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Economic and Behavioral Effects of Safety Net Programs

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Summary

This thesis contains three independent but interrelated articles. Essentially, in this work, I empirically investigate the economic and behavioral effects of safety net programs. To do so, I use econometrics techniques allowing for causal inference, combined with high-quality administrative data sets. Below follows a brief description of the three articles that make up this thesis.

I start by studying the behavioral effects of a large-scale Conditional Cash Transfer program, the Brazilian *Bolsa Família* (BF), on voting outcomes. Using a unique database on BF beneficiaries and where they vote, I explore random variation in program coverage among polling stations, a highly disaggregated level of observation with fewer than 400 registered voters. The findings indicate that the cash transfers positively affected voter turnout. I also find a positive effect on the support for the incumbent party that implemented and expanded the program. This positive effect comes from more people participating in the election but also from voters switching their choices, conditional on turnout. The electoral rewards to the incumbent candidate are mostly led by those beneficiaries who entered more recently in the program and the amount of money transferred matters for voting behavior.

In the second paper, we examine the effectiveness of cash assistance targeted to disadvantaged youth. We exploit an exogenous variation in the provision of cash transfers from BF Program in Brazil to credibly identify how an additional year of exposure at the critical age of 18 impacts on educational, labor market, and economic self-sufficiency outcomes. We do not find evidence of significant effects of additional exposure to the program on educational attainment and economic self-sufficiency. However, we observe a small (but still positive) impact on school enrollment, which is mostly driven by male beneficiaries. We also find effects on formal labor supply only for men. For them, we show that one additional exposure to the program decreases the probability of working in the formal sector by 5.38 percentage points during the extra year of exposure. Five years later, this pattern reverses to an increase in participation in the formal labor force.

Lastly, the third paper studies the effects of a maternity leave extension on labor market outcomes of women in Brazil, using detailed information on workers and firms in the formal labor market. Taking advantage of the exact leave taking dates and the staggered implementation of the extended leave policy across firms, our analysis compares outcomes within firms before and after the eligibility cutoff. While eligible women could have extended their leave period by 50% (from 120 to 180 days), take up only increases by 13 p.p. Also, employment effects are confined to the maternity leave extension spell, with no permanent effects on employment in the long run. Taken together, our findings indicate that this policy privileges a selected group of workers, while it is not able to retain them in the workforce.

Conditional Cash Transfers and Voting Behavior

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April 5, 2018

Abstract

This paper investigates the effects of a large-scale Conditional Cash Transfer program, the Brazilian *Bolsa Família* (BF), on voting behavior. Using a unique database on BF beneficiaries and where they vote, we explore random variation in program coverage among polling stations, a highly disaggregated level of observation with fewer than 400 registered voters. The findings indicate that the cash transfers positively affected voter turnout. We also find a positive effect on the support for the incumbent party that implemented and expanded the program. This positive effect comes from more people participating in the election but also from voters switching their choices, conditional on turnout. The electoral rewards to the incumbent candidate are mostly led by those beneficiaries who entered more recently in the program and the amount of money transferred matters for voting behavior.

Keywords: *Bolsa Família*, voting behaviour, Conditional Cash Transfers.

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1 Introduction

Over the past two decades, Conditional Cash Transfers (CCTs) have become the main strategy for poverty reduction and social protection in the world, especially in developing countries. Although the characteristics of CCTs programs differ among countries, they usually have the dual objectives of reducing poverty in the short-run, by providing cash assistance, and suppressing the transmission of poverty through generations in the long-run, by encouraging parents to invest in the health and education of their children. Given the remarkable scale and velocity in which these programs have been expanded around the world, numerous studies have explored the causal impacts of cash transfer programs on a variety of outcomes and contexts.[1]

Despite the extensive literature on CCTs programs, there is considerably less evidence of whether and how CCTs influence individual voting behavior, an inherent and unintended effect of any social policy. But the scarcity of research studying the causal impacts of CCTs on voting comes with little surprise, considering the challenge to access actual voting at the individual level as well as the caveats to address causal analysis. First of all, voting is secret in a large majority of countries, rendering individual voting unobservable. Secondly, most CCTs have universal targeting, and empirical evaluation, in this case, may be confounded by unobserved characteristics that influence simultaneously voting outcomes and enrolment in the program.

The political economy literature has already suggested several potential mechanisms regarding the relationship between social policies and political behavior of voters and politicians. For instance, since voters are self-interested, their political choices may be affected by the benefits they have received in the past (Kramer [1971]; Hibbs Jr et al. [1982]), by the fear of losing such benefits (Manacorda et al. [2011]) or by the feeling of reciprocity (Finan and Schechter [2012]). On the other hand, assuming that people

[1] To see results of CCTs on **education**, for instance, see Galasso [2006], Attanasio et al. [2011], Schultz [2004],Glewwe and Olinto [2004], Maluccio and Flores [2005], Filmer and Schady [2008], Benhassine et al. [2015], Barrera-Osorio et al. [2011]; Filmer and Schady [2011], Chaudhury and Parajuli [2010]. For the CCT effects on **health** see Shei et al. [2014], Gertler [2000], Glassman et al. [2013] Paxson and Schady [2005], Paxson and Schady [2010], Ranganathan and Lagarde [2012], Robertson et al. [2013]. See Alatas et al. [2012] and Ravallion [2009] for results of CCts on **poverty** reduction and Barros et al. [2006] to see effects on **inequality**. Attanasio and Mesnard [2006], Angelucci and Attanasio [2009] and Hoddinott and Skoufias [2004] report results on **consuption**. Edmonds and Schady [2012] explore effects on **child work**. For results on **adult labor supply**, see Parker and Skoufias [2000] and Skoufias and Di Maro [2008] and effects on **fertility** see Stecklov et al. [2006].

care about their peers, voters' choices can be influenced by the personal perception on the politicians' preferences for redistribution (Drazen and Eslava [2010]) as well as their competence to implement redistributive policies or even by the voting preferences of other voters (Manski [1993]; Stokes [2005]; Gerber et al. [2008]). In a medium- or long-term horizon, social policies also can shape the political orientation of voters (Edlund and Pande [2002]; Cantoni et al. [2017] and Ferreira and Gyourko [2009]). From the politician's side, it is possible to create or increase their own political support, by strategically allocating resources (Camacho and Conover [2011]; Fujiwara [2015]), by manipulating the program (Brollo et al. [2017]) or by using the program in a clientelistic way (De La and Ana [2013]; Robinson and Verdier [2013]).

In this paper we explore a highly disaggregated level of observation on actual voting, allowing to circumvent the spurious correlation between welfare dependence and voting and also examine the voting behavior as close as possible to the individual level. We first investigate whether and how a central government policy targeted to a specific and well-defined group affects the civic participation of individuals and their voting choices. We also study the effects of CCTs on the support for the incumbent candidate, separating the results of more people participating in the election from the voters shifting their choices between candidates. Other interests of this paper are to investigate whether the exposure time to CCTs matters for voting behavior as well as which voters are the most reciprocal. Ultimately, we evaluate in which extend the changes in the amount of money differ, regarding its impacts, from the movements of more people entering and leaving the program over time.

Bringing light to these issues, we study the case of Brazil, which is the world's fourth-largest democracy, with over 142 million voters, and also have the largest CCT program in the world, the Brazilian *Bolsa Família*[2] (hereafter referred as BF Program). Since its beginning, in 2004, BF Program has experienced substantial expansions and almost quadrupled the number of households enrolled in just over a decade. Currently, it reaches

[2]The *Bolsa Família* (BF) program, established in 2003 by President Lula, integrated four existing CCT programs. Soares (2012) summarizes previous studies and evidence that the BF program has achieved its main objectives by improving the quality of life for poor people without creating significant negative externalities. The BF program has been studied under many perspectives, such as: inequality (Hoffmann, [2010]), poverty (Soares et al., [2010]), schooling (Santarrosa, [2011]), nutrition (Segall-Corrêa et al., [2008]), work (Ribas and Soares, [2010]), infant mortality (Camelo et al., [2009]), fertility (Rocha, [2009]), crime (Chioda et al., [2016]), child labor (Ferro and Kassouf, [2005]), etc.

nearly 14 million households with around 50 million people (a quarter of the total population of the country). Subsequent expansions also increased the amount of money given to low-income families.

We construct a unique panel dataset at the polling station level, which has fewer than 400 registered voters. By matching three sources of administrative data, we calculate the BF Program coverage at the polling station level as well as a set of voter characteristics[3] and detailed information about the election outcomes[4] for two presidential elections (2010 and 2014). Importantly, we have information on the BF beneficiaries, including the amount of money received, since the inception of the program until 2014. To the best of our knowledge, this paper is the first to use such a rich and large dataset to examine the political impacts of social programs.

The source of variation explored in this paper comes from the temporal expansion of the BF program itself, combined with the very small size of each polling station and the random allocation of voters across polling stations in a given polling place[5]. Essentially, our empirical strategy uses the polling place identifier to control for the neighborhood and socio-economic characteristics and exploits a treatment variation as good as randomized based on the time-variation in the program's coverage across polling stations within the same polling place.

We find that cash transfers from the central government affect the most basic form of political participation, increasing the turnout in presidential elections by 0.64 percentage points. This magnitude is relatively high, considering the typical level of abstention in the Brazilian elections, at around 20 percent. This research shows that the BF program also positively affected the support for the incumbent party, which instituted and has been expanding the program for over more than one decade. This electoral rewards came not only from voter mobilization but also from voters shifting their choices in favor of the incumbent. The shifting effect is stronger when considering the variations in the amount of money, instead of in the fraction of recipients, per polling station as the treatment variable.

[3]Such as marital status, age, educational level and gender.

[4]Such as: registered voters, turnout, absent, votes for each candidate (party), and invalid votes.

[5]Basically, polling place is the building and polling station is the specific room (or part of a room) where voters cast their votes. In Brazil, voters are registered to vote at a polling place geographically close to their homes and, given the polling place, voters are sequentially assigned across polling stations in order to ensure randomness and mainly the balance in the number of voters per polling stations. Polling places contain several polling stations where the voters are designated to cast their votes.

Exploring information on the exact time of entry in the program, we find that the rewards for the incumbent party came mostly from those who entered later in the program, instead of from those who were participating in the program since its beginning.

This paper first relates to the empirical literature on the determinants of turnout. While many papers have investigated the impacts of campaigns that aimed to persuade people to vote on real participation (Gosnell [1926], Green et al. [2003], Green and Gerber [2001], Gentzkow [2006]), in this paper, we provide evidence about the unintended impact of social policies and, compared to this literature, we find relatively sizeable effects of CCTs on voter turnout.

Second, this paper is closely related to a body of research exploring the effects of CCTs programs on national politics through presidential elections, which is usually the level at which CCTs programs have been implemented. This literature has traditionally used very aggregated data (De La and Ana [2013]) combined with quasi-experimental methods to study actual voting (Baez et al. [2012]; Zucco [2013]), at the cost of losing external validity and having some degree of aggregation bias. Another standard approach has been the use of self-reported surveys (Manacorda et al. [2011]; Zucco [2013]) to make inference on individual preferences. Using highly disaggregated information on the BF program coverage and election outcomes, we can mitigate aggregation bias and, since we explore the actual results of the election, our findings do not require additional evidence validating the interpretation of self-reported support as actual behavior. Our results reinforce the evidence obtained in the literature, that cash transfers increase both the turnout and the support for the incumbent president. However, aside from finding smaller impacts compared to the previous researchers, our empirical approach combined with the richness of our database could better qualify the political effects of cash transfers.

Ultimately, we also contribute to the literature on the institutional determinants of political engagement of marginalized people. In many emerging democracies women are less likely to vote than man and also more likely to follow the wishes of the male household (see Bari [2005]). Gine and Mansuri [2018] assess the effects of an awareness campaign on female voting and find that treated women were more likely to participate and to make an independent decision. Another robust finding in the political economy literature is that compulsory voting compresses social inequality in turnout (Lijphart [1997], Jackman [2001]). However, Cepaluni and Hidalgo [2016] show evidence that in Brazil, the largest country to use such a rule, compulsory voting increases inequality in turnout because the non-monetary penalties for abstention primarily affect middle and upper-class voters. In these regards, we address social and gender inequality in political participation by showing evidence in which cash transfers targeted to poor women induce political participation and influence voters' choices.

The remainder of the paper is divided as follows. The next section provides background on the institutional environment in Brazil regarding the electoral and political system, as well as the *Bolsa Família* program. Section 3 presents the dataset and the matching procedure and section 4 shows the empirical strategy. Section 5 shows the results and section 6 concludes.

2 Institutional Context

2.1 Electoral and Political System

The current electoral and political basis of the Brazilian democracy was established through the Federal Constitution of 1988 and, based on the republican principle that people can choose their representatives, it guarantees the universal suffrage and the direct and secret vote of citizens.

In Brazil, voting is compulsory for citizens over 18, and up to this age, everyone must register to vote in the municipality of residence. However, the participation of citizens between 16 and 18 years old, older than 70 years and illiterate people is optional. While sanctions for voting abstention do exist, they are fairly weak, which makes voting a voluntary behavior. Basically, voters who fail to vote have to pay a very small fine at around 1.00 USD and those who do not pay the fine are forbidden to participate in civil service exams or public bidding processes, working for the government, obtaining a passport, enrolling in a public university, or obtaining loans from public banks[6]. The percentage of citizens who are eligible to vote is around 75 percent, which makes Brazil the world's fourth-largest democracy, with over 142 million voters.

Brazil follows the principle of separation of powers into executive, legislative and judicial. The national executive branch is headed by the president and at the sub-national

[6] For most Brazilians, the monetary penalty is small but the non-monetary penalties can be costly, especially for those who use many state services.

level, the executive branch consists of the governors (state level) and mayors (municipal level). The heads of the executive branches (president, governors, and mayors) are chosen through direct elections, and each candidate elected has a term of four years with a two consecutive term limit. Therefore, each candidate can run in the election as an incumbent only once. Lastly, Governor elections occur simultaneously with Presidential elections, while mayoral elections always happen between two presidential elections.

In this paper, we focus on the presidential elections of 2010 and 2014, first, because this is the level in which the BF Program has been implemented and, second, this is the period in which it is possible to obtain data at the highly disaggregated level. The election results for the national executive power are based on majority rule and, according to such rule, if none of the candidates receives a majority in the first round, a second round is held between the two most voted and then the winner is decided.[7]

2.2 Administrative divisions and voter allocation

Brazil is divided into 27 federative units, being 26 states and the capital of the country. Each federative unit is divided into polling districts (*Zona Eleitoral*), which are geographically delimited regions managed by the electoral offices that organize, among other things, the voter registration process of citizens living in a certain area[8]. Polling districts are composed of several polling stations (*Seção Eleitoral*), which are grouped together and placed in common locations called polling places[9]. Polling stations consist of very specific places (usually particular rooms) inside the polling places. Therefore, polling places generally contain several polling stations where voters cast their votes in a voting machine[10]. Figure 1 shows an example of polling station, containing a voter machine, located in a polling place during the election.

At the time of voter registration (from 16 to 18 years old) all Brazilian citizens are assigned to vote in a specific polling place. While voters can choose a polling place close to their residences, the allocation of voters to polling station inside the polling place follows

[7] For future research, we also plan to investigate the political repercussions of cash transfers on local's executive elections (for mayors and governors) and legislative elections (for deputies and senators).

[8] A polling district can be formed by more than one municipality or just by part of it.

[9] Usually, the polling place is a school or a public service center.

[10] In nearly all cases, each polling station corresponds to one voting machine. Only in exceptional cases (such as a very large number of voter registered in the polling station or defective machines) it is possible to observe more than one machine per polling station. Even in those cases, electoral data is aggregated at the polling station level.

a random algorithm from Electoral Supreme Court (*Tribunal Superior Eleitoral-TSE*), targeting an equal number of voters across polling stations. Moreover, voters are not allowed to choose a polling place from a different polling district and therefore very far from home. Currently, given a polling place, voters are sequentially assigned within the polling stations and the distribution of voters is electronically done to assure randomness and mainly to balance the number of voters among polling stations in the same polling place[11]. Each polling station must not have more than 400 (four hundred) voters in the capital cities, and 300 (three hundred) in the other cities, and no less than 50 (fifty) voters.[12]

It is worth to mention that the electronic random allocation of voters has not always been in place, at least not formally until 1999. In 1986 all voters were required to visit their respective polling districts during a given period of electoral re-registration and, at this opportunity, they were allocated across polling stations following alphabetical order, starting with the polling station with the smaller numbering in each polling place until all voters could be registered.[13]

While the electronic data system implemented in 1986 was decentralized among polling districts, around 1999 the voter registration process started to be directly managed by the Electoral Supreme Court, which made the process of allocating voters to specific polling stations more similar to how it is done currently. According to new rules, the polling districts would no longer be responsible for assigning voters to polling stations, but only for indicating the polling places. Instead, the polling district had to send the forms to the Electoral Supreme Court, which allocated voters to specific polling stations in each polling place by filling first those with more vacancies.[14] Since 2003, the distribution of voters across polling stations has been made electronically and automatically based on a computerized system that is controlled by the TSE, which now centralizes the process of designating voters to polling stations within polling places.

[11]Polling stations are regulated by Art. 117 of the Electoral Code as well as by complementary legislation. Each one of them must be designated as soon as the registration is granted.

[12]However, in exceptional cases, the Regional Courts can authorize the above-mentioned indexes to be exceeded if this can facilitate the voting process.

[13]On December 1985, the Law No. 7,444 was enacted by the Electoral Supreme Court in order to implement an electronic data management system and, to do so, started a process of re-registration and revision of the Brazilian electorate.

[14]In cases of the same quantity of vacancies, the system allocated the voter in the polling stations containing the smallest number of registered voters.

2.3 The *Bolsa Família* Program

The BF Program was established in October 2003, during the first year of Lula as President of Brazil and through the merger of four preexisting cash transfer programs: *Bolsa Escola*, *Bolsa Alimentação*, *Auxílio Gás* and *Cartão Alimentação*. The unification of these programs was an effort to improve the efficiency of the Brazilian social safety net and to scale up assistance to implement universal coverage of Brazil's poor. In 2004, the program was consolidated by Law 10,836, which provided more stability and the prospect of continuity. At this point, the Ministry of Social Development (MDS) was formed to reduce administrative costs and bureaucratic complexities for the administration of the BF program (see Figure 2).

As a standard CCT, the BF program aims to promote immediate poverty alleviation through direct income transfers and reinforce the access to essential social services in education and health. The program also provides supplementary assistance to encourage low-income families to overcome social vulnerability and the persistence of poverty across generations. The BF Program has two central goals: 1) provide cash transfers to help poor families with their essential daily needs and 2) create incentives and conditions for the beneficiary families to invest in the human capital of their children. Accordingly, the transfers are conditional on the children's education and proper medical care[15].

Since Workers Party won four consecutive presidential elections (with Lula in 2002 and 2006, and Dilma in 2010 and 2014), all the expansions of the program were administered by the same party over more than one decade. Analysing how the BF program has evolved throughout the years, we observe that after combining four existing programs in 2004, it was extensively expanded by finding and including more people in the pool of recipients (see Figure 3). This was the result of an effort to deliver on a campaign promise made by Lula to include all the 11 million poor families by the end of his first mandate, in 2006. From 2007 onwards, the federal government continued expanding the number of beneficiaries – even though on a smaller scale. More recently, the BF program has introduced new types of benefits for those families already enrolled in the program, and it has also presented successive increments in the total value of the benefits. Table 1 brings an annual overview

[15] Basically, all children under seven years old have to accomplish the health requirements and immunization schedule, children over six years old have to be enrolled at school (with a minimum percentage of attendance) and pregnant or lactating women have to participate in prenatal and postnatal care.

of the changes in the BF program benefits since its beginning. As we focus on the elections of 2010 and 2014, below we describe in more details the structure of the program for these two years.

In 2010, the BF Program provided cash assistance to families living in poverty and extreme poverty situation, characterized by a monthly *per capita* family income of up to 140 BRL (one hundred and forty reais) and 70 BRL (seventy reais), respectively. Also in 2010, BF Program had the following benefits: ***Basic Benefit*** of 68 BRL (sixty-eight reais), destined for families in situation of extreme poverty; ***Variable Benefit*** of 22 BRL (twenty-two reais) per beneficiary, up to the limit of 66.00 BRL (i.e., up to 3 members) per family, destined to poor and extremely poor families with children under 15 years of age in their composition and ***Variable Youth*** of 33 BRL (thirty-three reais) per beneficiary, up to the limit of 66 BRL (i.e., up to 2 benefits) per family, destined to poor and extremely poor families with sixteen and seventeen years in their composition.

