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Restricting the timing of Elective CS: evidence from Brazil ♦

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Abstract

Brazil has one of the highest Cesarean Section (CS) rates in the world. It is a share of 58.3% reported by the Living Births Information System (SINASC) 2015-2017. It is well above the maximum rate of 15% recommended by the World Health Organization (WHO). In this paper, we estimate impacts and unintended consequences of the Resolution 2,144 from the Federal council of Medicine (CFM) on outcomes of Low Risk First Born births (LRFB). The Resolution introduces a minimum of 39th weeks of gestation for Elective CS. Elective CS before the 39th week rate dropped 2.78 percentage points, which is statistically significant and equivalent to a 24% decrease in this outcome's mean. We also find increases in birth's time length: the percentage of births happening before the 39th week decreased 2.34 percentage points, which is a decrease of 6% in its average. Our results suggest that Elective CS's were postponed from the 37-38th week to after the begin of the 39th week. We show that the policy had an unintended consequence once it seems to have changed the way potential spontaneous Natural Deliveries are anticipated from weekends to weekdays through Scheduled CS.

Keywords

Scheduled Cesarean Section. Dif-in-Dif. Policy.

Resumo

O Brasil possui uma das maiores taxas de cesáreas do mundo: uma parcela de 58,3% segundo o Sistema de Informação de Nascidos Vivos (SINASC) 2015-2017. A parcela está acima da taxa máxima de 15% recomendada pela Organização Mundial da Saúde (OMS). Neste artigo, estimamos os impactos e consequências não intencionais da Resolução 2.144 do Conselho Federal de Medicina (CFM) sobre os *outcomes* de Primogênitos de Baixo Risco (LRFB). A Resolução introduz um mínimo de 39 semanas de gestação para a cesárea eletiva. A taxa da cesárea eletiva antes da 39ª semana caiu 2,78 pontos percentuais, o que é estatisticamente significativo e equivalente a uma redução de 24% na média desse resultado. Também encontramos aumentos na duração do tempo de nascimento: a porcentagem de partos ocorridos antes da 39ª semana diminuiu

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2,34 pontos percentuais, o que é uma diminuição de 6% em sua média. Nossos resultados sugerem que as cesáreas eletivas foram adiadas da 37-38ª semana para após o início da 39ª semana. Mostramos também que a política teve uma consequência não intencional, uma vez que ela parece ter mudado a maneira como potenciais partos naturais são antecipados de fins de semana para dias de semana por meio da cesárea agendada.

Palavras-Chave
Cesárea agendada. Dif-in-Dif. Política.

JEL Classification
I20. I180.

1. Introduction

Brazil has one of the highest Cesarean Section (CS) rates in the world. Living Births Information System (SINASC) reported a total rate of 58.3% in 2015-2017. It is well above the 15% maximum rate recommended by the World Health Organization (WHO). In this paper, we estimate the impacts and unintended consequences from a specific Brazilian policy on outcomes of Low Risk First Born births (LRFB).[1]

The policy in analysis is Resolution[2] 2,144 from the Federal council of Medicine (CFM). It determines that the Elective CS can only be performed after the beginning of the 39th week of gestation, maintaining the woman's right to choose a CS, only limiting it to a minimum gestational length. The resolution also requires the registration of a document that declares the physician clearly and completely informed the woman of the risks and benefits of each birth type. In addition to that, the woman declares in the document that she freely chooses Elective CS as the birth type.

The main goal of the policy was to reduce problems related to premature delivery ensuring the fetus safety. Additionally, the policy aimed at informing mothers about both types of births. After the policy enactment in June 22, 2016, doctors who do not register the document or perform a Elective CS before the 39th week may lose their medical license.

[1] We follow as close as possible the definition of LRFB births used in Card et al. (2019)
[2] This resolution was written in 17 March 2016, but published and enacted in 22 June, 2016.

Ideally, Elective CS is a CS's performed in a risk-free pregnancy chosen by mother or by an agreement between physician and mother with symmetric information and with equal bargain power. Nevertheless, Elective CS affected by the policy includes pregnant women who might have been manipulated by the physician or coerced by the health care system to do an "Elective" CS.

We identify Elective CS as those CS that occurred before any physical sign that the labor had started. Note that the fact that labor had have not started before the delivery indicates that the choice for CS was decided before the due date. Also note that in our LRFB sample, there are no observed clinical conditions for scheduling a CS as the safest procedure. Hereinafter, we use the term Elective CS and Scheduled CS interchangeably.

This paper will estimate the policy effects on the following outcomes: Cesarean Section (CS) rates, Scheduled CS rates; Scheduled CS's that happen before the 39th gestational week, and births happening before the 39th week rate.

The policy effects are identified using a Diff-in-Diff regression. The units in the treatment groups are those births happening at private hospitals, these units are treated in periods after the policy. The control group units are those births happening at public hospitals. The reasons for choosing these treatment and control groups are described in the following three paragraphs.

First, regarding to the Brazilian Private Health Care System, according to Spinola (2016) *"Women in the private sector (...) are better able to choose the doctors (and hospitals) who will deliver their babies in the way they want. However, as expectant mothers are usually assisted by the same doctor during prenatal care and delivery, doctors may have more opportunity to encourage expectant mothers to change their minds according to their own preferences"*. Therefore, Scheduled CS in our LRFB sample are very likely to have been chosen by mothers or by an agreement between the mother and the doctor. Naturally, there are agreements with asymmetric information and unequal bargain power, we include them in our definition of Elective CS.

Second, in regards to the Brazilian Public Health System, according to Spinola (2016) *"women have less power to enforce their will and thus physicians are the main agent in the decision-making process"*. Furthermore, Women are not allowed to choose their physicians for prenatal care nor obstetrician to perform the birth; they also have less options of hospitals to choose from according to Nobrega (2015). Furthermore, Nobrega (2015) also argues that public hospitals have stricter guidelines for the childbirth practices. Finally, Spinola (2016) argues that physicians do not have incentives to schedule a CS because fear of litigation. Therefore, in the Public Health Care System, Scheduled CS's are very unlikely to have been chosen by the mother or by an agreement between the mother and the doctor.

In conclusion, it is very likely that the impacts of the policy are higher in the Private Health Care System relative to the Public Health Care System. In other words, the probability of a Scheduled CS to be chosen by the mother or by an agreement between doctor and mother (Elective CS) is higher in the Private Health System than in the Public Health System. One limitation of this paper is that our experimental groups might be contaminated. Nevertheless, if the policy effect contaminated the control group then our estimates can be seen as lower bounds of policy effects' absolute values. Furthermore, we show that our identification of Elective CS is robust, implying that it is safe to assume that the contamination in the treatment group is negligible.

Because of the policy, we expect a postponement of Scheduled CS's from before the 39th week of gestation to after the beginning of the 39th week. In other words, we expect that the Scheduled CS rate drops for births happening before the 39th week but increases for births happening after the beginning of the 39th week. Since a spontaneous Natural Delivery (ND) or a spontaneous CS may occur during this time, an increase in spontaneous births is expected between the 37-39th weeks. We also expect an increase in the gestational length because of the Scheduled CS postponement.

The timing restriction along with the required document explaining each procedure's risks and benefits might imply a cost to physicians as a restriction on how they conveniently anticipate births from non-working days to work days by performing a Scheduled CS instead of a spontaneous birth.

This manipulation by physicians is pointed out by Spinola (2016) and Costa-Ramón et al. (2020).

Measuring this expected policy effect is very important. As a matter of fact, Costa-Ramón et al. (2020) shows that unnecessary CS can increase the probability of asthma diagnosis from early childhood onward. They find that the effect is clinically and economically relevant. Nevertheless, they do not find consistent evidence that CS affects the probability of developing atopic diseases at large, type 1 diabetes or obesity.

