Home Visiting Programs at Scale: The Impacts of Program Criança Feliz on Maternal and Child Health

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Abstract

Home visting programs are a promising option to stimulate the development of children from vulnerable families. We estimate the impact of Programa Criança Feliz, the largest home visting proram in the world, on maternal and child health. We explore sharp discontinuities that determine which municipalities can implement the program, and discontinuities that define the number of children treated. In addition, we analyse the phased implementation of the program across municipalities though a differences in differences approach. We find no significant impacts on medical care during pregnancy or at childbirth, on neonatal health indicators, on indicators of hospital admissions and on infant mortality.

JEL Classification: I12; I18; J13; L9; Q25

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1 Introduction

Early childhood is a sensitive and critical stage for the development of several essential life skills (Almond et al, 2006; Black et al, 2007; Currie and Thomas, 2001; Fox et al, 2010; Gertler et al, 2014; Nelson et al, 2014; Shonkoff and Phillips, 2000). The stimuli received in this early stage of life are determinants of various aspects in adulthood such as schooling, labor market participation, earnings and the propensity to engage in criminal activities (Heckman et al, 2010).

Many aspects, however, can limit a child's full development during early childhood. Inadequate nutrition (Grantham-McGregor et al, 2014; Hoddinott et al, 2013; Hoddinott et al, 2008; Victora et al, 2008), stress or violence in the family environment (Afif et al, 2012), recurrence of infectious diseases (Almond, 2006), little affection or interaction between children and their caregivers (Nelson et al, 2014) are some of the factors that impair child development. These factors are closely linked to the economic situation of the family (Currie and Hyson, 1999; Hart and Risley, 1995; Paxon and Schady, 2007). It is precisely the poorest children who are most likely to not experience an early childhood with adequate stimuli, which reinforces the transmission of poverty and inequality between generations (Currie and Moretti, 2007; Heckman and Raut, 2016).

The implementation of early childhood policies can mitigate the effects of poverty on child development. Preschool programs of high quality, for example, can have long lasting impacts (Deming, 2009; Garces et al, 2002; Ludwig and Miller, 2007; Ludwig and Phillips, 2008; Barnett et al, 2007, Garcia et al, 2023, Berlisnki et al, 2008; Berlinski et al, 2009; Naudeau et al, 2009, Pereira et al, 2017), with a high rate of return (Heckman et al, 2010).In addition to center-based ECD policies such as preschools, home visiting programs offer a high impact alternative that is specially attractive in places with constraints in the supply of daycare centers or preschools. In general, these home visiting programs aim to impact the child by strengthening family ties and by promoting positive and stimulating interactions between caregivers and children. In addition, these programs liaise with existing health services and can provide information on child health and nutrition.

The pioneering home visiting program, implemented in the 1980s on the outskirts of Kingston, Jamaica, not only improved the children's health and cognitive development indicators in the short and in the medium term: 20 years later, those who had participated in the program had 30% higher earnings than those who had stayed in the control group (Gertler et al, 2014). Since then, similar initiatives have been adopted elsewhere in the world, such as in Argentina (Programa Nacional Primeros Años), Colombia (Madres Líderes, evaluated by Attanasio et al, 2014 and Andrew et al, 2018), Chile (Chile Cresce Contigo, evaluated by Roman Eyzaguirre, 2017), Ecuador (Programa Creciendo com Nuestros Hijos), Mexico (Programa Educación Inicial PEI-CONAFE), Nicaragua (Amor para los Más Chiquitos), Peru (Cuna Más, described by Josefson et al, 2017 and evaluated by Rubio-Codina et al, 2018), as well as Bangladesh (Aboud, 2007; Aboud et al, 2013), Zambia (Rockers, 2018), Zimbabwe (Smith et al, 2018), Rwanda (Jensen et al, 2021), India (Andrew et al, 2020), Ireland (Doyle, 2022) and China (Zhou et al, 2022).

In Brazil, the Federal Government launched the Programa Criança Feliz (PCF) in 2016, which aims to promote child health and strengthen bonds between young children and caregivers. The target of the program are: i) pregnant women who receive cash transfers from the Bolsa Família Program (BFP), ii) children up to 3 years old whose families are beneficiaries of the BFP iii) children up to 6 years old beneficiaries of the federal disability transfer (Benefício de Prestação Continuada-BPC) and iv) and children up to 6 years old who have been removed from their families due to a protective order. The program consists of a series of decentralized actions involving the areas of Social Assistance, Education, Health, Justice, Culture and Human Rights aimed at stimulating the full development of children in their first years of life.

The most visible pillar of the program are the regular home visits made by trained professionals to support families in their interactions with the child, strengthening the family's bonds with the child, proposing playful activities to stimulate the child's development, and increasing the family's access to programs and services to which it is entitled to, such as health programs, daycare centers and the justice. In addition to the visits, the program encourages the integration of local (municipal and state) services from sectors such as health, education, and social protection, encouraging pregnancy monitoring through prenatal visits and promoting child vaccination.

This paper aims to estimate the impacts of the program on health indicators obtained from administrative bases using quasi-experimental methodologies such as difference-in-differences and regression discontinuity. By using a national panel of health indicators, we are able to study the program impacts for a large set of heterogeneous municipalities, and to study rare events, such as infant mortality, that cannot be captured by regular RCTs.

Specifically, we employ two different methodologies to estimate the impact of Criança Feliz on child health indicators. First, we explore program rules that determine a municipality's eligibility for the program and the maximum number of visits funded by the federal government. Our identifying hypothesis is is that the potential outcomes of interest vary smoothly around such discontinuities. Since both the municipality eligibility rule and the coverage rule were determined by the federal government without allowing the possibility for municipalities to manipulate the running variable (municipality population in 2010), strategic behavior of municipalities does not seem to be a major concern in our case. In this sense, our identification via RDD is quite similar to that used by Ludwig and Miller (2007) to estimate the impact of Head Start on infant mortality in the United States.

The second methodology employed is that of difference- in-differences. We exploit the launch of the program at the end of 2016 and the expansion of the target public in 2019 to compare municipalities that adhered and that did not adhere to the program over time, in an event study. Our hypothesis here is that the health indicators of the two groups of municipalities would follow a parallel trend if the program had not been launched [1]

Overall, we found no significant effects on indicators of pregnancy care, childbirth care, birth weight, neonatal health, and infant mortality, and mixed impacts of children's anthropometry. This might be due to low power of the tests when only a small fraction of children are treated. This, however, does not seem to be the case when we analyse municipalities close enough to the eligibility threshold. We also observe that many municipalities only treat a small fraction of the children that they could, and even a smaller fraction of pregnant women. Problems with the program implementation that have been captured by qualitative surveys (Ministério da Cidadania, 2019), such as poor training of visitors, high turnover of visitors and difficulties on transportation can also explain the absence of expected effects. Finally, the home visiting protocols might not promote child health, or visitors might not liaise the family with exisiting health services. We are not able to distinguish between these different mechanisms.

This paper is organized as follows: After this introduction, section 2 details the quasi-experimental techniques used, while section 3 describes both the program and datasets. In section 4, descriptive data on the PCF are initially presented to reflect its temporal expansion and the adoption of rules that generate the discontinuities. Next, we present the estimates of the impacts on pregnancy care and maternal, newborn, and child health, as well as some exercises to check the robustness of the results. The last section presents the concluding remarks.

[1] In the presence of heterogenous program effects, two way fixed effects estimates may yield biased estimates (Goodman-Bacon, 2021). We do not present yet here the estimates from newer estimators such as Callaway and Santanna (2021), Sun and Abraham (2021), Borusyak et al (2023) and de Chaisemartin and D'Haultfœuille (2020). However, the presence of a group of municipalities that was never treated alleviates the possible problem of biases from the TWFE estimation.

2 The Program, the discontinuities and its expansion over time

In this study, two distinct methods are applied to isolate the potential effects of the PCF on the outcome variables of interest. Each method exploits features of the program and available databases to identify the potential impacts of the intervention. These methods are presented in more detail below.

2.1 Regression Discontinuity

Many programs have eligibility rules defined based on pre-established cut-off points that define who can and cannot participate. For example, some programs use age or income rules as eligibility criteria. The proximity of the cut-off point tends to make the units of observation (e.g., persons, households or municipalities) very similar in both their observable and unobservable (by the analyst) characteristics, since being just below or just above the cut-off point is a situation that, in principle, is influenced by a factor that is outside the control of these units. One might think that being just above or just below the cut-off point is purely a matter of luck or bad luck. In this sense, the selection for program entry around the cutoff contains an element that resembles randomization.

The Regression Discontinuity Design (RDD) method seeks to identify the effects of interest based on this quasi-randomization that occurs in the vicinity of the program eligibility cutoff point. The basic assumption behind the method is that the mean of the outcome variable of interest in the treatment and non-treatment situations does not exhibit any intrinsic discontinuity at the cutoff point. In other words, this assumption requires that the characteristics of the observation units are similar and influence the outcome variable continuously around the cut- off point.

It is worth noting that the isolation of causal impacts occurs at the cutoff point, the identification of the effects of interest by the RDD method is local. That is, the identification is valid only for the surroundings of the cutoff point. There are two typical discontinuous regression designs: sharp and fuzzy. In the first case, participation in the program changes entirely when the cutoff point is crossed, that is, all those who are eligible participate in the program, while all those who are ineligible do not. In the second case, participation ends up occurring for a portion of the ineligible and non-participation for a portion of the eligible. In other words, while in the first case there is perfect compliance with the eligibility rule on both sides of the cutoff point, in the second, this compliance is only partial. This distinction between the two designs has some implications for the identification of the effects of interest, since imperfect compliance with the rule may be due to unobservable characteristics that influence both the decision to participate in the program and the outcome variables of interest themselves.

For example, it may be that some ineligible units find a way into the program because they anticipate a high return, while some eligible units prefer not to participate because they anticipate a small (or even negative) return from the intervention. In cases like this, eligibility for the program ends up influencing the participation of only those units that follow the exact rule, that is, participate if eligible and not if ineligible. Since only this group - called compliers - perfectly follows the rule, the average effect of the program is identified only for them. This effect corresponds to the so-called local average treatment effect (LATE), where the term 'local' is used in a fuzzy design both to denote that the effect refers to the cut-off point and to the specific group for which it is valid.

For the correct identification of the effect by the RDD method, it is important that the measured values of the variable that determines the eligibility of observation units - called the score variable - correspond to their true values. For example, it is possible that for some programs some units are able to purposely change the value of the score variable in order to become eligible or ineligible.

This type of situation is potentially problematic because the change in values can be correlated with the outcome variable, which introduces bias in the identification of the effect of interest. There are some tests that provide evidence of whether this type of "manipulation" has occurred. The most widely used is the test proposed by McCrary (2008) that analyzes whether there is a discontinuity in the density of the score variable at the cutoff point, that is, whether there is excessive concentration of units just above or below this point. The detection of such a discontinuity in the data is indicative that there is endogeneity in the allocation of units below or above the cutoff point, which greatly weakens or even invalidates the use of the RDD method.

The PCF has eligibility rules that can be explored to identify the impacts of the program through the RDD method. One of them (Resolution No. 20 of November 24, 2016) is that only municipalities with at least one Social Assistance Reference Center (CRAS), with a municipal average of the CRAS Development Index (ID-CRAS) higher than 3.0, and at least 140 individuals from the priority public, which is made up of pregnant women and children up to 36 months old who are beneficiaries of Bolsa Família, children up to 72 months old who are beneficiaries of BPC, and children up to 72 months old who are away from their family life due to a protection measure, were eligible for the program.

Another rule (coming from the same Resolution) is that the financing of the municipalities that join the PCF would be based on the number of participant individuals according to the following criteria:

- Municipality of small size I (up to 20 thousand inhabitants): Refinancing of up to 100 individuals per CRAS;

- Municipality of small size II (above 20 thousand and below 50 thousand inhabitants): Refinancing of up to 150 individuals per CRAS;

- Municipality of medium, large size and metropolis (Above 50 thousand inhabitants): Refinancing of up to 200 individuals per CRAS.

