Effects on Fertility of The Brazilian Cash Transfer Program: Evidence from a Regression Discontinuity Approach

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Abstract

The program *Bolsa Família* is a pillar of Brazil's welfare system. However, it is possible that the program encouraged beneficiaries to have more children. Using federal data and the eligibility rule, we propose a regression discontinuity to verify the program's effect on fertility outcomes. Problems associated with the data such as manipulation and attrition are solved by using novel procedures found in the literature. We found an effect on birth spacing but not on fertility rates. This study complements the literature in regard to cash transfers and fertility outcomes, and empirical evidence for the quantity-quality trade-off in fertility decisions.

Keywords: Bolsa Família, fertility, cash transfer, birth spacing

JEL Codes: J13, J18, I38, O12.

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1 Introduction

Since the pioneering *Progresa* program in Mexico in the late 1990s, cash transfers programs have been extensively used in the developing world, especially in Latin America, as a way to improve human capital accumulation and to break inter-generational cycles of poverty. In general, those programs have improved educational and health outcomes and, long-term human capital accumulation, and have promoted short-term reductions in poverty (Lagarde et al., 2007; Fiszbein et al., 2009, for a review). However, they can also have impacts on fertility decisions. An early theoretical literature suggests, there may be a substitute relation between quantity and quality of children in fertility decisions (Becker and Lewis, 1973; Cochrane, 1975; Schultz, 1997; Stecklov et al., 2007) and, depending on its design, a cash transfer program can affect this trade-off. A conditional cash transfer program imposes minimal school attendance and periodic visits to health officials, thus reducing the child "quality" price. An unconditional cash transfer, as the name suggests, does not have any kind of condition attached to the grant, but can still affect fertility decisions through an income effect. Here, we investigate whether the Brazilian cash transfer program *Bolsa Família* (BF), through both its conditional and unconditional components, encouraged beneficiary women to have children during the 2012-2015 period and whether it affected birth spacing, using the 2011 BF' allocation rules. This research evaluates exogenous variation in fertility by employing the eligibility rule used by the Brazilian government to authorize BF' payments. The data, however, is self-reported and self-updated by current and potential beneficiaries, so it has potential measurement errors as results of heaping, attrition and manipulation. Income is subject to heaping so manipulation tests may falsely find evidence of manipulation in heaping values (Barreca et al., 2016). Therefore, we applied our analysis on non-heaping observations, which is similar to a donut regression discontinuity approach. This solves the problem of heaping but not of attrition. By applying a non-parametric approach suggested by Gerard et al. (2020) in a subset of the observations, it is possible to identify bounds on the treatment effects, even under attrition of the data. We found no effect on fertility rates, but a large and significant effect on reducing birth spacing was found. While RD estimations for fertility rates are stable (i.e., we are able to identify the ATE through the LATE), the birth spacing findings are not. This study is divided as follows: section 2 discusses the economic theory behind cash transfers and fertility and gives a background on BF. Section 3 describes the data and its measurement problems. Sections 4 and 5 provide the empirical framework and results, and sections 6 and 7 the robustness and external validity of our results. Finally, section 8 concludes the paper.

2 Background

2.1 Income, Fertility and Cash Transfers

The relation between family size and income is widely studied in the social sciences. It is observed that within a country there is a negative association between family size and children's schooling (Hanushek, 1992; Gupta and da Costa Leite, 1999; Cleland, 2002; Schultz, 2005). In the macroeconomic literature, the negative relationship between income and population growth in modern history is extensively documented and analyzed (Barro and Becker, 1989; Robinson, 1997; Galor and Weil, 2000; Doepke, 2004; Angrist et al., 2010). To explain such observations, many mechanisms have been proposed, such as the trade-off between child quality and quantity (Becker et al., 1990; Moav, 2004); higher wages for women, which increase the opportunity costs of child rearing (Galor and Weil, 1996; Momota, 2000); the change of net flow of transfers from parents to children as an economy grows (Caldwell, 1976); and the technological progress that brings an increase in the return of education, thus triggering demographic transition (Galor and Weil, 2000). In general these macroeconomic studies have supported the view that, over several generations, large families reduce living standards. However, it is the traditional Becker and Lewis model (1973), which also gave birth to an extensive related literature (Cochrane, 1975; Robinson, 1997), that provides the basic mechanism for policy makers to reduce family size within a lifetime to foster economic development (Angrist et al., 2010). Those policies include cash transfer programs. Basically, in the Becker and Lewis (1973) framework, parents choose both quantity and quality of children but there is a substitution relation between them, and the consumption of one of these goods increases the shadow price of the other. Depending on its design, a cash transfer program can affect this trade-off (Becker and Lewis, 1973; Stecklov et al., 2007). Conditional cash transfers (CCTs) and unconditional cash transfers (UCTs) distribute cash to poor households to alleviate their income restraints, but the difference is that UCTs give cash with no strings attached, while CCT programs give cash contingent on certain behaviors that improve the household's human capital accumulation, such as minimal school attendance by the children and periodic visits to health centers (Lagarde et al., 2007; Fiszbein et al., 2009; Baird et al., 2014). The main argument for UCTs is that the key constraint for poor people is simply a lack of money. For CCTs, the argument involves the existence of market failures that cause suboptimal levels of education and also the view that redistribution policies should encourage socially desirable behaviors so they are more palatable to taxpayers (Baird et al., 2011, 2014). In summary, UCTs produce an income effect, and CCTs add an alteration to the relative price of schooling and health services. The final result is ambiguous: with more income, parents may improve their children's education and consume more health services (provided those are normal goods), but at same time, they may want another child. For instance, Willis (1973) shows that a decrease in the quality price may increase fertility if quality and quantity are complements. Further, CCTs and UCTs usually transfer an amount per children, so additional children can be perceived by the recipient as an extra source of income. The trade-off mechanism, however, has seen mixed evidence in empirical studies. On one hand, Angrist et al. (2010) found no evidence of the mechanism in Israel, and signs of increased fertility were not encountered in the majority of CCTs and UCTs analyzed in the developing world (Fiszbein et al., 2009), such as those implemented in Mexico or Nicaragua (Schultz, 2004; Todd and Wolpin, 2006; Stecklov et al., 2007; Todd et al., 2012), South Africa (Bor, 2013), Zambia (Palermo et al., 2016), Brazil (da Rocha, 2010; Signorini et al., 2011; Simões and Soares, 2012; Olson et al., 2019), or Argentina (Garganta et al., 2017), the exception being Honduras (Stecklov et al., 2007). On the other hand, pro-natal policies in some developed countries that aim to reduce child rearing costs seem to increase fertility, as found by Hoem (1990) in Sweden, Laroque and Salanié (2008) in France, Cohen et al. (2013) in Israel, and Zhang et al. (1994); Milligan (2005) in Canada. Although differences between programs may explain the findings,[1] it is possible that positive effects may be capturing a confounding effect. Alleviating restraints may anticipate births that parents were planning in the future, so reduced-form evidence can mistakenly indicate an effect on fertility. Many studies have a time period that restrains the possibility of capturing reductions in birth spacing, and thus an analysis that occurs within a restricted period might not fully capture lifetime impacts on fertility[2] (Todd et al., 2012; Kim, 2014). Some possibilities for controlling for this effect are larger time periods (Zhang et al., 1994; Kim, 2014), right censoring models (Todd et al., 2012), and dynamic structural models (Todd and Wolpin, 2006). Most of these studies points to no effects of financial incentives on fertility (Todd and Wolpin, 2006; Todd et al., 2012; Kim, 2014).

This paper contributes to the literature in the following ways. First, the available data allows us to analyze fertility outcomes as births occurring in a relatively large window, and also the timing between them. Analyzing birth spacing allow us to identify possible confounding effects in the fertility outcomes, and is also a point of policy interest. Very short or very long periods between births may negatively affect both the health of the newborn child and the mother, but in general, longer periods improve their health. That is, the effect of large birth spacing on health is a parabola (Conde-Agudelo et al., 2006, 2007; DaVanzo et al., 2007; Conde-Agudelo et al., 2012; Todd et al., 2012; Wendt et al., 2012). Furthermore, spacing decisions may be affected by economic context. On one hand, greater spacing may be optimal in smoothing consumption

[1] For instance, the financial incentives might be relatively larger in developed economies.

[2] Conditional cash transfer programs such as those in Mexico, Honduras and Nicaragua were analyzed during the controlled trial phase, which lasts for no more than a couple of years.

where credit markets are imperfect, but on the other, spreading out births can be costly when economies of scale exist in raising children (Heckman and Willis, 1976; Schultz, 1997; Todd et al., 2012).

Second, as opposed to comparable programs, BF did not start with a controlled trial, which makes effect analysis difficult due to omitted variable bias. Adding to the literature studying the Brazilian case,[3] we use data from the federal institution responsible for BF's allocation so that we can direct observe beneficiaries and non-beneficiaries. Additionally, we use a regression discontinuity design (RDD), a quasi-experimental approach that is the closest to a randomized experiment in terms of reliability in estimating unbiased treatment effects (Lee and Lemieux, 2010, 2014). Third, the quality-quantity trade-off is a concept that still guides both policy makers and the general population in cash transfer discussions, particularly in the case of unintended fertility (Todd and Wolpin, 2006; Stecklov et al., 2007; Angrist et al., 2010; Baird et al., 2014; Palermo et al., 2016). Additional evidence is important for this debate, since it can strengthen or weaken arguments of cash transfers' effects on fertility. The next section presents characteristics of the program and the data.

2.2 The Brazilian Program *Bolsa Família*

Bolsa Família was created in 2003, based on the merge of four earlier smaller-scale poverty-alleviating programs. Within three years, BF covered around 11 million families, becoming the largest CCT program in the world at that time. In 2009, the program was authorized to expand to around 12.4 million families (Lindert et al., 2007; Soares et al., 2010). For a family to be eligible, it must have a *per capita* income below a certain cutoff, which the government usually changes on a yearly basis, as shown in Table 1. Beneficiaries can then receive a *basic* grant and also a *variable* grant that depends on the number of children and their ages. Eligibility for each type of grant is divided into two categories based on income: *extreme poverty* and *poverty*. Only those in the first category receive the basic amount. The basic grant is unconditional, while the variable benefits have some sort of condition.

From 2011 to 2015, a family could claim up to five variable benefits. These variable benefits fell into three categories: pregnant, nursing, and 0–15 years old. The pregnancy benefit is given to families with pregnant women aged 14 to 44 years. The nursing benefit is given to families with infants from birth to six months of age, with each child corresponding to one benefit. Finally, the 0-15 years old benefit is given to families with children under the age of 15. Not subject to the five-benefit cap, there is a benefit for older teenagers,

[3] da Rocha (2010); Signorini et al. (2011); Simões and Soares (2012); Olson et al. (2019) used cross-sectional data provided by the Brazilian IBGE (National Institute of Geography and Statistics) which can be used to indirectly extract treatment and control groups.

a *benefício variável ao adolescente* (BVJ). Families with teenagers aged 15–17 years are eligible for at most two BVJs. In 2012, the government added a benefit to help families overcome poverty, called *superação da extrema pobreza* (BSP), given to families who fell into the extreme poverty category. This supplementary benefit provides the necessary amount to the family so that they have a per capita income at least above the extreme poverty line. Table 2 shows the evolution of the benefit values. Conditional cash transfers have a fixed number of installments, based on the children's age, so beneficiary families keep receiving benefits even if they are no longer eligible through increased income. To the best knowledge of the author, there are no fixed criteria for the number of UCT installments.

Conditions are defined by health and educational criteria. For health, children up to seven years old are required to have up-to-date vaccinations, and pregnant women must have regular medical check-ups and prenatal examinations. Children and teenagers must be enrolled in school and have a minimum attendance of 85% for those under 15 years old, and a minimum of 75% for those between 15 and 17 (MDS, 2011, 2015).

Since the rule used to allocate families between treatment (receiving a grant) and control groups is based on income thresholds, a regression discontinuity design (RDD) is appropriate to verify treatment effects (Lee and Lemieux, 2010). Using the allocation of beneficiaries in 2011, we can use the RDD to analyze fertility outcomes in 2015. This large window of four years is useful to reduce the possibility of a confounding effect of birth spacing, for example, of BF affecting how late beneficiaries will have an additional child (Todd et al., 2012). The next section discusses how our database was built, and the measurement problems present in the data that might invalidate identification on a RDD.

3 Data

3.1 *Bolsa Família* Registration

Family eligibility is determined based on household registry data collected and transmitted into a central database maintained by the Ministry for Agrarian and Social Development (MDS). Data collection and entry is decentralized to the municipalities, and database consolidation and management is controlled by the federal bank Caixa, which is contracted by the MDS via a performance-based contract (Lindert et al., 2007; MDS, 2015). Caixa, as the operating agent, is responsible for the payments. Finally, eligibility determination is centralized by MDS, which then establishes the monthly beneficiary payroll. Each municipality has an estimate of how many families in its territory should be eligible to become beneficiaries. Those estimates are calculated by the MDS, which uses information from census and annual demographic data from IBGE.

The Ministry can also utilize inputs from municipalities. Those calculations are open to the public (MDS, 2015). The order of priority for the program is as follows.

I. Priority families include indigenous groups, families in child labor situation, or families in insalubrious-working conditions.

II. Other families are ordered by per-capita income and number of children. That is, families with lower per capita income and more children are more successful in becoming beneficiaries.

Families fill out a standard federal questionnaire, which includes information on household composition, income, and living conditions. Families can register at a permanent site provided by the city hall, but municipalities can also sign people up at specific registration locations, provided they communicate this in advance to the local population (Lindert et al., 2007; MDS, 2015). The database that contains the responses is called *Cadúnico*, and is not only used by BF, but also by other social programs controlled by the government, at both federal and local levels.[4]

The federal government encourages municipalities to keep the information updated and as accurate as possible through financial incentives. For accuracy, the programs may cross validate Cadúnico information with other governmental data such as tax reports or official employment registries. These cross-validation procedures do not only occur during BF allocation, but also every other year to verify eligibility. Many official databases, however, have limited use, since it is difficult to find information about the poorest, which is BF's target. For this study, it does appear that fraud or non-eligibility is not a major concern. In 2016, the government reported that a large cross-validation process cancelled only 4% of the benefits, and 3% of beneficiaries were called to update their information.[5]

We use the 2011 Cadúnico database in conjunction with a second database that contains BF payment information, provided by Caixa, to determine BF eligibility, and the 2015 Cadúnico to analyze the outcomes. The Cadúnico databases are only available for researchers upon request to the MDS, whereas the Caixa data is open to the public. Each person in the Cadúnico and Caixa databases is allocated a unique identifier, called *número de identificação social* (NIS).

[4]A brief description of these programs are in table A1.

[5]There is reason to believe that data reliability was similar across the years in our study. Because BF is a major program, it has reasonable coverage by official channels and the media, and a high rate of fraud would have a high chance of being reported.

3.2 Database Construction

A *Cadúnico* database of a certain year is a cross-sectional file that stores information on every member of every registered family for that year. For subsequent years, the Cadúnico keeps information that was not updated on the previous year. Each household has an unique identifier and every individual has a NIS number. The data includes date of birth, current and past education, race, income, income composition (e.g., work, pension, unemployment benefits, donations), general expenses, physical disabilities, an indicator for the head of the household, and the degree of kinship in relation to the head of the household (e.g., spouse, son/daughter,parent, etc). There is also household information such as number of rooms, number of bedrooms, and floor and wall material, as well as indicators of whether electricity, running water, a sewer and trash collecting are present and how they are supplied. Finally, there is registry information: the original year of registry and the last date of update. Tables 3 and 4 provide summary statistics of the main variables in the 2011 Cadúnico database, both for individuals and households. In 2011, Cadúnico had information on more than 75 million individuals - roughly one third of the Brazilian population. As it is, however, the dataset is not suitable for our analysis. We proceeded to clean the data in several steps. To reduce the impact of confounding programs, we considered only families that started their registry in 2011 (7.5% of individuals) and also families where the woman was the head of the family, since only women can be recipients for BF and also because we can only verify motherhood through the kinship variables, which are always in relation to the head of the household. This corresponds to 82% of all reported heads of household that started their registry on 2011. We then skewed the data to women of childbearing age (16-44 years old in 2011), with at least one child (for CCT eligibility) and at most 10 children (women with more than 10 children corresponds to less than 0.01% of the observations). To obtain fertility outcomes variables at the end of the period, we then matched those observations to the 2015 Cadúnico database. The match was guaranteed to be 100%: due to privacy concerns, if a household or individual information is deleted from a Cadúnico database in a given year, it is deleted from *all* years. In other words, if someone was in the database in 2011 and deleted in 2015, there is no way to verify the former since this observation is missing in both datasets. However, as will be clarified in the next sections, complete deletion is uncommon, and the MDS regularly maintains observations that are up to four years of age. Our measures for fertility were a dummy variable indicating whether or not the registered woman had at least one childbirth in the 2012-2015 period and, *conditional* on having a childbirth in the 2012-2015, the time period between births, or birth spacing. Instead of averaging the difference in birth periods for each woman, which depends on the current parity, we considered the birth spacing between the women's last birth and the one immediately previous, that is, the final observed parity birth spacing. Also, if a woman had more than one birth, we only considered the last one. As will be shown in the next sections, less than 10% of all women who had a birth in the analyzed period had more than one birth. To make birth spacing comparable across different parities and to increase our statistical power, we standardized the birth spacing (i.e., z-score birth spacing), measured in days and across parities, using the 2011 Cadúnico distribution as a basis.[6] The mean and standard deviation of birth spacing can be found in Table A2.