The impact of the policy does not seem to decrease the overall Scheduled CS rate neither the CS rate at the usual 5% significance level. However, there are decreases in Scheduled CS at the 10% significance level. Scheduled CS's before the 39th week rate dropped 2.78 percentage points, which is statistically significant at 0.1% level and equivalent to a 24% decrease at this outcome's mean. We also find increases in birth's time length, which are statistically significant at 0.1%. As a matter of fact, the percentage of births happening before the 39th week decreased 2.34 percentage points, which is a decrease of around 6% in this outcome's mean. These results indicate that Scheduled CS's were postponed from the 37-38th week to after the beginning of the 39th week. We also show that the policy effects on the Scheduled CS before the 39th is lower for higher educated mothers. Finally, we also find out that the policy changed the way potential spontaneous ND are anticipated from weekends to weekdays through Scheduled CS.

Since one of the policy concerns was the high level of premature Elective CS in Brazil, the results we find are positive. If Scheduled CS's have negative impacts on newborn health, then our results are even more positive.

This research is divided into 7 sections, including this first one, the introduction. The second section details similar interventions to Resolution 2,144 and other related studies. The third section consists of an exploratory data analysis. The fourth section shows our identification strategy. The fifth section presents the regression specification; the estimations' results; the policy effect heterogeneity; and makes robustness exercises. The sixth section explores the unintended consequences of the policy. Finally, the seventh section concludes this research.

2. Interventions on time/type of delivery

In 2004, the United Kingdom's National Institute for Clinical Excellence (NICE) recommended that Elective CS should not be performed before completing 39 weeks. They argued that uncomplicated Elective CS before 39 weeks was associated with risk of respiratory morbidity in newborns. According to Gurol-Urganci *et al.* (2011) the proportion of Elective CS performed between 39 and 40 weeks increased from 39% to 63% between April 2000 (policy enactment) and February 2009 (last observed date), while the proportion of Elective CS deliveries done between the 37th and 39th week of pregnancy decreased from 49% to 30% .

The American College of Obstetricians and Gynecologists advised against elective deliveries being performed before 39 weeks of gestation. They claimed the reason was to avoid neonatal complications. They argued that a high number of elective deliveries were occurring before 39 weeks, so they developed and implemented a program to decrease the number of these early term elective deliveries. When analyzing the state of Utah in the United States, Oshiro *et al.* (2009) concluded that with institutional commitment, it is possible to substantially reduce and sustain a decline of elective deliveries before 39 weeks of gestation.

In regard to the evidence of neonatal outcomes of Elective CS gestational length, Glavind *et al.* (2013) conduced a Randomized Control Trial (RCT) to estimate differences in adverse outcomes between births at 38 weeks and 3 days and births at 39 weeks and 3 days. Among the measured outcomes were neonatal intensive care unit admission within 48 hours of birth, neonatal depression, and postpartum adverse events. The authors concluded that none of the outcomes measured had significant improvement with the additional gestational time.

Regarding Brazilian policies, the high C-section rate has been a concern since the oldest policy intervention we were able to find, which occurred in 1989. The Sao Paulo State Health Department defined the acceptance limit for CS's rate at 30% (SP,1989) blocking public payments of contracted hospitals with rates above it (public and contracted private hospitals).

There was also Ordinance 2,816 from 29 May 1998 and its redefinition in 1999 (Brazil, 1998). The aim of the policy was promoting ND delivery through financial incentives and childbirth practices. It is worth mentioning the following measures taken in the Public Health System by the Ordinance 2,816:

- Relative increase in physicians' fees for ND.
- Government included analgesia payment for ND.
- Government included payment for an obstetric nurse.
- Establishing the goal of reducing the CS's rate to a maximum of 40% until the end of the 2nd semester of 1998.
- Further reductions reaching a maximum rate of 30% in the 1st semester of 2000. The births performed above the established limit would not be paid.

The National Health Agency (ANS) published in September 2007 the Supplementary Health Qualification Program. Among some measures, there were new rules required for health care plans and insurers. Hospitals in insurers' network were required to have a minimum quality structure for ND's procedures. It also included low CS rates as a criterion in the overall insurers' quality index.

In 2013, there was a scoop in the news about childbirth practices in Brazil. The Federal Council of Medicine authorized obstetrician to charge additional fees for performing ND, a direct collection from the doctor to the patient, even if the insurer had already paid the physician for the birth. In contrast to that, the National Agency of Supplementary Health (ANS) stated that families should not have to pay extra fees, since ND and CS are covered by health care plans and insurers according to a specific law. Since this episode, obstetricians can charge extra fee for performing ND procedures and patients pay the extra fee whenever their chosen obstetrician requires so. This practice may have increased the relative price of ND in terms of CS procedures in the Private Health Care System. Furthermore, it might have increased the number of doctors offering ND delivery. Nevertheless, there is a lower number of pregnant woman willing to pay the difference between deliveries. The outcome of this practice is unknown and outside this research's scope.

On July 6, 2015 the new rules established by the National Agency of Supplementary Health (ANS) went into effect with the enactment of the Normative Resolution 368. The Resolution establishes that health plan operators/insurers, whenever requested, must publicize the CS rate by health care facility and by physician in their network. It requires the obstetricians in the health plan operators/insurers' network to use the Partogram, a graphic document that records everything that happens during the labor.

While this research was being written, the ANS was developing the Adequate Childbirth Project. The project begun in April 2015 and it aims at changing the current delivery model to promote ND; qualifying health care services in prenatal care and delivery birth; and finally, decreasing the numbers of unnecessary CS and possible adverse events. As a result, ANS reports that supported maternities had an average increase of 20% in the number of ND between April 2015 and October 2016 and that the social media campaigns achieved a significant number of visualizations.

3. Data description

This section describes the database we use and how we select the population of interest (LRFB) for the purpose of this research. The section also summarizes the variables we use in the analysis. In addition, we will summarize the sample's statistics by policy status and by hospital status as well (treatment status). Finally, we will display a graph of the the main outcome evolution.

The database consists of the Living Births Information System (SINASC) from 2015 to 2017 and the National Register of Health Establishments (CNES) 2015. We use the CNES only to assess whether the hospital is private or public. We define as private, those hospitals that do not have contracts with the Unified Health System (SUS). The public hospitals are integrated with the public health system: SUS. They consist of publicly owned facilities and privately-owned facilities that are affiliated with SUS through contracts for provision of health care services to public patients (Nobrega, 2015).

Our analysis restricts the population of total births in SINASC to only LRFB births (which will be defined soon). These births happened in Brazil between January 1, 2015 and December 31, 2017. After this restriction, and merging the SINASC with the CNES database, there are 1,902,430 birth observations. Before the policy enactment, there were 954,932 births and after the policy enactment there were 947,498 births.

There were some data set modifications.[3] Births observations reported as unknown were changed to missing.[4] The APGAR scores 1 and 5 minutes (*apgar1* and *apgar5*) that were equal to 99 were changed to missing.

The SINASC contains a lot of information. The focus of this research concerns on four sets of variables: (i) procedure characteristics such as birth date, hospital code, city where the procedure happened, type of birth, indicator for labor medically induced and indicator for birth that has happened before any sign that labor had started; (ii) gestation and pregnancy characteristics, such as type of pregnancy (single, twins, triplets or more), gestational length in weeks, number of prenatal consultations, month that the pre-natal care started and number of previous deliveries; (iii) maternal socio-economic characteristics, such as age, educational level, marital status, race; (iv) newborn characteristics and health outcomes, such as gender, indicator for congenital anomaly, fetus position in utero prior to birth, newborn birth weight and APGAR scores for one and five minutes. There is more information in SINASC, but we restrict attention to the mentioned variables above. We selected twenty-four of them. Their names and description are available in Table 1.