In this study, we explore discontinuities in eligibility rules according to: (1) the number of individuals of the priority public above 140 in the municipality; and, (2) funding for municipalities above 20 thousand inhabitants. As explained in the next section, data are not available at the individual level and, therefore, the units of observation in all analyses in the paper are based on municipal data.

2.2 Differences-in-differences

The second identification method employed in this study explores the expansion of the PCF over time. The expansion of the program took place sharply in its first two years of operation with almost 2500 municipalities having already joined the program in 2017. Thereafter, the pace of adherence slows with 500 more municipalities joining the program in the following two years.

The difference-in-differences (DD) method compares municipalities that have and have not adopted the program between years. The logic of the method makes use of the temporal dimension as it compares treated and untreated municipalities before and after entering the program. This temporal comparison between groups allows municipalities to differ not only in observable characteristics, but also in unobservable characteristics that are time invariant. In other words, the method is able to take into account all the characteristics of a structural nature of the municipalities whose observation or measurement is difficult to operationalize.

The basic hypothesis behind the DD method is that the evolution of the average of the outcome variable of the untreated municipalities represents the trajectory of what would have happened to the participating municipalities if they had not joined the program. Of course, this hypothesis is not directly testable, but evidence about its validity can be obtained based on a test of whether the trajectories of the mean of the outcome variable between the treated and untreated groups before the program are similar.

In its basic form, program impact is estimated by the double difference of the means of the outcome variable between the treated and untreated groups in the intervals after and before program entry.

For some programs, program entry occurs differentially between time periods, which allows the untreated units to be used as a control group for those already treated up to the time the former enter the program. In some cases, there are units that do not enter the program until the end of the observation window; these units constitute a (pure) control group that can be used for all treated units throughout the period. Seen as one of the possible designs of the DD methodology, this method - known as event studies - allows heterogeneous program effects to be estimated by period by temporally aligning the units of the treatment and control groups relative to the time of program entry. Since the program had a sequential entry of municipalities over time, we will apply the difference-in-differences methodology through the event study design, which allows us to test not only the parallelism of the trends before joining the program, but also the dynamic effects of the program.

To do this, we will estimate an event studies equation using the Ordinary Least Squares (OLS) method. We can estimate these dynamic effects by including binary treatment variables in which the treated municipality is assigned a value of 1 when treated and zero otherwise. These variables can be either leads, when we are referring to treatment that is still to come, or lags, when we are referring to the past. This allows us to estimate the effect of the program for different years of implementation, and to detect anticipation effects of the program [2].

Formally, we can estimate an equation of the type:

$$y_{mt} = \alpha_m + \theta_t + \sum_{\tau=0}^{h} \delta_{-\tau} T_{m,t-\tau} + \sum_{\tau=1}^{q} \delta_{+\tau} T_{m,t+\tau} + \rho X_{mt} + \epsilon_{mt} \tag{1}$$

where y_{mt} is a health indicator at the municipality level in year t, α_m are municipality fixed effects (which capture any time-invariant municipality characteristic, even unobserved ones such as location, climate, soil, local culture, infrastructure, quality of county administration) and θ_t are year fixed effects (which capture time shocks common to all municipalities such as climate variations, inflation, macroeconomic shocks, changes in federal administration). The summations on the right-hand side of the equation allow for the inclusion of lagged variables ($T_{m,t}$, $T_{m,t-1}$, ..., $T_{m,t-h}$), which estimate the effect after treatment, and anticipated variables

[2] In the presence of heterogenous program effects, two way fixed effects estimates may yield biased estimates (Goodman-Bacon, 2021). We do not present yet here the estimates from newer estimators such as Callaway and Santanna (2021), Sun and Abraham (2021), Borusyak et al (2023) and de Chaisemartin and D'Haultfœuille (2020). However, the presence of a group of municipalities that was never treated alleviates the possible problem of biases from the TWFE estimation.

$(T_{m,t}, T_{m,t+1}, ..., T_{m,t+q}$, which estimate treatment anticipation effects. Here, δ_0 captures the immediate effect of the PCF in the year of program implementation, while δ_{-1} measures the effect of the PCF in the second year of implementation, and so on.

3 Data

To estimate the impact of Programa Criança Feliz we will use three large groups of data, all aggregated at the municipality level. The first large group is data on action indicators, such as prenatal care, and outcome indicators, such as maternal and child health, and hospital production.

The health data come from different databases that make up DATASUS. The variables related to pregnancy, delivery, and neonatal health come from the Information System on Live Births, SINASC. The database contains information on the number of prenatal visits, the month of the first prenatal visit, whether the delivery was vaginal or by cesarean section, whether it was in the hospital or at home, whether it was attended by a doctor, nurse, or midwife, in addition to the child's birth weight and the APGAR index, which measures signs of vitality of the child at birth 1 minute and at 5 minutes of life.

The infant mortality data comes from the Mortality Information System, SIM. The base contains information about the age at which the individual died, in addition to the causes of death. Some of the causes of death are classified as preventable, either through vaccination, care for women during pregnancy and delivery, or through correct diagnosis and treatment. The data on hospital admissions were extracted from the Hospital Information System, SIH. The database has information about the number of hospitalizations, by age group, in the municipality where the person lives. In addition to hospitalizations, the database contains information about the average length of stay, the cost of hospitalizations, the cost accounting for professional services only, the amount paid by federal transfers, and the death rate in these hospitalizations. To complete the health data, we have the data from the Food and Nutrition Surveillance System, Sisvan. The system has anthropometric information of the children living in each municipality, by age group. We have, therefore, the number of children with weight or height below the expected for their age, or the proportion of children with adequate weight and height.

The second large group of data is administrative data from Programa Criança Feliz, which comes from the monitoring of the program. This data allows us to know how many children or how many pregnant women were assisted in the municipality, on what date the municipality joined the program, or how much the municipality received from the Federal Government to implement the PCF.

Finally, the third large group of data is composed of control variables, which allow us to better characterize the municipalities and increase comparability among them. These characteristics include the population composition of each municipality through data from the 2010 Census (IBGE), estimates of municipal GDP and GDP per capita (IBGE), data on municipal finances (FINBRA), proportion of the population with access to water and sewage (Census 2010), average score of students in standardized tests of the Basic Education Evaluation System (SAEB-INEP), pass, fail, and dropout rates of students in basic education (School Census - INEP), in addition to data on the hospital structure of the municipality (Census 2010), such as the number of health posts and hospitals, and the number of medical teams in the municipality (National Register of Health Establishments).

4 Results

4.1 The expansion of the program

In order for us to apply the difference-in-differences methodology, it is crucial to understand how the expansion of the program took place. The rules for joining the program were defined by Resolution 20 of National Council for Social Assistance. According to the resolution, the priority public of the program were children up to 36 months of age from BF beneficiary families, in addition to children up to 72 months of age who were BPC beneficiaries.

Figure 1 shows the expansion of the program over time. Soon after the publication of the resolution that marks the launch of the program, there is a period of strong adherence by municipalities. Between November 2016 and March 2017, more than 2,000 municipalities joined the program. After this initial period, there are two more adhesion windows, at the beginning of the second semester of 2017 and throughout the first semester of 2018.

After this period, the number of participating municipalities remained stable. The picture would only change after the publication of Ordinance N^o 1217/2019, which expanded the definition of the program's priority public to all beneficiary children of the CadÚnico in the municipality. The redefinition of the priority public allowed the inclusion of 308 more municipalities between the second semester of 2019 and the first semester of 2020.

How did this expansion of the program take place in the various Brazilian regions? Analyzing the expansion per region is important so that we can have a better idea of the control municipalities used in the exercise. Ideally, we would like to have treatment and control municipalities spread throughout the territory, perhaps neighbors. The more spread out the treatment and control municipalities are, the greater the chance that the two groups will be similar.

Figures 2 to 6 show how the adhesion to the program has evolved in each of the five major Brazilian regions. In the North Region, Tocantins was the state with the lowest adherence. Adherence in the stated was especially low in the first wave of adherence in 2016. Rondônia is another state with low initial adherence. In contrast, the state of Roraima joined the program heavily in the very early months. Adherence has also been high in Acre since the program's launch.

The Northeast is the region with the most massive presence of the program. There are few municipalities that did not adhere, most of them located in Bahia. No Ceará, all municipalities adhered to the program at some point. The map helps us understand the comparisons that will be made in the next sections. Most municipalities in the Northeast joined in the first window, but there are many municipalities that entered from 2018, especially in Maranhão, Pernambuco, and Bahia. These municipalities serve as a control for the municipalities in the first window, in the first year of implementation of the program. For the following years, however, we are getting fewer observations in the control group (in both methodologies), which may affect the precision of the estimates if we restrict ourselves only to the Northeast region.

In the Southeast Region, there is a great deal of heterogeneity regarding adhesion, which is positive for the identification of the effect of the program. There are many control municipalities that have never adhered, but there is also a large amount of municipalities that have adhered in Minas Gerais and São Paulo, although many of these municipalities are concentrated in the North of Minas. There are also many municipalities that entered in the second semester of 2019, spread throughout the region.

The picture is the opposite of the Northeast in the South Region, where few municipalities have adhered to Criança Feliz. In Santa Catarina, only 12 of the 295 municipalities in the state have joined the program. In Rio Grande do Sul, 63 municipalities have adhered soon after the launching of the program. In Paraná, 72 municipalities adhered, 36 of them right after the launch and 30 after the expansion of the program's target public in 2019. This situation is not surprising, and is understandable in light of the adhesion rules. The South Region has many small and relatively wealthier municipalities, which makes it difficult to reach the minimum number of 140 Bolsa Família Program children aged 0 to 3 years.

Finally, the Midwest Region presents a much more homogeneous distribution of adhesion to the program. In Goiás, a little more than half the municipalities eventually adhered to the program, while 36% and 40% of the municipalities in Mato Grosso and Mato Grosso do Sul, respectively, adhered to the program.

4.2 Balance

Ideally, we would like the municipalities that implemented the program to have the same characteristics as the municipalities that did not implement the program. If the two groups of municipalities were equal in observable characteristics, we would have more confidence that the unobserved characteristics were also similar between the two groups. This would allow us to have more confidence that differences in child health between the two groups is due to the program, and not to economic and social characteristics of the two groups of municipalities.

Since the program was implemented by adherence of the municipalities, it is unlikely that the group of municipalities that implement and that do not implement the program are similar. As we saw earlier, for example, municipalities from the Northeast have a much larger participation in the program than municipalities from the southern region. The program participation criteria themselves already induce a differentiated selection of municipalities, but other unobserved factors may also weigh on the mayor's decision to join or not, such as the mayor's political alignment with the state government and the federal government, the mayor's knowledge about the importance of early childhood (Hjort et al, 2019), among other factors.

Table 2 shows some characteristics of the municipalities that joined and did not join the PCF. As it is easy to notice, the two groups were already quite distinct before the launch of the program. Municipalities that implemented the program are more populous, with an average of almost 42 thousand inhabitants in 2010, against an average of 25 thousand inhabitants among those that did not join. The participating municipalities are also poorer: They have lower GDP per capita, lower income per capita in rural and urban areas, lower value generated in services, and lower net tax collection per capita.

The education in these municipalities is also lower, with lower approval rates in the early years (1st to 5th grade) and lower basic education development index (IDEB). The coverage of health services is also worse: There are fewer health establishments and fewer beds for every thousands inhabitants. None of this is surprising, since the municipalities that joined the program are concentrated in the North and Northeast regions of Brazil.

Regarding neonatal and child health indicators, children in the municipalities that would eventually join Programa Criança Feliz were born with 30 grams more, on average, in 2015, but with worse Apgar scores. According to SISVAN data, there was a lower proportion of children aged 0 to 6 months with height and weight below the appropriate for their age, but this advantage is reversed for older children, aged 2 to 5 years. These findings reinforce the inadequacy of simply comparing municipalities that have and have not implemented the program. How can we obtain comparable groups that allow us to isolate the impact of the program?

