UNIVERSITY OF CALIFORNIA
RIVERSIDE

Three Essays on Education and Female Labor Market Outcomes in Developing Countries

A Dissertation submitted in partial satisfaction
of the requirements for the degree of

Doctor of Philosophy

in

Economics

by

Hanbyul Ryu

June 2019

Dissertation Committee:

Professor Steven M. Helfand, Chairperson
Professor Anil Deolalikar
Professor Michael Bates

The Dissertation of Hanbyul Ryu is approved:

Committee Chairperson

University of California, Riverside

Acknowledgments

I am grateful to my advisor, Steven Helfand, for his consistent support and encouragement. His guidance was essential to construct my research pipeline and networks, and due to his willingness to share his experience and expertise, I was able to gain knowledge in numerous data sets and research techniques. I am fortunate to have him as my advisor and a collaborator. I also would like to thank the other members of my dissertation committee, Anil Deolalikar and Michael Bates. Their advice always helped me clarify complicated ideas and nudged me in the right direction. I will miss the meetings that I had regularly with my dissertation committee members. I also would like to express my appreciation to many UCR faculty members in applied microeconomics including Joseph Cummins, Matthew Lang, Bree Lang, Sarojini Hirshleifer, and Ozkan Eren. They were always willing to make time for me and provided invaluable comments and suggestion. I also thank to my fellow PhD students, who made the journey of doing a Ph.D. more enjoyable. I thank especially Sungjun Huh, Seolah Kim, Yoonjae Ro, Christian Gunardi, Jianghao Chu, Jayash Paudel, Ryan Kim, and Yoonkyung Kim for their constructive criticism and friendship.

Lastly, I would like to thank my family for being the best companions during my doctoral education and creating good memories that we can share throughout our lives. I would not have achieved these moments without my wife, Tzuyun Chen, who took the lead parenting and raising our beloved two children, Taehwan Ryu and Jimin Ryu. Also, I am indebted to my parents and parents-in-law who consistently supported us to pursue our goals and gave advice on how to be a good parent and scholar.

To my family for all the support.

ABSTRACT OF THE DISSERTATION

Three Essays on Education and Female Labor Market Outcomes in Developing Countries

by

Hanbyul Ryu

Doctor of Philosophy, Graduate Program in Economics
University of California, Riverside, June 2019
Professor Steven M. Helfand, Chairperson

This dissertation comprises three independent chapters in applied microeconomics.

In the first chapter, we investigate the effects of earlier and longer exposure to compulsory education on students academic performance. Previous studies were limited because they often used variation in only one variable, school entry age or duration. We overcome this limitation by using Brazil's 2006 school reform, which lowered the compulsory school entry age and increased its duration. Furthermore, we examine the heterogeneity in the effect of the reform based on students preschool educational status, thereby providing different policy implications for countries where preschool education is either limited or widespread. We find that students who were exposed to primary education a year earlier without any prior education exhibited a larger increase in test scores in the short run (5 years), but the students who attended preschool showed a more persistent increase in test scores in the medium run (9 years). These findings provide new evidence that earlier and longer exposure to compulsory education can generate positive effects on student performance, both in countries with universal and insufficient availability of preschool education.

The second chapter of my dissertation examines the effect of compulsory preschool education on maternal labor market outcomes using Brazil's 2009 school reform. I use a regression discontinuity analysis based on the preschool eligibility rule, which required students to start their first year of preschool education if they had turned four before March 31st. I find significant effects of preschool enrollment on maternal outcomes if 4-year-old children were the youngest members of the households and there were no other relatives present. When children were eligible for preschool entry, mothers not only increased hours of working but also were more likely to take formal jobs that guarantee employee rights and benefits. This study is the first paper to examine the effects of preschool enrollment using a school reform in a developing country.

In the third chapter of my dissertation, I examine the effect of the Zika virus outbreak in Brazil on fertility and female labor market outcomes. The Zika virus outbreak provided strong motivation to delay pregnancy as the Zika virus infection can cause serious birth defects like microcephaly. However, due to the high frequency of unintended pregnancy in developing countries including Brazil, determining whether women indeed delayed pregnancy was not certain. Using the variation of suspected microcephaly cases across states, I find that more suspected microcephaly cases provided incentives for women to delay pregnancy. This trend was more pronounced among younger and more educated women. Despite the fertility decline, my findings provide little evidence that female labor market outcomes were altered by the outbreak of the Zika virus.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

In the first chapter, we investigate the effects of earlier and longer exposure to compulsory education on students academic performance. Previous studies were limited because they often used variation in only one variable, school entry age or duration. We overcome this limitation by using Brazil's 2006 school reform, which lowered the compulsory school entry age and increased its duration. Furthermore, we examine the effect of the reform based on students preschool educational status, thereby providing different policy implications for countries where preschool education is either limited or widespread. We find that students who were exposed to primary education a year earlier without any prior education exhibited a larger increase in test scores in the short run (5 years), but the students who attended preschool showed a more persistent increase in test scores in the medium run (9 years). These findings provide new evidence that earlier and longer exposure to compulsory education can generate positive effects on student performance, both in countries with universal and insufficient availability of preschool education.

The second chapter of my dissertation examines the effect of compulsory preschool education on maternal labor outcomes using Brazil's 2009 school reform. I use a regression discontinuity analysis based on the preschool eligibility rule, which required students to start their first year of preschool education if they had turned four before March 31st. I find significant effects of preschool enrollment on maternal outcomes if 4-year-old children were the youngest members of the households and there were no other relatives present. When children were eligible for preschool entry, mothers not only increased hours of working but also were more likely to take formal jobs that guarantee employee rights and benefits. This study is the first paper to examine the effects of preschool enrollment using a school reform in a developing country.

In the third chapter of my dissertation, I examine the effect of Zika virus outbreak in Brazil on fertility and female labor market outcomes. Zika virus outbreak provided strong motivation to delay pregnancy as Zika virus infection can cause serious birth defects like microcephaly. However, due to the high frequency of unintended pregnancy in developing countries including Brazil, determining whether women indeed delayed pregnancy was not certain. Using the variation of suspected microcephaly cases across states, I find that more suspected microcephaly cases provided incentives for women to delay pregnancy. This trend was more pronounced among younger and more educated women. Despite fertility decline, our findings provide little evidence that female labor market outcomes were altered by the outbreak of Zika virus.

Chapter 2

The Effects of Changes in School Entry Age and Duration on Student Performance

2.1 Introduction

Lowering the 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 one 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 school entrance age by changing birth month cutoff dates.[1] Due to the reform, school duration increased by one year for every student, but 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.

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

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

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

[2]Duflo et al. (2011) found a 0.14 standard deviation increase after 18 months of a tracking pro-

9 years—the effect of the policy faded out such that Portuguese and Mathematics scores increased by approximately 0.03-0.06 standard deviations. Beyond test scores, the present study also finds that the policy significantly reduced the rates of grade repetition in the short and medium run.

Next, we used only early entrants and their counterfactual group to examine the effects of early exposure to compulsory education. We distinguish this analysis based on preschool education status. Primary reason is that when school entry age is lowered by one year, students with preschool education attained an additional year of schooling at the expense of their last year of preschool education. On the other hand, those without prior education attained an entire year of schooling with no such cost.[3] 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.Those who 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 evaluating the extent to which the effects dif-

gram.Muralidharan and Sundararaman (2011) found 0.27 and 0.17 standard deviation increases after 2 years of a teacher incentive program in India.

[3]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.

fered 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.

2.2 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,[4] unsafe weather conditions that cause temporary school closure,[5] or random varia-

[4] Brinch and Galloway (2012); banks and Mazzonna (2012); Dahmann (2017); Krashinsky (2014); Eble and Hu (2017).

[5] Aguero and Beleche (2013).

tion of test dates for military service.[6] 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. 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,[7] 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.[8] 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.[9]

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

[6] carlsson et al. (2015).

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

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

[9] 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.

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.

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.

Lastly, there are two studies that we are aware of that evaluate the school reform in Brazil (Moreira2014 and Rosa et al. (2018)), but this is the first study that uses the change of school cutoff dates to distinguish early and normal entrants. Note that early entrants were the only ones exposed to primary education one year early and have longer school duration. Therefore, we are able to estimate the effect of early exposure to primary education in addition to the overall effect of school reform. We are also able to explore heterogeneity by exposure to preschool, thereby providing different policy implications for countries where preschool education is either limited or widespread.

2.3 Context and compulsory education in Brazil

2.3.1 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 or other developed countries. 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.[10] 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 Gois--and a few municipalities, that chose to adopt the new system in 2004. Minas Gerais implemented the policy much more rapidly than Gois, as the state government mandated an immediate adoption of the new system. Gois, 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.

[10] 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.

2.3.2 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. States initially chose different cutoff dates, with most states adopting between February and April, and the new school cutoff date was officially set to March 31 in 2009 by the federal government.[11] We checked every state's legislation to identify the school cutoff date in the year of 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 how the policy generated two different treatment groups with an 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.

Starting with a control group, students under the previous system with Dec.31st cutoff date 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,

[11]Several states still appealed the decision and kept different cutoff dates.

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, treated students who began their education in 2005 attained 5 years of education and 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.

2.4 Data and Descriptive Statistics

2.4.1 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 2017.

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.

2.4.2 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 and the 5th year students in the base year, 2007. Each column is divided based on the year that the 9-year system was adopted. Column (5) to (8) show the same information for schools and the 9th year students.

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 several 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.

2.5 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.[12] 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:

$$Y_{ist} = \alpha_0 + \alpha_1 treated_{st-j} + \theta_s + \mu_t + \rho X_{ist} + \tau^s_{st} + \epsilon_{ist} \quad (j = 4, 8) \tag{2.1}$$

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.[13]

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

[13] We also have information about school characteristics such as the quality of the classrooms, piped

Lastly, we also include school specific linear time trends, τ_{st}^{s}, 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 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.[14]

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.[15] 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:

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.

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

[15]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.

$$
\begin{aligned}
Y_{ist} = \delta_0 + \delta_1[bmonth < cutoff]_{ist} + \delta_2 treated_{st-j} * [bmonth < cutoff]_{ist} \\
+\delta_3 treated_{st-j} * [bmonth > cutoff]_{ist} + \theta_s + \mu_t \\
+\rho X_{ist} + \tau^s_{st} + \epsilon_{ist} \quad (j = 4, 8)
\end{aligned} \tag{2.2}
$$

where $[bmonth < cutoff]_{ist}$ represents students that have birth months between Jan 1 and the new state-specific cutoff date, and $[bmonth > cutoff]_{ist}$ indicates students born in other months. Note that δ_1 measures the effects of pure age at test given that older students are at most half a year apart from younger students when taking the test.

We next interact each birth month indicator with treated variable to capture the treatment effect on early and normal entrants. By doing so, δ_2 captures the combined effects of an additional year of schooling and school entry age whereas δ_3 captures the combined effects of an additional year of schooling and age at test. Therefore, the difference between δ_2 and δ_3 represents the difference between the effect being one year older at test and the effect of starting school one year earlier.[16]

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

[16]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.

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.

2.6 Estimation Results

2.6.1 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 the 9-year system took the exam, and each point represents the average test score of the year that the test scores were available. Unlike conventional parallel trends, each school in our study has different number of pre-periods given that school have only 6 years of test score data and adopted the policy in different years. Therefore, we use dotted lines to represent pre-test scores before the reform and straight lines to show post-test scores.

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. Although summary statistics show that the schools that adopted the policy in 2010 and 2011 had inferior characterisitcs in several school and student characteristics, level or trends of test scores were similar to the schools that adopted the policy earlier.

Figure 6 shows the results for 9th year students. 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 and 2017, 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).

2.6.2 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 specifications:

$$Y_{ist} = \alpha + \sum_{k=-2}^{2} \beta_k E_{sk} + \theta_s + \mu_t + \rho X_{ist} + \epsilon_{ist} \quad (5th\ year\ student) \tag{2.3}$$

$$Y_{ist} = \alpha + \sum_{k=-3}^{0} \beta_k E_{mk} + \theta_m + \mu_t + \rho X_{imt} + \epsilon_{imt} \quad (9th\ year\ student) \tag{2.4}$$

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.[17]

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

[17]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. Due to compositional issues, we used the event dummies that can be identified with majority sets of school samples.

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 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.

2.6.3 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.

2.6.4 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.[18] Several studies have shown its negative impact on Brazilian students' academic outcomes and earning (Eemerson

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

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.[19] 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 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

[19] The effect on dropout rates is comparable to the effect of another program implemented in Brazil. For example, Glewwe and Kassourf (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.

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.

2.6.5 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.[20]

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 [21]. 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' outcomes.

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

[21] 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.

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 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.[22]

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.

2.6.6 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.

[22] Mani et al. (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.

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.[23] 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.

[23] 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.

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.[24]

[24] 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.

2.7 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 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 other 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.