Four years later, the ***Basic Benefit*** increased by 10%, (from 70 BRL to 77 BRL). The value paid for the ***Variable Benefits*** increased from 22 BRL to 35 BRL and the number maximum of benefits per family was also extended from three to five, changing the total amount limit for this benefit from 66 to 175 BRL consequently. The value of the ***Variable Youth*** increased from 38 to 42 BRL, maintaining the limit of two benefits per family. Finally, in 2014 a new type of benefit was being paid, called Benefit to Overcome Extreme Poverty (hereafter referred as BOEP). Essentially, since 2012, all the BF beneficiaries that, after receiving all the BF benefits (*Basic, Variable and Youth*), still had a familiar monthly income below 77 BRL per person were entitled to receive the exact amount of money necessary to overcome the extreme poverty line. It is worth noting that, from 2010 to 2014, the monthly income per person qualifying for the BF program changed from 140 BRL to 154 BRL (from 70 BRL to 77 BRL), which also re-defined the poverty (extreme poverty) line.

Therefore, the period 2010-2014 presented significant changes in the amount of money paid to BF beneficiaries. However, it is important to point out that even though there have been no very substantial changes in the total number of beneficiaries between 2010 and 2014, it does not indicate that there was no change in the extensive margin of the BF program coverage. In fact, while the BF Program covered almost 12.6 million families in 2010 and nearly 14 million in 2014, fewer than 8 million families were receiving the BF program in both years, indicating that there is a large number of recipients leaving and entering in the program over time.

In this regard, the families enter and leave the program for several reasons such as: meeting (or failing to meet) the eligibility criteria based on *per capita* family income, whether or not they have children in the age range that qualifies for variable benefits and, less likely, non-compliance with conditionalities on health and education or even for not updating the information in *CadUnico* regularly enough. Figures 3 and 4 present a graphical visualization of the changes occurred in the BF Program over time. While Figure 3 shows the numbers of families (or reference persons) beneficiaries from the program, Figure 4 shows the changes in the value of the stipends over the years by presenting the 1st, 50th and 99th percentiles of the total values received by the reference persons.

3 Data and matching procedure

This paper uses three sources of administrative data: 1) monthly payroll reports of the BF program; 2) Single Registry for Social Programs (called *CadUnico*) and 3) election results and voter characteristics from the Brazilian Superior Electoral Court (*Tribunal Superior Eleitoral-TSE*).

BF beneficiaries are identified in the monthly payroll data containing the complete information about cash payments from the BF program, which are made through a debit card issued to the reference person of the family (see Figure 5). Only this person is responsible for handling the card and receiving the stipends and, except in particular cases, the reference person must be a woman over 16 years old. We have administrative data on the monthly payroll since the beginning of the program until 2014 and we select those beneficiaries who were receiving the BF program during the elections of 2010 or 2014. Aside from reporting all the benefits of the program, the monthly payroll data also includes the Social Identification Number (or *Número de Identificação Social-NIS*) of the reference person. While payroll data contains rich information on benefits paid, it does not allow us to match this data with voting outcomes directly. In this regard, *CadUnico* is fundamental to our analysis, given that it contains additional variables (including NIS number) that allow recovering the voter registration information of the BF beneficiaries.

All the households that receive benefits from the BF program must be registered in the *CadUnico*[16], which is an important database that identifies and characterizes all the members of low-income families living in Brazil. It is organized by the Ministry for Social Development (MDS) to select all the beneficiaries of any federal social program, including the BF Program. Contrary to the monthly payroll data, *CadUnico* contains cumulative data on all people that have received the BF program until some point. For this paper, we use the 2014 version of the *CadUnico* and complement it with data from two other extractions, 2012 and 2013[17]. In particular, and essential to this paper, *CadUnico* provides information on voter registration, based on which we can identify the polling stations where beneficiaries are assigned to vote. We use the NIS number to link the voter registration information with the monthly payroll data. *CadUnico* also includes the date of birth, name and mother's name of the beneficiaries, which we use to work around the problem of missing information on the voter's registration data.

Everyone who receives the benefits from BF Program as the household reference person must have at least one of the following federal documents listed in the *CadUnico*: *Cadastro de Pessoa Física* (called CPF) or voter registration (called *título de eleitor*). Although only one of these documents is required, the percentage of reference persons who report voter registration is considerably high. However, since *CadUnico* does not require filling out the state of voting residence, there is an obstacle for uniquely identifying the polling stations of voters, which can be accurately made only by concatenating three variables: state registered to vote (two-digit), *Zona Eleitoral* (three-digit) and *Seção Eleitoral* (four-digit). Figure 5 shows an example of the voter registration (left side), where the information about *Zona* and *Seção* are circulated.

To overcome this obstacle, we pick in *CadUnico* the following supplementary information: name, mother's name and date of birth. Using these three variables, we collect information on *Zona Eleitoral*, *Seção Eleitoral*, and state registered to vote by scrapping the Superior Electoral Court-TSE website. The data obtained in the TSE website is more

[16] *CadUnico* was initially conceived to register all poor families in the country to facilitate their access to social programs. The *CadUnico* is a crucial tool to manage the BF program and other social programs and services. The registration process is shared between local and federal authorities. The federal government institutes the total number of poor families to survey and register in the system, and the municipalities conduct the household registry process as well as the interviews. Municipalities and states also check whether the families have accomplished the required conditions and, based on the information sent by them, the federal government controls the approval and cancellation of benefits and provides all payments on a monthly basis.

[17] So far we do not have access to the previous versions of *CadUnico*, but we plan to use the *CadUnico* since its beginning in order to mitigate the missing data problem (later described).

reliable, in comparison to that from *CadUnico*, since it is less subject to errors of misreporting or misfiling. Of the total number of beneficiaries who were receiving the benefits during the elections of 2010 or 2014 (over 18 million), we succeed in recovering at around 70% of them by scraping the website from September 2017 to February 2018.

In order to recover the polling station information for the 30% remainder, we take advantage of the fact that *CadUnico* also collects several variables that allow us to infer with a certain degree of confidence the state where the beneficiaries are registered to vote. Specifically, *CadUnico* provides, aside from state of birth and residence, the states recorded in other two documents (*Registro Geral* and *Carteira de Trabalho*). For those beneficiaries that we could not find the state registered to vote by web scraping, we pick the most frequent state amongst the four possibilities listed above. In the very few cases in which the modal state was not identified, we pick the most updated information, which is the state of residence. Following this scheme, we finally recover polling station information for almost 94% of all BF beneficiaries considered in our analysis (96.8% in 2014 and 92.7% in 2010).

Linking all the BF beneficiaries with their respective polling stations, we can calculate the total number of BF beneficiaries per polling station as well the amount of money coming from the program in each polling station[18]. After collapsing the individual data to polling station level, we match the collapsed dataset to two other polling station level datasets containing the voter characteristics and election outcomes, which are both provided by TSE. The voter characteristics include marital status, age, educational level and gender, while the "Report of Electronic Voting Machine" provides full information on election results at the polling station level[19]. An example of Report of Electronic Voting Machine can be found on the right side of the Figure 1, where it is possible to observe the number of registered voters, turnout, absent, votes for each candidate (or party) and invalid (blank and null) votes for the two presidential elections of 2010 and 2014.

In sum, based on a list of beneficiaries of the BF program, we find their polling stations by web scraping the TSE's website. For those not found in this manner, we rely on the information provided by the *CadUnico* to infer the polling stations where the beneficiaries are registered to vote. Then, we aggregate both the total number of recipients and the

[18]Later on this paper we will describe how we deal with the missing data problem.

[19]The disclosure of this information has been done since 2008 to offer more transparency about the electoral procedures.

amount of money received at the polling station level. Lastly, we match this collapsed dataset with information from TSE containing details on both electorate characteristics and electoral outcomes at the polling station level.

With the aggregated database at the polling station level, we selected only those polling stations containing more than 50 registered voters[20] and polling stations that were active in both elections (2010 and 2014). Moreover, our empirical strategy (detailed in the next section) requires having at least two polling stations per polling place. After these restrictions, our dataset is composed of 325,753 polling stations distributed in over 55 thousands polling places across the country. Table 2 summarizes the descriptive statistics of the data used for the estimation. Panel 1 shows the voting outcomes for the two presidential elections of 2010 and 2014, and for both the first and the second rounds. Panel 2 shows the treatment variables, the fraction of BF beneficiaries (as reference person) per polling station and the amount of money from BF program per registered voters, and Panel 3 presents the voters' characteristics.

4 Empirical Strategy

4.1 Baseline model

We are interested in studying how CCTs programs affect the individual voting behavior, but in most countries, including Brazil, the use of individual-level voting data is not possible. However, we can measure the coverage of the BF program as well as voting outcomes at the polling station level to estimate the following econometric model[21]:

$$Y_{s(p)t} = \alpha_{pt} + \lambda_{s(p)} + \beta BF_{s(p)t} + Z'_{s(p)t}\Theta + \epsilon_{s(p)t} \tag{1}$$

where the subscript $s(p)$ indexes polling stations in a given polling place, p, and t indexes the year of the election (2010 or 2014). The primary variable of interest in this study, $BF_{s(p)t}$, is the coverage of the BF program, which we measure in two ways, first, the frac-

[20]Smaller polling stations are very rare and only occur in exceptional cases, which are out of the scope of this paper.

[21]This is the most disaggregated level available and each polling station regularly contains fewer than 400 voters. Since we use the number of voters registered in each polling station as weights in the estimation procedure, this equation must be understood as an aggregate version of $Y_{i(p)t} = \alpha_{pt} + \lambda_{i(p)} + \beta BF_{i(p)t} + Z'_{i(p)t}\Theta + \epsilon_{i(p)t}$, where i indexes the individual.

tion of people (registered to vote[22]) in each polling station who received BF assistance (as a reference person) at the moment of the election and, second, the total amount of money received per registered voters in each polling station. The outcomes of interest, $Y_{s(p)t}$, are defined as following: Turnout (% of registered people who voted); Inc (% of registered people who voted for the Incumbent); Opo (% of registered people who voted for the Opposition) and Invalid (% of registered people who voted blank or null)[23]. $Z_{s(p)t}$ represents the (averages) voter profile at the polling station level, such as marital status (percentage of single, married, divorced and widower); several groups of age (percentage of 16, 17, 18-20, 21-24, 25-34, 35-44, 45-59, 60-69, 70-79 and 79 years of age or older); educational level (percentage of illiterate, "read and write" but no formal education, primary incomplete, primary completed, secondary education incomplete, secondary education completed, college incomplete and college completed) and gender (percentage of men and women). The econometric model also includes two groups of fixed effects, α_{pt}, to controls for the time-variant neighborhood characteristics, and $\lambda_{s(p)}$, which controls for all the time-invariant characteristics at the polling station level. We use cluster at the municipality level and the total number of registered voters in each polling station as weights in the estimation procedure.

4.2 Source of variation

The lack of randomized rollout of CCTs (including the BF Program) creates a major difficulty for having unbiased estimates of their effects, since the estimation may be confounded by unobserved variables potentially correlated with both the voting outcomes and the placement of the program. Geographical aggregation by income, for instance, render the poor trapped in low SES neighborhoods. In this case, we need to consider the inherent endogenous relationship between voting outcomes and the enrolment in the program. Accordingly, the inclusion of polling place fixed effects, α_{pt}, play an important role in controlling for time-variant socioeconomic characteristics of the neighborhoods that are associated with the place of residence and welfare assistance.

Furthermore, aside from being allocated at a polling place geographically close to their homes, given a polling place, Brazilian voters are sequentially assigned within polling

[22]The total number of voters expected to vote at the polling station in each election.
[23]See Figure 6.

stations in order to ensure randomness and mainly the balance in the number of voters among polling stations at the same polling place[24]. Since we exploit the time-variation in the coverage of the program across polling stations within the same polling place, we rely upon the (pretreatment) voter allocation process inducing a treatment variation as good as randomized.

Essentially, the source of variation explored in this paper is driven by the temporal expansion of the BF program itself in two margins. First, there is a variation generated by the entry and exit of beneficiaries over time. It is important to point out that the inclusion of more families occurred from 2010 to 2014 was not based on income criteria; rather it was a central government effort to assist all the extremely poor people across the country[25]. On the other hand, although the exclusion of beneficiaries could have happened for several reasons, the most common one was when the families no longer had children in the age range qualifying for the program. Secondly, during the period 2010-2014 also happened successive increases in the total amount of money given to poor families already enrolled in the program. These increases came from three origins: the increase in the maximum limit for the number of variable benefits per family (from 3 to 5), the increase in the amount of money per dependents of up to 18 years old, and mainly by the creation of a new type of benefit (called BOEP)[26], destined to eliminate extreme poverty in Brazil.

Nonetheless, one may wonder how the coverage varies among polling stations in a given polling place if voters in the same polling place are supposed to be very homogenous. We argue that the variation comes from the very small size of the polling stations. To illustrate this, consider the estimation of $BF_{s(p)t} = \pi_{pt} + \rho_{s(p)} + \kappa_{s(p)t}$, where the variation that we exploit comes exactly from the residual variation in $\kappa_{s(p)t}$. For this regression, we find an R-squared over 0.98. Moreover, when we divide the data into three subsamples based on the sizes of the polling stations and plot the distributions of $\kappa_{s(p)t}$ by dividing the dataset according to three quantiles, we see that the variance of $\kappa_{s(p)t}$ declines as the size of the polling station increases and $\kappa_{s(p)t}$ is aways distributed around the zero mean (see Figures 7 and 8).

[24]More details in the subsection 2.2.

[25]Following this goal, the federal government supported and encouraged the municipalities to maintain a better coverage and the quality of information registered in the *CadUnico* though an active search and registration, which culminated in the inclusion of new beneficiaries.

[26]See Table 1 and section 2.3 for details.

4.3 Dealing with missing data

The main caveat in building our database is the impossibility of finding polling station information for some BF beneficiaries. Since we have *CadUnico* only from 2012 onwards, the quality of the matching also increased between the elections of 2010 and 2014. In this case, discarding individuals with missing in voter registration could confound the effects of time expansions of the BF program with changes in the quality of matching itself. We take this into account when computing both the total number of beneficiaries and the amount of money from BF Program per polling station. Instead of simply discarding individuals, we adjust these quantities so that those beneficiaries with voter registration reported can represent those with similar characteristics but without voter registration information.

To describe the approach used in this paper, consider a variable indicating the participation in the BF program, $BF_{is(p)t}$, for individual i who must cast a vote in the polling station s into the polling place p at year t. To calculate the fraction of BF beneficiaries among registered voters (the treatment variable), i.e., $BF_{s(p)t} \equiv \frac{\sum_{i \in s} BF_{is(p)t}}{Registered_{spt}}$, it is necessary to have information on the polling stations where all the beneficiaries are designated to vote. Otherwise, we can only observe those of whom we have information, BF_i^{found}, whereas $BF_i \equiv BF_i^{found} + BF_i^{not-found}$. In this case, we need to obtain expansion factors, λ_i, that make $\sum_i BF_i = \sum_i BF_i^{found} \times \lambda_i$. If the missing data were at random, it would be enough to define the factor as $\lambda_i = N/N^f$ (equivalentely, dividing by the unconditional probability of being found $\frac{N^f}{N}$) and weighted summing-up BF_i^{found} over polling stations $s(p)$ to calculate the adjusted amounts.

However, missing at random is often unlikely and the way commonly used to deal with not random missing data is the Inverse Probability Weighting (IPW), which inflates the weight for individuals who are under-represented due to the missing information by considering the observables determinants of the missing data. We use a logit model to estimate the probability of the voter's registration being found, $P(f_i = 1|obs)$, based on a large group of dummies indicating each month in which the beneficiaries were participating in the BF program as well as a set of fixed effects of the states of residence.

These variables are important predictors of the probability of having polling station reported, first, because as more time the family stays in the program as higher the chance for them to update the voter registration in *CadUnico*. Secondly, the process of filling/updating the information of beneficiaries depends on local management and hence vary within states. To gain precision, we also control for the number of benefits for children under six years of age, number of benefits for children aged between 7-15 years, number of member in the family, an indicator variable for extreme poverty status, the age of the youngest child and the mean age of all children. Finally, after estimating $P(f_i = 1|obs)$, we collapse the individual data, BF_i, at the polling station level by summing-up over $s(p)$ and using the inverse of the estimated probability as weights (i.e., $\lambda_i = 1/P(f_i = 1|obs)$). This method gives the numerator of the treatment variable by adjusting for missing data.

5 Results

5.1 Results on Turnout

Before showing the main results, Table 3 evidences the importance of including controls and mainly the polling place fixed effects in the model. From the first column (a simple regression model) to the column 2 (which adds time-varying polling place fixed effects) the coefficient of interest changes substantially and further it switches its sign. This suggests that controlling for fixed effects of polling place could mitigate the spurious correlation between the factors influencing both the welfare dependence and the voting behavior. Column 6 shows a more complete and reliable estimate and, relative to the column 2, the magnitude of the impact also changes, indicating the importance of incorporating all the controls in the baseline model. Finally, in column 7 of Table 3, we adjust the treatment variable for missing data and observe that, relative to column 6, the treatment effect of the BF Program only marginally changed[27]. The most reliable estimative comes from column 7, where the BF program increased the turnout rate by 8.3 percentage points.

To put this result in perspective, consider the change on the outcome triggered by a variation from zero-coverage to the actual mean coverage, i.e, $\overline{BF}(= 0.077)$[28]. In this case, the treatment effect would be around 0.64 percentage points (= 0.083*0.077), which is relatively high, especially considering that this is an nonintended effect of the program. Moreover, this effect means a Persuasion Rate[29] of 3.2, which is also relatively high in com-

[27]Even though the missing data problem does not seem very relevant, the estimation in the sequence of this paper considers the adjustment for missing data in the coverage of the program.

[28]Notice that $E(Y_{s(p)t}|BF_{s(p)t} = \overline{BF}, ...) - E(Y_{s(p)t}|BF_{s(p)t} = 0, ...) = \beta\overline{BF}$

[29]The *Persuasion Rate*, is defined in DellaVigna and Gentzkow (2010) as $PR = 100 *$

parison with what previous papers have found by investigating campaigns that aimed to persuade people to vote by card reminding of registration (Gosnell [1926] found PR=13.4), door-to-door canvassing (Green et al. [2003] found PR=11.5), phone calls 18-30 year-olds (Green and Gerber [2001] found PR= 4.5) and exposure to television (Gentzkow [2006] found PR=4.4).

Since voting in Brazil is mandatory, one may wonder how the BF program could affect the participation in the election. First, the monetary penalty is not very high, even for a poor person. On the other hand, non-monetary penalties are more costly only for the middle classes and more educated voters. Thus, even under compulsory voting, some voters may prefer justifying their absence or paying a little fine after the election. Moreover, there is a sizable fraction (around 14%) of voters who are not required to vote such as people aged 16-17 and over 70 years of age as well as illiterate people.

5.2 Voter Choices

In principle, more people participating in the election is advantageous for all candidates, since they have an additional margin to increase their support, but cash transfers can also affect the voter choices between candidates. As important as measuring the effects of CCTs on turnout is exploring how the program changes the voting behavior and, consequently, the results of the elections.

It is possible to decompose the effect on turnout into three components, given that $\Delta Turnout = \Delta Inc + \Delta Opo + \Delta Invalid$, represented by the coefficients displayed in Table 4. The evidence regarding the effects of the BF program on the choices between candidates indicates a positive net effect on the vote share for the incumbent party in both rounds of the elections[30]. Therefore, the BF program positively affected both the turnout and the support for the incumbent party. On the other hand, the program also had a positive net effect on the oppositions vote share in the second round, at around 1.5 percentage points, which is much smaller than the impacts on the incumbent vote share (6.11 percentage points), indicating an electoral advantage for the incumbent explained by the BF Program.

$\frac{(y_T - y_C)}{(BF_T - BF_C)} \frac{1}{(1-y_0)}$, with T symbolizing the "treatment" group and C the "control", $(1-y_0)$ is the size to be convinced (% of abstention) and $\frac{(y_T - y_C)}{(BF_T - BF_C)}$ would be the β coefficient. We calculate *Persuasion Rate* by considering $dBF = \overline{BF}$.

[30]In the first round, we aggregate the fraction of votes for all opposition candidates.

In order to better qualify these net effects on voting behavior, it is important to notice that the BF program may affect voting through two mechanisms: more voters turning out to vote (*mobilizing effect*) and voters *shifting* their choices between candidates (or invalid vote). To decompose these effects, let the incumbent vote share, $Inc_{s(p)t}$, be written as

$$Inc = \frac{I}{R} = \frac{I}{V} \times \frac{V}{R}$$

where R symbolizes registered voters and V those who actually voted. Therefore, assuming a variation, dBF, in the program we have

$$\frac{d(I/R)}{dBF} = \frac{d(V/R)}{dBF}\frac{I}{V} + \frac{d(I/V)}{dBF}\frac{V}{R} = Mobilizing + Shifting$$

Where the left side is directly observed from the estimates in Table 4, as well as the effect on turnout $\frac{d(V/R)}{dBF}$. The terms $\frac{I}{V}$ and $\frac{V}{R}$ can be observed in the Table 2.[31] For instance, using the information from Tables 4 and 2:

$$0.06116 = 0.08286 * 0.49 + \frac{d(I/V)}{dBF} * 0.788 = Mobilizing + Shifting$$

Examining the results of this decomposition for the first and the second round of the elections (see Figure 9), we observe that the BF program affected the advantage of the incumbent party not only by mobilizing more people to vote but also by persuading voters from the opposition (or from Invalid votes) to support the incumbent candidate.