Our first strategy, as we have already discussed, seeks to compare the evolution of health indicators over time. In this sense, it is not a problem to have municipalities with different characteristics in the treatment and comparison groups, as long as the evolution trends of the indicators are parallel before joining the program. When analyzing the evolution of the number of monthly hospitalizations of children (per thousand inhabitants) aged 1 to 4 years in municipalities that never joined the PCF, and in municipalities that joined soon after the launch and after the expansion of the target public in 2019, although the average number of hospitalizations is different among the three groups of municipalities, the evolution of trends is similar.

When we use the regression discontinuity methodology, we consider that the arbitrary cutoff of 140 children from the target public works as a draw around this discontinuity. Thus, we expect the characteristics of the municipalities close to the cutoff point to be balanced. And this is exactly the case. Table 3 reproduces the previous table, showing the difference in averages of numerous variables of the municipalities above and below the cutoff point, but when we restrict the sample to a window of municipalities with more than 130 and less than 150 children in the target public.

In all, there are 118 municipalities that have between 130 and 139 children in the target public, and therefore were not eligible for the PCF, and 93 eligible municipalities that had between 140 and 150 children in the target public. As can be seen, within this window there are no more differences in relation to municipal GDP, nor in relation to tax collection, HDI, quality of basic education in the municipality, or even in relation to the supply of health services.

What we will do in the next sections to identify the impact of the PCF is precisely to restrict the window around the cutoff point, but at the same time to control flexibly for the relationship between health indicators and the number of children in the target public. If this relationship is discontinuous around the cutoff point, then we have a program impact

4.3 Does discontinuity exist in practice?

Before properly estimating the impact of the PCF on maternal and child health, we need to verify whether in fact the cut-off points defined by the program's rules actually generate a difference in the municipality's probability of joining the program or in the number of children served. Figures 7 to 10 show, for each year, the probability that the municipality has adhered to the PCF, according to the number of target children in 2016. In all figures, we worked with the number of children in the target public in 2016, not the number of children in the current year. With this we avoid any possibility of "manipulation" of this index. The mayor or the municipal secretary of assistance that wants to join the program can make an additional effort to enroll more children in the Bolsa Família Program and thus exceed the limit of 140 children. This could introduce a correlation between the number of target children (our running variable) and participation in the program, which would take away the randomness around the discontinuity. Even if the mayor does this, he cannot change the number of eligible children at the time the program was launched. In this way, therefore, our running variable is exogenous and not manipulable. Figures 7 to 10 show that the cut-off point of 140 target children does indeed change the likelihood of the municipality entering the program. The first figure below shows that after the first wave of municipalities entering the program, between late 2016 and early 2017, approximately 60% of small Brazilian municipalities having between 140 and 160 children of the target public joined the program. In contrast, no municipalities below the cutoff point had joined the program. Already by the end of the 1st half of 2018, a few municipalities that had fewer than 140 children of the target public in 2016 were able to eventually join the program, as they eventually managed to exceed the threshold of 140 children.

Still, the discontinuity implied an increase of approximately 50 pp in the chance of the municipality participating in the program. The situation hardly changes in the following year. However, the expansion of the program's target public in the second half of 2019 changes the picture somewhat, causing 25% of municipalities below the cutoff point (from 2016) to enter the program. With this entry, the differential in probability of adherence to the left and right of discontinuity drops a bit, from 50 pp to approximately 35pp.

Having verified that indeed the discontinuity of 140 children from the target public does in fact induce a higher probability of the municipality adhering to the program, we need to verify whether the fact that the municipality adheres actually implies that children are being visited. The administrative monitoring data of the program helps us to answer this question. Unfortunately, however, we have not so far obtained monitoring data for the years 2016 and 2017, a crucial period that corresponds to the first year of program implementation for most of the municipalities that joined. Still, in possession of the data for 2018 and 2019 we can verify if in fact how many children are being served.

One way to answer this question is to use the difference- in-differences methodology, using an event study specification. That way, we can dynamically see the impact of the program in its first, second, and third year of operation. Since we do not have the data for 2016 and 2017, our sample for the first year of operation will be smaller than it should be, increasing the variance of the estimate somewhat. However, since we have municipalities entering the program in 2018 and 2019, we are still able to estimate this effect.

In fact, we see that the introduction of the program in a municipality raises the number of children served, and this number grows over time, as shown in figure 11. The large municipalities pull the average upwards. In the first year of introduction, there are approximately 200 children visited per month on average, and the number rises to more than 400 children per month in the 3rd year of implementation.

The figure also shows a large dispersion in the number of children treated over the years of implementation of the program. This means that, while some municipalities were able to visit the number of children agreed upon in the goals (some even more children than agreed upon), others were not able to get the program off the ground, making few visits. In addition to analyzing the impact of municipality membership on field visits, it is important that we look at whether the discontinuities, both the 140 children for membership and the municipal size discontinuity lead to an increase in the number of children visited.

Figure 12 shows the impact of the municipality's eligibility (due to having more than 140 children in the target public) on the number of children visited in 2018. As we cross the cutoff point, the number of children served jumps from around zero to approximately 30 children. Since we know exactly the number of eligible children, the program in these municipalities was reaching only 20% of eligible children.

Considering that these are typically small municipalities, with less than 20 thousand inhabitants and with 1 CRAS, the typical target for these municipalities is to serve 100 children. We are talking, therefore, about an attendance of only 30% of the agreed target.

Figure 13 shows how the discontinuity of size affects the number of children actually visited per month in the municipality. As can be seen, cities of up to 20 thousand inhabitants, considered small, attend around 100 children per month. This number jumps to 150 children per month for cities with more than 20 thousand inhabitants in the 2010 Census. It is possible to observe that some cities manage to have a slightly higher number, because they have more than one CRAS. When we observe the discontinuity of 50,000 inhabitants, we also observe a jump, from approximately 150 children served to a little less than 250 children. In this second discontinuity, however, we have few municipalities on the right side of the discontinuity, and high variance in the number of children served. Because of this high variance, the difference in the number of children served around this discontinuity is not statistically significant. Therefore, in the next analyses we will only consider the first discontinuity around 20,000 children.

5 Results

5.1 Impact on care during pregnancy and childbirth

Having verified that indeed the introduction of the program implies more children cared for, we can estimate the impact of the program on maternal and child health. We first present the results on pregnancy care, then we move on to neonatal health rates, then to health indicators for children under 1 year old, and then to children 1 to 4 years old. Programa Criança Feliz has as one of its objectives to take care of the child from conception. This is one of the foundations of all the discussion about the first thousand days of a child's life, beginning at conception. We know that the development of the child's entire neurological system begins in the first trimester of pregnancy, and that a healthy pregnancy favors child development after birth. That is exactly why the program's guidelines include intersectoriality, seeking the integration with the health area to ensure that all pregnant women monitored by the program have an adequate pregnancy, with prenatal care, early detection of possible complications for the child and the mother, and a birth with all the necessary assistance.

The estimation of program impact was operationalized based on the procedure proposed by Calonico et al. (2014). This procedure makes use of local regressions to measure the size of the discontinuity over the cut-off point of the score variable. More specifically, regressions are estimated against a polynomial of the score variable within windows defined to the left and right of the cutoff point in such a way as to obtain the distance from the mean of the variable of interest exactly at the cutoff point. Typically, these windows are defined so as to minimize both potential estimation biases and the variance of the estimate of interest.

Table 4 shows the impact of the program on the number of fetal deaths in the municipality, the number of preventable deaths of the mother or baby by number of preventable deaths from childbirth care, number of prenatal visits, probability of cesarean delivery, and the probability that the birth was attended by a doctor or midwife. The data are from the SIM and SINASC. The table reflects the estimates from the difference-in-differences model using the event study specification. Since all these data are available only through 2018, we only have estimates for the year in which the program was implemented and the year following implementation.

Table 4 shows that the program had no statistically significant impact on any of these variables, except for the probability of the birth being attended by a midwife. Only 1.3% of the deliveries in the sample are performed by a midwife, and this probability drops by 0.2 pp, which may be a sign of increased referral of pregnant and parturient women.

Figure 14 illustrates the impact of the program on the number of prenatal visits for the municipalities around the discontinuation of 140 children from the target public. The confidence interval just below the cutoff point is quite wide, which makes the impact not statistically significant. This result is qualitatively similar for all other variables, as can be seen in table 5. We therefore find no impact of the program on the pregnancy and childbirth care variables using the regression discontinuity methodology when we look at the discontinuity that defines municipal eligibility.

Finally, we can also apply the same exercise to identify the impact of the program using the discontinuity of size for municipalities around 20,000 inhabitants. Again, using the number of prenatal visits to illustrate the RDD method graphically, we are unable to observe any impact around the discontinuity (figure 15). This is corroborated by the estimates present in table 6.

In this case, we cannot reject the hypothesis that these variables are equal to zero. The number of preventable deaths from childbirth care is significant at 10%, with a high coefficient in front of its mean. Given the magnitude of the impact, it is likely that this result is due to some outlier municipality.

5.2 Impact on neonatal health

The most immediate consequence of adequate care during pregnancy is good newborn health. The SINASC and SIM databases allow us to analyze some neonatal health indicators, such as birth weight, APGAR index, and neonatal mortality. Table 7 shows the impacts of the program on neonatal health using the difference-in-differences methodology. We found no significant effects in relation to infant birth weight, nor in relation to the APGAR index for the first minute of life or the number of infant deaths between the first week and the end of the first month. In contrast, we found a small negative effect on the APGAR index at the 5th minute of life and a reduction in deaths in the first week of life, both significant at the 10% level.

When using the RDD methodology around the eligibility cutoff point of 140 children in the target public, we found no impact of the program on the aforementioned variables (Table 8). Figure 16 illustrates the lack of significant impact on birth weight.

The same result is found when we apply the RDD methodology on the discontinuity defined by the size of the municipality around 20,000 inhabitants, with the exception of deaths between the first week and the end of the first month of life, for which we find a slight increase, significant at the 10% level (Table 9 and Figure 17).

5.3 Impact on children's health – Children up to 1 year old

Finally, we analyze the impact of the program on some other child health indicators. We start with children under 1 year of age. Since the SIH and SISVAN databases are available until 2019, we were able to calculate the impact of the of the program in the 1st, 2nd, and 3rd year of implementation. We did not find any impact of the program on the percentage of children with appropriate weight or height for the age, nor on the number of hospitalizations, hospital stays, or the total amount spent on hospitalizations of children aged 0 to 1 year. We did find, however, a significant 10% reduction in the total number of deaths of children under 1 year of age (table 10).

Regarding the identification via RDD, using the cutoff of 140 children, we found a significant impact of 5% reduction in the proportion of children with appropriate weight for their age, and a reduction of almost 10 hospitalizations per year, significant at 10% (table 11). It is possible that the reduction in the proportion of children with appropriate weight is due to an increase in malnourished children, or also an increase in the number of overweight children.

Figure 18 illustrates the impact on the percentage of children with weight appropriate to age. It is possible that this result is due to some outlier municipality, just to the right of the discontinuity, with an extremely low proportion of children with weight appropriate to age. As we have a relatively small number of municipalities on either side of the discontinuity, the impact of an outlier municipality here could be large.

When we analyze the size discontinuity, however, the result is the opposite. We observe an increase of 4.5 pp in the proportion of children with appropriate weight for their age (figure 19 and table 12). Note that here, however, we have many more observations and a much smaller standard error of the estimates than in the previous RDD. We do not observe, however, any impact on height, on the number, duration and value of hospital admissions, nor on the total number of deaths.

5.4 Impact on children's health - Children aged 1 to 6 years

In this last section of results, we present the results of the estimations for children older than 1 year. Unfortunately, the age cutoffs are not uniform among the DATASUS databases. SISVAN, for example, works with a range of children from 6 months to 2 years of age and from 2 years to 5 years. The SIH, on the other hand, works with a cutoff of 0 to 1 year of age and 1 to 4 years of age. Since it is impossible to harmonize these age cutoffs, we work with all the cutoffs up to 5 years of age.