Figure 2.1: The example of policy implementation

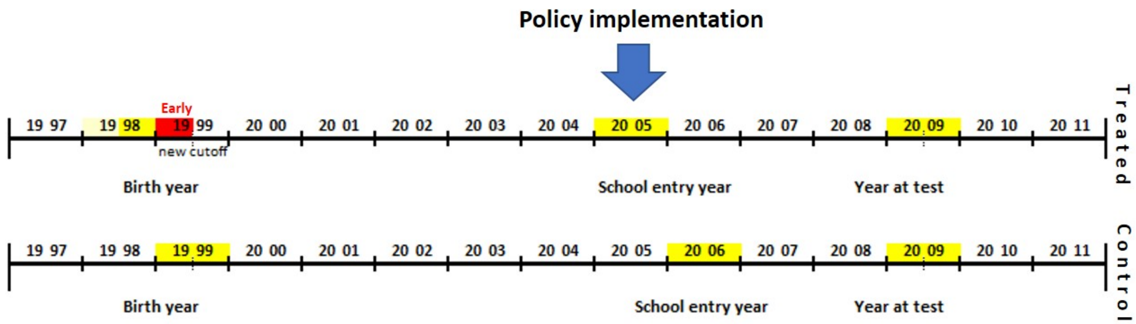


Figure 2.2: The comparison groups for early exposure to compulsory education (Early Entrants)

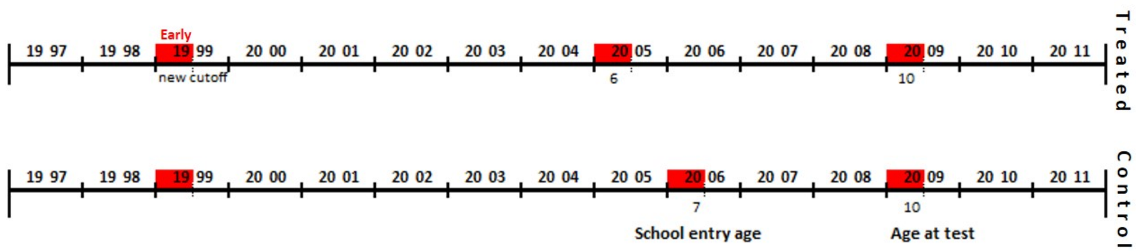


Figure 2.3: The comparison groups for an additional year of schooling and being one year older at test (Normal Entrants)

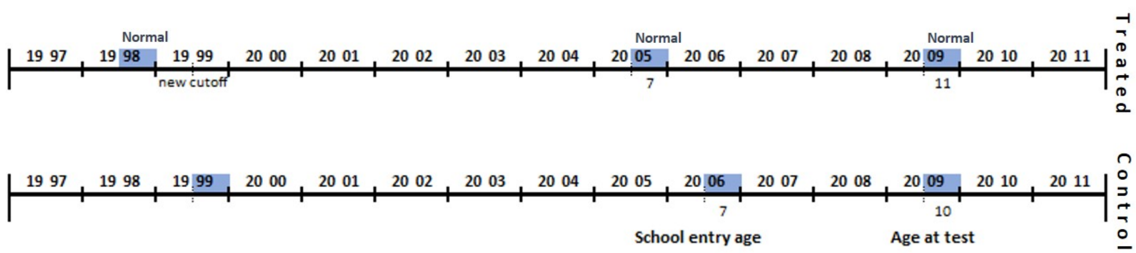
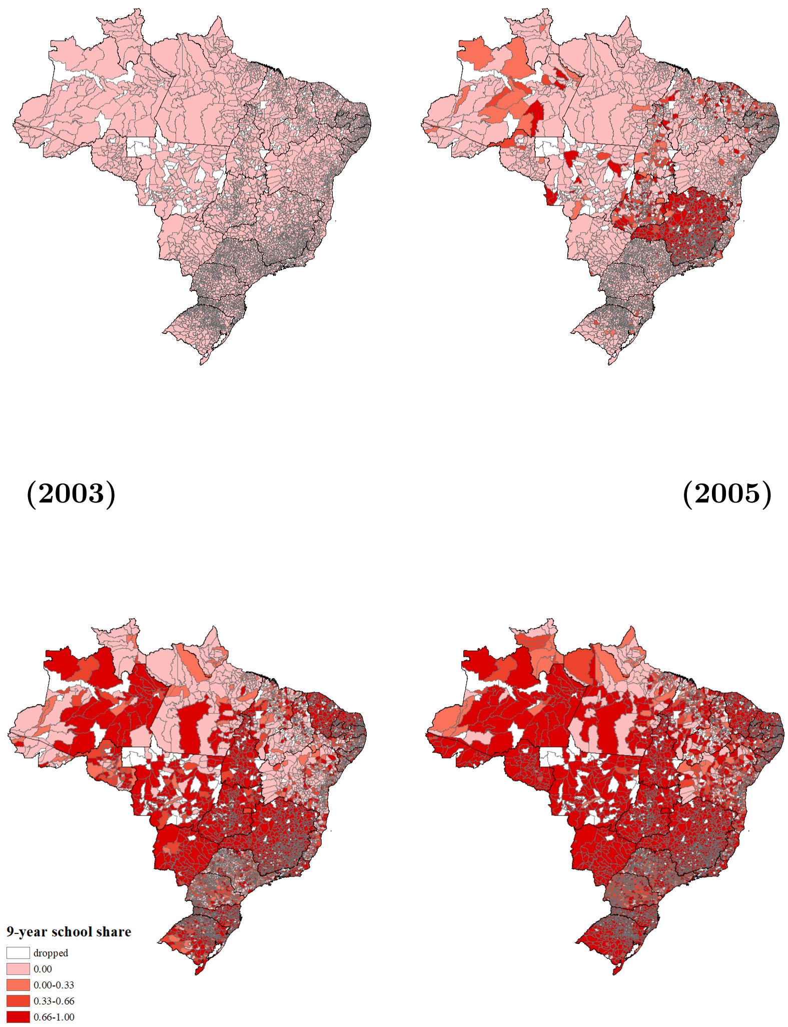


Figure 2.4: Share of schools with the 9-year system from 2003 to 2009

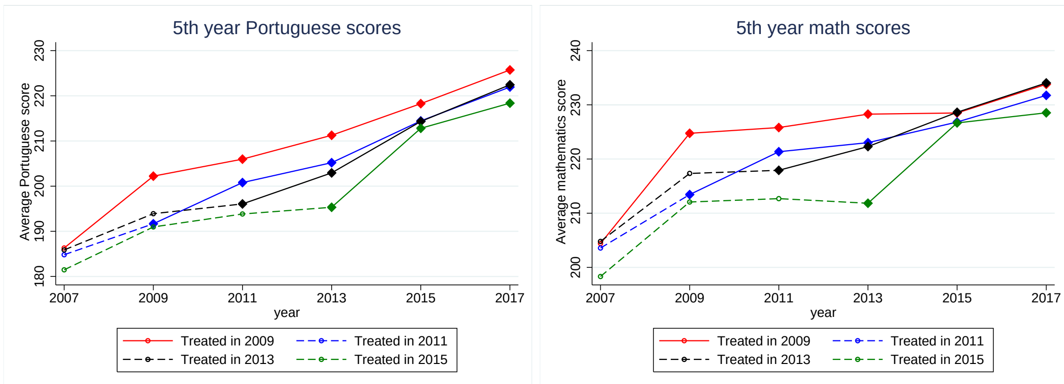


(2003) **(2005)**

(2007) **(2009)**

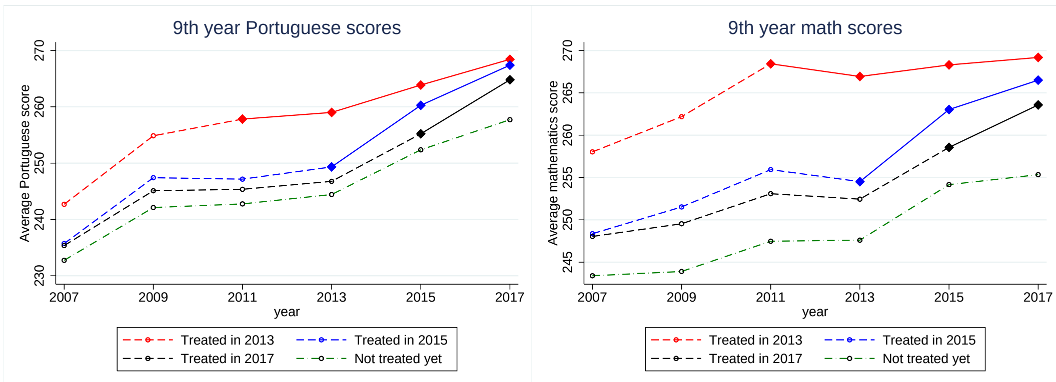
Notes: Municipalities shown in white are excluded in the main analysis because every school in these municipalities had inconsistency between the school duration indicator and enrollment in grade/year.

Figure 2.5: Parallel trend of test scores (5th year students)



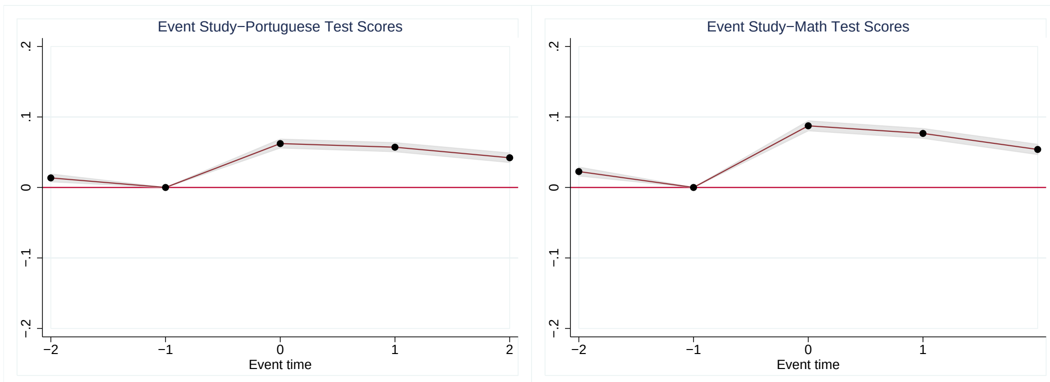
Notes: Schools are divided into groups based on the year that students under the 9-year system took the exam. For example, students who entered primary schools under the 9-year system in 2004/2005 took the 5th year exam in 2009. These groups are denoted as "treated in 2009." Test score data is not available 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 2.6: Parallel trend of test scores (9th year students)



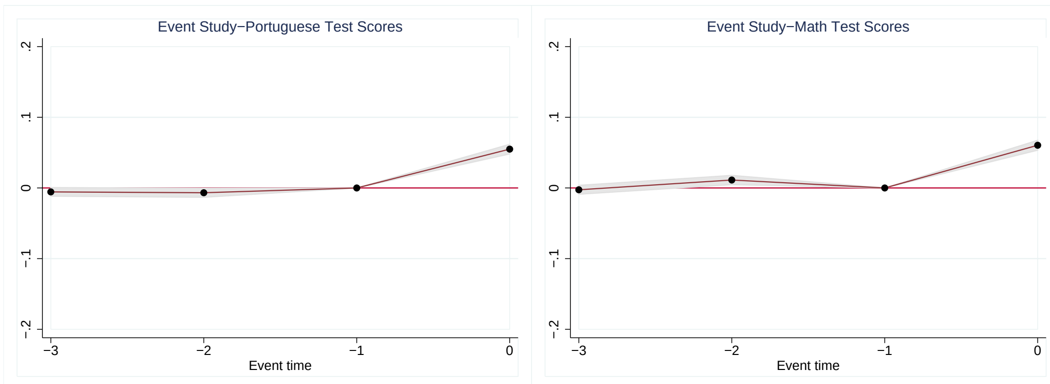
Notes: Schools are divided into groups based on the year that students under the 9-year system took the exam. For example, students who entered primary schools under the 9-year system in 2004/2005 took the 9th year exam in 2013. These groups are denoted as "treated in 2013." Test score data is not available 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 2.7: 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 2.8: 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 2.1: Baseline summary statistics for 5th and 9th year students by year of adoption

		5th year students				9th year students			
		2004&05	2006&07	2008&09	2010&11	2004&05	2006&07	2008&09	2010&11
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of schools	2044	4979	2585	1316	2085	2436	873	857
	Number of municipalities	590	1546	859	352	890	1118	471	392
Stud. characteristics									
	White	0.32	0.45	0.4	0.29	0.35	0.42	0.38	0.33
	Female	0.49	0.49	0.5	0.5	0.55	0.56	0.57	0.56
	Color TV	0.95	0.95	0.95	0.92	0.96	0.96	0.96	0.95
	Refrigerator	0.91	0.93	0.92	0.86	0.93	0.93	0.93	0.90
	Repeat grade	0.30	0.31	0.31	0.41	0.33	0.36	0.35	0.34
	Drop out	0.07	0.06	0.07	0.1	0.07	0.07	0.08	0.08
	No preschool education	0.18	0.2	0.18	0.23	0.19	0.20	0.22	0.18
	Mother w/o primary edu	0.51	0.52	0.52	0.53	0.61	0.55	0.57	0.57
	Father w/o primary edu	0.53	0.53	0.53	0.56	0.65	0.59	0.60	0.61
	Work	0.15	0.15	0.16	0.19	0.27	0.24	0.22	0.23
	Average age at exam	10.69	10.65	10.73	11.15	14.95	14.97	15.06	15.11
	Single parent	0.22	0.19	0.21	0.25	0.20	0.20	0.22	0.23
Sch. characteristics									
	Classroom quality	0.58	0.63	0.61	0.47	0.57	0.58	0.54	0.54
	Piped water system	0.44	0.51	0.49	0.35	0.41	0.45	0.44	0.43
	Electricity quality	0.46	0.54	0.54	0.39	0.43	0.46	0.43	0.46
	Illumination quality	0.88	0.88	0.89	0.78	0.88	0.90	0.87	0.87
	Ventilation quality	0.81	0.82	0.79	0.71	0.84	0.81	0.76	0.78
	Policy exists for violence	0.20	0.27	0.35	0.18	0.24	0.27	0.29	0.33
	Portguese homework checed	0.84	0.84	0.84	0.84	0.88	0.86	0.84	0.84
	Math homework checed	0.85	0.86	0.85	0.83	0.89	0.88	0.86	0.86

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.

Table 2.2: The overall effect of compulsory schooling reform on standardized test scores

	Portuguese	Portuguese	Portuguese	Math	Math	Math
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 5th year students						
Treated	0.092***	0.091***	0.103***	0.111***	0.112***	0.118***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	1,392,887	1,392,887	1,392,887	1,392,887	1,392,887	1,392,887
Panel B: 9th year students						
Treated	0.036***	0.037***	0.063***	0.023***	0.025***	0.060***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N	1,420,218	1,420,218	1,420,218	1,420,218	1,420,218	1,420,218
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 specific time trend	No	No	Yes	No	No	Yes

Notes: Year fixed effects for exam years and school 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 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 2.3: 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	-0.037***	-0.007***	-0.005***	-0.027***	-0.001	0.004**
	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)
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 specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Share in base year	0.26	0.05	0.12	0.30	0.06	0.21
N	1,392,887	1,392,887	1,392,887	1,420,218	1,420,218	1,420,218

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school fixed effects, and school 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 2.4: The effect of the compulsory schooling reform on the test scores of early and normal entrants

	5th year students		9th year students	
	Portuguese	Math	Portuguese	Math
	(1)	(2)	(3)	(4)
Treated*Early	0.067***	0.088***	0.044***	0.047***
	(0.004)	(0.005)	(0.006)	(0.006)
Treated*Normal	0.125***	0.147***	0.071***	0.065***
	(0.004)	(0.005)	(0.005)	(0.005)
Early	0.025***	0.028***	0.015***	0.012***
	(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 specific time trend	Yes	Yes	Yes	Yes
N	1,048,129	1,048,129	1,419,428	1,419,428

Notes: Every specification includes control variables for student characteristics (household appliance ownership, parent's education, sex, race, etc.), year and school fixed effects, and school 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 2.5: 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	0.084***	0.092***	-0.044***	0.013	0.011	-0.002
	(0.016)	(0.016)	(0.009)	(0.019)	(0.019)	(0.011)
N	55,035	55,035	55,035	59,851	59,851	59,851
Share in base year			0.36			0.45
Panel B: With preschool						
Treated	0.048***	0.066***	-0.013***	0.046***	0.047***	-0.021***
	(0.007)	(0.007)	(0.003)	(0.008)	(0.007)	(0.004)
N	269,988	269,988	269,988	335,727	335,727	335,727
Share in base year			0.21			0.28
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 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 fixed effects, and school 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 2.6: 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	0.121***	0.150***	-0.056***	0.072***	0.070***	-0.031***
	(0.01)	(0.01)	(0.006)	(0.011)	(0.011)	(0.006)
N	165,687	165,687	165,687	167,053	167,053	167,053
Share in base year			0.38			0.43
Panel B: With preschool						
Treated	0.122***	0.137***	-0.034***	0.070***	0.063***	-0.03***
	(0.005)	(0.005)	(0.01)	(0.006)	(0.006)	(0.003)
N	708,870	708,870	708,870	846,880	846,880	846,880
Share in base year			0.23			0.28
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 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 fixed effects, and school 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 2.7: Placebo test based on false treatement years (9th year)

	Portuguese	Math
	(1)	(2)
Treated	-0.015**	-0.002
	(0.008)	(0.008)
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School specific time trend	Yes	Yes
N	431,122	431,122

Notes: Year fixed effects for exam years and school 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 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 2.8: Test of change in school characteristics with the policy adoption

	5th year students	9th year students
Piped water system	-0.009	0.013
	(0.009)	(0.015)
(share)	0.49	0.45
Electricity quality	0.004	-0.013
	(0.009)	(0.016)
(share)	0.50	0.47
Illumination quality	-0.013**	0.000
	(0.006)	(0.006)
(share)	0.89	0.90
Ventilation quality	0.003	-0.016*
	(0.008)	(0.008)
(share)	0.82	0.83
Policy exists for Violence	0.005	0.022
	(0.011)	(0.02)
(share)	0.35	0.32
Classroom quality	-0.016	0.016
	(0.01)	(0.017)
(share)	0.62	0.58
Teachers regularly checking	-0.002	-0.009**
Portuguese homework	(0.003)	(0.004)
(share)	0.83	0.85
Teachers regularly checking	0.000	-0.004
math homework	(0.003)	(0.004)
(share)	0.85	0.88
Average class size	0.034	0.027
	(0.107)	(0.204)
(share)	24.69	24.34
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School specific time trend	Yes	Yes
N	32,256	20,292

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.