These findings contrast with the literature that has assessed these mechanisms behind the effects of CCTs on voting behavior (Galiani et al. [2016], De La and Ana [2013]). These authors found that CCTs increased turnout and incumbent vote share in similar magnitudes, while opposition parties' vote shares were unaffected by CCTs programs. Then they argue that this evidence suggests that mobilization of the incumbent party base can best explain the impacts of CCTs rather than voters switching their choices. However, given the high level of aggregation (municipality/village), this conclusion is not unequivocal, since they only observe the net impacts of the programs on the aggregated election outcomes. In this paper, we introduced a simple way to decompose these two effects, and we find evidence to support the presence of both mobilization and shifting

[31] The same idea was applied to decompose the total effect on the opposition and invalid votes.

effects.

5.3 Additional Discussion

The BF program entry and exit dynamics

Examining how the BF program has evolved throughout the years, it can be characterized by three distinct phases. First, in the beginning (Jan. 2004), the BF program only combined four existing social programs. Later, over the period 2004-2006, the BF program was extensively expanded mainly by finding and including new poor people in the pool of recipients until it reached 11 million families. After 2006, the BF program did not suffer remarkable changes regarding the effort to find new recipients, compared to the first three years of the program, when the program was being consolidated. The changes in the program during the period 2007-2014 are much more associated to the regular movement of people entering and leaving the program and the introduction of new types of benefits for those already enrolled in BF program.

We explore the time since the beneficiaries have been participating in the program and investigate whether and how it matters for voting behavior. Specifically, the percentage of people (registered) who received BF assistance at the moment of the election, $BF_{s(p)t}$, can be decomposed into three mutually exclusive parts: a) the fraction who received BF program **since the beginning**; b) the fraction who **entered during 2004-2006** in the BF program and c) the fraction who **entered after 2006**. Thus, we can decompose the fraction of recipients for each election as:

$$\beta\overline{BF} = \beta_a\overline{BF_a} + \beta_b\overline{BF_b} + \beta_c\overline{BF_c}$$

The magnitude of each effect in this decomposition depends on both the coefficients estimated and the proportion of beneficiaries belonging to each group. Thus, to calculate the contribution of each group, we use information from Table 5 and the descriptive statistics from the Panel 2 of Table 2.[32] Basically, we find that most of the advantage for the incumbent came from beneficiaries who entered into the BF Program later, especially after 2006, while the opposition was positively affected by voters who have been participating

[32] Table 2 shows the proportion of beneficiaries divided into three parts to be used in this decomposition analysis. These fractions were calculated based on the time since which the beneficiaries started to receive the benefits of the BF program.

in the program since its beginning (see Figure 10).

Taking the second round, for example, almost all the positive effect on incumbent support came from those beneficiaries who started in the program after 2007, while this group negatively influenced oppositions' vote share. On the other hand, those who participated in the program since its beginning and those who entered in the BF program during 2004-2006, tended to support the opposition. These findings suggest that the effects of CCTs programs on support for the incumbent are significant in the short run, but it has a reduced long-term impact since those beneficiaries who have been participating in the program for a long time do not tend to support the incumbent candidate.

For future research, we also plan to investigate the effect of losing the benefits of BF Program on the voting behavior. We can address this point by including in our definition of treatment all of those beneficiaries who have participated in the program at some point in their lives (not only at the moment of the election). By doing so, we can further separate the effects of people who still receiving at the moment of the election and beneficiaries who no longer receive the benefits from the BF Program. Our preliminary results in this direction have already shown that beneficiaries who lost their benefits tend to punish the incumbent.

Changes in the fraction covered v.s in the value of the benefits

While Table 4 has shown the impacts of changes in the proportion of voters receiving BF Program, Table 6 considers the changes in the amount of money per registered voters (in 100 BRL). In addition to the basic results found previously, something new can be seen in this analysis, which is a significant and negative net impact of the BF program on the opposition's vote share. Taking the second round for example, for each increase of 100 BRL *per capita* in the BF Program coverage, the turnout increases 4.4 percentage points, while it increases more than proportionally the vote share for the incumbent, in 6.07 percentage points. On the other hand, the fraction of votes for the opposition reduces by 2.2 percentage points, considering similar changes in the amount of money per polling station.

Figure 11 presents the mobilization-shifting decomposition considering the variations in the amount of money from BF Program per polling station. For this analysis, we find that the shifting effect is more pronounced, indicating that changes in the amount of money can be more effective in increasing the support for the incumbent by persuading the opposition voters. It is important to point out that the changes in the amount of money between 2010 and 2014 came mostly from the BOEP benefits, which is a new stipend, created in 2012, focused on eliminating extreme poverty. This extra benefit gives to all BF beneficiaries the exact amount of money which is necessary to overcome the extreme poverty line.

6 Conclusion

This paper investigated the effects of the largest Conditional Cash Transfer in the world, the Brazilian *Bolsa Família* program, on voting behavior. Using three sources of administrative data, we calculate the BF program's coverage as well as voting outcomes at a highly disaggregated level of observation, the polling stations, which has fewer than 400 registered voters. Based on a rich data structure and a novel empirical strategy, we circumvent spurious correlation between characteristics that determine both welfare dependence and voting. We presented preliminary results on the effects of the BF program on presidential elections. This paper, which is still in progress, extends the literature on the relationship between public policy, political institutions, and development economics. We contribute to a literature that is only beginning to emerge, regarding the political effects of CCT programs on politics. As a natural extension of this research, we also plan to understand: i) the effects of CCTs programs on executive local elections (governor and mayors) and legislative elections; ii) the mechanisms driving our results, exploring heterogeneity by age, education and family composition; iii) potential non-linearities and symmetries in the effects of the program; iv) asymmetries in the effects for household head and other members.

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Figure 1: Polling Station, Polling Place and Report of Electronic Voting Machine

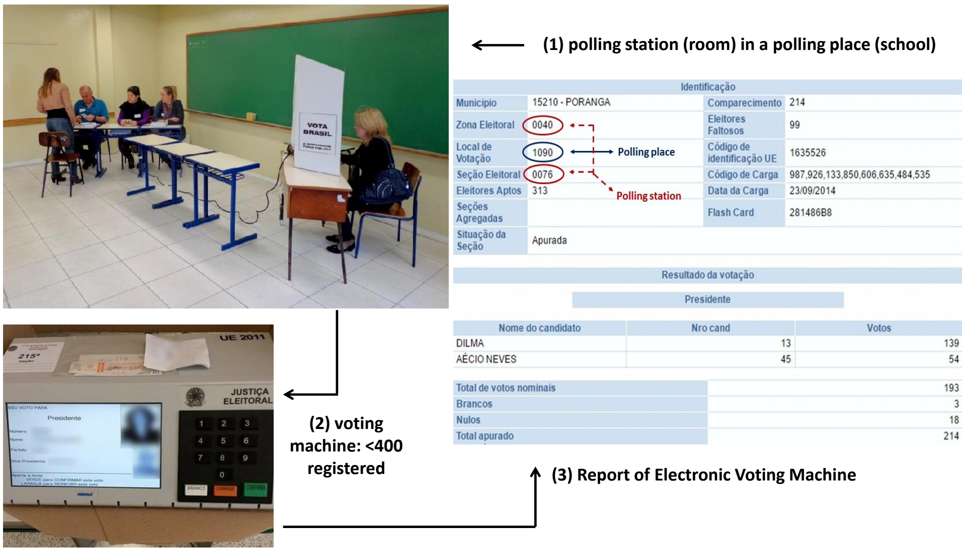

Figure 2: Timeline of important events for this research: BF program, elections and data/information availability

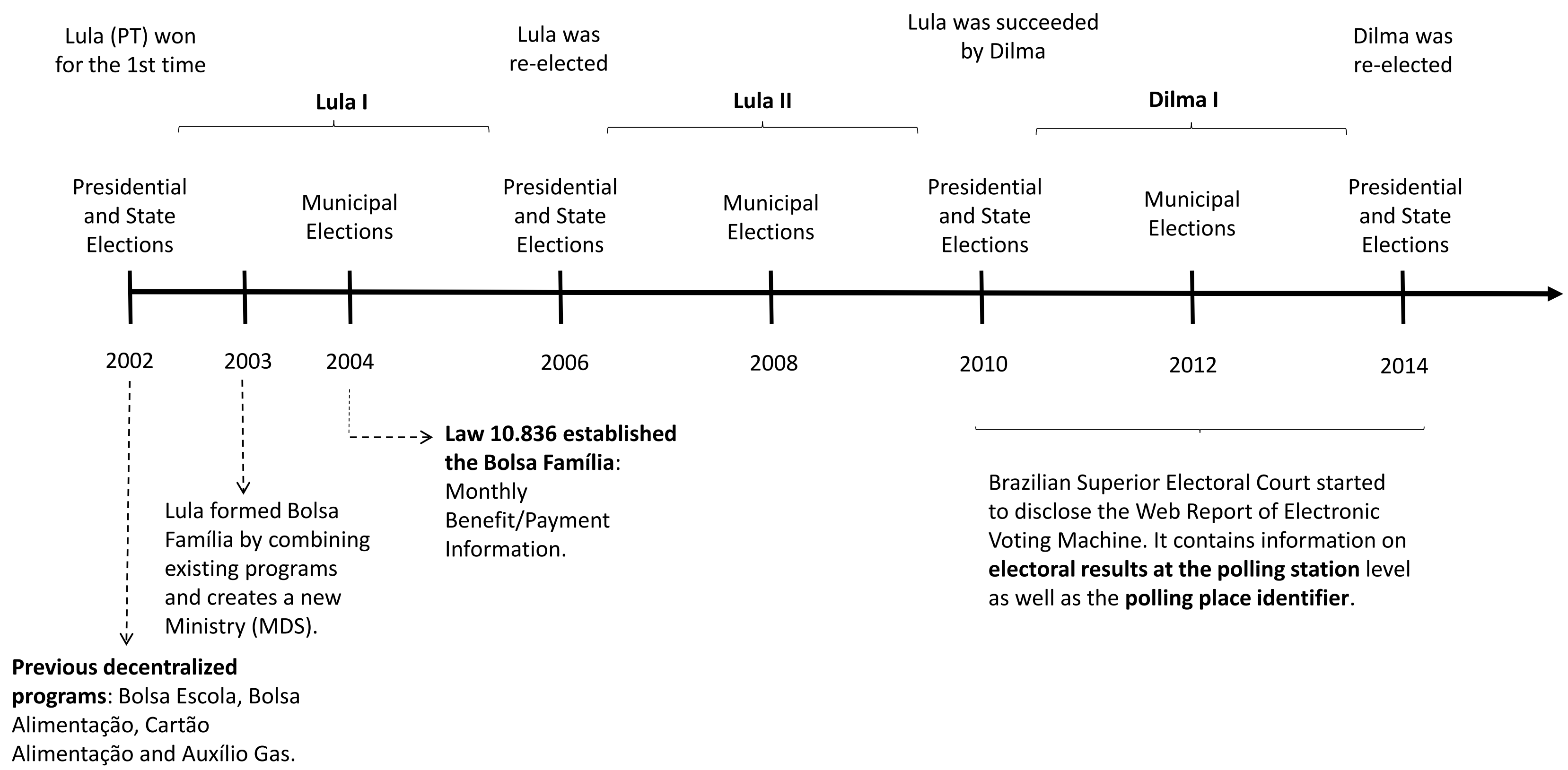

Figure 3: Changes in the number (in million) of BF beneficiaries over time

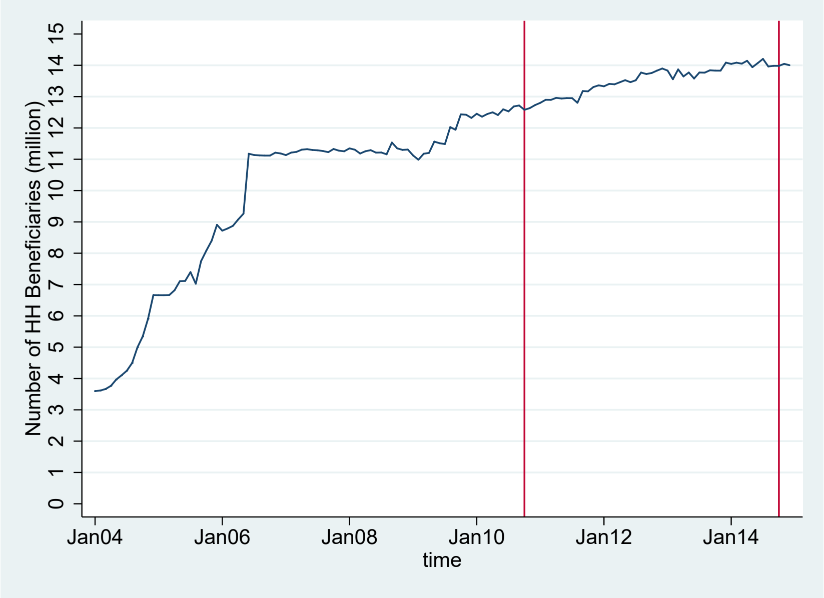

Figure 4: Changes in the amount of money (in 100 BRL) from BF beneficiaries over time

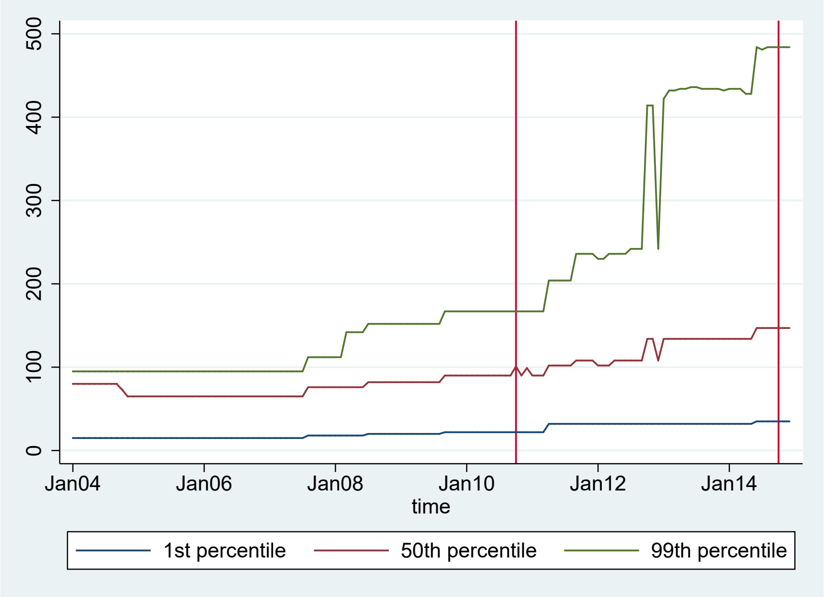

Figure 5: CadÚnico: NIS and voter registration

Voter registration (with polling station)

Debit Card (with NIS)

Figure 6: Outcomes of the Election

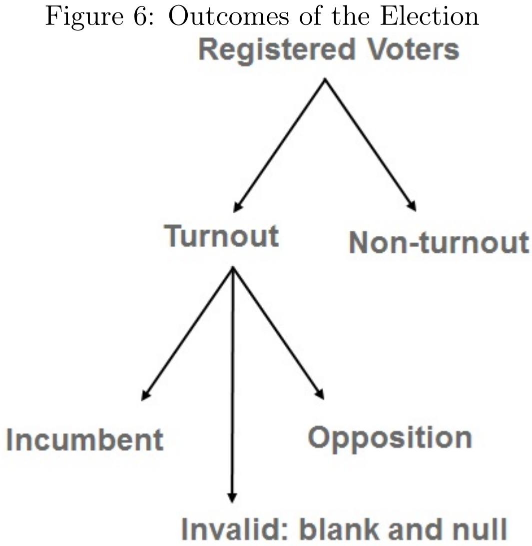

Figure 7: Source of variation (% of BF beneficiaries) and the size of the polling station

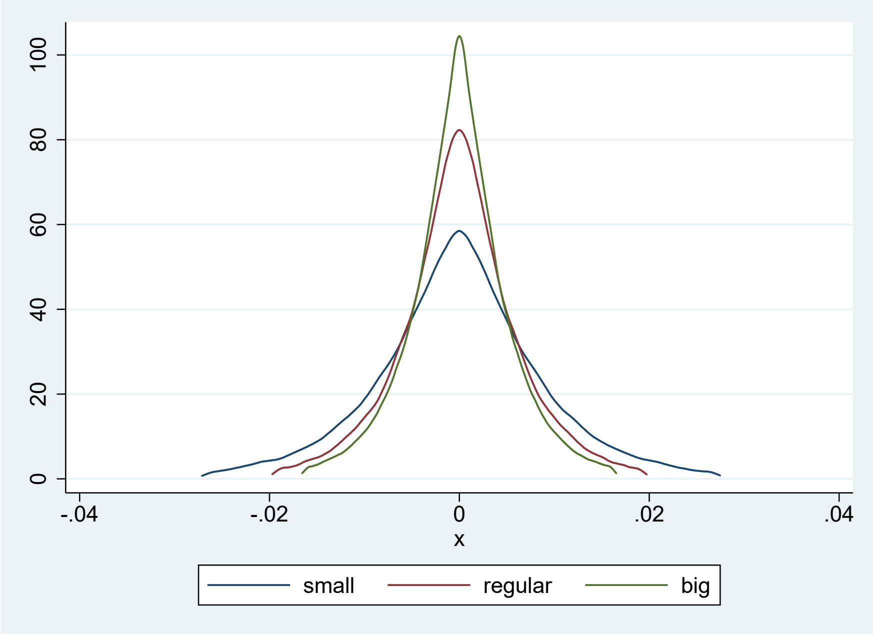

Figure 8: Source of variation (amount per capita from BF) and the size of the polling station

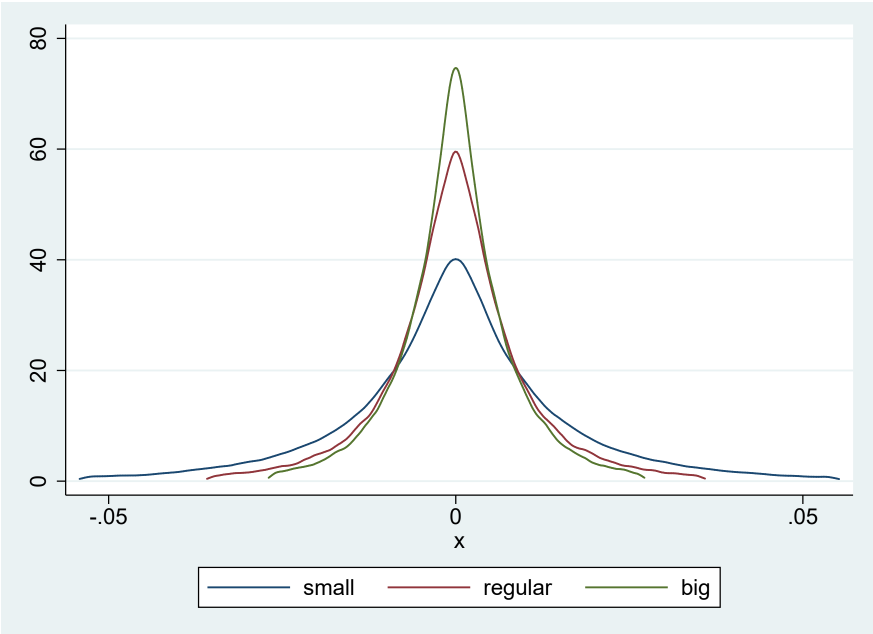

Figure 9: Effects of the BF Program in the extensive margin by mobilizing and shifting effect