The Cadúnico database does not provide information on whether a family was a BF beneficiary. To verify whether or not a woman was a beneficiary, we used the Caixa database. The Caixa database provides monthly information on BF payments, starting in 2011. It includes the head of the household NIS, the municipality, and the monthly payment. We then matched the Caixa NISs with Cadúnico NISs to determine which observations in the Cadúnico database are BF beneficiaries. It was not possible to identify whether a certain transfer was unconditional, conditional or a combination of both, but we could verify possible combinations of UCT and CCT payments that could match each monthly amount. The complete procedure of benefit identification can be found in the Data Appendix, Table B1. The match for 2011 performed well: around 99.5% payments are uniquely identified as either CCT, UCT, or a combination. Finally, Table 5 provide summary statistics of this more manageable dataset, which includes Cadúnico and payment information.

It is important to mention that our measures of occurring births and birth spacing were not perfect. Since we relied mostly on self-registry, there were delays between the birth and the registry of the child, and child mortality was imperfectly captured, since a death and a birth within the analyzed period counts as no additional children. Our fertility measure was also not able to capture reproductive intentions, since it did not take into account miscarriages and stillbirths. Finally, one might be concerned about measurement error regarding reported births. The families themselves must provide proof of existence, and this can only be formally achieved through birth certificates or government provided IDs. Therefore, birth information, and consequently, fertility outcomes, are less prone to this kind of measurement error. The next section discusses other potential measurement concerns in our database.

3.3 Measurement Concerns: Update Pattern in Cadúnico

To remain eligible for social programs, registered families need to update their information on Cadúnico on a biennial basis. Thus, a family that did not update in a given year should not be seen as an attrition problem

[6]To obtain a better picture of Brazilian birth spacing pattern, we included all head of the household women registered up until 2011

if their registry was updated in the previous two years. One might question if there might be an attrition problem if information older than two years is erased from the data. Fortunately, the MDS is flexible in that regard: if a family does not update, their registry remains in the database for at least two more years. To reduce the risk of manipulation by municipalities, the only reasons that a registry can be excluded from the database are refusal to update information when solicited by an official agent, fraud, court decision, by family request, or death. There is a difference in how BF beneficiaries and non-beneficiaries update their information, however. For the observations in Table 5, while 90% of BF observations were up-to-date at 2015 (i.e., their last year of update was at least 2013), less than half of non-beneficiaries (46%) had their registry up-to-date. Particularly, beneficiaries have more incentives to update due to program monitoring, or due to fear of losing eligibility. That is, the marginal cost of updating is low compared to the financial benefits. It would seem that non-beneficiaries self-update if and only if they find that the marginal improvement in BF eligibility or payment is higher than the cost of self-reporting. This marginal improvement may be very small in the case that there is no new information to update, or in the case of minor events. Since additional children improves eligibility because it reduces per-capita income and is an eligibility tiebreaker, non-beneficiaries have a great incentive to update their registry in the event of childbirth. Roughly, non-updated fertility information is not a classical attrition problem, but rather indicates that the previous registry did not change in the period. Further, using only up-to-date information could have *underestimated* our results: beneficiaries update frequently, while non-beneficiaries could be updating only when they have a child. In light of what was presented, we used updated and non-updated fertility information in our main outcomes, which can give unbiased estimates based on a reasonable hypothesis: both beneficiaries and non-beneficiaries have, in the absence of treatment, the same incentives to keep information about the number of children up-to-date. For robustness checks, we will also present results using only up-to-date registry information, and we also applied a non-parametric approach presented by Gerard et al. (2020) that takes into account the difference in update patterns to estimate bounds on the treatment effects.

3.4 Measurement Concerns: Manipulation and Heaping

We conduct manipulation tests to verify if there was manipulation of the running variable. More formally, we implemented the test proposed by McCrary (2008) to check for jumps in the density of per capita income in 2011 at the R$ 140 cutoff. The standard McCrary test shows strong signs of manipulation, as can be seen in Figure 2 (*p-values* ≤ 0.01). Manipulation is enough to invalidate the RD strategy, but, as is common in income variables, we have a common behavior that might be affecting the manipulation: heaping

(Barreca et al., 2016; Zinn and Würbach, 2016). Manipulation tests do not differentiate between heaping and manipulation, and, when the cutoff is a point of heaping, there is a strong possibility that these tests will indicate manipulation where there is none (Barreca et al., 2016).

Variables that can take real values are subject to rounding, which agglomerates observations at specific points. Some examples of such variables includes income, expenditure, weight, and time (Pudney, 2008; Manski and Molinari, 2010; Barreca et al., 2011; Zinn and Würbach, 2016). Heaped values have negative effects on statistical analyses when they are not random, resulting in inconsistent estimates (Manski and Molinari, 2010; Barreca et al., 2011; Dong, 2015; Barreca et al., 2016; Zinn and Würbach, 2016; Giustinelli et al., 2018). In an approach known as donut RD, observations close to the cutoff that might be heaping points are dropped, and the analysis for the other observations can be considered to be valid (Almond and Doyle, 2011; Eggers et al., 2015; Barreca et al., 2016). This approach has been used for variables such as GPA scores (Carruthers and Özek, 2016), time of birth (Barreca et al., 2011), and electorate results (Eggers et al., 2015). In the Cadúnico database, both family and per capita income are rounded to the nearest integer.[7] Since family income is reported by the families, we identified heaping points in the family income. As in most similar previous studies, we identify most of the heaped values graphically. Figure 1 indicates possible heaping points for *total family income*. Similar to other surveys where income is reported, there is a strong preference for final digit zero (Manski and Molinari, 2010; Zinn and Würbach, 2016). Additionally, many individuals may round their income to the minimal wage imposed by law. Therefore, we identified two types of heaping: in multiples of the minimal wage (MW) in 2011 (R$540 until March, R$545 from March to December) and in multiples of 10. In our case, the 2011 cutoff - R$140 - was a heaping value. If we used only households that did not heap their income, we could obtain consistent estimations provided that basic assumptions for the RD identification were met for that subset (Barreca et al., 2016). Figure 3 shows the McCrary tests for non-heaped observations, around the CCT cutoff, which demonstrates no sign of manipulation, tested either on the main group of analysis or on the birth spacing group (main group, conditional on giving birth).

[7] It is not known why there was rounding in the per capita income; it might be easier to analyze or store the data. Nonetheless, our manipulation tests, first and second stage results were robust to the use of unrounded per capita income

4 Empirical Framework and First Stage Estimation

Following closely the notation in Hahn et al. (2001), in a traditional RD setup,[8] our average treatment effect for treatment compliers (LATE) is

$$\Theta_{BF} = E[Y(1) - Y(0)|X = c, lim_{x \to c^-} D_{BF}(x) > lim_{x \to c^+} D_{BF}(x)] \quad (1)$$

in which the dummy variable D_{BF} denotes the treatment - being a BF beneficiary - as a function of the running variable X (per capita income in R\$2011 values). The variables $Y(1)$ and $Y(0)$ denote our outcomes of interest - an indicator of having at least one birth between 2012 and 2015 and z-score birth spacing - as a function of the BF treatment, and c is our poverty line of R\$140. To analyze the first dependent variable, we used our main group of analysis: women from 16-44 years of age, who registered with Cadúnico in 2011 as head of the household. To analyze birth spacing, we restricted the main group to women who *had* births, since there was no variance on the z-score for the subset with no births. This is the *birth spacing group*. Most RD estimations restrict the analysis within a bandwidth around the cutoff. Within optimal bandwidths calculated using the Calonico et al. (2014) methodology[9] around the cutoff, we defined our (linear) parametric form to estimate equation (1):

$$Y_i = \alpha_0 + \Theta_{BF} D_{BF,i} + \theta_1 (X_i - c) + \theta_2 D_{BF,i} \times (X_i - c) + \epsilon_i \quad (2)$$

where X_i is the per-capita income of woman i in 2011 and ϵ_i is the error term. To correctly identify and estimate the treatment effect, we must show that the conditions of equation (1) are satisfied (i.e., the first stage of a RDD[10]). Formally, the first stage corresponds to estimating the following regression:

$$D_{BF,i} = \gamma + \gamma_0 \mathbb{1}[X_i \leq c] + \gamma_1 (X_i - c) + \gamma_2 \mathbb{1}[X_i \leq c] \times (X_i - c) + \eta_i \quad (3)$$

where $\mathbb{1}[X_i \leq c]$ is a dummy variable to indicate if the per capita income is below the cutoff and η is an error term. We estimate equation (3) using both first and second degree polynomials for the $(X_i - c)$ variable, and also for the following treatments: BF (grouping its two components), CCT, and UCT.

Our first stage estimations are graphically represented by Figures 4 and 5. For the main group, there is a

[8]See Lee and Lemieux (2010) for a theoretical review and applications of the RD design

[9]Many different optimal bandwidths calculations are presented in Calonico et al. (2014) Unless otherwise stated, we will make use of mean squared error optimal and under-smoothing bandwidths approaches

[10]Implicitly assumed are the standard conditions for the RD estimation: there are no defiers and the distribution of potential outcomes and potential treatment states is continuous in X, with finite moments.

clear discontinuity for BF and CCT, and, surprisingly, a small but significant jump on the UCT component, despite this benefit being given only to those below the extreme poverty cutoff. Although it is not a large effect, we decided on a more conservative estimation, using BF as a treatment instead of the CCT component, since the UCT might be a confounding factor. For the birth spacing group, however, the UCT had no discontinuity. In that case, the BF effect can roughly be translated as a CCT effect. Table 6 shows the first stage regressions. As expected, BF and CCT have virtually the same results, since all BF beneficiaries are CCT beneficiaries as well. Using second-degree polynomials strengthens the results of the first stage estimation. Notice, however, that compliance is not perfect. There is delay between registry, analysis, and benefit assignment, and this depends on the family position in the waiting list. Municipalities are able to enroll MDS's estimated number of eligible families, but if there are more registered individuals than predicted, a waiting list for eligibility analysis is created (MDS, 2011, 2015). Moreover, this quota is not binding in all municipalities. This leads to a fuzzy RD design, rather than a sharp one, as compliance is not perfect. In that case, $\mathbb{1}[X_i \leq c]$ and $\mathbb{1}[X_i \leq c] \times (X_i - c)$ act as instrumental variables for $D_{BF,i}$ and $D_{BF,i} \times (X_i - c)$.

For our results to be valid, no covariate should drive the results. That is, covariates should be continuous at the cutoff to ensure that the randomness brought by the eligibility rule is as good as possible. We thus compared observable variables above and below the cutoff, using the same parametric form as in equation (3). Visual and regression results can be found in the appendix, in Figures A1 to A7 and table A3 for the main group, and Figures A8 to A14 and Table A4 for the birth spacing group. In general, the differences are statistically insignificant. Most of the statistically significant differences are small in magnitude, specially in the case of covariates that might be related to fertility, such as work related variables, ethnicity, or number of children.

5 Second Stage Estimation and Results

Before we proceed in presenting the estimates, it is important to notice that, since BF is allocated on a monthly basis, dynamics in treatment assignment are present. The estimates in this paper represent a dynamic *intent-to-treat* effect (Cellini et al., 2010), where we verify the effect of cash transfers on fertility rates without controlling for the families - or the government's - behavior after the 2011 allocation. To strengthen the reduced form results and to give a better economic interpretation of them, we present an estimate that gives a rough sense of the benefit dynamics, the total number of monthly installments received between 2011 and 2015, around the R$ 140 cutoff. These estimates are visually represented in Figures 6. The discontinuity jump for the main group is roughly 20 installments, a bit more than one and a half years of benefits. Considering that most beneficiaries would start being paid at the end of 2011 or at 2012, and that the cutoff shifted to the right in 2014, the dynamics showed that those above the cutoff were not immediately selected to treatment after 2011. If that was the case, our estimates would not bring meaningful results. For the birth spacing group, the difference is smaller, albeit not surprising. Since the birth spacing group had at least one birth, the per capita income may have decreased substantially, increasing the probability to be selected to the program.

Table 7 summarizes the accumulated number of births between 2012-2015, by women with a 2011 per capita income within a R$50 bandwidth around the cutoff. Only 15% had births, and of that subset, around 93% had only one child. Visualizations of the dependent variables around the cutoff are in Figure 7. Visually, there is a small but insignificant difference in the birth outcome.

Second stage results[11] for equation (2) can be found in Table 8. In the table, Panel A shows a naïve sharp estimation, while Panel B shows the result for fuzzy estimates. First, we noted that for the childbirth outcome, both sharp and fuzzy are very close to zero. Note that using only up-to-date units for our fuzzy design changes the sign, which supports our suspicions of a downward bias caused by attrition. Regarding birth spacing, we noted that we had a negative, statistically significant effect in all of the estimations. The LATE corresponds to a roughly -0.5 z-score. For example, considering the third parity statistics in table A2, beneficiaries reduced their birth spacing - relative to the second child - by 460 days, or 15 months, on average.

We can estimate sharp bounds on the treatment effects in case the attrition in the registries' updates are driving the results. Consider U a dummy variable for attrition for the year 2015, assuming 1 when the individual has an up-to-date info in 2015, and 0 otherwise. For the interaction $U \times X$, it is possible to use the bounds of Gerard et al. (2020) to address the attrition problem, since a jump at the cutoff for $U \times X$ might be seen as a manipulation. Gerard et al. (2020) derives a non-parametric methodology with which it is possible to identify sharp bounds on treatment effects under manipulation or other phenomena that can alter the continuity around the cutoff - with only few additional hypotheses on the RD design.[12] The

[11] To reduce confoundedness, we drop observations that changed municipalities in 2015, relatively to 2011. We ran regressions using all observations, and the results were similar. Around 85% of the observations remained after this procedure for both groups

[12] In short, the approach relies on two types of unobservable units. First is the *always-assigned units*, for which the realization of the running variable is always on one side of the cutoff. Those include manipulators, those that update more frequently, and nonrandom heaping. The second type are *potentially assigned* units - units that satisfy the standard conditions for identification under the RDD. One of the hypotheses is that the manipulation is one-sided and that are no mass points on the running variable around the cutoff value. This might be the case in our context: it is not rational for someone to manipulate their income up,

parameter of interest is similar to the one in equation (1), where we estimate LATE for those below the cutoff, and the *potentially assigned* units would be also units that in the absence of the treatment would not change their update patterns:

$$\Theta_{BF}^{bounds} = E[Y(1) - Y(0)|U \times X = c^-, D_{BF}^- > D_{BF}^+] \tag{4}$$

Figure 8 shows the update pattern around the cutoff for 2015. For the main group, there is a jump in the update pattern, which could be the result of BF assignment. Not surprisingly, we found no evidence of a jump in the update pattern for the birth spacing group, since families update the registry to inform a birth. The bounds results can be found in Table 9. Panel A shows the basic inputs for bound estimation. Always assigned are those that always keep their info updated in the presence of available treatment, and are always below the cutoff. The nonparametric estimation of the treatment take-up is close to the parametric estimations. Using $U \times X$ as the running variable is similar to using X but with only up-to-date units; notice that the fuzzy coefficient of Panel B is not statistically different from the 'up-to-date only' estimation in Table 8. As is the case in the parametric regressions, the results are not statically different from zero for a 95% confidence interval. The bounds estimations reveal a rather asymmetrical result, with a large lower bound. Indeed, how the 'non-updaters' would update their information influences the treatment effect and, although a lower point of -25 p.p. might be difficult, we can infer that at most, the possible positive effects of CCT on fertility are rather small. However, it is important to note that the large bounds could be the effect of the relatively small difference between the take-up increase and the share of always assigned (i.e., 'nonupdaters').

6 Robustness

We made several robustness checks. Particularly, we estimated equation (2) with different bandwidths and kernels. Table 10 present the results for the main group, and Table 11 for the birth spacing group. The estimations for at least one birth were consistent across different bandwidth and kernel choices, with only two specifications that used only up-to-date observations being statistically significant. Most of the results were similar for the z-score birth spacing, although the magnitude was reduced for larger bandwidths. In summary, the main results were robust to different specifications.

since all benefits are given below a certain threshold. Upward rounding may happen, but for those just below the cutoff - they will still be *at* the cutoff and thus eligible. Further, we also suppose that manipulators round their reported income as well

7 External Validity

The treatment effect identified in our study only applies around the cutoff, and, being a fuzzy RD, only for compliers. Yet, researchers and policy makers may also be interested in generalizing the findings, particularly to subpopulations away from the threshold. Recent work has explored ways of testing whether RD findings may be generalized, with some studies exploring the use of pretreatment covariates (Angrist and Rokkanen, 2015; Rokkanen, 2015). We proceeded to evaluate the stability of our RD results by implementing tests suggested by the recent work of Dong (2015); Cerulli et al. (2017); Bertanha and Imbens (2019), which require no use of additional covariates other than the forcing variable and require fewer additional assumptions than those already assumed here. Dong and Lewbel (2015); Cerulli et al. (2017) presented the treatment effect derivative (TED), which requires an additional assumption of smoothness in higher order derivatives of the regression function. The TED is basically the derivative of the parametric RD outcome function with respect to the running variable, close to the cutoff. A large TED means that a small change in the running variable is associated with a large change in the treatment effect, calling into question the estimated LATE's external validity. In fuzzy designs, the change in the effect may come from a true change in the effect on the compliers, or a change in the treatment compliance distribution. Cerulli et al. (2017) applied the TED framework to the first stage to estimate a change in compliance composition, which the authors call a complier probability derivative (CPD). A large CPD means a large change in compliance. Therefore, small values of TED and CPD reinforce the external validity of the estimated treatment effects. Bertanha and Imbens (2019) created mean tests that can be used to support the hypothesis of generalization. External validity means that the compliance distribution must be independent of the conditional potential outcomes distribution, not only on the expected value. Thus, it implies that average treatment effects for compliers and non-compliers just below and just above the cutoff are the same. Essentially, the tests proposed by Bertanha and Imbens (2019) are mean tests of such averages where the non-rejection of equality reinforces the assumption of external validity.