Table 13 shows the difference-in-differences estimation of the impact of the intervention on children's anthropometrics (weight and height) and on the number of hospitalizations, the total cost of hospitalizations, and the number of deaths in hospitalizations. We found no significant impact on any of these variables.

When estimating the impact using the RDD methodology around the eligibility cutoff point, we also found no impact on these variables, except for the proportion of children aged 6 months to 2 years with height appropriate for their age (table 14). We observed a 7 pp drop in this proportion, significant at the 5% level. Figure 20 illustrates this impact.

When analyzing the impact on the height of children 6 months to 2 years, consistent with the findings for children under 1 year old, we also found the opposite result for the RDD based on population size versus the RDD based on the number of children (figure 21 and table 15). We observed a percentage point increase in the proportion of children with appropriate height to age, significant at the 1% level.

6 Conclusion

Home visitation programs are a promising option to stimulate child development among families in situations of social vulnerability. The Programa Criança Feliz, conceived under the inspiration of Jamaica's successful experience and launched in 2016, is already the largest home visitation program in the world. The size of the program, its national coverage, and the need for intersectoral integration between the areas of social assistance, health, education, and justice bring enormous challenges to the implementation of the program with the quality needed to change the life trajectory of the children being assisted.

Complementing the randomized evaluation of program, this paper assesses the program's impact on child health. Instead of employing a primary data collection, we used public data from DATASUS, and explored 2 different quasi-experimental methodologies enabled by the program rules to identify its impact.

Overall, we found no significant impacts of the program on pregnancy care, such as number of prenatal visits, probability of fetal death, or probability of delivery via cesarean-section, although we did find a small reduction in the probability of delivery being assisted by a traditional midwife. However, we found no impact on newborn health indicators such as birth weight, APGAR index, or probability of infant death in the first weeks of life. Nor did we find any impact on the number of infant hospitalizations or infant mortality over the first 4 years of life. Regarding anthropometry, we found mixed results with opposite signs depending on the discontinuity analyzed.

One of the possible reasons that may be behind these results comes from the data limitations of the study. The program's treatment takes place at the individual level, in a few families, but the variables analyzed refer to averages at the county level. The lower the proportion of treated children in the municipality in relation to the total number of children in the analyzed age group, the lower the chance of finding a statistically significant effect even if the program does have an impact on the treated families. Since few children are treated in relation to the total, the positive impact would be diluted in the municipal average.

Another possible reason is the low coverage of the program in relation to the target public. On average, municipalities monitor only 60% of the families agreed upon in their target, and in only 70% of the cases do the visits take place on a weekly basis. While there are some municipalities that perform slightly better than their target, almost 7% of the municipalities that had joined by 2017 did not perform any visits in 2018.

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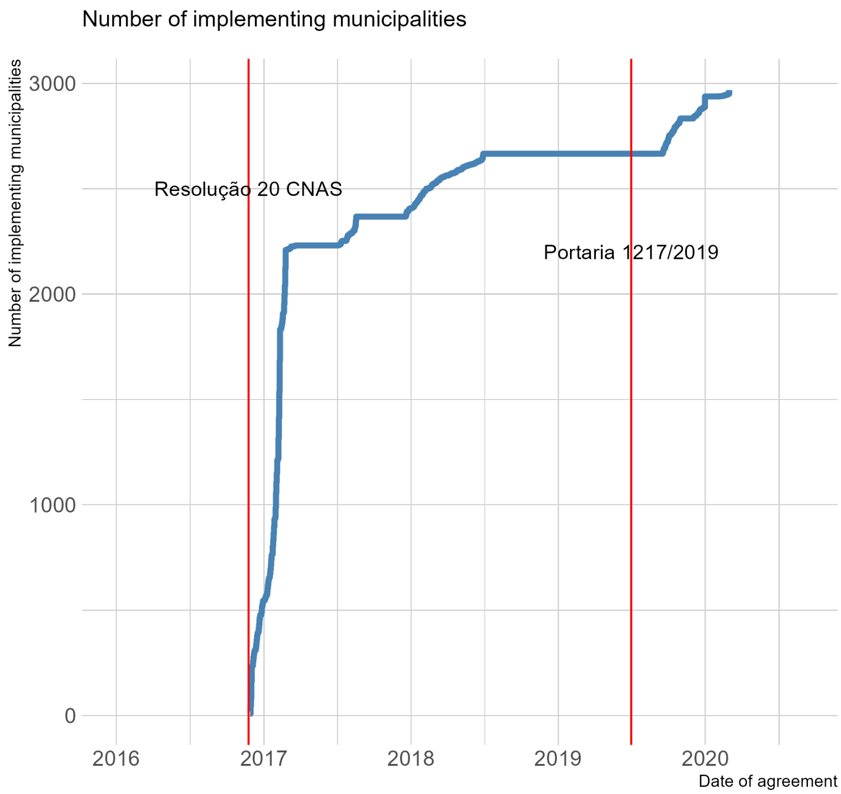
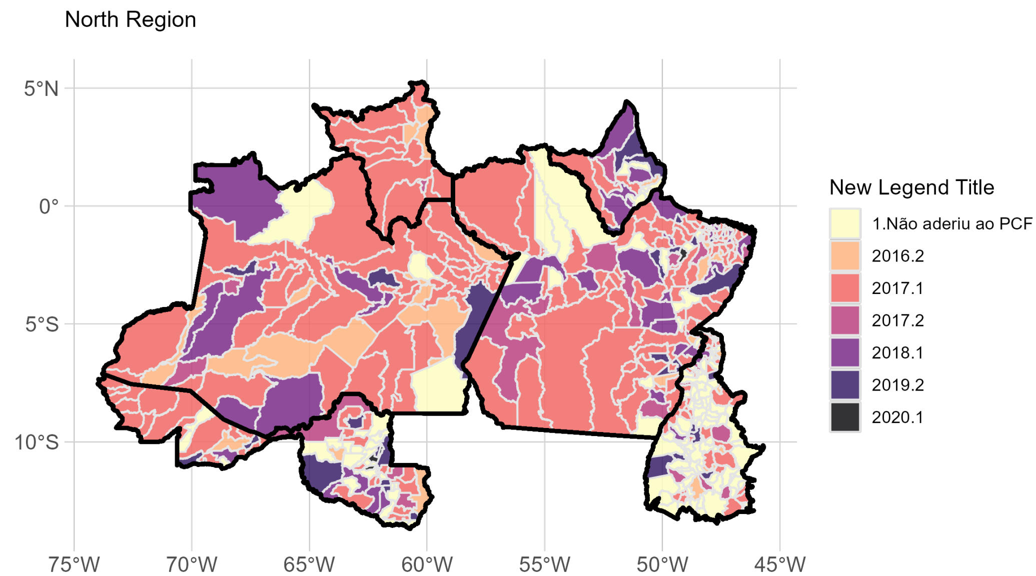
8 Figures

Figure 1: Expansion of Program Criança Feliz



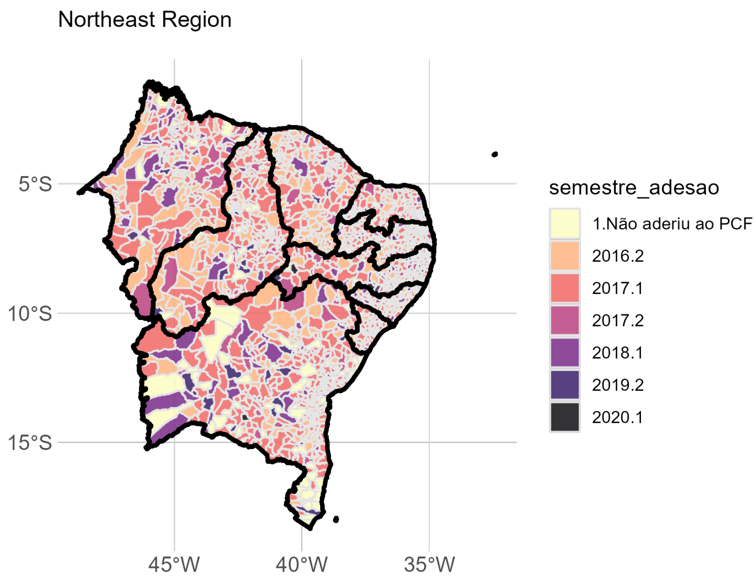
Notes. The figure shows, over time, the number of municipalities that signed the agreement with the Federal Government to implement Program Criança Feliz. Municipalities start signing implementation agreement by the end of 2016, after the passing of "Resolução 20-CNAS". By mid 2019, the ruling "Portaria 1217/2019" expanded the target group of the program, which allowed additional municipalities to implement it.

Figure 2: Expansion of Program Criança Feliz: North Region



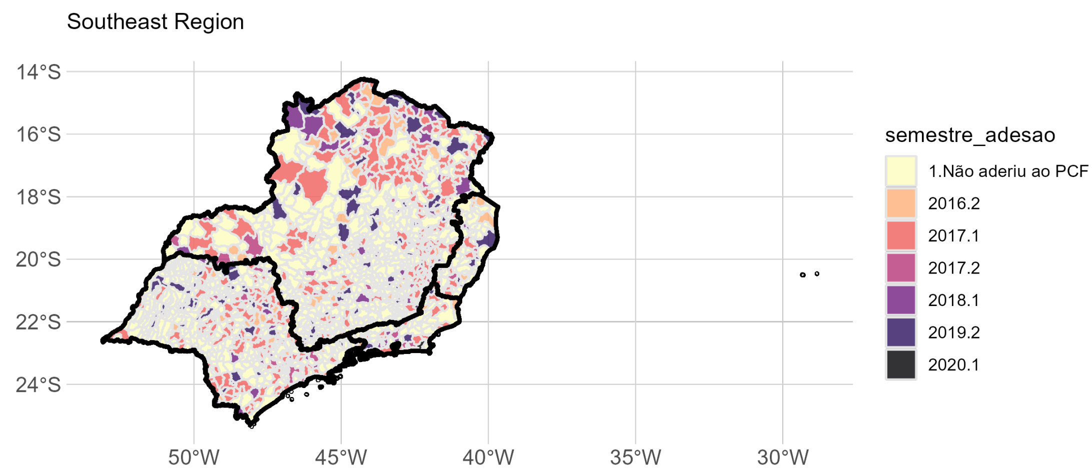
Notes. The map shows the expansion of Program Criança Feliz over time. Municipalities in yellow did not implement the program.

Figure 3: Expansion of Program Criança Feliz: Northeast Region



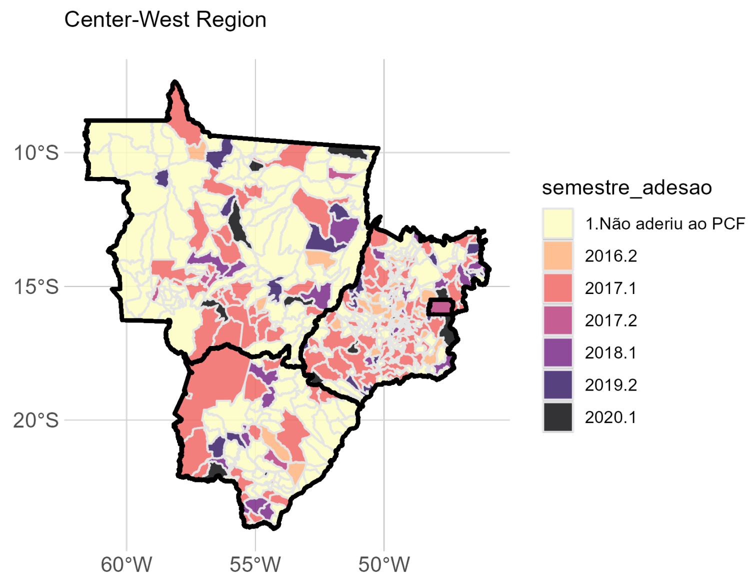
Notes. The map shows the expansion of Program Criança Feliz over time. Municipalities in yellow did not implement the program.

