Early Schooling and Maternal Labor Market Outcomes: Evidence from a New School Entry Policy Using Exact Date of Birth in Brazil

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Abstract

This paper aims to use a national age-at-school-entry policy to identify the causal effect of mothers getting their children enrolled a year earlier in Primary School. Brazilian public and private primary schools admit children up to one year younger than the national minimum age to enter school if their birthday is before an arbitrary threshold, defined as March 31. Using a Fuzzy Regression Discontinuity Design and a Fixed-Effects Event-Study, we estimate the impact of having children enrolled in Primary School on mothers' labor outcomes, using longitudinal data from a quarterly Brazilian socioeconomic survey. We found positive and significant impact on employment rates, employment status, hours in labor and income, besides some evidence on participation rate and no significant effect on formality rate and working hours. Our results show no impact of the treatment on fathers' labor income, suggesting that mothers benefits disproportionately by having children enrolled in primary school. Besides, our most positive results are concentrated on the second quarter, when there's no winter or summer vacation, with enrolled children attending school for the whole period.

JEL Classification: I24 I26 I28 J24 J31
Keywords: Education, Female Labor Market Outcomes, RD Design, Event-study

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1 Introduction

Female labor supply, despite significant improvements over the last decades, has remained considerably lower than men's, especially in developing countries. Among various reasons, gender norms such as children care-taking responsibilities are regarded as a key determinants of this gap.

In countries with no legal restrictions on female work, labor force participation is mainly determined by intrahousehold time allocation. For women - especially mothers -, time allocation is significantly affected by predominant perceptions of gender roles, under which they should be the ones primarily responsible for activities such as unpaid care work [Blau and Robins, 1988]. Parenthood requires an enormous amount of time for care-taking of the child. This burden, since pregnancy, is often over beared by mothers. So lower fertility rates have been correlating with increases in female labor force participation.

In order to escape from the trade-off between female labor supply and fertility rates, schools may play a role in not only improving children's human capital, but also alleviating time restrictions imposed on their mothers. Since basic care-taking is provided at school, the resulting increase in free time should permit mothers to (re)join the labor market, following the theoretical models developed from Becker and Lewis [1973].

Cascio [2009] presents evidence that single mothers are benefited by children getting enrolled in school at five, increasing their labor market participation. Padilla-Romo and Cabrera-Hernández [2018] find that, with evidence from Mexico, extending the duration of the school day increases mothers' labor supply, with an impact on labor force participation of 5.5 percentage points and on number of weekly hours worked by 1.8. Also, Berlinski and Galiani [2007] found a positive impact of a program of expansion of kindergarten facilities in Argentina on pre-primary school attendance and maternal labor supply.

Brazil is an upper middle income country, where the share of adult women on the labor market was around 49.6% in 2019, almost 20 percentage points below men (69.5%). Brazilian women also face, like in many developing countries, higher unemployment, less formality and less working hours than men. These relevant gaps have several implications on gender inequality and welfare.

Sanfelice [2018] estimates the impact of formal childcare consumption on maternal employment in Brazil's largest state, São Paulo, exploiting a variation in the excess demand for public childcare across age cohorts within city neighborhoods. The results show that using center-based care increases the probability of maternal employment by 44 percentage points, makes mothers more likely to work full time and more likely to work in the formal sector - suggesting increases in wages as well.

However, having a son or daughter enrolled in school correlates with many other variables that also have an impact on the decision of women of joining the labor market. In order to address these and other endogeneity problems, we exploit legislation on the maximum date-of-birth required for children to get enrolled in primary school before they are 6 years old. We also exploit the longitudinal nature of our data, allowing us to use a fixed-effects event-study, adding robustness to our methodology. Our results show in general positive and significant impacts of having a child enrolled in primary school on labor market outcomes and income on mothers, but not fathers.

The remainder of the paper is organised as follows. Section 2 explains the institutional background. Section 3 details the datasets used in the analysis and the identification strategy applied to estimate the causal effect. Section 4 presents and explains the results and robustness checks, while Section 5 presents the final remarks.

2 Institutional Background

Brazil, since the 90s, has had a major transformation in its educational system, with big changes covering all of its segments. The public investment destined directly to the sector, for example, grew from less than 3% of GDP in early 80's to 5.6% in 2014. More capable of increasing education supply, after four centuries of educational exclusion among the poorest of the population, Brazil has experienced rapid expansion of basic education attendance in the last 20 years, achieving universal attendance of children in school by the late 90s.

The Brazilian new Law of Guidelines and Bases (LDB) of 2013 requires school enrollment and free education from the age of four, and states and municipalities would have until 2016 to provide it. However, public preschool provision remains limited, so a children's age might induce a relevant, positive consumption shock on the household.

Since promulgation of law Nº 9.394, of December 20 (1996) and, in 2005, institutionalization of the nine years Elementary School program, children must attend school from the time they're 6 years old. However, since school enrollment date is in the first or second month of the year, schools accept students younger than the obligatory age if their birthday is before a given threshold. In 2010, the Brazilian National Education Council established March 31 as a national policy for school entry rule of children younger than the minimum age for primary school at the time of the enrollment registration.

On the other hand, the Brazilian National Education Council's decision has no power of law, and each state education system used to legislate on its date of day of birth threshold to ingress in local primary schools in the event of the child not being old enough by the time of admission. According to a survey by Todos pela Educação [Todos pela Educação], in 2015 at least 19 of the 27 Brazilian state capitals were adopting the March 31 date as primary school entry policy, as were other 718 municipalities from the 1230 that participated in the survey. In Brazil, no official population survey but the National Census has municipality-level information, usually constraining precision on the threshold choice for researchers. Peña [2014] utilized Brazilian School Census data to find discontinuities on enrollments by date of birth to determine each city's heuristic on the topic.