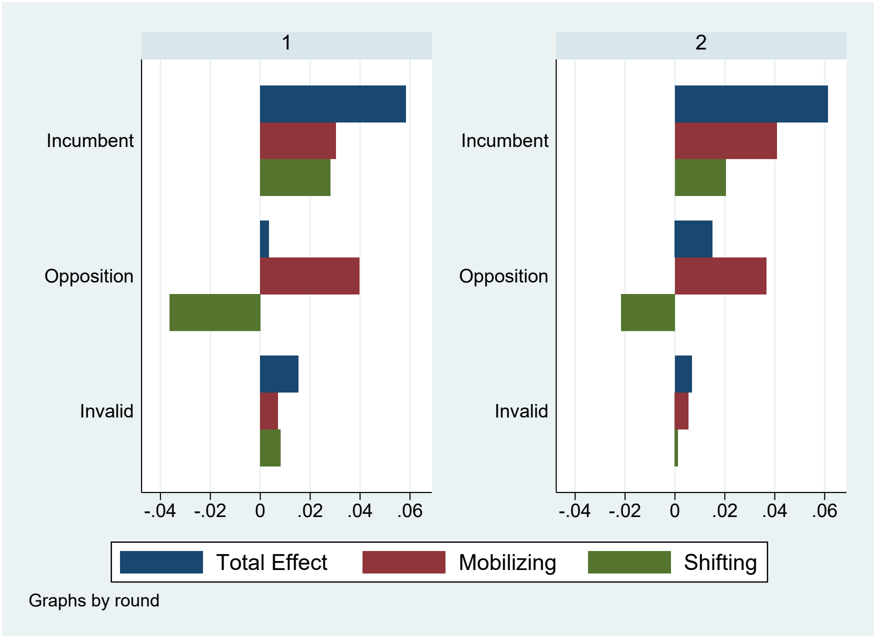

Figure 10: Decomposing the BF effect based on the time of entry in the program

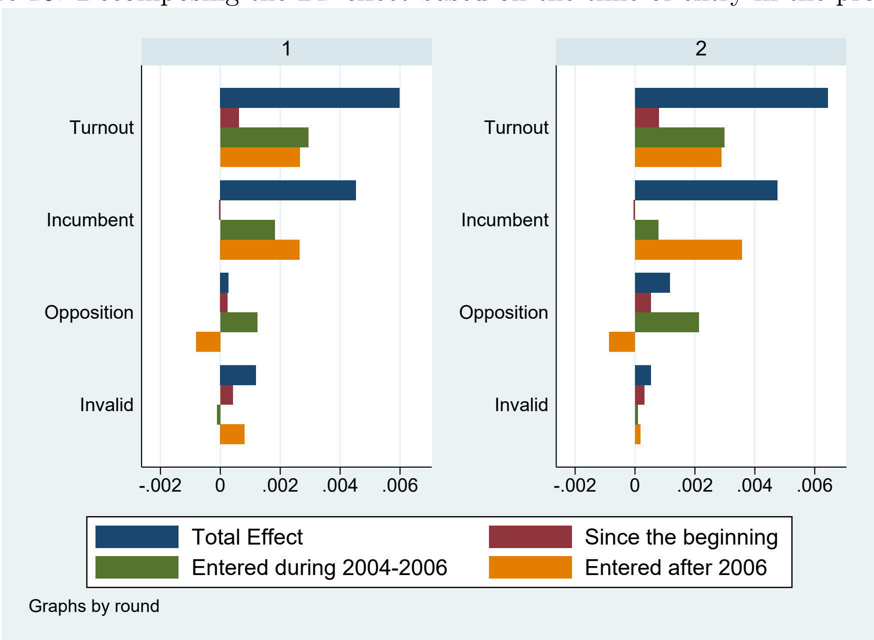

Figure 11: Effects of the BF Program in the intensive margin by mobilizing and shifting effect

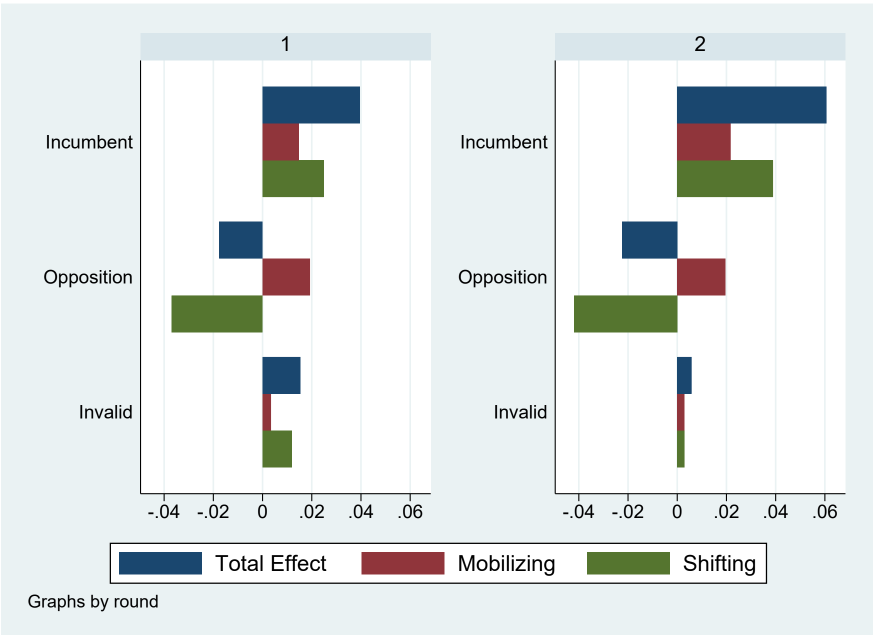

Table 1: Changes in the BF program in the period 2004-2014

Year	-	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Eligibility Criteria												
Monthly family income per capita	Extreme poverty	50	50	60	60	60	70	70	70	70	70	77
	Poverty	100	100	120	120	120	140	140	140	140	140	154
Benefits												
Extremely poor	Basic	50	50	50	58	62	68	68	70	70	70	77
	Variable	15	15	15	18	20	22	22	32	32	32	35
	(limit per family)	-	-	-	-	(up to 3)	(up to 3)	(up to 3)	(up to 5)	(up to 5)	(up to 5)	(up to 5)
	Variable Youth	-	-	-	-	30	33	33	38	38	38	42
	(limit per family)	-	-	-	-	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)
	BOEP	-	-	-	-	-	-	-	-	variable	variable	variable
Poor	Basic	-	-	-	-	-	-	-	-	-	-	
	Variable	15	15	15	18	20	22	22	3	32	32	35
	(limit per family)	-	-	-	-	(up to 3)	(up to 3)	(up to 3)	(up to 5)	(up to 5)	(up to 5)	(up to 5)
	Variable Youth	-	-	-	-	30	33	33	38	38	38	42
	(limit per family)	-	-	-	-	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)	(up to 2)

Table 2: Summary Statistics

	(1) 2010 mean	(2) 2014 mean	(3) Pooled mean
Panel 1: Voting outcomes			
Panel 1.A: First Round			
Turnout	0.820	0.805	0.813
Incumbent	0.341	0.292	0.317
Opposition	0.408	0.435	0.421
Invalid	0.070	0.079	0.075
Panel 1.B: Second Round			
Turnout	0.787	0.789	0.788
Incumbent	0.401	0.370	0.386
Opposition	0.332	0.369	0.350
Invalid	0.054	0.050	0.052
Panel 2: BF Program			
Fraction of BF beneficiaries (complete cases)	0.067	0.078	0.073
Since the beginning	0.012	0.009	0.010
Entered during 2004-2006	0.028	0.020	0.024
Entered after 2006	0.028	0.049	0.038
Fraction of BF beneficiaries (IPW-adjusted)	0.073	0.081	0.077
Since the beginning (IPW-adjusted)	0.013	0.009	0.011
Entered during 2004-2006 (IPW-adjusted)	0.030	0.021	0.025
Entered after 2006 (IPW-adjusted)	0.030	0.051	0.041
Value per capita from BF (in 100 BRL)	0.066	0.131	0.098
Value per capita from BF (IPW-adjusted)	0.071	0.135	0.103
Panel 3: Control Variables (TSE)			
Male	0.480	0.477	0.479
Female	0.520	0.523	0.521
Illiterate	0.056	0.051	0.053
Primary incomplete	0.331	0.306	0.318
Primary completed	0.078	0.074	0.076
Secondary incomplete	0.194	0.193	0.193
Secondary completed	0.135	0.164	0.150
Read and write, but no formal education	0.139	0.119	0.129
College incomplte	0.029	0.036	0.032
College completed	0.040	0.056	0.048
16 years old	0.006	0.002	0.004
17 years old	0.010	0.006	0.008
18-20 years old	0.064	0.046	0.055
21-24 years old	0.099	0.085	0.092
25-34 years old	0.242	0.237	0.239
35-44 years old	0.198	0.202	0.200
45-59 years old	0.228	0.243	0.235
60-69 years old	0.084	0.098	0.091
70-79 years old	0.046	0.052	0.049
over 79 years old	0.023	0.029	0.026
Married	0.314	0.317	0.315
Single	0.647	0.635	0.641
Widower	0.016	0.018	0.017
Divorced	0.024	0.030	0.027
N polling stations		325,753	
N polling places		55,043	

Table 3: The importance of including fixed effects and adjust the treatement for missing data. Effects of the BF Program on Presidential Election (Runoff)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout IPW-adj.
%BF Beneficiaries	-0.25585***	0.19082***	0.32849***	0.11169***	0.11966***	0.09130***	0.08286***
	(0.01604)	(0.01109)	(0.06184)	(0.00730)	(0.00693)	(0.00629)	(0.00564)
Constant	0.80685***						
	(0.00194)						
Observations	651,506	651,506	651,506	651,506	651,506	651,506	651,506
R-squared	0.07751	0.81670	0.87559	0.96025	0.96158	0.96512	0.96512
Year*Place FE		Yes		Yes	Yes	Yes	Yes
Booth FE			Yes	Yes	Yes	Yes	Yes
Controls						Yes	Yes
Controls-except age					Yes		
Year FE			Yes				

Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
The control variables include: marital status (percentage of single, married, divorced or widower), groups of age (percentage of 16, 17, 18-20, 21-24, 25-34, 35-44, 45-59, 60-69, 70-79, 79 years of age or older), educational level (percentage of illiterate, "read and write" but no formal education, primary incomplete, primary completed, secondary education incomplete, secondary education completed, college incomplete or college completed) and gender (percentage of men and women). The results are clustered at the municipality level and the number of registered voters in each polling booth are used as weights in the estimation.

Table 4: Effects of the BF Program (extensive margin) on Presidential Election

	First round				Second round			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Turnout	Inc	Opo	Invalid	Turnout	Inc	Opo	Invalid
% of BF beneficiaries	0.07715***	0.05838***	0.00349	0.01529***	0.08286***	0.06116***	0.01503***	0.00667***
	(0.00589)	(0.00647)	(0.00537)	(0.00321)	(0.00564)	(0.00579)	(0.00542)	(0.00247)
Observations	651,506	651,506	651,506	651,506	651,506	651,506	651,506	651,506
R-squared	0.96500	0.98805	0.99021	0.91521	0.96512	0.98845	0.99081	0.92413
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year*Place FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Station FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The control variables include: marital status (percentage of single, married, divorced or widower), groups of age (percentage of 16, 17, 18-20, 21-24, 25-34, 35-44, 45-59, 60-69, 70-79, 79 years of age or older), educational level (percentage of illiterate, "read and write" but no formal education, primary incomplete, primary completed, secondary education incomplete, secondary education completed, college incomplete or college completed) and gender (percentage of men and women). The results are clustered at the municipality level and the number of registered voters in each polling booth are used as weights in the estimation.

Table 5: Decomposing the Effects of the BF Program on Presidential Election

	First round			
VARIABLES	Turnout	Inc	Opo	Invalid
Since the beginning	0.05571***	-0.00393	0.02203	0.03761***
	(0.01776)	(0.02187)	(0.01897)	(0.01266)
Entered during 2004-2006	0.11487***	0.07105***	0.04827***	-0.00446
	(0.01107)	(0.01424)	(0.01380)	(0.00702)
Entered after 2006	0.06465***	0.06455***	-0.01965**	0.01975***
	(0.00733)	(0.01150)	(0.00861)	(0.00427)
	Second round			
VARIABLES	Turnout	Inc	Opo	Invalid
Since the beginning	0.07167***	-0.00388	0.04793***	0.02761***
	(0.01848)	(0.02122)	(0.01812)	(0.00924)
Entered during 2004-2006	0.11661***	0.03007**	0.08300***	0.00353
	(0.01078)	(0.01235)	(0.01259)	(0.00557)
Entered after 2006	0.07016***	0.08705***	-0.02098***	0.00409
	(0.00747)	(0.00842)	(0.00727)	(0.00379)
Controls	Yes	Yes	Yes	Yes
Year*Place FE	Yes	Yes	Yes	Yes
Station FE	Yes	Yes	Yes	Yes

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. The control variables include: marital status, groups of age, educational level and gender. The results are clustered at the municipality level and the number of registered voters in each polling booth are used as weights in the estimation.

Table 6: Effects of the BF Program (intensive margin) on Presidential Election

	First round				Second round			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Turnout	Inc	Opo	Invalid	Turnout	Inc	Opo	Invalid
Value *per capita* from BF (R$ 100)	0.03745***	0.03957***	-0.01754***	0.01541***	0.04418***	0.06068***	-0.02238***	0.00587***
	(0.00327)	(0.00475)	(0.00338)	(0.00227)	(0.00316)	(0.00386)	(0.00320)	(0.00172)
Observations	651,506	651,506	651,506	651,506	651,506	651,506	651,506	651,506
R-squared	0.96496	0.98805	0.99021	0.91525	0.96509	0.98848	0.99081	0.92413
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year*Place FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Station FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The control variables include: marital status (percentage of single, married, divorced or widower), groups of age (percentage of 16, 17, 18-20, 21-24, 25-34, 35-44, 45-59, 60-69, 70-79, 79 years of age or older), educational level (percentage of illiterate, "read and write" but no formal education, primary incomplete, primary completed, secondary education incomplete, secondary education completed, college incomplete or college completed) and gender (percentage of men and women). The results are clustered at the municipality level and the number of registered voters in each polling booth are used as weights in the estimation.

Youth Responses to Cash Transfers: Evidence from Brazil

Cecilia Machado, V. Pinho Neto, and Christiane Szerman*

April 5, 2018

Abstract

Identifying successful interventions for disadvantaged youth has recently proven challenging. This paper examines the effectiveness of cash assistance targeted to this group. We exploit an exogenous variation in the provision of cash transfers in Brazil to credibly identify how an additional year of exposure at the critical age of 18 impacts on educational, labor market, and economic self-sufficiency outcomes. We use individual-level administrative data of the largest conditional cash transfer program in the world and link them to educational and formal labor market records. We do not find evidence of significant effects of additional exposure to the program on educational attainment and economic self-sufficiency. However, we observe a small (but still positive) impact on school enrollment, which is mostly driven by male beneficiaries. We also find effects on formal labor supply only for men. For them, we show that one additional exposure to the program decreases the probability of working in the formal sector by 5.38 percentage points during the extra year of exposure. Five years later, this pattern reverses to an increase in participation in the formal labor force.

Keywords: welfare programs, conditional cash transfer programs, disadvantaged youth, education, labor market outcomes, self-sufficiency.

JEL Classification: I25, I28, I32, I38, J13, J22.

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1 Introduction

Welfare programs in developing countries have rapidly expanded over the past several years for disadvantaged citizens. Noteworthy examples are cash transfer programs, which are established to reduce the persistence of poverty across generations by providing opportunities to improve the educational and health outcomes. These programs have been successful in reducing poverty and inequality rates and providing incentives for parents to invest in health and education of their children (Gertler (2000), Gertler (2004), Schultz (2004), Fiszbein et al. (2009)). In designing such schemes, a key feature of interest is targeting (De Janvry and Sadoulet (2006), Ravaillon (2009), Alatas et al. (2012)): for which age group are the conditional transfers mostly effective?

In general, many cash transfer programs strategically set an upper limit to eligibility at primary school age in order to boost school enrollment and prevent early dropout.[1] Over time, these programs might be scaled up to reach other vulnerable groups.[2] Because cash assistance might be very costly to administer (Benhassine et al. (2015))[3], changes in targeting inevitably lead to questions about their effectiveness. In particular, both policymakers and scholars are interested in understanding whether eligibility extension effectively generates benefits that exceed its costs, in the sense that an extra exposure to these programs raises the probability of better outcomes in the future. Nonetheless, identifying causal effects of eligibility extension is challenging for two main reasons. First, as cash assistance is often not randomly assigned, it is difficult to disentangle the impacts of eligibility extension from other possible influences of unobservable differences between recipients. Second, lack of detailed administrative data is another common constraint for researchers, especially in developing countries.

In this paper, we overcome these challenges by investigating the impacts of eligibility

[1] To name few examples, the Mexican PROGRESA program provides monthly transfers to mothers with children enrolled in grades 3-9. The Colombian program consists of payments to parents of children enrolled in both primary and secondary schools. In Nicaragua, the program is focused on children in primary school (see Glewwe and Muralidharan (2015) for details).

[2] For example, the extension of eligibility for children beyond the upper age limit of standard eligibility can be implemented to include youth and enhance their enrollments in post-secondary education. For instance, in 2003, a new component of the Mexican PROGRESSA program (*Jóvenes con Oportunidades*) was created for youth to incentivize them to finish high school and support their transition to adulthood. In Brazil, the *Bolsa Família* program expanded in 2008 to reach youth aged 16 and 17 as well.

[3] Because targeting and conditionalities are features that make these programs very costly to administer (Benhassine et al. (2015)) and budgets are inevitably tight, a cost-benefit analysis of targeting is essential to ensure that these programs are tailored to produce the highest possible impact.

extension, in the context of a large-scale welfare program in a developing country, on educational, labor market, and economic self-sufficiency outcomes. Currently reaching about 14 million households, or equivalently 50 million individuals, the Brazilian *Bolsa Família* program is the largest conditional cash transfer program in the world (Brollo et al. (2012), Brollo et al. (2015)). In 2015, about 27.7 billion BRL (equivalent to 8.7 billion USD) were given to families. Created in 2003, this program initially targeted poor families with children up to 15 years of age with the goal of promoting immediate poverty alleviation and reinforcing their access to basic services in education and health.[4]

The positive impact on primary education[5] (De Janvry et al. (2012), Glewwe and Kassouf (2012)), combined with worryingly low enrollment rates in secondary education for poor young people aged between 15 and 17 years old, culminated with the expansion of the program. In March 2008, the federal government announced that the program would also reach disadvantaged youth aged 16 and 17 years old. In particular, they would become eligible to receive cash transfers until the end of the academic year of their 18^{th} birthday if they are regularly enrolled in school and attending at least 75% of academic days.

This paper exploits a unique feature of the program — the exclusion rule. After the implementation of a new benefit for youth, recipients who were born until December 31^{st} become immediately ineligible for the benefit when turning 18 years old. On the other hand, those turning 18 years old after January 1^{st} are still eligible for an entire extra year of cash assistance if they are enrolled in school. We take advantage of this sharp discontinuity embedded in the exclusion rule to evaluate the effects of a higher exposure to cash transfer program on educational, labor market, and economic self-sufficiency outcomes for five cohorts of interest. In our setting, we examine whether beneficiaries that were born slightly before and after the birthday cutoffs exhibit persistent differences in future outcomes after an additional exposure to the program. To further support the validity of our research design, we do not find evidence of manipulation in the running variables or

[4] Vulnerable children were eligible to receive conditional cash payments until the end of the academic year of their 16^{th} birthday if they were regularly enrolled in school and attending a minimum of 85% of school days.

[5] Using a survey data of selected municipalities in the Northeast of Brazil, De Janvry et al. (2012) find that the Bolsa Escola, which was subsequently incorporated into the current *Bolsa Família*, had a strong impact on school attendance by reducing dropout rates by 8 percentage points. Glewwe and Kassouf (2012) reinforce these results with a nationwide data, the Brazilian School Census. Overall, the authors find that the program effectively raised enrollment, increased grade promotion rates, and reduced dropout rates.

sharp discontinuities in observable characteristics around the thresholds. We restrict our analysis to a specific, but still representative, state to ensure that our quasi-experimental design is not confounded by school starting age.[6]

We highlight that the advantage of our empirical approach over much of the existing literature stems from not relying on *per capita* income eligibility thresholds used to identify potential beneficiaries for social welfare programs. These thresholds can be highly manipulated in several ways (Camacho and Conover (2011), Firpo et al. (2014)). For instance, income information are often self-reported and people can change their answers in the questionnaire during the registration process in order to meet the eligibility criteria. Another type of manipulation can be individuals adversely adjusting labor supply, especially in a country in which the informal sector accounts for a large share of employment.

We use a comprehensive administrative data from the program covering the universe of all recipients, which contains detailed information on various household and individual characteristics. We combine the universe of young beneficiaries during the period of 2009 and 2014 with other educational and labor market administrative data[7] to construct a unique panel dataset with detailed information on cash payments from the program, as well as educational and formal labor market outcomes for each recipient. To our knowledge, we are the first researchers to link these sources together.

