Estimated Effect of the Rede Cegonha Program on the Health of Mothers and Babies

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Abstract

Regarding prenatal care and its possible effects on the health of mothers and babies, few studies analyze the health impacts in developing countries. This work investigates the effect of the entry of the Rede Cegonha program in Brazilian municipalities on the postnatal health of women and babies. Differences in program timing were explored using the new difference-in-differences for multiple periods model. We found evidence that adherence to the program increases the percentage of deliveries where mothers had seven or more prenatal visits and the percentage of deliveries in a hospital environment. We found no evidence that these increases improve the postnatal health of mothers and children.

Keywords: Health Economy, Prenatal Care, Access to health services, Difference-in-Differences

JEL Codes: H51, I12, I18

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1 Introduction

This paper studies the direct effects of the program Rede Cegonha on maternal and infant health post-childbirth. According to Ordinance No. 1459, the program, based on the principles of humanization of care, promised to expand access and quality of prenatal care, subsidize the transport of pregnant women for prenatal care and delivery, connect the pregnant woman to a reference health institution, encourage the use of good obstetric practices recommended by the World Health Organization (WHO) during labour and birth, quality and committed assistance to children from 0 to 2 years of age. The program included a series of actions that would help municipalities adopt care principles, such as investment in health units, logistics, transport, new prenatal exams and training of health professionals, and financial incentives through goals and indicators so that the program could be effective.

Most complications and deaths that arise during pregnancy, childbirth and puerperium are preventable by the actions of health services. Therefore, prenatal care has been recommended for many years to monitor maternal and fetal health during pregnancy (Koonin et al., 1991; Kaunitz et al., 1984; McCarthy and Maine, 1992). Health articles published between 2005-2015 about prenatal care in Brazil reveal that although the coverage of prenatal care reaches almost universal levels, it is estimated that 80% to 99% of the quality of this care was still deficient. According to them, a maximum of 66.1% of prenatal care can be considered adequate, but when considering a more complete and rigorous analysis, such as the start date of follow-up, the number of consultations performed, and performance of labour procedures and examinations of routine this percentage of adequacy dropped to 4.5%.(Nunes et al., 2016) In 2010, for every 100,000 live births in Brazil, there were 68.9 maternal deaths[1], a number still above the 35.8 stipulated by ONU in the Millennium Development Goals (MDGs) until 2015 (Brasil, 2021a). In turn, the deaths of children under one year of age were 16 per thousand live births in 2010, while the Brazilian target for the

[1] In 2019, the maternal death rate was still 58 maternal deaths per 100,000 live births. The rate exceeds 30, the tax defined as a goal of the Sustainable Development Goals (SDGs) to be achieved by 2030.

Millennium Development Goals in 2015 was 15.7 deaths[2] per 1000 live births(Brasil, 2021b).

Although organizations and health workers advocate using quality prenatal care to reduce infant and maternal mortality, there needs to be more solid econometric evidence on the effectiveness of such intervention, especially in developing countries. That is justified because, in general, it is difficult to estimate the causal effects of prenatal care due to the diverse heterogeneous characteristics in the female population that becomes pregnant. Furthermore, it would be unethical to carry out randomized experiments that deny care to a part of the sample.

This work has two objectives. First, we sought to assess whether the program was efficient in improving prenatal care and delivery of mothers, observing whether there was an increase in access to health services and prenatal consultations. We then evaluated whether this affected babies' postpartum health and maternal and infant mortality. We use new methods and detailed data on the program's expansion and health. Between 2011 and 2013, the program expanded from 0 to more than 98% of Brazilian municipalities.

Specifically, this work uses data from the municipal level to analyze the impact of the Rede Cegonha program on the primary postpartum health outcomes and mortality of mothers and babies. We explored the fact that the program was implemented in a staggered way between 2011 and 2014, and we used the new differences-in-differences tool from Callaway and Sant'Anna (2021) that avoids common biases in staggered projects to estimate their effects. We found significant effects on the number of prenatal visits and the baby's birthplace. We found that entering the program increases the probability of being born in a hospital by 0.2%, reaching up to 0.5% after one year of the program. In addition, we found that the percentage of deliveries where mothers had seven or more prenatal consultations increased on average by almost 1% in a confidence interval of at least 95% after five trimesters of program implementation in the municipality. However, these results do not generate positive effects

[2]The infant mortality rate in 2019 was 13.3 deaths of children between 0 and 1 years old per 1000 live births, the rate is the same as in 2015, while the Sustainable Development Goals target by 2030 is 12 deaths per 1000 live births.

on the postpartum health of babies, nor a reduction in the mortality rates of mothers and babies.

The rest of the work follows the following structure. Section 2 reviews the economic literature on the effects of prenatal care on health. Section 3 describes the program's institutional framework, and Section 4 describes the data sets used in our analysis. Section 5 presents our empirical strategy, and Section 6 presents our main results regarding implementing the Rede Cegonha program. Section 7 presents the Conclusion.

2 Literature Review

The effects of prenatal health conditions and early life events span a broad spectrum of educational, cognitive, behavioural and demographic outcomes. For this reason, it has received much attention from the economic literature on child development (Almond et al., 2018; Currie and Almond, 2011). In this sense, many models of skills training have highlighted the importance of good results at birth and adequate prenatal care (Cunha and Heckman, 2007, 2008; Cunha et al., 2010; Almond and Currie, 2011). However, as women become pregnant with different resources, health status, and family composition preferences, randomized controlled trials that deny care to be performed would not be acceptable. Identifying causal effects is particularly challenging, and studies are frequently inconclusive.

In this sense, trying to control as many fixed effects as possible and with a base of almost 1.5 million births, Yan (2020) executes an intra-family analysis. Their model includes fixed maternal effects and finds a modest mean effect of prenatal care on mean birth weight but significant effects at the lower end of the birth weight distribution. According to the study, adequate prenatal care prevents low maternal weight gain and makes mothers stop smoking during pregnancy. Nonetheless, some economic studies have used quasi-experimental methods and experiments to estimate prenatal care's effects on babies' health. Evans and Lien (2005) explores variations in the number of prenatal visits caused by a bus strike in Pittsburg, Pennsylvania. The reduction in visits caused by the strike found that the number of prenatal consultations did not significantly interfere with birth weight. On the other hand, the study suggests that babies whose mothers were affected by the strike in early pregnancy weighed an average of 57 grams less. About Randomized Clinical Trials (RCT), Carter et al. (2016) perform a systematic review comparing the effects of group prenatal care versus traditional prenatal care. They do not find differences in preterm births, neonatal intensive care admissions, low birth weight, or breastfeeding initiation.

From this perspective, the literature review carried out by Corman et al. (2018) finds that the magnitudes of the effects of prenatal care on childbirth outcomes are pretty divergent in United Estates, even among studies that use the same prenatal care measures and, therefore, should produce similar estimates. According to the review, unlike the estimated effects of prenatal smoking, which are less variable and directly related to more significant adverse birth outcomes, the effects of prenatal care on birth weight are variable and often not significant. In this sense, the authors find that prenatal care in the first trimester (versus the absence of care in the first trimester) may not reduce birth weight or reduce it by 1% to 3.8%. In the most recent studies analyzed by the authors on delay in months in prenatal care, the effects range from no effect to 40 grams for each month of delay. The average effects of smoking were about 160 to 230 grams on birth weight. Moderate increases in birth weight were found on the effects of programs intended to improve prenatal care over standard prenatal care. In addition, the studies analyzed by the authors show that care in the first trimester may not reduce neonatal mortality or reduce but only between 1% and 2%.

The authors also find evidence of heterogeneity in the effects of prenatal care by race and ethnicity. However, these heterogeneities have not been sufficiently explored to allow generalized inferences, and the patterns among the studies performed proved inconsistent (Corman et al., 2018). On the other hand, if the effects of more prenatal visits are modest, the quality of prenatal care - including new tests and access to more valuable information on health and nutrition - can be significant. Studies found that prenatal care in early pregnancy leads to decreased postpartum maternal smoking, increased use of infant care, and possibly increased breastfeeding, which can impact child development and health (Reichman et al., 2010; Evans and Lien, 2005). Furthermore, prenatal care can identify high-risk deliveries and refer them to more qualified hospitals, reducing neonatal deaths and morbidity (Grossman, 2017).

Since the federal government intends to increase the availability of hospital services through the program, our study also contributes to the economic literature on hospital access (Fischer et al., 2022; Battaglia, 2022; Petek, 2022). Where Fischer et al. (2022) studies how the loss of obstetric units due to a centralization of the health system affected the health of mothers and babies. The authors find that closures induce fewer women to give birth in their municipality of residence, deduct the number of prenatal consultations, and increase the number of early births and programmed induction. However, contrary to the common discourse, the effects on maternal and child health outcomes, including mortality, are not significant. It contrasts the results of Avdic et al. (2020) where maternity ward closures in Sweden led to positive health effects for babies but negative impacts on mothers. According to the authors, one of the factors that contributed to the adverse effects on the health of mothers was hospital overcrowding. The authors conclude that although the women had to travel longer distances, the closures meant they received better quality care.

Battaglia (2022) examines the closure of maternity wards in rural hospitals in the United States in 1996 and 2018 and finds that increasing distance can lead to decreased use of prenatal care or increased out-of-hospital births. However, they find zero effects on child outcomes or maternal health. On the other hand, they find small increases in infant mortality that the authors justify by the low initial mortality rate. Petek (2022) from the variation in access to hospitals caused by 1300 hospital admissions and exits during the period 1982-2010 and find that the opening and closure of a hospital causes an immediate and persistent change in the amount of hospital medical care that people receive. However, both open and closure do not interfere with the mortality rate in the short term.

Our study also contributes to the literature evaluating Brazilian public policies that impact maternal and child health (Bhalotra et al., 2019; Rocha and Soares, 2010; Carrillo and Feres, 2019). Bhalotra et al. (2019) investigates the effects of universalizing access to health care in Brazil, finding reductions in maternal, fetal, neonatal, and post-neonatal mortality and an increase in hospital deliveries, cesarean sections and maternal hospitalization for complications. Rocha and Soares (2010) analyzes the direct and indirect impacts of the Brazilian Family Health Program and finds consistent effects in reducing mortality throughout the age distribution, especially at earlier ages. Moreover, Carrillo and Feres (2019) analyzes the effects of implementing the Mais Médicos Program. This program increased the use of doctors in prenatal visits but did not increase the total number of visits received by women, indicating the change of health professionals in care. In addition, the study found no evidence of improvements in child health, suggesting that nurses would be good substitutes for doctors in prenatal care. Thus, estimating the causal effects of prenatal programs on the postpartum health of babies and mothers remain much needed.

3 Institutional Background

Maternal mortality is a serious public health problem in developing countries, where 99% of maternal deaths occur (WHO, 2010). In 2010, for every 100,000 live births in Brazil, there were 68.9 maternal deaths[3], a number far above the 35.8 stipulated by ONU in the Millennium Development Goals (MDGs) until 2015 (Brasil, 2021a). In turn, the deaths of children under one year of age were 16 per thousand live births in 2010, while the Brazilian target for the Millennium Development Goals in 2015 was 15.7 deaths[4] per 1000 live births (Brasil, 2021b).

[3]In 2019 the maternal death rate was still 58 maternal deaths per 100,000 live births. Rate still well above MDGs and the 30 defined as a goal of the Sustainable Development Goals (SDGs) to be achieved by 2030.

[4]The infant mortality rate in 2019 was 13.3 deaths of children between 0 and 1 years old per 1000 live births, the rate is the same as in 2015, while the Sustainable Development Goals target by 2030 is 12 deaths per 1000 live births.

Aware of the importance of reducing maternal and child-avoidable mortality, the Brazilian federal government instituted some initiatives in this direction. Among them was the implementation of the National Pact for the Reduction of Maternal and Neonatal Mortality in 2004. Law 11,108/2005, known as the Companion Law, guarantees pregnant women the right to have a companion during labour, delivery and the puerperium when these are carried out through the Unified Health System (SUS). Moreover, Law 11,634/2007 guarantees the pregnant woman the knowledge and bond with the maternity hospital where she will be assisted. Moreover, in order to systematize this model of humanized care for pregnancy, childbirth, and the postpartum period for pregnant women and the right to safe delivery, healthy growth and development for children, in 2011, the Ministry of Health, through Ordinance No. 1459, established the Rede Cegonha (RC).

For this, the following components organize the Rede Cegonha: i) Prenatal; ii) Delivery and Birth; iii) Puerperium and Comprehensive Child Health Care and IV) Logistical System: Sanitary Transport and Regulation. Each of these components comprises a series of health care actions between them: prenatal care at the Basic Health Unit (USB) with early contact with the pregnant women and qualification of care; reception of cases of complications during pregnancy with assessment and classification of risk and vulnerability; timely access to high-risk prenatal care; carrying out prenatal exams; educational programs related to sexual and reproductive health; prevention and treatment of STD/HIV and Hepatitis; support for pregnant women in travel for consultations and childbirth; obstetric and neonatal beds; health care practices based on scientific evidence; home visit in the first week after delivery; active search for vulnerable children, among others.

