave. The *Prova Brasil* includes data on student achievement that we can use to estimate the average performance of students in their last year of primary school in 2007, 2009, 2011 both in schools that chose to implement the earlier entry age in 2004/5 or 2007 and those schools that did not implement the reform at either of those times. The *Prova Brasil* also includes demographic data on students in the schools and the state in which the school is located.

The two additional data sets we are a table showing the percentage of students in each state that were enrolled in schools that had implemented 9-year basic education schools (begun admitting students into the first grade at 6 years-old) by 2005 (Table 1, above) and the Brazilian School Census of 2007, the country's official school census administered every year by the Brazilian Ministry of Education. The Table 1 data help us choose states in which to estimate extra year of schooling effects in 2007-2009. The School Census contains data about all students, teachers, and schools from kindergarten through high school, and has more than 40 million observations at the student level. The Brazilian School Census of 2007 dataset includes a variable that allowed us to identify which schools had already implemented the six year-old entry age—hence to identify the schools that had implemented the new 9-year compulsory basic education reform by 2007.

The combination of these datasets generates 6,772 observed schools (of 24,599 schools surveyed by the *Prova Brasil* in 2011) meeting four criteria. First, more than 20% of the students in the school had to have taken the *Prova Brasil* test in 2011—the data report the average test score for the school in 2011. Second, the data from *Censo Escolar* must contain information on whether the school implemented the fifth year of primary education in 2007 or after 2007. Third, the schools had to have taken the 2007 and 2009 *Prova Brasil* test as well as the 2011 test. Fourth, both the 5^{th}/4th grade and the 9^{th}/8^{th} grade students in the school had to have taken the *Prova Brasil* test.

Unfortunately, the 2007 *Censo Escolar* is the first school census that allows us to identify such schools. Therefore, in our DDD analysis of additional year of schooling gains in 2007-2009 in states that had already begun implementing the reform by 2005 (and in those states that had not), the only way that we can identify schools that had implemented the reform is from the 2007 *Censo Escolar*. This means that of the 8 states (Amazonas, Piauí, Ceará, Rio Grande do Norte, Minas Gerais, Rio de Janeiro, Mato Grosso, and Goiás) that had implemented the reform, in 5 (all but Minas Gerais, Rio de Janeiro, and Mato Grosso) we greatly over-identify schools as having implemented the reform in 2005. Yet, 75 percent of all students in schools that had implemented the reform in 2005 are in just two states, Minas Gerais and Rio de Janeiro, where there is little over-identification from using the 2007 *Censo Escolar* data, since in those two states almost all students were already enrolled in the 9 year program in 2005. Thus, the DDD estimates for the impact of the additional year on students in the 2007-2009 period are biased downward, but the bias may not be great.

Our unit of observation is the school, so all the variables we use in the difference-in-difference-in-differences analysis are measured as averages at the level of the school.

4.2. Causal Inference Strategy

In order to estimate the effect the extra year of primary education had on students' achievement we used a triple difference (DDD) identification strategy (see Tyler et al, 2010; Ravallion et al, 2005).[7] The DDD estimator is a combination of two DD models. The first DD model computes the *difference* in the *difference* (gain) of average student

[7] Angrist and Pischke (2008) pointed out that this model is a modification of the Difference-in-Differences (DD) model with possible higher order control groups that contribute to the identification strategy of programs with more than one dimension of external variation.

achievement in 2011 (post-treatment) and 2007 (pre-treatment) average test scores of students in schools that implemented the fifth year in 2007 (treated schools) and those schools who implemented it later (control schools). The second DD model computes the *difference* in the *difference* (gain) of 9th graders in treated and untreated schools. We also estimate a similar DDD in 2009 versus 2007 for the states that had more than 18 percent of students in 9-year basic education in 2005 and for states that had 10 percentage or less students in 9-year basic education in 2005.[8]

We model this difference-in-difference-in-differences strategy as follows:

$DD_1 = \Delta Y_{\text{fiveyear},5g} - \Delta Y_{\text{fouryear}, 4g}$;

$DD_2 = \Delta Y_{\text{fiveyear},8g} - \Delta Y_{\text{fouryear},8g}$;