We present three sets of results. We start by assessing whether one additional year of exposure to the program encourages recipients to attend school. We do so by evaluating its impact on the probability of not being enrolled in the school, being high school graduate, educational attainment in elementary school, educational attainment in high school, and probability of being enrolled in college until two years after the birthday cutoffs. Preliminary results suggest small, albeit positive, effects on school enrollment, but no impacts on educational attainment.

Second, we examine whether an additional exposure to cash transfers impacts on early-life formal labor market outcomes. This topic is particularly of interest in a context in which informality rates reach about 33% of employed workers and social welfare programs

[6]In some schools, the threshold date for mandatory enrollment is December 31^{st}. Given that Brazilian states are granted autonomy to decide these cutoff dates, the sample is restricted to Rio de Janeiro in which the birthday cutoff date to start school is not December 31^{st}.

[7]We link administrative data from the *Bolsa Família* Program to the School and Higher Education Censuses, as well as to the Brazilian matched employer-employee dataset.

often require recipients to not be employed in the formal labor market (Levy (2010), Gerard and Gonzaga (2016)). In our setting, we are able to credibly investigate whether there is a disincentive effect to work in the formal sector due to extension eligibility in a cash transfer program. We find strong evidence of behavioral responses to cash incentives. Our preliminary findings indicate that a higher exposure to the program is associated with smaller participation and earnings in the first year in the formal labor market, suggesting that beneficiaries are induced to not work in the formal sector only when they are still eligible to receive cash transfers. Nonetheless, this effect is not persistent and becomes positive five years later. When we divide the analysis by gender, we find that these effects are concentrated on males. We show that male beneficiaries born after the cutoff birthdays are less likely to be employed in the formal labor market by about 5.38 percentage points (p.p.) in comparison to those born immediately before these cutoff dates only in the first year. Over time, when all recipients became ineligible, this negative difference tends to fade away. After five years, it becomes positive with an increase of 10 p.p. It is important to notice that our analysis presents a very important limitation: we are not able to track individuals in the informal sector due to lack of data. We then are not able to identify, for instance, whether a lower participation in the formal sector is counterbalanced by a higher labor supply in the informal sector.

Last but not least, we examine the persistence of poverty across generations. We investigate whether the additional year of exposure changes the likelihood of program participation in subsequent years. We consistently do not find any relevant effect on the probability of relying on the program support in later years. Taken as a whole, these three results somewhat support the skepticism about the effectiveness of educational interventions for disadvantaged youth, given that the harmful effects of poverty might be too ingrained and improving academic outcomes can be very challenging and costly (Cook et al. (2014)). Interventions targeting early childhood are more likely to generate larger private and social benefits (Heckman (2006), Heckman et al. (2013)) rather than interventions targeting youth. Therefore, such targeting should take into account the gender differences in terms of behavioral response to cash transfers.

Related Literature:

A large literature has studied the effects of social welfare programs on economic outcomes[8], including for youth (Deshpande (2016)). In developing countries, the introduction of these programs is frequently followed by an increase in time spent in schools (see Glewwe and Muralidharan (2015) for an overview). Although the positive association between the provision of cash transfers and economic outcomes has been extensively documented in many recent works (Schultz (2004), de Janvry et al. (2006), Bobonis and Finan (2009), Fiszbein et al. (2009), De Brauw and Hoddinott (2011), De Janvry et al. (2012), Dubois et al. (2012), Glewwe and Kassouf (2012)), we note that much of the existing studies typically overlook the impacts of specific components of the programs. In particular, designing the programs' targeting is crucial to achieve greater efficiency (de Janvry et al. (2006), Ravaillon (2009), Alatas et al. (2012)) because it is not clear that these programs always generate positive outcomes for all recipients.[9] We contribute to this literature by presenting negligible effects on different economic outcomes when we consider a marginal exposure to the cash transfer program.[10] Our findings also underscore the importance of producing a cost-benefit analysis of targeting based on age and gender.

More broadly, this paper is also related to an emerging literature on youth disengagement. The growing number of young people who are neither working nor studying in recent years, especially in developing countries, raises questions about the effectiveness of interventions to tackle this issue (Jensen (2010), Cullen et al. (2013)). There are remarkably few overarching programs that have produced positive impacts on various outcomes for disadvantaged adolescents. For instance, Cook et al. (2014) argue that there is a strong

[8]More broadly, there is a growing empirical literature estimating the medium- and long-term impacts of safety net programs on economic outcomes in adulthood in the U.S. For example, Aizer et al. (2016) study the long-term effects of the first government welfare program and find that cash transfers are associated with an increase in longevity, possibly due to better outcomes in education, nutritional status, and income. Another related work is Hoynes et al. (2016), who find that *in utero* exposure to Food Stamp Program increases economic self-sufficiency in the future. Price and Song (2016) investigate the long-term impacts of cash assistance through the Income Maintenance Experiment in Seattle and Denver. The authors find no sizable effects of the program on various outcomes for children.

[9]For instance, Galiani and McEwan (2013) take advantage of the stratified design of a randomized experiment in Honduras to show that the positive effects on educational outcomes are only found for the poorest strata. Meanwhile, the impacts in richer, but still poor, strata are close to zero.

[10]For more references on the expansion of the program in Brazil, see Reynolds (2015) and Chitolina et al. (2016). Reynolds (2015) examines the impact of the 2008 eligibility extension to 16- and 17-years-old. The author finds that receiving one additional year of *Bolsa Família* is associated with a significant increase in school attendance when comparing 16-years-old individuals who were eligible to continuously receive the benefit to those 17-years-old individuals who had a gap of one year in treatment eligibility. The author does not find evidence of a decrease in labor market participation. Chitolina et al. (2016) show evidence that the effects on education are stronger for young males than for females. They also find that the impacts on attendance were greater in the Northeast and Southeast regions.

mismatch between what the students — especially those from less affluent backgrounds — need and what the schools deliver. In this sense, the authors exploit an intervention that provides social-cognitive skills training and find positive impacts on grades and graduation rates. Oreopoulos et al. (2014) evaluate the effects of a large youth support program in Canada, the Pathways to Education, and find sizable effects on high school graduation and post secondary enrollment rates. Heller et al. (2016) present the results of three interventions targeted to disadvantaged male youth to reduce crime engagement. The authors find a reduction in several crime measures and an improvement in school engagement. They further exploit why these programs change youth behavior. On the opposite side, critics of these programs argue that more resources should be devoted to early childhood interventions instead of being invested on youth (Heckman and Carneiro (2003), Heckman (2006), Heckman et al. (2013)). The results presented in this paper bring new evidence to this debate. We show that eligibility extension of cash payments to youth does not generate sizable impacts on educational attainment and economic self-sufficiency outcomes. On the contrary, we find suggestive evidence of behavioral response of cash transfer incentives by reducing incentives to work in the formal sector (Foguel and Barros (2010), Ribas and Soares (2011), Banerjee et al. (2015), de Brauw et al. (2015), Garganta and Gasparini (2015)). However, these disincentive effects are not persistent over time and are observed only for men.

The remainder of the paper is organized as follows. In Section 2, we discuss the educational system and the institutional context of the *Bolsa Família* program. Section 3 describes the data in details. In Section 4, we outline our empirical model. Section 5 describes our main findings. Finally, Section 6 describes the next steps and offers some concluding remarks.

2 Institutional Context

2.1 Education in Brazil

In the 2000s, Brazil has experienced a robust economic growth and a sharp decline of social inequality and poverty rates. Meanwhile, the country has also achieved universal enrollment of primary-school aged children, particularly after the introduction of conditional cash transfer schemes. Nonetheless, the quality of free public schools still remains at lower levels.[11]

In terms of academic structure, the academic year typically runs from February until December. The education system is divided into three categories: primary (grades 1-5), lower secondary (grades 6-9), and upper secondary education (grades 10-12). For children aged 6-14, education is compulsory and free. In 2009, the Brazilian Congress enacted a new constitutional amendment that increased the length of compulsory and free education from 9 to 14 years. The new law stipulates that children from 4 to 17 years of age would be required to attend school, but it is expected to phase out by the end of 2016.[12]

Current numbers suggest that the universalization of secondary education is quite far from being reached. In 2013, only 54.3% of young people up to 19 years of age have completed upper secondary schooling, while the average fraction in OECD countries is 80%.[13] The National Household Sample Survey (PNAD) indicates that only 54.3% of youth between 15 and 17 years of age are currently enrolled in upper secondary education. Those who did not complete upper secondary schooling and are not studying account for 15.6% of the sample.[14]

Not surprisingly, the number of youth between 15 and 24 years of age who are neither studying nor working has not significantly fallen over the past decade. This number has actually increased in the last few years, following the trend in Latin American countries. In 2014, one in five Brazilian youth — which represent nearly 7 million young people — are neither in school nor in the labor market.[15]

When directly asked about their main reasons for dropping out of school[16], approximately one-fourth of 15-17 years old teenagers reported the lack of income (e.g. need to work, need to help at home, not having funding for school expenses, etc.) as the primary

[11] The Basic Education Development Index (IDEB), which measures the quality of public schools, has been stagnated in 3.7 points (on a scale from zero to ten) in the last years. In comparison to the 65 countries that participated in the 2012 PISA Exam, Brazil's performance is below the OECD average in mathematics (ranks between 57 and 60), reading (rank between 54 and 56) and science (rank between 57 and 60).

[12] We still do not have new data to evaluate the compliance of this law by the end of 2016.

[13] The significant proportion of youth who are in the wrong grade for their age, which is explained by the students who repeat the school grade and age-grade distortion rates, is another serious problem in the Brazilian educational system.

[14] The remaining population is found in different activities: 19.6% are still attending lower secondary school; 1.7% are attending youth and education program; 2.6% are found in the higher education system; 0.3% are those who are preparing to enter college; and 5.9% have already completed high school.

[15] Source: World Bank.

[16] Supplementary questionnaires of the 2004 and 2006 PNAD ask directly to a group of 15-17 years old adolescents who do not attend school their main reasons for leaving school.

cause. One-tenth of the sample claimed that supply issues (e.g. students have disability or disease, lack of spots in schools, lack of schools next to home, lack of transportation arrangements, etc.) play a key role. Strikingly, more than 40% of dropouts mentioned pure lack of interest by students or parents who do not regard school as an attractive option.[17]

The consequences of dropping out school often involve harsher economic and social prospects. People who dropped out of school are more likely to experience worse job prospects, given that they earn substantially lower wages and have higher probability of unemployment, when compared to those who completed secondary education (Neri et al. (2009)). Youth face additional limitations in the labor market: unemployment rates for them are 2 or 3 times higher than for adults, they experience stronger barriers to enter the labor market, and they present higher risks to lose their jobs. Disadvantaged youth also face higher levels of informality and more unemployment spells (Calero et al. (2016)). Taken together, it is not surprising that young people who dropped out of school represent one of the most vulnerable groups in both formal and informal labor markets, with weak attachments and more frequent dismissals.

Financial constraints and need to help family inevitably pull poor students out of school, even in a context in which public schools are free. Therefore, the provision of financial incentives can effectively alleviate their harsh economic situation. Conditional cash transfer is an example of these incentives.

2.2 *Bolsa Família* Program

In October 2003[18], the federal government created the *Bolsa Família* Program (henceforth "BFP") to consolidate four existing cash transfer programs[19] into a single program (Lindert et al. (2007)). According to the Ministry of Social Development (MDS), the program is designed to accomplish three major goals: (1) promote an immediate poverty alleviation; (2) reinforce access to basic social services in education and health in order

[17] Other 20% report other causes that are not included in the previous categories.

[18] The *Bolsa Família* program was initially established by Provisional Measure 132, which was converted into Law 10.836 in January 2004.

[19] Prior to BFP, the four major cash transfer programs targeted to the poor were: 1) the School Allowance (or *Bolsa Escola*), which provided conditional transfers to boost school enrollments for poor families with children age 6 to 15; 2) the Food Allowance (or *Bolsa Alimentação*), which was a health and nutrition program focused on improving nutritional conditions and decreasing infant mortality; 3) the Gas Aid (or *Auxílio Gás*), which consisted of cooking gas subsidies; and 4) the Food Card (or *Cartão Alimentação*), designed to eradicate extreme hunger by stimulating food purchases.

to break the persistence of poverty across generations; and (3) coordinate supplementary services to empower poor families to overcome poverty and social vulnerability.

Registering in the *Cadastro Único* is necessary to qualify for the benefits.[20] The registration process is completely decentralized. While the federal government establishes the number of poor families to survey and register in the system[21], all municipalities conduct the household registry process by identifying and interviewing poor families to fill up this quota. Local governments are responsible for enrolling eligible families in the program, registering and updating the *Cadastro Único* database, and monitoring whether the families meet all conditionalities. The federal government establishes the rules, controls the approval and cancellation of benefits, and provides payments to beneficiaries.

After registering in the *Cadastro Único* database, only families living in "poverty" and "extreme poverty" conditions can enroll in BFP.[22] Current rules define that "extremely poor" families are those with *per capita* income up to 85 BRL (equivalent to 26 USD) per month, while "poor" families are those with *per capita* income between 85 BRL and 170 BRL (52 USD) per month. Two eligibility criteria determine the final amount of transfers for each family: demographic composition (that is, the number of family members and their age) and income.

There are two types of payments: conditional and unconditional. While all "extremely poor" families receive an unconditional payment (the basic benefit) per month[23] for the entire family, regardless of their demographic composition or the number of family members, "poor" families are not eligible to receive this basic benefit. In addition to the unconditional transfer for "extremely poor" families, the program also provides a conditional stipend (the variable benefit) to "poor" and "extremely poor" families with children under 18 years of age (until 2008, 16 years of age) or pregnant (or lactating) mothers. The final amount of conditional transfers largely depends on the number of family members who are children or pregnant (or lactating) mothers. These transfers involve some education

[20] *Cadastro Único*, or Single Registry for Social Programs of the Federal Government, was initially conceived to register all poor families in the country to facilitate their access to safety net programs. The *Cadastro Único* is a crucial tool to identify poor individuals and run the *Bolsa Família* program, as well as other numerous social programs and services.

[21] The number of poor families to reach in a municipality is previously established from decennial Census.

[22] Even though eligibility is based on self-reported income, home interviews and visits might be conducted to verify whether all information are valid. The *per capita* income thresholds to define"poverty" and "extreme poverty" conditions are not stable. They have changed over time.

[23] In 2016, the stipend was BRL 85 per month.

and health requirements. For pregnant or lactating women, the requirements are prenatal and postnatal care, as well as participation in educational health and nutrition seminars. For all children under the age of seven years, health requirements involve compliance with childhood immunization schedule and regular monitoring visits. For children aged 6-15, a minimum school attendance of 85% of school days is compulsory.

Currently reaching nearly 14 million households, or equivalently around 50 million people, BFP is probably the largest cash transfer scheme in developing countries. Since its inception, the program has expanded geographically and the values of the benefits have changed. New stipends have been incorporated into the program over time with new eligibility criteria. This paper focus on one of these stipends, the Variable Benefit for Youngsters (hereafter, BVJ)[24], created by the federal government in March 2008.

The positive impact on primary education[25], combined with low school enrollment rates for poor young people aged between 15 and 17 years old, was the main reason behind the creation of BVJ. This stipend consists of conditional cash transfers to both "poor" and "extremely poor" families with members between 16 and 17 years of age enrolled in school. The education requirement is a minimum school attendance of 75%.[26] Extending the upper age limit for eligibility is expected to improve educational outcomes for disadvantaged youth. Currently, each family is allowed to receive up to two BVJ benefits.

Exclusion Rule: As previously mentioned, the BVJ benefits target poor youth until the age of 18, aiming to keep them enrolled in school until that age. Because the school year typically runs from February to December, stipends are provided until the end of the

[24] Although other variable benefits were also created, they are out of the scope of this paper.

[25] De Janvry et al. (2012) and Glewwe and Kassouf (2012) rigorously examine the impact of the provision of conditional cash payments to poor families with children between 6 and 15 years of age on educational outcomes. Using a survey of selected municipalities in the Northeast of Brazil, De Janvry et al. (2012) estimate that the Bolsa Escola — which was subsequently incorporated into the current *Bolsa Família* — had a strong impact on school attendance by reducing dropout rates by 8 percentage points. Glewwe and Kassouf (2012) reinforce these results with a nationwide data, the Brazilian School Census. Overall, the authors find that the program not only effectively reduced dropout rates by 0.5 percentage points for $2nd$ to $5th$ graders and 0.4 percentage points for $6th$ to $9th$ graders, but also raised enrollment and grade promotion rates. These results are consistent with international evidence that CCTs generate positive impacts on a wide range of educational outcomes for children in many developing countries (Schultz (2004), Gitter and Barham (2008), Behrman, Parker and Todd (2009), Attanasio et al. (2010)).

[26] Reynolds (2015) exploits the 2008 eligibility extension to 16- and 17-years-old and finds that receiving one additional year of the program is associated with a significant increase in school attendance when comparing 16-years-old individuals who were eligible to continuously receive the BVJ stipend to those 17-years-old individuals who had a gap of one year in treatment eligibility. Our paper does not exploit the 2008 eligibility. Instead, we focus on the exclusion rule in force after 2008 for individuals who receive the BVJ benefits.

academic year in the year when the recipient turns 18 years old. Thus, if the participant is regularly enrolled in school, the exclusion process does not occur immediately after the birthday. Instead, the benefit is only canceled by the end of the school year. For example, a youth who completed 18 years of age shortly after December 31^{st}, 2012 could remain in the program over the next year (conditional on school enrollment). By contrast, a youth who turned 18 slightly before that date was no longer qualified for BVJ in 2013. Our empirical strategy exploits the ineligibility rule induced by the 18^{th} birthday after 2008, as we describe in details later.

3 Data

3.1 Data Description

We have access to five confidential administrative sources, heretofore not used to link together: (1) the *Cadastro Único* database; (2) BFP payroll data; (3) the School Census; (4) the Higher Education Census; and (5) RAIS, the Brazilian matched employer-employee dataset. In this paper, we track five cohorts of interest over time by recovering their educational and employment records in the formal labor market between 2009 and 2015. In Section 3.2, we explain in more details how we construct our final cohorts.

The first two sources of data come from MDS. BFP payroll datasets consist of monthly information on all transfers made by the federal government to all individuals enrolled in the program. The details of these payroll datasets allow us to distinguish all benefits each family receives, including the basic and variable ones. We use monthly payroll data spanning the period between 2009 and 2015.

Payroll datasets can be linked to *Cadastro Único* through social identification number (NIS), which is unique for all beneficiaries of social safety net programs in the country. *Cadastro Único* contains detailed information on individual and family characteristics, including dwelling characteristics (e.g. address, total number of rooms, sanitation, water source, etc.), income sources (e.g. labor income, retirement benefits and unemployment benefits, etc.), and expenses (e.g. rent, food, electricity, transport, etc.). We use this source to recover individual and household characteristics.

Educational outcomes are drawn from the National Institute for Educational Studies and Research (INEP). The main source is the School Census, which contains detailed information on all private and public schools in Brazil.[27] Our analysis employs yearly data from 2009 to 2014.[28] We match individuals in the payroll data to these School Censuses using the following sequential linking variables: first, name and date of birth; second, the social identification number; third, name and mother's name; fourth, mother's name and date of birth. We ensure that individuals are uniquely identified for the matching procedure. Our matching rate is about 80% for the studied cohorts.

All schools are required to update students' enrollment status[29] and grade level.[30] By combining information on enrollment and situation, we construct the following indicator variables for whether: (i) is not enrolled in the school; (ii) is a high school graduate; (iii) educational attainment is elementary school and (iv) educational attainment is high school.

Our analysis on educational outcomes are also supplemented by the Higher Education Census, which provides a comprehensive overview of all college institutions and students in the country. We limit the years of the Censuses to the period between 2009 and 2014. We use the Higher Education Censuses to identify whether and when the individual was enrolled in college for the first time. We create an indicator variable for whether the student is enrolled in college institution.[31]

To investigate the effects on labor market outcomes, we use RAIS (*Relação Anual de Informações Sociais*), the Brazilian matched employer-employee dataset provided by the Ministry of Labor. We exploit annual datasets spanning the period between 2009 and 2015. The data consist of identifiers with name, date of birth and social identification number, which allow us to track all individuals in the formal labor market. We match the BF payrolls with employment records from RAIS using beneficiaries' social identification number.[32] We use RAIS to construct the following outcomes: (i) labor market participation, which is an indicator variable for whether the individual ever appears in RAIS in the

[27]Each school principal fills out a questionnaire with information on schools' infrastructure, teachers, classrooms and students.

[28]We plan to supplement our analysis with the 2015 School Census soon.

[29]Schools must inform to students' status at the end of each year. There are six possible status: pass (original status: *aprovado*), fail (*reprovado*), abandonment (*abandono*), deceased (*falecido*), missing (*sem informação de rendimento, falecimento* or *abandono*), and graduated (*concluinte*). Only restricted access data provide these complete information on students' status.