Table 2.9: Test of change in student characteristics with the policy adoption

	5th year students	9th year students
White students	-0.021***	0.008***
	(0.002)	(0.002)
(share)	0.40	0.40
Female	0.010***	0.003
	(0.002)	(0.002)
(share)	0.49	0.55
Have a car	0.009***	-0.008*
	(0.003)	(0.004)
(share)	0.65	0.58
Have a computer	0.010***	0.000
	(0.002)	(0.003)
(share)	0.38	0.38
Mother less than	-0.003	0.007***
high school degree	(0.002)	(0.002)
(share)	0.43	0.49
Father less than	-0.005***	-0.001
high school degree	(0.002)	(0.002)
(share)	0.43	0.50
Single parent	-0.001	0.002
	(0.002)	(0.003)
(share)	0.21	0.22
School FE	Yes	Yes
Year FE	Yes	Yes
Control variables	Yes	Yes
School specific time trend	Yes	Yes
N	1,392,887	1,420,218

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 fixed effects, and school specific time trends. Each coefficient represents the coefficient on being treated.

Table 2.10: 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	0.106***	0.121***	0.051***	0.036***
		(0.004)	(0.005)	(0.008)	(0.008)
	N	1,144,650	1,144,650	941,103	941,103
Restricted samples 2: drop schools that adopted the policy after 2009 for 5th year students					
	Treated	0.099***	0.107***	0.075***	0.078***
		(0.004)	(0.005)	(0.005)	(0.005)
	N	1,188,379	1,188,379	1,183,043	1,183,043
Restricted samples 3: Schools that have 9 years of primary education					
	Treated	0.127***	0.149***	0.070***	0.079***
		(0.007)	(0.007)	(0.007)	(0.007)
	N	455,997	455,997	667,939	667,939
School FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Control variables		Yes	Yes	Yes	Yes
School 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 fixed effects, and school 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.

Chapter 3

The Effect of Compulsory Preschool Education on Maternal Labor Supply

3.1 Introduction

Recently, several countries have implemented school reforms that lower school entry ages.[1] The major arguments in favor of such reforms hold that public provision of earlier education provides more equitable educational opportunities with potential benefits to students' subsequent academic performance. However, it is important to note that mothers may also be impacted by such a policy. Increased school enrollment among younger children can free up mothers' time, thereby increasing maternal labor market activities.

[1] For example, Mexico and Brazil announced the policy to lower the school entry age to 4 years old in 2004 and 2008, respectively, and France recently lowered the school starting age to 3 years old in 2018.

The current study examines how maternal labor market outcomes were affected by Brazil's 2009 reform that lowered the age of school entry from six to four years old. Under the reform, students were required to start their first year of preschool education if they had turned four years old by March 31st. State or municipal education systems had until 2016 to adopt this change. The compliance rate did not reach 100% by the deadline that the government had set; however, the reform created a significant discontinuity in the probability of school enrollment around the cutoff date. In the present study, I employ a regression discontinuity approach based on the eligibility rule to examine the effect of preschool enrollment on maternal labor outcomes.

There exist several reasons that preschool enrollment could generate significant effects on maternal outcomes in Brazil. First, there was limited space available in the daycare centers that students could attend before preschool begins. A recent survey showed that 45% of families with children younger than four years old wish to enroll their children in daycare or preschool.[2] In the families sampled, 85% of primary-care providers were mothers. Therefore, it is likely that a nontrivial share of mothers wished to enroll their children in daycare or preschool, perhaps in order to engage in labor market activities. Second, the compulsory school reform mostly increased the enrollment rates in public preschools where education is provided free of charge. This fact may have eased the concern of mothers surrounding tuition, which is required by private preschools and daycare programs.

This study separately evaluates the effect of preschool enrollment on mothers inhabiting two types of households: 1) where a 4-year-old child is the youngest in the family member and no other relatives cohabit, and 2) where either a 4-year-old child is not the

[2] Pesquisa Nacional por Amostra de Domiclios (PNAD) in 2015.

youngest in the family member or other relatives, such as grand-parents or brothers/sisters cohabitate. The samples were divided into two groups because mothers' labor market outcomes in the first group could be affected more than those in the second group when their 4-year-old child enrolled in preschool. For example, having an additional younger child to take care of may restrict mother's labor supply even after the preschool enrollment of her 4-year-old children. In addition, mothers who previously used informal childcare provided by grandparents or other relatives may not show a change in their labor market activities if their child's informal care is merely substituted by preschool. Informal childcare may be particularly important in Brazil because a recent study (Attanasio et al. (2017) showed that grandparents play a relatively important role as caregivers.

I found that the 4-year-old students who were born before the cutoff date were approximately 5-8 percentage points more likely to enroll in preschool. Considering that the preschool enrollment rate of ineligible 4-year-olds was approximately 35%, this was about 14-22% increase in probability in relation to the base value. The effect of the eligibility rule on preschool enrollment was observed in both single families without an additional younger child and its complementary group. Despite the significant discontinuity in school enrollment rate, I found no significant effect of preschool enrollment on the maternal labor market outcomes if the child was not the youngest member of the household or there were other relatives present. By comparison, statistically significant changes in maternal labor outcomes were observed among mothers whose 4-year-old child was the youngest and live with no other relatives.

Reduced form estimates indicate that the mothers whose children were eligible for preschool spent 1.8 more hours working and 1.5 fewer hours on household chores. Moreover, these mothers were approximately 3.8 percentage points more likely to take jobs with formal contracts that guaranteed employee rights and benefits. This finding is particularly relevant in many developing countries including Brazil, where a large share of employees, particularly women, often take informal jobs that do not provide access to unemployment insurance, sick benefits, maternal leave, or family wage (Joana Simoes de Melo Costa (2016)). Second-stage estimates indicate that when eligible 4-year-old children were induced to enroll in preschool due to the eligibility rule, the mothers were approximately 45 percentage points more likely to take jobs with formal labor contracts. In addition, average working hours per week also increased about 21 hours and the number of hours spent for household chores decreased by 18 hours.

The current study is related to a number of previous studies that examined the effect of school enrollment on maternal labor market outcomes. Previous studies have reached different conclusions depending on the existing labor market conditions for mothers, the availability of alternative child care, or pre-school enrollment rates prior to policy implementation. Several studies (Fitzpatrick (2010); Havnes and Mogstad (2011); Bettendorf et al. (2015) and Baker et al. (2008)) found a small or no effect of preschool enrollment, and attributed their finding to the pre-existing high maternal employment rate, substitution of informal childcare for preschool, or low elasticity in the female labor supply. By comparison, a set of studies (Fitzpatrick (2012); Goux and Maurin (2010); Cascio (2009) and Carta and Rizzica (2018)) found a positive effect of preschool enrollment on labor market activities among a subset of mothers who were single or who did not have a second, younger child.

Lastly, other studies (Bauernschuster and Schlotter (2015); Berlinski and Galiani (2007); Berlinski et al. (2011); Herbst (2017); and Nollenbergea and Rodrigues-Planas (2015)) found a significant effect of preschool enrollment on their entire sample of mothers.

The current study is one of very few studies that examines the effects of preschool enrollment in a developing country.[3] Previous studies were mostly conducted in the context of developed countries, where several important factors such as female labor supply elasticity or the availability of alternative childcare options for preschool, were different. Furthermore, the current study examined the effects of the compulsory school reform, which was implemented to provide more equal educational opportunities for younger children. Given that there were substantial effects of the policy on maternal labor outcomes, the findings of the study could be considered additional indirect effects when similar policies are implemented in other countries. Lastly, by using detailed descriptions for the types of jobs that mothers had in a reference week, the current study shows that preschool enrollment not only increased maternal working hours, but also impacted the quality of jobs that mothers performed.

The remainder of the paper is organized as follows: Section 2 and 3 provide the context of Brazil's education, institutional details, and description for the data. Then, section 4 presents empirical estimation strategies. Section 5 shows the results with several robustness checks, and section 6 offers a conclusion.

[3]Among the studies introduced in the previous paragraph, Berlinski and Galiani (2007); Berlinski et al. (2011); Attanasio et al. (2017) are the only ones that evaluated the effect of children's school enrollment on maternal labor outcomes in a developing country. Attanasio et al. (2017) is more closely related to this study as they examined the effect of access to free daycare on maternal employment in Rio de Janeiro, Brazil. While their study evaluated the lottery program for daycare access in one state of Brazil, the current study evaluated the school reform and included more than half of the Brazilian states.

3.2 Context

Brazil is one of the few countries in the world that lowered the entry age of compulsory education to 4 years old. Originally, compulsory education in Brazil started at age 7 and had a duration of 8 years. Starting in 2006, the system underwent several changes in an effort to provide more equal educational opportunities. In the first major reform, the Brazilian government lowered the minimum age of school entry from 7 to 6 years old and increased the duration of education from 8 to 9 years. Not long after the first reform, the compulsory school entry age was lowered even further, to 4 years old, in 2009. Under Constitutional Amendment No. 59 and the resolution of the National Council for Education CNE/CEB No. 20, children who turned 4 years old by March 31st were required to start their preschool education. Law No. 12. 796 in 2013 made this change compulsory and mandated states and municipalities to adopt the new system until 2016. The current study used this second reform to examine the effect of preschool enrollment on maternal outcomes.

Piror to the reform, the decision about preschool enrollment depended mainly on available space rather than on particular cutoff dates. Public preschools were not legally allowed to reject children unless there was not enough capacity (Bastos and Straume (2016)). Therefore, when the new cutoff date was proposed at the beginning, it ignited great controversy, particularly among the parents whose 4-year-old children barely missed enrollment. Several state governments were also against the reform, believing that such a policy could violate the educational rights of younger children who missed the age cutoff. Because the eligibility rule was controversial and Brazil has a very decentralized educational system, there was certainly variation across states or municipalities in how strictly the rule was enforced. Nevertheless, the reform still had significant effects on the probability of school enrollment around the cutoff date before 2016, as will be shown in later sections.

Preschool education in Brazil is provided by both public and private sectors. According to Brazil's 2013 school census data, approximately 70% of preschool education was provided by the public sector and 30% by the private sector. The public sector includes federal, state, and municipal governments; municipal governments provide most of public preschool education, with the other two subsectors providing less than 1% of preschool education. One of the major differences between public and private schools is the tuition. Public education is provided free of charge, whereas private education requires tuition that varies by region.

The academic year for preschool, as other stages of education, begins in September and runs through June. It is year-round and has major school breaks during the holiday seasons for Christmas, Carnival, and Easter. A majority of preschools have instruction for approximately 4 or 5 hours per day, and a small share of schools provide full-time instruction consisting of 8-9 hours or more.[4] Furthermore, like many other developing countries such as Mexico, India, or Russia, many Brazilian schools adopted a double-shifting system in which different classes of students share the same curriculum and building at different times in the day. Therefore, preschool instruction is mostly provided either in the morning (i.e., 7 or 8 am) or afternoon (i.e., 12 or 1pm).

[4]According to 2013 school census data, approximately 85% of preschools had equal to or less than 5 hours of instruction per day.

3.3 Data and Descriptive Statistics

The major dataset for the analysis is from PNAD, Brazilian national household sample survey from 2011 to 2015. It collects socio-demographic characteristics of household information over 300,000 individuals and 10,000 households every year. Because the data provide every household member's exact birthday with the identifier of mother for each child, I could identify the child that is eligible for preschool entry, and his/her mother. In addition, the data provide detailed information about mothers' labor market outcomes such as the average number of hours spent working and on chores each week, as well as whether or not the mother worked during a given reference week. Among the mothers who worked during a given reference week, I further divided their status based on whether they worked with or without formal labor contracts. In this study, I defined formal contracts by the possession of a formal labor card (CTPS) registered with an employer. A nontrivial share of employees in Brazil hold jobs without formal registration of the labor card, which results in limited employee benefits and often unfair treatment in the work environment.

For the current analysis, several states were excluded because they either enforced much later cutoff dates or were more linient toward to the eligibility rule. First, a few of states believed that enforcement of the cutoff date violated the educational rights of children who barely missed the cutoff. As a result, these state governments successfully appealed the federal government's decision and used the cutoff date, Decmber 31st. I excluded these states because their cutoff date was much later than the newly enforced cutoff dates.[5] In addition, states that leniently enforced the cutoff date were not included in the analysis.[6]

[5] I excluded states, such as Sergipe, Mato Grosso do Sul, Paraba and Mato Grosso for the current analysis because they eventually adopted Dec.31st cutoff date between 2014 and 2015.

[6] Santa Catarina allowed primary school enrollment as long as individuals possess sufficient capacity.

Table 1 shows the summary statistics of the samples used in the current analysis. The columns are first divided by the two groups explained earlier; single families where a 4-year-old child is the youngest and the households where a 4-year-old child is not the youngest or relatives present. Each column is then further divided by the birth months of children; children become 4 years old before the cutoff date, March 31st (older children), and after the cutoff date (younger children). In the group of single families without any younger child, the probability of children being enrolled in preschool is approximately 17 percentage points higher among older children. The difference of preschool enrollment rates suggests a potential discontinuity around the cutoff date.[7] Next, the table shows similarities of predetermined mothers' characteristics such as education, race, or age between mothers who have older and younger children. I tested the smoothness of these characteristics around the cutoff date analytically and graphically in the next section. Family size, number of children in different age groups, and number of observations also show the similarity between the two groups.