To make better use of the TED framework, we use second-order polynomials in the regressions. A visual explanation of the TED is in Figures 9 and 10: the TED corresponds to the tangent lines. We present our estimations in Tables 12 and 13. On both uniform and triangular kernels, the results are similar. For the birth variable, given the LATE estimate, it is not surprising that the TED estimate for childbirth is close to zero. The compliers composition may change statistically, as the CPD results shows, but it is small in magnitude. Further, we do not reject the hypothesis of equality of means of Bertanha and Imbens (2019). This strengthens the view that BF had no impact on fertility for the 2012-2015 period, measured as at least one birth, for non-heaping units. Unless the heaping behavior is connected to a change in fertility decisions, we can apply this result to the general population. Going further, and assuming that heaping behavior is not influenced by cutoff changes and is not correlated with fertility decisions, then it is possible to say that BF has no effect on fertility.

For the birth spacing outcome, the CPD/TED analysis is similar: the TED estimate is not large in magnitude or statistically, but the complier composition shows evidence of instability, albeit small. However, the mean test for the compliers is statistically significant, and is large enough so the joint test is also statistically significant. Given these results, it is more difficult for us to assume that the birth spacing reduction brought by BF can be generalized.

8 Discussion and Conclusion

More than a decade after its release, BF is arguably the most important pillar of Brazil's welfare system. However, cash transfers can, in theory, alter fertility decisions. If fertility increases overall, human capital accumulation per capita may decrease if cash transfers' incentives to increase children's quality are few. This study reinforces past literature results using a more reliable data, with a new identification strategy and verifying more than one fertility outcome. Using data from Cadúnico, the database used to allocate BF benefits, we took advantage of a discontinuity in the eligibility rule, implementing a regression discontinuity design to verify the program's impact on birth spacing and on the probability of having more children. In this study, the causal effect of the program at the cutoff in the probability of having a childbirth is statistically zero, and although update patterns are very different for treatment and control, the nonparametric approach presented by Gerard et al. (2020) showed that at most, the positive effects on fertility were rather small. This result is in line with other studies that analyzed the same outcome but with different methodologies, and thus it shows the importance of using different methods to provide robust evidence in policy debates. Using a different measure in regard to birth spacing, the results were surprising. Conditional on having a birth, BF reduced birth spacing by, on average, 0.5 standard deviations in the pooled z-score. For families waiting for a second child, this represents 19 months, and for the third child, 15 months. We also used new econometric tools to check our RD's external validity. While the RD results for childbirth seem to be stable, providing support for a true average treatment effect of the program, the same was not found for birth spacing: the effect on birth spacing is unstable and might vary away from the R$ 140 cutoff.

The findings in this study have support in the literature. In a context where technological advance requires large investments in child education, marginal financial incentives for more children seem to have little effect in fertility decisions. Moreover, poor families in developing countries face a lack of credit markets, so a cash transfer can alleviate credit constraints. This allows for more efficient investment in human capital, which reduces incentives to increase fertility (Galor and Zeira, 1993; Galor and Weil, 2000; Todd and Wolpin, 2006). It might seem worrying that a cash transfer would reduce birth spacing, since reductions in birth spacing might affect the health of the mother or of the children (Conde-Agudelo et al., 2006, 2012; Todd et al., 2012), but the average birth spacing of Brazilian families registered in Cadúnico seems to be well above the harmful threshold of 18 months. Before BF, this could be due to the effect of the lack of credit markets, so families increased birth spacing to smooth consumption. Again, the transfer may be alleviating credit constraints, so families may choose a new optimal birth spacing that can increase future income if economies of scale exist when raising children (Galor and Weil, 1996; Schultz, 1997; Todd et al., 2012). This study also supports the view that for a lifetime analysis, the quantity-quality trade-off might not be as clear-cut as the general Becker and Lewis (1973) model demonstrates (Angrist et al., 2010). Therefore, we believe that the policy debate should take into account the general evidence that cash transfer programs do not easily affect fertility decisions (Stecklov et al., 2007; Fiszbein et al., 2009).

As it was designed, the BF program showed no effect on fertility rates, but it reduced the birth spacing, on average. This reduction seems to be consequence of alleviating income restraints. Additionally, we believe that the problems of manipulation and rounding we encountered may be affecting the targeting of the program. Further studies on whether or not the eligibility rule is appropriate, which is mainly based on self-reported income, are necessary.

Compliance with Ethical Standards

The author certifies that he has NO affiliations with or involvement in any organization or entity with any financial interest (such as honoraria, educational grants, participation in speakers' bureaus, membership, employment, consultancies, stock ownership or other equity interest, expert testimony, or patent-licensing arrangements) or non-financial interest (such as personal or professional relationships, affiliations, knowledge, or beliefs) in the subject matter or materials discussed in this manuscript.

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Tables

Table 1: BF's eligibility categories from 2011 to 2015, by nominal monthly per capita income cutoffs (In Brazilian Reais - R$)

Category	2003-2005	2006	2007	2008	2009-2010	2011-2013	2014-2015
Poverty	50	60	60	60	70	70	77
Extreme Poverty	100	120	120	120	140	140	154

Notes: A household falls into the extreme poverty category if its monthly per capita income is equal or less than the extreme poverty threshold. A household falls into the poverty category if its monthly per capita income is above the extreme poverty threshold but less or equal than the poverty cutoff. Source: MDS (2011, 2015)

Table 2: BF's Benefits (excluding BSP) monthly payments from 2011 to 2015 in nominal values (In Brazilian Reais - R$), by BF's categories

Category	Benefit	2003-2005	2006	2007	2008	2009-2010	2011-2013	2014-2015	
Extreme Poverty	Basic	50	50	58	62	68	70	77	
	Variable	15	15	18	20	22	32	35	
	BVJ	-	-	30	30	33	38	42	
Poverty	Basic	-	-	-	-	-	-	-	-
	Variable	15	15	18	20	22	32	35	
	BVJ			30	30	33	38	42	

Source: MDS (2011, 2015)

Table 3: Summary statistics for 2011 *Cadúnico* - Individual Information

Variables	**Percentage/Mean**		**Percentage/Mean**
Race: White	29.3%	Kinship: Head of the household	29.7%
Race: Black-Brazilian	7.2%	Kinship: Spouse (of head)	13.7%
Race: Oriental	0.3%	Kinship: Son/daughter (of head)	51.6%
Race: Pardo-Brazilian	61.1%	Kinship: Others	5.3%
Race: Indigenous	0.6%		
Race: Missing	1.6%	Year of Registry: Before or on 2007	15.7%
Gender: Female	45.2%	Year of Registry: 2008	10.5%
Gender: Male	54.8%	Year of Registry: 2009	30.9%
Age	26.2	Year of Registry: 2010	35.3%
Has disabilities	0.6%	Year of Registry: 2011	7.6%
Obsevations			75,934,171

Table 4: Summary statistics for 2011 *Cadúnico* - Household Information

Variables	**Percentage/Mean**		**Percentage/Mean**
Household: Per capita income	R$ 108	Wastewater: Others	1.9%
Household: N^{o} of individuals	3.4	Wastewater: Missing	3.5%
		Garbage: Collected	67.5%
Household: Urban	79.3%	Garbage: Burned or buried	18.5%
Household: Number of rooms	4.23	Garbage: Discharged in open air	5.9%
Household: Piped water	70.5%	Garbage: Others	8.1%
Household: Piped water (missing info)	0.0%	Electricity: Electric supply network	90.1%
Household: Have bathroom	86.1%	Electricity: Generators	2.0%
Household: Have bathroom (missing info)	0.0%	Electricity: Candles	1.8%
Water: Supply network	68.2%	Electricity: Others	5.0%
Water: Artesian Aquifer	22.6%	Electricity: Missing	1.1%
Water: Others	8.1%	Region: North	9.7%
Water: Missing	1.1%	Region: Northeast	44.6%
Wastewater: Sewer system	40.4%	Region: Center-west	6.4%
Wastewater: Septic tank	42.7%	Region: Southeast	28.9%
Wastewater: Directly discharged at water sources	11.5%	Region: South	10.4%
Obsevations			22,176,258

Table 5: Summary statistics: women of 16-44 years of age, registered in Cadúnico as head of the family and with at least one child

Variables	**Percentage/Mean**		**Percentage/Mean**
Race: White	29.9%	Wastewater: Sewer system	46.2%
Race: Black-Brazilian	8.4%	Wastewater: Septic tank	39.8%
Race: Oriental	0.7%	Wastewater: Directly discharged at water sources	5.5%
Race: Pardo-Brazilian	60.1%	Wastewater: Others	1.2%
Race: Indigenous	0.7%	Wastewater: Missing	7.2%
Race: Missing	0.2%	Garbage: Collected	77.0%
Work: Worked in the last week	29.1%	Garbage: Burned or Buried	14.2%
Work: Missing	36.0%	Garbage: Discharged at open air	1.8%
Education: None or some (<4yrs of education)	12.6%	Garbage: Others	7.0%
Education: Primary Incomplete	32.9%	Electricity: Electric supply network	89.8%
Education: Some High School education	25.0%	Electricity: Generators	1.1%
Education: High school or higher	29.3%	Electricity: Candles	1.2%
Education: Missing	0.3%	Electricity: Others	4.6%
Age	28.0	Electricity: Missing	3.4%
Have disabilities	0.6%	Per Capita Income	106.6
Live with spouse	36.7%	Income: Receive pension	12.7%
Number of Children	1.66	Income: Donations	6.5%
Household: Urban	82.1%	Income: Unemployment benefits	0.6%
Household: Number of rooms	3.88	Income: other sources than job related	24.2%
Household: Piped water	78.7%	Region: North	14.2%
Household: Piped water (missing info)	3.4%	Region: Northeast	33.2%
Household: Has bathroom	78.7%	Region: Center-west	8.3%
Household: Has bathroom (missing info)	3.4%	Region: Southeast	33.9%
Water: Supply network	72.4%	Region: South	10.3%
Water: Artesian Aquifer	18.6%	BF beneficiaries (2011 allocation)	75.8%
Water: Others	5.6%	CCT beneficiaries (2011 allocation)	75.6%
Water: Missing	3.4%	UCT beneficiaries (2011 allocation)	58.6%
Observations			**600,402**

Notes: This table presents summary statistics for key variables. Data comes from merging yearly data from Cadúnico and Caixa payment registries, from 2011 up to Apr 2012. We considered only 2011 entering families, in which the woman was considered head of the family (>80% of the families), within fertile age (16-44) and with at least one child.

Table 6: Effect of having a 2011 per capita income below the poverty cutoff (R$140) on the probability of being a BF, CCT or UCT beneficiary.

A. Main Group	**Linear Regressions**			**B. Birth spacing Group**	**Linear Regressions**		
A1. BF	[1]	[2]	[3]	**B1. BF**	[1]	[2]	[3]
Below cutoff	0.58**	0.49**	0.46**		0.55**	0.44**	0.50**
	(0.01)	(0.02)	(0.02)		(0.03)	(0.03)	(0.05)
Income Linear Interaction	No	Yes	Yes		No	Yes	Yes
Income Quadratic Interaction	No	No	Yes		No	No	Yes
Observations	15,274	15,274	15,274		2,030	2,030	2,030
R^2	0.37	0.37	0.37		0.36	0.37	0.37
A2. CCT				**B2. CCT**			
Below cutoff	0.58**	0.49**	0.46**		0.55**	0.44**	0.50**
	(0.01)	(0.02)	(0.02)		(0.03)	(0.05)	(0.03)
Income Linear Interaction	No	Yes	Yes		No	Yes	Yes
Income Quadratic Interaction	No	No	Yes		No	No	Yes
Observations	15,274	15,274	15,274		2,030	2,030	2,030
R^2	0.36	0.37	0.37		0.36	0.37	0.37
A3. UCT				**B3. UCT**			
Below cutoff	0.14**	0.07**	0.07**		0.12**	-0.01	-0.01
	(0.01)	(0.02)	(0.02)		(0.02)	(0.04)	(0.06)
Income Linear Interaction	No	Yes	Yes		No	Yes	Yes
Income Quadratic Interaction	No	No	Yes		No	No	Yes
Observations	15,274	15,274	15,274		2,030	2,030	2,030
R^2	0.03	0.03	0.03		0.02	0.02	0.02

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for 1st and 2nd order polynomials and uniform kernels. Main group unit of observation: women of 16-44 years of age, registered in Cadúnico as head of the family and with at least one child. Birth space group unit of observation: main group, conditional on having at least one birth during the 2012-2015 period. Bandwidths: R$ 36 for the main group and R$ 32 for birth spacing group. Heteroscedastic robust-errors in parentheses. P-values: ** p<0.01, * p<0.05.

Table 7: Number of births per women in 2012-2015, within a $40 bandwidth around the BF cutoff

Childbirths	**N**	**Percentage (%)**
0	12,695	85.41
1	2,014	13.55
2	151	1.02
3	4	0.03
Total	**14,864**	**100**

Notes: Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family.

Table 8: Average treatment effects of BF's CCT for the period 2012-2015. Dependent variables: *dummy* for at least one birth during 2012-2015, and birth spacing z-score

	At least one birth in 2012-2015			**Birth spacing z-score**		
	Estimate	95% CI	Observations	Estimate	95% CI	Observations
A.Sharp RD estimate	0.010	[-.011; .032]	13,888	-0.246	[-.455; -.036]	1,732
B.Fuzzy RD estimates						
Θ_{cct}	0.019	[-.024; .062]	13,888	-0.494	[-.940; -.047]	1,732
up-to-date units						
Θ_{cct}	-0.046	[-.117; .024]	9,695	-0.523	[-.966; -.080]	1,706

Notes: Fuzzy regressions follow equation (2) specification. Units that changed municipality during the 2012-2015 period are not considered. Bandwidths calculated by Calonico et al. (2014) under-smoothing methodology, with a 1st order polynomial and an uniform kernel. Bandwidth for the **At least one birth** variable: R$ 36. Bandwidth for the **birth spacing z-score** variable: R$ 32. Confidence intervals calculated with heteroscedastic robust errors.

Table 9: Average treatment effects on the probability of having births in 2012 to 2015.
Forcing variable: interaction of up-to-date 2015 registry dummy with 2011 per capita income.
Dependent variable: dummy for at least one birth in 2012-2015

Basic inputs for bounds estimation		
Share of always assigned units	0.17	-
Increase in treatment take-up at the cutoff	0.42	-
	At least one birth	
	Estimate	95% CI
A. Sharp RD estimates		
point estimate	-0.025	[-.05; .004]
Bounds on Θ^{-}_{BF}	[-.19; .011]	[-.23; .04]
B. Fuzzy RD estimates		
point estimate	-0.065	[-.137; .007]
Bounds on Θ^{-}_{BF}	[-.25; .025]	[-.29; .095]

Notes: Total number of observations: 73,969 women with at least one child, aged 16-44 years old, with non-heaped income. Bandwidth for estimation of the discontinuity in the density of $U \times X$: 127. Bandwidth calculated by McCrary methodology (McCrary, 2008). Bandwidth for the local linear estimation of conditional means: R$36. Bandwidth calculated by Calonico et al. (2014) under-smoothing methodology, with uniform kernel. Confidence intervals are based on 500 bootstrap samples.

Table 10: Robustness check for at least one birth dependent variable. LATE estimations using different kernel and polynomial degree inputs

	At least one birth					
Fuzzy RD estimates						
Θ_{BF}	0.019	0.020	0.019	0.016	0.023	0.015
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Kernel	uniform	triangular	triangular	uniform	triangular	triangular
Polynomial degree	1	1	1	2	2	2
Left bandwidth	36	43	49	51	47	49
Right bandwidth	36	43	74	51	47	132
Observations	13,888	15,989	23,833	19,117	17,644	33,273
(only update units)						
Θ_{BF}	-0.046	-0.048	-0.051*	-0.060	-0.049	-0.059**
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)
Kernel	uniform	triangular	triangular	uniform	triangular	triangular
Polynomial degree	1	1	1	2	2	2
Left bandwidth	36	43	49	51	47	49
Right bandwidth	36	43	74	51	47	132
Observations	9,695	11,133	15,864	13,245	12,275	20,772

Notes: Regressions follows (2) specification, with different inputs for kernel and polynomial degree. Unit observation: women, aged 16-44 years old, with non-heaped income. Bandwidths calculated by Calonico et al. (2014) under-smoothing methodology. Heteroscedastic robust-errors in parentheses. P-values: ** p<0.01, * p<0.05.