[30]If the same student is found in different grades in the same year (it can occurs because the same student can be found in different schools, for example), we consider the highest grade level.

[31]If the same recipient is found in both School and Higher Education Censuses in the same year, we consider the highest education level, which is the college education.

[32]*Caixa Econômica Federal* is responsible for issuing social identification numbers (NIS), which are the same than the workers' identification codes (PIS) found in RAIS datasets.

current year; and (ii) earnings, which is reported as the average annual wage (in minimum wages).

Furthermore, we are also interested in estimating the persistence of poverty across generation (that is, economic self-sufficiency). We use payroll data to construct an indicator variable for whether the individual receives any stipend from the *Bolsa Família* program in subsequent years. Because payroll data allow us to identify whether the recipient is a dependent or a household head, we track individuals over time and check whether they rely on BFP support in the future by verifying whether they have dependents enrolled in the program. In most cases, these dependents are their children, but this condition is not necessary.[33]

3.2 Sample

We take a number of steps to construct our sample of interest. Since 2008, we are able to exploit the exogenous variation generated by the exclusion of BVJ beneficiaries after their 18^{th} birthday. Our first sample is drawn from the payroll data of December 2009. It comprises individuals who were born between November 1, 1991 and February 28, 1992, and received the BVJ benefit in December 2009. As explained before, those who were born in 1992 could receive the variable benefit from January to December of 2010, but those who were born in 1991 became ineligible to receive this benefit over the same period. We refer this sample as Cohort 1. Similarly, from the payroll data of December of each year from 2011-2014, we track individuals who were born between November/1993 and February/1994, November/1994-February/1995, November/1995-February/1996, and November/1996-February/1997, respectively. Thus, our analysis consists of five cohorts (see Table 1).

After outlining the cohorts and restricting them to the dates of birth of interest, we drop individuals that were **not** born in the state of Rio de Janeiro.[34] We do so to avoid the birthday cutoffs coinciding with the school starting age. In many schools, the cutoff date for compulsory enrollment is December 31^{st}, which can be a serious confounding factor to our research design. One might argue, for this reason, that any positive effect

[33]The program gives priority to women to register the household head. To estimate the effects on economic self-sufficiency, we restrict the sample to female recipients.

[34]Information on place of birth is drawn from *Cadastro Único*.

on educational attainment is a result of people born in January starting school later than people born in December, rather than being the actual impact of an additional exposure to BFP. Thus, we restrict the sample to individuals born in the state of Rio de Janeiro, where cutoff dates to start school are not December 31^{st}.

Table 2 shows descriptive statistics for the full sample in Column 1 and after restricting to Rio de Janeiro in Column 2. We note that the sample of Rio de Janeiro remains similar to the full sample in many observable characteristics, reinforcing our interpretation that the restricted sample is virtually identical to the full sample. Also, we present the sample for Rio de Janeiro by using a 30 days window (Column 3). In addition, the sample we use in the estimation contains those individuals matched to the School Census, namely **matched sample**, as presented in Column 4.

4 Empirical Strategy

4.1 Research Design

We study the effects of providing one additional year of transfer to youth on educational and labor market outcomes by exploiting a unique exogenous variation in the provision of benefits created by the discontinuity in date of birth. In this case, identification is based on comparing the outcomes of "treated" beneficiaries, born on or just to the right of cutoffs, with "untreated" beneficiaries, born just to the left of cutoffs. Our identification strategy hinges upon the assumption that assignment to the treated group is as good as random *near* the eligibility cutoffs and other characteristics associated with the outcomes of interest remain similar. We argue that individuals below the cutoff can be a credible counterfactual group for individuals above the cutoff. The only difference between both groups is that individuals above the cutoff received additional transfers for one year.[35]

Our estimation sample consists of five cohorts of interest and we run regressions for the pooled cohorts. Our baseline model is described by the following regression:

[35] Using PNAD data, Barbosa and Corseuil (2014) compare households who receive the basic benefit and have the youngest child turning 16 years old immediately after December 31^{st}, 2005 with those with the youngest child turning 16 slightly before this date. Our approach is different in several dimensions. First, we focus on the exclusion induced by the BVJ benefits, rather than the basic benefit. Second, we extend our analysis to educational and self-sufficiency outcomes, instead of limiting to labor market outcomes. Third, our unit of observation is an individual, not a household head.

$$y_{ik} = \alpha + f(a_{ik} - c) + \beta * 1[a_{ik} > c] + \gamma * 1[a_{ik} = 01/01] + \varepsilon_{ik} \quad (1)$$

where y_i is the outcome variable of individual i and cohort k; a_i is the date of birth; c is the birthday cutoff after which the individual is eligible to receive one additional year of the program; $1[a_i >$ c] is a dummy variable that takes value one if the individual is born after the birthday cutoff of reference; f($a_i >$ c) is a polynomial distance from the cutoff; and ε_i is an error component. To ensure that our results are not driven by heaping at the cutoff date, we include a dummy for birthday on January 1^{st}. Robust standard errors at the birthday level are reported (Lee and Card (2008)).

We use local linear regressions around the discontinuity to non-parametrically estimate the coefficient of interest β. We estimate the equation above using triangular weighted OLS, which assigns less weight to observations further away from the cutoff, within a chosen window around the cutoff. Our preferred specification considers a window of 30 days below and above the birthday cutoffs, as well as a linear slope on each side of the cutoff.

4.2 Treatment Effect

Before reporting the results, we provide a stringent inspection of the sharp discontinuity induced by the eligibility rules of the program. In particular, we check whether there are differences in the probability of participating in the program for those who were born before and after the birthday cutoffs. To do so, we estimate Equation (1), in which the outcome variable is a dummy variable equals one whether the beneficiary received the BVJ benefit in a specific combination of month and year. For each cohort, we estimate this regression repeatedly over a 36-month window, comprising one year before and two years after the birthday cutoff of reference.

We display graphically all 36 point estimates, in which each point represents one month of the 36-month period of interest. These estimates provide a clear and graphical representation of the treatment effect. In Figure 1, each point represents the difference in the probability of participating in the program between individuals who were born before and after the birthday. The difference ranges from about 78% to 100%. Overall, the treatment effect can be interpreted as the effect of receiving one additional year of the BVJ benefit.

4.3 Validity of the Research Design

In this section, we check for the validity of our empirical strategy. Under key assumptions, the estimation strategy provides as credible estimates as those from randomized experiments (Lee and Card (2008)). The crucial assumptions are that: 1) other factors that might affect our outcomes do not present sharp differences around the cutoffs; 2) assignment to the treated group is as good as random *near* the cutoffs.

What could be more troubling to the first assumption above is the school starting age. As explained before, the cutoff date for compulsory enrollment is December 31^{st}, which can be a serious confounding factor to our quasi-experimental design. Because states are granted autonomy to establish the birthday cutoff dates for school enrollment, we restrict the sample to individuals born in Rio de Janeiro to address any concern related to cutoff dates for compulsory schooling.

In addition, to confirm that the first assumption is valid, we use a regression discontinuity specification to check for the smoothness of observable household and individual characteristics. We consider the following individual characteristics: gender, race, an indicator for whether the recipient resides in the urban area, year of registration in the *Cadastro Único* database, *per capita* family income, presence of piped water in the residence, and total number of family members in the residence. We also take into consideration the following household characteristics: gender, schooling, and a dummy if the head works. The balance tests are conducted by estimating Equation (1) with the matched sample for the pooled cohorts. Table 3 suggests that all estimates are statistically insignificant and close to zero.[36]

We also verify whether there is a manipulation in the running variable around the cutoff to qualify for one more additional year of benefits. For instance, if recipients could manipulate the birthdays reported during the registration process, we then might expect to notice a higher concentration of birthdays slightly above the cutoff. To test this possibility, in Figure 2 we plot a histogram of birthdays relative to the threshold dates for the pooled cohorts, using the matched sample. We do not find any evidence of heaping in the distribution of birthdays above the threshold, which is unsurprising given that the beneficiaries have to present original documents to register in the program. It is reasonable

[36]Because we link the payroll data to the *Cadastro Único* database of 2014 , some observations are not found in the latter.

to assume that it is virtually impossible to manipulate beneficiaries' birthdays.

We supplement the visual inspection by performing McCrary test to check for the presence of a density discontinuities (see McCrary (2008) for more details). As shown in Figure 3, we do not find any statistically significant difference of density in each side of all thresholds. We note that birthday densities are smooth across the cutoffs for each cohort. We interpret these figures as evidence that assignment to the treated group is as good as random *near* the cutoffs.

5 Results

In this section, we estimate any discontinuous change in educational, labor market, and economic self-sufficiency outcomes due to an extra exposure to the conditional cash transfer program at the critical age of 18.

5.1 Effects on Educational Outcomes

First, we investigate whether an extra exposure to the welfare program reflects in higher educational attainment of the studied cohorts. We particularly focus on five outcomes drawn from both the School and Higher Education Censuses: not enrolled in the school, high school graduate, educational attainment in elementary school, educational attainment in high school and enrolled in college.

We select beneficiaries from the payroll data of December of the year immediately before exclusion of the program, which we refer as "year t". Enrollment information in the School Census are annually collected in May, while students' situation are reported by the end of the school year, in December. Due to differences in the timing of data collection, we will consider "year t-1" as the baseline year for educational outcomes. We acknowledge that "year t" corresponds to the year in when those born before and after the birthday cutoffs receive the BVJ benefit. Nonetheless, those born before December will be ineligible shortly after they turn 18 years old by the end of the year, and non-compliance can take time to be finally detected. Therefore, we consider educational outcomes from years t, t+1, and t+2. Table 4 presents descriptive statistics for each cohort.

Table 5 reports the effect of one extra year of exposure to the program on educational outcomes for years t, t+1, and t+2 for the pooled cohorts. We find a small (but still significant) effect on the probability of being enrolled in the school (Column 1). One extra year of BVJ reduced the likelihood of not being enrolled in school by 2.6 percentage points. However, we do not find any significant effect on educational attainment (Columns 2—5).

We also document the results by dividing the sample into men and women, as shown in Table 6, and we find that the overall effect on enrollment is entirely driven by male recipients (Panel C, Column 1). Furthermore, for both genders, we do not find evidence of relevant impact of one extra year on educational attainment. In sum, beneficiaries born after the cutoff dates are not more likely to achieve higher levels of education than the ones born slightly before these dates. All estimates are neither statistically nor economically significant at conventional levels.

Given the negative long-term consequences for both individuals and society, seeking for policies focused on decreasing the number of youth who fail to upper secondary education is relevant. Our findings on educational outcomes support the skepticism about the effectiveness of educational interventions for disadvantaged youth because the harmful effects of poverty might be too ingrained to be reverted (Cook et al. (2014)). Improving their academic outcomes then can be quite difficult and costly, especially in contexts in which stipends are quite small. One possible way to increase educational attainment is through the provision of performance-based incentives tied to academic performance.[37]

5.2 Effects on Labor Market Outcomes

Supporters of welfare programs often argue that they are essential for those who face difficulties in the labor market, while opponents state that they create perverse incentives to push them away from work, given that they often require beneficiaries not be employed in the formal labor market (Levy (2010), Gerard and Gonzaga (2016)). Our context provides us an opportunity to examine whether an additional exposure to a welfare program can be somewhat associated with a reduction in formal labor supply.

Although the RAIS data present remarkably detailed information on all formal workers in the country, they have one important limitation: they do not contain information about

[37] From the international experience, we highlight two initiatives in Latin America: the Youth with Opportunities program (*Jovenes con Oportunidades*), in Mexico, and the Conditional Subsidies for School Attendance program (*Subsídios Condicionados a la Assistencia Escolar*), in Colombia (Barrera-Osorio et al. (2011)). These experiences indicate that incentivizing on graduation rather attendance is particularly effective, generating higher levels of both attendance and enrollment at the secondary and tertiary education.

the informal sector, which accounts for a significant share of employment in the country. In fact, informality rate is about 33% among employed workers. Informality rates are particularly larger for young people aged between 16 and 24, who also represent one of the most vulnerable groups in the formal labor market.

We start by reinforcing the validity of our RD design. Column (1) of Table 7 documents the estimates for Equation (1) focusing on labor market outcomes in the baseline year (which we refer as the "year t"). In Panel A, the outcome of interest (variable ***employment***) is an indicator variable of whether the beneficiary is found in the formal labor market in the baseline year, in which all beneficiaries are still eligible to receive cash payments. Panel B, in its turn, reports the impacts on earnings (variable ***wage***), which are measured by the average annual wage (in minimum wage), also in the baseline year. To minimize selection bias concerns, we replace missing earnings by zero. We find no evidence of a significant discontinuity in labor market outcomes at the cutoff dates for the baseline year.

We now turn to the estimated impacts on labor market outcomes for subsequent years. Columns (2) refers to the likelihood of being employed and earnings in "year t+1", when only beneficiaries born in January are eligible to receive the cash transfer over the entire year. We report regression results until six years after the discontinuity. Table 7 suggests a negative and statistically significant effect on formal labor market participation and earnings only in the first year after the birthday cutoffs. We find that beneficiaries born after the cutoff birthdays are less likely to be employed in the formal labor market by about 3.4 percentage points in comparison to those born immediately before the cutoff date only in the first year after the exclusion of the program. However, this impact is only marginally significant. Two and three years later, this negative difference tends to diminish and eventually fades away, being positive but marginally significant five years later. Comparable results are found regarding the impacts on wage.

When we separate the results by gender (see Table 8), we find a null impact on female participation up to six years after receiving one extra year of BVJ. On the other hand, the extra year induces a negative and significant effect on male employment, reducing the probability of being employed by 5.38 percentage points. The impacts on male employment increase over time and become considerably high and significant after five years (Columns 6 and 7).

5.3 Effects on Economic Self-Sufficiency

The transmission of poverty across generations is a major interest for both scholars and policymakers. The main goal of safety net programs is probably to break this intergenerational transmission by reducing the dependence of vulnerable individuals on government support. Recent evidence suggests that the need of government assistance is quite persistent across generations. This correlation, however, does not necessarily imply causality. Indeed, establishing causality is an important challenge in assessing the impact of welfare programs, given that there is little socioeconomic mobility across generations: children of low-income parents are more likely to also have low incomes in the future.

Identifying a credible counterfactual often requires a randomized (quasi-) experiments with a large sample of individuals. In most cases, these experiments are politically infeasible to implement. We highlight that we do not evaluate whether the program was effective in breaking the transmission of poverty across generations. Instead, we assess whether the provision of one additional year of a conditional cash transfer could affect the probability of receiving any benefit of the program as a household head[38].

In this exercise, it is important to point out that the BFP gives priority to women to be listed as the responsible for the family in the registration process. We use the payroll data to check whether the individuals in our sample become responsible for families enrolled in the program in later years. Because the payroll data identify all recipients as responsible or dependent, we are able to evaluate their dependence on BFP support over time.

Table 9 depicts the results by year for a time horizon of six years. The first column refers to one year after the birthday cutoff, while the last column indicates the estimates for six years after the same birthday cutoff. Panel A refers to results for women, while Panel B shows results for men. In general, we consistently do not find remarkable effects on the probability of relying on the program support in later years, except for a small impact six years later.

6 Conclusion

In this paper, we provide empirical evidence of the relationship between an additional exposure to a welfare program for disadvantaged youth and their educational, labor market

[38]Thus far, our sample consists of dependent recipients, not responsible recipients.

and economic self-sufficiency outcomes. To do so, we exploit a sharp discontinuity induced by the exclusion rule of a very large cash transfer program. In 2008, the Brazilian federal government scaled up the conditional cash transfer program to reach a new group of vulnerable poor individuals: disadvantaged youth aged 16 and 17 years old who were enrolled in school. The rationale behind this expansion is that requirements associated with cash transfers would increase enrollment rates in upper secondary education for youth by reducing their opportunity costs of staying in school.

We take advantage of a unique exclusion rule, which establishes that eligible beneficiaries are only excluded from the program at the end of the school year, not immediately after completing 18. Our unique research design allows us to study five cohorts of beneficiaries born just before and after birthday cutoffs, while the latter are unintentionally eligible to receive one additional year of cash assistance.

Our preliminary results indicate insignificant effects on various educational attainment outcomes. Taken as a whole, we do not find any evidence of increase in educational attainment, but only a small (but still positive) impact on school enrollment, mostly driven by male beneficiaries. In addition, we find that a higher exposure to the program is associated with lower labor force participation and earnings in the formal labor market only in the first year and for male. Over time, this difference in labor market outcomes tends to fade away and, five years later, this pattern reverses to an increase in participation in the formal labor force. Finally, we find no evidence that a further exposure affects participation in the program for the following years.

We plan to examine the channels behind our initial results with a more detailed educational data, as well as look at heterogeneous effects. Our preliminary findings suggest that interventions for disadvantaged youth to entice them to stay in school can be costly and generate negligible benefits that perhaps do not justify their costs. In this vein, early childhood interventions can be more effective to break the persistence of poverty across generations.

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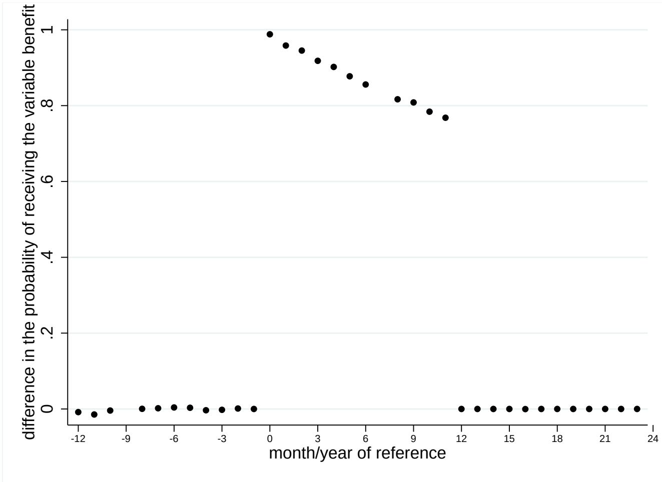
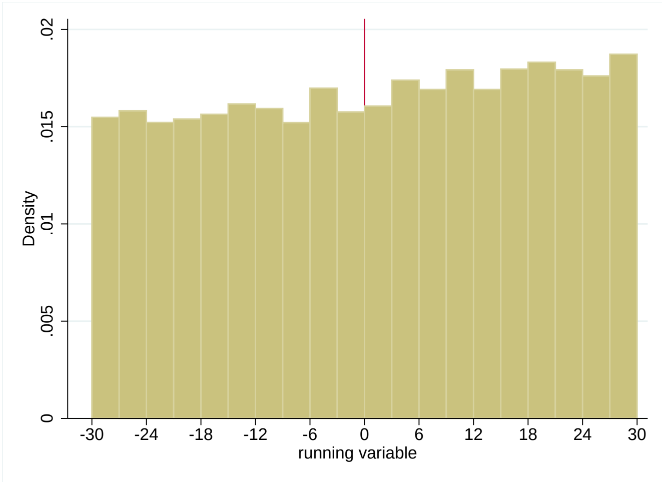
Figure 1: Difference in the Probability of Participating in the BF Program (Pooled Cohorts)



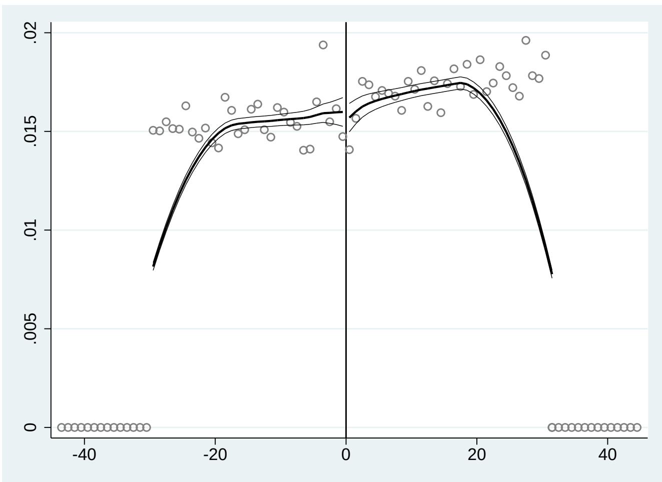
Note: This figure plots the difference in the probability of receiving the variable benefit. To do so, we estimate Equation (1) using triangular kernel and a window of 30 days below and above the threshold. Each point denotes the estimated coefficient for each month in a period of 36 months. The figure reveals that those who were born after the birthday cutoff have high propensity to receive one additional year of the BVJ stipend. N = 34,671.

Figure 2: Density of Birthday Distribution: Pooled Cohorts



Note: This figure shows density of birthday distribution around the birthday cutoffs for five cohorts. We consider a window of 30 days below and above the birthday cutoffs using the final matched sample. Bins have width of three points. N=34,671.