Figure 4: Expansion of Program Criança Feliz: Southeast Region



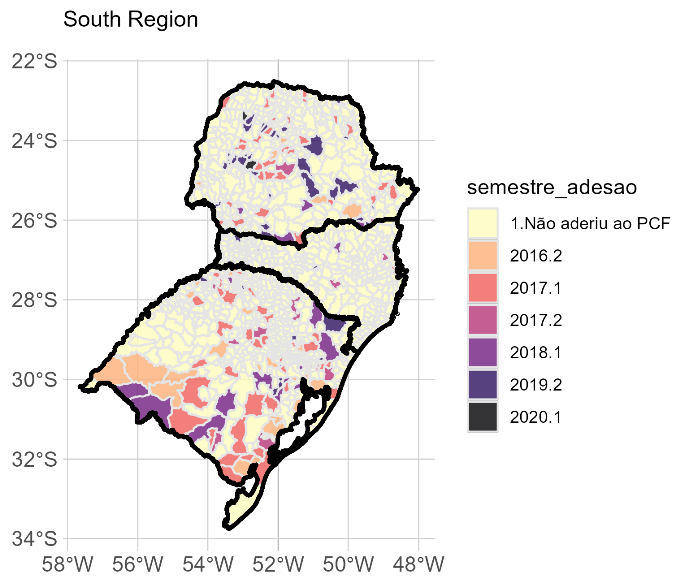
Notes. The map shows the expansion of Program Criança Feliz over time. Municipalities in yellow did not implement the program.

Figure 5: Expansion of Program Criança Feliz: MidWest Region



Notes. The map shows the expansion of Program Criança Feliz over time. Municipalities in yellow did not implement the program.

Figure 6: Expansion of Program Criança Feliz: South Region



Notes. The map shows the expansion of Program Criança Feliz over time. Municipalities in yellow did not implement the program.