The program would be gradually implemented throughout the national territory (even in municipalities that did not yet have their childbirth care services) considering epidemiological criteria, such as infant mortality, maternal mortality ratio and population density. The Union, States and Municipalities contribute to its funding. The federal government is responsible for paying the following resources: 100% (one hundred per cent) of the cost of transfers of new prenatal exams, to be paid in two instalments. The value of the first instalment will be according to the estimated number of pregnant in the municipality, and its transfer will be through the presentation of the Regional Action Plan. The second instalment is transferred after six months, calculated according to the number of pregnant women enrolled in the program and the results of the exams verified on time. After that, the transfers will be monthly and proportional to the number of pregnant women monitored through SISPRENATAL.

The Federal Government provides resources for the construction, expansion and renovation of Normal Delivery Centers and adapting the environment that performs deliveries, for purchasing equipment and materials and for expansion of neonatal and adult ICU beds. In addition, the government also committed to providing 100% of the kits that help in the prenatal process for the UBS, pregnant women and traditional midwives, and also providing travel allowances for all SUS users at the time of prenatal care and childbirth. Furthermore, the federal government would make available all funds for the cost of the Normal Birth Centers, Home for Pregnant Women, Babies and Puerperal Women and Kangaroo Bed. According to the Ordinance, these resources must be incorporated into the financial ceilings of states, municipalities and the Federal District and transferred to services as an incentive for achieving goals. In addition, the municipality would receive from the federal government 80% (eighty per cent) of funding for the expansion and qualification of beds for High-Risk Pregnant Women following the above-mentioned requirements.

The State and Municipality must complement the 20% of the funding provided for the cost of beds following the regional pact. Behind verifying compliance with the health care actions defined for the prenatal component, the municipality will receive a monthly incentive of R$ 10.00 (ten *reais*) per pregnant woman using the service. After certification by Rede Cegonha, the municipality will earn annually R$ 10.00 (ten *reais*) per pregnant woman using the service in the year, both according to SISPRENATAL.

The accession process takes place in two ways. Regional Adhesion: aimed at the municipalities of the health regions prioritized by the State Conductor Group of Rede Cegonha (GCE/RC) based on epidemiological and populational criteria, with actions being agreed for the four components of Rede Cegonha: I) Prenatal; II) Delivery and Birth; III) Puerperium and Comprehensive Child Health Care; IV) Logistics System: Sanitary Transport and Regulation. The municipality that wants to implement Regional Adhesion should seek the GCE/RC of its state to participate in the discussion of priority regions to be elected by this group. Municipalities not part of a priority region can join the program through Facilitated Adhesion: which gives access to the following program components I) Prenatal and III) Puerperium and Comprehensive Child Health Care. To join through the Facilitated Adhesion, The Ministry of Health prepared the System of Action Plan for Thematic Networks (SISPAR); in this system, the municipality must inform its municipal action plan to formalize its request for facilitated Adhesion to the Rede Cegonha.

In all of the 5,571 Brazilian municipalities, 5,488 participated in the program. Among these, 2,325 had the Regional membership covering the prenatal, childbirth and puerperium components of comprehensive child health care and logistical system, and 3,163 the Facilitated membership that includes the prenatal, postpartum and comprehensive health care components of the child. The municipalities joined the program between 2011 and 2014; as shown in Figure 1, most municipalities joined the program in 2012, and only two municipalities joined the program in 2014.

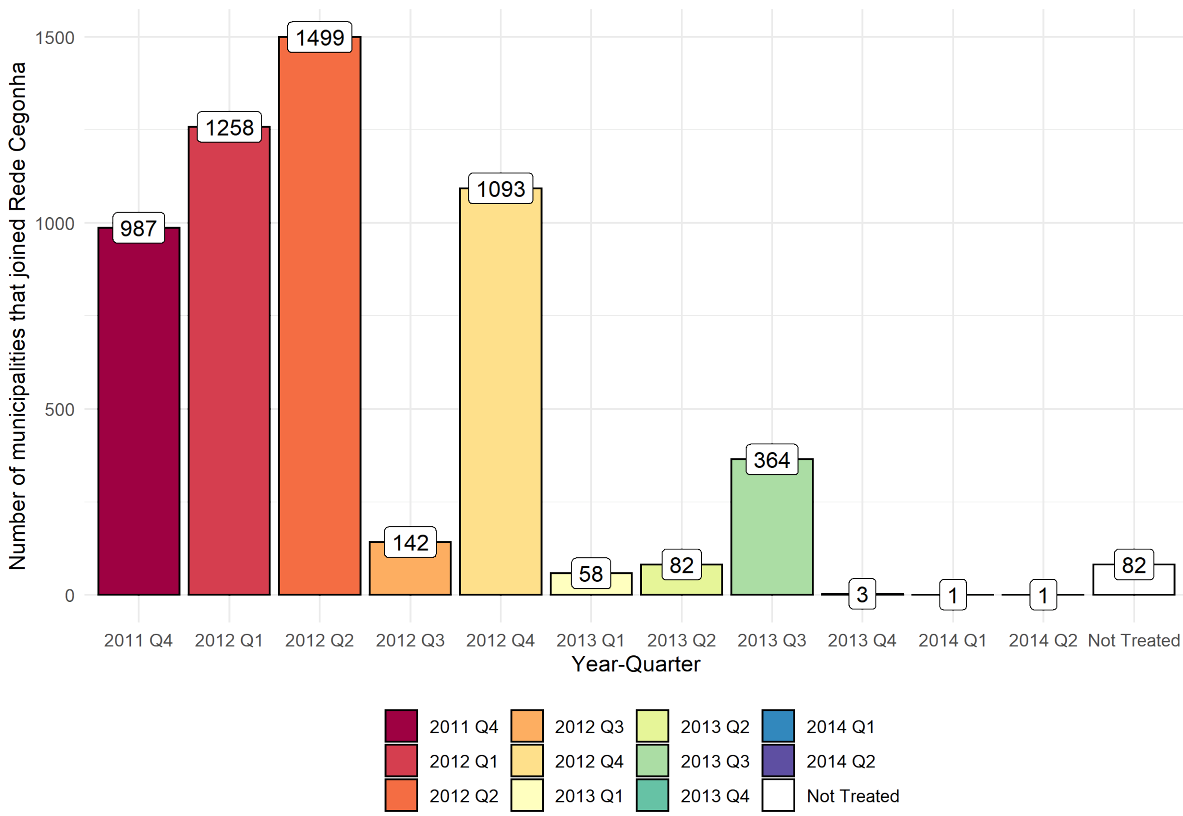
As established in the program, the implementation of Rede Cegonha should respect epidemiological criteria, such as infant mortality rate, maternal mortality ratio and population density. Table 1 presents the epidemiological and population characteristics assessed for membership in the program's regional or non-Regional modality. Most municipalities that have not joined the program have maternal and infant mortality rates below the national average and do not belong in the north and northeast regions. None of them is part of a metropolitan area or has more than 100,000 inhabitants, which is the priority criteria for joining the program.

Table 1: Characteristics of Brazilian Cities that have adopted the program

		Regional (N=2325)		Facilitated (N=3163)		Not Treated (N=82)	
		N	Pct.	N	Pct.	N	Pct.
Female Mortality Rate	above	367	15.8	408	12.9	5	6.1
	below	1958	84.2	2755	87.1	77	93.9
Maternal Mortality Rate	above	388	16.7	409	12.9	5	6.1
	below	1937	83.3	2754	87.1	77	93.9
Infant Mortality Rate	above	1100	47.3	1364	43.1	26	31.7
	below	1225	52.7	1799	56.9	56	68.3
Fetal Mortality Rate	above	1015	43.7	1325	41.9	21	25.6
	below	1310	56.3	1838	58.1	61	74.4
Metropolitan region	No	1952	84.0	2919	92.3	82	100.0
	Yes	373	16.0	244	7.7	0	0.0
More than 100,000 inhab.	No	2145	92.3	3060	96.7	82	100.0
	Yes	180	7.7	103	3.3	0	0.0
North or Northeast Region	No	1308	56.3	1943	61.4	75	91.5
	Yes	1017	43.7	1220	38.6	7	8.5

Notes: For the construction of indexes, we used data from the Brazilian Ministry of Health (MS/ Datasus), which provides data on health outcomes, population counts from the Brazilian Census and spatial data from the Institute for Applied Economic Research (IPEA). The Female Mortality Rate considers maternal mortality per 1000 women of childbearing age (10 to 49 years old). The Maternal Mortality Rate reflects maternal mortality per 1000 babies aged 0 to 1 year. Infant Mortality Rate considers deaths of babies aged 0 to 1 year concerning the population aged 0 to 1 per 1000 inhabitants. The Fetal Mortality Rate estimate the number of fetal deaths per birth (number of live births more fetal deaths)

Figure 1: Number of cities that joined the Rede Cegonha per year-quarter



Analyzing the cities that joined the program through the two membership types according to the program criteria, the group of cities where the Regional modality was adopted is composed of municipalities proportionately more present in metropolitan areas with high population densities and have infant and maternal mortality rates worse than the national average. On the other hand, most of the municipalities that join the program through Facilitated membership are part of the North and Northeast region, which was a criterion for Regional membership.

The Rede Cegonha program can improve child and maternal health indicators at all stages of the program through several mechanisms. As the program has financial incentives to reach pregnant women at the beginning of their pregnancy, implementing the program can increase the number of prenatal visits and thus improve the indicators. The training of health professionals can offer an improvement in prenatal care. The program also increases the number of exams paid by *SUS*. It proposes to speed up the delivery of these results, which can facilitate the discovery of diseases in the mother at the beginning of pregnancy that can affect the baby, such as HIV, congenital syphilis, anaemia and urinary tract infection. In addition, the program guaranteed pregnant transport for consultations and delivery since, as indicated by Evans and Lien (2005), access to adequate transportation also plays a significant role in prenatal care. Consequently, prenatal care can identify high-risk deliveries and refer them for delivery in better-prepared hospitals, reducing neonatal deaths and morbidity (Grossman, 2017).

4 Data

We used data from several sources to estimate the effects of the Rede Cegonha Program on the health of mothers and babies. Data on implementing the Rede Cegonha Program at the municipal level are not publicly disclosed, and we obtained them through the Women's Health Coordination. For this, we requested access to them through the Access to Information Law, No. 12.527/2011 and requested through the portal Fala.BR[5]. The data contains information such as the type of adherence, the name of the municipality, the ordinances of the action plans, the estimated number of pregnant women in the municipality and the date of adherence.

Health data are freely accessible and made available by the Ministry of Health of Brazil through Datasus[6], where it is possible to find data on health outcomes and access to health care. Data on births are made available by the National Information System for Live Births (Datasus/SINASC). On this system, we find all births registered in Brazil and information such as type of delivery, number of births, gestational age, baby's weight, place of birth, results on the baby's health and information about the mother, such as occupation, education and marital status. Infant and maternal mortality data are available in the Mortality

[5]See more https://falabr.cgu.gov.br/

[6]These data can be accessed through the website https://datasus.saude.gov.br/transferencia-de-arquivos/

Information System (Datasus/SIM). The SIM provides information on all deaths officially registered in Brazil, such as cause of death, date of birth, age, sex, place of birth and municipality of residence.

In addition, we use data on the municipality's population by age and sex from the Brazilian Institute of Geography and Statistics (IBGE) to complete our dashboard and aid our analysis. As Datasus data are available at the individual level, we aggregate these results and information at the municipality level, where the policy was implemented, to examine the program's effects. We use the mother's municipality of residence as a reference. The administrative data we use to build our dashboard covers 2009 to 2013. The table 7 presents some descriptive statistics of our panel, where each observation corresponds to a municipality year-quarter.

5 Empirical Strategy

We took advantage of the program's staggering implementation process between the fourth quarter of 2011 and the third quarter of 2013 to estimate its effects on the outcome variables.[7] We used the new difference-in-differences estimator Callaway and Sant'Anna (2021) to allow the program effects to be heterogeneous according to the treatment cohort and the exposure time.

The context of evaluating public interventions presents some econometric concerns. The first occurs when the adoption of public policy depends on the initial conditions of the municipality and, therefore, adherence to the program would be endogenous. However, according to data from the Women's Health Coordination, less than 1.5% of Brazilian municipalities did not adhere to the program, suggesting that eventual adherence did not suffer so much from the problem of self-selection. Another problem could occur if the specific moment of

[7] Although the program's enrollment period was between 2011 and 2014, we removed the groups treated in the fourth semester of 2013 and the year 2014. As they are groups composed of 1 to 3 municipalities, their data could be very noisy and incomparable to those treated in previous quarters. We also excluded cities that were never treated from our analysis, as Callaway and Sant'Anna (2021) suggests.

adoption of the program in a given municipality were correlated with some pre-existing condition. In this case, the fixed effects of the municipality are present in the differences and differences approach that we use to solve this problem. However, there are two more possibilities regarding the time of joining the program. One is whether municipalities subject to adverse health shocks are willing to adhere more quickly to the program. The moment of adoption of the program is related to the dynamic characteristic of the dependent variable. The other is when there is a trend towards convergence towards the mean. That is municipalities in the worst situation initially naturally reach the richest municipalities. Table 7 shows that, on average, these situations do not apply in our case. To confirm the assumption of parallel trends across treatment cohorts and to show that program implementation is not associated with a dynamic characteristic of our dependent variables, we added pre-program trends graphs of all our response variables, population and number of deliveries. Since we use Callaway and Sant'Anna (2021) *not-yet-treated* estimator which uses units as control groups for previously treated units, and it does not constrain pre-trends involving periods before the first unit is treated or restrict pre-trends to the first treatment group (Marcus and Sant'Anna, 2021)

Our graphs show the period between the first treated group and the last. That is, the estimator imposes parallel pretreatment trends between $t = g_{min-1}$ to $t = T$, where T is the latest period for each of the treated groups except the first treated group (which is treated in time g_{min}). In our case, when we analyse the program's effect on the treated group in the second quarter of 2012, the model's assumption of parallel trends requires parallel trends between the second quarter and the third quarter of 2012 ($t = 2012Q2$ and $t = 2012Q3$). Still, it does not need the first quarter and the second quarter of 2012 ($t = 2012Q1$ and $t = 2012Q2$) since the treated group is compared with what will be treated in the following period. Furthermore, the model's assumption of parallel trends does not require pre-trends from the municipalities treated in the fourth quarter of 2011 (the group treated early).