$DDD = D_1 - D_2$;

where Y = test score on the *Prova Brasil* test. The Δ represents the difference in time, so each component with a Δ differences out any time fixed effect of each cohort in schools in municipalities that implemented five year primary schooling or did not. Where the DD_1 differences net out any constant effect for achievement gains for students in the last year of primary school fifth graders versus fourth graders, both with the same age distribution in the grade, and the DD_2 differences net out eighth graders' (called 9th graders in the new terminology) constant effects. This can be represented by the following regression:

$$Y_{jt} = \beta_0 + \beta_1 \text{ fiveyear}_{jt} + \beta_2 2011_{jt} + \beta_3 \text{grade5}_{jt} + \beta_4 \text{ fiveyear}_{jt} * 2011_{jt} + \beta_5 \text{grade5}_{jt} * 2011_{jt}$$

$$+ \beta_6 \text{ fiveyear}_{jt} * \text{grade5}_{jt} + \beta_7 \text{ fiveyear}_{jt} * 2011_{jt} * \text{grade5}_{jt} + \varepsilon_j \qquad (1)$$

[8] The 18 percent figure is taken from Table 1. There are 8 states with that percentage or higher. Below that percentage no state had more than 10.2 percent enrolled in reformed schools.

where Y_{jt} is school j's average 2007 (pre-treatment) and 2011 (post-treatment) *Prova Brasil* score in mathematics or Portuguese language; $fiveyear_{jt}$ is a dummy variable indicating whether school j had chosen to become a five year primary school in 2007 ($fiveyear_{jt} = 1$) or had *not* chosen to become a five year primary school in 2007, and therefore a control school ($fiveyear_{jt}=0$); 2011_{jt} is a dummy variable indicating whether the average *Prova Brasil* score for school j is for 2011; 5^{th} $grade_{jt}$ is a dummy for the fifth grade in each school ($9^{th}/8^{th}$ grade = 0). ε_j is the school-level error term.

The DDD effect is measured by estimating the β_7 coefficient, which is obtained by the interaction of the fifth grade dummy, the 2011 year dummy, and the five year dummy. This interaction term represents the difference in average gain score in 2007-2011 between our treatment schools and our control schools. It represents the difference-in-difference-in-differences estimate of the impact of the extra year of compulsory education in Brazil.

To control for possible differences in the demographic composition in the schools that chose to implement the new law in 2007 and the demographics of the control schools, in Equation (2), we include vectors of school average student characteristics and family level covariates. The vector of student characteristics (X_j) includes average student age in the school's fifth grade[9] and the proportion of students who attended some form of preprimary schooling. Both repeating grades and early childhood educational interventions (Campbell & Ramey, 1994, 1995) are correlated with academic achievement. The vector of student family level covariates (F_j) includes students' average mother's education (Leibowitz, 1977; Murnane, 1981), estimated as the proportion of

[9] Since our dataset does not include information on student grade repetition, we use the student average age to identify students that are behind their cohort.

students' mothers with high school of more, and a socio-economic status index composed of items in the home and parents' education. This index is called the Criterio Brasil (CB Parcial), and it is considered the official socioeconomic classification in Brazil (Colom & Flores-Mendoza, 2007). In addition, the schools that decided to adopt the new law may also not have been randomly drawn from Brazilian states. Even when controlling for student demographics, student achievement gains varied considerably across states in 2007-2011 (Authors, 2014)—μ_s is a state-level fixed effect.

$$Y_{jt} = \beta_0 + \beta_1 \text{ fiveyear}_{jt} + \beta_2 2011_{jt} + \beta_3 \text{grade5}_{jt} + \beta_4 \text{ fiveyear}_{jt} * 2011_{jt} + \beta_5 \text{grade5}_{jt} * 2011_{jt} + \beta_6 \text{ fiveyear}_{jt} * \text{grade5}_{jt} + \beta_7 \text{ fiveyear}_{jt} * 2011_{jt} * \text{grade5}_{jt} + \gamma X_j + \delta F_j + \mu_s + \varepsilon_j \quad (2)$$

Due to the correlation of school error terms within as opposed to between states, we estimate cluster-corrected Huber-White estimators for Equation (2), in which schools are considered to be clustered in states.