However, in August 2018, the Brazilian Supreme Court, demanded by the General Prosecutor of the Republic, judged March 31 as the national threshold, without possibility of local legislation on this matter. To eliminate uncertainties, the Ministry of Education published Ordinance No. 1,035 / 2018 on October 8th, which ratified the National Council of Education / Basic Education Chamber Document No. 2/2018, approved on September 13th, guiding all education systems to follow the threshold. With this new change, it is possible to exploit the discontinuity on the probability of being enrolled in primary school to test its impact on mothers' labor market outcomes.

3 Data and Methodology

3.1 Data

We use data from the Brazilian National Sample Household Continuous Survey (PNADC), a survey conducted by the Brazilian Institute of Geography and Statistics every quarter since 2012, presenting country-wide information about schooling, labor market and income. We will present estimates for all quarters from 2018.3 to 2019.4, considering the first quarter of 2019 as the beginning of the treatment.

In PNADC, there is a variable of day, month and year of birth for all individuals. Since we are focusing on the the second quarter of 2019, our sample will be restricted to households with mothers of children born 89 days before and after 2013-03-31, that is, from 2013-01-01 to 2013-06-28. The running variable will be reported as days of birth after/before the threshold, where negative values are days of birth after this date, while positive values are days of birth before it.

3.2 Empirical Procedure

3.2.1 RDD

The first empirical strategy relies on "Fuzzy" Regression Discontinuity Design estimates. On that matter, first, we will model the mother's i of a child ci, aged 5 or 6, probability of having the infant enrolled in primary school (S_{ci}) as a non-monotonic function of children' dates of birth (N_{ci}). $\alpha(N_i)$ is a continuous function at $N_{ci}\,\forall N_{ci} \neq 0$, implying a cutoff at 0. To be more precise, $\nexists \lim_{N_{ci}\to 0} \alpha(N_{ci})$. Let $D_{ci} = 1$ if $N_i > 0$ and μ_i be a normally distributed, zero-centered error term. Parameter β measures the local average treatment effect (LATE) of the primary school entry rule. Consider the model:

$$S_{ci} = \alpha(N_{ci}) + \beta_1 D_{ci} + \mu_i \tag{1}$$

In a Fuzzy RDD, the first stage estimation is one in which the probability of being enrolled in school is partially affected by the discontinuity. The second stage follows this model:

$$Y_i = \beta_0 + \beta_2 \hat{S}_{ci} + \epsilon_i \tag{2}$$

Where Y_{ic} is the outcome of interest. In this paper, we will assess the impact of having a child enrolled in primary school over labor force participation, employment, its sector (and also whether it is formal), effectively worked hours and labor income. This empirical procedure is very similar to Fitzpatrick [2010], who estimated the effects of universal pre-kindergarten availability in three states from the US on overall preschool enrollment and maternal labor supply.

One of most important arguments for the internal validity of an RD framework is the choice of the bandwidth. In this paper, the assessment of the impact may be estimated by a standard Fuzzy RD Design, with different bandwidths: first, we implement bandwidth selectors developed in Calonico et al. [2014], Calonico et al. [2014] and Calonico et al. [2019]. Among the estimated bandwidths, if there's any below 178 days around the cut-off (which implies birth 89 days before and after the threshold), it's chosen. However, if not, we adopt the 178 bandwidth, since the threshold is March 31, and any bandwidth above 178 days would include children that would be 6 years old before 2019.

Our results will be reported with robust t-statistics, as proposed by Calonico et al. [2014], constructed by bias-corrected confidence intervals, which are constructed using a bias-corrected RD estimator together with a novel standard error estimator.

RD Design is most similar to a randomized experiment, so that it's possible to argue that the coefficient found in the regression do represent a causal effect. Despite the fact its LATE estimates may be interpreted as only applicable to the sub population of individuals at the discontinuity threshold, and uninformative about the effect anywhere else, Lee and Lemieux [2010] argue that, in the presence of heterogeneous treatment effects, RD estimates can be interpreted as a weighted average treatment effect, with weights relative to the ex-ante probability that the value of an individual's assignment variable (in this case, date of birth) will be around the threshold.

In order to give additional support to the hypothesis that being born just before the primary school entry threshold implies an exogenous variation in schooling, we also make use of covariates, so we can check whether both treated and control groups are balanced, besides adding them as controls as well. Those variables will be mothers' age, education, marital status, race and fathers' income, and state dummies. In all regressions, triangular kernel weighting will be used, giving a higher weight to the observtions closest to the threshold.

Also, it will be required to show frequencies of birthdays at the 0 cutoff, besides applying a McCrary Density Test [McCrary, 2008], in order to check if parents are able to manipulate the running variable (date of birth). Betrán et al. [2016] shows that, in a sample of 150 countries in 2014, Brazil has a cesarean section rate of 55.6%, the highest one in Latin America and the Caribbean, which has the highest rates in the world. Qualitative data suggests that one of the reasons for some Brazilian mothers rather have this procedure is due to the possibility of time of birth [Kasai et al., 2010]. If mothers are able to manipulate when their children are going to be born by scheduling a cesarean section, and the possibility of this procedure and women preferences for day of birth of their children is not a smooth function around the cutoff, our estimates may be biased because of mother's choices.

McCrary and Royer [2011] states that using school entry rules as a source of exogenous variation of schooling has its limitations. That happens because not all children enter school in the year predicted by primary school entry policy - the parents of the ones born before the threshold may alternatively keep them at home or preschool for another year, as it is also possible for the ones parenting children born after the school entry date to make a petition for their child to start school a year before typically allowed, or even put their child in a private school. This means that it's possible for estimates to be disproportionate relative to low-income families, whose parents are more likely to comply with school entry rules because they might lack access to private schools and lawyers.