Moreover, to try to get around the problem that averages of small municipalities are very noisy, which also interferes with the standard error and the possibility that norms of health outcomes can be strongly related to the number of residents. We weighted the results related to the baby's birth by the number of deliveries and the regressions on the mortality outcomes by the population size.

5.1 Event study framework

A conventional way of estimating the dynamic effects of treatment is to use a time-unit fixed-effects event study regression. In our analysis, the equation that assesses the impact of entering the Rede Cegonha program on health outcomes at the municipal level is:

$$Y_{mt} = \sum_{e=-5}^{5} \beta_e 1(EntranceRC)_{mt} + \tau_t + \mu_m + \varepsilon_{mt} \quad (1)$$

where Y_{mt} is a measure of the health outcome of mothers or babies in municipality m in quarter t, the variable $1(EntranceRC)_{mt}$ represents the quarter of entry from county m to do the program, τ_t and μ_m are the fixed effects of time and county, respectively. Previously, researchers interpreted β_e as the causal effect of treatment on interest outcome e periods after the shock. However, the dynamics of this treatment effect can introduce bias, as conventional event studies often use previously treated units as controls for units treated in later periods. Therefore, if the results vary between groups of units receiving treatment at different times, this interpretation may be incorrect (Goodman-Bacon, 2021).

We admit heterogeneous effects in its implementation according to the cohort in which the municipality was treated and treatment time. Because we consider that the program's impact may vary according to the exposure time and the treatment cohort, for example, in our case, because the logistics in the initial implementation phases are complicated, or some of the health impacts can be felt only after some delay. Therefore, to use an adequate method to estimate the dynamic effects of the treatment, we opted for the difference-in-differences event-study technique developed by Callaway and Sant'Anna (2021).

We look at the program's effects up to 5 quarters after the quarter of joining. Specifically, our study looks at the group of municipalities that received treatment between the fourth quarter of 2011 and the third quarter of 2013 (each quarter is a group of g). The estimator denoted by $ATT_{g,t}$ allows identifying the average effects of the treatment in the group that received the treatment in the period g in the period t. Thus, the notation that formalizes the potential results of the average effects of the specific treatment of the group will be:

$$ATT(g,t) = E[Y_t(1) - Y_t(0)|G_g = 1] \tag{2}$$

where G_g is a dummy variable equal to one when the unit has been on the treatment group g, $Y_t(1)$ is a response variable at time t for units that have already received treatment and $Y_t(0)$ is the potential outcome for those units if they had not been treated. As we cannot observe the untreated result for already treated units, $Y_t(0)$ is not observed for periods after g. To solve this problem, since our program has been adopted on a large scale, with a tiny group of untreated units, we use a control group of units not-yet-treated ($D_t = 0$). Thus, we will have a proxy of what would have happened if that unit, already treated, had not been treated. As per the method, assuming that treated and not-yet-treated groups follow parallel trends, we can express the treatment effect on 2 as:

$$ATT(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|D_t = 0] \tag{3}$$

the first term is the evolution of the outcome for the group that has already been treated in g. The second term is the outcome equivalent evolution for the group that has not yet been treated, our control group. As we can see in the equation 3 only compares treated groups with those that have not been treated yet, not allowing units treated early to be used as a control of units treated later.

First, we follow the dynamic aggregation approach of Callaway and Sant'Anna, indicated for the study of events. With the $ATT(g,t)$ of Equation 3 calculated for each treatment group

g and period t, we aggregate these estimates to make them better interpretable. We expect the causal effect of program entry to be heterogeneous according to the exposure time. So since each event-time e, where $e = t - g$, we find the referring $ATT(g, t)$ for each g that corresponds to that relative period. For example, if we want to find the effect after one-quarter of treatment ($e = 1$) for the municipalities that joined the program in the fourth quarter of 2011, we would be looking at the impact in the first quarter of 2012. For the municipalities that joined in the first quarter of 2012, we will examine the effects in the second quarter of 2012. We then averaged these $ATTs$ across groups, weighting by group size. This procedure results in a single average estimate of the treatment effect for each comparable period e, e will be negative in the periods before the treatment, positive in the later ones and zero in the quarter of adhesion to the program. We plot these averages, equivalent to the relative time coefficients generated from the standard regression in Equation 1. When our goal is to create a single overall point estimate, taking a weighted average of all group time ATEs, weights proportional to group size for all $t \geq g$. To present results more robustly, we balanced the groups in the periods before and after the treatment. Specifically our e varies between -5 and 5.[8]

We use the bootstrapping procedure recommended by Callaway and Sant'Anna (2021) for inference. We report the intervals with 90% and 95% confidence, and the errors are clusterized by the municipality. The Callaway and Sant'Anna procedure also allows for the incorporation of pretreatment covariates to create propensity score-based matches between treatment and control units. This adjustment is necessary when the parallel trend assumption is believed to be conditional on pre-treatment covariates. We observed unconditional pre-trends for the results in our analysis; as our control group is composed of the groups not yet treated, we chose not to use them. Finally, for comparative purposes, in the Appendix, we estimate the effects by Two-Way and Sun and Abraham (2021).[9]

[8]For a complete discussion of this method, see Sections 2 and 3 of Callaway and Sant'Anna (2021). We calculated all treatment effects using Callaway and Sant'Anna's R Package, DiD, version 2.1.1. For more information about this package, see https://bcallaway11.github.io/did/

[9]For these estimates, we use Laurent Berge's R Package, Fixest, version 0.10.4. See https://cran.r-

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6 Results

We begin by presenting event studies of our preliminary outcomes related to mother and child health. We defined the treatment groups g according to the quarter they joined the program and defined our baseline the quarter before treatment, $t-1$. To account for possible 0's, we record the results as $log(Y+1)$, but these results are robust when measured in their original units. Figure 2 shows the mean effects of municipal-level treatment on treated ($ATTs$) for three outcome variables related to hospital access. Panel A shows that the municipality's accession to the program increases the hospital birth rate by an average of 0.2%, with a significance of less than 1%, reaching almost 0.4% after one year. We also present the effects of entry into the program on outcomes related to birth in another health facility (different from a hospital) and birth at home. As we can see in panels B and C, both are associated with a reduction in rates with a significance of less than 1%, demonstrating our results' robustness.

Regarding the increase in the number of prenatal visits, Figure 3 shows that the entry into the program is associated with an average increase of 0.4% in the number of deliveries in which mothers had seven or more prenatal visits, reaching almost 1% after five trimesters of the program in the municipality. These results are significant at less than 95% confidence (panel D). Panel A indicates that the program reduces by almost 1% the number of mothers who had children without prenatal care, but these values are not significant, at least at 10%. Regarding the percentage of delivery performed where mothers realized 1 to 3 prenatal visits or 4 to 6 prenatal visits, we could not reject the null hypothesis that the program has no positive or negative effect.

Looking at the program's impact up to 5 quarters after its implementation in the municipality, Figure 4. In line with the findings of Corman et al. (2018), where the increase

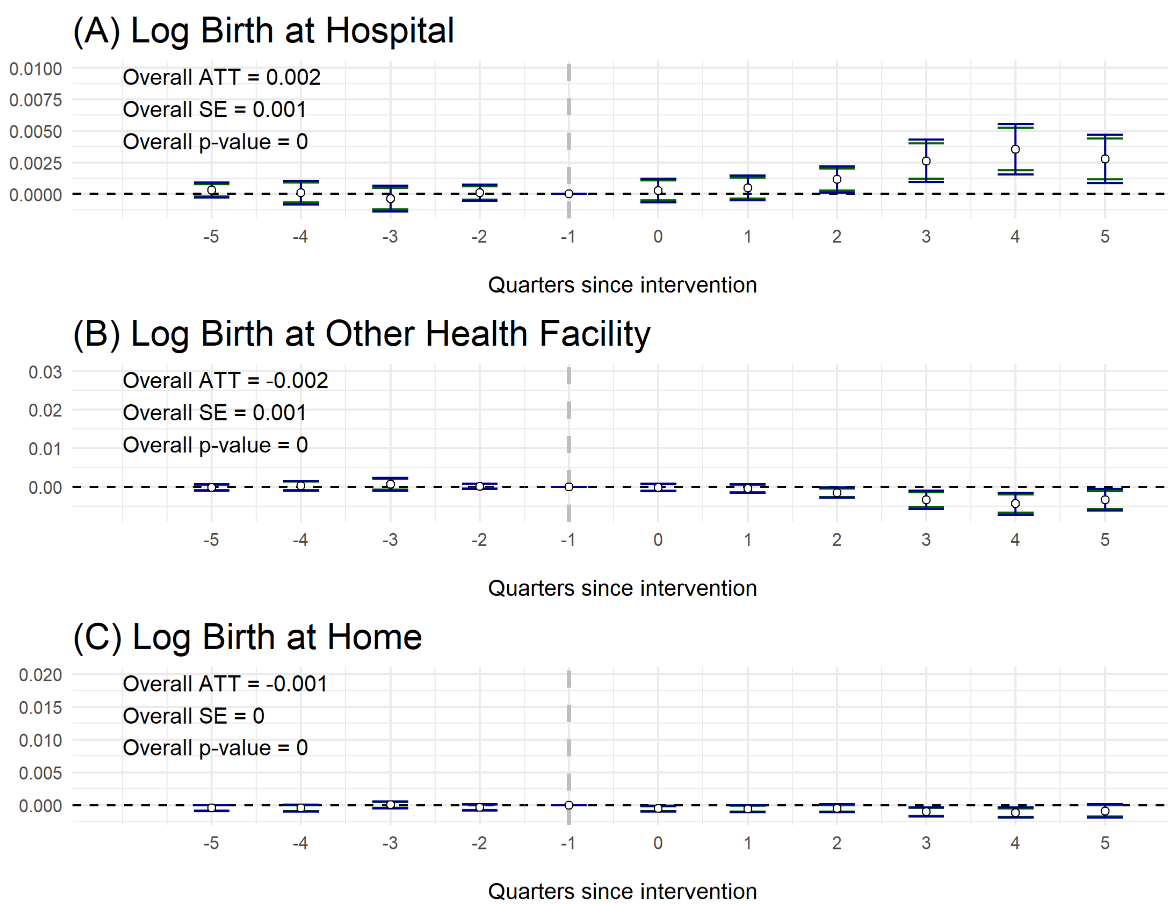
project.org/web/packages/fixest/ for more information about this package.

in prenatal consultations has modest or even non-existent effects, the child's health outcomes,such as birth weight, do not accompany the increase in the birth rate where mothers had seven or more prenatal visits. In general, we could not reject the null hypothesis, where the program's effect is zero for any of our outcomes related to the baby's health at birth.

Finally, Figure 5 presents the results of the program's entry on maternal and infant mortality. On average, the results are associated with increased mortality after the first five quarters of program implementation. Nevertheless, these results are not significant, with at least 10% of significance. Panel A shows an increase of almost 10% in fetal deaths in the quarter before joining the program after five quarters, but this increase is not significant within a confidence interval of at least 90%. After one year of program implementation, Panel B showed an average increase of 6% in infant mortality in the municipality, with almost 5% of significance and an average increase of 1.6% in deaths of children up to 1 year of age during the five quarters of the program. However, it is not possible to reject the null hypothesis where there were no effects of adherence to the program in the five first quarters of treatment. When we look at maternal mortality rates, panels C and D, we find that program implementation is associated with an increase in maternal death tax after five quarters. However, these results are not significant within a range of at least 95% confidence.