In order to define LRFB births we closely follow Card and Silver (2019). Nevertheless, our data sets are different from theirs, hence it is not possible to use exactly the same definition as they use. In this sense, we define LRFBs as first births, happening in term (37+ weeks gestation), singleton, vertex positioned fetus, excluding mothers under 18 or over 35 years old. We also exclude children with congenital anomaly and very low APGAR scores.[5] Finally, we exclude newborn weight outliers.[6]

[3] We did not remove any observation in this procedure, only changed some variables from sets of observations

[4] *marital_status, mothers_school, visits ,breech_present,month_prenatal and induced_labor.*

[5] lower than 3 for apgar 1 and 5 minutes.

[6] 1st pct or lower or 99pct or higher

The population restriction is done to keep births without observable risk, doing so allows us to further select Elective Scheduled CS according to our definition. We recognize that the our outcome variable (birth type choice) and our dependent variable (the policy enactment) may be correlated to the variables we used to select the sample, specially whether the birth happened after the beginning of the 37th week or not. Nevertheless, we believe that the self-selection in the sample is negligible. The reason is that the policy aimed at births happening at the 39th week, but we exclude births before the beginning of the 37th week. In this sense, there is a two-weeks difference between the gestations we exclude, and the gestations affected by the policy. Furthermore, the policy targets potential Elective CS, in which the mother can choose between ND or CS and therefore there is no risk in the pregnancy, this fact associated with our population of interest being the LRFB's births lead us to believe that self-selection is negligible again. Nevertheless, these are assumptions that cannot be tested.

Table 1 - Variable's description

Variable	Description	Variable	Description
birth_id	Child's identification	*mother_city_cd*	Mother's city
weight	Child's weight	*date*	Procedure date
apgar1	APGAR Score 1'	*hospital_cd*	Hospital code.
apgar5	APGAR Score 5'	*mothers_age*	Mother's age
n_prev_preg	Nº of previous pregnancies	*city_cd*	City of procedure realization
weeks	Gestational length in weeks	*month_prenat*	Month prenatal care started.
cs	Type of delivery : 0: Natural; 1: CS.	*marital_status*	1: Single; 2: Married; 3: Widow; 4: Separeted;
i_sex	0: female; 1: male.	*position*	Fetus position: 1:Cephalic; 2: Breech; 3:Transverse
induced_labor	0: No; 1:Yes;	*type_preg*	1: Unique; 2: Double; 3: Triple and more;
i_anomal	Congenital anomaly : 1: Yes; 0 : No	*cslabor*	1:Caesarean section before labor began; 0: CS after labor or Natural delivery:
mothers_school	Mother's educational attainament: 1: None; 2: 1 to 3 years; 3: 4 to 7 years; 4: 8 to 11 years; 5: 12 or more;	*mothers_race*	1: White; 2: Black; 3: Yellow; 4: Non-white; 5: Idigenous people.
birthplace	1: Hospital; 2: Other Health est.; 3: Home; 4: Others.	*visits*	Nº of prenatal consultations: 1: None; 2: 1 to 3; 3: 4 to 6; 4: 7 or more.

Source: Author's elaboration based on SINASC.

We construct one variable denoted as $policy_t$ assuming unit for periods post policy enactment until the last observable date and zero for periods before the policy enactment. It assumes value one between July 22, 2016 and December 31, 2017 and zero between January 1, 2015 and July 21, 2016. The policy was announced, implemented, and activated on June 22, 2016. After this date, physicians doing Elective Scheduled CS before 39 weeks of gestation could lose their medical license. Nevertheless, the policy was written by the CFM on March 17, 2016. We make robustness exercise changing the $policy_t$ variable according to this date. We verify that our results are robust to this change.

Table 2 provides continuous and dummy variables summary statistics for pre-policy periods, for post policy periods and for all periods included. Table 3 reports the categorical variables frequencies by policy status as well. In Table 2, *CS* refers to a dummy indicating CS procedure type: CS or ND; *Sched. CS* is a dummy assuming unit if the birth satisfies our definition of Scheduled CS and 0 otherwise; *Private* is a dummy for private hospital; *Before 39 week* is a dummy indicating if the birth procedure happened before the 39th week; *Labor Induced* is a dummy for procedures that had labor inducement; *apgar1* is the one minute apgar score; *apgar5* is the five minutes apgar score; *Weight* is the newborn weight; *Age* is the mother's age; *weeks* refers to how many weeks the gestation lasted; *Month Prenatal* is the month of pregnancy that the prenatal care consultations started; *Sex* is a dummy indicating if the new born is male. We emphasize that the number of total births in our sample is 1,902,430. The variables names in Table 3 are *Mother's Race, Marital Status* and *Educational Level*, they are all self-explanatory. In the following analysis, if the hospital is associated with SUS, then it is classified as public, otherwise it is classified as private.

Table 2 - Continuous Variables description by policy status

	CS	Sched CS	Private	Weeks	Before 39	Induced	Apgar 1
Policy 0							
Mean	0.59	0.30	0.21	39.06	0.33	0.23	8.44
Std. Error	0.49	0.46	0.40	1.25	0.47	0.42	1.04
Sum	566194	286536	196635		317813	211925	
Policy 1							
Mean	0.59	0.29	0.19	39.11	0.31	0.22	8.45
Std. Error	0.49	0.45	0.39	1.23	0.46	0.41	1.02
Sum	556208	271817	180576		290526	200521	
Total							
Mean	0.59	0.29	0.20	39.09	0.32	0.22	8.44
Std. Error	0.49	0.46	0.40	1.24	0.47	0.42	1.03
Sum	1122402	558353	377211		608339	412446	

	Apgar 5	Weight	Age	Visits	Month Prena	Sex
Policy 0						
Mean	9.43	3229	24.35	3.74	2.37	0.51
Std. Error	0.68	413	4.85	0.56	1.33	0.50
Policy 1						
Mean	9.43	3236	24.41	3.76	2.32	0.51
Std. Error	0.67	414	4.86	0.54	1.27	0.50
Total						
Mean	9.43	3233	24.38	3.75	2.34	0.51
Std. Error	0.67	413	4.86	0.55	1.30	0.50

Source: SINASC and CNES

Table 3 - Categorical Variables description by policy status

Mothers race				Education attainment			
	Policy 0	Policy 1	Total		Policy 0	Policy 1	Total
White	424,597	409,371	833,968	None	979	822	1,801
	50.91	49.09	100		54.36	45.64	100
Black	44,906	47,896	92,802	1 to 3	6,572	5,097	11,669
	48.39	51.61	100		56.32	43.68	100
Asian	3,835	4,114	7,949	4 to 7	68,146	57,208	125,354
	48.25	51.75	100		54.36	45.64	100
Brown	453,812	459,816	913,628	8 to 11	615,367	609,950	1,225,317
	49.67	50.33	100		50.22	49.78	100
Indigene	2,769	3,234	6,003	12 or more	254,174	265,147	519,321
	46.13	53.87	100		48.94	51.06	100
Total	929,919	924,431	1,854,350	Total	945,238	938,224	1,883,462
	50.15	49.85	100		50.19	49.81	100

Marital Status			
	Policy 0	Policy 1	Total
Single	402,272	415,445	817,717
	49.19	50.81	100
Married	355,044	341,790	696,834
	50.95	49.05	100
Widow	862	777	1,639
	52.59	47.41	100
Divorced	5,706	6,118	11,824
	48.26	51.74	100
Stable Union	184,071	176,377	360,448
	51.07	48.93	100
Total	947,955	940,507	1,888,462
	50.2	49.8	100

Source: SINASC.