3.2.2 Fixed-effects event-study

We also takes advantage that PNADC survey visits the same household for five quarters, so it can be used as a household panel. Therefore, we provide estimates from a fixed-effects event-study specification, which is similar to a difference-in-differences methodology, with the exception that we can assess the treatment effect over time.

For this specification, our treatment group will be considered as mothers of children born from January 1st to March 31, while the control group will those with children born from April 1st to June 31. Thus, we estimate the following equation:

$$Y_{it} = \alpha_i + \sum_{\tau \neq -2} \gamma_\tau \cdot \text{Treated}_i \cdot \mathbb{I}(\tau = t) + \beta X_{it} + \lambda_t + \epsilon_{it} \tag{3}$$

where Y_{it} is the outcome variable of individual i at time t, Treated_i is a dummy variable equal to 1 if individual i is treated (i.e. if the child is to the right of the cutoff, meaning she is eligible to mandatory school enrollment), α_i is an individual fixed-effect, X_{it} is a vector of explanatory variables, β is a vector of coefficients, λ_t is a time fixed-effect and ϵ_{it} is an error term. We are interested in the coefficients γ_τ, which capture the effect along time of mandatory school enrollment. Notice τ is the temporal distance from the baseline. So, for instance, $\tau = 0$ is the first quarter of 2019, $\tau = 2$ is the second quarter of 2019, etc. Furthermore, observe we exclude $\tau = -2$, which will be our baseline.

The advantage of this specification is to control our estimates for every time-invariant differences among households in the treatment and control groups, providing additional support for our treatment to be exogenous. However, it's main setback is its time limit of 4 quarters, due to our survey framework. It's also relevant to take note that our estimated in this specification will be under a reduced-form framework, differently from our Fuzzy RDD specification.

4 Results

4.1 Bandwidth

As stated in Section 3.2, we use a different bandwidths, following an optimal bandwidth selectors procedure, based on Calonico et al. [2014], Calonico et al. [2014] and Calonico et al. [2019]. However, we'll adopt a limit of 89 days of birth after and before threshold, in order to exclude any child born in the previous year. Our larger sample, therefore, has 1,611 observations below and 1,568 observations above the threshold.

Table 1, below, shows the results of many popular optimal bandwidth selectors for the first stage (i.e. primary school enrollment). The abbreviations represent the selectors as follows: `mserd` is a one common Mean Square Error-optimal bandwidth selector for the RD treatment effect estimator; `msetwo` is a two different Mean Square Error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator; `msesum` is a one common Mean Square Error-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); `msecomb1` is the minimum between `mserd` and `msesum`; `msecomb2` is the median of `msetwo`, `mserd` and `msesum`, for each side of the cutoff separately; `cerrd` is the Coverage Error Rate-optimal bandwidth selector for the RD treatment effect estimator; `certwo` is two different Coverage Error Rate-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator; `cersum` is one common Coverage Error Rate-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); `cercomb1` is the minimum between `cerrd` and `cersum`; `cercomb2` is the median between `certwo`, `cerrd` and `cersum`, for each side of the cutoff separately.

Table 1: Bandwidth selectors according to each methodology

	No Controls		Controls	
Methodology	Left of c	Right of c	Left of c	Right of c
mserd	129	129	125	125
msetwo	131	663	129	662
msesum	155	155	156	156
msecomb1	129	129	125	125
msecomb2	131	155	129	156
cerrd	72	72	70	70
certwo	73	369	72	370
cersum	86	86	87	87
cercomb1	72	72	70	70
cercomb2	73	86	72	87

Source: Estimates from PNADC, based on Calonico et al. [2014], Calonico et al. [2014] and Calonico et al. [2019].

Our minimal optimal bandwidths are presented in Table 5, in the Appendix. While some bandwidths are larger than 89, most labor market outcomes are smaller, specially after 2018.

4.2 Summary Statistics and First Stage Estimates

Table 4.2 presents the summary statistics of our sample of mothers of children born around 2013-03-31. 'Employment' is a dummy variable of employment status. 'Employment Rate' is the same variable but, on the other hand, it assumes any value only for mothers in the labor force. 'Formality Rate' and 'Working Hours' represent, respectively, a dummy variable of formality status and a continuous variable for monthly hours spent in labor, both only for occupied women. 'Hours in labor', on the other hand, is the same as 'Working Hours', but for all mothers (so those who were unemployed or not in the labor force had 0 hours in labor). Finally, 'Participation Rate' is a dummy variable of participation in the labor force status - that is, whether women were occupied or at least looking for a job, or whether they were in leisure or engaged in an unpaid activity.