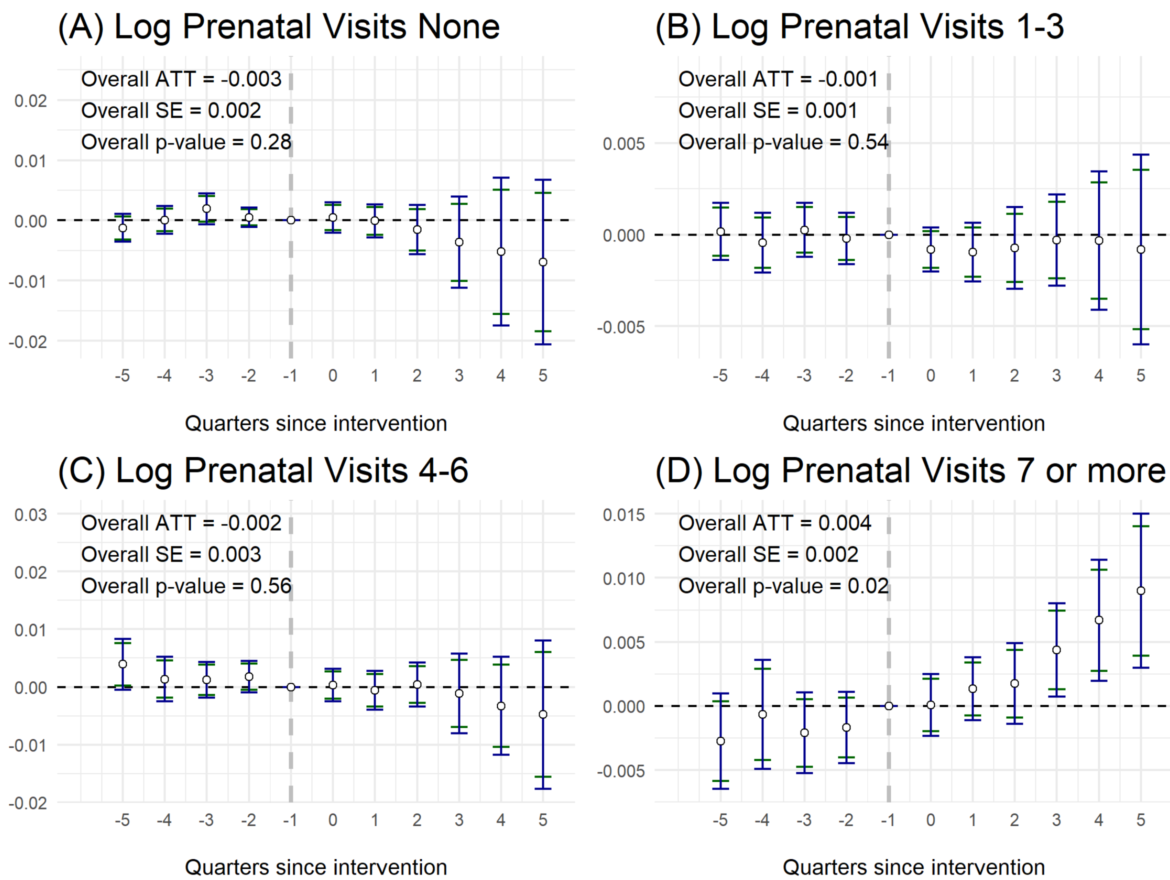
In summary, our event studies show that the program did not present significant and robust results concerning the primary evaluation measures of improvements in the health of mothers and children in the first five quarters of program implementation. In the appendix sections, we investigate the same results through the Two-Way Fixed Effects model and the Sun and Abraham (2021) model.

Figure 2: **Effect of the program on hospital access**



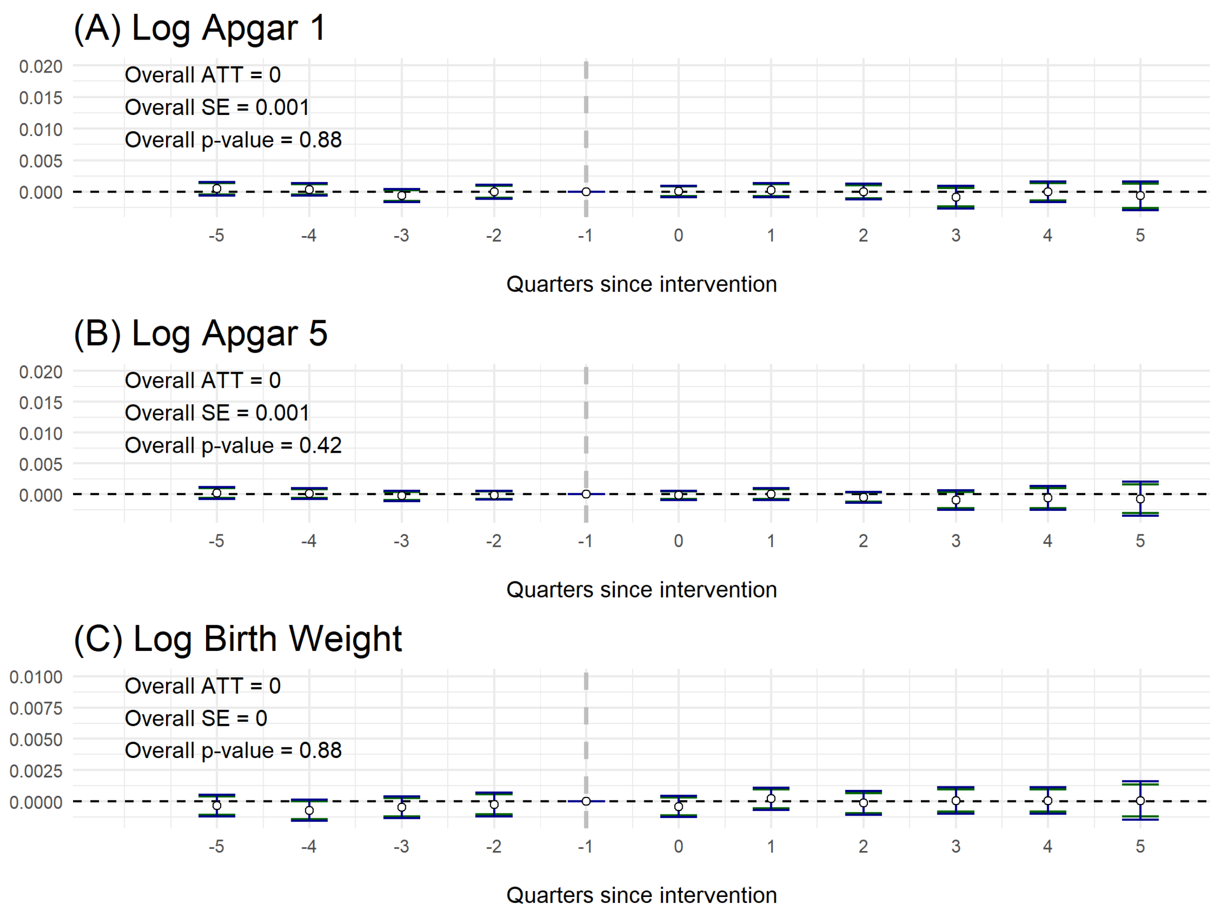
Event study estimates were generated through the method Callaway and Sant'Anna (2020) described in Section 5 on outcomes related to hospital access and hospital procedures. The estimates reference is the year before to the program's implementation, -1 on the x-axis. The response variables are (A) Cesarean sections, (B) Birth in the hospital, and (C) Birth in other health facility. All figures show simultaneous 95 and 90 per cent confidence intervals. Standard errors are grouped at the county level.

Figure 3: **Effect of the program on Prenatal Care**



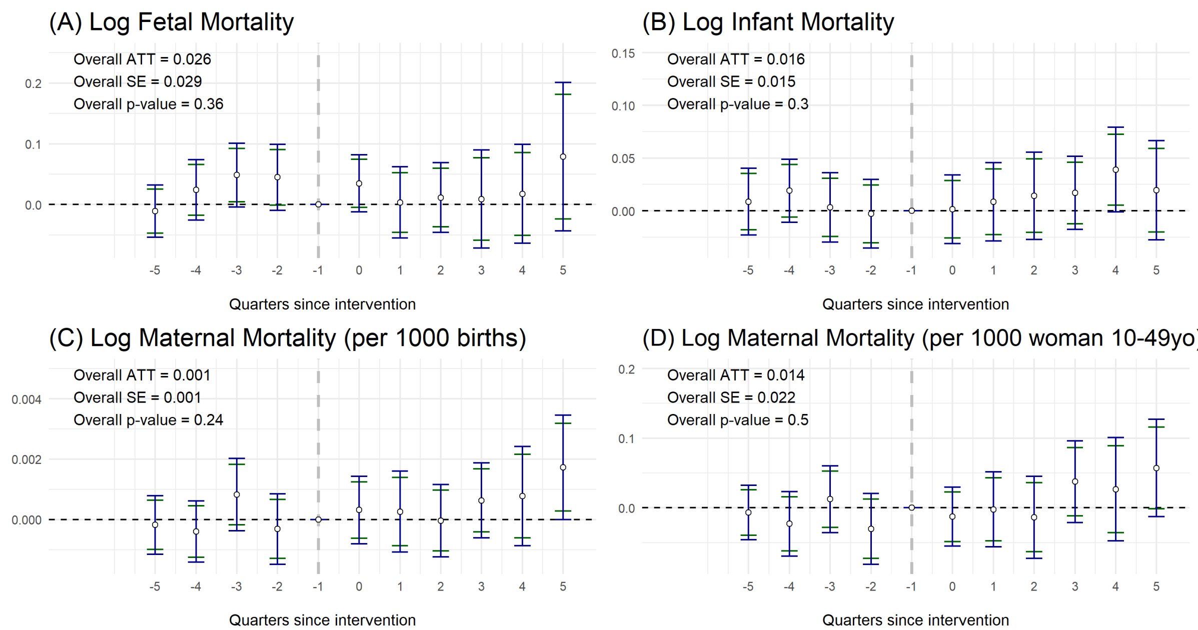
Event study estimates were generated through the method Callaway and Sant'Anna (2020) described in Section 5 on outcomes related to prenatal care. The estimates reference is the year before to the program's implementation, -1 on the x-axis. The response variables are (A) Prenatal Visits None, (B) Prenatal Visits 1 to 3, (C) Prenatal Visits 4 to 6, and (D) Prenatal Visits 7 or more. All figures show simultaneous 95 and 90 per cent confidence intervals. Standard errors are grouped at the county level.

Figure 4: **Effect of the program on Birth Outcomes**



Event study estimates were generated through the method Callaway and Sant'Anna (2020) described in Section 5 on outcomes related to birth. The estimates reference is the year before the program's implementation, -1 on the x-axis. The response variables are (A) Apgar scores at 1 minute, (B) Apgar scores at 5 minutes, (C)Birth weight, and (D) Premature Birth. All figures show simultaneous 95 and 90 per cent confidence intervals. Standard errors are grouped at the county level.

Figure 5: **Effect of the program on Mortality**



Event study estimates were generated through the method Callaway and Sant'Anna (2020) described in Section 5 on outcomes related to mortality of mothers and babies. The estimates reference is the year before to the program's implementation, -1 on the x-axis. The response variables are (A) Fetal mortality, (B) Infant mortality, (C) Maternal mortality per 1000 births, and (D) Maternal mortality per female population of childbearing age. All figures show simultaneous 95 and 90 per cent confidence intervals. Standard errors are grouped at the county level.

7 Conclusion

In this article, we study the effects of prenatal care on maternal and child health. The economic literature that used causal methods to analyze the effect of prenatal care indicates effects of varying sizes, which can be large and positive, small and positive, or null (without significance). The positive effects would be more associated with the quality of prenatal care and care at the beginning of pregnancy than the number of consultations. We addressed this issue in the context of the Rede Cegonha Program, using an event study methodology to verify that the program delivers what it promised: improving access and prenatal care. We found that the program increased the number of deliveries where mothers had seven or more prenatal consultations and the number of deliveries in hospitals. However, this increase does not reflect the health of mothers and babies.

Some health studies point out that although access to prenatal care in Brazil is universal, it is often still not adequate (Nunes et al., 2016). About the program, Nascimento et al. (2018) reviews articles published between 2013 and 2016 and finds that although the program recommended that the beginning of prenatal care be early, quality care, examinations with access to results on time and linking the pregnant woman to the place of delivery, when put into practice this does not occur. According to the authors, even with the program and financial incentives, the recruitment of pregnant women continued to be late, there was a delay in scheduling appointments, laboratory tests were not delivered promptly, and the performance of rapid tests was well below what was recommended by Rede Cegonha. In addition, the link between pregnant women and the place of delivery was non-existent, leading them to wander between overcrowded and precarious maternity hospitals, many of which do not have enough obstetric beds for pregnant women (Nascimento et al., 2018). All these factors may have compromised the quality of prenatal care provided to mothers.

The Rede Cegonha program was replaced by the Maternal and Child Care Network (Rami) in April 2022 through Ordinance No. 715. The resources passed to the Prenatal Component, between 2011 and 2022 totaled the amount of $R\$277,429,509.17$, being

$R\$113,626,461.45$ in the period analyzed in this work.