Equation (2) was also estimated using *Prova Brasil* 2007 and 2009 test scores for a subsample of eight states that implemented 9-year basic education in a substantial number of schools by 2005, and for the subsample of states that had not substantially implemented 9-year basic education by 2005.

Finally, in order to investigate whether schools which serve, on average, higher or lower socioeconomic students or higher or lower test score may have benefited more from the treatment, we also tested for heterogeneous treatment effects by estimating the Equation (2) difference-in-difference-in-differences in 2007-2011 across quintiles of average student SES schools and estimate a quintile regression of the DDD effect across student test scores.

5. Results

5.1. Difference-in-difference-in-differences estimates of the Five Year primary school effect

Table 2 shows the average *Prova Brasil* mathematics and language test scores for treatment and control schools in 2007 (pre-treatment) and 2011 (post-treatment) for our sample of 6,772 schools that met the four criteria outlined earlier. On average, students in treatment schools scored significantly higher in 2007 on the $5^{th}/4^{th}$ grade test than students in control schools in both mathematics and Portuguese. In addition, students in treatment schools made much larger gains in both mathematics and Portuguese between pre-treatment (2007) and post-treatment (2011) than students in control schools.

Table 2 also presents the means and standard deviations of school averages of our control variables—the proportion of students in each school that took some form of pre-schools, the percent of students whose mother had high school or more education, the average socio-economic index [CB Parcial], and the average age of the students in the $4^{th}/5^{th}$ grade in 2011. The means suggest small differences in these control variables between treated and untreated schools.

In Table 3 we formalize these results in our DDD regression analysis. The coefficients in Table 3 present the effect of the reform, adjusted for the impact of covariates (averaged for each school) and state fixed effects (Equation 2).[10] The effect of the extra year of primary schooling net of gains in 2007-2011in the same school for untreated 9^{th} (8^{th}) graders—"Fiveyear*2011*grade5"— is 4.75 additional points in the *Prova Brasil* mathematics test score and 5.00 additional points in the Portuguese test score ($p<0.001$). The DDD coefficient for mathematics represents an effect-size of 0.22

[10] Since schools are clustered in states, we

standard deviations and the coefficient for Portuguese language represents an effect size of 0.26 standard deviations. These results confirm the hypotheses presented at the beginning of this paper, namely that the additional year of primary school in Brazil positively affected the academic achievements of the students that were exposed to an extra year of schooling. The effect sizes are large in terms of other measured effects of educational interventions (McEwan, 2014). For example, the effect size of a major early literacy intervention in Brazil's state of Ceará in this same period was estimated as 0.1-0.18 standard deviations (Authors, 2015).

[Table 2 and 3 about here]

5.2. Estimating gains in states that adopted extra year reform early and in those states that did not.

We identified eight states in which a significant fraction of schools adopted the extra year of schooling reform by 2005. We would expect that students in those schools, who were in 5th grade when they took the Prova Brasil test in 2009, would have made greater gains in between 2007, when all students even in those states would have been in the 4th grade (untreated), and 2009, than students in control schools in those states still taking the test in 4th grade. In states where few schools adopted the reform before 2007, we would expect no difference in gain by 2009 between students in schools that only adopted the reform in 2007 ("treatment" schools) and students in schools that did not adopt the reform by 2007 (control schools). In both "treatment" and control schools, students taking the *Prova Brazil* test in 2009 would all still have had only four years of schooling in 2009. The first cohort to reach 5th grade would have done so only in 2011.

Table 4 shows the absolute average test scores for 2007, 2009, and 2011 for the treatment and control schools in the two sets of states. We see that by 2007, only 95 schools in the sample of schools that took both 5th and 9th grade tests in 2007, 2009, and 2011 in the eight states implementing the extra year reform early had not implemented the reform. This underestimates the number of schools that had not implemented the reform in 2004-2005, but as noted, we have no way of identifying implementing schools except through the school census of 2007. Thus, our estimate below of the effect of the extra year reform on 5th test score gains in the states that implemented early is almost certainly biased downward. On the other hand, the estimate of gains in 2007-2009 in states that did not implement the reform early is an unbiased check on the DDD gains in the absence of the extra year treatment. If there was no net gain for students in "treated" schools before the treatment took effect, this strengthens the argument that the gains in 2007-2011 were the result of the extra year of schooling.