Table 2: Summary Statistics for Mothers of children around the threshold in 2018.3

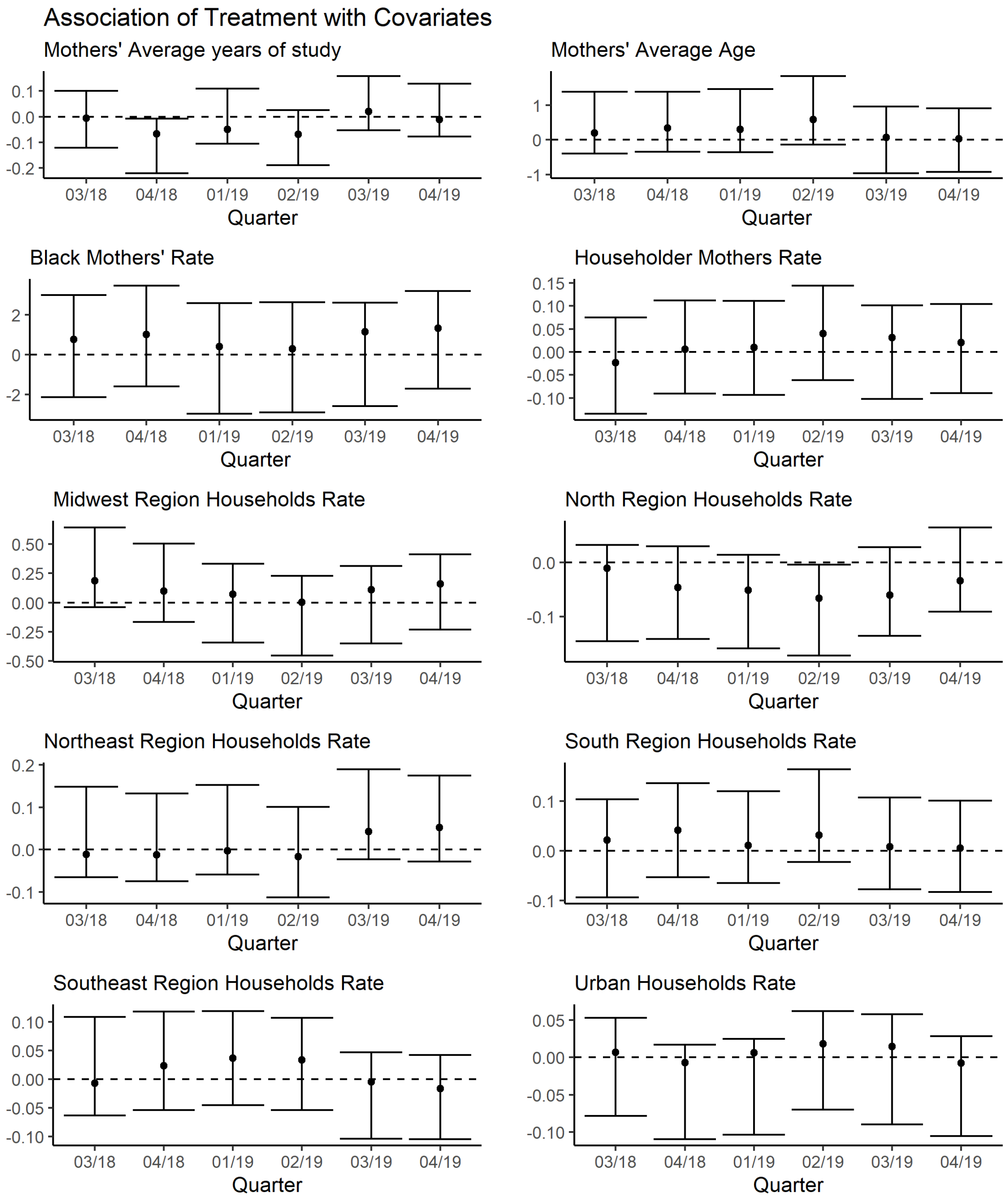
Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Children Enrollment in Primary School	2,721	0.062	0.241	0	0	0	1
Mothers' Participation Rate	2,857	0.552	0.497	0	0	1	1
Mothers' Employment Rate	1,578	0.875	0.330	0	1	1	1
Mothers' Working Hours	1,382	138.654	55.698	4	96	176	448
Mothers' Employment	2,857	0.483	0.500	0	0	1	1
Mothers' Formality	1,382	0.446	0.497	0	0	1	1
Mothers' Hours in Labor	2,857	67.005	79.314	0	0	160	448
Mothers' Labor Income	2,857	799.113	1,906.289	0	0	1,035.7	29,099.9
Fathers' Labor Income	2,912	1,559.4	3,116.7	0	0	1,869.4	41,915.0
Black Mothers' Rate	2,836	0.660	0.474	0	0	1	1
Mothers' Average years of study	2,857	9.875	4.156	0	7	12	16
Mothesrs' Average Age	2,857	36.324	11.636	16	28	41	91
Urban Households Rate	2,912	0.710	0.454	0	0	1	1
North region Households Rate	2,912	0.172	0.377	0	0	0	1
Northeast region Households Rate	2,912	0.340	0.474	0	0	1	1
Southeast region Households Rate	2,912	0.244	0.429	0	0	0	1
South region Households Rate	2,912	0.148	0.355	0	0	0	1
Midwest region Households Rate	2,912	0.096	0.295	0	0	0	1

Source: PNADC 2018.3

Table above shows our sample is composed mostly by black mothers (66%) in urban households (71%), one third of them in the Northeast Region (the poorest one in Brazil). Their average age was 36 and their average years of study were slightly higher than completed Middle School (9 years), but significantly lower than completed High School (12 years). Almost half of them were occupied in a paid activity (48.3%), with a average monthly workload of 138.6 hours (little more than 34 hours/week). Finally, fathers' labor income in those households were around BRL 1,500/month, about USD 9,000 per year in purchase power parity, according to OECD conversion rate.

Figure 1, below, shows discontinuity check on all of our covariates by date of birth of children (as days after/before 2013-03-31), in order to check whether our sample is balanced. It is possible to see that there is no discontinuity around the threshold for all variables, but fathers' labor income. The difference on this one, on the other hand, is not statistically significant at 95% confidence interval.