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Tables

Table 2: Summary statistics: municipality -by- quarter-year panel

	N	Mean	SD	Min	Max
Home Birth	72345	0.010	0.041	0.000	0.753
Birth at Hospital	72345	0.976	0.083	0.000	1.000
Birth at Other Health Facility	72345	0.011	0.062	0.000	1.000
Prenatal Visits None	72345	0.022	0.049	0.000	1.000
Prenatal Visits 1-3	72345	0.063	0.077	0.000	1.000
Prenatal Visits 4-6	72345	0.279	0.168	0.000	1.000
Prenatal Visits 7 or more	72345	0.634	0.221	0.000	1.000
Apgar 1	72207	8.253	0.485	0.000	10.000
Apgar 5	72206	9.357	0.385	0.000	10.000
Birth Weight	72277	3.196	0.145	0.500	4.600
Maternal Mortality Rate (per 1000 live births)	72345	0.598	4.978	0.000	250.000
Maternal Mortality Rate (per 1000 Women 10-49yo)	72345	0.007	0.050	0.000	2.092
Fetal Mortality Rate (per 1000 births)	72345	11.343	24.003	0.000	1000.000
Infant Mortality Rate (before 1 year old, per 1000 babies 0-1 year old)	72345	3.406	6.173	0.000	181.818
Population	72345	34 277.772	203 095.776	805.000	11 253 503.000
Number of Deliveries	72345	130.141	776.428	0.000	46 486.000

Notes: This table shows the summary statistics of our municipality-quarter panel described in Section 4. All our summary statistics are at the municipality-by-quarter level, and our time series includes the period from July 2010 to July 2013. Data come from the DATASUS and IBGE.

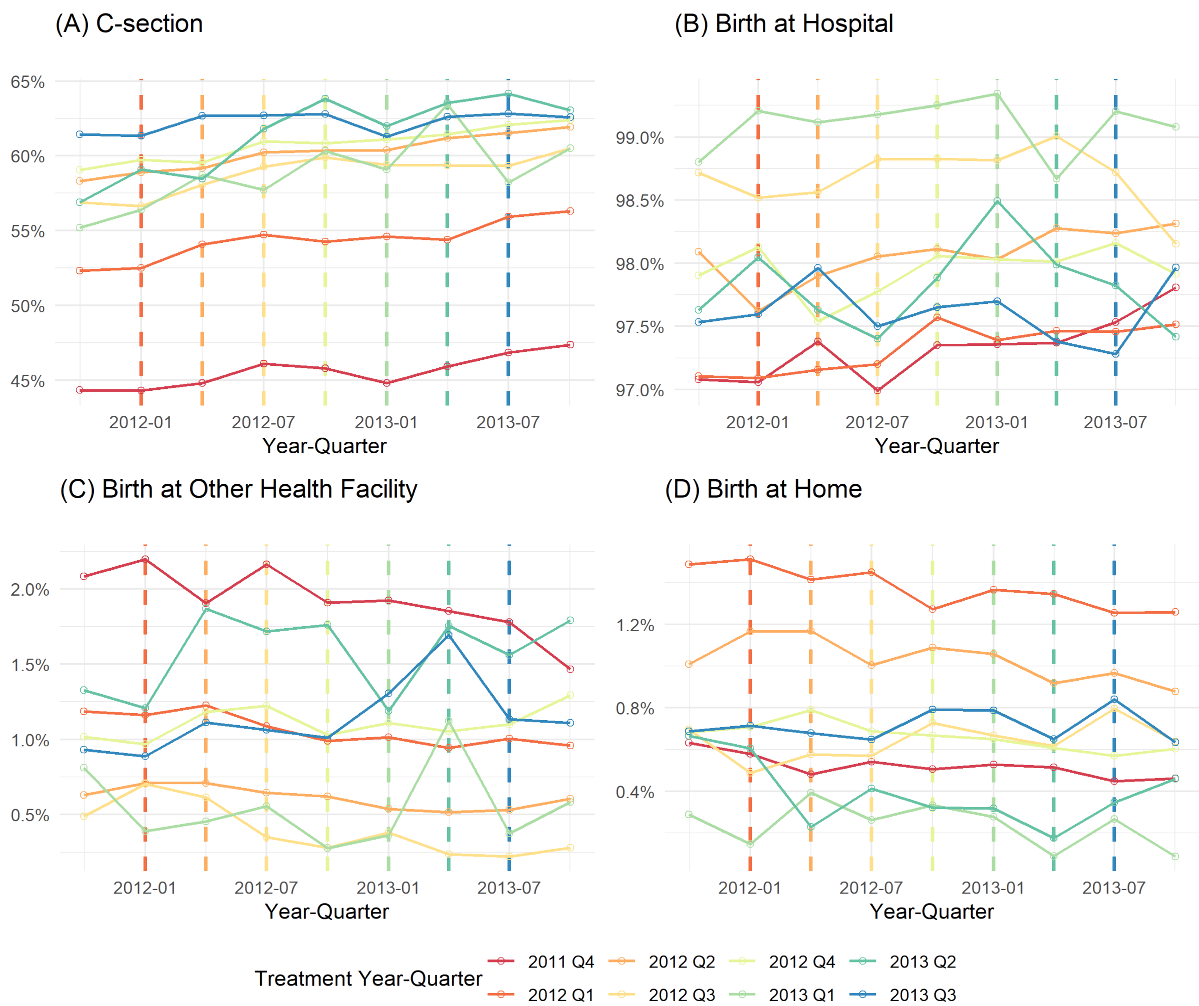
Table 3: Descriptive Statistics: Municipalities that joined the program by year-quarter of membership

	2011 Q4 (N=987)	2012 Q1 (N=1258)	2012 Q2 (N=1499)	2012 Q3 (N=142)	2012 Q4 (N=1093)	2013 Q1 (N=58)	2013 Q2 (N=82)	2013 Q3 (N=364)
Birth at Hospital	0.971	0.969	0.979	0.972	0.980	0.990	0.984	0.978
	(0.081)	(0.088)	(0.076)	(0.099)	(0.075)	(0.026)	(0.047)	(0.082)
Birth at Other Health Facility	0.021	0.013	0.008	0.010	0.011	0.005	0.011	0.012
	(0.078)	(0.063)	(0.052)	(0.046)	(0.061)	(0.015)	(0.041)	(0.055)
Home Birth	0.006	0.016	0.012	0.008	0.008	0.004	0.003	0.005
	(0.018)	(0.052)	(0.047)	(0.029)	(0.038)	(0.014)	(0.012)	(0.024)
Prenatal Visits None	0.031	0.024	0.020	0.031	0.017	0.022	0.014	0.015
	(0.054)	(0.042)	(0.044)	(0.062)	(0.035)	(0.052)	(0.029)	(0.036)
Prenatal Visits 1-3	0.071	0.080	0.060	0.071	0.057	0.058	0.046	0.052
	(0.063)	(0.098)	(0.084)	(0.076)	(0.077)	(0.062)	(0.057)	(0.083)
Prenatal Visits 4-6	0.318	0.295	0.261	0.309	0.272	0.231	0.297	0.264
	(0.153)	(0.159)	(0.177)	(0.179)	(0.173)	(0.113)	(0.182)	(0.166)
Prenatal Visits 7 or more	0.580	0.602	0.659	0.582	0.654	0.688	0.643	0.666
	(0.205)	(0.227)	(0.239)	(0.239)	(0.221)	(0.125)	(0.205)	(0.205)
Apgar 1	8.139	8.265	8.249	8.328	8.295	8.202	8.301	8.296
	(0.464)	(0.439)	(0.476)	(0.383)	(0.523)	(0.415)	(0.456)	(0.511)
Apgar 5	9.262	9.358	9.363	9.435	9.401	9.312	9.335	9.410
	(0.363)	(0.371)	(0.400)	(0.312)	(0.372)	(0.330)	(0.329)	(0.380)
Birth Weight	3.195	3.207	3.192	3.195	3.188	3.216	3.196	3.192
	(0.113)	(0.124)	(0.141)	(0.122)	(0.145)	(0.125)	(0.143)	(0.181)
Maternal Mortality Rate	0.590	0.721	0.494	0.362	0.385	1.041	0.273	0.919
(per 1000 live births)	(4.613)	(4.250)	(5.260)	(2.186)	(2.778)	(6.249)	(1.959)	(7.822)
Maternal Mortality Rate	0.007	0.010	0.005	0.006	0.005	0.013	0.004	0.011
(per 1000 Women 10-49yo)	(0.056)	(0.061)	(0.051)	(0.036)	(0.035)	(0.076)	(0.025)	(0.085)
Fetal Mortality Rate	11.221	11.565	10.522	13.370	10.696	11.474	8.502	7.870
(per 1000 births)	(16.117)	(21.291)	(24.137)	(27.331)	(25.988)	(20.396)	(20.220)	(18.025)
Infant Mortality Rate (before 1 year old,	4.032	3.757	3.575	3.535	3.340	3.159	4.057	3.633
per 1000 babies 0-1 year old)	(5.939)	(5.830)	(6.213)	(6.078)	(6.559)	(5.235)	(6.652)	(7.982)

Notes: The table presents descriptive statistics of the main results in the second quarter of 2011, the program announcement quarter when the program was still not implemented in any municipality. The columns identify the treatment cohorts year-quarter in which the municipalities joined the program. The first line for each result corresponds to the mean, and in the second line are reported standard deviations in parentheses.