As can be seen in Table 2, the share of CS is lower in post-policy periods (0.593 to 0.587), as the Sched. CS (0.300 to 0.287). The share of births before 39 weeks is 2.6 p.p. lower in post-policy periods (0.333 to 0.307). The share of private hospitals, induced labor, the month prenatal care started and apgar5 are lower in post-policy periods, but reductions are in smaller magnitudes. In addition to that, the variables Weeks, apgar1, weight, Age and Visits are slightly higher in post policy periods. The variable Sex does not change. In general, the variables' means are similar between pre and post policy periods because the policy was enacted in 22 July 2016, which is very close to the median of the days contained in our 2015-2017 sample.

Table 3 reveals a similar pattern for categorical variables' means between policy periods. Although there are few exceptions, the averages do not change so much between pre and post policy periods. It is worth mentioning the change in indigenous mothers' percentage from 46.13% before the policy to 53.87% after the policy and the change in the percentage of Widow mothers from 52.59% before the policy to 47.41% after the policy. Finally, there were changes in mothers' educational level, periods post policy had higher percentage of mothers with educational level of 12 years or more, but lower percentage of mothers with lower educational levels. We estimate that the mother's educational level was 5% higher in post policy periods.

We now turn attention to sample's descriptive statistics grouped by treatment status. Table 4 describes the same variables as Table 2, but they are grouped by hospital status instead of by pre-post policy periods. Since hospital status define the treatment and control groups in our identification strategy, these descriptive statistics deserve special attention. Note that we omit the total sample statistics description because they can be seen in the bottom of the previous Table 2.

Table 4 - Continuous Variables description by hospitals' status

	CS	Sched. CS	Policy	Weeks	Before 39	Induced	Apgar 1
Public							
Mean	0.53	0.23	0.60	39.17	0.30	0.24	8.40
Std. Error	0.50	0.42	0.49	1.27	0.46	0.43	1.05
Sum	804931	343350	910153		450797	362875	
Private							
Mean	0.84	0.57	0.57	38.74	0.42	0.14	8.59
Std. Error	0.37	0.50	0.50	1.06	0.49	0.34	0.93
Sum	317471	215003	216180		157542	49571	

	Apgar 5	Weight	Age	Visits	Month Prena.	Sex
Public						
Mean	9.41	3233.71	23.62	3.72	2.45	0.51
Std. Error	0.68	417.70	4.61	0.57	1.32	0.50
Private						
Mean	9.52	3228.76	27.45	3.87	1.94	0.51
Std. Error	0.62	395.16	4.62	0.42	1.12	0.50

Source: SINASC and CNES.

Table 5 - Categorical Variables description by hospitals' status

Mothers Race				Educational Attainment	
	Public	Private	Total		Public
White	594,646	239,322	833,968	None	1,753
	40.08	64.58	44.97		0.12
Black	78,149	14,653	92,802	1 to 3	11,340
	5.27	3.95	5		0.75
Asian	5,145	2,804	7,949	4 to 7	121,189
	0.35	0.76	0.43		8.04
Brown	800,124	113,504	913,628	8 to 11	1,071,056
	53.92	30.63	49.27		71.05
Indigene	5,727	276	6,003	12 or more	302,236
	0.39	0.07	0.32		20.05
Total	1,483,791	370,559	1,854,350	Total	1,507,574
	100	100	100		100

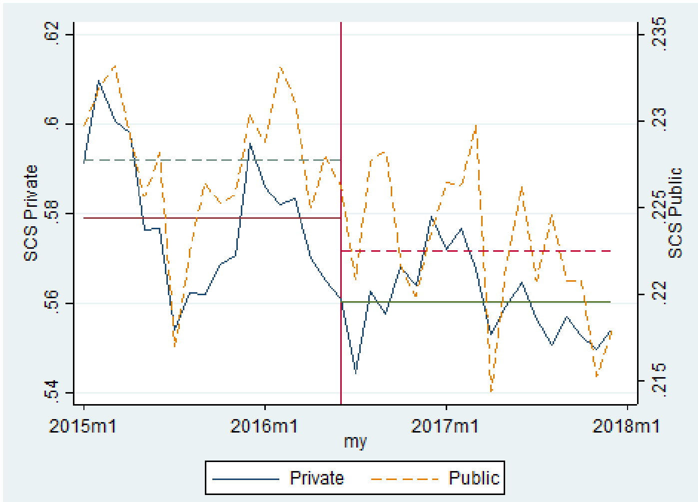
Marital Status			
	Public	Private	Total
Single	706,843	110,874	817,717
	46.72	29.51	43.3
Married	466,514	230,320	696,834
	30.84	61.31	36.9
Widow	1,313	326	1,639
	0.09	0.09	0.09
Divorced	8,391	3,433	11,824
	0.55	0.91	0.63
Stable Union	329,713	30,735	360,448
	21.8	8.18	19.09
Total	1,512,774	375,688	1,888,462
	100	100	100

Source: SINASC and CNES.

Table 4 reveals the many differences between the Brazilian Private Health Care System and the Public Health Care System. As a matter of fact, it can be seen that CS rate is 0.528 in Public Hospitals and 0.842 in private hospitals. Sched. CS rates are lower in public hospitals (0.2250 vs 0.570). The share of births that happen before the 39th week is also lower in public hospitals (0.296) than in public hospitals (0.418). Measures of newborns' health and numbers of pre-natal consultations Visits are marginally better at private hospitals. Mothers are significantly younger in public hospitals (23 vs 27) (Age). Labors are more frequently induced in public hospitals (Labor Induced) (0.244) than in private hospitals (0.135) and prenatal care starts later in public hospitals (Month Prenatal) (2.446 vs 1.940). In addition to that, gestational length is longer in public hospitals (39.172 vs 38.743). In summary, the treatment and control groups are not as similar as the pre-post policy groups. Nevertheless, levels differences between treatment and control groups do not mean that their trends are not parallel (an identification hypothesis of our estimation approach)

Table 5 shows that private hospitals have higher percentages of white, married, and highly educated mother, lower percentage of brown, black, indigenous, single, and low educated mothers. All in all, these variable's descriptive statistics and relative frequencies reveal the high inequality between the private and the Public Health Care System.

Figure 1 reports the monthly averaged Scheduled CS rate by hospital status in our restricted sample. The blue time series refers to private hospital Scheduled CS rate and the dashed orange time series refers to public hospitals Scheduled CS rate. The vertical line refers to the policy's date of enactment. The horizontal lines are averages of each hospital status estimated for pre and post policy time periods.



Source: SINASC and CNES.

Regarding pre-post periods average differences, Figure 2 shows that Scheduled CS in private hospitals were 2 p.p. higher in periods after the policy. Scheduled CS in public hospitals were only 0.5.p.p higher in periods after the policy.

It is noteworthy that the outcome levels differ between treatment group and control group, but this is not a concern for our identification strategy if each group (treat. and control) pre-trends are parallel and would continue to be parallel in the absence of the policy.

4. Base of identification strategy

Our approach consists of using the Diff-in-Diff methodology. We consider a generic Diff-in-Diff method, which we adapt from Angrist and Pischke (2008). We denote the potential outcome in the case that a birth in our LRBF sample has been exposed to the policy regardless if it was actually exposed or not as *Y(1)*; it assumes value 1 if it realizes a Scheduled CS (*Y(1)=1*) and assumes value 0 (*Y(1)=0*) if the potential outcome was spontaneous CS or ND. Similarly, denote *Y(0)* a potential outcome, but in the case that it would not have been exposed to the policy, regardless if it was actually exposed or not. It is worth note that we observe only $Y(1)=1$ and $Y(0)=0$

The ideal experiment would be to implement the policy for a while and observe each birth outcome $Y(1)$, then come back in time and not implement the policy, observing every population outcome $Y(0)$. Another possibility is to randomly assign the policy to a group of births, the treatment group would be denoted as outcome $Y(1)$. Meanwhile those births that were not drawn from the random assignment are in the control group with outcome denoted as $Y(0)$. The average treatment effect of interest is $E[Y(1)-Y(0)]$. In other words, it is the percentage point changes in the Scheduled CS rate caused by the policy. Note that we could change Scheduled CS for any of our outcomes of interest, such as a dummy indicating a CS, dummy indicating Scheduled CS that happened before the 39th week or dummy indicating whether the birth happened before the 39th week of gestation.