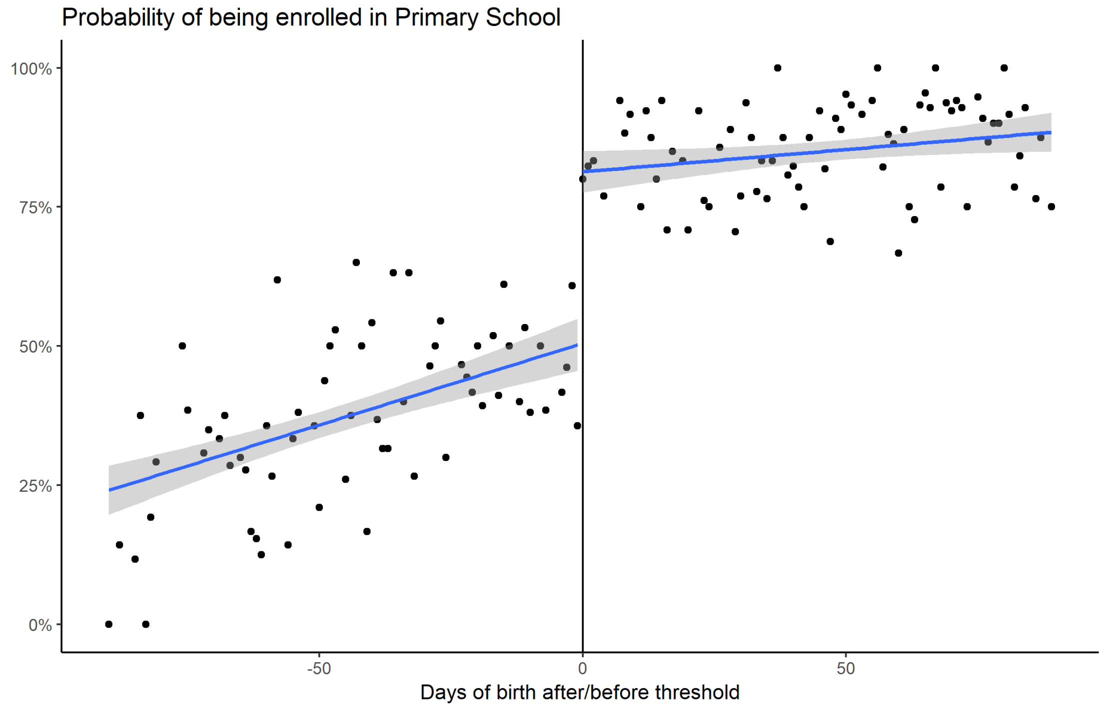
Figure 1



Source: PNADC

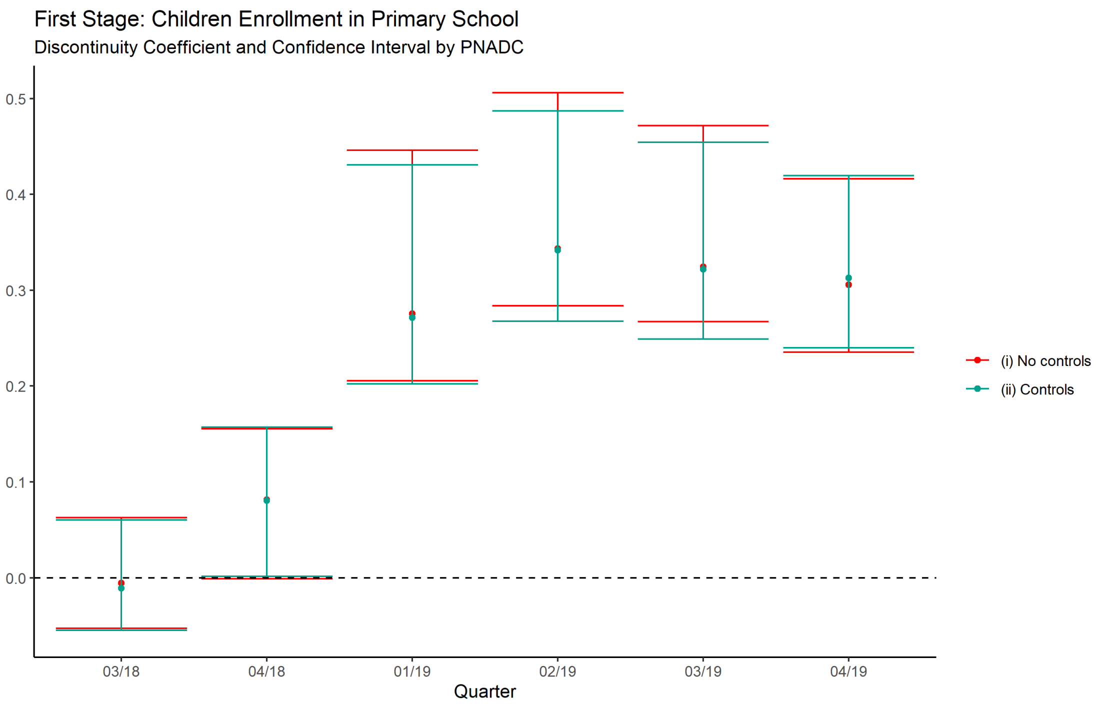
Figure 2 below, presents evidence that, in the second quarter of 2019, there was a high and significant discontinuity on the probability of children born around the Date of Birth 2013-03-31 being enrolled in primary school. Figure 3 shows the coefficients, besides conventional and robust confidence intervals, for the rate of children attending primary school over the discontinuity on the threshold.

Figure 2



Source: PNADC 2019.2

Figure 3



Source: Estimates from PNADC

As one may see, estimates are relevant, around 0.3 and 0.4, and statistically significant at 95% confidence level. That means that the probability of enrollment discontinuously increases by 35 percentage points. Adding controls only slightly decrease coefficients, without much loss of significance.