In Table 5, we show the results of using the same sample of 6,772 schools that were the basis of our estimates in Table 2 to estimate the DDD Equation (2 in the states with early implementation and those without early implementation. The coefficients of net gains for mathematics and Portuguese in early implementing states are of the same order of magnitude in 2007-2009 as the estimate for all states for 2007-2011 (Table 2), although not quite statistically significant at a 0.10 level. Furthermore, the gains in 2007-2009 for students in treated and untreated schools—before the treatment took effect—are negative and significant. This suggests that it was the extra year of schooling in these states, affecting the cohort that took the test in 2011, which produced the gains we saw in Table 3.

[Tables 4 and 5 about here]

5.3. Estimating heterogeneity of the additional year effect across school SES quintiles and across mathematics and Portuguese test score quintiles.

Figure 2 shows the estimated DDD gains in math and Portuguese scores from an additional year of primary schooling in each quintile of average school SES, using the model shown in Equation 2. Public schools with students from lower SES families had similar or higher gains from the added year of education than schools with students from more advantaged families, controlling for average student characteristics and state fixed effects, except for schools whose students averaged in the highest SES quintile. Treatment schools serving students whose families, on average, were in the lower two quintiles of the SES scale scored about 0.25 standard deviations higher in mathematics and language, than control schools with similar average SES students. Treatment schools serving students whose families were, on average, from the highest quintile of the SES scale had effect size gains in mathematics from the extra year of education that were about the same as schools whose students averaged in the lowest two quintiles of SES. However, in Portuguese, the effect size of the gain for treatment schools with highest quintile students was much higher that for lowest two quintile students.

[Figure 2 about here]

Figure 3 shows the quintile regression based on average school test scores. The highest scoring quintile schools hat were treated with an extra year had considerably higher gains than treated schools in the bottom four quintiles. Thus, both in terms of SES and tests scores, schools in the highest quintile generally benefited more from the reform than other schools.

6. Discussion

Thanks to a natural experiment that added an extra instructional year to Brazilian primary education but not in all schools at once, along with the availability of national test scores for students in treated and untreated schools, allowed us to identify unbiased estimates of the effects on student achievement of an additional year of schooling in the early grades.

We applied a difference-in-difference-in-differences strategy and used *Prova Brasil*'s 2007, 2009, and 2011 mathematics and Portuguese language test data to measure differences in student achievement gains for students in schools that had adopted the reform and those that had not. We found large gains for students in schools with the additional year: about 0.22 standard deviations in mathematics and 0.26 standard deviations in Portuguese. We also tested for a similar effect in states that had begun adopting the extra year reform as early as 2004-2005 and found that the size of the gain made by students in those states, already realized by 2009, was of about the same magnitude as nationally in 2007-2011.

In addition, we found that schools serving students of both high and low average SES made achievement gains from the additional year of primary education, but the effect of that added year on achievement for schools with highest average SES students was larger in Portuguese than the effect on schools with lower average SES students. The larger benefit from the added year of schooling to highest SES students is consistent with findings of other studies on educational inputs (Agüero & Beleche, 2013), including estimates for the early literacy intervention in Brazil's Ceara state (Authors, 2015). We found similarly larger gains from the extra year for schools in the highest quintile of initial test score.

One possible reason for large effect size of this extra year of primary school is that it was implemented by lowering school entry age by one year. There is increasing evidence that focusing on early educational interventions, including additional years of education, has a greater impact on student achievement than later interventions (Doyle et al, 2009). Further, advocates of early educational interventions contend that they should benefit lower SES children more than higher SES children (Stipek and Byler, 2001), mainly because early schooling compensates for fewer academic activities at home. However, if lower SES children are not as school ready at 6 years old as those from higher socio-economic background families, schools with lower average SES students might benefit less from the additional year of schooling as schools with higher average SES students. It may be true that achievement gains from an additional year of schooling are generally lower in schools with students of lower average SES, although that does not appear to be the case in the United States (Alexander et al, 2001). It also may be true that schools in Brazil with higher SES students were better organized to take advantage of the additional year of schooling.