If births were randomly assigned between periods before and after the policy, the average policy effect would be the average Scheduled CS rate difference between both periods, which is estimated to be a Scheduled CS rate decrease of 1.3 p.p. as can be seen in Table 2. However, they are not randomly assigned in this way. The changes in Scheduled CS rate between periods could be due to other facts not related to the policy implementation, to name a few: seasonality, trend, preferences and informational changes, hospitals costs, physicians' hourly wage changes, etc.

In order to solve this problem, the births are put into groups classified as treatment (potentially affected by the policy) and control group (potentially unaffected by the policy). We assume that births in private hospitals are the treatment group and births in public hospitals are the control group for reasons already argued in the Introduction.

Our identification assumption is that the trend of treatment and control groups' outcomes are parallel before the policy enactment and they would continue to be parallel if the policy were not implemented.

We index a birth with i, the hospital status of this birth is indexed by h and the month of the year that this birth has happened is indexed by t. We decompose the Conditional Expectation Function (CEF) of $Y_{iht}(0)$ as in the following Equation 1:

$$Y_{iht}(0) = E[Y_{iht}(0)|h, t, X] + \varepsilon_{iht} \tag{1}$$

Let's also assume that the Conditional Expectation Function (CEF) is linear as in the following Equation 2:

$$E[Y_{iht}(0)|h, t, X] = \theta_h + \lambda_t + \alpha X \tag{2}$$

Where θ_h refers to group specific effects (treatment or control), λ_t refers to time specific effects, X is a vector of control variables and ε_{iht} follows an i.i.d random shock. We believe that the CEF linearity assumption is an adequate functional form to fit our strategy for the following reasons. First, the linear form is the one that has the Minimum Mean Square Error. Secondly, according to Kahn and Lang (2020) if one assumes that the CEF is linear, then a p.p. variation in the control group in the counterfactual case (absence of policy) would be accompanied with a Δp.p. variation in the counterfactual treatment group (without policy) as well. Since we are measuring the policy effects on outcomes in Δp.p., then it is a valid functional form. Nevertheless, our strategy is limited in cases in which there are confound factors affecting the births in public hospitals, but not affecting births in the private hospitals after the policy enactment or vice and versa.

We also assume that the policy effects are constant:

$$E[Y_{iht}(1) - Y_{iht}(0)|h, t, X] = \beta \tag{3}$$

The following paragraphs demonstrates how our assumptions led us to our main specification.

We can write, omitting the subscripts, the observable \$Y\$ in function of the potential outcomes as:

$$Y = Y(0) + [Y(1) - Y(O)]D \tag{4}$$

Where D is a dummy that assumes one if the potential outcomes are treated and zero otherwise. Then, taking the conditional expectation on Equation 4 and substituting Equation 2 and Equation 3 on Equation 4, we obtain the following DiD-in-Diff linear regression:

$$Y_{iht} = \theta_h + \lambda_t + \beta D_{ht} + \alpha X + \epsilon_{iht} \tag{5}$$

Where θ_h refers to group specific effects, λ_t refers to time specific effects, D_{ht} is a dummy that assumes value one for births happening in private hospital after the policy enactment and zero otherwise, X is a vector of control variables and ϵ_{iht} follows a i.i.d random shock. The next section will estimate β, our coefficient of interest. It refers to the policy effects on the outcomes of interest.

5. Estimations

a. Main specifications

We estimate the policy effect on four outcomes of interest. The first is the one specified in the previous Section: a dummy assuming unit if the procedure was a Scheduled CS and assuming value zero if it was a spontaneous CS or ND. The second is a dummy assuming unit if the procedure was a Scheduled CS before the 39th week and zero otherwise; the third is a dummy assuming unit if the procedure was a CS (regardless if it was spontaneous or Scheduled) and zero if it was a ND; the fourth is also a dummy indicating if the birth (any type) was before the 39th week.

All the outcomes will be denoted as *Y*. The following linear equation specification refers to our DiD identification using OLS.

$$Y_{iht} = c + \delta_1 pol_t + \delta_2 priv_h + \beta pol_t * priv_h + \gamma X_{iht} + \epsilon_{iht} \tag{6}$$

Where i indexes births, h hospital and t time, the dependent variable, *Y*, represents any of the mentioned outcomes. The pol_t variable assumes unit in periods post policy and 0 for periods before policy. $priv_h$ assumes unit if the hospital is private and zero otherwise. X_{iht} is a vector of covariates consisting of: categorical variable of *mothers age*; categorical variable of mother's race *mothers_race*; grouped categorical variable of mother's schooling attainment *mothers_school*; and categorical variable of the mother's marital status *marital_status*. These variable descriptions are reported in Table 1. The term ϵ_{iht} is a random i.i.d. shock. Note that the policy effect is captured by the coefficient β.

We estimate Equation 6 in three ways, they are called Model I, II and III. Model I add to Equation 6 month, year and day of the week fixed effects. It clusters standard errors at hospital level, but it excludes the vector of covariates X_{iht}. Model II is similar to Model I, but it adds hospital fixed effects and estimate clustered standard errors at city level. Model III is our preferred specification; it has time and hospital fixed effects and adds the vector X_{iht}. The Tables 6 to 9 report the policy effect estimates for all outcomes.

Table 6 - Outcome: Scheduled CS

	I	II	III
$pol_t * priv_h$	-0.0137	-0.0163	-0.0174
	0.0106	0.00924	0.0093
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
Std. Cluster Level	Hospital	City	City
N	1902430	1902430	1830813
adj.R2	0.101	0.324	0.332

* p<0.05, **p<0.01,***p<0.001

Table 7 - Outcome: Sch. CS before 39th week

	I	II	III
$pol_t * priv_h$	-0.0278***	-0.0269***	-0.0278***
	0.00552	0.00493	0.00496
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
Std. Cluster Level	Hospital	City	City
adj.R2	0.05	0.162	0.167

* p<0.05, **p<0.01,***p<0.001

Table 8 - Outcome: CS

	I	II	III
$pol_t * priv_h$	-0.0034	-0.00463	-0.00521
	0.00409	0.00336	0.00355
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
Std. Cluster Level	Hospital	City	City
N	1901845	1901845	1830312
adj. R2	0.074	0.236	0.256

* p<0.05, **p<0.01,***p<0.001

Table 9- Outcome: Births before 39th week

	I	II	III
$pol_t * priv_h$	-0.0231***	-0.0217***	-0.0234***
	0.00429	0.00347	0.00364
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
Std. Cluster Level	Hospital	City	City
N	1902430	1902430	1830813
adj. R2	0.05	0.162	0.167

* p<0.05, **p<0.01,***p<0.001

The following results mention all policy effects β estimated within our preferred specification: Model III. Table 6 reports a 1.7 p.p. decrease at the Scheduled CS rate, which corresponds to a 5.7% decrease in the overall rate. Estimations are not significant at the 5% level. Meanwhile, Scheduled CS before the 39th week decreased by 2.8 p.p., which is equivalent to a 24.5% decrease at this outcome mean and it is statistically significant at the 0.1% level. The overall CS rate had a 0.5 p.p decrease, but it is not statistically significant at the 5% level. The share of births that happened before the 39th week decreased 2.3 p.p., which is equivalent to a 6% decrease in the number of births happening before the 39th gestational week, this estimate is statistically significant at the 0.1% level.

These results present evidence that the policy was effective in reducing the Scheduled CS before the 39th week. In addition to that, the policy also made the gestation length increase as the share of births before the 39th week reduced by 6%. It is important to note that if the policy also affected the public hospitals, then our estimates consist of lower bounds of the policy effects' absolute value. The CS rate and Scheduled CS have not had any significant effect at the 5% significance level.