Figure 3: McCrary Density Test: Pooled Cohorts



Note: This figure illustrates the density test proposed by McCrary (2008) for five cohorts. In this distribution, 0 corresponds to the eligibility cutoff birthday to receive treatment. We plot the density of observations by birthdays relative to the threshold. Optimal binsizes and bandwidths are computed as in McCrary (2008). Point estimate (standard error): - 0.029 (0.036). N=34,671.

Table 1: Definition of the Cohorts

Cohort	**Born Between**	**Who Received the Benefit in:**
1	nov/1991 – feb/1992	payroll data from dec/2009
2	nov/1993 – feb/1994	payroll data from dec/2011
3	nov/1994 – feb/1995	payroll data from dec/2012
4	nov/1995 – feb/1996	payroll data from dec/2013
5	nov/1996 – feb/1997	payroll data from dec/2014

Table 2: Summary Statistics and Sample Restrictions

	(1) Full Sample (BR)	(2) Full Sample (RJ)	(3) 30-Days Window (RJ)	(4) Matched Sample (RJ)
	Individual Characteristics			
% female	0.49	0.49	0.49	0.49
% black	0.76	0.71	0.71	0.71
N	1,774,713	92,367	46,529	34,671
	Household Characteristics			
% receive basic benefit	0.87	0.83	0.83	0.84
# benefits by family	2.58	2.46	2.46	2.45
registration year	2009.04	2009.13	2009.15	2009.2
% living in urban areas	0.7	0.91	0.91	0.91
per capita income	77.63	80.14	79.81	80.56
% child labor	0.03	0.01	0.01	0.01
% piped water	0.73	0.87	0.87	0.87
% electricity	0.93	0.96	0.96	0.96
# people	4.3	4.02	4.02	4.02
# rooms	4.6	4.22	4.22	4.25
N	1,774,713	92,367	46,529	34,671
	Head of Household Characteristics			
% female	0.94	0.95	0.95	0.95
% lower sec. school	0.83	0.8	0.8	0.79
% dummy if works	0.41	0.45	0.45	0.45
N	1,666,562	87,637	44,074	32,933

Note: this table reports descriptive statistics for the Pooled Cohort. Table displays means and number of observations on both individual and household characteristics. Column 1 refers to the full sample (Brazil) and in Column 2 the sample is restricted to Rio de Janeiro (RJ). Column 3 comprises the RJ sample using a 30 days window, while the Column 4 refers to this sample matched to the School Census. Sources: *Cadastro Único* database, payroll data, and the School Census.

Table 3: Balancing Test of Several Characteristics

	(1)	(2)	(3)	(4)	(5)
	female	black	urban area	registration year	*per capita* income
extra year	0.0011	-0.0031	-0.0061	0.1381	-2.0919
	(0.0124)	(0.0108)	(0.0083)	(0.1986)	(2.9014)
N	34,671	34,159	34,274	34,240	34,274
	piped water	total family members	female (head)	elemendary school (head)	dummy if works (head)
extra year	-0.0120	-0.0172	-0.0036	-0.0022	0.0038
	(0.0093)	(0.0473)	(0.0058)	(0.0131)	(0.0127)
N	33,446	33,186	32,933	30,350	29,589

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Discontinuity of both individual and household characteristics at the birthday cutoffs for the Pooled Cohort. Local linear regressions consider a window of 30 days below and above the thresholds, triangular kernel and linear slope on each side of the cutoff. We restrict the sample to the state of Rio de Janeiro. The unit of observation is an individual. The name in each column refers to the dependent variables. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: *Cadastro Único* database, payroll data, and the School Census.

Table 4: Summary Statistics of Education Variables

	C1	C2	C3	C4	C5	Pooled
N initial	7,403	5,470	10,371	10,423	11,113	44,780
N available	6,182	5,455	10,011	10,316	10,877	42,841
% matched to School Census in t-1	0.87	0.85	0.82	0.82	0.86	0.84
N sample	**5,391**	**4,653**	**8,218**	**8,448**	**9,332**	**36,042**
	Panel A: Education Level in Year t-1					
% high school graduate	0.00	0.00	0.00	0.00	0.00	0.00
% not enrolled	0.00	0.00	0.00	0.00	0.00	0.00
% elementary school	0.53	0.50	0.45	0.45	0.40	0.46
% high school	0.47	0.50	0.51	0.53	0.56	0.52
% 1st year HS	0.23	0.26	0.26	0.28	0.26	0.26
%2nd year HS	0.21	0.21	0.23	0.23	0.27	0.24
%3rd year HS	0.02	0.01	0.02	0.02	0.02	0.02
% college education	0.00	0.00	0.00	0.00	0.00	0.00
	Panel B: Year t					
% high school graduate	0.01	0.01	0.01	0.01	0.01	0.01
% not enrolled	0.13	0.15	0.17	0.17	0.13	0.15
% elementary school	0.31	0.25	0.23	0.22	0.19	0.23
% high school	0.55	0.57	0.58	0.57	0.66	0.59
% 1st year HS	0.17	0.20	0.18	0.17	0.18	0.18
%2nd year HS	0.15	0.16	0.17	0.18	0.19	0.18
%3rd year HS	0.18	0.17	0.19	0.19	0.24	0.20
% college education	0.00	0.00	0.00	0.00	0.00	0.00
	Panel C: Year t+1					
% high school graduate	0.13	0.12	0.14	0.13	-	0.13
% not enrolled	0.30	0.33	0.33	0.33	-	0.32
% elementary school	0.14	0.11	0.10	0.10	-	0.11
% high school	0.40	0.40	0.39	0.41	-	0.40
% 1st year HS	0.10	0.07	0.05	0.06	-	0.07
%2nd year HS	0.08	0.09	0.09	0.09	-	0.09
%3rd year HS	0.10	0.11	0.11	0.13	-	0.12
% college education	0.02	0.03	0.03	0.03	-	0.03
	Panel D: Year t+2					
% high school graduate	0.22	0.22	0.21	-	-	0.21
% not enrolled	0.45	0.46	0.46	-	-	0.46
% elementary school	0.06	0.05	0.04	-	-	0.05
% high school	0.23	0.22	0.22	-	-	0.22
% 1st year HS	0.04	0.02	0.02	-	-	0.03
%2nd year HS	0.05	0.03	0.03	-	-	0.04
%3rd year HS	0.05	0.05	0.06	-	-	0.06
% college education	0.04	0.05	0.06	-	-	0.05

Note: this table reports descriptive statistics on educational attainment for the Pooled Cohort. Table displays proportions and number of observations. By combining information on enrollment and situation, we track individuals in years t-1, t, t+1, and t+2, and construct the following variables: indicator if not enrolled, indicator if high school graduate, indicator if educational attainment is elementary school, indicator if educational attainment is high school and indicator if individual is enrolled in college institution. Sources: *Cadastro Único* database, payroll data, and the School Census.

Table 5: Effects on Educational Outcomes

	(1)	(2)	(3)	(4)	(5)
	Outcomes				
	not enrolled	high school graduate	elementary school	high school	college education
Panel A: Pooled Cohorts, year t					
after	0.002	-0.003	0.009	-0.002	0.000
	(0.010)	(0.002)	(0.015)	(0.014)	(0.001)
Mean of Dep. Var.	0.150	0.010	0.340	0.632	0.003
Observations	36042	36042	36042	36042	36042
R-squared	0.003	0.000	0.008	0.007	0.001
Panel B: Pooled Cohorts, year t+1					
after	-0.026*	0.001	0.008	-0.011	0.004
	(0.013)	(0.011)	(0.016)	(0.015)	(0.004)
Mean of Dep. Var.	0.329	0.134	0.301	0.522	0.029
Observations	26710	26710	26710	26710	26710
R-squared	0.002	0.001	0.003	0.001	0.002
Panel C: Pooled Cohorts, year t+2					
after	-0.007	-0.004	0.022	-0.019	-0.000
	(0.014)	(0.015)	(0.019)	(0.015)	(0.007)
Mean of Dep. Var.	0.459	0.211	0.286	0.437	0.055
Observations	18,262	18,262	18,262	18,262	18,262
R-squared	0.001	0.000	0.002	0.001	0.003

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of the discontinuity at the birthday cutoffs for the Pooled Cohort. The sample consists of beneficiaries matched to the School Census and restricted to the state of Rio de Janeiro and 30 days window. Each panel represents a calendar year. The dependent variables are indicator variables for whether the recipient is enrolled in a college institution, has completed high school education, and has finished lower secondary school education in year t, t+1 and t+2 respectively. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: payroll data, and School and Higher Education Censuses.

Table 6: Effects on Educational Outcomes by Gender

	(1)	(2)	(3)	(4)	(5)
	Outcomes				
	not enrolled	high school graduate	elementary school	high school	college education
Panel A: Pooled Cohorts (Male), year t					
after	0.004	0.003	0.027	-	-0.001
	(0.012)	(0.003)	(0.019)	-	(0.002)
Observations	18,113	18,113	18,113	-	18,113
R-squared	0.000	0.000	0.001	-	0.000
Panel B: Pooled Cohorts (Female), year t					
after	0.001	-0.008**	-0.011	-	0.001
	(0.016)	(0.003)	(0.018)	-	(0.002)
Observations	17,929	17,929	17,929	-	17,929
R-squared	0.000	0.001	0.001	-	0.000
Panel C: Pooled Cohorts (Male), year t+1					
after	-0.055***	0.018	0.005	-	0.001
	(0.014)	(0.013)	(0.021)	-	(0.003)
Observations	13,404	13,404	13,404	-	13,404
R-squared	0.002	0.001	0.001	-	0.001
Panel D: Pooled Cohorts (Female), year t+1					
after	0.001	-0.014	0.004	-	0.009
	(0.019)	(0.014)	(0.020)	-	(0.007)
Observations	13,306	13,306	13,306	-	13,306
R-squared	0.000	0.000	0.000	-	0.000
Panel E: Pooled Cohorts (Male), year t+2					
after	-0.012	0.005	0.032	-	-0.003
	(0.020)	(0.019)	(0.026)	-	(0.006)
Observations	9,155	9,155	9,155	-	9,155
R-squared	0.001	0.000	0.001	-	0.000
Panel F: Pooled Cohorts (Female), year t+2					
after	-0.009	-0.009	0.001	-	0.006
	(0.024)	(0.017)	(0.021)	-	(0.014)
Observations	9,107	9,107	9,107	-	9,107
R-squared	0.000	0.000	0.000	-	0.001

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of the discontinuity at the birthday cutoffs separately by gender for the Pooled Cohort. The sample consists of beneficiaries matched to the School Census and restricted to the state of Rio de Janeiro and 30 days window. Each panel represents a calendar year and gender. The dependent variables are indicator variables for whether the recipient is enrolled in a college institution, has completed high school education, and has finished lower

Table 7: Effects on Labor Market Outcomes

	(1) year t	(2) year t+1	(3) year t+2	(4) year t+3	(5) year t+4	(6) year t+5	(7) year t+6
	Panel A: Pooled Cohorts, Employment						
extra year	-0.0046	-0.0340**	-0.0220	-0.0110	0.0287	0.0477*	0.0222
	(0.0070)	(0.0162)	(0.0160)	(0.0240)	(0.0256)	(0.0266)	(0.0289)
Mean of Dep. Var.	0.09	0.26	0.41	0.48	0.52	0.55	0.52
N	34,671	34,671	25,724	17,619	9,646	5,173	5,173
	Panel B: Pooled Cohorts, Wage						
extra year	-0.0039	-0.0405*	-0.0372	-0.0218	0.0026	0.1239*	0.0524
	-0.0102	-0.022	-0.0275	-0.0376	-0.0457	-0.067	-0.0621
Mean of Dep. Var.	0.09	0.32	0.54	0.67	0.77	0.9	0.84
N	34,671	34,671	25,724	17,619	9,646	5,173	5,173

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of the discontinuity at the birthday cutoffs for the Pooled Cohort. The sample consists of beneficiaries matched to the School Census and restricted to the state of Rio de Janeiro and 30 days window. The Columns in Panel A report the effects on the likelihood of being employed in the formal labor market and Panel B shows the impacts on annual earnings (in minimum wage) in the formal labor market in year t,...,t+6. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: Payroll data, School Census, and RAIS datasets. Robust standard errors clustered at birthday level are reported in parenthesis.

Table 8: Effects on Labor Market Outcomes by Gender

	(1) year t	(2) year t+1	(3) year t+2	(4) year t+3	(5) year t+4	(6) year t+5	(7) year t+6
	Panel A: Pooled Cohorts, Employment (Female)						
extra year	-0.0116	-0.0134	-0.0181	-0.0115	0.02	-0.0157	-0.0481
	(0.0085)	(0.0183)	(0.0191)	(0.0277)	(0.0403)	(0.0540)	(0.0515)
Mean of Dep. Var.	0.09	0.25	0.37	0.42	0.45	0.49	0.46
N	17,066	17,066	12,709	8,749	4,817	2,563	2,563
	Panel B: Pooled Cohorts, Employment (Male)						
extra year	0.0022	-0.0538**	-0.0278	-0.0169	0.0374	0.1142***	0.0959***
	(0.0100)	(0.0218)	(0.0237)	(0.0330)	(0.0291)	(0.0320)	(0.0347)
Mean of Dep. Var.	0.1	0.26	0.45	0.54	0.58	0.61	0.57
N	17,605	17,605	13,015	8,870	4,829	2,610	2,610
	Panel C: Pooled Cohorts, Wage (Female)						
extra year	-0.0145	-0.0166	-0.0098	-0.0229	0.0182	0.0052	-0.0588
	(0.0142)	(0.0254)	(0.0332)	(0.0387)	(0.0565)	(0.0915)	(0.071)
Mean of Dep. Var.	0.08	0.3	0.46	0.55	0.62	0.69	0.68
N	17,066	17,066	12,709	8,749	4,817	2,563	2,563
	Panel D: Pooled Cohorts, Wage (Male)						
extra year	0.0064	-0.0633**	-0.0679*	-0.0331	-0.0139	0.2541**	0.1745*
	(0.0148)	(0.0312)	(0.0391)	(0.0605)	(0.0668)	(0.1217)	(0.0991)
Mean of Dep. Var.	0.1	0.33	0.62	0.79	0.93	1.09	1
N	17,605	17,605	13,015	8,870	4,829	2,610	2,610

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of the discontinuity at the birthday cutoffs separately by gender for the Pooled Cohort. The sample consists of beneficiaries matched to the School Census and restricted to the state of Rio de Janeiro and 30 days window. The Columns in Panel A report the effects on the likelihood of being employed in the formal labor market and Panel B shows the impacts on annual earnings (in minimum wage) in the formal labor market in year t,...,t+6. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: Payroll data, School Census, and RAIS datasets. Robust standard errors clustered at birthday level are reported in parenthesis.

Table 9: Effects on Economic Self-Sufficiency Outcomes

	(1) year t+1	(2) year t+2	(3) year t+3	(4) year t+4	(5) year t+5	(6) year t+6
	Panel A: Pooled Cohorts, Female					
extra year	-0.0019	0.001	0.011	-0.0213	0.0171	0.0381**
	(0.004)	(0.0077)	(0.0109)	(0.0193)	(0.0236)	(0.019)
Mean of Dep. Var.	0.01	0.03	0.06	0.11	0.13	0.17
N	17,066	12,709	8,749	4,817	2,563	2,563
	Panel B: Pooled Cohorts, Male					
extra year	0.0005	0.0007	0.0004	-0.0072	-0.0125*	-0.0092
	(0.0011)	(0.0017)	(0.0037)	(0.0055)	(0.0066)	(0.0071)
Mean of Dep. Var.	0	0	0	0.01	0	0
N	17,605	13,015	8,870	4,829	2,610	2,610

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of the discontinuity at the birthday cutoffs separately by gender for the Pooled Cohort. The sample consists of beneficiaries matched to the School Census and restricted to the state of Rio de Janeiro and 30 days window. The dependent variable is an indicator of whether the individual relies on BFP support in year t,...,t+6. Robust standard errors clustered at birthday level are reported in parenthesis.

The Labor Market Effects of Maternity Leave Extension

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Abstract

This paper studies the effects of a sizeable maternity leave extension on labor market outcomes of women in Brazil, using detailed information on workers and firms in the formal labor market. Taking advantage of the exact leave taking dates and the staggered implementation of the extended leave policy across firms, our analysis compares outcomes within firms before and after the eligibility cutoff. While eligible women could have extended their leave period by 50% (from 120 to 180 days), take up only increases by 13 percentage points. Moreover, employment effects are confined to the maternity leave extension spell, with no permanent effects on employment in the long run. Taking together, our findings indicate that this policy privileges a selected group of workers, while it is not able to retain them in the workforce.

Keywords: paid maternity leave, maternal employment, gender equality.

JEL Classification: J13, J18, H42

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1 Introduction

In the last century, the participation of women in the labor market has increased substantially, which led many countries to adapt their labor legislation in order to accommodate pregnant women and mothers of young children (Rossin-Slater [2017] and Addati et al. [2014]). Women generally carry the greatest share of responsibility towards their children, and especially newborns. There is now extensive evidence on the "family gap" and researchers have emphasized the role of young children on the career interruption of women and its consequent effects on gender inequality (e.g., Waldfogel [1998]; Waldfogel [1997]; Budig and England [2001]; Bertrand et al. [2010]; Kleven et al. [2018]; Molina and Montuenga [2009]).

Maternity leave policies, in turn, could help undo some of these adverse effects associated with motherhood. Advocates of such policies argue that time spent at home with newborns, besides improving child development[1], can help mothers cope with their work and family responsibilities. On the other hand, opponents argue that long periods outside the labor market can reduce skills as well as employment and earnings, especially if employers who find leave-taking costly discriminate against female employees.

While there exists extensive evidence on the effects of maternity leave policies on employment using credible research designs, those mostly come from developed countries (Olivetti and Petrongolo [2017]; Rossin-Slater [2017]), notably in North America (Baum and Ruhm [2016]; Han et al. [2009]; Blau and Kahn [2013]; Baker and Milligan [2008a]) and Europe (Gregg et al. [2007]; Lalive and Zweimüller [2009]; Kluve and Tamm [2013]; Bergemann and Riphahn [2015] Lequien [2012]). However, maternity leave policies vary widely across countries and its efficacy may depend on specific rules (paid vs. unpaid leave, length of leave, eligibility requirements, job protection, etc.), cultural norms on gender roles, level of development of local labor markets and the child care arrangements available in each country.

This paper investigates the effects of maternity leave extensions in Brazil, a developing country characterized by sizeable gender inequality (Agénor and Canuto [2015], Van Klav-

[1] While an extensive literature has shown evidence of little impact of leave extensions on children's well-being in developed countries, such as, Canada, Denmark, Austria, Germany and Norway (see Baker and Milligan [2008b]; Baker and Milligan [2010]; Baker and Milligan [2015]; Rasmussen [2010]; Danzer and Lavy [2017]; Dustmann and Schönberg [2012]; Dahl et al. [2016]), the evidence suggests positive effects of leave introduction (rather than extensions) in Norway and US (Carneiro et al. [2015]; Rossin [2011].

eren et al. [2009], Pinheiro and Medeiros [2016], Fernandes and Menezes-Filho [2016]) and informality (Ulyssea et al. [2014], Meghir et al. [2015]), as well as limited universal health care and childcare/education provision (Bhalotra et al. [2016], Victora et al. [2011], Attanasio et al. [2014]).

In Brazil, the default maternity leave policy entitles eligible workers 100 percent income replacement for a mandatory 120 days of leave, which is paid by the employer and reimbursed by the social system. Furthermore, there is no eligibility requirement for this mandatory leave (e.g., no minimum period of work or minimum contribution), except that the women must be (formally) employed at the time of childbirth. Job protection is guaranteed throughout pregnancy and up to the fifth month after the leave-taking date. Leave extension from 120 to 180 days was made possible through a program that gave tax compensation to firms offering additional 60 days of paid leave[2]. Created in 2008, the *Empresa Cidadã* Program (hereafter referred as EC Program) only came into effect after its regulation in 2010.

While EC adoption was not mandatory, our empirical strategy compares women who took maternity leave within firms before and after adoption of EC Program using precise event dates that are available in our dataset. Specifically, we explore the time of leave-taking relative to EC adoption in a regression discontinuity design. We select women around the threshold and investigate leave extension and monthly employment status up to three years before and five years after maternity leave. We use administrative data on all Brazilian formal workers reported by RAIS (*Relação Anual de Informações Sociais*), which is the Brazilian matched employer-employee dataset provided by the Ministry of Labor, with detailed information about firms and workers[3]. Even though RAIS is an annual stock of workers and firms, we can recover monthly employment information of workers taking leave using start and end dates of each employment spell. Since 120 days of leave is mandatory, this dataset contains information on all women that give birth while in the formal labor market.