4.3 Second Stage

In the Appendix, Table 5, we show our estimates for all variables in the larger and smaller sample, using regressions with all 6 covariates as controls and without controls as well. Table 3 shows the estimates for all labor market variables,

Table 3: Fuzzy RDD Estimates by quarter

Mothers' Labor Outcomes	2018.3	2018.4	2019.1	2019.2	2019.3	2019.4
Employment	-3.244	0.11	-0.037	0.28	0.202	0.014
	(10.497)	(0.677)	(0.211)	(0.168)**	(0.178)	(0.163)
Employment Rate	2.749	-0.216	0.035	0.155	0.195	0.042
	(14.771)	(0.548)	(0.191)	(0.144)**	(0.16)*	(0.121)
Formality Rate	8.981	0.489	0.226	0.206	-0.016	0.196
	(53.125)	(0.82)	(0.309)	(0.27)	(0.257)	(0.226)
log of Hours in Labor	-22.124	0.939	0.177	1.373	1.06	0.197
	(65.902)	(3.313)	(1.014)	(0.805)**	(0.881)	(0.793)
log of Working Hours	13.601	0.553	0.555	0.119	0.161	0.244
	(58.274)	(0.911)	(0.393)**	(0.277)	(0.276)	(0.222)
Participation Rate	-3.611	0.48	-0.024	0.325	0.097	-0.031
	(16.623)	(0.774)	(0.224)	(0.189)*	(0.187)	(0.176)
Income						
log of Mothers' Labor Income	-29.417	3.206	1.149	2.039	1.779	0.577
	(88.646)	(4.881)	(1.502)	(1.151)**	(1.245)*	(1.140)
log of Fathers' Labor Income	8.23	-0.639	-0.281	-0.092	1.358	9.652
	(39.176)	(4.281)	(1.322)	(1.082)	(1.137)	(1.098)

Notes: Using covariates presented in Figure 1 as controls
Robust SE in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: Estimates from PNADC

Our estimates show an significant increase on working hours in 2019.1, our first quarter after the treatment. In the second one, however, there's an increase on 0.28 on employment and 0.155 on employment rate. There's also a significant increase of 1.37 log points in hours in labor, but an insignificant increase in working hours, which suggests that employed mothers that starts increasing their workload at first, followed by others having occupations, decreasing average working hours.

In the second quarter we also find significant increases in participation rate of 32.5 p.p., but significant at 10% rather than 5%. Finally, mothers labor income increase significantly on that quarter, by more than 2 log points, while fathers' income tend to have no significant change.

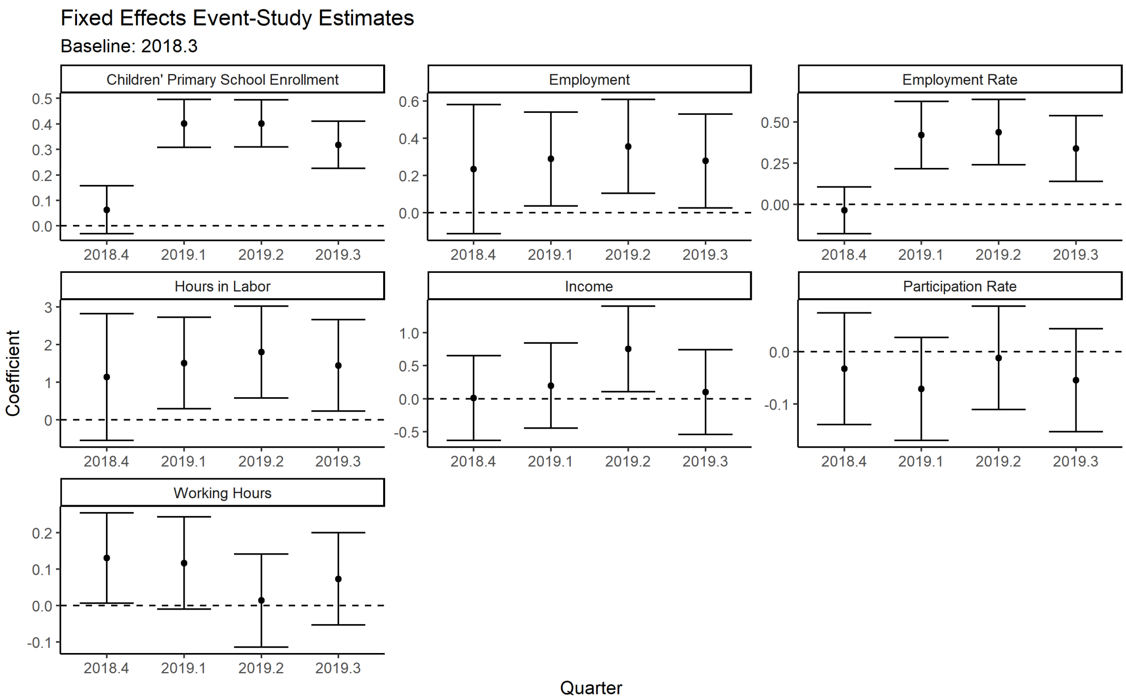
By the third quarter, however, the treatment tend do lose its impacts. There's still some evidence of increase on employment rate and income, but both significant at just 10%. In the last quarter, there's no significant effect whatsoever.

It's expected that the second quarter of 2019 shows most significant results, since in this period all children are already attending school: summer vacations are usually over by the end of February (2019.1), and winter vacations do not begin until July (2019.3), while summer vacations usually begin by December (2019.4). Thus, mothers in this quarter are benefiting from children enrolled in primary school in the whole period.