Figure 7: Program eligibility and implementation: 2017

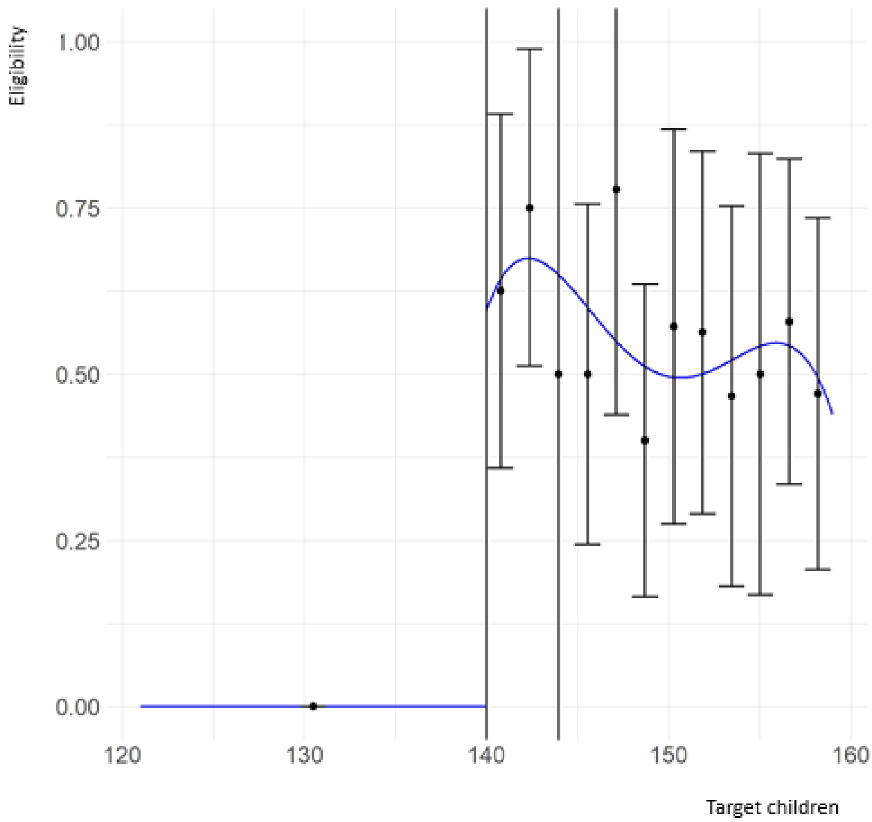


Figure 8: Program eligibility and implementation: 2018

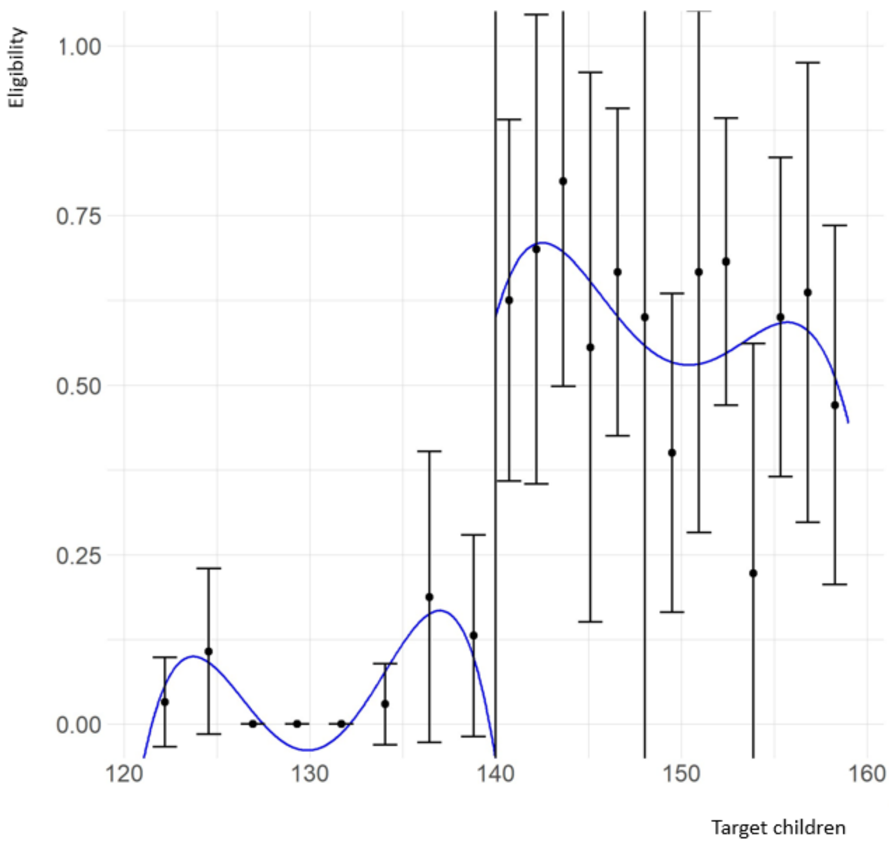


Figure 9: Program eligibility and implementation: 2019

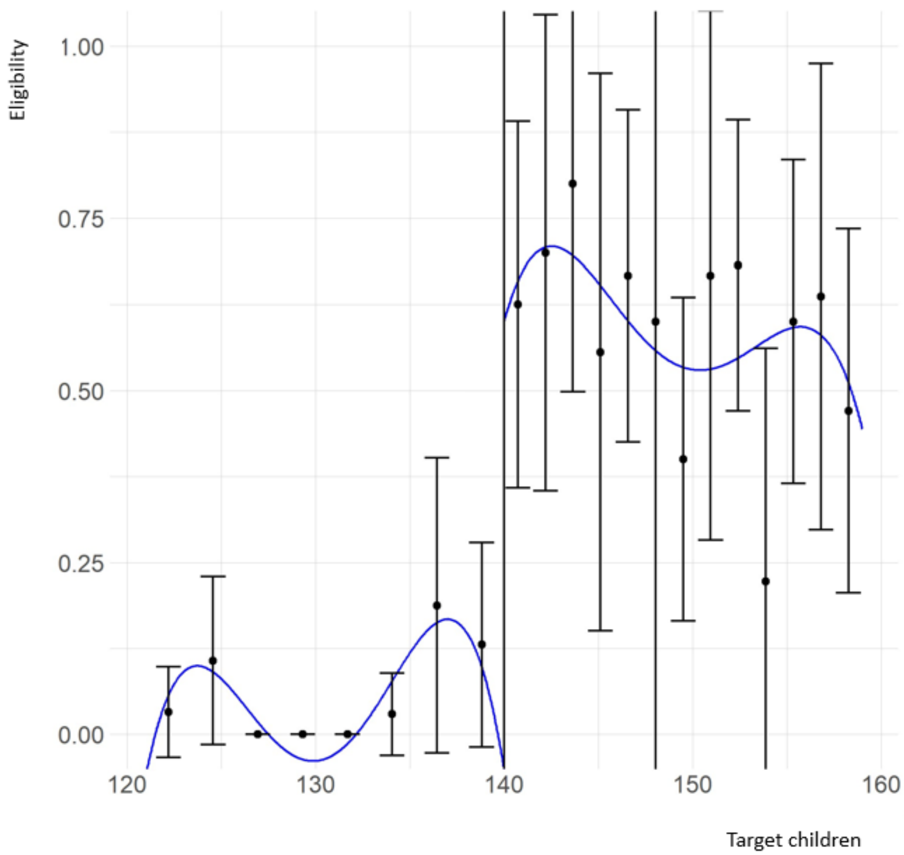


Figure 10: Program eligibility and implementation: 2020

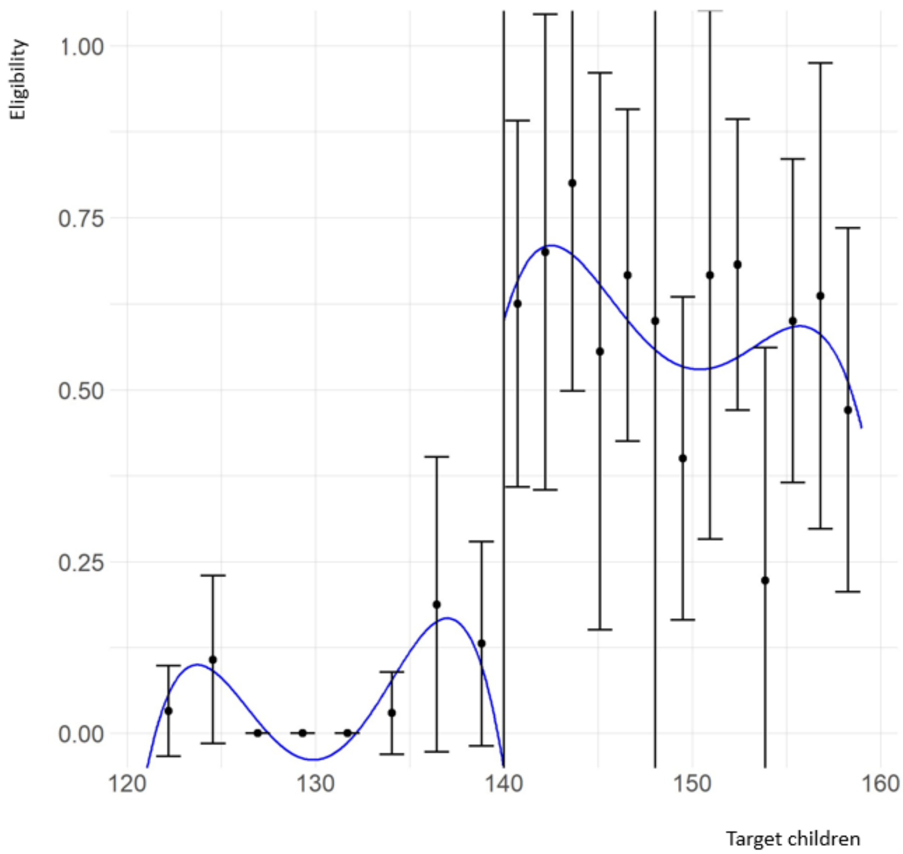


Figure 11: Event study: Number of children visited

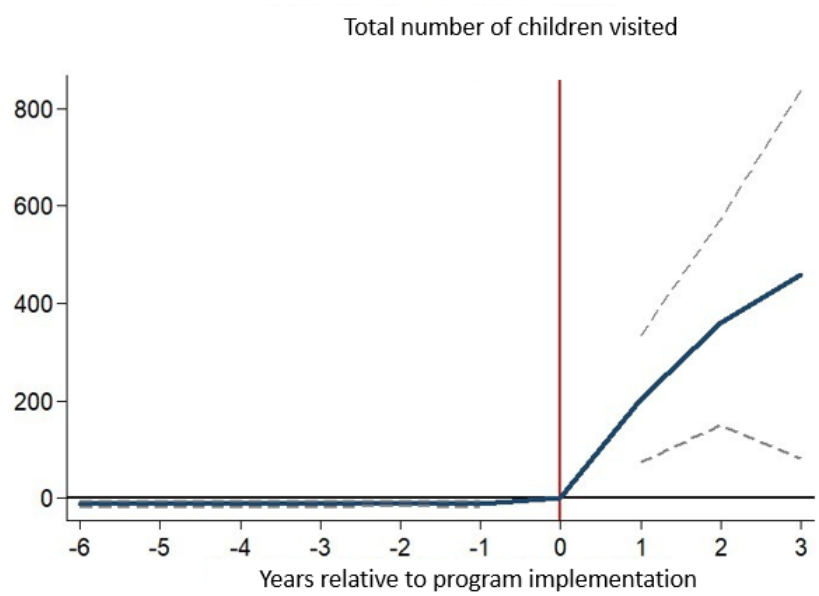


Figure 12: Eligibility discontinuity: Number of children visited

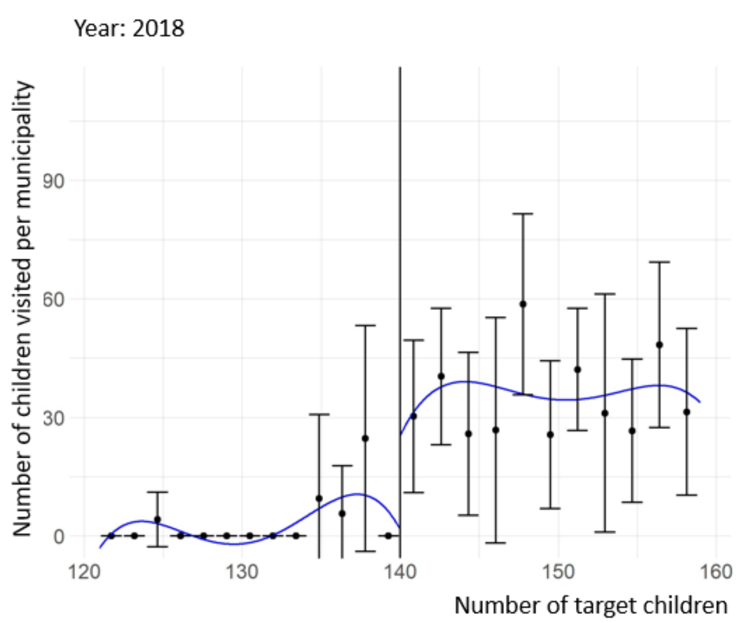


Figure 13: Municipal population and number of visits financed

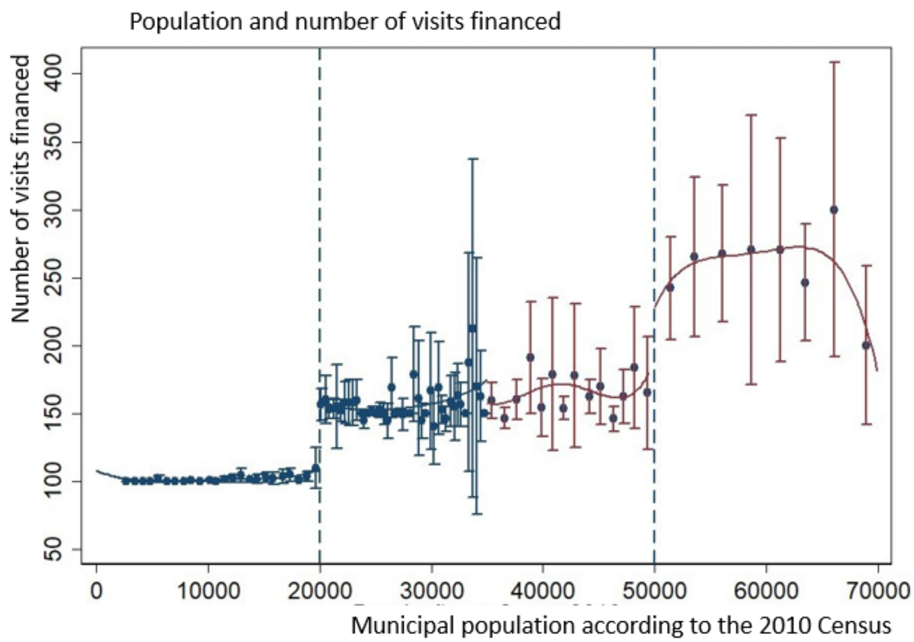


Figure 14: Eligibility discontinuity and antenatal visits

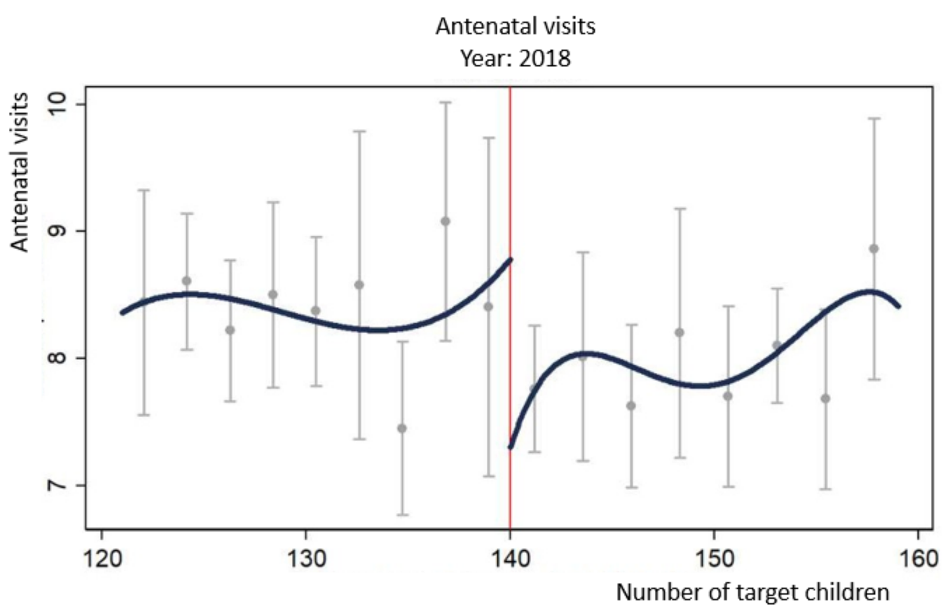


Figure 15: Financing discontinuity and antenatal visits

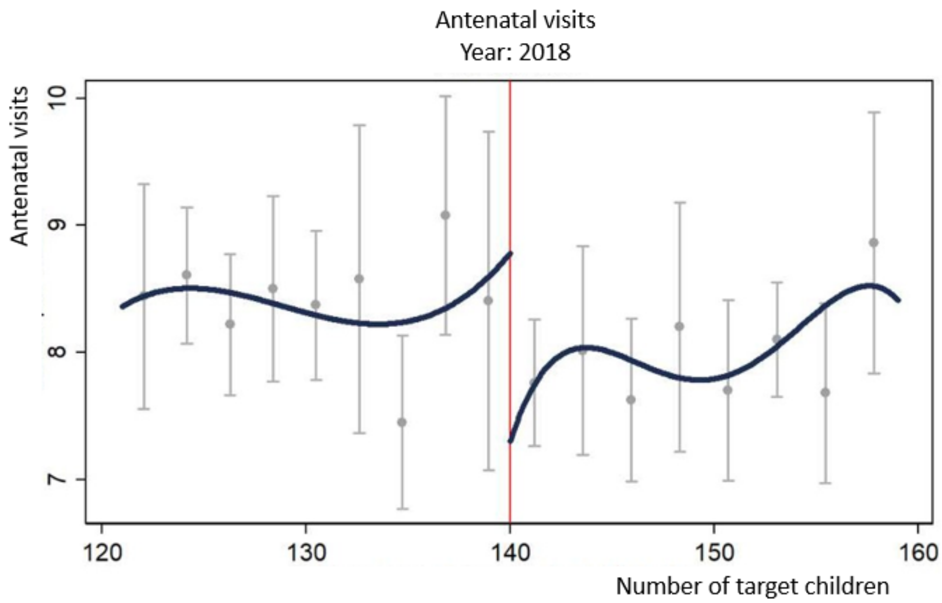


Figure 16: Eligibility discontinuity and birth weight

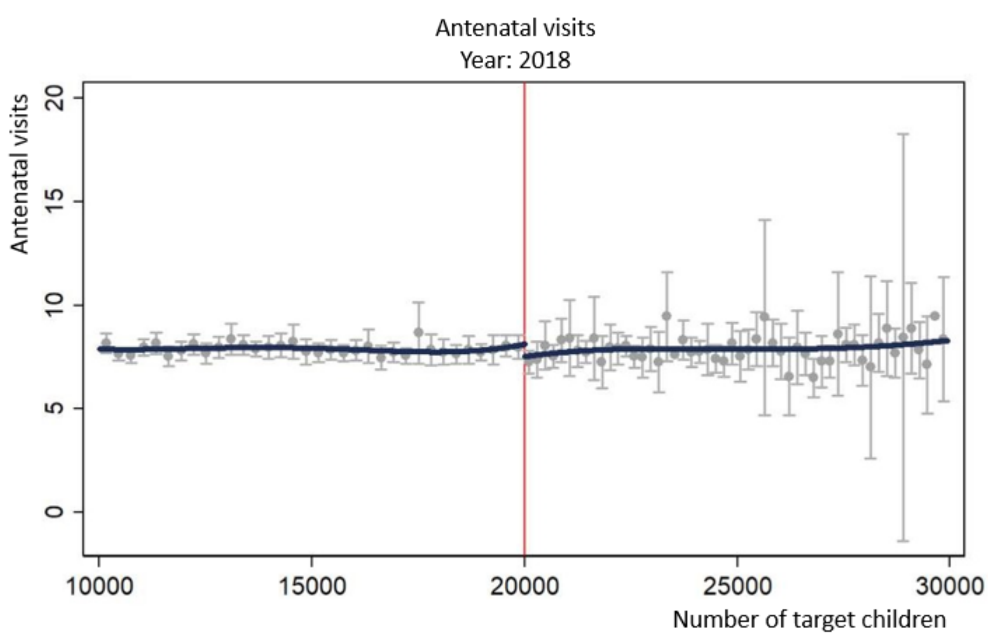


Figure 17: Financing discontinuity and birth weight

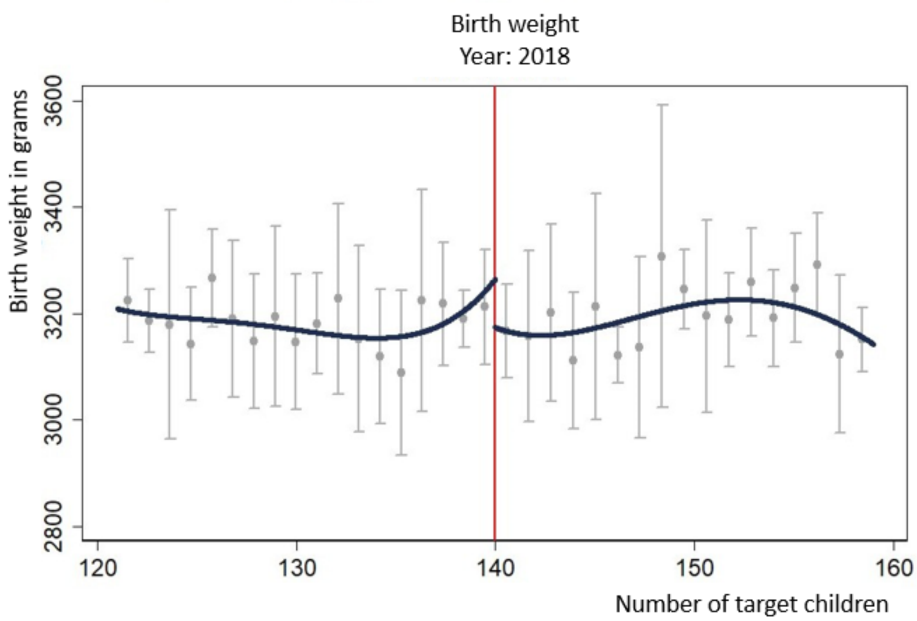


Figure 18: Eligibility discontinuity and adequate weight for age

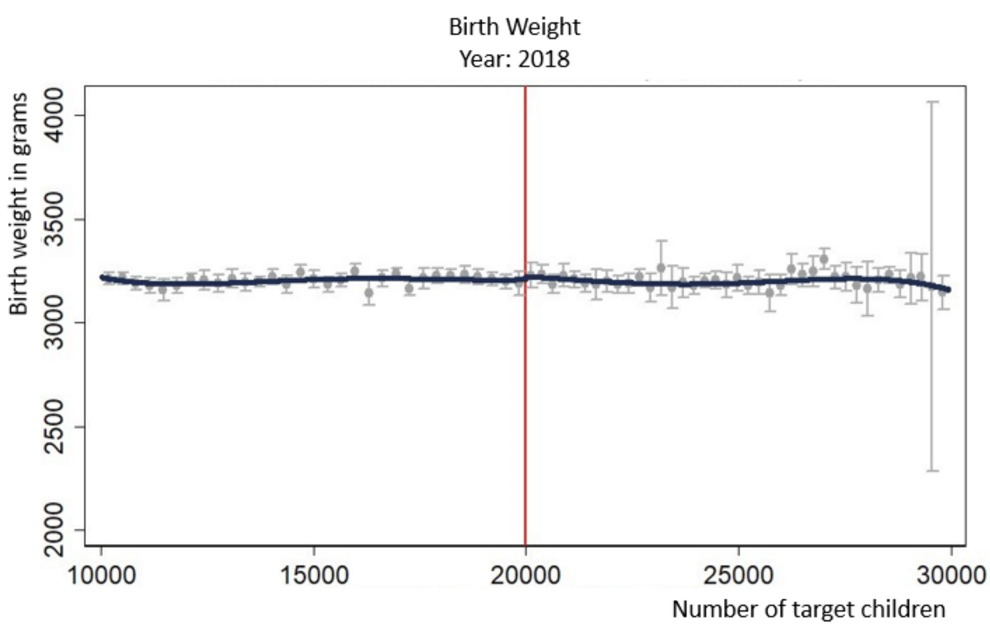


Figure 19: Financing discontinuity and adequate weight for age

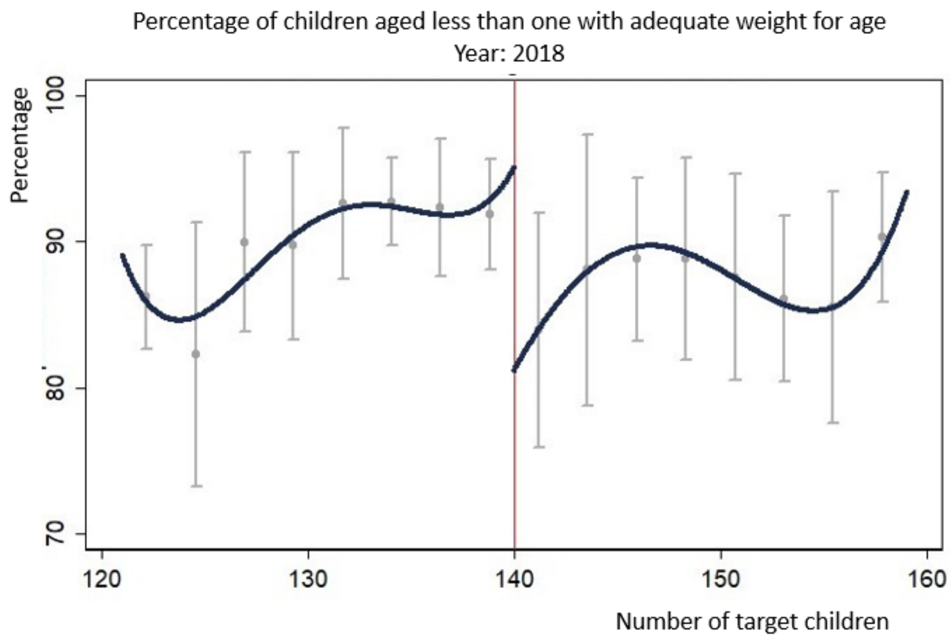


Figure 20: Eligibility discontinuity and adequate height for age

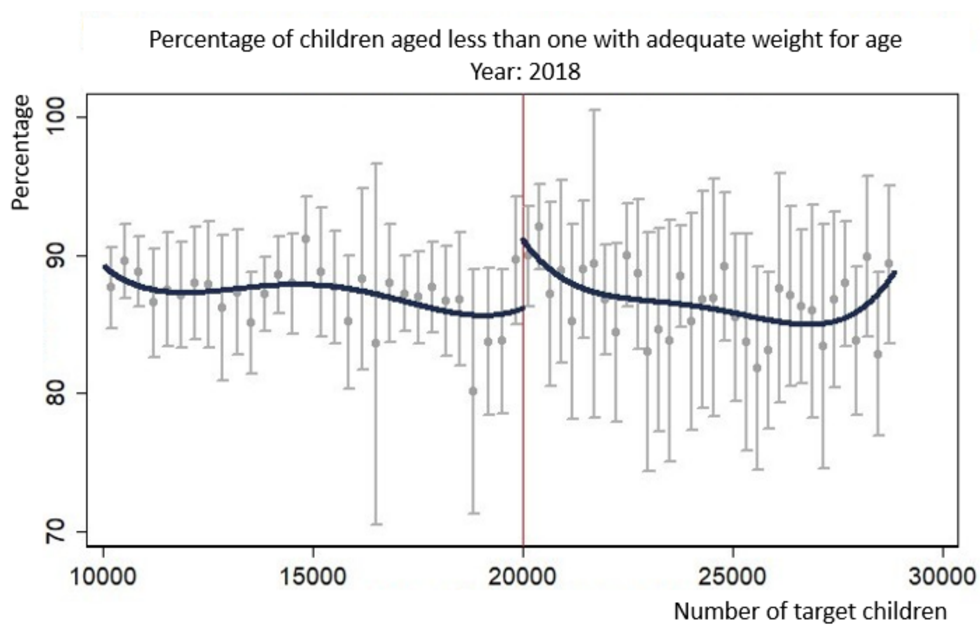
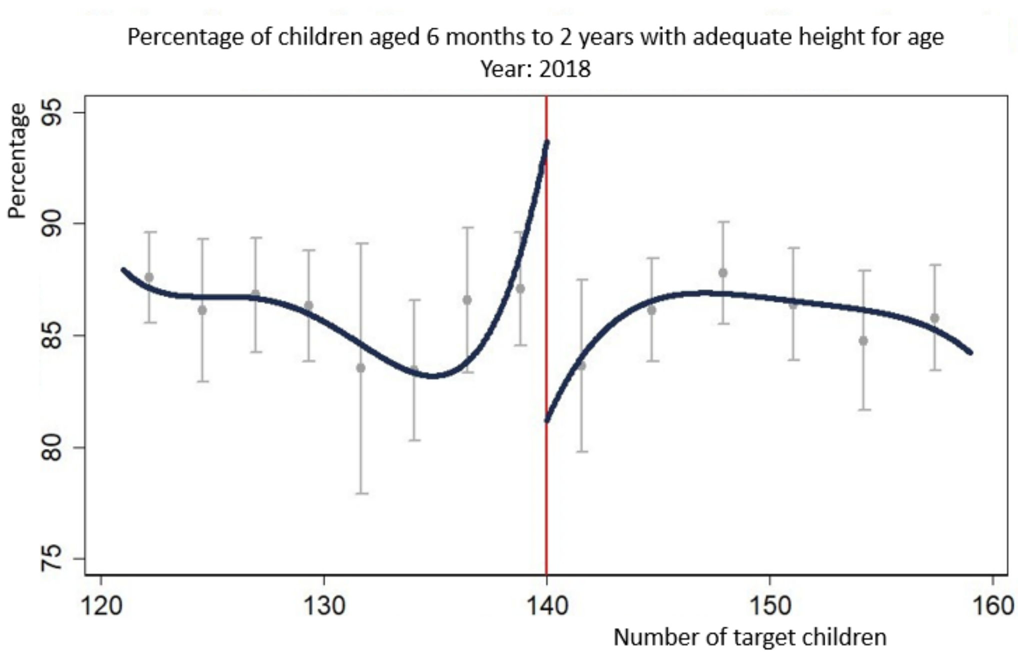


Figure 21: Financing discontinuity and adequate height for age