The summary statistics for the households, in which 4-year-old child is not the youngest or other relatives present, show generally similar patterns as those in its complementary group. Preschool enrollment rate is again approximately 13 percentage points higher for eligible children whereas predetermined mothers' characteristics generally do not

Also, Rio de Janeiro and Paran enforced Dec 31st cutoff date for primary school enrollment. Given that the similar rule was usually applied to preschool enrollment, these states were not included in the current study. Lastly, Tocantins was not included because it also allowed unrestricted access to preschool education regardless of the age at the cutoff date.

[7]Note that the preschool enrollment rate for eligible children was still far from 100%. There was still a need to increase the capacities of preschools, and many 4-year-old students who were eligible for preschool entry were enrolled in daycare. However, no discontinuity was found in the probability of daycare enrollment as the reform affected only preschool enrollment policies. Daycare enrollment around the preschool cutoff date is examined in the Appendix Figure 1.(b).

show much difference. Continuities of maternal characteristics for this group are also tested both analytically and graphically in the next section.

3.4 Empirical estimation

The current study uses a regression-discontinuity design to find a causal relationship between children's preschool enrollment, which was induced by the school reform, and maternal labor market outcomes. Conventional OLS estimation that measures the relationship between these two outcome variables is potentially problematic as mothers often simultaneously decide their children's school enrollment and participation in labor force. In addition, more career-motivated women possibly decide to send their children to schools and participate in workforce.

To delve into the causal relationship, I used the cutoff date for preschool entry enforced by Brazilian government as an instrumental variable. Specifically, the following estimation strategy is used in the first stage:

$$C_{ist} = \beta_0 + \beta_1 1[D_{ist} \leq 0] + f(D_{ist}) + X_{ist}\theta + \mu_t + \gamma_s + \epsilon_{ist} \tag{3.1}$$

where C_{ist} is a variable indicating preschool enrollment of a child i in state s in year t. It is assigned one if he/she enrolled in preschool and otherwise zero. $1[D_{ist} \leq 0]$ is an indicator variable, which is equal to one for 4-year-old students born between October 1st and March 31st and zero for those students born between April 1st and September 31st.[8] Students are more likely to enroll in preschool if the indicator variable equals to one as they are eligible for preschool entry under the compulsory school reform. $f(D_{ist})$ is a quadratic

[8]PNAD data are collected every September of the year. Therefore 4-year-old students born after March 31st are not eligible for preschool entry even though they turned 4 by the time of survey.

polynomial function of the running variable D_{ist} that can vary on either side of the cutoff date. The running variable indicates the distance between child's birth day and the school cutoff date. For example, D_{ist} is -182 for the children born in Oct 1st as they were 182 days far away from the cutoff date, March 31st. Similarly, +183 is assigned for D_{ist} if the children were born in September 31st.[9] X_{ist} includes individual specific characteristics such as mothers' age, age squared, race, and child gender.

The primary coefficient of interest is β_1, which captures the discontinuity in the probability of school enrollment. As students who were born before March 31st were more likely be enrolled in school, β_1 is expected to be positive. However, the estimate will be smaller than one, which indicates the fuzzy regression discontinuity design. There are several reasons that this cutoff date does not completely bind for the groups of the students, before and after the cutoff date. First, given that the deadline for fully implementing compulsory preschool education was 2016, schools and municipalities were gradually increasing the enrollment of preschool for eligible children between 2011 and 2015. Eligible children might not be enrolled in preschool during this period for reasons, such as limited space of preschool, slower development, or availability of other care options, such as grandparents.

In the second stage, I estimate the following equation with restricted samples of the women in ages between 19 and 50 whose children were analyzed in the first stage equation:

$$Y_{ist} = \delta_0 + \delta_1 \hat{C_{ist}} + f(D_{ist}) + X_{ist}\rho + \mu_t + \gamma_s + v_{ist} \tag{3.2}$$

[9]The state government of Sao Paulo chose June 30st, and the state government of Minas Gerais changed its cutoff date from March 31st to June 30st in 2013. I included these states and rescaled the running variable between students' birth dates and the cutoff date for these two states to make them compatible with those of other states

Where Y_{ist} represents labor market outcomes of the women, such as labor force participation, working status in a reference week, average working hours per week, and average hours spent on house chores. $\hat{C_{ist}}$, preschool enrollment of 4-year-old child, is predicted by an instrumental variable using school cutoff date. δ_1 represents local average treatment effect of child being enrolled in preschool on maternal employment rate.

The underlying assumption for validity of above strategy is that maternal outcome variables would be continuous if there was no discontinuity of school enrollment around the cutoff date. This assumption could be violated if mothers were able to manipulate the children's birth month for their preschool enrollment. In the next sections, I conducted several tests to test the assumption. First, I used the McCrary test whether there exists a discontinuity around the cutoff date in the density of mothers whose children are four years old. Next, I tested graphically and analytically whether mothers' socio-economic characteristics such as mother's year of schooling, race, and age are continuous around the cutoff date.

3.5 Estimation Results

3.5.1 Graphical analysis

This section first presents graphical evidence supporting the validity of regression discontinuity design. What follows is a visualization of how preschool enrollment rates and maternal labor market outcomes were affected by the cutoff date. To test the validity of empirical estimation, I examined whether there were discontinuities in mothers' predetermined characteristics (e.g., years of education, race, and age). I also used the McCrary test to check whether there was a discontinuity in the distribution of mothers around the cutoff date. These validity tests were conducted for the following subsamples: single families without a younger child and its complementary group (i.e., families with an additional younger child and/or other relatives in the household). In each graph, I draw fitted line using quadratic polynomials and 95% confidence interval with dotted lines. Each dot represents the average value of the outcome variable in 7 days of birthday bins.

First three graphs in Fig. 1 show the continuity in the predetermined maternal characteristics in single families without a younger child. There is no evidence that any of the observable characteristics differed before and after the cutoff date. Also, the McCrary test for the same subgroup does not show any discontinuity in the distribution of mothers across the cutoff dates. These results are reassuring that mothers in the single families were not likely to manipulate their children's birth dates. The next three graphs in Fig. 2 show the trends of the same maternal characteristics in families with an additional younger child or other relatives. Unlike the trends observed in the first group, mothers' age show potential discontinuities across the cutoff date, whereas mothers' years of education and the share of white mothers show a smooth trend over the running variable.

The potential concern regarding the finding among mothers living with an additional younger child or other relatives is the possibility that younger mothers manipulated their children's birthdates for the school eligibility. However, this is less likely to happen for several reasons. First, we do not see the same trend among mothers in single families without a younger child. If younger mothers had manipulated eligibility, the same trend should have been observed in the first group of mothers.

Next, if eligibility had been manipulated, there should have been higher density in mothers' population before the cutoff date. The McCrary test in Figure 2 shows the opposite result; there was higher density after the cutoff date. This finding reduces the concern that a greater number of younger mothers changed their children's birth dates to ensure preschool eligibility. Lastly, any potential problems associated with sample selection or eligibility manipulation might have exaggerated the effect of preschool enrollment on mothers' labor market outcomes. However, there was no effect of preschool enrollment among mothers living in multi-families or living with an additional younger child. As will be shown in the next section, I found significant effects on mothers' labor market outcomes only among mothers living in single families without an additional younger child, which had no discontinuities in any of the pre-determined maternal characteristics. Given the listed reasons, the discontinuity observed in one of the multiple mothers' characteristic is less likely to bias the estimates of the current study.

Next, Figures 3 and 4 illustrate the discontinuity in school enrollment rates and maternal labor outcomes at the cutoff date. In Figure 3, the upper figure presents the school enrollment rates among 4-year-olds in single families without an additional younger child, and the lower figure shows the school enrollment rates among 4-year-olds who are not the youngest in the household or live with other relatives. Both figures show a clear discontinuity in school enrollment rates around the cutoff date, regardless of the type of households that 4-year-olds reside. Students who turned 4 after the cutoff date were much less likely to be enrolled in preschool compared to their older counterparts. Given that preschool education became compulsory for every 4-year-old child, there existed similar trends in preschool enrollment rates across the two groups. This expectation was confirmed in Figure 3.

Figure 4 presents the relationship between the maternal labor market outcomes and the birth dates of children. Fig 4. (a) show suggestive evidence that child's preschool enrollment affected maternal outcomes among mothers who do not have an additional child or live with other relatives. If their children were eligible for preschool entry, the mothers were more likely to participate in the labor force, spend longer hours for working, and spend less hours on household chores. However, as will be shown in the next section, the discontinuity observed in the labor force participation is not statistically significant whereas the discontinuities for the other two variables were statistically significant. In Figure 5. (a), I further examined whether there exist changes in the occupation that mothers took in a reference week among the same group of mothers. The figures indicate that if their children were eligible for preschool entry, the mothers were more likely to take a job with formal labor contract and less likely to take a job without the contract.[10] By comparison, Fig 4. (b) and Fig 5. (b) present that the cutoff eligibility rule did not influence the labor market outcomes of the mothers who have an additional younger child or live with other relatives. All of the maternal labor outcomes showed a continuous trend across the cutoff date. Despite the significant effect of the eligibility rule on school enrollment rates in both types of households, maternal outcomes exhibit discontinuity only among mothers in the first group.

[10]Mothers who were public employees, self-employed, and employers were separately analyzed in the appendix but the school eligibility rule did not make significant changes in these statuses. None of the estimates for discontinuities were statistically significant. I did not analyze mothers who worked to produce goods for their own consumption, worked as construction workers to repair their own house, and were unpaid jobs because these mothers were not paid from these jobs. Lastly, mothers who worked in the military were not analyzed as they represent less than 0.1% of the entire sample.

3.5.2 Regression results

Previous sections provided a graphical analysis for the validity of regression discontinuity design and the relationship between preschool eligibility rule, school enrollment rates, and maternal labor market outcomes. This section provides analytical evidence that supports the results shown by graphical analysis. Like previous graphical analysis, separate estimates were drawn for each labor market outcomes between two different groups of mothers.

First, Table 2 shows the results that compared the observable characteristics of mothers across the cutoff date. Eq. 1 is used to test the smoothness of predetermined characteristics. As confirmed in graphical analysis, most of the observable characteristics do not change significantly across the cutoff date. Among the group of mothers living with relatives or an additional younger child, mothers whose children were eligible for preschool were approximately 0.3 years younger than their counterparts. However, this is less likely to be a result of sample selection for several reasons stated in the previous section. Overall, Table 2 does not support that mothers with certain observable characteristics manipulate children's birthdates around the cutoff date.

Table 3 reports the estimates in the first stage of regression and reduced form estimates. All the estimates were calculated with state and year fixed effects and the control variables such as mothers' years of education, age, and race. First column in Table 3 reports the estimates for the effect of school eligibility rule on school enrollment rate for each group of mothers. As shown in graphical analysis, both estimates show that the eligibility rule for preschool entry significantly increased the school enrollment rate for preschool-eligible children. A child that turned 4 years old before the cutoff date is approximately

5-8 percentage points more likely to enroll in school compared the child who turend 4 years old after the cutoff date. Considering that the average school enrollment rate between 2011 and 2015 for non-eligible children was approximately 35%, the estimates indicated around 14-22 percentage increase of the enrollment rate.

Next columns from (2) to (6) show the reduced form estimates, which indicate intent-to-treat effects. Despite the significant discontinuity in school enrollment rate, there were no significant effects of the preschool eligibility rule on any maternal labor market outcomes if the child was not the youngest member of the household or there were other relatives present. This finding indicates that either mothers still faced constraints due to the presence of a younger child, or informal childcare was substituted by preschool enrollment. By comparison, statistically significant effects on most of maternal outcomes were observed among the mothers in single families without an additional younger child.

Mothers whose children eligible for preschool entry did not exhibit statistically significant change in external margin, labor force participation. However, there were statistically significant changes observed in internal margin. When children were eligible for preschool, their mothers were approximately 3.8 percentage points more likely to work with formal labor contract in a reference week and 4.4 percentage points less likely to work without formal labor contracts. Considering that aproximately 20% (14%) of mothers with preschool ineligible children work with (without) formal contract, preschool enrollment significantly affected the probability of working with(without) formal contract. It is also important to note that although there is almost one to one relationship between these two variables, this result is not guaranteed as mothers might have different occupations in a reference week such as public employees or self-employed. Given that there was no significant changes in the participation of such occupations, this finding suggests that mothers potentially took better quality jobs when their children were eligible for preschool entry.[11]

There were also statistically significant changes of average working hours per week and weekly hours spent for household chores among mothers whose 4-year-old children were the youngest and no other relatives present in the household. Preschool eligibility increased average weekly working hours by 1.8 hours and decrease hours spent on chores by 1.5 hours. Given that mothers with preschool ineligible children spent hours working and on chores approximately 17 and 30 hours, respectively, preschool enrollment increased working hours approximately 10% and decreased hours on chores by 5%.

Table 4 rescaled the intent-to-treat effect by dividing the estimate obtained in the first stage result. There was again the statistically significant effect of preschool enrollment only in the first group of mothers. When 4-year-old children were induced to enroll in preschool due to the eligibility rule, the mothers were approximately 45 percentage points more likely to take the jobs with formal labor contracts and 53 percentage points less likely to take the jobs without formal contracts. Moreover, average working hours per week increased by 22 hours and hours spent on household chores decreased by 18 hours. These are considerable changes compared to the findings of previous studies. Berlinski et al.(2011) found that preschool attendance of youngest 4-year-old children in households increased mothers' weekly working hours by 7.8 hours in Argentina. Given that the preschool program in Brazil usually offers 4 hours of instruction per day and 20 hours for a week, 22 hours increase in working hours is consistent with the time that is freed for eligible mothers.

[11] Discontinuities of the other occupations in a reference week were examined in the Appendix Figure 1.(a).

3.5.3 Heterogeneous effects

Table 5 reports the heterogeneous effects of preschool enrollment on maternal labor market outcomes based on mothers' education, age, and survey year. Given that there were significant effects of preschool enrollment only among the mothers in single families without a younger child, this section focuses on this group of mothers. The first two columns represent the effect of preschool enrollment based on the completion of compulsory education. The estimates suggest that mothers with higher education were more likely to take the jobs with formal contract and spent a significantly higher number of hours working during a reference week if their children were eligible for preschool. Columns (3) and (4) present the heterogeneous effects based on maternal age. Similar patterns were observed in younger mothers. They were more likely to take formal employment and spent longer hours working than relatively older mothers if their children enrolled in preschool.

Lastly, I measured the heterogeneous effects depending on the survey year. There are several reasons why the effect of preschool enrollment could differ by survey years. For example, it is possible that the cutoff date was loosely enforced at the beginning stage of educational reform and then was more strictly enforced in later years if we consider that 2016 was the year set by the government to complete the transition. Also, municipalities or state governments could have more rapidly increased the school capacity as the deadline for universal preschool education was approaching.