4.4 Fixed-Effects Event-Study

Figure 4 shows our estimates for our event-study regressions. Besides household and year fixed-effects, we also include mothers' schooling and age as controls.

Figure 4



Source: PNADC

The fixed-effects event-study estimates reveal that children's primary school enrollment increase the employment by up to 40 p.p. throughout the three following quarters; increase overall employment by up to 35 p.p. and hours in labour by 509%. We do not find, however, significant changes both in working hours and participation rate. Also, we find an increase in income of 112% in the first quarter following the child's entry into school.

Although we find different coefficient magnitudes, these results are fairly consistent with those from the RDD estimates presented earlier, backing up our findings.

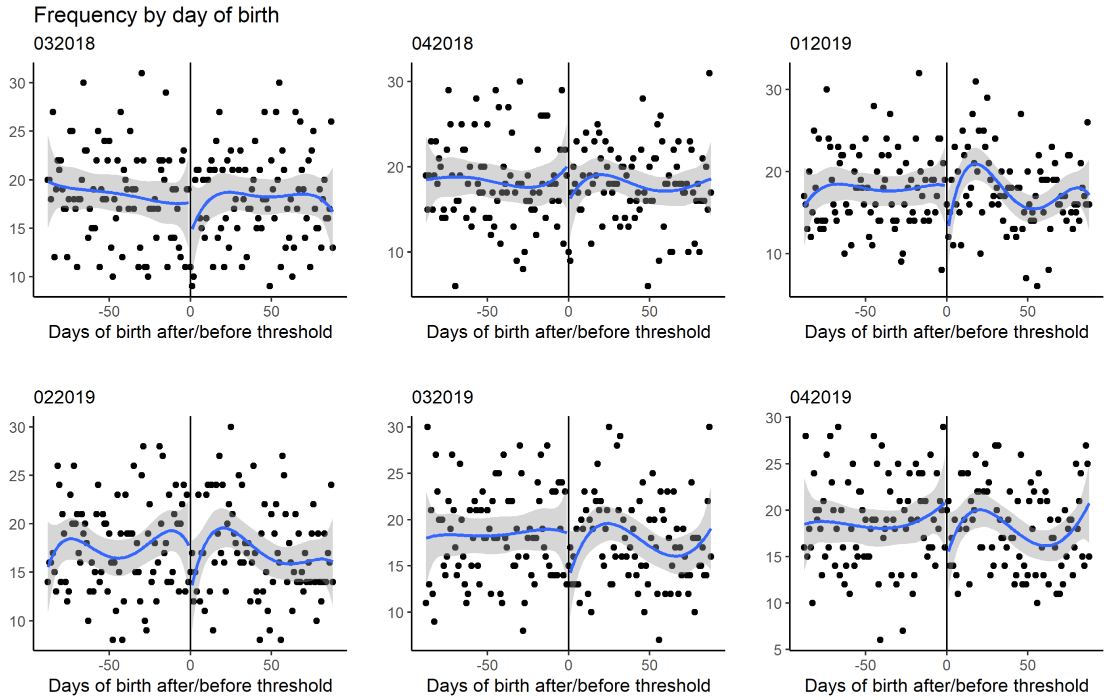
4.5 Robustness checks

We've provided evidence that the discontinuity happens only on our treatment, presenting consistent no difference between control and treatment group on covariates. Now we investigate whether it is being manipulated or discontinuities on it represent credible exogenous variation.

Birth-date manipulation by parents is a possibility, who could theoretically delay or accelerate birth through c-sections. Given the sharp, acute nature of the cutoff this could present a problem, an especially relevant one given more educated mothers (whose children are more likely to be themselves more educated) should have more access to c-sections and information regarding school entry dates.

Figure 5 shows the density function of dates of birth around 31-03-2013 by quarter. It is possible to see that there is no strong discontinuity in the whole sample.

Figure 5



Source: PNADC

Finally, Table 4 presents McCrary Sorting Test results on our running variable by each quarter. The p-value of the coefficient testing for discontinuity around the cutoff is above 0.4 for all quarters. Thus, there is no evidence of discontinuity on the frequency of dates of birth around 31-03-2013.

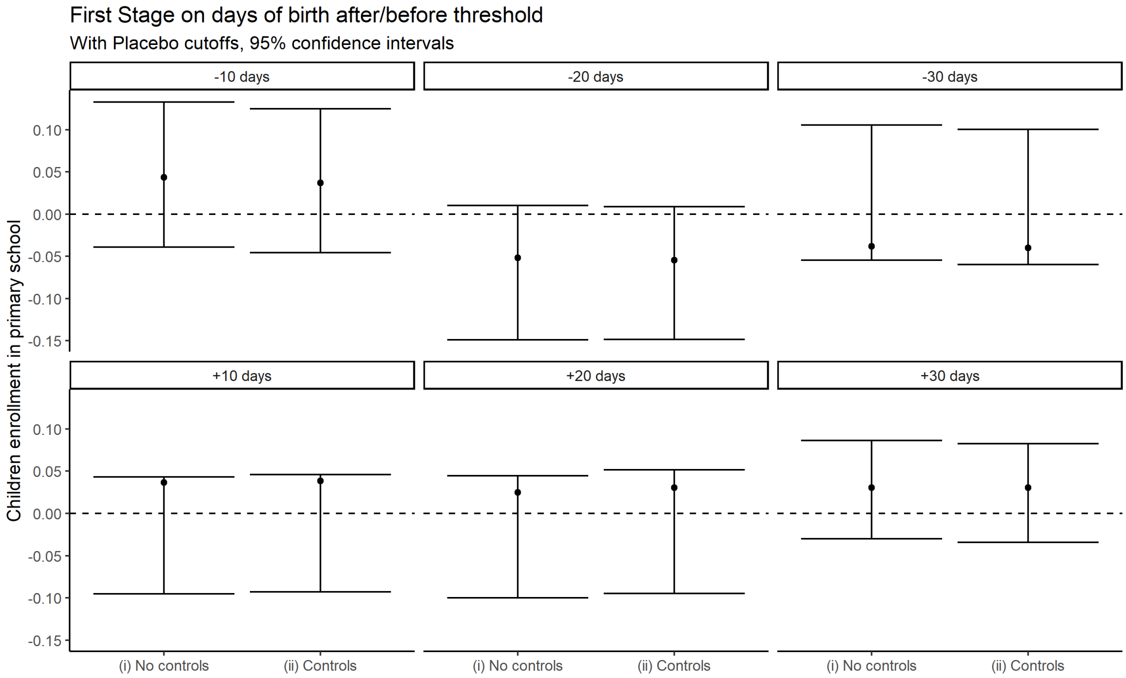
Table 4: McCrary Test on the running variable by quarter