9 Tables

Table 1: PCF adherence by state

UF	Did not join	Joined	Total
Rondônia	20	32	52
Acre	2	20	22
Amazonas	3	59	62
Roraima	0	15	15
Pará	15	129	144
Amapá	3	13	16
Tocantins	81	58	139
Maranhão	10	207	217
Piauí	23	201	224
Ceará	0	184	184
Rio Grande do Norte	24	143	167
Paraíba	30	193	223
Pernambuco	7	178	185
Alagoas	2	100	102
Sergipe	6	69	75
Bahia	60	357	417
Minas Gerais	534	319	853
Espírito Santo	51	27	78
Rio de Janeiro	44	48	92
São Paulo	414	231	645
Paraná	327	72	399
Santa Catarina	283	12	295
Rio Grande do Sul	408	89	497
Mato Grosso do Sul	47	32	79
Mato Grosso	90	51	141
Goiás	118	128	246
Distrito Federal	0	1	1
Total	2,602	2,968	5,570

Table 2: Balance of municipal characteristics between joining and non joining municipalities

Variable	(1) Did not join PCF	(2) Joined PCF	Difference t-test (1)-(2)
Municipality population in 2010	382000	1690000	-1310000
	[89833.098]	[896000]	
Municipal Human Development Index - 2010	0.74	0.71	0.035***
	[0.004]	[0.011]	
GDP per capita (R$)	33674.01	26790.882	6883.132**
	[1231.930]	[2982.318]	
Collection: net taxes per capita	4.999	4.089	0.91
	[0.341]	[0.685]	
Primary school pass rate (1st to 5th grade)	94.293	91.794	2.499***
	[0.363]	[0.610]	
Primary school quality index (Ideb) (1st to 5th grade)	5.821	5.257	0.564***
	[0.056]	[0.086]	
Total public health facilities per thousand inhabitants	0.279	0.27	0.009
	[0.013]	[0.026]	
Hospitalization beds per thousand inhabitants	2.348	2.22	0.128
	[0.109]	[0.083]	
(0 to 6 months old) Percentage of children with low height for age	7.806	7.16	0.646