Table 11: Robustness check for z-score birth spacing dependent variable. LATE estimations using different kernel and polynomial degree inputs

	Z-score birth spacing					
Fuzzy RD estimates						
Θ_{BF}	-0.494**	-0.423**	-0.324*	-0.359**	-0.352**	-0.261*
	(0.23)	(0.20)	(0.17)	(0.18)	(0.17)	(0.16)
Kernel	uniform	triangular	triangular	uniform	triangular	triangular
Degree	1	1	1	2	2	2
Left bandwidth	32	48	41	55	76	72
Right bandwidth	32	48	82	55	76	99
Observations	1,732	2,557	3,229	2,926	4,418	4,656
only update units						
Θ_{BF}	-0.523**	-0.456**	-0.337**	-0.375**	-0.367*	-0.273*
	(0.23)	(0.20)	(0.17)	(0.18)	(0.17)	(0.15)
Kernel	uniform	triangular	triangular	uniform	triangular	triangular
Degree	1	1	1	2	2	2
Left bandwidth	32	48	41	55	76	72
Right bandwidth	32	48	82	55	76	99
Observations	1,706	2,523	3,180	2,887	4,350	4,584

Notes: Regressions follows (2) specification, with different inputs for kernel and polynomial degree. Unit observation: women, aged 16-44 years old, with non-heaped income, conditional on having a birth in 2012-2015. Bandwidths calculated by Calonico et al. (2014) under-smoothing methodology. Heteroscedastic robust-errors in parentheses. P-values: ** p<0.01, * p<0.05.

Table 12: External Validity Statistics for Birth and Z-score birth spacing Outcomes - Uniform Kernel

	Birth on 2012-2015	
	Estimate	**Bandwidth**
LATE	.019	36
	(.022)	
TED	-.0008	43
	(.004)	
CPD	-.004*	43
	(.002)	
mean tests		
$E[Y\|D=0,(X-c)\to 0^+] - E[Y\|D=0,(X-c)\to 0^-]$	-.05	30
	(.034)	
$E[Y\|D=1,(X-c)\to 0^+] - E[Y\|D=1,(X-c)\to 0^-]$	.004	36
	(.027)	
Joint F-test	2.179	
	Birth spacing z-score	
	Estimate	**Bandwidth**
LATE	-.494**	32
	(.230)	
TED	-.023	44
	(.036)	
CPD	-.009	44
	(.005)	
mean tests		
$E[Y\|D=0,(X-c)\to 0^+] - E[Y\|D=0,(X-c)\to 0^-]$	-.1	55
	(.46)	
$E[Y\|D=1,(X-c)\to 0^+] - E[Y\|D=1,(X-c)\to 0^-]$	-.58**	38
	(0.22)	
Joint F-test	6.72*	

Notes: TED and CPD calculated based on Dong and Lewbel (2015); Cerulli et al. (2017) framework. Means tests and joint mean test based on Bertanha and Imbens (2019) framework. Bandwidths calculated by Calonico et al. (2014) under-smoothing methodology, with 1st order polynomial for mean tests and 2nd order for TED and CPD. Heteroscedastic robust-errors in parentheses. P-values: ** p<0.01, * p<0.05. Errors are based on 500 bootstrap samples.

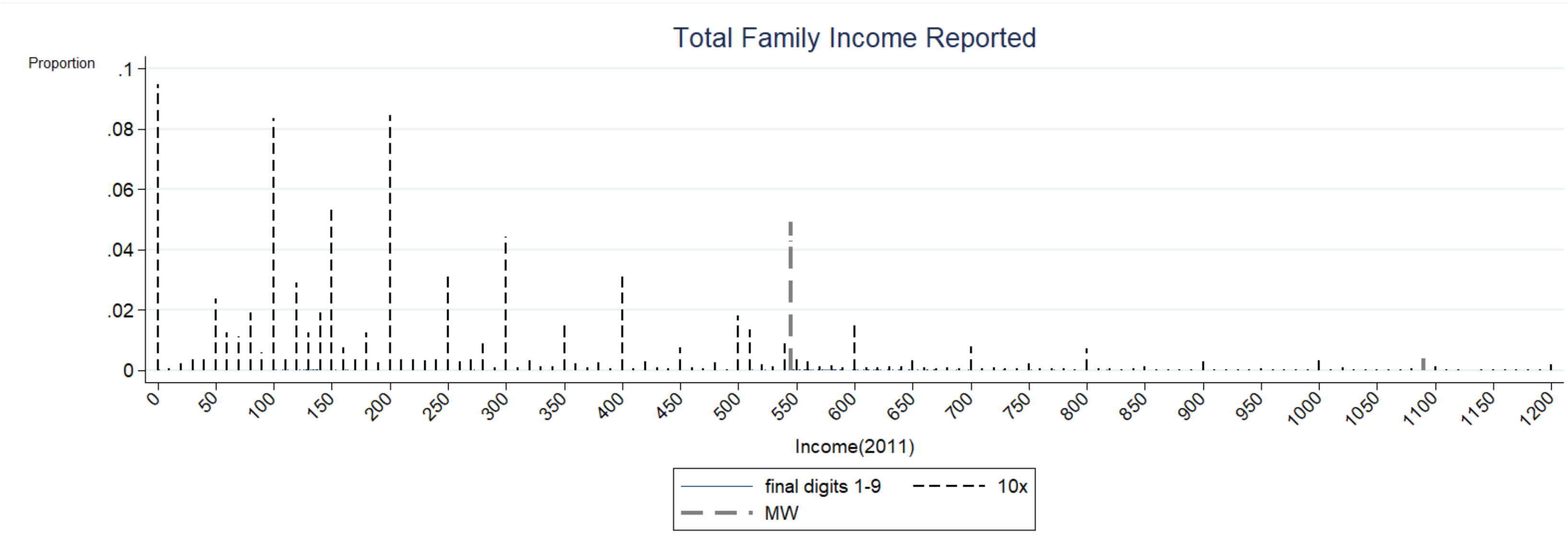
Table 13: External Validity Statistics for Birth and Z-score birth spacing Outcomes - Triangular Kernel

	Birth on 2012-2015	
	Estimate	**Bandwidth**
LATE	.02	43
	(.02)	
TED	.001	99
	(.001)	
CPD	-.004**	99
	(.001)	
mean tests		
$E[Y\|D=0,(X-c)\rightarrow 0^{+}]-E[Y\|D=0,(X-c)\rightarrow 0^{-}]$	-.046	45
	(.029)	
$E[Y\|D=1,(X-c)\rightarrow 0^{+}]-E[Y\|D=1,(X-c)\rightarrow 0^{-}]$	-.008	57
	(.020)	
Joint F-test	2.74	
	Birth spacing z-score	
	Estimate	**Bandwidth**
LATE	-.423**	48
	(.200)	
TED	-.021	73
	(.016)	
CPD	-.007**	73
	(.003)	
mean tests		
$E[Y\|D=0,(X-c)\rightarrow 0^{+}]-E[Y\|D=0,(X-c)\rightarrow 0^{-}]$	-.13	53
	(.46)	
$E[Y\|D=1,(X-c)\rightarrow 0^{+}]-E[Y\|D=1,(X-c)\rightarrow 0^{-}]$	-.56**	41
	(.20)	
Joint F-test	8.03*	

Notes: TED and CPD calculated based on Dong and Lewbel (2015); Cerulli et al. (2017) framework. Means tests and joint mean test based on Bertanha and Imbens (2019) framework. Bandwidths calculated by Calonico et al. (2014) under-smoothing methodology, with 1st order polynomial for mean tests and 2nd order for TED and CPD. Heteroscedastic robust-errors in parentheses. P-values: ** p<0.01, * p<0.05. Errors are based on 500 bootstrap samples.

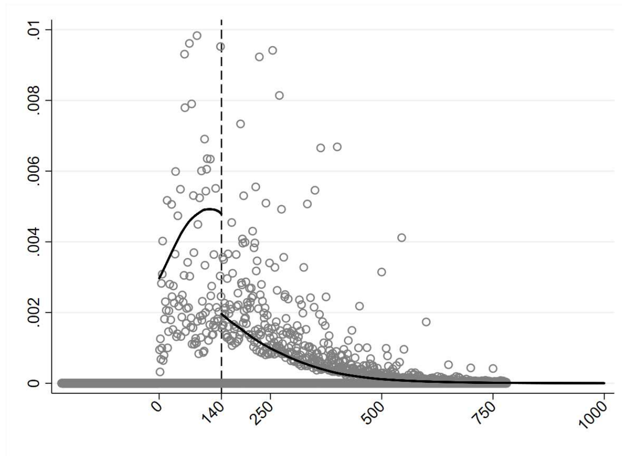
Figures

Figure 1: Cadúnico Family Income



Notes: Family income divided in multiples of 10, Minimal Wages(MW) or in final digits 1 to 9.

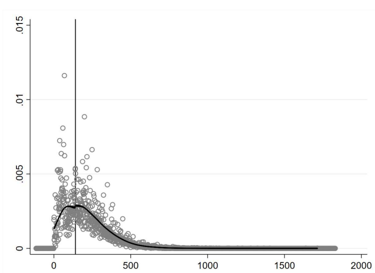
Figure 2: McCrary Tests on R$ 140 Cutoffs (Non-heaped income observations)



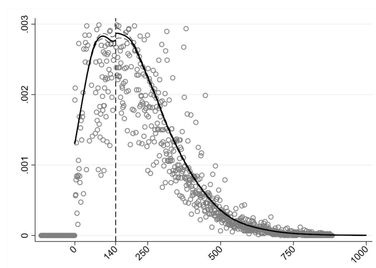
(a) Main group: density value < .01

Notes: Standard McCrary (2008) test. Income skewed at 99%. Dashed lines correspond to 95% CI. Main group corresponds to all observations at table 5: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family.

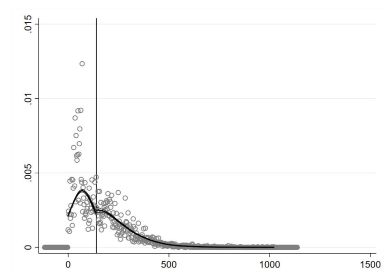
Figure 3: McCrary Tests on R$ 140 Cutoffs (Non-heaped income observations)



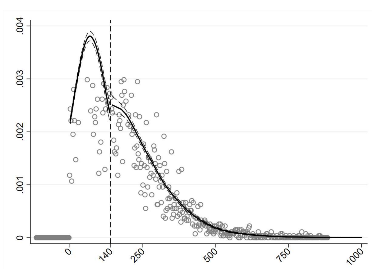
(a) Main group: full histogram



(b) Main group: density value < .003



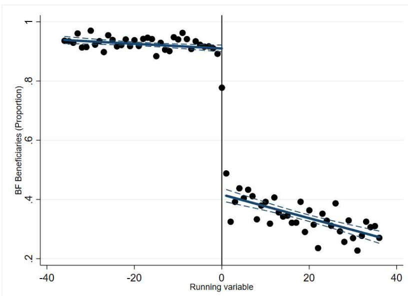
(c) Birth spacing group: full histogram



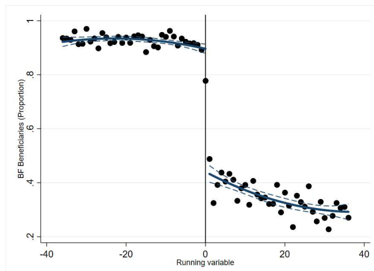
(d) Birth spacing group: density value < .003

Notes: Standard McCrary test McCrary (2008). Income skewed at 99%. Dashed lines correspond to 95% CI. Main group corresponds to all observations at table 5: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Birth spacing group corresponds to main group, conditional on having at least one birth in the period 2012-2015

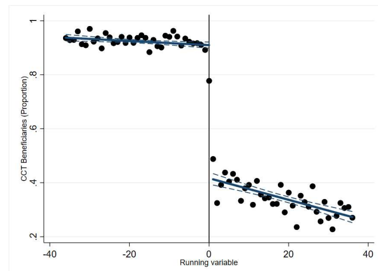
Figure 4: Probability of being a BF,CCT and UCT recipient as a function of the running variable - Main group



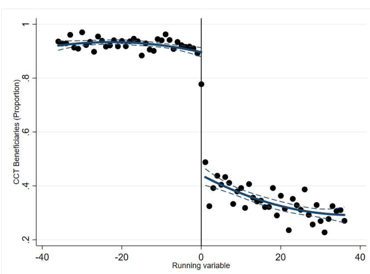
(a) 1st degree polynomial for BF treatment



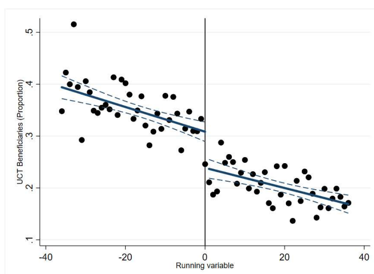
(b) 2nd degree polynomial for BF treatment



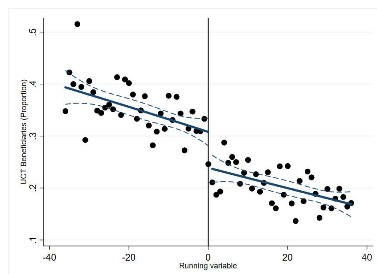
(c) 1st degree polynomial for CCT treatment



(d) 2nd degree polynomial for CCT treatment



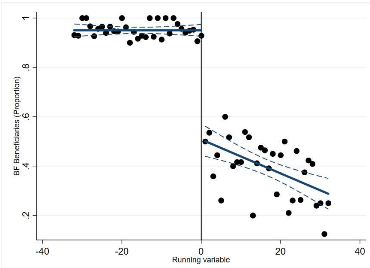
(e) 1st degree polynomial for UCT treatment



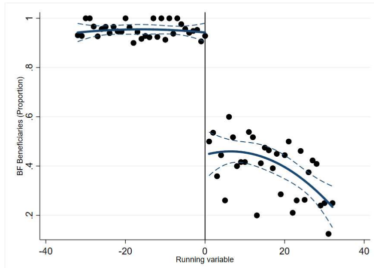
(f) 2nd degree polynomial for UCT treatment

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for 1st and 2nd orders polynomials and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table 6 for p-values and N^{o} of observations.

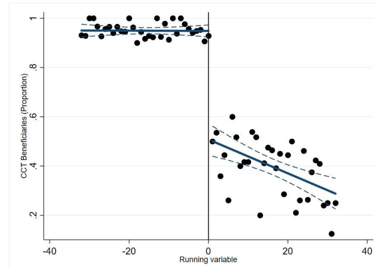
Figure 5: Probability of being a BF, CCT and UCT recipient as a function of the running variable - Birth spacing group



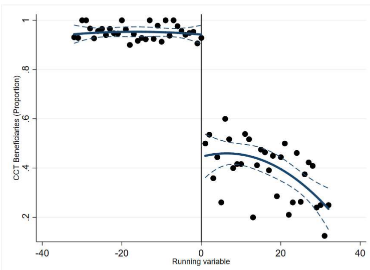
(a) 1st degree polynomial for BF treatment



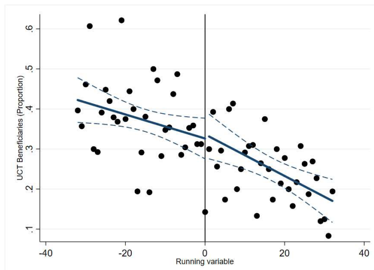
(b) 2nd degree polynomial for BF treatment



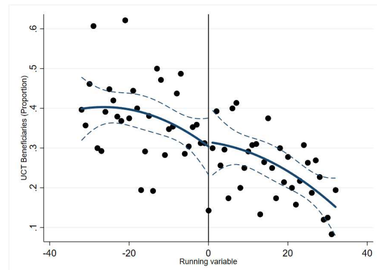
(c) 1st degree polynomial for CCT treatment



(d) 2nd degree polynomial for CCT treatment



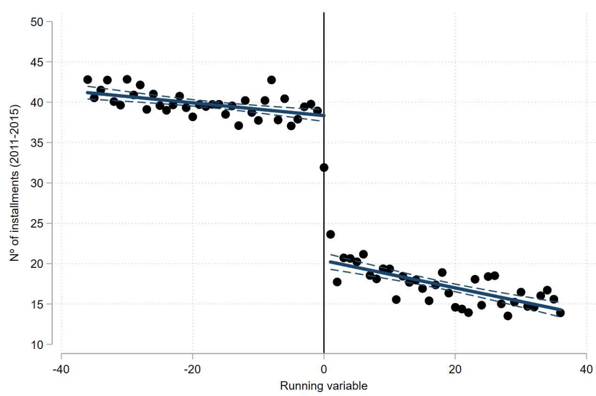
(e) 1st degree polynomial for UCT treatment



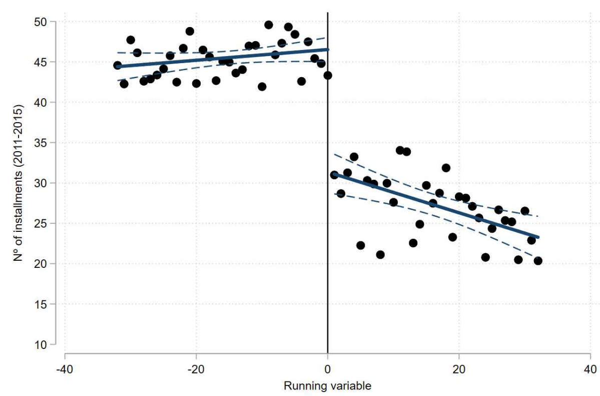
(f) 2nd degree polynomial for UCT treatment

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for 1st and 2nd orders polynomials and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth on the period 2012-2015. See table 6 for p-values and N° of observations.