	p-value
2018.3	0.696
2018.4	0.781
2019.1	0.402
2019.2	0.995
2019.3	0.675
2019.4	0.731

Source: PNADC

Finally, we present placebo tests our First Stage results for six different cut-offs: 10, 20 and 30 days earlier and later than the real one. Since 2019.2 present most positive results, we focus on this one.

Figure 6



Source: PNADC 2019.2

Graphs above show that response to treatment in these placebo cutoffs is erratic and presents no clear pattern, nor it's statistically significant. Therefore, one may discard any other treatment impacts around our threshold.

5 Conclusion

Women present less participation and employment rate than men in many developed and specially in developing countries. Many authors found that some of this gender gap is explained by gender norms, by whose mothers are expected to allocate disproportional hours of their day to take care of their children. So, when there are kindergarten or school available, many researchers have been finding positive impact on mothers' labor outcomes.

Brazil is a upper-middle income country, with universalized Primary, but not early childhood Education. In 2018, Brazilian Supreme Court established that children a year younger than the minimal age of entrance in Primary School (6 years old) may still get enrolled by January or February if their birthday is earlier than April 1st. So, in the second quarter of 2019, we found a strong and significant discontinuity in the probability of children enrolled in Primary School around the Date of Birth of 2013-03-31.

Using a Fuzzy RDD and a Fixed-Effects Event-Study framework, we test the response of mothers labor market outcomes when their children are attending Primary School. We check for six quarters, from 2018.3 to 2019.4. Our maximum bandwidth used is of of 89 (that is, for children born from 2013-01-01 to 2013-06-28) in order to not include children born in 2012 (we use optimal bandwidths if they're smaller than 89 days). In our RDD framework, we use robust standard-errors corrected in our results, as proposed by Calonico et al. [2014], also using covariates as controls. For our Event-Study framework, besides household and year fixed-effects, we also include mothers' schooling and age as controls

In summary, we've showed evidence that (i) children joining school has a significant impact on maternal employment status, employment rate, hours in labor and income, but

(ii) no significant effect on working hours and formality - possibly due to composition changes, for women joining labor market might be less productive than average. There is also some weak evidence on participation rate, but it is not robust. Finally, (iii) Brazilian parents aren't systematically exploiting school entry rules, nor our first stage estimate is spurious by any neighbourhood cut-off assignment variable.

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Appendices

Table 5: Minimal Optimal Bandwidths, by dependent variable and quarter

Variable	03/2018	04/2018	01/2019	02/2019	03/2019	04/2019
Black mothers	356	412	276	468	449	459
Mothers' schooling	201	219	307	396	330	521
Mothers' Age	222	275	285	438	291	435
Urban Households	318	319	339	319	344	334
Mothers' Rate of Householders	277	368	388	259	417	543
Households in North Region	414	463	385	506	345	493
Households in Northeast Region	381	308	297	411	407	467
Households in Southeast Region	441	453	462	477	396	406
Households in South Region	440	401	263	383	426	508
Households in Midwest Region	360	306	380	440	401	348
First Stage (without covariates)	104	116	73	73	80	94
First Stage (with covariates)	102	117	74	70	77	101
Mothers' Employment Rate (without covariates)	106	112	146	103	103	101
Mothers' Participation Rate (without covariates)	92	112	90	69	78	118
Mothers' Formality Rate (without covariates)	112	118	148	131	122	129
Mothers' Employment (without covariates)	92	112	89	73	79	112
Mothers' log of Hours in Labor (without covariates)	91	112	87	72	77	113
Mothers' log of Working Hours (without covariates)	111	116	133	120	100	138
Mothers' log Income (without covariates)	92	111	83	73	78	109
Fathers' log Income (without covariates)	94	108	80	87	87	98
Mothers' Employment Rate (with covariates)	120	130	140	106	107	100
Mothers' Participation Rate (with covariates)	136	143	94	78	85	131
Mothers' Formality Rate (with covariates)	123	176	152	169	108	139
Mothers' Employment (with covariates)	141	143	92	99	91	136
Mothers' log of Hours in Labor (with covariates)	142	144	88	99	86	138
Mothers' log of Working Hours (with covariates)	98	142	113	114	96	137
Mothers' log Income (with covariates)	156	149	77	140	99	145
Fathers' log Income (with covariates)	110	123	81	88	77	81

Notes: If minimal optimal bandwidth is above 89, effective bandwidth is set to be 89.
Source: Estimates from PNADC, based on Calonico et al. [2014], Calonico et al. [2014] and Calonico et al. [2019].