	[0.293]	[0.449]	
(6 months to 2 years old) Percentage of children with low height for age	7.696	8.887	-1.191***
	[0.142]	[0.400]	
(2 years to 5 years old) Percentage of children with low height for age	5.494	7.266	-1.772***
	[0.150]	[0.561]	
(0 to 6 months old) Percentage of children with low weight for age	5.612	4.902	0.711*
	[0.266]	[0.259]	
(6 months to 2 years old) Percentage of children with low weight for age	2.447	2.656	-0.209
	[0.088]	[0.251]	
APGAR Index - 1 minute	8.33	8.256	0.074***
	[0.016]	[0.011]	
APGAR Index - 5 minutes	9.355	9.284	0.071**
	[0.021]	[0.020]	
Birth Weight	3167.779	3187.648	-19.868***
	[4.285]	[3.784]	
N	2597	2968	

Note: All missing data is treated as zero.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 3: Balance of municipal characteristics between joining and non joining municipalities, close to the eligibility threshold

Variable	(1) Did not join PCF	(2) Joined PCF	Difference t-test (1)-(2)
Municipality population in 2010	14543.198	15305.87	-762.672
	[1758.339]	[3662.461]	
Municipal Human Development Index - 2010	0.702	0.692	0.01
	[0.006]	[0.008]	
GDP per capita (R$)	22743.702	23428.568	-684.865
	[1120.878]	[2439.199]	
Collection: net taxes per capita	2.076	3.694	-1.619
	[0.216]	[1.616]	
Primary school pass rate (1st to 5th grade)	95.678	95.376	0.302
	[0.355]	[0.539]	
Primary school quality index (Ideb) (1st to 5th grade)	5.929	5.847	0.083
	[0.072]	[0.104]	
Total public health facilities per thousand inhabitants	0.524	0.517	0.007
	[0.035]	[0.042]	
Hospitalization beds per thousand inhabitants	2.51	2.062	0.448
	[0.456]	[0.319]	
(0 to 6 months old) Percentage of children with low height for age	15.012	10.48	4.532***

	[0.951]	[0.798]	
(6 months to 2 years old) Percentage of children with low height for age	8.162	8.297	-0.135
	[0.505]	[0.573]	
(2 years to 5 years old) Percentage of children with low height for age	4.767	4.536	0.231
	[0.310]	[0.397]	
(0 to 6 months old) Percentage of children with low weight for age	8.91	5.807	3.103***
	[0.511]	[0.373]	
(6 months to 2 years old) Percentage of children with low weight for age	3.237	2.878	0.358
	[0.224]	[0.184]	
APGAR Index - 1 minute	8.416	8.299	0.117
	[0.047]	[0.056]	
APGAR Index - 5 minutes	9.515	9.387	0.128***
	[0.033]	[0.036]	
Birth Weight	3163.179	3175.531	-12.353
	[11.634]	[14.835]	
N	118	93	

Note: All missing data is treated as zero.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 4: DID: Impacts on pregnancy and childbirth outcomes

	Death of the fetus	Preventable deaths through care at childbirth	Prenatal visits	Cesarean delivery	Physician Assisted Delivery	Midwife-assisted Delivery
Implementation year	0.537	1.744	0.184	-0.001	0.003	-0.002***
	(0.743)	(2.375)	(0.293)	(0.003)	(0.004)	(0.001)
Send year of implementation	0.238	-0.947	0.162	0.005	0.002	-0.002***
	(0.750)	(1.431)	(0.374)	(0.004)	(0.007)	(0.001)
Observations	43,429	43,317	38,790	59,643	32,793	32,793
R-2	0.175	0.976	0.311	0.824	0.901	0.888
Mean in 2016	2.381	0.836	7.765	0.473	0.922	0.0139
Std in 2016	8.870	3.786	2.293	0.213	0.159	0.0643

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 5: RDD: Impacts on pregnancy and childbirth outcomes

	Death of the fetus	Preventable deaths through care at childbirth	Prenatal visits	Cesarean delivery	Physician Assisted Delivery	Midwife-assisted Delivery
RDD estimate: Eligibility	0.177	-0.190	-1.283	0.096	-0.063	0.040
(140 target children)	(0.564)	(0.127)	(1.575)	(0.092)	(0.057)	(0.046)
Observations	217	246	374	374	374	374
Mean in 2018	2.051	0.163	8.218	0.531	0.949	0.00276
Std in 2018	8.294	0.412	1.876	0.203	0.124	0.0325
RDD estimate: Funding	-0.543	0.211*	-0.141	-0.032	-0.005	0.001
(20k inhabitants)	(0.609)	(0.127)	(0.236)	(0.030)	(0.036)	(0.017)
Observations	1,502	1,611	1,726	1,726	1,726	1,726
Mean in 2018	2.082	0.387	7.889	0.463	0.890	0.0137
Std in 2018	5.302	0.686	1.762	0.189	0.175	0.0607

Robust standard errors in parenthesis.
***, **, and * indicate significance at 1%, 5% and 10%.

Table 6: DID: impact on birth and neonatal outcomes

Variables	(1) Birth weight (grams)	(2) APGAR index (1 min)	(3) APGAR index (5 min)	(4) Deaths 0 to 6 days of life	(5) Deaths 7 to 27 days of life
Implementation year	-2.110	0.008	-0.001	-3.214	2.178
	(2.442)	(0.007)	(0.007)	(3.157)	(2.184)
Send year of implementation	1.922	0.005	-0.015*	-11.724*	2.297
	(2.975)	(0.011)	(0.009)	(6.446)	(3.071)
Observations	59,643	59,591	59,590	47,736	47,736
R-2	0.523	0.648	0.703	0.998	0.996
Mean in 2016	3193	8.287	9.363	4.405	1.413
Std in 2016	168.6	0.472	0.388	19.89	7.478

Robust standard errors in parenthesis.
***, **, and * indicate significance at 1%, 5% and 10%.

Table 7: RDD: impact on birth and neonatal outcomes

Variables	(1) Birth weight (grams)	(2) APGAR index (1 min)	(3) APGAR index (5 min)	(4) Deaths 0 to 6 days of life	(5) Deaths 7 to 27 days of life
RDD estimate: Eligibility	-70.043	-0.009	0.004	0.457	0.154
(140 target children)	(54.992)	(0.235)	(0.139)	(0.386)	(0.137)
Observations	374	374	374	246	246
Mean in 2018	3186	8.356	9.405	0.911	0.293
Std in 2018	180.8	0.480	0.404	0.935	0.568
RDD estimate: Funding	14.863	-0.031	0.090	0.290	0.273*
(20k inhabitants)	(26.475)	(0.109)	(0.063)	(0.331)	(0.151)
Observations	1,726	1,726	1,726	1,611	1,611
Mean in 2018	3201	8.292	9.363	1.881	0.528
Std in 2018	117.8	0.387	0.321	1.778	0.779

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 8: DID: Impacts on health outcomes of children aged 0-1 year

Variables	(1) Percentage of children 0-1 year old with appropriate weight for their age	(2) Percentage of children 0-1 years old with appropriate height for their age	(3) Number of hospital admissions of children aged 0 to 1 year	(4) Total cost (thousand R$) of hospitalizations of children aged 0 to 1 year	(5) Total number of days in the hospital children aged 0 to 1 year	(6) Deaths of children aged 0 to 1 year
Implementation year	-4.739	1.541	42.469	-115,431.868	-601.023	-5.682*
	(4.723)	(1.680)	(123.340)	(142,808.595)	(791.616)	(3.344)
Send year of implementation	1.374	1.056	227.270	345,562.365	-131.575	-6.379
	(1.990)	(1.800)	(231.243)	(360,574.406)	(1,463.650)	(4.674)
Third year of implementation	1.763	-0.048	68.312	153,961.994	-2,464.607	-20.434*
	(2.523)	(2.082)	(322.164)	(348,102.107)	(2,435.046)	(10.722)
Observations	28,415	44,946	48,997	48,997	48,997	48,997
R-2	0.373	0.218	0.995	0.998	0.996	0.994

Mean in 2016	86.53	90.07	104.7	222585	851.2	3.381
Std in 2016	11.72	11.28	682.5	1.387e+06	5854	19.23

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 9: RDD: Impacts on health outcomes of children aged 0-1 year

Variables	(1) Percentage of children 0-1 year old with appropriate weight for their age	(2) Percentage of children 0-1 years old with appropriate height for their age	(3) Number of hospital admissions of children aged 0 to 1 year	(4) Total cost (thousand R$) of hospitalizations of children aged 0 to 1 year	(5) Total number of days in the hospital children aged 0 to 1 year	(6) Deaths of children aged 0 to 1 year
RDD estimate: Eligibility	-11.914**	-8.324	-9.898*	-8,170.090	-61.171	0.022
(140 target children)	(5.768)	(5.801)	(5.335)	(18,059.981)	(58.499)	(0.343)
Observations	329	331	372	372	372	372
Mean in 2018	88.53	89.59	21.68	58407	170.8	0.618
Std in 2018	13.17	13.31	21.40	70223	176.9	0.940
RDD estimate: Funding	4.500**	0.604	-0.125	-13,984.661	-23.744	0.284
(20k inhabitants)	(2.114)	(1.915)	(4.209)	(13,629.142)	(36.225)	(0.277)

Observations	1,627	1,641	1,722	1,722	1,722	1,722
Mean in 2018	87.19	89.72	47.30	104240	354.9	1.586
Std in 2018	11.96	11.23	30.31	80844	223.4	1.553

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 10: DID: Impacts on health outcomes of children aged up to 4 years old

	(1) Percentage of children aged 6 months to 2 years old with appropriate weight for age	(2) percentage of children age 6 months to 2 years old appropriate height for age	(3) percentage aged 2 to 5 aged 2 to 5 years with appropriate weight for age	(4) percentage aged 2 to 5 aged 2 to 5 years with appropriate height for age	(5) Hospitalizations of children aged 1 to 4 years old	(6) Total cost of hospitalizations of children aged 1 to 4 years old
Implementation year	-0.260	0.929	-1.211	-2.039	-122.228	-93,187.540
	(0.507)	(1.527)	(1.528)	(1.943)	(125.452)	(105,537.844)
Send year of implementation	4.517	5.278	0.108	-0.095	-104.837	-9,496.201
	(4.565)	(5.946)	(0.730)	(1.061)	(173.944)	(49,309.780)
Third year of implementation	5.448	6.624	0.870	0.664	-108.683	142,759.902
	(5.483)	(6.941)	(0.688)	(1.283)	(242.966)	(121,139.929)

Observations	64,446	53,614	65,039	65,045	48,869	48,869
R-2	0.346	0.494	0.417	0.555	0.997	0.998
Mean in 2016	86.13	84.91	86.66	89.54	96.26	74042
Std in 2016	7.397	8.851	6.230	6.743	514.5	463403

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.

Table 11: RDD: Impacts on health outcomes of children aged up to 4 years old

	(1)	(2)	(3)	(4)	(5)	(6)
	Percentage of children aged 6 months to 2 years old with appropriate weight for age	Percentage of children aged 6 months to 2 years old with appropriate height for age	Percentage of children aged aged 2 to 5 years old with appropriate weight for age	Percentage of children aged aged 2 to 5 years old with appropriate height for age	Hospitalizations of children aged 1 to 4 years old	Total cost of hospitalizations of children aged 1 to 4 years old
RDD estimate: Eligibility	-0.084	-7.183**	-2.030	-1.818	-3.002	-3,602.784
(140 target children)	(1.209)	(3.317)	(1.901)	(2.254)	(6.676)	(6,244.507)
Observations	374	374	374	374	367	367
Mean in 2018	89.72	85.80	89.40	91.52	21.63	17310
Std in 2018	4.642	7.745	3.866	5.038	23.70	22636
RDD estimate: Funding	-0.374	4.071***	-0.691	0.767	11.279	1,901.26
(20k inhabitants)	(1.005)	(1.331)	(0.589)	(0.788)	(10.003)	(8241.859)

Observations	1,726	1,726	1,726	1,726	1,722	1,722
Mean in 2018	88.15	83.49	88.73	89.7	56.97	41704
Std in 2018	4.916	8.133	4.043	5.637	57.3	42407

Robust standard errors in parenthesis.

***, **, and * indicate significance at 1%, 5% and 10%.