Figure 6: Benefit dynamics: Number of monthly BF installments received, from 2011 to 2015, as function of the running variable



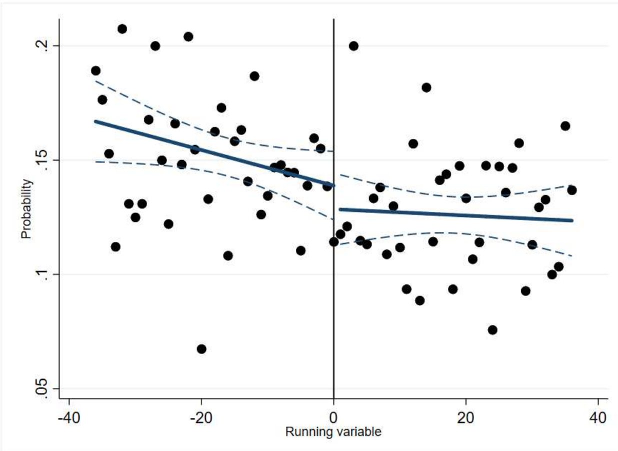
(a) Main group



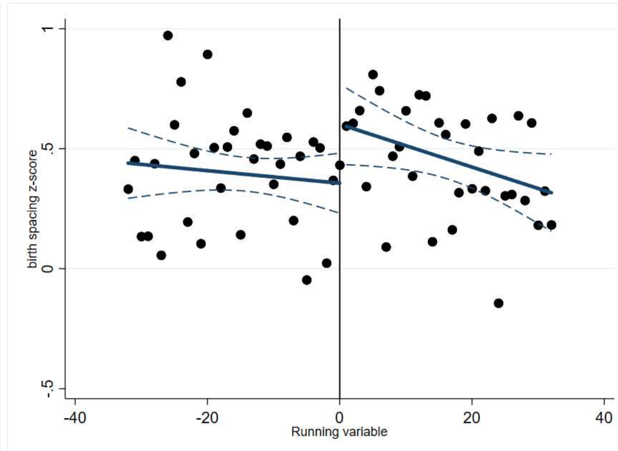
(b) Birth spacing group

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Main group unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Birth spacing group unit of observation: main group conditional on having at least one birth during 2012-2015

Figure 7: Outcomes as a function of 2011 income per capita - R$ 140 cutoff



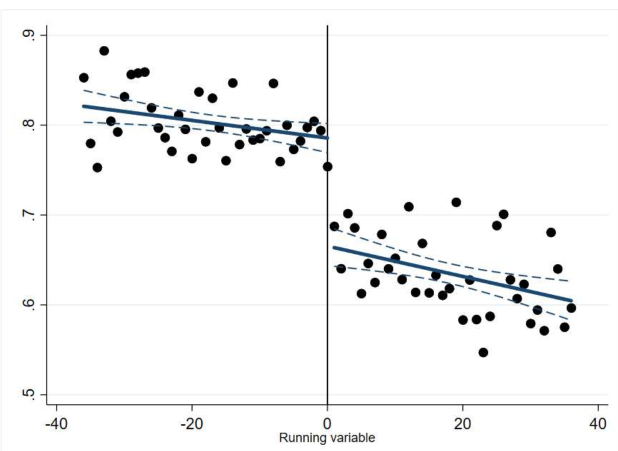
(a) Probability of having at least one birth in 2012-2015



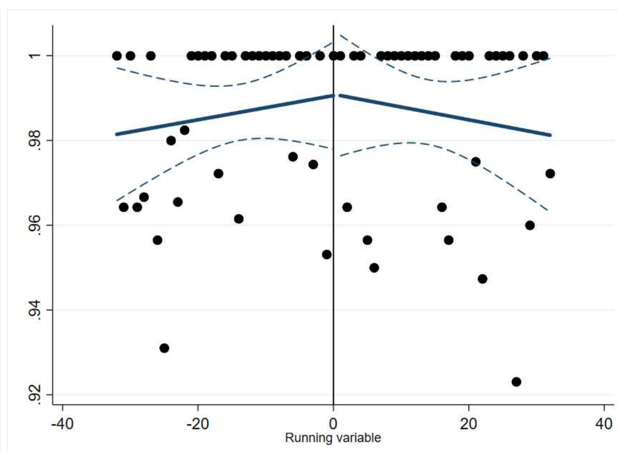
(b) Z-score birth spacing (2015)

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Main group unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Birth spacing group unit of observation: main group conditional on having at least one birth during 2012-2015

Figure 8: 2015 update pattern around the R$ 140 cutoff



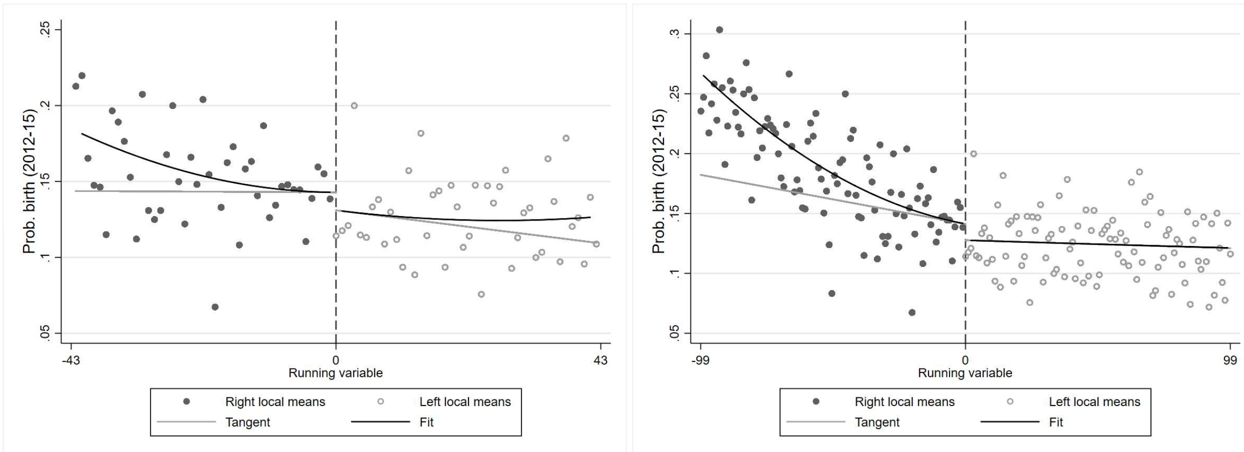
(a) Main group



(b) Birth spacing group

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Main group unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Birth spacing group unit of observation: main group conditional on having at least one birth during 2012-2015

Figure 9: TED: Fuzzy RD discontinuity in the probability of having a birth in 2012-2015, and tangent lines

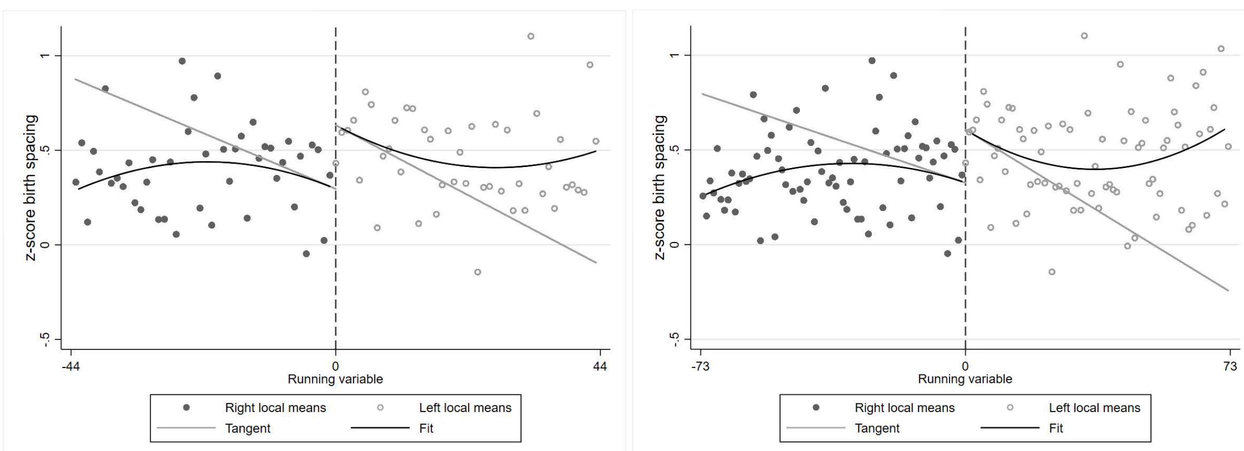


(a) Uniform Kernel

(b) Triangular Kernel

Notes: Regressions follow (2) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 2nd order polynomial and uniform/triangular kernels, following Cerulli (2016); Cerulli et al. (2017) methodology. TED corresponds to the difference(s) of the tangent lines at Running variable = 0. Running variable: 2011 per capita income, centralized at R$ 140. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Dependent variable: Had at least one birth in the period 2012-2015 (=1/0).

Figure 10: TED: Fuzzy RD discontinuity in the z-score birth spacing (2015), and tangent lines



(a) Uniform Kernel

(b) Triangular Kernel

Notes: Regressions follow (2) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 2nd order polynomial and uniform kernel, following Cerulli (2016); Cerulli et al. (2017) methodology. TED corresponds to the difference(s) of the tangent lines at Running variable = 0. Running variable: 2011 per capita income, centralized at R$ 140. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Dependent variable: Birth spacing z-score.

Appendix

Table A1: *Cadúnico*'s federal programs

Program	**Description**	**Target**
BPC	1 minimum wage per month	Elderly or Handicapped
Carteira do Idoso	Interstate transportation discounts	Elderly
Cestas Nutricionais para Gestantes	Complementary feeding program	Pregnant women in Alagoas State in food insecurity situations
PRONATEC	Free professional training	Registered individuals between 16 and 59 years old
PETI	Social activities and cash transfers through BF	Child labor vulnerable families below the poverty line.
Minha Casa, Minha Vida	Housing loans	Any registered family
Tarifa Social de Energia Elétrica	Electric Bills discounts	BPC's beneficiary families
Programa de Cisternas	Subsidized cisterns	Any registered family
Telefone Social	Telecommunication services discounts	Families below the poverty line
Fee isemption for examinations	Fee exemption for civil service entrance examination	Any registered individual
Projovem	Conditional cash transfer program	anyone between 15 to 29 years of age with educational deficits

Source: MDS (2011, 2015)

Table A2: Summary Statistics of Birth Spacing between Two Parities in the Cadúnico Database of 2011, Measured in Days

Starting Parity|Final Parity	**Median**	**Mean**	**Standard Deviation**	**N**
1|2	1,108	1,443	1,144	10,916,827
2|3	989	1,270	918	5,082,771
3|4	905	1,134	799	1,913,769
4|5	854	1,041	697	726,246
5|6	820	970	614	281,592
6|7	789	911	550	109,659
7|8	763	863	493	41,716
8|9	730	821	460	15,098
9|10	698	786	443	5,162

Notes: Data obtained in Cadúnico database of 2011. All Dadúnico observations are considered, independent of the year of registry. First year of registry in the data: 2001.

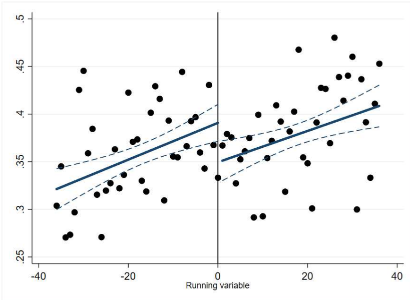
Table A3: Covariates comparison for the main group - above and below the poverty cutoff (R$ 140)

Variables	**Mean below the cutoff**	**Mean above the cutoff**	**Difference p-value**	**N^o Observations**
Race: White	0.36	0.38	0.01	15,274
Race: Black-Brazilian	0.10	0.09	0.37	15,274
Race: Oriental	0.01	0.01	0.27	15,274
Race: Pardo-Brazilian	0.53	0.51	0.00	15,274
Race: Indigenous	0.00	0.00	0.56	15,274
Race: Missing	0.00	0.00	0.04	15,274
Work: Worked in the last week	0.34	0.37	0.76	15,274
Work: Missing	0.36	0.28	0.00	15,274
Education: None or some Primary education (<4yrs of education)	0.11	0.09	0.85	15,274
Education: Primary Incomplete	0.35	0.32	0.16	15,274
Education: Some High School education	0.26	0.26	0.28	15,274
Education: High school or higher	0.28	0.33	0.53	15,274
Education: Missing	0.00	0.00	0.43	15,274
Live with spouse	0.52	0.60	0.01	15,274
Age	29.96	30.31	0.06	15,274
Have disabilities	0.01	0.01	0.87	15,274
Number of Children	2.16	2.04	0.00	15,274
Birth spacing z-score (2011)	0.15	0.25	0.31	9,863
Household: Urban	0.88	0.90	0.10	15,274
Household: Number of rooms	4.16	4.30	0.63	14,923
Household: Piped water	0.87	0.89	0.16	15,274
Household: Piped water (missing info)	0.02	0.02	0.36	15,274
Household: Have bathroom	0.95	0.95	0.54	15,274
Household: Have bathroom (missing info)	0.02	0.02	0.36	15,274
Water: Supply network	0.80	0.83	0.05	15,274
Water: Artesian Aquifer	0.14	0.13	0.14	15,274
Water: Others	0.04	0.02	0.01	15,274
Water: Missing	0.02	0.02	0.36	15,274
Wastewater: Sewer system	0.56	0.60	0.09	15,274
Wastewater: Septic tank	0.36	0.33	0.40	15,274
Wastewater: Directly discharged at water sources	0.03	0.03	0.06	15,274
Wastewater: Others	0.01	0.00	0.25	15,274
Wastewater: Missing	0.04	0.04	0.84	15,274
Garbage: Collected	0.82	0.85	0.09	15,274
Garbage: Burned or Buried	0.07	0.06	0.05	15,274
Garbage: Discharged at open air	0.01	0.01	0.15	15,274
Garbage: Others	0.08	0.06	0.48	15,274
Electricity: Electric supply network	0.93	0.95	0.37	15,274
Electricity: Generators	0.00	0.00	0.64	15,274
Electricity: Candles	0.00	0.00	0.74	15,274
Electricity: Others	0.04	0.03	0.81	15,274
Electricity: Missing	0.02	0.02	0.36	15,274
Income: Receive pension	0.13	0.11	0.01	15,274
Income: Receive pension (missing)	0.41	0.34	0.00	15,274
Income: Donations	0.07	0.05	0.69	15,274
Income: Donations (missing)	0.45	0.37	0.00	15,274
Income: Unemployment benefits	0.00	0.01	0.40	15,274
Income: Unemployment benefits (missing)	0.45	0.37	0.00	15,274
Income: other sources than job related	0.18	0.11	0.41	15,274
Income: other sources than job related (missing)	0.31	0.29	0.00	15,274
Region: North	0.09	0.07	0.07	15,274
Region: Northeast	0.23	0.19	0.15	15,274

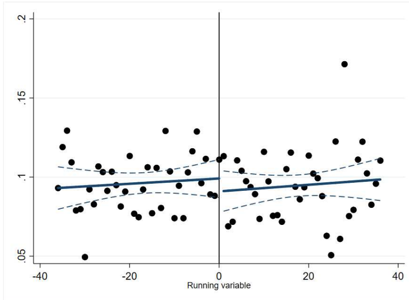
Region: Central-West	0.09	0.11	0.07	15,274
Region: Southeast	0.45	0.47	0.95	15,274
Region: South	0.15	0.15	0.00	15,274

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel, and heteroscedastic-robust errors. Bandwidths: R$ 36 for the main group and R$ 32 for birth spacing group. Running variable: 2011 per capita income, centralized at R$ 140. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. Difference p-value corresponds to the p-value obtained for γ_0 in equation (3).