The date that the policy was enacted and announced to the date that the policy was written (March 17, 2016) does not alter the results. The qualitative results remain the same, statistical significance as well, but the coefficients have smaller magnitudes.

b. Policy effect heterogeneity

This subsection deals with the policy effect's heterogeneity on the outcomes that were statistically significant: Scheduled CS rate before the 39th week and rate of births happening before the 39th week. First across mothers' educational levels, second across pregnant women's level of health care services consumption.

The following regression is specified in order to capture the policy effect across mother's educational attainment:

$$\mathrm{Y_{iht}} = \mathrm{c} + \delta_1 \mathrm{pol_t} + \delta_2 \mathrm{hs_i} + \delta_3 \mathrm{priv_h} + \beta_1 \mathrm{pol_t} * \mathrm{priv_h} + \omega_1 \mathrm{hs_i} * \mathrm{priv_h} + \omega_2 \mathrm{hs_i} * \mathrm{pol_t} + \sigma_1 \mathrm{hs_i} * \mathrm{pol_t} * \mathrm{priv_h} + \gamma \mathrm{X_{iht}} + \eta_{\mathrm{iht}} \tag{7}$$

Where, Y_{iht} is the outcome(dummy) of birth i in hospital h happening at period t. As before, pol_t assumes unit in post policy periods. The variable hs_i is a dummy assuming unit if the mother has 12 years of education or more and assuming value 0 if she has less than 12 years of education. The variable $priv_h$ is a dummy assuming unit if the hospital is private hospital; X_{iht} is the same vector of covariates used in Equation 6; η_{iht} is an i.i.d. random shock.

In this setting, β_1 measures the policy effect on Scheduled CS rate before the 39th week in percentage points exclusively for mothers with less than 12 years of education. On the other hand, $\beta_1 + \sigma_1$ measures the same policy effect, but for mothers with 12 or more years of education. As a result, σ_1 is the difference of policy effects between mothers with more than 12 years of education and those with less years of education.

We now consider the policy effect differences among mothers with different amounts of prenatal health care services. The categorical variable $visits_i$ groups the number of prenatal consultations into 4 categories: (1) no prenatal consultation, (2) 1 to 3 consultations, (3) 4 to 6 consultations; and (4) 7 or more consultations. We create the new variable vis_i, which is a dummy assuming unit if the mother had 7 or more appointments and 0 otherwise. The aim is to capture the different policy effects between groups of mothers that had different numbers of consultations. The following regression is specified to capture this heterogeneity.

$$Y_{iht} = c + \delta_1 pol_t + \delta_2 vis_i + \delta_3 priv_h + \beta_2 pol_t * priv_h + \omega_1 vis_i * priv_h + \omega_2 vis_i * pol_t + \sigma_2 vis_i * pol_t * priv_h + \gamma X_{iht} + u_{iht} \quad (8)$$

Where, Y_{iht} is the outcome(dummy) of birth i in hospital h happening at period t. As before, pol_t assumes unit in post policy periods. The variable vis_i is a dummy assuming unit if the mother had 7 or more pre-natal consultations and assuming zero otherwise. The variable $priv_h$ is a dummy assuming unit if the hospital is private hospital; X_{iht} is the same vector of covariates used in Equation 6 ; u_{iht} is an i.i.d. random shock..

We estimate Equations 7 and 8 in two different ways - Model A and Model B. Model A estimates Equations 6 and 7 excluding covariates X_{iht} and clustering standard errors at hospital level. Meanwhile, Model B includes covariates X_{iht}; includes month, year, and day of the week fixed effect; and clusters standard errors at the hospital level as well.

In the following Tables 10 and 11, the first column (A-SCS) refers to estimation of Model A using the Scheduled CS before the 39th week as dependent variable. Likewise, the second column (B-SCS) refers to the estimation of Model B also using the Scheduled CS before the 39th week as dependent variable. Meanwhile, columns 3 (A-Births) and 4 (B-Births) make estimations using Model A and B with births before the 39th week as dependent variables respectively.

Table 10 - Education Heterogeneity

	A-SCS (1)	B-SCS (2)	A-Births (3)	B-Births (4)
$pol_t * priv_h$	-0.0254***	-0.0255***	-0.0237***	-0.0234***
	0.00643	0.00627	0.0061	0.00593
$hs_i * pol_t * priv_h$	0.0133*	0.0137*	0.00762	0.0075
	0.00621	0.00629	0.00597	0.00596
Time related F.E.	YES	YES	YES	YES
Covariates	NO	YES	NO	YES
Std. Cluster Level	Hospital	Hospital	Hospital	Hospital
N	1918624	1862453	1918624	1862453
adj. R2	0.062	0.073	0.021	0.026

* p<0.05, **p<0.01,***p<0.001

Table 11 - Health Care Consumption Heterogeneity

	A-SCS (1)	B-SCS (2)	A-Births (3)	B-Births (4)
$pol_t * priv_h$	-0.0240**	-0.0263***	-0.0245**	-0.0264**
	0.00797	0.00766	0.00897	0.00869
$vis_i * pol_t * priv_h$	-0.00186	-0.00132	-0.00199	-0.00139
	0.00706	0.00695	0.00785	0.00776
Time related F.E.	YES	YES	YES	YES
Covariates	NO	YES	NO	YES
Std. Cluster Level	Hospital	Hospital	Hospital	Hospital
N	1918624	1847636	1918624	1847636
adj. R2	0.051	0.072	0.017	0.026

* p<0.05, **p<0.01,***p<0.001

Since estimation of Models B are our preferred (Columns 2 and 4), the following comments are only about their results. Table 10 shows a lower policy effect for mothers with 12 years of education or more, a liquid effect of - or 1.8 p.p.. The policy effect on mothers with lower educational attainment is 2.5 p.p.

Columns 3 and 4 of Table 10 show that the policy effect on the rate of births happening before the 39th week does not vary by mothers' educational level

As can be seen in Table 11, mothers that went to more pre-natal consultations or less pre-natal consultations are equally affected by the policy. Although point estimates indicate that the policy was more effective among mothers with more prenatal care consumption, they are not statistically significant at 5% level.

In summary, the reduction in Scheduled CS ratio before the 39th week is significantly different among mothers' educational attainment, mothers with 12 years of education or more are had a lower policy impact. This is probably because the information provided by the physician, required by the policy, was more valuable to mothers with less educational years. On the other hand, the policy effect on share of births before the 39th week has no heterogeneity between mothers' educational attainment. Finally, there is evidence that the policy effects on both outcomes have no heterogeneity between mothers with more or less prenatal care consultation.

c. Robustness exercise

In this section, we do three main robustness exercises. The first test shows positive results towards the robustness of our identification strategy. The second test presents evidence that the experimental groups had no parallel trends for the period 2012-2014 (before our sample of 2015-2017). Nevertheless, when allowing trends to linearly vary between experimental groups, a placebo policy effect has no significant effect on our main outcomes implying that the null hypothesis of no parallel trend is rejected for this different specification. The third test verifies that the policy had no effect on high-risk Scheduled CS and high-risk Births before the 39th week. The following paragraphs detail each test and explain how they influence our results.

The first test is a new specification which adds a linear trend that varies between experimental groups. According to Angrist and Pischke (2008) *"This allows treatment and control states (groups) to follow different trends in a limited but potentially revealing way. It's heartening to find that the* *estimated effects of interest are unchanged by the inclusion of these trends, and discouraging otherwise"*. In this sense, the following model is specified for the outcomes that were statistically significant in our preferred specification:

$$Y_{iht} = c + \delta_1 pol_t + \delta_2 priv_h + \alpha_1 t + \alpha_2 t * priv_h + \beta_t pol_t * priv_h + \gamma X_{iht} + \epsilon_{iht} \quad (9)$$

Where pol_t, $priv_h$ and X_{iht} have already been defined under Equation 6. t is a linear trend.