In later survey years, we also might expect the increase of preschool enrollment of the children whose mothers have no intention to participate in labor market activities as the reform made preschool education compulsory. The last important factor is economic recession, which plagued Brazil in 2014. The country experienced one of the most severe economic downturns in its history, and this could have suppressed the response of mothers' labor market outcomes in later years. Considering all these factors, there exist possibilities that the effects of preschool enrollment in earlier years to be different than those in later years. The columns (5) and (6) suggest that the effect of preschool enrollment was quite similar across years but statistically significant in earlier years.

3.5.4 Robustness checks

This section provides multiple robustness checks to show that the results of the study are robust to alternative specifications. I test the robustness of the results with varying polynomial degrees and window sizes of samples. In addition, I conduct placebo tests by running the same analysis for three- and five-year-old children as well as assigning multiple false cutoff dates.

I first test the results with varying window sizes. I started the analysis with mothers whose children were born 60 days before and after the cutoff date, then reran the analysis with more individuals by adding 15 days to each side of the cutoff date. Figure 6 presents the second-stage coefficients for each outcome variable over the number of days from each side of the cutoff date. Overall, the results show that the estimates were robust to different sample sizes.

In the next step, I obtained the estimates for each outcome with different polynomial degrees of the running variable. The main specification used the second polynomial degrees, but I tested whether the estimates were robust to the alternate polynomial degrees. Figure 7 shows that the estimates for each outcome were robust to the use of different polynomial degrees. Furthermore, I checked whether the cutoff date for preschool education had effects on school enrollment rates for three- and five-year-olds and their mothers' labor outcomes. This served as a placebo test because I expected to observe no discontinuity in their school enrollment or their parents' maternal outcomes around the preschool cutoff date. It is known that some daycare programs, which were designed for children between the ages of 0 and 3, apply the same eligibility rule for their enrollments; however, this decision is more likely to be optional as there is no legislation mandating the same cutoff date for enrollment. The preschool eligibility rule was also less likely to make an effect on school enrollment of five-year-olds because every five-year-old students are eligible for preschool entry. To compare the main outcomes of the study, this robustness check used mothers whose three- and five-year-old children were the youngest in the household and there were not other relatives present.

Figure 8 shows the graphical results of the first and second stage for 3-year-old children and their mothers' maternal outcomes, respectively. Figure 7. (a) shows that school enrollment rates did not change significantly around the cutoff date of March 31st. Although some public daycare programs used the same cutoff date, there is no evidence that the children who turned 3 before March 31st were more likely to enroll in school than three-year-olds who turn 3 after March 31st. Next, Figure 7.(b) examines maternal labor market outcomes over the distance between three-year-old children's birth dates and the cutoff date. As expected, there was no discontinuity in school enrollment rates and maternal outcomes. Figure 9 provides the similar graphical results for 5-year-old children and their mothers' maternal outcomes. Nearly 70% of 5-year-old children enrolled in preschool and there was no discernable discontiunuity in enrollment rates around the cutoff date. Mothers' labor market outcomes also do not exhibit noticeable discontinuities, and none of the estimates for the discontinuities were statistically significant and robust with the inclusion of different polynomial degree.

Lastly, I conducted multiple sets of placebo tests by assigning false cutoff dates. For each regression, I used a one-year window with 6 months before and after the cutoff date. I ran the analysis with this true cutoff date, then reran it 180 times with the false cutoff dates, which increased by one day from the true cutoff date until September 31st. Figure 10 shows the distribution of all estimates for each outcome obtained from the placebo test. For the distribution of every outcome variable, it was clear that the estimate obtained from assigning the true cutoff date was distinguishable from other estimates that were obtained from assigning the false cutoff dates. The true estimates mostly indicated larger effects on maternal outcomes, which suggests that the results of the current study were less likely to be driven by chance.

3.6 Conclusion

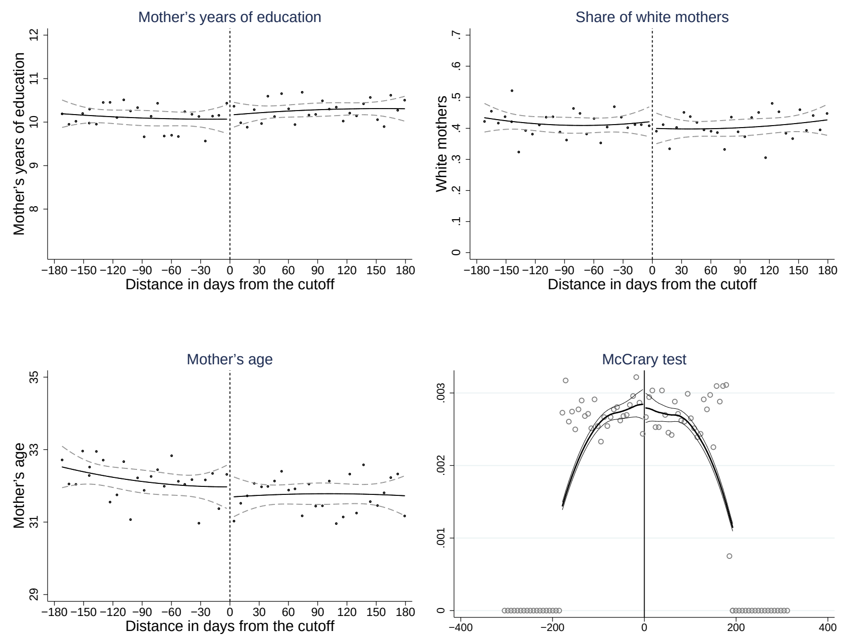
The main purpose of lowering the entry age to compulsory education is to provide equal educational opportunities for children. Although the main focus of the policy is on students' outcomes, there are potential effects on maternal labor outcomes that result from increasing school enrollment rates among younger children.

In Brazil, the policy of lowering the school starting age from 6 to 4 and mandating preschool enrollment among children who turned 4 by March 31st significantly increased school enrollment of eligible 4-year-old children. Consequently, mothers were more likely to take a job with formal labor contracts, to increase their weekly hours of working, and to decrease their hours spent on chores if their preschool-eligible 4-year-old children were the youngest family members and no other relatives were cohabiting in the household. By comparison, there were no effects of preschool enrollment on maternal labor market outcomes if the child was not the youngest or other relatives were present in the households. To support the main findings of the paper, I also ran a battery of robustness checks and did not find evidence that the results were driven by the particular setting of the RD design.

The present study was limited by the inability to evaluate the effect of the reform on students' outcomes. In Brazil, there is currently no available data that assesses outcomes (i.e., academic performance or emotional stability) for preschool or early primary school students. To evaluate the impact of the policy on the main targeted group, more time is needed for the treated students to advance to the point where the data are available. Previous studies have shown inconclusive results regarding the impact of lowering the school starting age on students' short-and long-term outcomes. Moreover, the effect could vary significantly depending on the context or the extent to which the entry age is lowered. Therefore, future studies should examine how this policy affects students' outcomes.

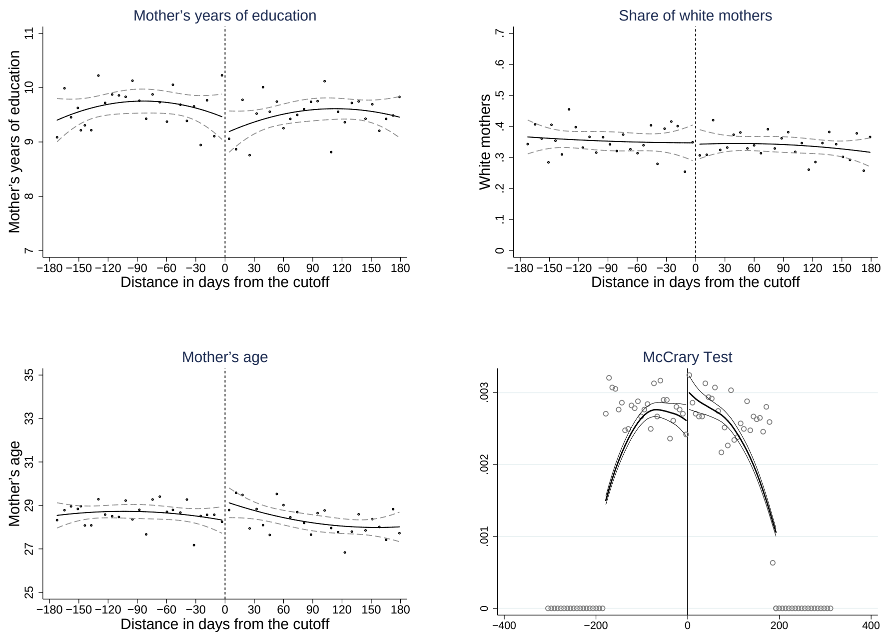
In a nutshell, the present research provides important insights into the substantial indirect effects of the policy on maternal labor outcomes. Considering that developing countries with limited availability of early childhood education often lower the school starting age, this finding adds an important piece of evidence for cost-benefit analysis in policy decision-making.

Figure 3.1: Validity of regression discontinuity design (youngest child and no other relatives)



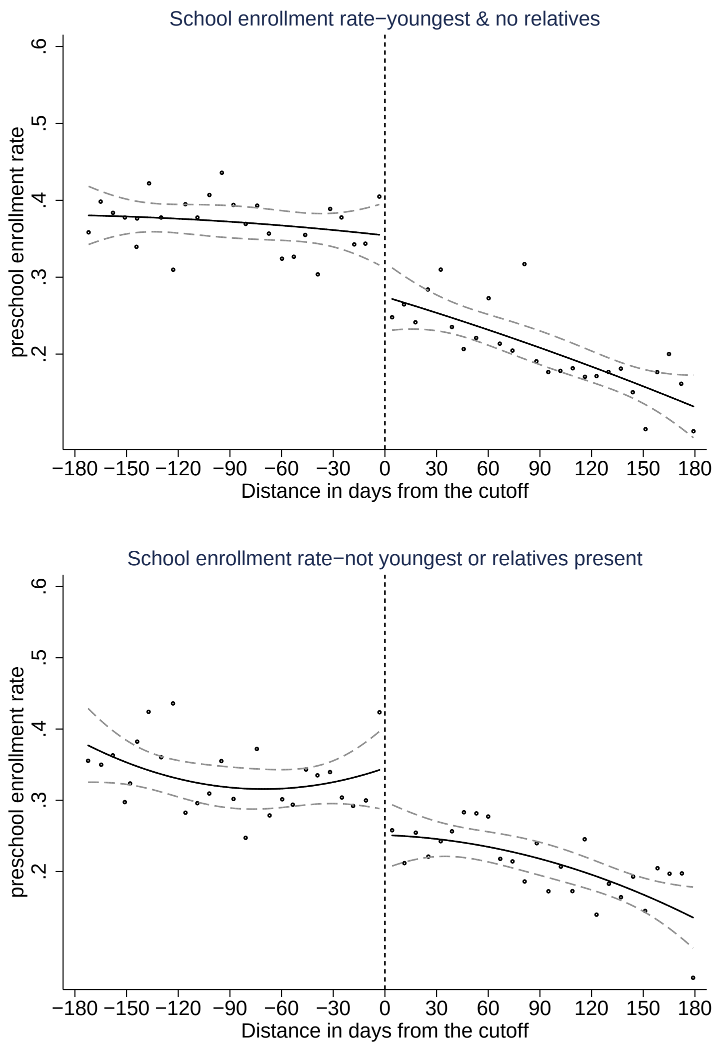
Notes: The first three graphs show the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins. McCrary test shows the density of sample across assignment variable on both sides of the cutoff date, March 31st.

Figure 3.2: Validity of regression discontinuity design (not youngest child or other relatives present)



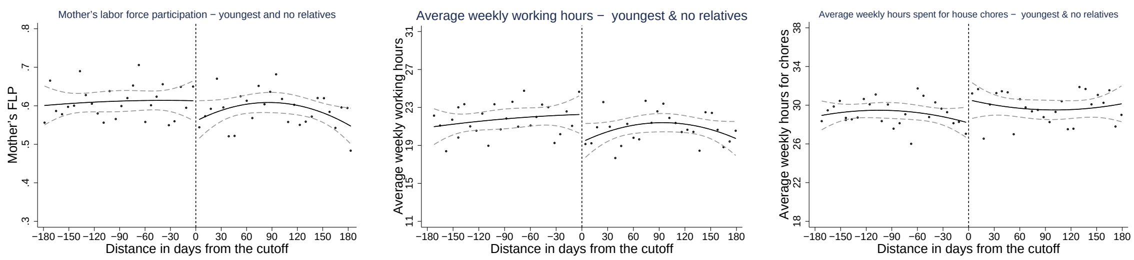
Notes: The first three graphs show the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins. McCrary test shows the density of sample across assignment variable on both sides of the cutoff date, March 31st.

Figure 3.3: First Stage-graphical analysis

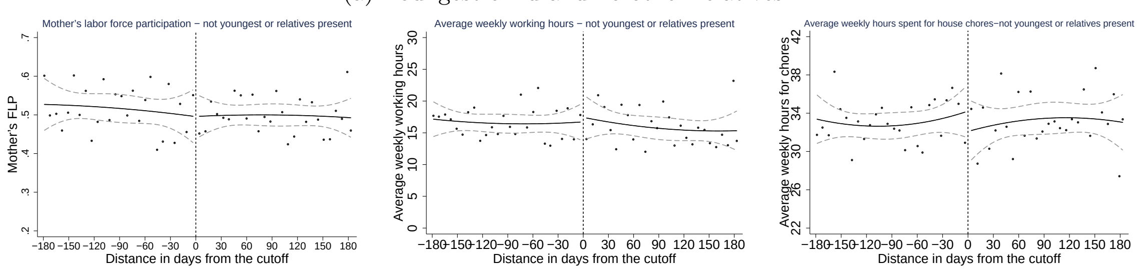


Notes: The figure shows the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of school enrollment rates in 7 days of birthday bins.