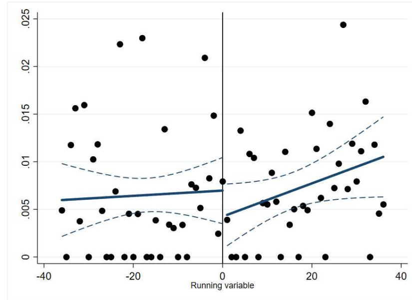
Figure A1: Covariate Balancing - Main group



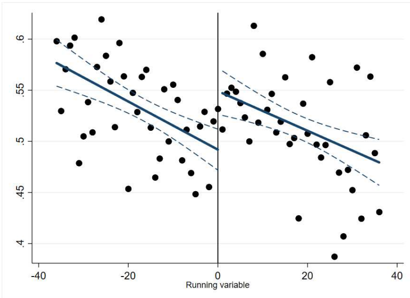
(a) Race: White



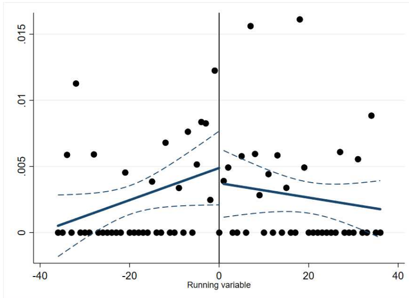
(b) Race: Black-Brazilian



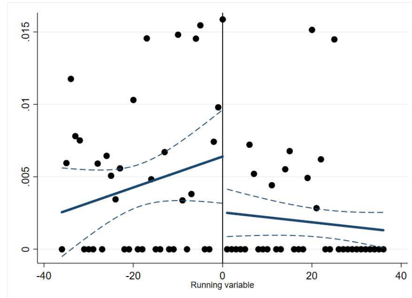
(c) Race: Oriental



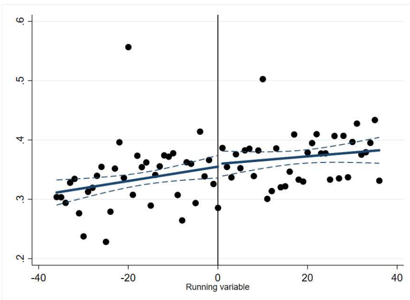
(d) Race: Pardo-Brazilian



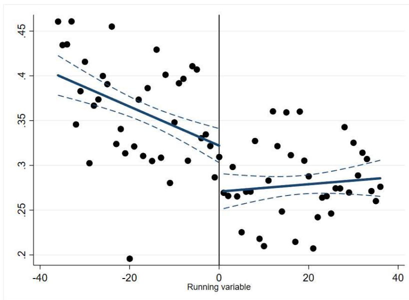
(e) Race: Indigenous



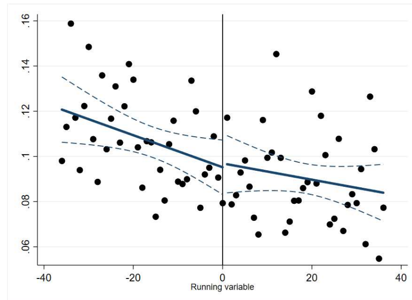
(f) Race: Missing



(g) Work: Worked in the last week



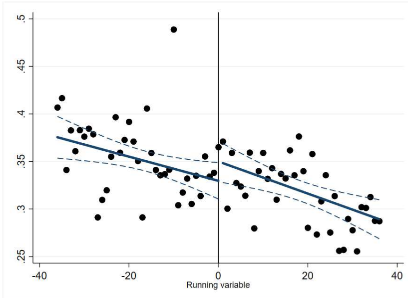
(h) Work: Missing



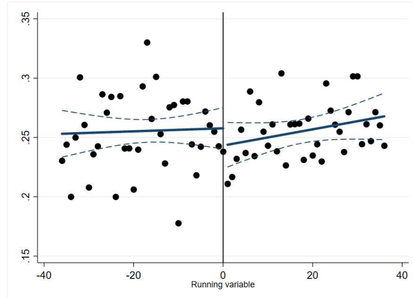
(i) Education: None or some primary education

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

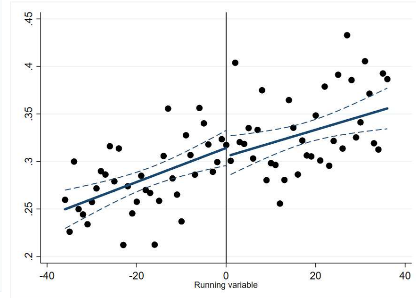
Figure A2: Covariate Balancing - Main group



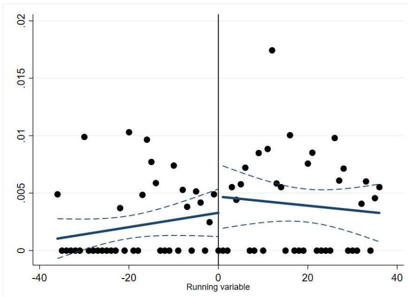
(a) Education: Primary Incomplete



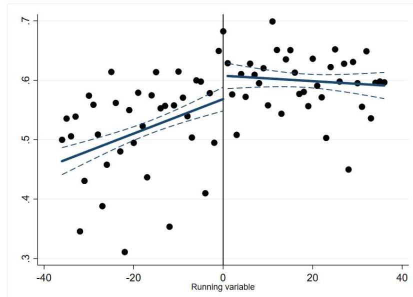
(b) Education: Some High School education



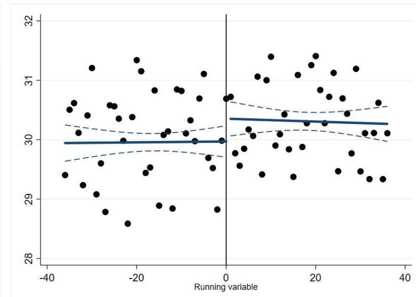
(c) Education: High school or higher



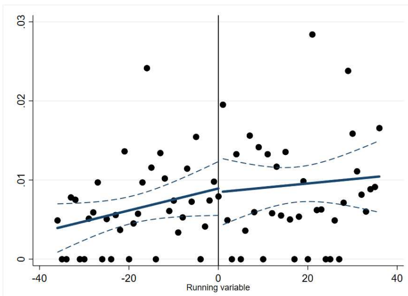
(d) Education: Missing



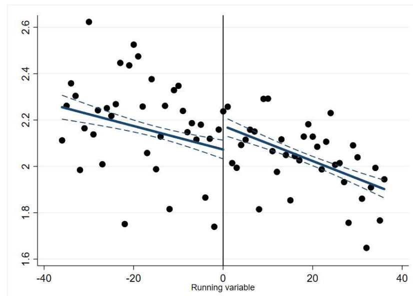
(e) Live with spouse



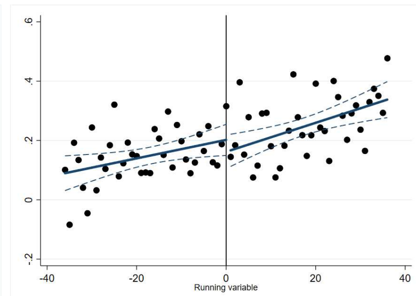
(f) Age



(g) Have disabilities



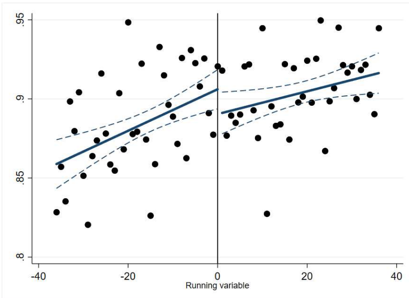
(h) Number of Children



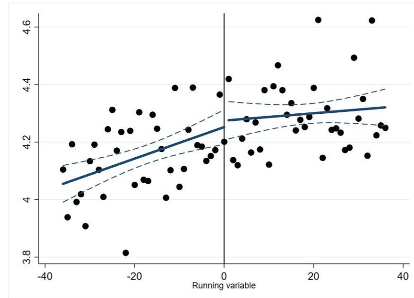
(i) Birth spacing z-score (2011)

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

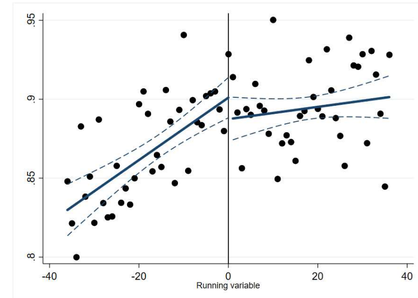
Figure A3: Covariate Balancing - Main group



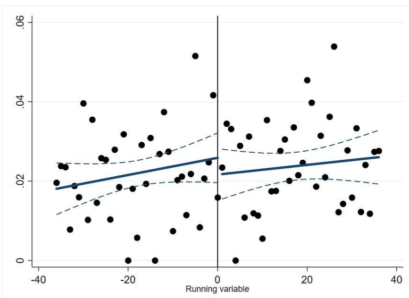
(a) Household: Urban



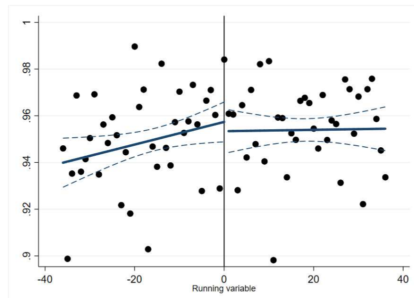
(b) Household: Number of rooms



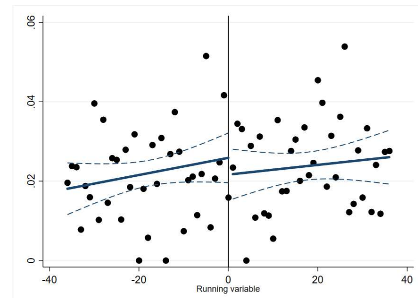
(c) Household: Piped water



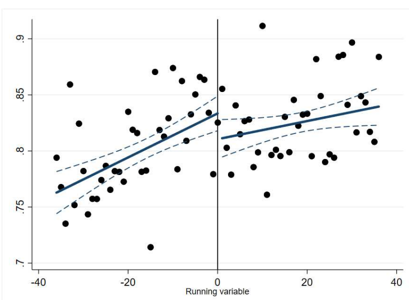
(d) Household: Piped water (missing info)



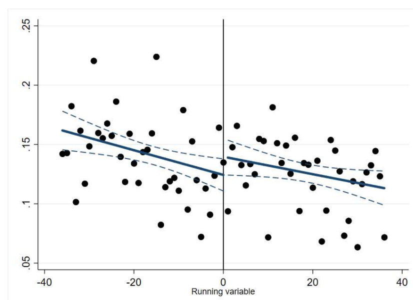
(e) Household: Has bathroom



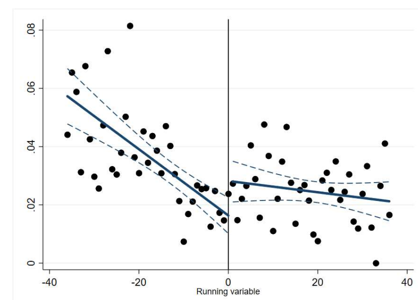
(f) Household: Has bathroom (missing info)



(g) Water: Supply network



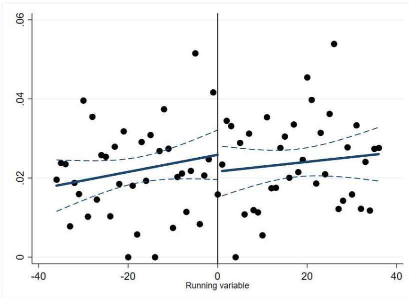
(h) Water: Artesian Aquifer



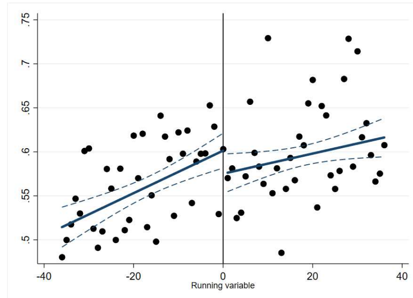
(i) Water: Others

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

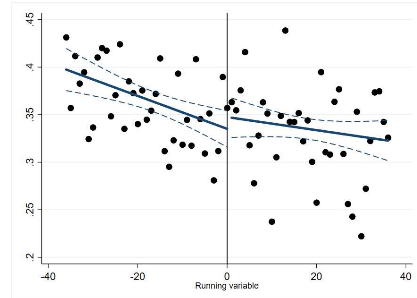
Figure A4: Covariate Balancing - Main group



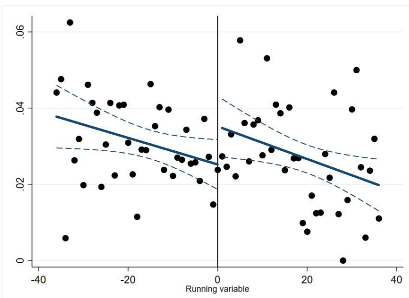
(a) Water: Missing



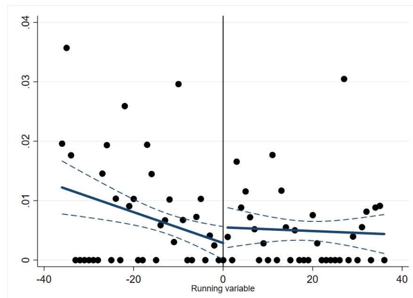
(b) Wastewater: Sewer system



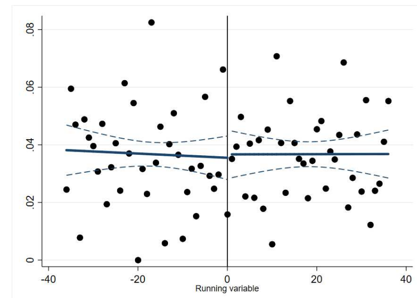
(c) Wastewater: Septic tank



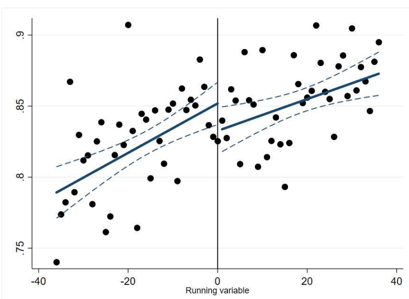
(d) Wastewater: Directly discharged at water sources



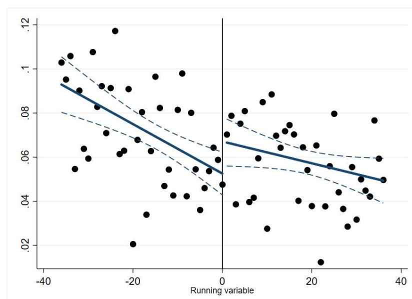
(e) Wastewater: Others



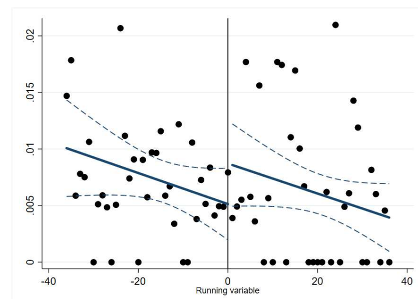
(f) Wastewater: Missing



(g) Garbage: Collected



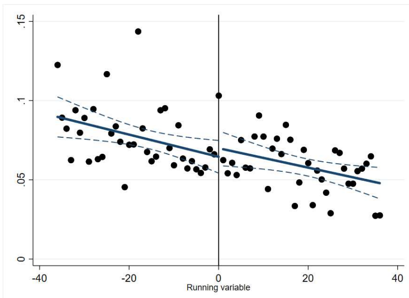
(h) Garbage: Burned or buried



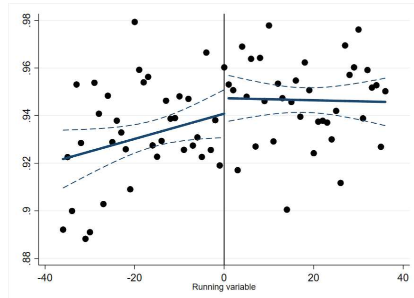
(i) Garbage: Discharged at open air

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R\$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

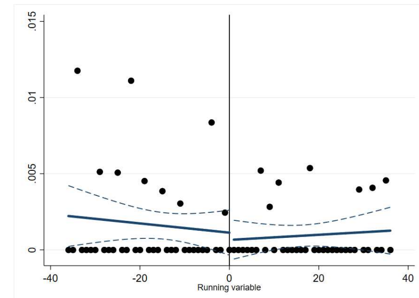
Figure A5: Covariate Balancing - Main group



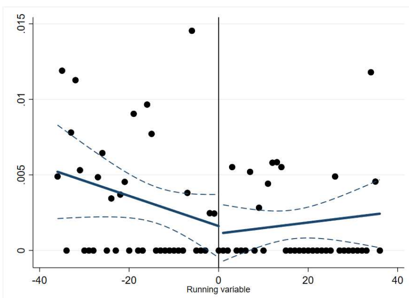
(a) Garbage: Others



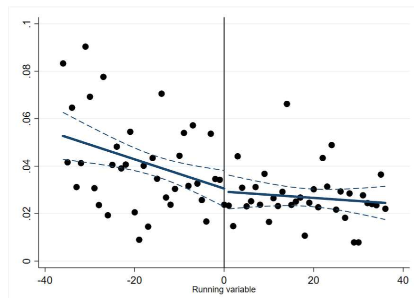
(b) Electricity: Electric supply network



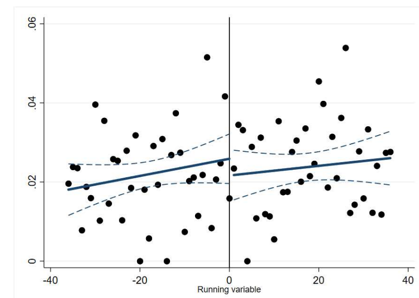
(c) Electricity: Generators



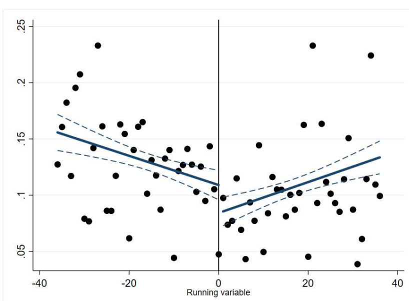
(d) Electricity: Candles



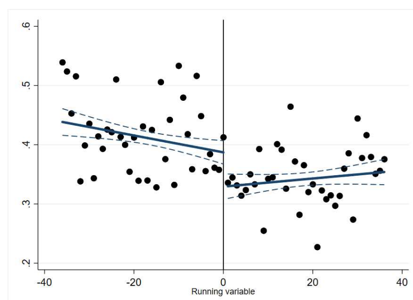
(e) Electricity: Others



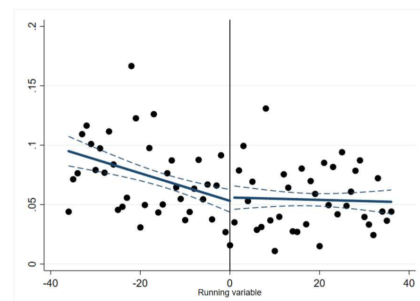
(f) Electricity: Missing



(g) Income: Receives pension



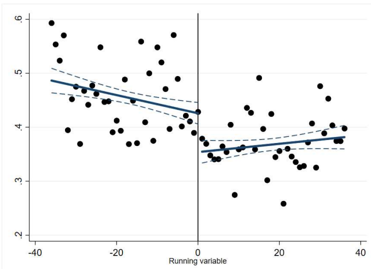
(h) Income: Receives pension (missing)