The estimation of the policy effect β_t for both outcomes are reported in Tables 12 and 13.

Table 12 - Adding trends: Scheduled CS before 39th week

	I	II	III
$pol_t * priv_h$	-0.0105	-0.0111*	-0.0101
	0.0065	0.00562	0.00575
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	1918624	1918624	1847636
adj. R2	0.049	0.162	0.166

* p<0.05, **p<0.01,***p<0.001

Table 13 - Adding trends: Births before 39th week

	I	II	II
$pol_t * priv_h$	-0.0172***	-0.0173***	-0.0166***
	0.00512	0.00439	0.00433
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	1918624	1918624	1847636
adj. R2	0.016	0.051	0.057

* p<0.05, **p<0.01,***p<0.001

Tables 12 and 13 show that adding trends that vary between groups does not change our qualitative results. Our baseline result of policy effect is a decrease of 2.78 p.p in Scheduled CS rate, but the policy effect estimated by our preferred specification (column III) in Equation 9 drops to 1.01 p.p.. In addition to that, their p-values increased from 0.001 to 0.077. In regards to the *Births before the 39th week* outcome, our baseline policy effect is a 2.34 p.p. decrease in its rate, while the estimation of β_t in Equation 9 is a 1.66 p.p. decrease, it is also statistically significant at the 0.1% level. In summary, our results are robust to allow different linear trends for experimental groups. Although their magnitude diminishes.

The second test refers to a pre-trending test. For this test, we use the SINASC and the CNES for the period from 2012 to 2014. As mentioned in Section 4, the hypothesis that underlies our work is that trends of outcomes at private and public hospitals were parallel before the policy and would continue to be parallel in the absence of the policy. In this sense, we test if trends were parallel before the policy. In order to do this, we changed the pol_t variable to assume unit between June 22, 2013 and December 31, 2014. Then we estimate the same model specified in Equation 6 for the outcomes that were statistically significant (*Scheduled CS before 39 weeks* and *Births before 39th week*). This test do not reject the null hypothesis of no parallel trends. These results are omitted, but available upon request.

In contrast to the previous test, we now proceed to estimate β_t specified in Equation 9 instead of β in Equation 6 but for the sample 2012-2014. This exercise tests if the previous placebo policy effect found is robust. The results can be seen in Tables 14 and Table 15.

Table 14 – Placebo: Sched CS before 39th week

	I	II	III
$pol_t * priv_h$	-0.00623	-0.00808	-0.00744
	0.00781	0.00654	0.00638
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	1819195	1819195	1749795
adj. R2	0.074	0.199	0.205

* p<0.05, **p<0.01,***p<0.001

Table 15 - Placebo: Births before 39th week

	I	II	II
$pol_t * priv_h$	0.00104	-0.00089	-0.00006
	0.006	0.00513	0.00508
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	1819195	1819195	1749795
adj. R2	0.029	0.073	0.08

* p<0.05, **p<0.01,***p<0.001

Table 14 shows that the placebo policy had no statically significant effect on *Scheduled CS before the 39th week* when using the Equation 9 and the 2012-2014 sample. Table 15 reports similar results, the placebo policy had no effect on the rate of *birth before the 39th week*. Note that the same coefficients estimates of Equation 9 were statistically significant when using the 2015-2017 sample. This result shows that the placebo policy effect is not robust and that we can not reject the null hypothesis of non--parallel trends whenever using Equation 9.

Last but not least, the third robustness exercise estimates the policy effects on a different population. We select births with observable medical conditions indicating that a Scheduled CS is the safest procedure. We expect that the policy does not affect these high-risk Scheduled CS because it is very unlikely that they are Elective Scheduled CS.

The sample is a new restriction of the birth population reported in SINASC 2015-2017. Note that we do not use the previous restriction of LRFB births, which means that we include all gestational weeks. We restrict the population to newborns with weights lower than 2000g or above 4000g, fetus in transverse or breech position, we include fetus in cephalic position only if he has congenital anomaly and we include only mothers under 16 years old or above 40 years old.

Using conversations we had with obstetricians as a baseline, we assume that performing CS in these observable high-risk births is a standard practice among the obstetrician community in Brazil. Therefore, each one of these restrictions favors Scheduled CS for maximizing the safety of the mother and the baby. Since we use all the restrictions together, the realization of Scheduled CS in the new restricted population is very likely due to medical reasons rather than by mother's choice.

The estimates of the policy effect specified in Equation 6 for the high-risk Scheduled CS are reported in the following Table 16. Meanwhile, Table 17 reports estimates of the policy effect specified in Equation 6 for high-risk births before the 39th week.

Table 16 - High-risk Scheduled CS before 39th week

	I	II	III
$pol_t * priv_h$	0.00346	0.0177	-0.00441
	0.0254	0.027	0.0209
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	7516	7516	6569
adj. R2	0.01	0.237	0.226

* p<0.05, **p<0.01,***p<0.001

Table 17 - High risk births before 39th week

	I	II	II
$pol_t * priv_h$	-0.0251	-0.0299	-0.0588
	0.0419	0.0445	0.0425
F.E.related to time	YES	YES	YES
F.E. of Hospital	NO	YES	YES
Covariates	NO	NO	YES
N	7516	7516	6569
adj. R2	0.036	0.205	0.21

* p<0.05, **p<0.01,***p<0.001

Table 16 reports that the policy does not affect high-risk Scheduled CS before the 39th week. Conversely, Table 17 shows that the policy effect on high-risk births before the 39th week is also statistically insignificant.

In summary, the first test we made proceeded with a new specification adding a linear trend that varies between control and treatment groups. In this test, we reject the null hypothesis of no policy effect in our preferred specification of Equation 9 at 7% significance level. Therefore, our results remain valid when we allow for different linear trends between experimental groups. The second test fails to reject the hypothesis of no parallel pre-trends, but this result can be attenuated when analyzing the interventions done in 2013, which were described in Section 2. Notwithstanding, we were able to reject the hypothesis that a placebo policy in periods before the policy had any impact on our outcomes whenever adding linear trends that vary between treatment and control groups. This last test is equivalent to rejecting the null hypothesis of no parallel trends for this different specification. Finally, the third test makes us even more confident on our identification of Elective CS because the policy has no statistically significant effect on high-risk Scheduled CS neither on high-risk births before the 39th week.

6. Policy's unintended consequences

We now explore an unintended consequence of Resolution 2,144. As discussed in the introduction, Spinola (2016) present evidence that doctors manipulate birth-time and/or type without clinical reasons. The authors focus on public hospitals in order to estimate the manipulation that occurs purely by physicians' convenience. The authors find that births without risk that could have happened after spontaneous labor on non-working days are anticipate to workdays mainly through the use of Scheduled CS (Spinola, 2016). Gans and Leigh (2008) also provide evidence that physicians manipulate birth time and/or type without clinical reason. They find that births are scheduled away from physicians' inconvenient days to convenient days. Once we have established that the policy diminished the rate of Scheduled CS before the 39th week, the question of whether and how the policy affects the birth type/time manipulation by doctors arises.

After the policy enactment, doctors/parents cannot anticipate potential spontaneous births at the 37-38th week. In particular, doctors are now unable to change birth's time from non-working days to workdays by changing a spontaneous birth to a Scheduled CS, at least for those happening at the 37-38th week. In this sense, we expect that the policy would increase the spontaneous ND rate on weekends for births before the 39th week. In other words, the policy might have had an unintended consequence a restriction on the manipulation of spontaneous births from non-workdays (weekends) to workdays (weekdays) before the 39th week due to non-medical reasons. It is noteworthy that the restriction of not anticipating spontaneous births through Scheduled CS is binding only for those procedures before the 39th week and after the policy enactment.