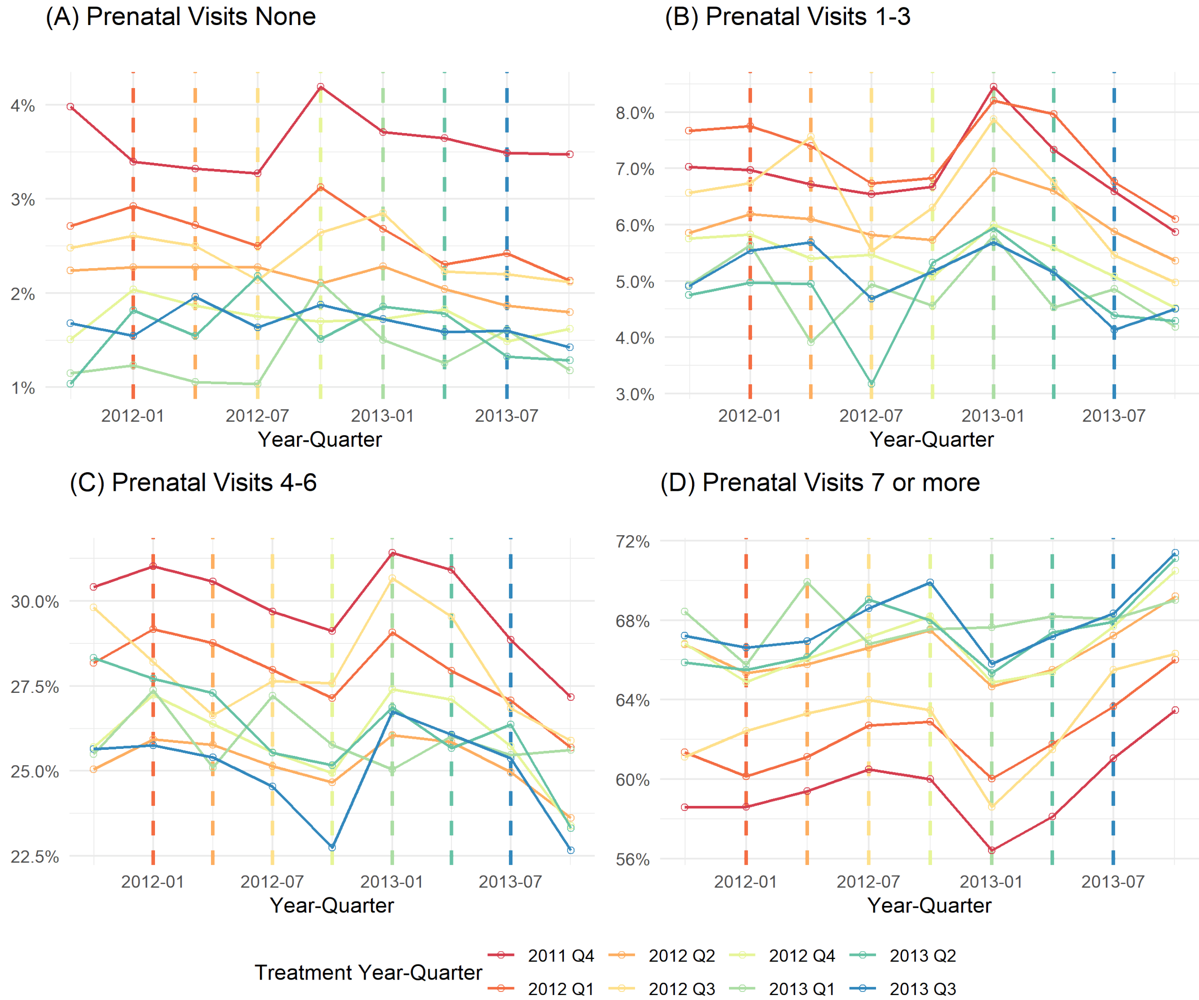
Figures

Figure 6: **Trends in Hospital Access and Surgical Procedures**



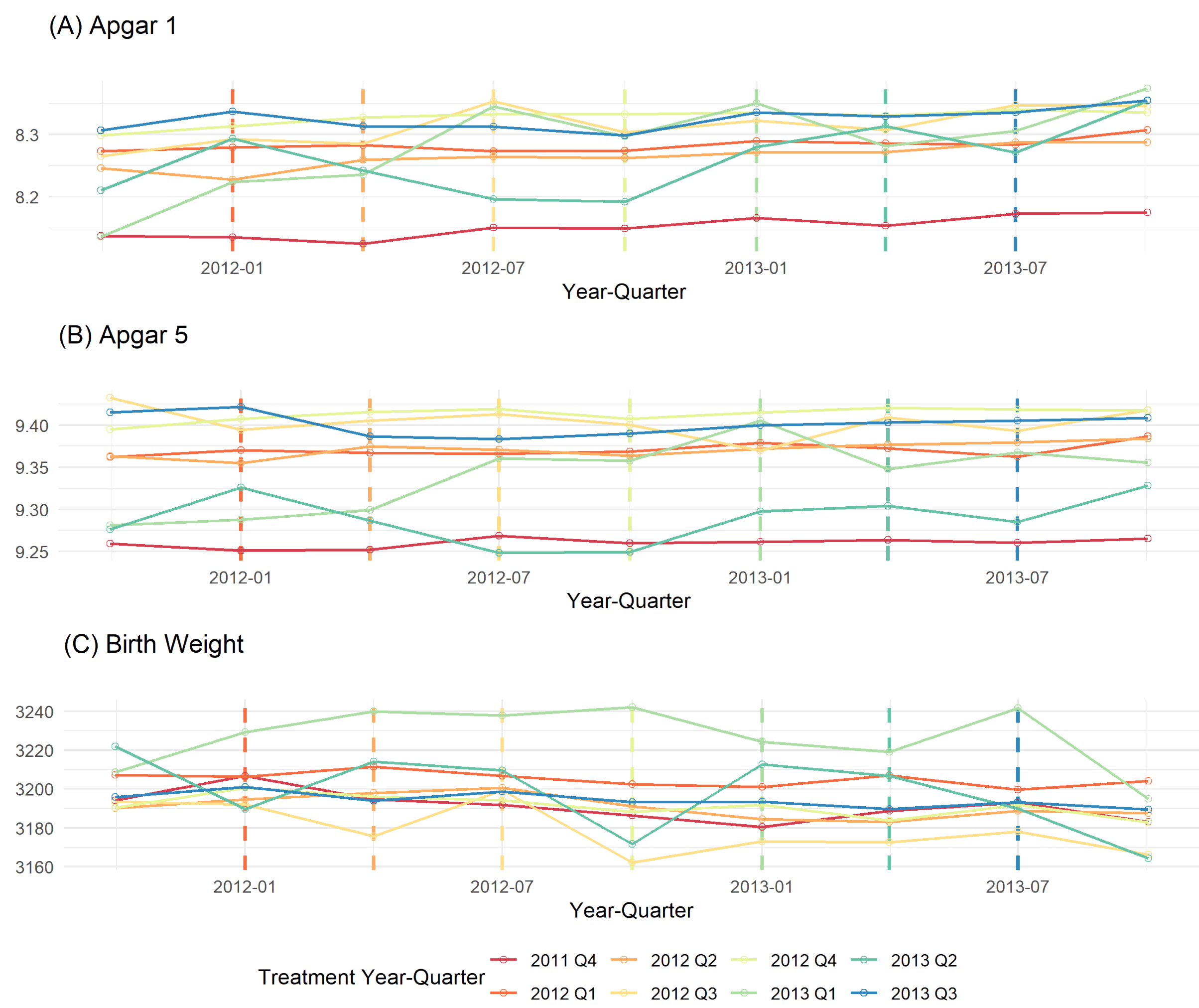
Source: Brazilian Ministry of Health, SINASC/DATASUS.
Note: Historical series of mean rates of a) Cesarean delivery; b) Hospital birth; c) Birth in a health facility (except hospital; d) Home birth, grouped by the quarter in which the municipality received treatment — data from October 2011 to October 2013.

Figure 7: **Trends in Prenatal Care**



Source: Brazilian Ministry of Health, SINASC/DATASUS.
Note: Historical series of mean rates of a) No prenatal visit; b) One to three prenatal visits; c) Four to six prenatal visits; d) Seven or more prenatal visits, grouped by the quarter in which the municipality received treatment — data from October 2011 to October 2013

Figure 8: **Trends in Births Outcomes**



Source: Brazilian Ministry of Health, SINASC/DATASUS.
Note: Historical series of mean rates of a) Apgar 1; b) Apgar 5; c) Birth Weight, grouped by the quarter in which the municipality received treatment — data from October 2011 to October 2013.

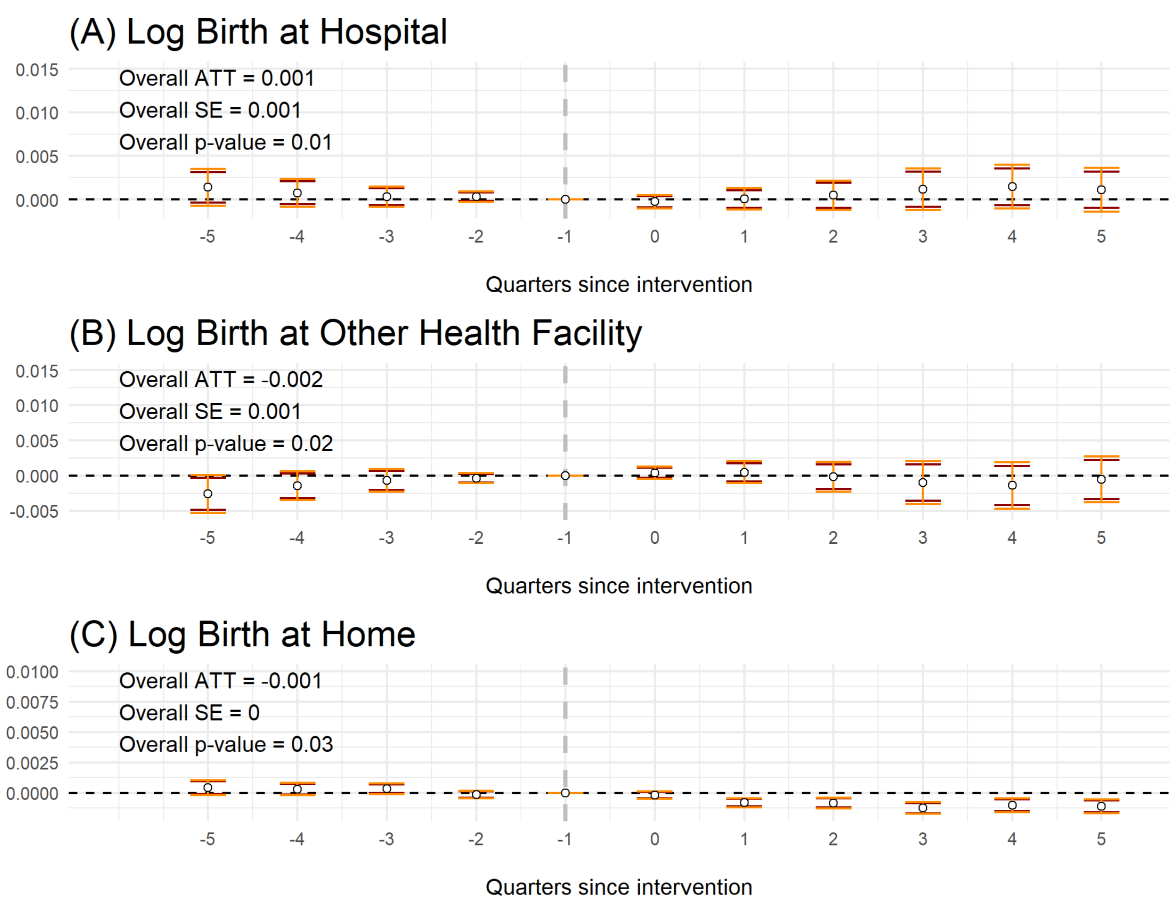
Figure 9: **Trends in Mortality**

Source: Brazilian Ministry of Health and Census, SINASC/DATASUS/IBGE.
Note: Historical series of mean rates of a) Fetal Mortality; b) Infant mortality children between 0 and 1 years old; c) Maternal Mortality per 1000 births; d) Maternal mortality per 1000 women of childbearing age, grouped by the quarter in which the municipality received treatment — data from October 2011 to October 2013.

Appendix A. Estimated Results by TWFE

In this section and the next, we exercise how event studies behave when estimating the program's effects through other tools that seek or have been used in the economic literature to estimate the effects with the difference-in-differences method. We started this exercise through Two-Way Fixed Effects, a technique commonly used before but has fallen into disuse due to some problematic findings.

Figure 10: **Effect of the program on hospital access**



Event study estimates were generated through Two-Way Fixed Effects, with controls for municipality fixed effects and quarter fixed effects, outcomes related to hospital access and hospital procedures. The response variables are (A) Cesarean sections, (B) Birth in the hospital, and (C) Birth in another health facility. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 on the x-axis—the number of deliveries weights regressions. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

We can see in Figure 10 on effects at the birthplace of the baby that the effects by Two Way Fixed Effect are similar to those estimated through the estimator of Callaway and

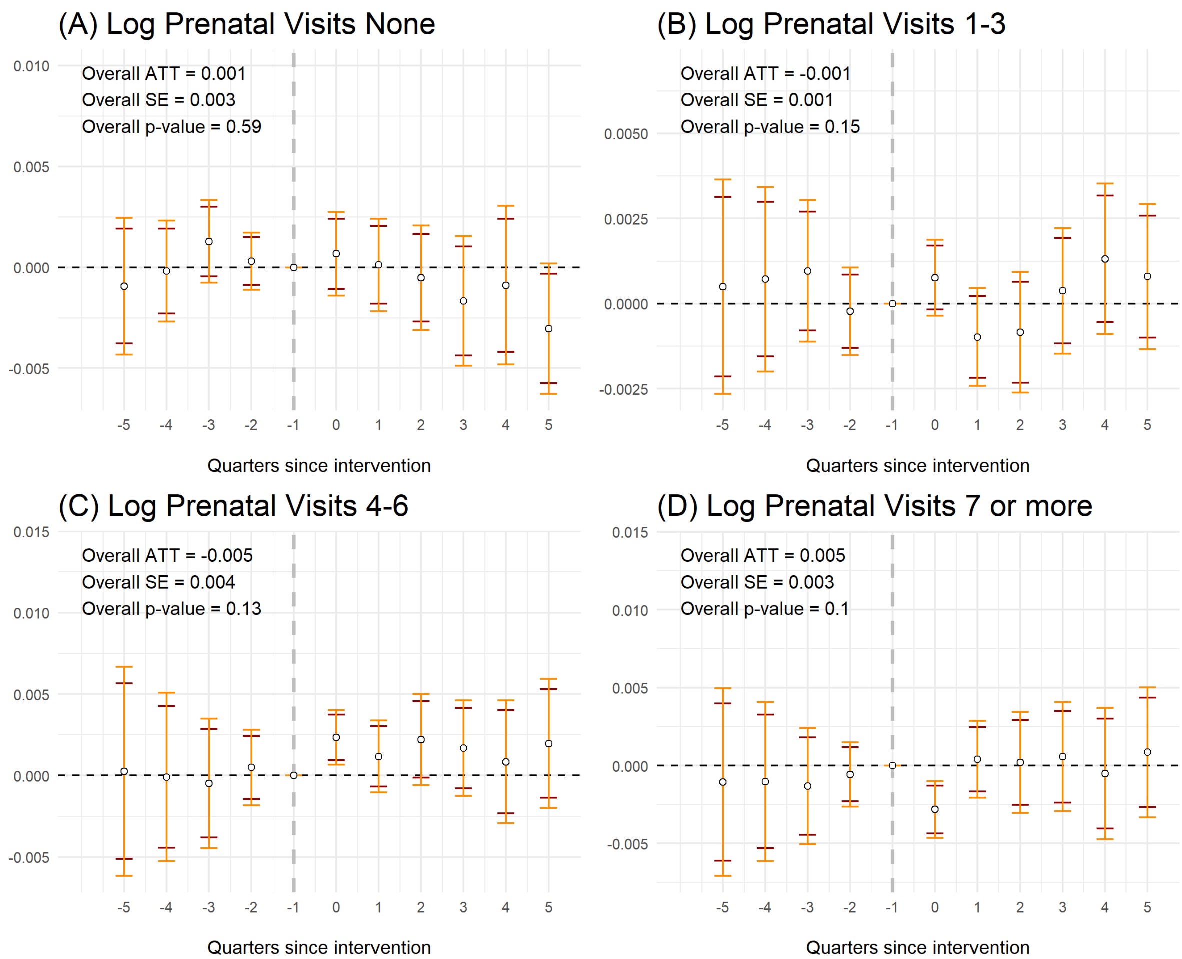
Sant'Anna (2021). Here, the program is also associated with an increase in the rate of babies born in hospitals and a rate decrease in the of babies born at home or in a health facility other than the hospital. In Panel A, we can see that, on average, in the five quarters of program implementation, the presence of the Rede Cegonha program is associated with an increase of 0.1% in the hospital birth rate, with a significance of 1%. However, if we look at the confidence intervals for each quarter, all intervals include zero. Thus, exists a possibility of a null effect of the program on the baby birth rate in hospitals. Panels B and C show that, as in Callaway and Sant'Anna (2021), negative effects on average on the rate of births at home or in a health care location other different a hospital during the first five trimesters after program implementation. However, only the effects on home births do not include zero within the 95% and 90% confidence intervals.