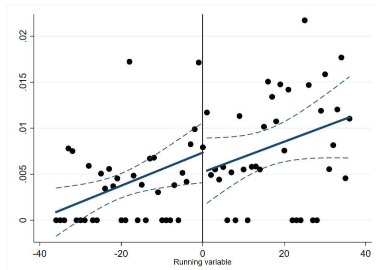
(i) Income: Donations

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

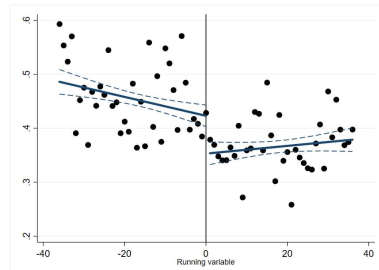
Figure A6: Covariate Balancing - Main group



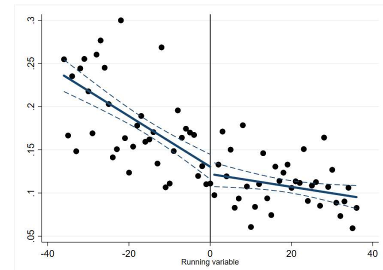
(a) Income: Donations (missing)



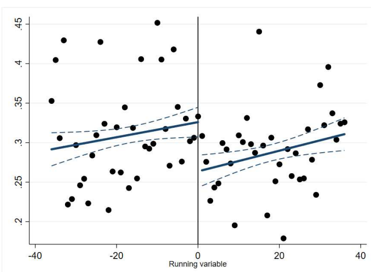
(b) Income: Unemployment benefits



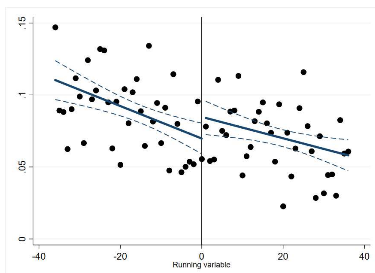
(c) Income: Unemployment benefits (missing)



(d) Income: other sources than job related



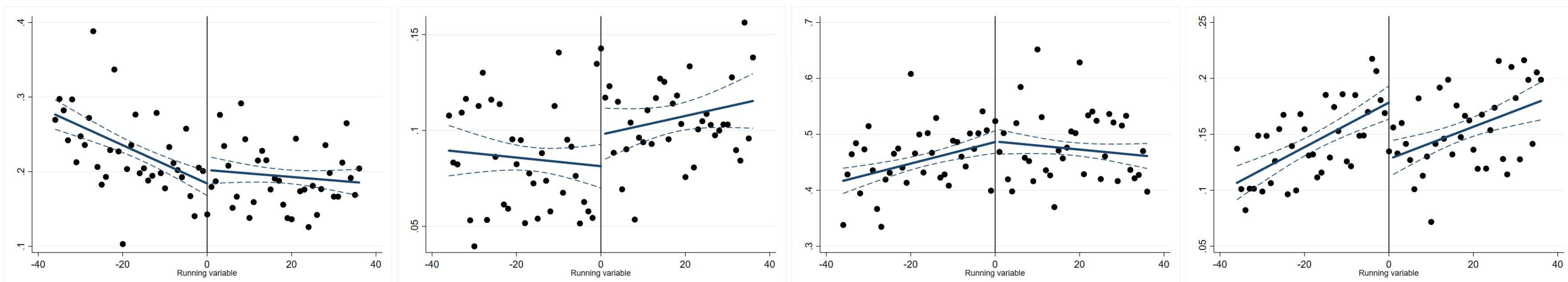
(e) Income: other sources than job related (missing)



(f) Region: North

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N° of observations.

Figure A7: Covariate Balancing - Main group



(a) Region: Northeast (b) Region: Central-West (c) Region: Southeast (d) Region: South

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Running variable: 2011 per capita income, centralized at R$ 140. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family. See table A3 for p-values and N^{o} of observations.

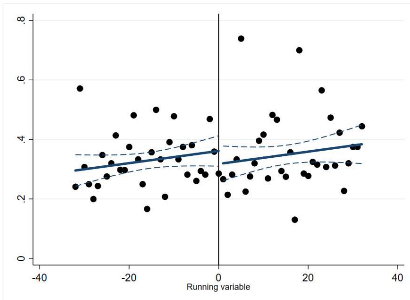
Table A4: Covariates Comparison for the birth spacing group - Above and below the poverty cutoff (R$ 140)

Variables	**Mean below the cutoff**	**Mean above the cutoff**	**Difference p-value**	**Nº Observations**
Race: White	0.33	0.35	0.28	2,030
Race: Black-Brazilian	0.10	0.11	0.97	2,030
Race: Oriental	0.01	0.01	0.68	2,030
Race: Pardo-Brazilian	0.56	0.52	0.33	2,030
Race: Indigenous	0.00	0.01	0.32	2,030
Race: Missing	0.00	0.00	0.98	2,030
Work: Worked in the last week	0.32	0.35	0.75	2,030
Work: Missing	0.35	0.27	0.01	2,030
Education: None or some Primary education (<4yrs of education)	0.09	0.08	0.96	2,030
Education: Primary Incomplete	0.40	0.36	0.32	2,030
Education: Some High School education	0.29	0.28	0.67	2,030
Education: High school or higher	0.21	0.27	0.34	2,030
Education: Missing	0.00	0.01	0.08	2,030
Live with spouse	0.50	0.57	0.83	2,030
Age	26.18	26.38	0.75	2,030
Have disabilities	0.01	0.00	0.34	2,030
Number of Children	1.97	1.89	0.99	2,030
Birth spacing z-score (2011)	-0.08	-0.04	0.51	1,092
Household: Urban	0.88	0.87	0.11	2,030
Household: Number of rooms	3.94	4.06	0.22	1,981
Household: Piped water	0.84	0.87	0.55	2,030
Household: Piped water (missing info)	0.02	0.03	0.17	2,030
Household: Have bathroom	0.93	0.94	0.51	2,030
Household: Have bathroom (missing info)	0.02	0.03	0.17	2,030
Water: Supply network	0.79	0.79	0.02	2,030
Water: Artesian Aquifer	0.14	0.16	0.08	2,030
Water: Others	0.05	0.03	0.69	2,030
Water: Missing	0.02	0.03	0.17	2,030
Wastewater: Sewer system	0.55	0.56	0.01	2,030
Wastewater: Septic tank	0.35	0.35	0.10	2,030
Wastewater: Directly discharged at water sources	0.04	0.03	0.14	2,030
Wastewater: Others	0.01	0.01	0.21	2,030
Wastewater: Missing	0.05	0.05	0.55	2,030
Garbage: Collected	0.83	0.82	0.00	2,030
Garbage: Burned or Buried	0.07	0.07	0.05	2,030
Garbage: Discharged at open air	0.01	0.01	0.13	2,030
Garbage: Others	0.06	0.08	0.02	2,030
Electricity: Electric supply network	0.93	0.94	0.56	2,030
Electricity: Generators	0.00	0.00	0.47	2,030
Electricity: Candles	0.01	0.00	0.32	2,030
Electricity: Others	0.04	0.03	0.56	2,030
Electricity: Missing	0.02	0.03	0.17	2,030
Income: Receive pension	0.14	0.10	0.28	2,030
Income: Receive pension (missing)	0.39	0.33	0.13	2,030
Income: Donations	0.10	0.08	0.42	2,030
Income: Donations (missing)	0.43	0.35	0.02	2,030
Income: Unemployment benefits	0.00	0.01	0.19	2,030
Income: Unemployment benefits missing)	0.43	0.35	0.02	2,030
Income: other sources than job related	0.19	0.13	0.34	2,030
Income: other sources than job related (missing)	0.29	0.27	0.16	2,030
Region: North	0.11	0.08	0.31	2,030
Region: Northeast	0.23	0.18	0.29	2,030

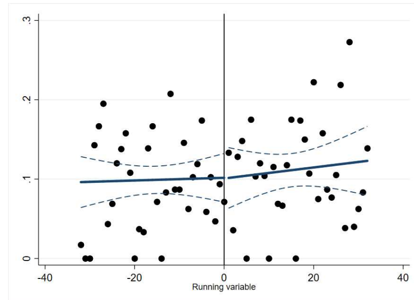
Region: Central-West	0.08	0.12	0.53	2,030
Region: Southeast	0.41	0.45	0.59	2,030
Region: South	0.16	0.17	0.52	2,030

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel, and heteroscedastic-robust errors. Bandwidths: R$ 36 for the main group and R$ 32 for birth spacing group. Running variable: 2011 per capita income, centralized at R$ 140. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth in the 2012-2015 period. Difference p-value corresponds to the p-value obtained for γ_0 in equation (3).

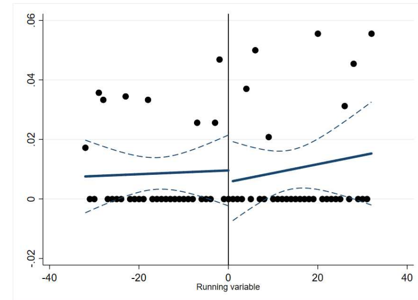
Figure A8: Covariate Balancing - Birth spacing group



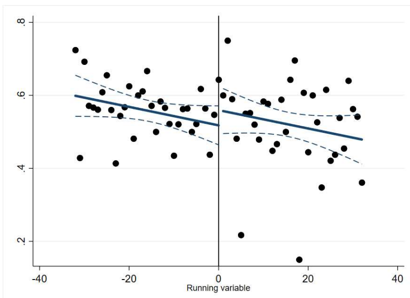
(a) Race: White



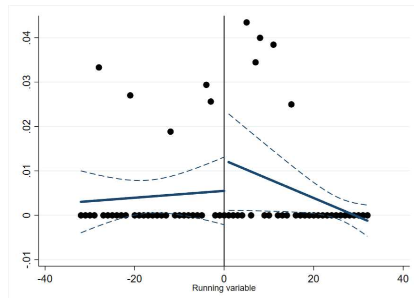
(b) Race: Black-Brazilian



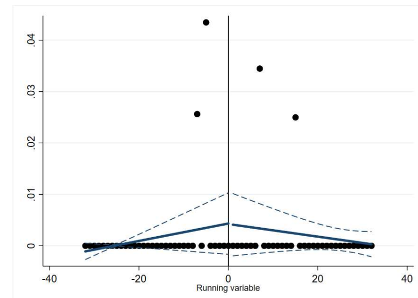
(c) Race: Oriental



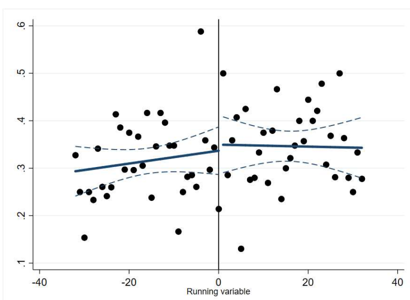
(d) Race: Pardo-Brazilian



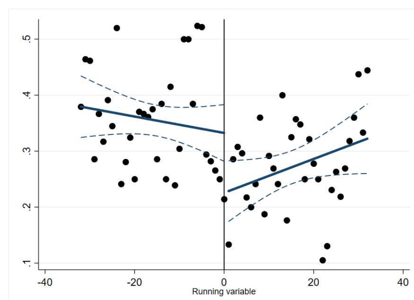
(e) Race: Indigenous



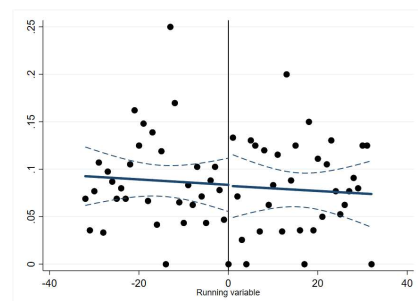
(f) Race: Missing



(g) Work: Worked in the last week



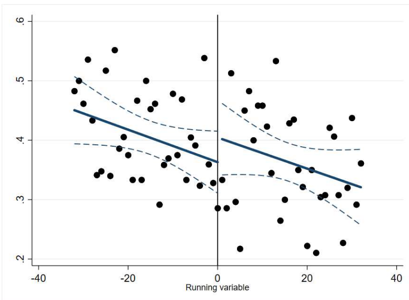
(h) Work: Missing



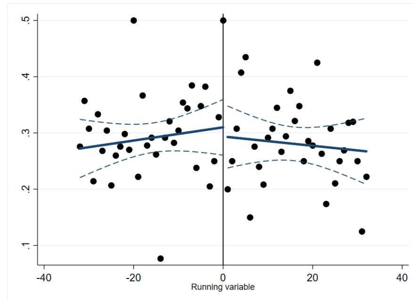
(i) Education: None or some primary education

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

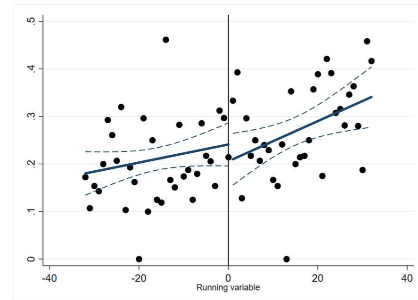
Figure A9: Covariate Balancing - Birth spacing group



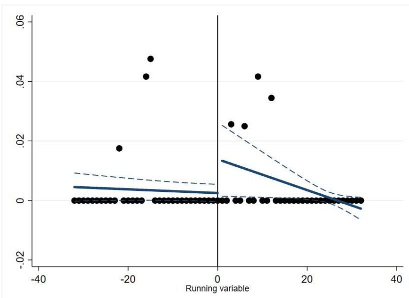
(a) Education: Primary Incomplete



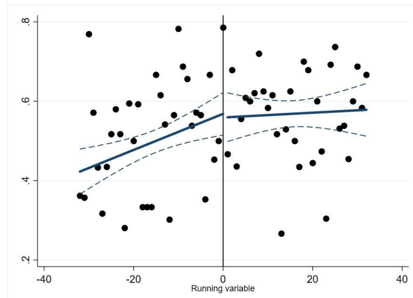
(b) Education: Some High School education



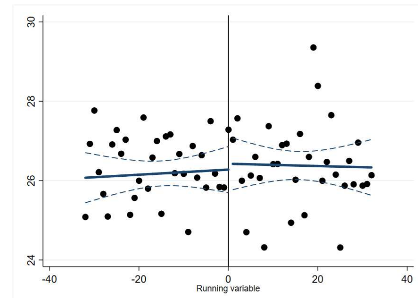
(c) Education: High school or higher



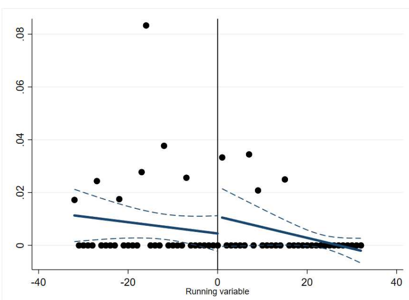
(d) Education: Missing



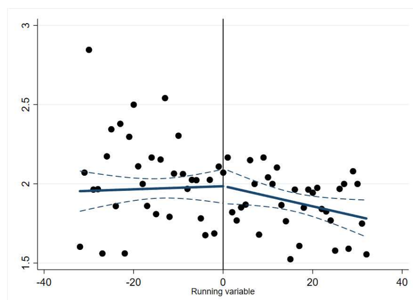
(e) Live with spouse



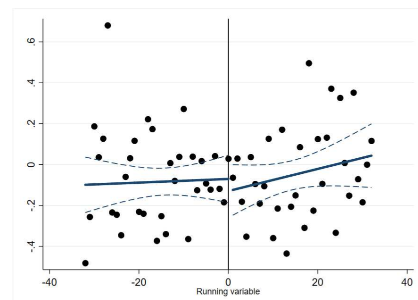
(f) Age



(g) Have disabilities



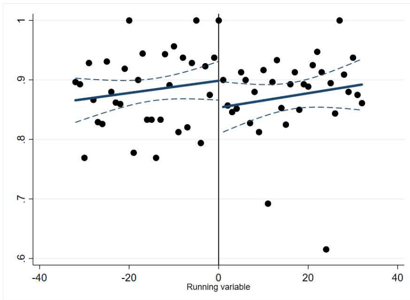
(h) Number of Children



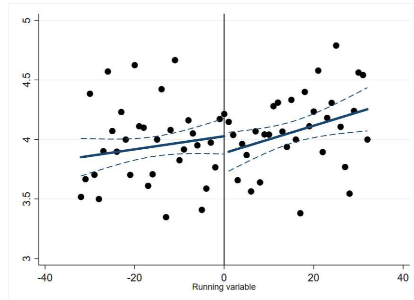
(i) Birth spacing z-score (2011)

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

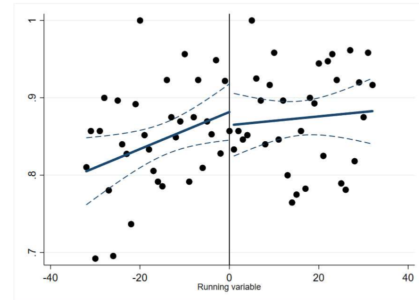
Figure A10: Covariate Balancing - Birth spacing group