Spinola (2016) focus on public health system in order to isolate the convenience effect from financial and other mechanisms to manipulate birth type/time. However, this does not mean that the manipulation did not occur in private hospitals as well. In this sense, we will not use our traditional treat-control groups in the following analysis.

We want to assess the policy effect on the birth type/time manipulation. Since potential spontaneous births happening on weekends are manipulated to scheduled CS on weekdays, then it is expected that the policy increases the difference of spontaneous ND rates between weekends and weekdays for births before the 39th week. We estimate the following coefficient, that can be written as a conditional expectation:

$$\beta_w = \{E[Y|pol = 1, wkd = 1; W] - E[Y|pol = 1, wkd = 0; W]\} \\ - \{E[Y|pol = 0, wkd = 1; W] - E[Y|pol = 0, wkd = 0; W]\} \quad (10)$$

Where β_w is the conditional expectation of spontaneous ND rates on weekends that were not anticipated to workdays through Scheduled CS correlated with the policy enactment. The expectation is conditioned on W, a random variable assuming values between 37 to 45. is a dummy indicating post policy periods. wkd assumes unit if the birth happened during a weekend and 0 if it happened on a weekday.

The mechanisms behind the policy effect is more easily understood defining the inequalities resulted in β_w estimates, which can be represented in a dif-in-dif Equation.

Suppose that β_w is positive and statistically significant for gestational week $W = w$. Then, the following inequality holds:

$$\{E[Y|pol = 1, wkd = 1, W = w] - E[Y|pol = 1, wkd = 0, W = w]\} > \{E[Y|pol = 0, wkd = 1, W = w] - E[Y|pol = 0, wkd = 0, W = w]\} \quad (11)$$

The inequality implies that the outcome's difference between weekends and weekdays in periods post policy is greater than the same outcome's difference in periods before the policy for gestational week $W = w$.

The following equation assumes that the condition expectation β_w is linear and that it is independent of the random variables $pol * W$:

$$Y_{it} = c + \delta pol_t + \psi wkd_t + \varphi_w + \phi pol_t * wkd_t + \sum_{w=37}^{42} \beta_w pol_t * wkd_t * I_w + \gamma X_{it} + \epsilon_{it} \quad (12)$$

We estimate Equation 12 for two outcomes Y: dummy for Scheduled CS and dummy for spontaneous ND. The variable wkd_t is a dummy indicating that the date of birth was on a Saturday or on a Sunday, φ_w is a vector of gestational weeks specific effects and I_w is a dummy for each gestational week $W = w$, ϵ is a i.i.d. shock. Note that some interaction terms were excluded in Equation ϵ_{it}, we did so because they were highly correlated with the other independent variables. The coefficients are relative to the following omitted realization values of variables: 37th week, pre-policy periods and workdays. Table 18 shows estimates of β_w as a function of W relative to the 37th week. The first Column refers to the outcome Scheduled CS and the second refers to the outcome of spontaneous ND.

Table 18 - Unintended Consequences

	Sched. CS (1)	Spont.ND (2)
$pol_t * wkd_t * I_{38}$	-0.0446***	0.0348***
	0.00513	0.00688
$pol_t * wkd_t * I_{39}$	-0.0125**	0.0132*
	0.00475	0.00644
$pol_t * wkd_t * I_{40}$	0.0370***	-0.024***
	0.0048	0.00656
$pol_t * wkd_t * I_{41}$	0.0519***	-0.037***
	0.00567	0.00758
$pol_t * wkd_t * I_{42}$	0.0275***	-0.0101
	0.00835	0.0124
$pol_t * wkd_t * I_{43}$	0.0327*	-0.0279
	0.0143	0.0225
$pol_t * wkd_t * I_{44}$	0.0167	-0.00848
	0.0184	0.0325
$pol_t * wkd_t * I_{45}$	0.00488	0.109*
N	1527806	1527806
adj. R2	0.016	0.006

* p<0.05, **p<0.01,***p<0.001. Estimates are relative to the 37th week

The estimates on Scheduled CS rate (Column 1) had opposite signals from those obtained in estimates of spontaneous ND (Column 2).

It indicates that birth/type manipulation increased for births after the 39th week in periods post policy compared to pre policy periods. The manipulation of Scheduled CS to weekdays are more intense for non-binding weeks in post policy periods.

Results in Column 2 of Table 18 indicate that spontaneous ND in the 38-39th weeks rates on weekends increased by 3.5 p.p (7.6%) and 1.3p.p (2.8%) relative to weekdays and relative to policy enactment periods. However, this result reverts for births after the 39th week. There are decreases in spontaneous ND on weekends relative to workdays for periods post policy after the beginning of the 39th week. Spontaneous ND rate decreased 2.5 p.p (5.4%) in the 40th week and 3.7 p.p (8.0%) in the 41th week on weekends relative to weekdays due to the policy enactment. Note that the coeficients' sign are opposite after the 39th gestational week (when the policy stopped being binding).

We point out that our estimates do not consist in causal evidence of the policy effect. Though, they are correlated to the policy enactment. In addition to that, if coefficients' signs were random, the probability of having our sequence of negative and positive sign in Scheduled CS and in spontaneous ND outcome would be.

$$\left(\frac{\binom{8}{2}}{2^8}\right)^2 = 0.0104 \tag{13}$$

In summary, there is evidence that the policy restricted the way spontaneous ND were anticipated from weekends to workdays through Scheduled CS. Although our results are not causal evidence, they indicate that after the policy enactment, the manipulation of potential spontaneous ND to Scheduled CS diminished before the beginning of the 39th gestational week and increased it after the beginning of the 39th week. In addition to that, it is very likely that this pattern was not randomly obtained.

7. Conclusion

This research presented evidence that Resolution 2,144 from the CFM reduced the Scheduled CS rate for births that happen before the beginning of the 39th week of gestation. In Brazil, reductions were up to 24% in the total number of Scheduled CS happening before the 39th week. We also found that the share of births happening before the 39th week decreased 6% at the mean. The policy had no statistically significant effect at the 5% level on the overall Scheduled CS rate. Therefore, it is possible to infer that Scheduled CS from before the beginning of the 39th week were postponed to after the beginning of the 39th week.

Heterogeneity results indicate that Scheduled CS before the 39th week reduced in a lower magnitude for more educated mothers. This heterogeneity was not captured on the share of births happening before the 39th week. Finally, the policy effect does not vary across different levels of pregnant woman prenatal consultations, a proxy for mothers' information.

Our results are robust to adding experimental groups' linear trends. On one hand, we do not reject the null-hypothesis of no parallel trends two years before the policy period, which is not a positive result related to our main assumption. On the other hand, not rejecting this hypothesis does not invalidate our results according to Kahn and Lang (2020). Furthermore, other intervention described in Section 2 lead us to believe that the two years period prior to the policy might be contaminated. Finally, when using a specification that allows different linear trends for experimental groups, we reject the null hypothesis of no parallel trend.

We also explore the unintended consequences Resolution 2,144. Our research provides suggestive evidences that the policy restricted the way potential spontaneous ND were anticipated from weekends to weekdays through Scheduled CS. More specifically, we provided evidence that decreases in this type of manipulation for births happening before the 39th week and increases in it for births happening after the 39th week are correlated with the policy enactment.

In conclusion, this research shows that discouraging the Scheduled CS before the 39th week and providing information to mothers as done in Resolution 2,144 have a significant impact on increasing the length of gestation at a very low cost. Nevertheless, it seems that this policy had no effect on the overall Elective CS rate. Future research can explore this exogenous variation on gestational length to assess how it affects newborns health outcomes. Furthermore, future researches exploring the restriction on the manipulation of births' type/time evidenced in this paper can also be of high value.

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