Our finding that the highest SES and highest test score students benefit more from the additional year is somewhat surprising, given that one of the main goals of the policy was to privilege lower socioeconomic status families by offsetting the assumed positive achievement effects of higher social families' enrolling their children in private school kindergartens. Our results suggest that in the public school student population tested by *Prova Brasil*, the additional year of schooling failed to close the achievement gap between lower and higher SES students. If Brazilian policy makers intend to equalize access to the same opportunities in life for children starting from different social positions, apparently other policies will need to compensate for disadvantages at school entry.

This also raises the broader issue of possible bias in our estimate of the additional year effect. As in the case of estimating the effect on student college performance of Ontario's reduction in the length of high school, our estimate of the additional year effect in Brazil may be biased by an unobserved student age effect. Since the additional year means that students entering each grade in the treatment schools are a year younger than students in the control schools, it is possible that, on average, they may not be as developmentally ready to absorb the curriculum of that grade. Alternatively, as we pointed out, under the law, municipalities and schools are given considerable leeway in developing curriculum for the new, longer basic education cycle (primary plus middle school). Schools may "reduce" the old curriculum down to accommodate the younger age of students in each grade. If either of these "age effects" is true, our estimated additional year of schooling effect would be smaller than had the law been implemented by keeping the entry age at 7 years-old and adding a year of primary school for the 2007 cohort—that is, making primary school five years long for students entering at 7 years old and, ultimately, finishing 9th grade (and high school) a year older.

Even though the particular circumstances of implementing the Brazilian law and the existence of a universal testing regime allowed us to employ successfully a DDD identification strategy, the particular way the reform was introduced should caution the reader in extrapolating our findings to different conditions of adding instructional time. The effect size we estimated for an extra year of schooling at age six might be different from an extra year at age five or other ages in students' school career. For example, Brazil intends to invest in two more additional years of schooling, making education compulsory beginning at age of 4 by 2016. In our study, we control for the proportion of students in each school that had taken pre-school, and the large and significant coefficients—5 points higher mathematics scores and 4 points higher Portuguese scores in schools with 10 percent higher percentage of students that had attended pre-school—suggest that universalizing pre-school would have a positive effect on student achievement. However, it would be premature to assume that the additional two years of instruction that are planned to be implemented by 2016 would increase average student achievement significantly in primary school and beyond. Future research needs to examine whether such interventions increase the number of years students stay in school have a positive effect on the academic performance of Brazilian students.

It is also important to mention that the implementation of this police required schools and municipalities to deal with issues such as the provision of extra space, definition of curriculum, resizing the teacher supply and training, as well as organizing the additional instruction time over the course of primary and middle school education, and eventually secondary education. It is natural to assume that because of the complexity of these issues, it would take some time before most schools master the provision of this additional educational input. As a result, the effect sizes found in this study might be different from the ones found in the years following the implementation of this policy, when the learning curve either dampens the initial effect (schools dumb down the curriculum) or increases it (schools learn how to teach students more in the five years than they did initially).

Prior research found positive and statistically significant effects on student achievement in Chile of a large and expensive increase in instructional time through lengthening the school day (Bellei, 2009). Our study of Brazil adds empirical evidence that additional years (time) in school, particularly in early childhood, has a large impact on learning gains. The results contradict claims that additional years in school fail to produce substantial learning gains. In 2015 the first cohort that had access to the additional year of instruction in Brazil will finish the whole nine-year cycle of compulsory education, and the *Prova Brasil* test will assess them. This will be an important opportunity for future research to examine whether the effect of the extra year of schooling in Brazil lasts for longer periods. Nevertheless, studies in the United States that investigated the relationship between additional instructional time at the early stages of life and academic achievement gains found that the positive effects tended to fade as students progress in school (Anderson, 2008). These two contradictory trends in findings make future investigations on the long-term effects of the Brazilian reform even more important.

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