Figure 3.4: Second stage-graphical analysis 1

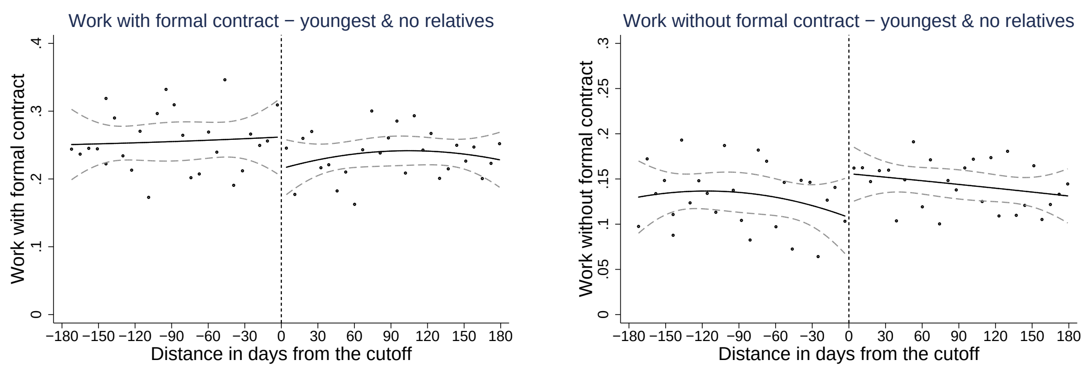


(a) Youngest child and no other relatives

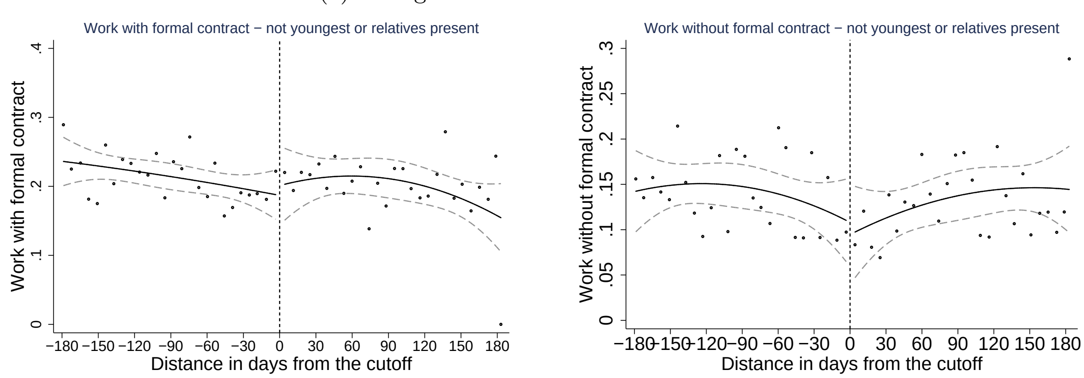


(b) Not youngest child or other relatives present

Figure 3.5: Second stage-graphical analysis 2



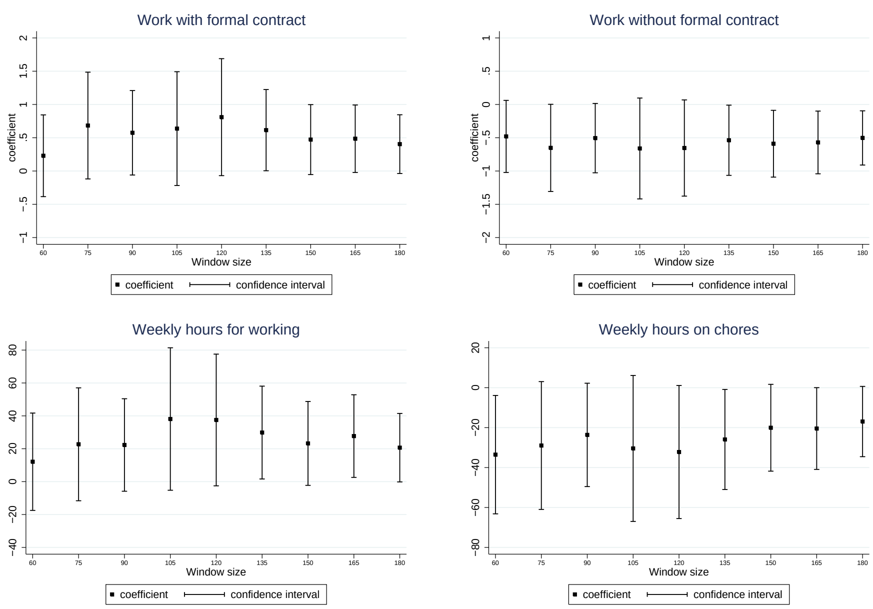
(a) Youngest child and no other relatives



(b) Not youngest child or other relatives present

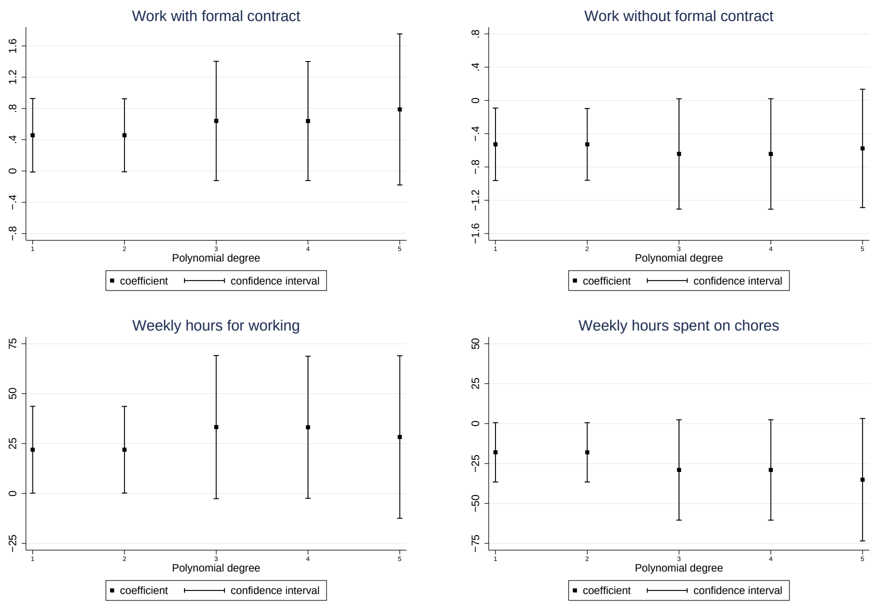
Notes: The figure shows the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins.

Figure 3.6: Specification-different window sizes



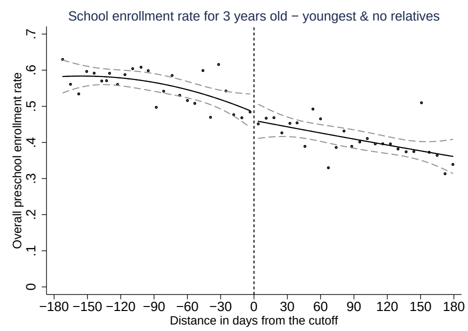
Notes: The horizontal axis shows the varying degrees of window size, and each dot represents the coefficient of second-stage regression with 95% confidence interval.

Figure 3.7: Specification-polynomial degrees

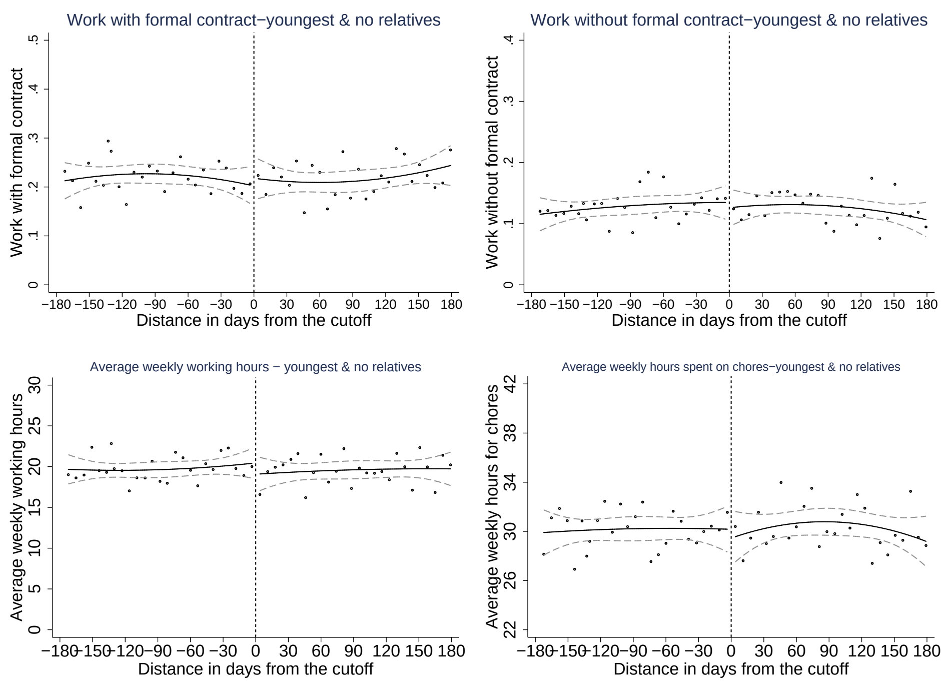


Notes: The horizontal axis shows the varying degrees of polynomial degrees used in estimate equation, and each dot represents the coefficient of second-stage regression with 95% confidence interval.

Figure 3.8: Effects of the school enrollment of 3-year-olds on maternal outcomes



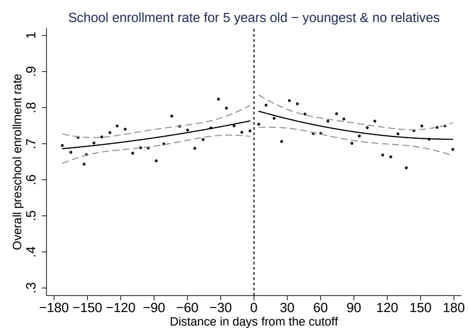
(a) school enrollment rates for three year olds



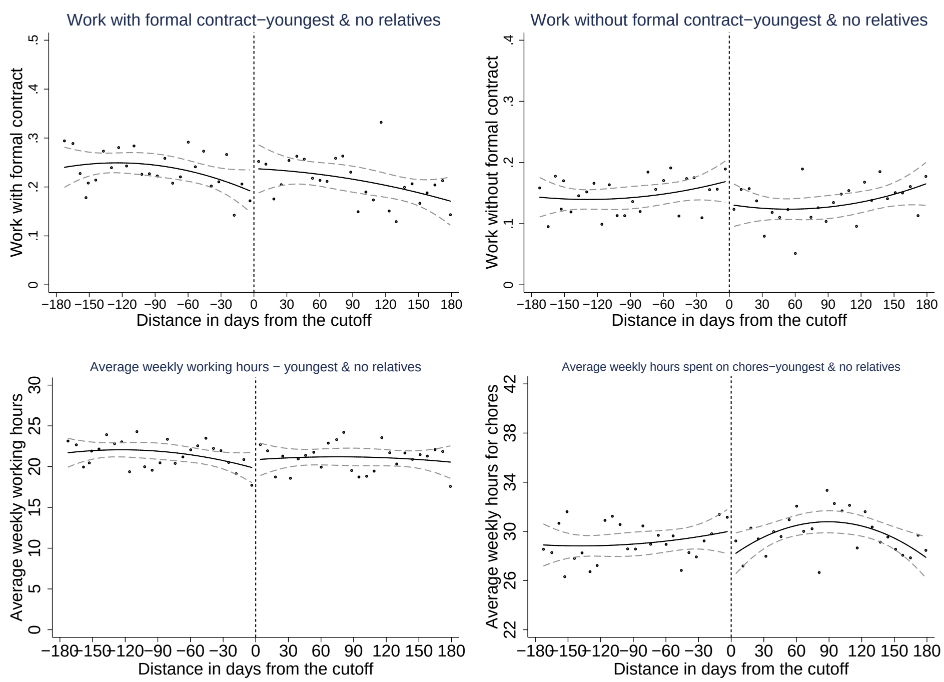
(b) Labor market outcomes of mothers

Notes: The figure shows the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins.

Figure 3.9: Effects of the school enrollment of 5-year-olds on maternal outcomes



(a) school enrollment rates for three year olds



(b) Labor market outcomes of mothers

Notes: The figure shows the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins.

Table 3.1: Baseline summary statistics for 5th and 9th year students by year of adoption

	Youngest & no other relatives		Not youngest or relatives present	
	Born Oct.1st - Mar.31st	Born Apr.1st - Sep. 31st	Born Oct.1st - Mar.31st	Born Apr.1st - Sep. 31st
	(1)	(2)	(3)	(4)
Child enrolled in preschool	0.37 (0.48)	0.20 (0.40)	0.34 (0.47)	0.21 (0.41)
Mother's years of education	10.10 (3.94)	10.27 (3.80)	9.64 (3.80)	9.49 (3.77)
White mother	0.42 (0.49)	0.41 (0.49)	0.36 (0.48)	0.34 (0.47)
Mother's age	32.18 (6.32)	31.75 (6.35)	28.64 (6.19)	28.35 (6.37)
Family Size	3.86 (1.14)	3.84 (1.13)	5.40 (1.84)	5.52 (1.96)
Number of children younger than 3 years old	N/A	N/A	1.15 (0.40)	1.13 (0.40)
Number of children with ages between 5 and 15	1.37 (0.67)	1.36 (0.66)	1.53 (0.85)	1.57 (0.91)
Number of days to cutoff	20.91 (20.25)	20.10 (20.14)	17.49 (20.00)	17.07 (20.02)
Number of observations	4653	4667	3789	3651

Notes: The columns are first divided by the two groups; single families where a 4-year-old child is the youngest and the households where a 4-year-old child is not the youngest or relatives present.Each column is then further divided by the birth months of children; children become 4 years old before the cutoff date, March 31st (older children), and after the cutoff date (younger children).

Table 3.2: Validity of RD design

	Mother's years of education (1)	Mother's age (2)	White mothers (3)
Youngest & no relatives			
Cutoff date	-0.155	0.165	0.024
	(0.158)	(0.288)	(0.021)
N	9320	9320	9320
Not youngest or relatives present			
Cutoff date	0.351**	-0.430	-0.012
	(0.178)	(0.303)	(0.022)
N	7440	7440	7440
Control variables	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Every specification includes control variables for mothers' predetermined characteristics (years of education, age, age squared, and race), which were not used as a dependent variable, and State and survey year fixed effects. Standard errors are reported in parentheses and clustered at children's birth date level. *p¡0.1, **p¡0.05, ***p¡0.01.

Table 3.3: First stage and reduced form results

	First stage	Reduced form				
	Preschool enrollment	Labor force participation	Formal contract	Without formal contract	Average hours working	Agerage hours on chores
	(1)	(2)	(3)	(4)	(5)	(6)
Youngest & no relatives						
Cutoff date	0.084***	0.020	0.038**	-0.044***	1.835**	-1.548**
	(0.018)	(0.023)	(0.018)	(0.015)	(0.852)	(0.734)
First stage F-stat	21.27					
Base value	0.37	0.50	0.19	0.14	17.48	30.15
N	9278	9278	9278	9278	9278	8984
Not youngest or relatives						
Cutoff date	0.045**	-0.010	-0.030*	0.007	-0.683	0.256
	(0.021)	(0.024)	(0.018)	(0.017)	(0.934)	(1.011)
Base value	0.33					
First stage F-stat	4.51	0.60	0.23	0.13	20.91	29.47
N	7397	7397	7397	7397	7397	6930
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Base values represent the average values of maternal outcome variables for those with preschool inelgible children. Every specification includes control variables for mothers' predetermined characteristics (years of education, age, age squared, and race), state and survey year fixed effects. Standard errors are reported in parentheses and clustered at children's birth date level. *p¡0.1, **p¡0.05, ***p¡0.01.