As with the Callaway and Sant'Anna (2021) method, our overall ATT estimate is only significant, with at least 10% significance for the percentage of deliveries performed where mothers had seven or more antenatal visits, this effect being positive. However, unlike Callaway and Sant'Anna (2021), only the estimate of the first quarter after program entry does not include zero in the 90% and 95% confidence intervals and has a negative effect on the program. Therefore, we do not consider the effects robust.

When we looked at the effects on our response variables related to the birth of the baby, Figure 12, as in Callaway and Sant'Anna (2021), we do not find during the first five quarters effects different zero in average any of our response variables. Panel C shows a decrease in average birth weight from the second quarter onwards; however, our overall ATT estimates are equal to 0. This apparent reduction may be caused by the problem of the negative weights demonstrated by Goodman-Bacon (2021) since this occurs when there are more units treated than untreated. Therefore, we do not consider this result robust.

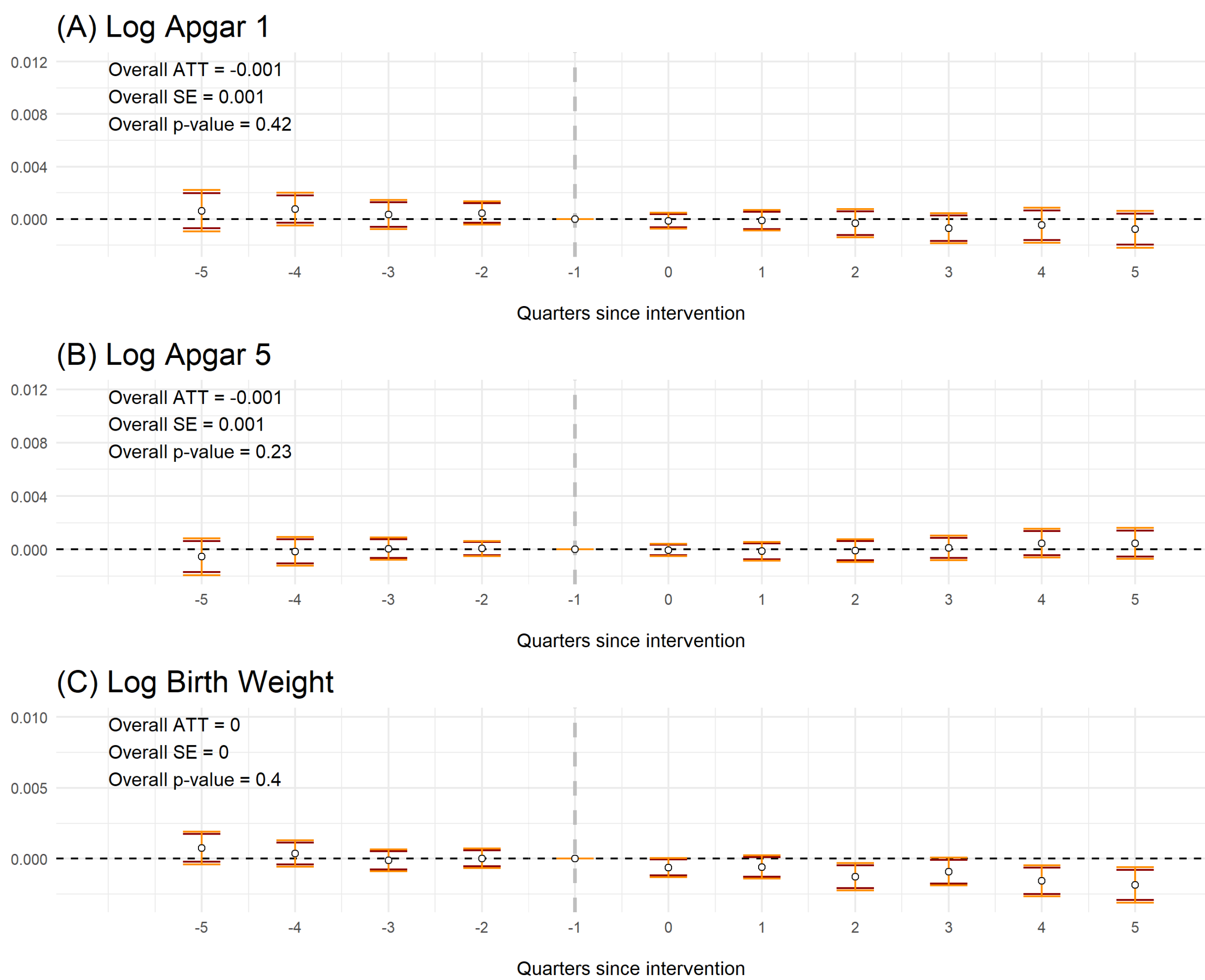
As per Callaway and Sant'Anna (2021), our maternal and infant mortality outcomes do not show aggregate effects significant over five quarters in an interval of at least 90% confidence. Our response variables in some quarters presented do not include 0 in the confidence intervals of 90% or 95%. However, all the effects shown in these quarters have the opposite sign of our aggregate effects throughout the period. Therefore, we believe that this difference in result is due to what was demonstrated by Goodman-Bacon (2021); Sun and Abraham (2021).

Figure 11: **Effect of the program on Prenatal Care**



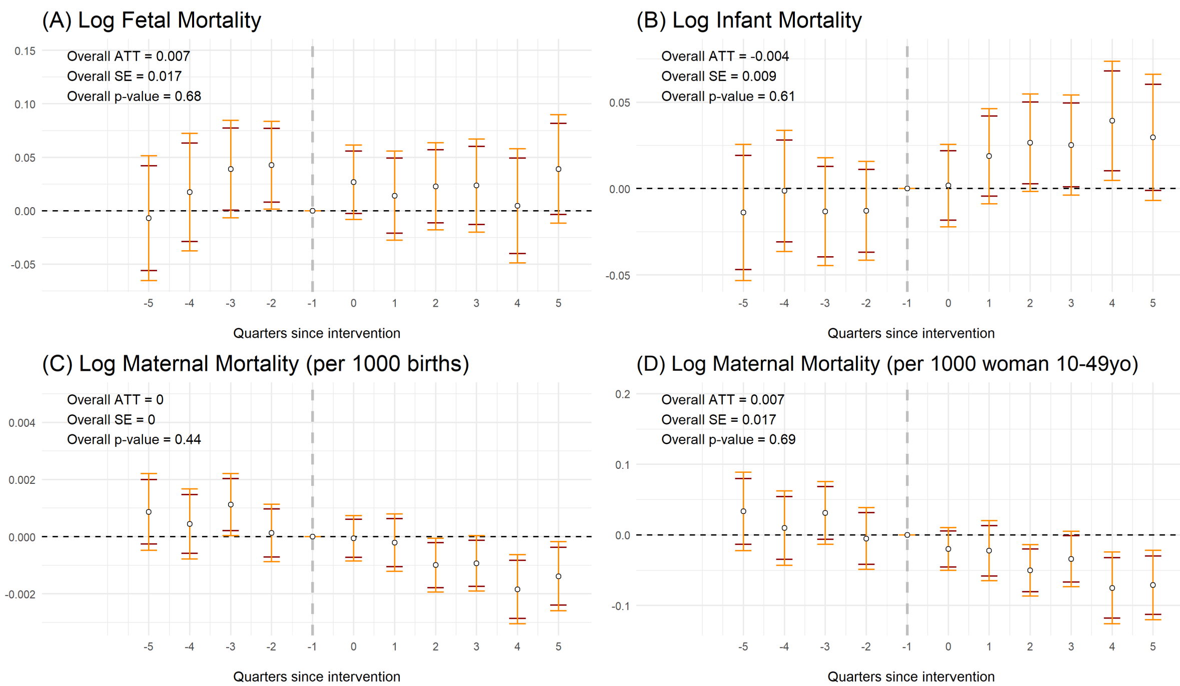
Event study estimates were generated through Two-Way Fixed Effects, with controls for municipality fixed effects and quarter fixed effects, outcomes related to prenatal care. (A) Prenatal Visits None, (B) Prenatal Visits 1 to 3, (C) Prenatal Visits 4 to 6, and (D) Prenatal Visits 7 or more. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 on the x-axis—the number of deliveries weights regressions. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

Figure 12: **Effect of the program on Birth Outcomes**



Event study estimates were generated through Two-Way Fixed Effects, with controls for municipality fixed effects and quarter fixed effects, outcomes related to birth. (A) Apgar scores at 1 minute, (B) Apgar scores at 5 minutes, (C) Birth weight, and (D) Premature Birth. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 on the x-axis — number of deliveries weights regressions. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals

Figure 13: **Effect of the program on Mortality**

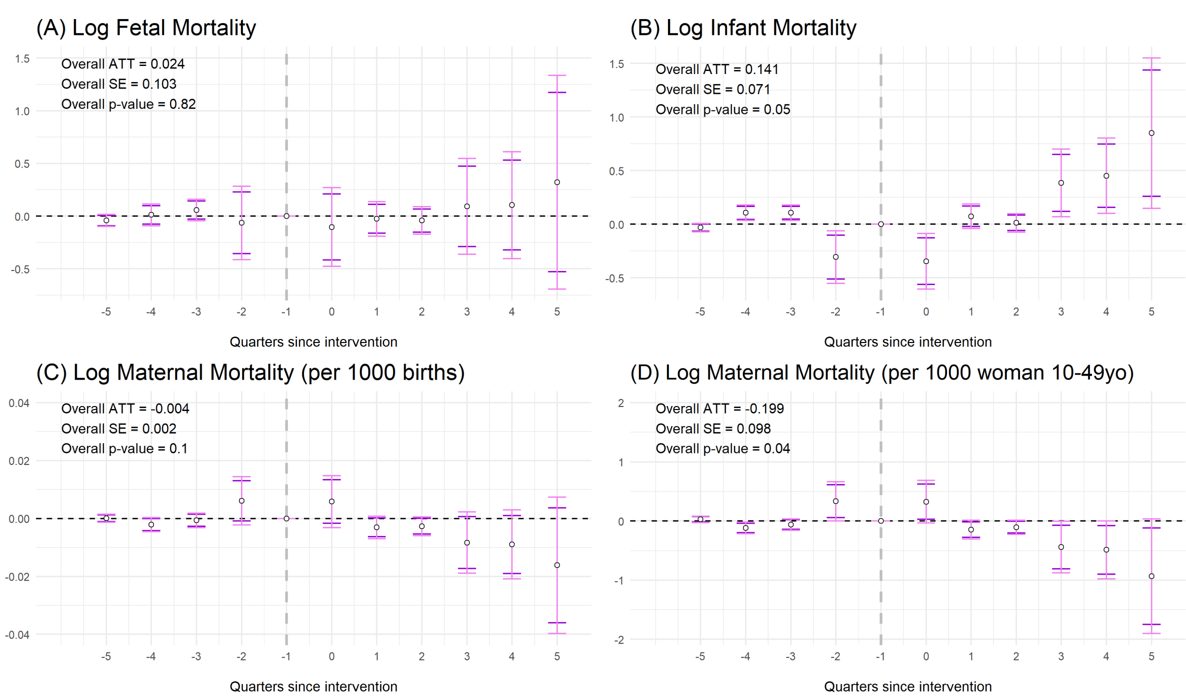


Event study estimates were generated through Two-Way Fixed Effects, with controls for municipality fixed effects and quarter fixed effects, outcomes related to mortality of mothers and babies. The response variables are (A) Fetal mortality, (B) Infant mortality, (C) Maternal mortality per 1000 births, and (D) Maternal mortality per female population of childbearing age. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 on the x-axis — regressions weighted by population. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

Appendix B. Estimated Results by Sun and Abraham (2021)

In this section, we analyze the results of our outcomes through the Sun and Abraham (2021) model; interaction weighted estimator. This estimator is a simple correction of the TWFE, where estimates are not counted using units "already treated" as controls, which can generate biased and inconsistent estimates. In Sun and Abraham (2021), the control group is composed only of the units never treated (or by the units of the last cohort treated), and the coefficients are weighted by an estimated share of units in a cohort. The method eliminates the possibility of possible negative weights, and the weights and the leads are consistently estimated.