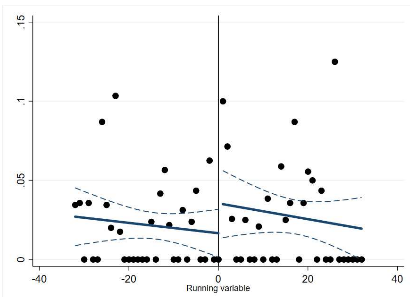
(a) Household: Urban



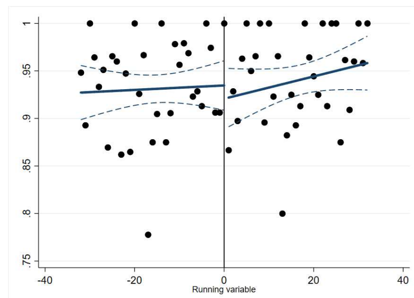
(b) Household: Number of rooms



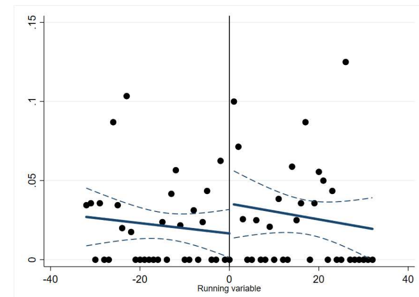
(c) Household: Piped water



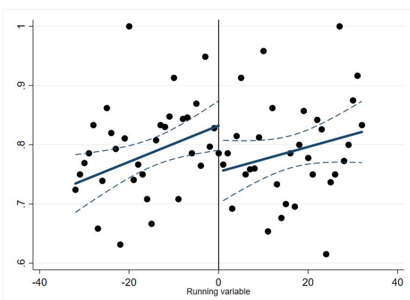
(d) Household: Piped water (missing info)



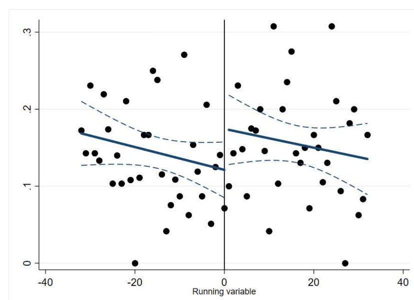
(e) Household: Has bathroom



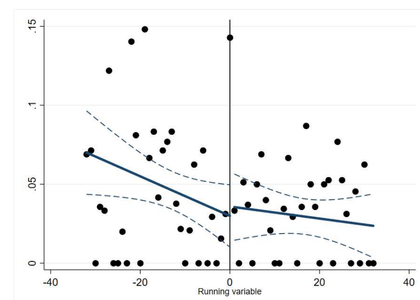
(f) Household: Has bathroom (missing info)



(g) Water: Supply network



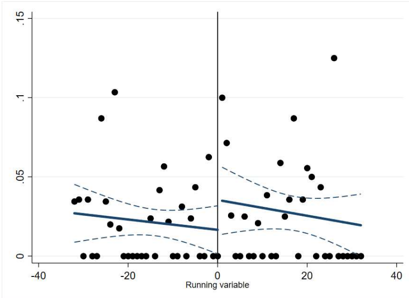
(h) Water: Artesian Aquifer



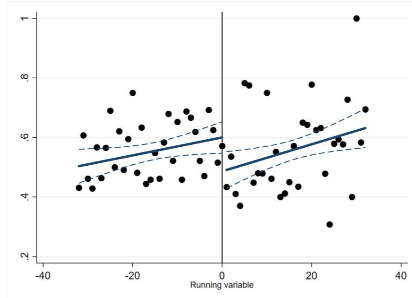
(i) Water: Others

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

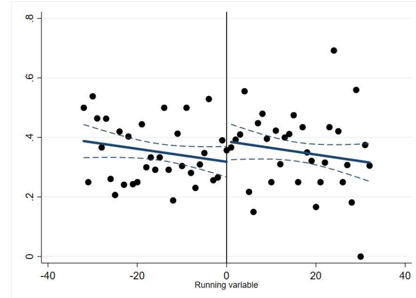
Figure A11: Covariate Balancing - Birth spacing group



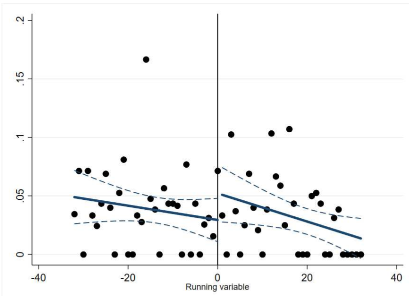
(a) Water: Missing



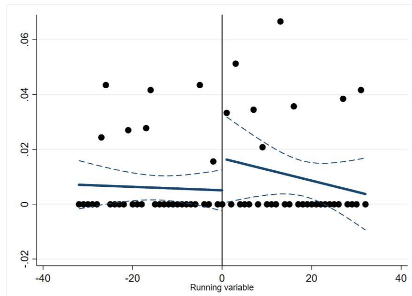
(b) Wastewater: Sewer system



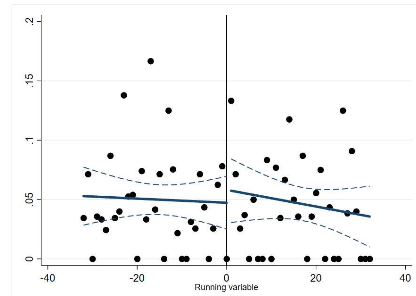
(c) Wastewater: Septic tank



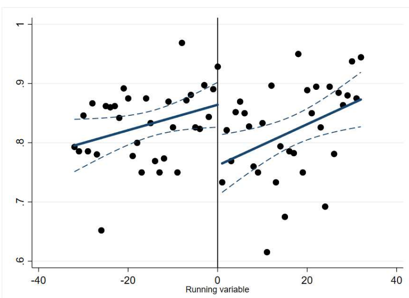
(d) Wastewater: Directly discharged at water sources



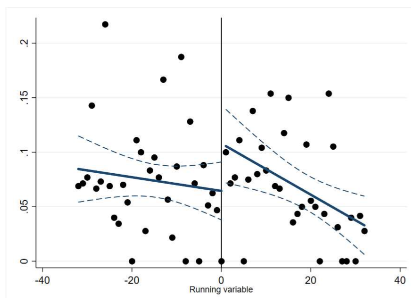
(e) Wastewater: Others



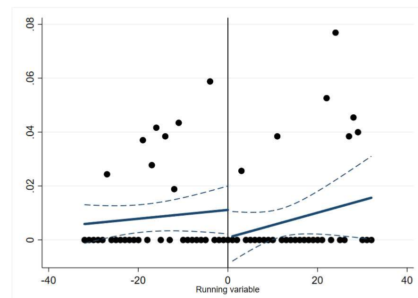
(f) Wastewater: Missing



(g) Garbage: Collected



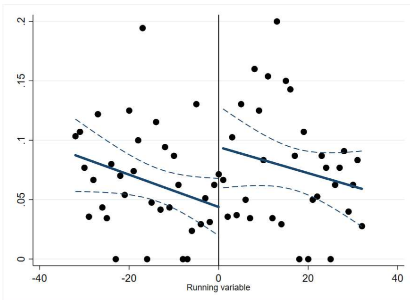
(h) Garbage: Burned or buried



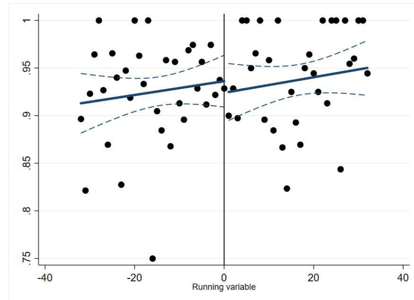
(i) Garbage: Discharged at open air

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

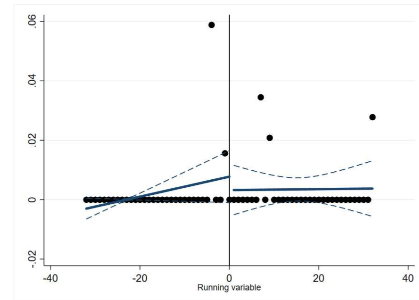
Figure A12: Covariate Balancing - Birth spacing group



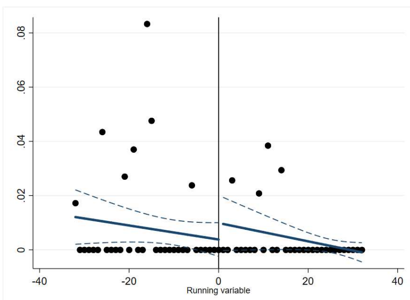
(a) Garbage: Others



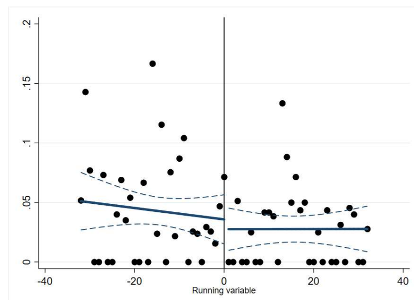
(b) Electricity: Electric supply network



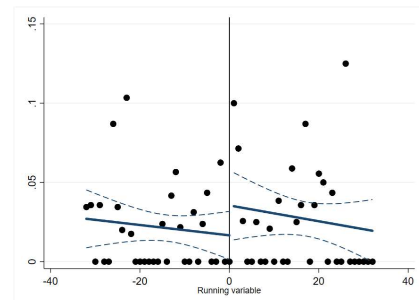
(c) Electricity: Generators



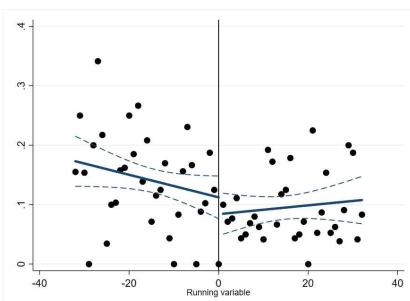
(d) Electricity: Candles



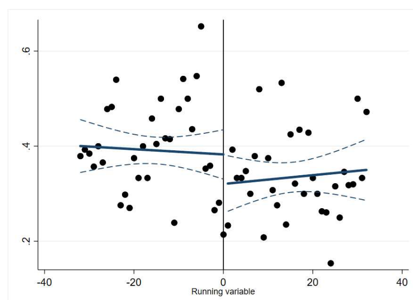
(e) Electricity: Others



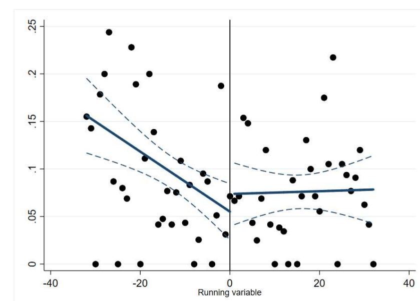
(f) Electricity: Missing



(g) Income: Receives pension



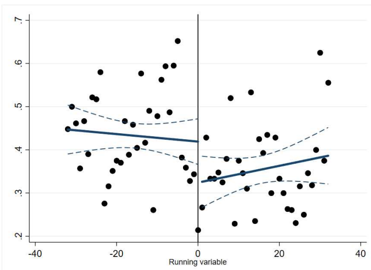
(h) Income: Receives pension (missing)



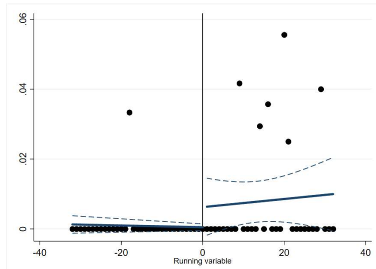
(i) Income: Donations

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

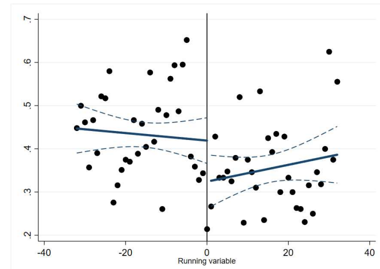
Figure A13: Covariate Balancing - Birth spacing group



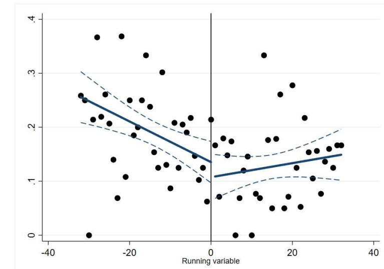
(a) Income: Donations (missing)



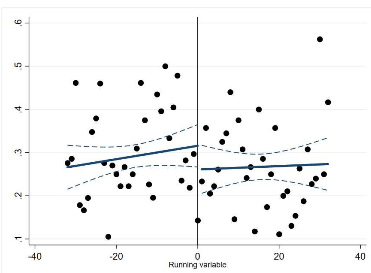
(b) Income: Unemployment benefits



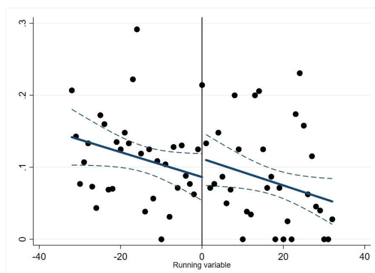
(c) Income: Unemployment benefits (missing)



(d) Income: other sources than job related



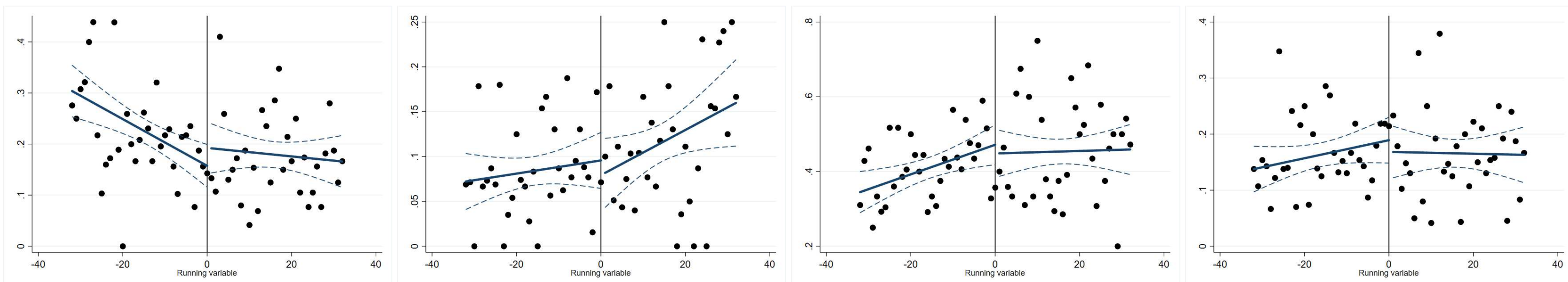
(e) Income: other sources than job related (missing)



(f) Region: North

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N° of observations.

Figure A14: Covariate Balancing - Birth spacing group



(a) Region: Northeast (b) Region: Central-West (c) Region: Southeast (d) Region: South

Notes: All regressions follow (3) specification, within the bandwidths calculated by Calonico et al. (2014) methodology, for a 1st order polynomial and uniform kernel. Running variable: 2011 per capita income, centralized at R$ 140. Dashed lines corresponds to 95% confidence interval, with heteroscedastic-robust errors. Bin size: 1. Unit of observation: women of 16-44 years of age, with at least one child, entered in Cadúnico in 2011 as the head of the family, conditional on having at least one birth during 2012-2015. See table A4 for p-values and N^{o} of observations.

Data Appendix

Table B1: CCT and UCT combination values (nominal R$) for benefit type identification, from 2011 to 2015

	Months (year)			
Benefit combinations	Jan-Mar (2011)	Apr-Oct (2011)	Nov(2011) - May(2014)	May(2014) - Dec(2015)
CCT combinations				
5 Bvar + 2 BVJ	-	-	236	259
5 Bvar + 1 BVJ	-	-	198	217
5 Bvar	-	-	160	175
4 Bvar + 2 BVJ	-	-	204	224
4 Bvar + 1 BVJ	-	-	166	182
4Bvar	-	-	128	140
3 Bvar + 2 BVJs	132	172	172	189
3 Bvar + 1 BVJ	99	134	134	147
3 Bvar	66	96	96	105
2Bvar + 2 BVJs	110	140	140	154
2 Bvar + 1 BVJs	77	102	102	112
2 Bvar	44	64	64	70
1 Bvar + 2 BVJs	88	108	108	119
1 Bvar + 1 BVJ	55	70	70	77
1 Bvar	22	32	32	35
2 BVJ	66	76	76	84
1 BVJ	33	38	38	42
UCT	68	70	70	77
UCT + CCT combinations				
UCT + 5 Bvar + 2 BVJ	-	-	306	329
UCT + 5 Bvar + 1 BVJ	-	-	268	287
UCT + 5 Bvar	-	-	230	245
UCT + 4 Bvar + 2 BVJ	-	-	274	294
UCT + 4 Bvar + 1 BVJ	-	-	236	252
UCT + 4 Bvar	-	-	198	210
UCT + 3 Bvar + 2 BVJs	200	242	242	259
UCT + 3 Bvar + 1 BVJ	167	204	204	217
UCT + 3 Bvar	134	166	166	175
UCT + 2Bvar + 2 BVJs	178	210	210	224
UCT + 2 Bvar + 1 BVJs	145	172	172	182
UCT + 2 Bvar	112	134	134	140
UCT + 1 Bvar + 2 BVJs	156	178	178	189
UCT + 1 Bvar + 1 BVJ	123	140	140	147
UCT + 1 Bvar	90	102	102	105
UCT + 2 BVJ	134	146	146	154
UCT + 1 BVJ	101	108	108	112

Notes: Unitary payment values can be found in Table 2. Payment values change dates are set by law, and the change dates can be found in MDS (2011, 2015). Anyone that received a positive amount in a month is considered to be a BF beneficiary in said month. If a payment cannot be identified as a combination in the table, the receiver is still a BF beneficiary, but not a CCT or UCT recipient. Additionally, if a payment is identified more than once, for example, as CCT and UCT + CCT (i.e., an identity conflict), then the payment is considered as not identified. The selection process for BF takes an average of four months, so we extended the allocation window of 2011 until April 2012 - using information on the 2012 Cadúnico - for women that registered between October and December 2011. For instance, women that registered in November and started receiving a BF payment on February 2012 were considered to be in the treatment group.