Table 3.4: Second stage results

	Labor force participation (1)	Formal contract (2)	Without formal contract (3)	Average hours working (4)	Average hours on chores (5)
Youngest & no relatives					
Cutoff date	0.244	0.457*	-0.528**	21.918**	-17.983*
	(0.282)	(0.238)	(0.220)	(11.079)	(9.470)
N	9278	9278	9278	9278	9278
Not youngest or relatives					
Cutoff date	-0.214	-0.665	0.152	-15.028	5.581
	(0.543)	(0.466)	(0.374)	(21.107)	(22.008)
N	7397	7397	7397	7397	6930
Control variables	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

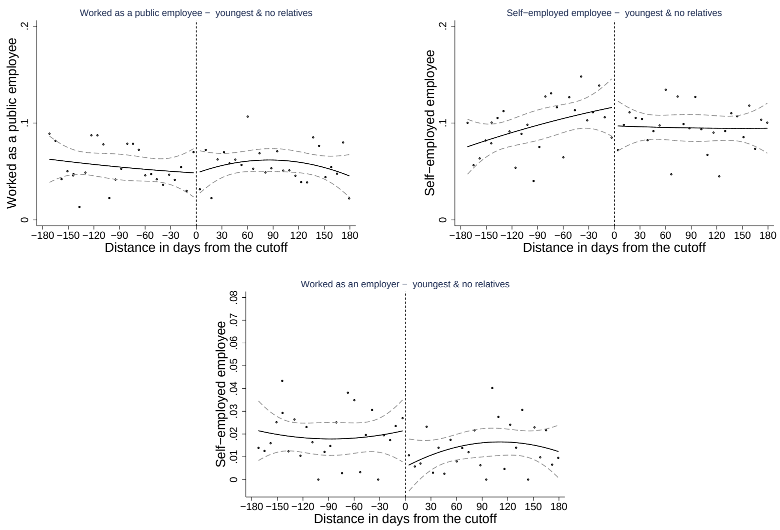
Notes: Every specification includes control variables for mothers' predetermined characteristics (years of education, age, age squared, and race), state and survey year fixed effects. Standard errors are reported in parentheses and clustered at children's birth date level. *p¡0.1, **p¡0.05, ***p¡0.01.

Table 3.5: Heterogeneous effects of preschool enrollment

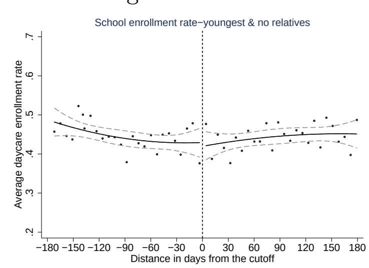
	Mother's education		Mother's age		Years	
	Low	High	Less than 30	Higher than 30	Before 2014	After 2014
	(1)	(2)	(3)	(4)	(5)	(6)
Formal contract						
Cutoff date	0.168	0.710*	0.537*	0.343	0.535*	0.421
	(0.255)	(0.405)	(0.287)	(0.415)	(0.323)	(0.453)
N	3646	5632	4059	5219	5644	3634
Without formal contract						
Cutoff date	-0.635*	-0.435	-0.358	-0.794*	-0.545*	-0.542
	(0.372)	(0.271)	(0.241)	(0.450)	(0.305)	(0.387)
N	3646	5632	4059	5219	5644	3634
Average working hours						
Cutoff date	8.224	31.342*	29.570**	11.131	19.958	27.857
	(14.562)	(17.325)	(14.560)	(18.390)	(13.972)	(22.033)
N	3646	5632	4059	5219	5644	3634
Average hours on chores						
Cutoff date	-24.495	-12.601	-14.002	-24.133	-17.466	-20.567
	(15.958)	(11.303)	(11.255)	(17.616)	(12.097)	(18.529)
N	3559	5425	3956	5028	5449	3535
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: In the first two columns, mothers' education is divided based on the completion of compulsory education. Every specification includes control variables for mothers' predetermined characteristics (years of education, age, age squared, and race), state and survey year fixed effects. Standard errors are reported in parentheses and clustered at children's birth date level. *p¡0.1, **p¡0.05, ***p¡0.01.

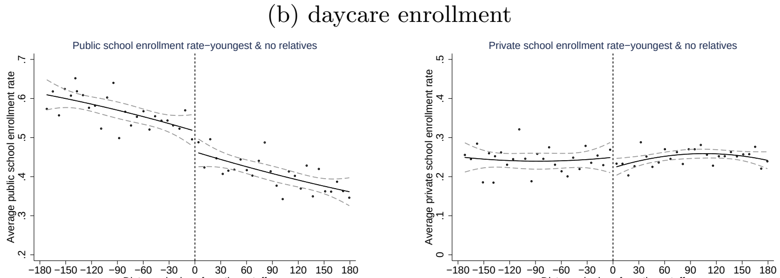
Figure 3.10: Graphical analysis for other occupations/school enrollment



(a) Other working status in a reference week



(b) daycare enrollment



(c) Division of public and private school enrollment

Notes: The figure shows the second order polynomial approximation with 95% confidence interval. The dots in scatterplots represent the average value of outcome variables in 7 days of birthday bins.

Chapter 4

The effect of the Zika-virus outbreak on Fertility and female labor market outcomes

4.1 Introduction

Zika virus infections were first reported in Brazil in May 2015. Starting from several states of the Northeast region, the Zika virus spread quickly to most states in 2016(PAHO (2016); Zanluca et al. (2015); Lowe et al. (2018)). Although Zika virus infection generally caused mild symptoms such as fever, rash, or muscle pain, the infection during pregnancy is associated with serious birth defects, such as microcephaly. The number of suspected microcephaly cases increased rapidly after the outbreak and reached to 13,914 cases in May 2017, with a total of 2,775 cases that were confirmed(PAHO(2016)). Due to a potential health hazard, the WHO declared a Public Health Emergency of International Concern on February 1st, 2016, and the Brazilian government officially encouraged the delay of pregnancy. In this paper, we used the variation of suspected microcephaly cases across states to investigate whether women delayed pregnancy following the outbreak of the Zika virus. We then analyze whether women's labor market outcomes were altered if women indeed delayed motherhood.

We used the quarterly National Household Sample Survey (Continuous PNAD) and weekly reports of microcephaly cases to examine how the outbreak of Zika virus affected fertility and female labor market outcomes. We found that among married or cohabiting women, one cumulative microcephaly case per 10,000 residents decreased the probability of having a newborn child by approximately 0.12% point. Considering that only 1.5% of the women in our sample had newborn children before the outbreak of Zika virus, the fertility rate decreased about 8% from the base value. The tendency to delay pregnancy was stronger among women younger than 35 years and those with at least a high school degree.

After we confirmed delayed motherhood, we further examined how women's labor market outcomes were influenced by the outbreak of the Zika virus. Given that microcephaly cases were more concentrated in the northeast region, which is relatively poorer than other regions, there was a possibility that the outbreak of the Zika virus influenced female labor market outcomes through channels other than delaying motherhood. We minimize this issue by controlling for local economic factors such as state-level employment and unemployment rates. Our results suggest that despite a decrease in fertility rate, there were generally no significant changes in female labor market outcomes.[1]

[1]Labor force participation slightly increased for females older than 35 years, but it was less likely to be

Our study made several meaningful contributions. First, using nationally representative data we provide more generalizable empirical evidence that women delayed their pregnancy due to the outbreak of the Zika virus. There have been several studies that examined the effects of the Zika virus on fertility, but they relied on limited samples or the surveys focused on few states of the Northeast region (Quintana-Domeque et al. (2018), Marteleto et al. (2017)). Also, there was analysis at the state level showing the relationship between microcephaly cases and fertility, but it was not able to incorporate individual's socioeconomic backgrounds as well as local economic factors (Diaz-Quijano et al. (2018)). We overcome these problems by controlling state-level employment and unemployment rates as well as individual's race or education. In addition, we provide suggestive evidence that the outbreak of the Zika virus did not make a significant impact on female labor market outcomes even if fertility declined. This is consistent with Aguero and Marks (2011), one of very few studies that examined the impact of delaying motherhood on female labor market outcomes in developing countries.

4.2 Data and Empirical Strategy

The main dataset comes from weekly reports of microcephaly cases and PNAD quarterly data. Each wave of PNAD quarterly data covers approximately 211,344 permanent private households and 35,000 municipalities. Beginning in 2012, the quarterly data have been released and contained demographic and socioeconomic characteristics of households as well as their labor market information. We used PNAD quarterly data from the first

the result of delayed motherhood given that there was very small change in fertility behavior among this group.

quarter of 2012 until the third quarter of 2017.[2] To identify newborn children, we use quarter of birth and age of individuals. The children were considered as newborn children if their age is zero and their quarters of birth is the same as the quarter of survey. Because the data do not provide information on marriage or parents of children, we made few restrictions to properly identify married (or cohabiting) individuals including parents. First, we used the households, in which both household head and spouse/partner are present. Next, we exclude households where grandchildren or relatives present because of difficulties to properly identify biological parents of newborn children.[3] After the restriction, main analysis uses the sample that consists of married or cohabiting individuals between the ages of 17 and 49. Table 1 presents the descriptive statistics.

For microcephaly cases, we use weekly reports of suspected microcephaly cases published by the Brazilian Ministry of Health. Starting in November of 2015, this report closely monitored the progress of microcephaly, which was a major consequence of Zika virus. Such report did not exist prior to the last quarter of 2015 due to a very small number of microcephaly cases. In addition, according to PAHO (2016) and Zanluca et al. (2015), potential symptoms of Zika virus infection appeared from the beginning of 2015. Considering that pregnancy normally lasts 9 months, there was less chance to observe Zika-associated microcephaly cases before the fourth quarter of 2015. Thus, we assumed there were zero cases for every state in pre-periods. We used cumulative microcephaly cases in the analysis because individuals could be more aware of the presence or risk of Zika virus infection as the number is cumulated.

[2]State level employment and unemployment data were obtained from IBGE website.
[3]Due to the restriction based on grandchildren or relatives, we dropped approximately 8% of our sample.

To analyze the effect of Zika virus outbreak on fertility and labor market outcomes, we used the following equation:

$$Y_{ist} = \beta_0 + \beta_1 cases_{st-3} + \beta_2 X_{ist} + \beta_3 M_{st} + \theta_s + \mu_t + \epsilon_{ist} \tag{4.1}$$

Where Y_{ist} represents fertility and labor market outcomes. For fertility outcome, it becomes one if individual i in state s has a newborn child in quarter t of a particular year. The variable, $cases_{sqt-3}$, indicates the number of cumulative suspected microcephaly cases per 10,000 residents in state s at t-3. It is lagged to the dependent variable by three quarters because it would take at least 9 months to observe a drop in birth rates after women decided to delay their pregnancies. we also scaled the number of microcephaly cases per 10,000 state residents to take account of population size in each state. [4] X_{ist} includes individual characteristics such as race, education and M_{st} represents state-level employment and unemployment rates. In addition, state and quarter fixed effects were included, and the standard errors were clustered at the state level.

4.3 Results

Panel A (and Panel B) in Table 2 shows the effects of Zika virus on fertility and female (Male) labor market outcomes. First rows of each panel show the effect of microcephaly cases on the probability of having a newborn child. For the overall sample, one cumulative suspected microcephaly case per 10,000 residents decreases the probability of having a newborn child by 0.12 percentage point. The results were similar in both female and male

[4] Given that abortion is usually prohibited in Brazil, it is less likely to detect the immediate change in birth rates after the outbreak of Zika virus.

samples as the analysis includes only married or cohabiting individuals. Given that only 1.5 percentage of the women in our sample had newborn children before the fourth quarter of 2015, the probability of having a child decreased by approximately 8 %.[5] The tendency toward delayed motherhood was more pronounced among mothers younger than 35 years old and those with at least high school degree. Among male samples, the probability of having a child show a larger change for those older than 35 years old, which seems contradictory to the findings from women. However, this is mainly because males tend to get married with females younger than themselves.

As discussed above, fertility rates declined due to the outbreak of Zika virus. We further extend our analysis by examining whether female (male) labor market outcomes were affected by the outbreak of Zika virus. Second and third rows in panel A show that despite delayed motherhood, women's labor market outcomes were not significantly affected by Zika virus. Most of the estimates were statistically and economically insignificant. Labor force participation slightly increased for women above 35 years old. However, given that there was almost no change in fertility rate among this group, this is more likely to be the result of other factors such as recession, which occurred from 2014 in Brazil. Also, the results show that some of males' labor market outcomes negatively changed. We cannot rule out the possibility that this is the result of delayed fatherhood; however, the magnitude of changes was negligible, and it was also possible that the results were driven by other factors including recession.

[5] In the Northeast region, where the highest number of suspected microcephaly cases were reported, the average cumulative microcephaly case during the sample period was 1.00. Pernambuco had the highest number of cases in fourth quarter of 2016 as 2.4.

4.4 Conclusion

Our study provides new evidence on the relationship between the outbreak of the Zika virus and fertility. Using nationally representative survey data and variation of microcephaly cases across states, we found that fertility declined due to the incidence of microcephaly in Brazil. The tendency to delay pregnancy was stronger if mothers were younger and more educated. The results regarding pregnancy were consistent with previous studies (Marteleto et al. (2017), Quintana-Domeque et al. (2018)) showing that more educated women were less likely to suffer from Zika symptoms, and socioeconomic status could play an important role in women's fertility decision. We also examined whether female labor market outcomes were influenced by the outbreak of the Zika virus, but there was little evidence that female labor outcomes were altered by the Zika virus.

Table 4.1: Summary statistics

Variable (Female)	Obs.	Mean	Std.Dev.	Min	Max
Have a newborn child	1,424,913	0.016	0.12	0	1
High school degree	1,424,913	0.511	0.50	0	1
Age	1,424,913	33.611	7.62	17	49
Labor force participation	1,424,913	0.488	0.50	0	1
Working hours	1,424,913	20.154	20.34	0	120

Notes: The table represents statistics for married or cohabiting females. The table was constructed by using PNAD quarterly data from 2012 to 2017.