Figure 14: **Effect of the program on hospital access**



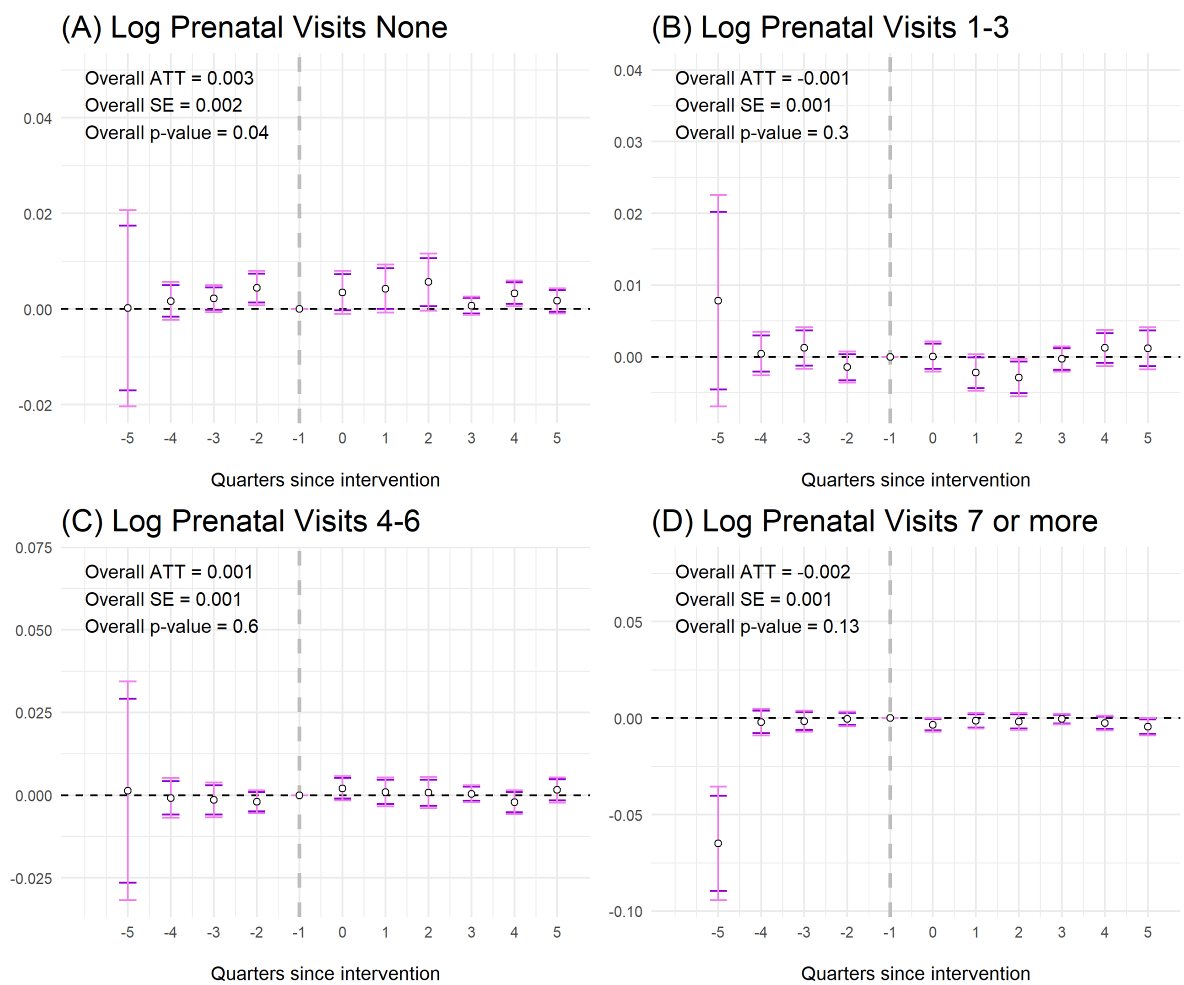
Event study estimates were generated by Sun and Abraham (2020) method, with controls for municipality and quarter fixed effects, outcomes related to hospital access and hospital procedures. The response variables are (A) Cesarean sections, (B) Birth in the hospital, and (C) Birth in other health facilities. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 and never treated. Regressions are weighted by the number of births. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

Analyzing the effects calculated by the estimator on our results of hospital access and hospital procedures, Figure 14, we see that, contrary to the estimate by Callaway and Sant'Anna

(2021), our results by Sun and Abraham (2021) are not significant. Furthermore, our response variables have a null effect after program entry. The selection of control groups explains this difference. As the control group used by Callaway and Sant'Anna (2021) is composed of "not yet treated" units, that is, it compares the treated group (g_t) with the one that will receive treatment in the following period (g_{t+1}). In contrast, the estimator of Sun and Abraham (2021) compares each treated group g_t, where t represents the quarter-year in which the group joined the program, with the last group that will receive the treatment (g_T), here the group of municipalities that was treated in the third quarter of 2013. We believe that since the estimator of Callaway and Sant'Anna (2021) compares municipalities that joined the program at closer dates, this would be the specification that best fits the problem.

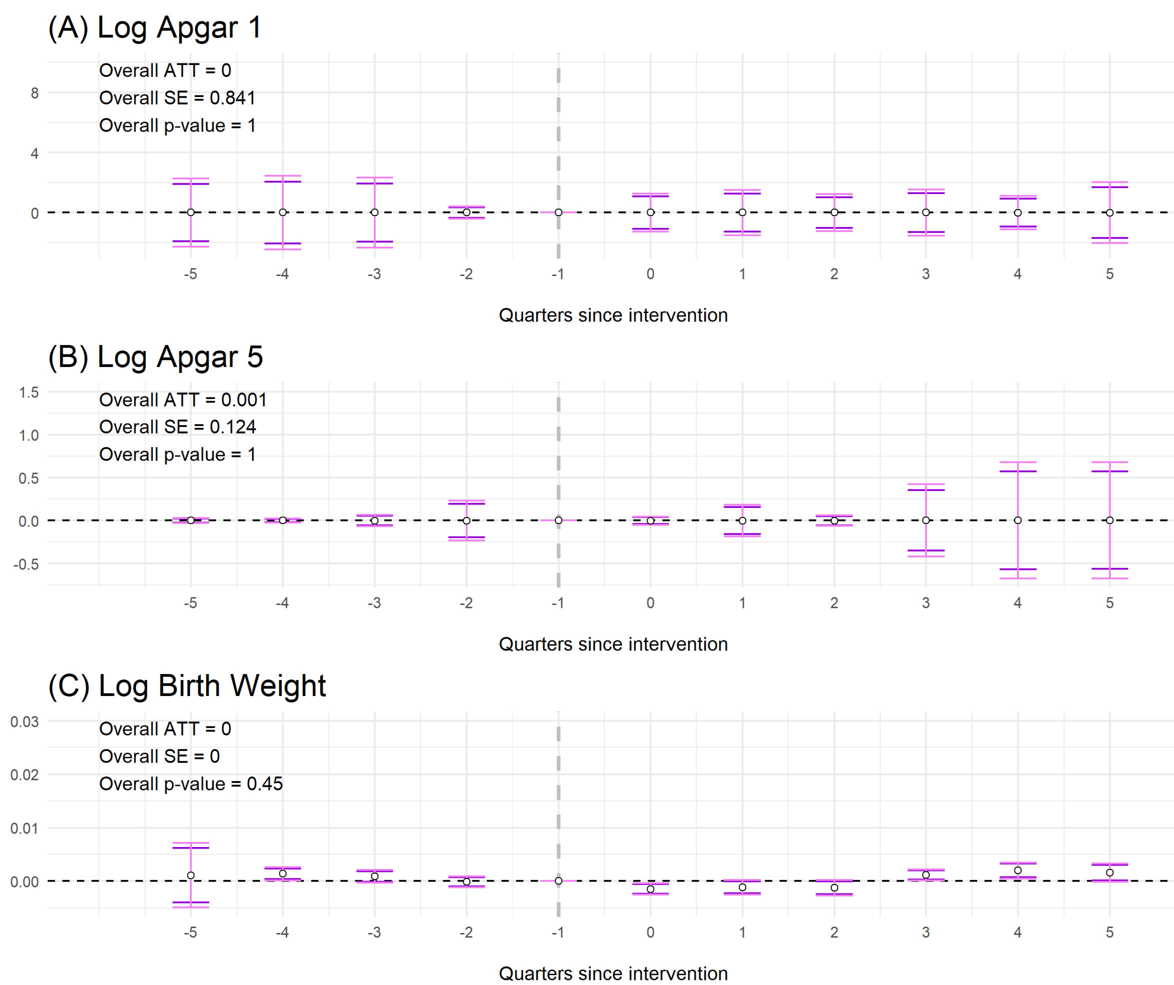
When we look at our outcomes related to prenatal care (Figure 15) different results are also presented from those estimated using the Callaway and Sant'Anna (2021) estimator, mainly in the -5 period. In this case, this difference occurs due to some noise in the control group, composed of 364 municipalities, five periods before treatment. As with Callaway and Sant'Anna (2021), we did not find considerable effects on average during the first five trimesters of program implementation on birth weight and Apgar scores. When we look at our mortality estimates, the event studies are inconsistent with the method. The results presented differ from zero before and after joining the program when we use the period before joining and the last period as controls, as indicated by Sun and Abraham (2021). However, due to the propensity score of the doubly robust estimator recommended by the authors, our pre-treatment estimates using the Callaway and Sant'Anna (2021) method have smaller confidence intervals, demonstrating greater consistency. In addition, we did not use bootstrap procedures in our results estimated through Two Way and Sun and Abraham (2021).

Figure 15: **Effect of the program on Prenatal Care**



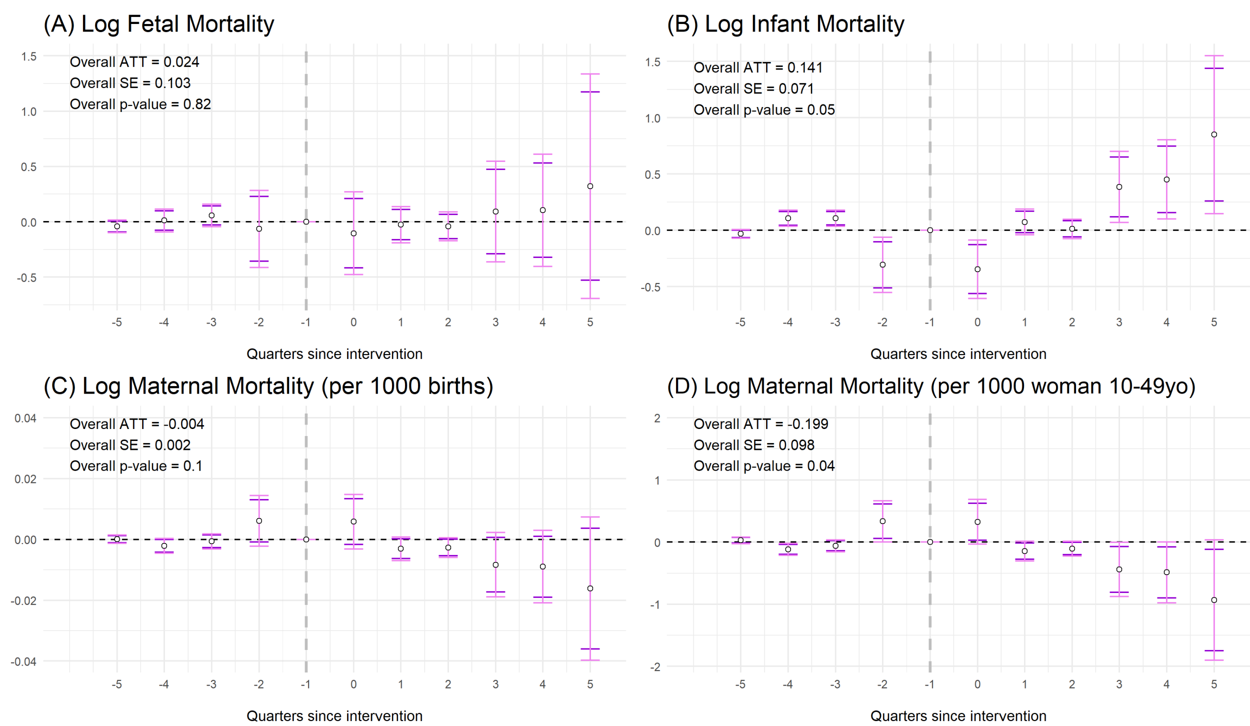
Event study estimates were generated through Sun and Abraham (2020), with controls for municipality and quarter fixed effects, outcomes related to prenatal care. The response variables are (A) Prenatal Visits None, (B) Prenatal Visits 1 to 3, (C) Prenatal Visits 4 to 6, and (D) Prenatal Visits 7 or more. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 and never treated. Regressions are weighted by the number of births. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

Figure 16: **Effect of the program on Birth Outcomes**



Event study estimates were generated through Sun and Abraham (2020), with controls for municipality and quarter fixed effects, outcomes related to birth. The response variables are (A) Apgar scores at 1 minute, (B) Apgar scores at 5 minutes, (C)Birth weight, and (D) Premature Birth. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 and never treated. Regressions are weighted by the number of births. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.

Figure 17: **Effect of the program on Mortality**



Event study estimates were generated through Sun and Abraham (2020), with controls for municipality and quarter fixed effects, outcomes related to mortality of mothers and babies. The response variables are (A) Fetal mortality, (B) Infant mortality, (C) Maternal mortality per 1000 births, and (D) Maternal mortality per female population of childbearing age. The year in which the program was introduced is normalized to zero. The estimated reference is the year before the program's implementation, -1 and never treated. Regressions are weighted by population. Standard errors are grouped at the county level. All figures show simultaneous 95 and 90 per cent confidence intervals.