Table 4.2: The effect of Zika virus on fertility and labor market outcomes

	All	Age>35	Age≤35	HS degree or above	Less Than HS Degree
	(1)	(2)	(3)	(4)	(5)
Panel A: Female					
Have a newborn child	-0.0012**	-0.0006	-0.0017*	-0.0015**	-0.0007
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
(Base value)	0.0154	0.0050	0.0230	0.0171	0.0132
LFP	0.0068**	0.0071*	0.0063	0.0038	0.0044
	(0.003)	(0.004)	(0.004)	(0.006)	(0.003)
(Base value)	0.5328	0.5726	0.5035	0.6147	0.4305
Work hours	0.1448	0.2398	0.0553	0.0524	0.0312
	(0.180)	(0.184)	(0.232)	(0.229)	(0.171)
(Base value)	21.8399	23.2650	20.7938	25.1548	17.7042
Observation	1424913	587757	837156	728384	696529
Panel A: Male					
Have a newborn child	-0.0013**	-0.0055	-0.0018	-0.0012	-0.0012**
	(0.001)	(0.005)	(0.001)	(0.001)	(0.000)
(Base value)	0.0154	0.0082	0.0244	0.0168	0.0141
LFP	-0.0051	-0.0055	-0.0045	-0.0061*	-0.0063
	(0.005)	(0.005)	(0.005)	(0.003)	(0.006)
(Base value)	0.8874	0.8834	0.8925	0.9201	0.8565
Work hours	-0.3355*	-0.4902**	-0.1413	-0.4248***	-0.3072
	(0.170)	(0.199)	(0.155)	(0.148)	(0.201)
(Base value)	40.2748	40.1763	40.3973	41.3776	39.2284
Observation	1424913	784362	640551	623824	801089
State FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes
State Economic Factors	Yes	Yes	Yes	Yes	Yes

Notes: Individual characteristics include race and education. State economic factors include employment and unemployment levels. *p¡0.1, **p¡0.05, ***p¡0.01.

Chapter 5

Conclusions

Although changing the starting age of compulsory education has important implication for an educational system, there have been relatively few studies evaluating such policies. The first chapter of my dissertation finds that lowering the school entry age can improve students' academic performance. It also provides important policy implications by showing that earlier exposure to compulsory education can impact students' outcomes in contexts of countries where preschool education is limited versus widespread. The second chapter of my dissertation evaluates the impact of the policy that lowers school entry ages on maternal labor market outcomes. Although the primary purpose of such a policy is to provide more equitable educational opportunities with potential benefits to students' academic performance, there is potential that mothers may also be impacted by the policy. I show that the policy could affect maternal outcomes in a country where early childhood education is limited. Preschool enrollment not only increased working hours, but also impacted the quality of jobs that mothers took. In the last chapter of the dissertation, I provide new evidence that women delayed pregnancy following the outbreak of Zika virus. This pattern was more pronounced among women younger than 35 years and those with at least a high school degree. However, female labor market outcomes were hardly affected by the outbreak of Zika virus although fertility rates declined.

This dissertation uses unique events in Brazil as quasi-experiments and shows convincing results that relate maternal and students' outcomes. There can be several studies in the future, which extend these original projects. First, it will be important to examine the long-term effects of the compulsory educational reform. Given that we were able to examine only students' short-term and intermediate outcomes, further studies will be needed to examine the long-term outcomes including students' labor market results. Also, this study shows how Brazilian maternal labor market outcomes were affected by a certain policy or a natural event. It would be valuable to compare the impact of these events with those from other Brazilian policies targeting maternal labor market outcomes.

Bibliography

[1] Jorge M. Agúero and Trinidad Beleche. Test-mex: Estimating the effects of school year length on student performance in mexico. *Journal of Development Economics*, 103:353 – 361, 2013.

[2] Jorge M. Aguero and Mindy S. Marks. Motherhood and female labor supply in the developing world evidence from infertility shocks. *The Journal of Human Resources*, 46(4):800–826, 2011.

[3] Joshua D. Angrist and William N. Evans. Children and their parents' labor supply: Evidence from exogenous variation in family size. *The American Economic Review*, 88(3):450–477, 1998.

[4] Joshua D. Angrist and Alan B. Krueger. Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014, 1991.

[5] Orazio Attanasio, Ricardo Paes de Barro, Pedro Carneiro, David Evans, Lycia Lima, Pedro Olinto, and Nobert Schady. Impact of free availability of public childcare on labour supply and child development in brazil. *Impact Evaluation Report 58*, 2017.

[6] Michael Baker, Jonathan Gruber, and Kevin Milligan. Universal child care, maternal labor supply, and family well-being. *Journal of Political Economy*, 116(4):709–745, 2008.

[7] Abhijit V. Banerjee, Shawn Cole, Esther Duflo, and Leigh Linden. Remedying education: Evidence from two randomized experiments in india*. *The Quarterly Journal of Economics*, 122(3):1235, 2007.

[8] World Bank. World development report 2012: Gender equality and development. *Washington, DC: World Bank*, 2012.

[9] James Banks and Fabrizio Mazzonna. The effect of education on old age cognitive abilities: Evidence from a regression discontinuity design*. *The Economic Journal*, 122(560):418–448, 2012.

[10] Paulo Bastos and Odd Rune Straume. Preschool education in brazil: Does public supply crowd out private enrollment? *World Development*, 78:496 – 510, 2016.

[11] Stefan Bauernschuster and Martin Schlotter. Public child care and mothers' labor supply-evidence from two quasi-experiments. *Journal of Public Economics*, 123:1 – 16, 2015.

[12] Kelly Bedard and Elizabeth Dhuey. The persistence of early childhood maturity: International evidence of long-run age effects*. *The Quarterly Journal of Economics*, 121(4):1437–1472, 2006.

[13] Kelly Bedard and Elizabeth Dhuey. School-entry policies and skill accumulation across directly and indirectly affected individuals. *The Journal of Human Resources*, 47(3):643–683, 2012.

[14] Cristián Bellei. Does lengthening the school day increase studens' academic achievement? results from a natural experiment in chile. *Economics of Education Review*, 28(5):629 – 640, 2009.

[15] Samuel Berlinski and Sebastian Galiani. The effect of a large expansion of pre-primary school facilities on preschool attendance and maternal employment. *Labour Economics*, 14(3):665 – 680, 2007.

[16] Samuel Berlinski, Sebastian Galiani, and Paul Gertler. The effect of pre-primary education on primary school performance. *Journal of Public Economics*, 93(1-2):219 – 234, 2009.

[17] Samuel Berlinski, Sebastian Galiani, and Patrick J. McEwan. Preschool and maternal labor market outcomes: Evidence from a regression discontinuity design. *Economic Development and Cultural Change*, 59(2):313–344, 2011.

[18] Leon J.H. Bettendorf, Egbert L.W. Jongen, and Paul Muller. Childcare subsidies and labour supply - evidence from a large dutch reform. *Labour Economics*, 36:112 – 123, 2015.

[19] Sandra E. Black, Paul J. Devereux, and Kjell G. Salvanes. Too young to leave the nest? the effects of school starting age. *The Review of Economics and Statistics*, 93(2):455–467, 2011.

[20] Mariano Bosch and Julen Esteban-Pretel. Job creation and job destruction in the presence of informal markets. *Journal of Development Economics*, 98(2):270 – 286, 2012.

[21] Christian N. Brinch and Taryn Ann Galloway. Schooling in adolescence raises iq scores. *Proceedings of the National Academy of Sciences*, 109(2):425–430, 2012.

[22] Magnus Carlsson, Gordon B. Dahl, Bjorn ockert, and Dan-Olof Rooth. The effect of schooling on cognitive skills. *The Review of Economics and Statistics*, 97(3):533–547, 2015.

[23] Francesca Carta and Lucia Rizzica. Early kindergarten, maternal labor supply and children's outcomes: Evidence from italy. *Journal of Public Economics*, 158:79 – 102, 2018.

[24] Elizabeth U. Cascio. Maternal labor supply and the introduction of kindergartens into american public schools. *Journal of Human Resources*, 44(1):140–170, 2009.

[25] Elizabeth U. Cascio and Ethan G. Lewis. Schooling and the armed forces qualifying test: Evidence from school-entry laws. *The Journal of Human Resources*, 52(2):294–318, 2006.

[26] Elizabeth U. Cascio and Diane Whitmore Schanzenbach. First in the class? age and the education production function. *Education Finance and Policy*, 11(3):225–250, 2016.

[27] Thomas Cornelissen, Dustmann Christian, and Trentini Claudia. Early school exposure, test scores, and noncognitive outcomes. *unpublished*, 2013.

[28] Claire Crawford, Dearden Lorraine, and Ellen Greaves. When you are born matters: evidence for england. *The Institute for Fiscal Studies*, 2010.

[29] Guillermo Cruces and Sebastian Galiani. Fertility and female labor supply in latin america: New causal evidence. *Labour Economics*, 14(3):565 – 573, 2007.

[30] Sarah C. Dahmann. How does education improve cognitive skills? instructional time versus timing of instruction. *Labour Economics*, 2017.

[31] Ashlesha Datar. Does delaying kindergarten entrance give children a head start? *Economics of Education Review*, 25(1):43 – 62, 2006.

[32] Fredi Alexander Diaz-Quijano, Daniele Maria Pelissari, and Alexandre Dias Porto Chiavegatto Filho. Zika-associated microcephaly epidemic and birth rate reduction in brazilian cities. *American Journal of Public Health*, 108(4):514–516, 2018.

[33] Carlos Dobkin and Fernando Ferreira. Do school entry laws affect educational attainment and labor market outcomes? *Economics of Education Review*, 29(1):40 – 54, 2010.

[34] Esther Duflo, Pascaline Dupas, and Michael Kremer. Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in kenya. *American Economic Review*, 101(5):1739–74, August 2011.

[35] Alex Eble and Feng Hu. The power of credential length policy: schooling decisions and returns in modern china. *unpublished*, 2017.

[36] Todd E. Elder and Darren H. Lubotsky. Kindergarten entrance age and children's achievement: Impacts of state policies, family background, and peers. *The Journal of Human Resources*, 44(3):641–683, 2009.

[37] David K. Evans and Katrina Kosec. *Early Child Education : Making Programs Work for Brazil's Most Important Generation.* World Bank Study, 2012.

[38] Christina Felfe, Natalia Nollenberger, and Núria Rodríguez-Planas. Can't buy mommy's love? universal childcare and children's long-term cognitive development. *Journal of Population Economics*, 28(2):393–422, 2015.

[39] Michael Fertig and Jochen Kluve. The effect of age at school entry on educational attainment in germany. *IZA discussion paper 1507*, 2005.

[40] Maria Donovan Fitzpatrick. Revising our thinking about the relationship between maternal labor supply and preschool. *Journal of Human Resources*, 47(3):583–612, 2012.

[41] MariaDonovan Fitzpatrick. Preschoolers enrolled and mothers at work? the effects of universal prekindergarten. *Journal of Labor Economics*, 28(1):51–85, 2010.

[42] Jason Fletcher and Taehoon Kim. The effects of changes in kindergarten entry age policies on educational achievement. *Economics of Education Review*, 50:45 – 62, 2016.

[43] Peter Fredriksson and Bjorn Ockert. Life-cycle effects of age at school start. *The Economic Journal*, 124(579):977–1004, 2014.

[44] Chloe Gibbs. Experimental evidence on early intervention: The impact of full-day kindergarten. *unpublished*, 2014.

[45] Dominique Goux and Eric Maurin. Public school availability for two-year olds and mothers' labour supply. *Labour Economics*, 17(6):951 – 962, 2010.

[46] Tarjei Havnes and Magne Mogstad. Money for nothing? universal child care and maternal employment. *Journal of Public Economics*, 95(11):1455 – 1465, 2011.

[47] Rachel Heath and Seema Jayachandran. The causes and consequences of increased female education and labor force participation in developing countries. *The Oxford Handbook of Women and the Economy*, 2017.

[48] Chris M. Herbst. Universal child care, maternal employment, and children's long-run outcomes: Evidence from the us lanham act of 1940. *Journal of Labor Economics*, 35(2):519–564, 2017.

[49] Guilherme Hirata Joana Simoes de Melo Costa, Ana Luiza Neves de Holanda Barbosa. Effects of domestic worker legislation reform in brazil. *International Policy Centre for Inclusive Growth working paper*, (149), 2016.

[50] Harry Krashinsky. How would one extra year of high school affect academic performance in university? evidence from an educational policy change. *Canadian Journal of Economics*, 47(1):70–97, 2014.

[51] Edwin Leuven, Mikael Lindahl, Hessel Oosterbeek, and Dinand Webbink. Expanding schooling opportunities for 4-year-olds. *Economics of Education Review*, 29(3):319 – 328, 2010.

[52] Rachel Lowe, Barcellos Christovam, Brasil Patrícia, Oswaldo G. Cruz, Alves Honorio Nildimar, Kuper Hannah, and Sa Carvalho Marilia. The zika virus epidemic in brazil: From discovery to future implications. *International Journal of Environmental Research and Public Health*, 15(1):1–18, 2018.

[53] Petter Lundborg, Erik Plug, and Astrid Wrtz Rasmussen. Can women have children and a career? iv evidence from ivf treatments. *American Economic Review*, 107(6):1611–37, June 2017.

[54] Leticia J. Marteleto, Abigail Weitzman, Raquel Zanatta Coutinho, and Sandra Valongueiro Alves. Women's reproductive intentions and behaviors during the zika epidemic in brazil. *Population and Development Review*, 43(2):199–227, 2017.

[55] Patrick J. McEwan and Joseph S. Shapiro. The benefits of delayed primary school enrollment: Discontinuity estimates using exact birth dates. *The Journal of Human Resources*, 43(1):1–29, 2008.

[56] Amalia R. Miller. The effects of motherhood timing on career path. *Journal of Population Economics*, 24(3):1071–1100, 2011.

[57] Karthik Muralidharan and Venkatesh Sundararaman. Teacher performance pay: Experimental evidence from india. *Journal of Political Economy*, 119(1):39–77, 2011.

[58] Natalia Nollenbergera and Nuria Rodriguez-Planas. Full-time universal childcare in a context of low maternal employment: Quasi-experimental evidence from spain. *Labour Economics*, 36:124 – 136, 2015.

[59] OECD. Education at a glance: Oecd indicators 2012. *Country Note*, 2012.

[60] OECD. Programme for international student assessment (pisa) results from pisa 2015. *Country Note - Results from PISA 2015*, 2016.

[61] PAHO. Zika - epidemiological report brazil. *Pan American Health Organization / World Health Organization*, 2016.

[62] Rasyad A. Parinduri. Do children spend too much time in schools? evidence from a longer school year in indonesia. *Economics of Education Review*, 41:89 – 104, 2014.

[63] Patrick A. Puhani and Andrea M. Weber. Does the early bird catch the worm? *Empirical Economics*, 32(2):359–386, 2007.

[64] Climent Quintana-Domeque, Jose Raimundo Carvalho, and Victor Hugo de Oliveira. Zika virus incidence, preventive and reproductive behaviors: Correlates from new survey data. *Economics and Human Biology*, 30:14 – 23, 2018.

[65] Gabriel Ulyssea. Regulation of entry, labor market institutions and the informal sector. *Journal of Development Economics*, 91(1):87 – 99, 2010.

[66] Sunanda Vaidheesh. Delivering on the promise of access to education:improving primary school completion rates in brazil. *unpublished*, 2013.

[67] Sher Verick. Female labor force participation in developing countries. *IZA World of Labor*, 87, 2014.

[68] Camila Zanluca, Andrade de Melo Vanessa Campos, Pamplona Mosimann Ana Luiza, Viana dos Santos Glauco Igor, Duarte dos Santos Claudia Nunes, and Luz Kleber. First report of autochthonous transmission of zika virus in brazil. *Mem Inst Oswaldo Cruz*, 110(4):569–572, 2015.