We find that extensions in the paid maternity leave period had a limited impact on labor force participation of women. First of all, the take up by mother was very limited.

[2]While companies decide whether or not to participate in the EC Program, the decision to accept or not accept an extended leave depends solely on the employee's choice.

[3]RAIS informs labor force movement (hiring and firing) across the year as well as basic demographic, occupational and income characteristics of the employees.

The likelihood of a leave of 180 days increases by 13 percentage points, which amount to an average increase of around eight days. Further, the expansion of the maternity leave only affects employment during the extended leave period, and the impact becomes null after that.

These results do not agree with what Rossin-Slater [2017] concludes after surveying recent papers that found causal effects of maternity leave policies for developed countries. Her evidence suggests that, first, extending the period of maternity leave increases the leave-taking rates and, second, for maternity leaves lasting less than one year, it tends to improve women's employment several years later (Baker and Milligan [2008a] and Kluve and Tamm [2013]). Our findings on employment are similar to those encountered in Dahl et al. [2016], which find no significant effects of extensions on paid maternity leave (from four to eight months) on labor force participation among mothers in Norway. On the other hand, contrary to our finding, they observe a take-up rate of 100%.

We then turn to examine to what extent the findings can be attributed to characteristics of firms joining the EC program. Indeed, the number of eligible companies[4] for the program represents only a small share of the formal labor market and, from those eligible, less than 10 percent, in fact, joins the program. Furthermore, companies that joined the program are much bigger in comparison to the rest of the market and women working for *Empresa Cidadãs* are more educated and earn higher payments.

We complement the analysis by applying an event study strategy (see Appendix for details) to investigate the labor market consequences of taking maternity leave on women's employment throughout time. We examine heterogeneous effects of maternity leave based on education, wage, job position, firm size and gender composition of the workforce in the firms. Further, we compare employment trajectories, passing through the leave-taking period, of women working for typical firms vis-à-vis those women who work in *Empresas Cidadãs*, but before they have joined the program. The event study analysis shows an inverted U-shaped employment pattern which peaks at the time of leave-taking[5]. Employment is stable until the fourth month after the beginning of the leave period, but falls monotonically after that and stabilizes again after around 12 months. We also find that

[4]EC program is addressed to those companies that are taxed based on actual profit, which excludes, for example, firms that declare presumed profit or that participate in the *Simples Nacional* Program.

[5]Since the analysis conditions on leave-taking, and leave-taking eligibility depends on the formal job, the employment peak at the first month of leave-taking is expected.

the higher educated women as well as those women who earn higher wages, at the time of maternity leave, are more secure against dismissal. Since we find that women working in *Empresa Cidadãs* are, on average, more educated and wealthy, our findings imply that women employed in these companies are benefiting relatively more from the Brazilian maternity leave policy. In other words, the EC Program privileges proportionally more those women who are already more protected.

In sum, taking all our findings together, we notice that the maternity leave policy in Brazil is not sufficient to retain women in the workforce, even for those women who had the opportunity to extend the maternity leave period in 50% (from 120 to 180 days). Furthermore, the expanded maternity leave arrangement adopted in Brazil had adverse redistributive effects, privileging a selected (in terms of wage and education) group of workers.

Previous research in Brazil has found mixed evidence on the effects of maternity leave extensions. Carvalho et al. [2006] estimate the effects of a mandatory expansion in the maternity leave period occurred in 1988, which changed the period of leave from 12 weeks to 120 days. By using a differences-in-differences strategy for the period 1986-1991, they find that the extension in the leave-taking period did not significantly affect neither women's employment nor wages. Meireles et al. [2017] study the effects of the adoption of the EC Program, by using RAIS combined with a difference-in-difference approach. Unlike this paper, the authors use aggregate data at the firm level with annual information and find a positive impact on hirings and a reduction (increase) in women's (man's) wages.

We note our findings are limited to employment trajectories of women in the formal labor market. For instance, our data do not allow us to say anything about women who took maternity leave while were employed in a formal job but went out of the formal labor market permanently. Also, we can not identify whether higher participation in the informal sector counterbalances lower participation in the formal labor market.

The remainder of the paper is divided as follows. The next section provides a background on maternity leave policies in Brazil as well as the women participation in the Brazilian labor market. In section three and four, we present our data and the empirical strategy, respectively. Section five shows the results and the section six concludes.

2 Institutional Context

In this section, we provide the background on maternity leave policies in Brazil and the necessary background on the female participation in the Brazilian labor market, which can be skipped by those familiar with these statistics.

2.1 Maternity Leave Policies in Brazil

2.1.1 The Constitutional Right

Maternity leave in Brazil is a constitutional right established in 1988. It guarantees paid maternity leave for a mandatory 120 days, without any prejudice in employment, job position or salary. Legally, every woman who is formally employed is entitled to receive this benefit.[6] The Federal Constitution also guarantees that from the moment in which the pregnancy is confirmed up to five months after giving birth, women cannot be fired, protecting them from arbitrary dismissals. Terminations that occur during this job-protected period are subject to full indemnity by firms to workers[7].

Workers receive their full salaries regularly during the leave, which are paid by firms but fully reimbursed by the Brazilian Social Security Administration (INSS). Leave-taking generally starts at some point between the last month of gestation (28 days before the birth) and the birth of the child.[8] The rest periods can be increased by 2 (two) weeks, if necessary and through medical recommendation. Moreover, pregnant women can take time off for six medical consultations and should be transferred to an alternative job position if their job poses any risk to her health, being reinstated in her regular position when possible.

[6] In general, not only pregnant women have this right, but also those who adopt or obtain judicial custody for adoption purposes, regardless of the child's age. Women who have been spontaneous abortions or those provided by law (rape, anencephalic fetus, and risk to the pregnant woman's life) are also entitled to benefit. In these cases, the period of maternity leave is only 14 days. In case of a stillborn baby, after the 23rd week of gestation (before that it is considered an abortion), maternity leave period, as well as the benefit amount, are entirely guaranteed.

[7] When the woman realizes a pregnancy, she immediately acquires job stability, even if she has not yet communicated it to the employer. Furthermore, the period of stability is a right even in a temporary employment contract or if a woman becomes pregnant during the period of work experience. The woman can break the commitment resulting from an employment contract, whether she considers that it is harmful to the gestation, testified by medical recommendations.

[8] If there is a medical recommendation for a woman to be absent for more than 28 days before the expected delivery, she must present a medical letter proving this need. However, as a rule, the total leave period must be mandatory 120 days, regardless of when it starts.

2.1.2 The *Empresa Cidadã* Program

Enacted on September 9th of 2008, the *Empresa Cidadã* Program was designed to extend the maternity leave period through a tax compensation. However, the program only became effective for the private sector after 2010.[9] The program intended to expand the maternity leave by 60 (sixty) days (becoming 180 days' leave in total) for those companies that are taxed based on actual profit.[10]

The participation in the program is voluntary, and the company can deduct, as an operating expense, the full additional wage bill (in general, two extra salaries for two months) in the income tax of the preceding tax year. Therefore, the firms do not face any additional costs from the wage bill, except those that are offered to employees in their benefits package (e.g., health insurance, childcare assistance, etc.) and possible operational costs of having an employee out of the position during the period of the extension.

While companies decide whether or not to participate in the EC program, the right to extend the maternity leave period has to be guaranteed to every employee who works for an *Empresa Cidadã*. On the other hand, the decision to accept or not accept the extension depends solely on the employee's choice. When the employee, working for the EC firm, wants to take the extension, she must apply up to 30 days after giving birth, and the extra 60 days of leave start immediately after the regular maternity leave of 120 days ends.[11]

Although only after 2010 the Brazilian federal government established its official incentive policy aiming to extend the maternity leave period, some companies already used to offer a 60-days extension of the maternity leave period. Usually, that was motivated by companies that wanted to send a positive signal to the market, regarding the benefits of working in that group and its sense of social responsibility. Moreover, in some situations, unions also negotiate with the companies about the extension of the period of maternity leave for the sector they represent.

[9] This happened because the Brazilian Fiscal Responsibility Law requires that all impacts from any tax exemption must be included in the Budget Law one year before. However, there was not enough time to include the *Empresa Cidadã* impacts in 2008. The program came into force after January 1st, 2010, with its regulation by Decree No. 7,052 of December 23, 2009.

[10] Despite the rule only applying for companies that are taxed on actual profits, firms that declare presumed profit or that participate in the Simples Nacional Program can also join the program, but they will not be entitled to any deduction.

[11] During the extension time, employees must not participate in any paid activity, and the child cannot be kept in daycare or similar organization, otherwise, the extension will be forfeited.

2.2 Women in the Brazilian Labor Market

Over the past two decades, Brazil has made significant progress in reducing gender inequality, especially in the labor market (Elborgh-Woytek et al. [2013]). According to Agénor and Canuto [2015], the female to male labor force participation rate increased from 52.2 in 1990 to 66.7 in 2000, and 73.3 in 2010. However, gender gaps in access to formal employment still persist. According to data from the National Household Sample Survey (PNAD), in 2015 Brazil had almost 32 million of women aged between 25-44 years old, of which approximately 62 percent had worked during the reference week of the survey and 38.4 percent of these women were formally/legally employed in the private sector.[12] The fraction of men in the same age group employed in 2015 was over 85 percent (see Table A.1).

When looking at domestic work, almost 92% of women reported having worked in household services, and this fraction for men was just around 54%. Not surprisingly, women also earn on average lower wages in comparison to men and, considering income from all sources, men earn approximately 60% more than women (PNAD-2015). Furthermore, these gender inequalities are not explained by differences in human capital, since women and men in the sample are quite similar, in fact, women are even marginally more educated.

In Brazil, women carry much more responsibility for their children relative to men. The employment gap between married men and women increases considerably after the birth of a child (see Table A.2). In 2015, the share of women who were working was 64.5 percent, while for men that percentual was 88 percent. Moreover, conditioning on parents with a 6-month-old child, the share of women working decreases to 35.1 percent, while for men it increases to 92.7 percent. The employment gap remains for parents of 6-year-old children, and it is even higher if we consider only full-time jobs. While this correlation is far from being causal, there is research suggesting the causal relationship.

[12] According to the Brazilian constitution all these women (but not only them) are eligible to receive paid maternity leave for 120 days.

3 Data and Background

3.1 Data Description

In this paper, we use RAIS (*Relação Anual de Informações Sociais*), which is the Brazilian matched employer-employee dataset provided by the Ministry of Labor. Aside from firm and worker identifiers[13], RAIS also provides important demographic characteristics of workers (such as gender, age, race, and education), firms sectors (public/private) and characteristics of the employment contract (such as date of dismissal/admission, wages and hours worked per week). Important to our analysis, maternity leave start and end dates became available from 2007 onward.[14]

Although RAIS is an annual stock data, we can generate precise monthly information on maternity leave and employment status using start/ends dates of the maternity leave, admission, and separations for each employer-employee pair. Individual identifiers (Social Identification Number-NIS) allow us to track individuals over time to build a rich monthly panel dataset, containing basic demographic, occupational and income characteristics of the employees.

Finally, we generate information on all companies that joined the *Empresa Cidadã* program and the corresponding date of adoption, which was provided by the Brazilian Treasury Department thorough *Receita Federal*. From the beginning of the program until 2016, 19,640 companies have joined the EC Program, and more than half of them (10,946) participated immediately after its beginning. In the second year, 2011, the program covered another 4,723 firms. Therefore, only the first two years of operation concentrate nearly 80 percent of the total number of companies joining the program.

3.2 Descriptives

We start by showing descriptive statistics of leave-taking in Brazil. Using data from RAIS, we focus on women aged 25-44 years old taking maternity leave in 2009[15]. It is worth

[13]RAIS provides identifiers for both firm (National Registry of Legal Entities-CNPJ) and the employee (Social Identification Number-PIS).

[14]In recent years, control mechanisms were instituted to enforce the firms to comply with the legislation, which makes RAIS mandatory. Moreover, declaration through the internet facilitate compliance and improve data quality. According to the Ministry of Labor, RAIS is annually declared by 98% to 99% of officially existing firms.

[15]We focus on 2009 for tractability, but the results for different years are virtually the same. Results for other years are available upon request.

noting that the informal sector in Brazil is large and, by definition, informal companies are not included in RAIS, that covers only legal and registered firms.[16]

Applying an event study approach, we track female employment over a long period, before and after leave-taking (the event), controlling for month-year fixed effects, demographic characteristics, and firm fixed effects. Precisely, we investigate the work trajectories of women three years before and five years after leave-taking. More details on variables and data construction are in the Appendix A.

In the Brazilian formal labor market, the likelihood of employment increases monotonically since three years before the maternity leave and, as expected, reaches its maximum at the moment of the leave-taking.[17] During the mandatory leave of 120 days, employment is stable, but it falls sharply mainly after job protection is no longer guaranteed (five months after the birth). Employment falls monotonically reaching 38 percent one year after the event and it remains stable at 41 percent five years later (see Table A.3). While these rates may seem high, turnover is also substantial in the formal labor market. The exit rate of women in the same age group is 21 percent in one year and 35 percent in five years. The numbers for men are 32 and 35 percent, respectively (see Table A.4).

We find a similar pattern when looking at separation, which means being employed at 't' but unemployed at '$t+1$'. They are lower during the pregnancy, consistent with the job protection guarantee, and reaches zero at the time of the event, staying at that range during the first three months after the leave-taking.[18]. Although statistically significant, the effects on separation for two and three months after the event are very close to zero. At the fourth month, the probability of separation (not employed next month) increases approximately 3.58 percentage points, doubling to 7.7 percentage points in the fifth month, and it reaches its maximum (9.6 p.p) in the sixth month after leave-taking. After three years, the probability of separation is higher than its previous level of three years before the leave-taking[19]. On the other hand, the probability of hiring increases subtly until one year

[16] According to IBGE, non-registered and own account workers represent 50% of total employment in Brazil.

[17] The relevant event in the analysis is leave-taking and all women are employed at the moment of the leave.

[18] Notice that this result means that the employment was on average guaranteed for four months, given that separation here means being employed at 't' but unemployed at '$t+1$'. Moreover, it is worth remembering that, depending on mother's choice, the maternity leave can start one month before the birth date.

[19] Dividing the separation variable according to its causes (Fair or No Fair) and based on who takes action (Employer's or Employee's Initiative)[20]. This division considers the effects of maternity leave, taking into

before the leave-taking, when it begins to drop to zero in the month of maternity leave. During the first five months after the leave-taking, the likelihood of hiring is statistically around zero, meaning that during this period hiring does not occur very frequently. After one year from the leave-taking, the likelihood of hiring seems to return to its previous levels (see Table A.3).

We find that these patterns are very similar among women of different education levels, wage and job position and also among firms of different characteristics (such as size and gender workforce composition). However, the probability of being employed is systematically higher for the more educated group, indicating their higher attachment to the workforce (see Table A.6). We also observe that well-paid women are more likely to be employed throughout three years before the leave until five years after it (see Table A.7). Moreover, women taking maternity leave in firms with more workers have a higher probability of being employed during all the years before and after the maternity leave (see Table A.8). On the other hand, the higher the proportion of women in the company, the riskier for women to be out of the formal labor market after they take maternity leave.

3.3 *Empresa Cidadã* Characteristics

Since our analysis is restricted to firms joining the EC Program, we now show the characteristics of adopters. Using data from RAIS, also in 2009 (one year before the EC program), we compare firms that ever adopted EC with non-adopters. We notice that they are special for several reasons. First, they are on average much larger than the rest of the firms in the country. They also pay higher wages and have a more educated workforce (see Tables 1 and 2). Finally, employment rates are also higher for these firms and women taking leave in EC adopters already have their jobs more protected (see Table 3). Our empirical strategy will perform within firm analysis, but the external validity of the results is confined to firms with these characteristics.

account the employees and employers choices and it works as a decomposition of the pattern observed on separation. The results on Table A.5 show that the effect on separation in the fourth month is similarly explained by "No Fair and Employer's Initiative" and "No Fair and Employee's Initiative". However, after the fifth month of the maternity leave beginning, we observe that more than half of the total effect on separation comes from "No Fair and Employee's Initiative".

3.4 Sample

We focus on firms that ever adopted EC Program at some point from 2010 to 2014 and women who took maternity leave, while working in those firms, only once during the period 2009-2015. We consider women aged 25-44 and who were working for private companies at the time of their leaves[21]. Furthermore, we restrict our datasets to maternity leave lasting a total of 120 days (standard maternity leave period), 135 days (standard period plus two weeks) or 180 days (extended maternity leave). In fact, we allow a 2-days margin of error to each one of them, i.e., we consider maternity leave periods lasting 118-122, 133-137 or 178-182 days. These are the regular leave taking spells, and these restrictions eliminate reporting errors as well as shorter leaves due to adoption or abortion.[22]

Our sample consists of 4,118 firms and 46,063 female workers, followed for seven years (from 2009 to 2015), yielding 3,8 million person-month observations. Table 4 brings the descriptive statistics of the overall sample (from column 1 to 6) as well as the sample that we use for the estimation (from column 7 to 10).

4 Empirical Strategy

4.1 Research Design

Let t_i^{ml} be the calendar time (in month-year) in which worker i takes maternity leave and t_i^e the calendar time (in month-year) when firm e (where i works at the time of maternity leave) joined EC Program. We define the running variable as $R_i \equiv t_i^{ml} - t_i^e - 1$. We subtract one month because, according to the legislation, a woman who works for a company that joined the EC program can request an extension of maternity leave anytime during the first month of pregnancy. Therefore, we consider the cases in which the accession to the program has happened during the first month of the leave.

Moreover, it is convenient to normalize the calendar time to represent the elapsed time since maternity leave. Precisely, for each worker i we consider the number of months between calendar time, t, and the moment of leave-taking t_i^{ml} (i.e., $r = t - t_i^{ml}$). Thus, we set the relative time r as the number of months since the month in which the maternity

[21] Public sector employment, in contrast, is very stable.

[22] Appendix B shows the filters that we applied to the datasets.

leave started and evaluate its effects on the employment over r.[23]

We estimate the following equations:

$$D_i = g_r(R_i) + \pi I(R_i \geq 0) + \nu_{ir} \tag{1}$$

$$y_{ir} = f_r(R_i) + \delta_r I(R_i \geq 0) + \eta_{ir} \tag{2}$$

where D_i indicates if the worker took extended maternity leave of 180 days; y_{ir} is the employment indicator (equal 1 if employed and zero otherwise) for women i at r months after the leave event (i.e, when $r = 0$); $I(R_i \geq 0)$ is a dummy variable that takes value one if the woman took the maternity leave after the company, where she was working, joined the EC Program and zero for women who took maternity leave at the same firms but before that time; functions $f_r()$ and $g_r()$ are polynomial distances from the cutoff; and η_{ir} and ν_{ir} are the errors.

We are primarily interested in the patterns of δ_r across different horizons in r. These coefficients measure employment differences between women taking maternity leave after their companies joined EC (and hence are treated) and those taking the leave before that. Therefore, plotting δ_r versus r in a graph we can investigate the impacts of extended maternity leave for each month before and after the leave-taking. Equation 1 estimates the extended leave-taking take up, whereas equation 2 captures the effects of maternity leave extension from 120 to 180 days on employment.

We evaluate the impact of the program three years before and five years after the maternity leave by estimating the model over $r \in [-36, 60]$. We further restrict observations around the threshold, by limiting the running variable to 12 months before and after the cutoff (i.e., $R_i \in [-12, 12]$). Regressions use triangular weights and add individual controls (such as age, race, education, and occupation) as well as firm fixed effects for precision.

4.2 Validity of the Research Design

Manipulation of the running variable could occur both by the timing of EC adoption and the time of birth. While it is unlikely that mothers can delay births in order to meet

[23] Note that each worker i may have a different relative time, r, but we omit the subscript i in r for the ease of notation.

the EC entitlement, firms could plausibly anticipate the adoption of EC depending on the number of pregnant women in their workforce. We empirically investigate this possibility by plotting the histogram of the running variable in Figure 1. We do not find evidence of heaping in the distribution around the threshold.

A second concern would be if there were a jump in predetermined characteristics of workers around the cutoff qualifying for the extra 60 days in the standard maternity leave of 120 days. If this occurs, it would evidence that the assignment to the treatment does not give us a treatment variation as good as random around the threshold. We do not find any evidence of discontinuity of baseline variables. There is no jump in educational outcomes, measured as the probability of having less than elementary school degree, completed elementary school, completed high school and college degree or more. Also, there is no discontinuity in the probability of belonging to the following age groups [25,30], (30,35], (35,40] and (40,45] (see Figure 2). Regarding the employment situation at the baseline, we also do not see any jump in earning and weekly hours worked at the time of the maternity leave (see Figure 3). Table 5 provides a test of difference of means using the RDD approach, where it is verified that the control and treatment groups are significantly similar.

5 Results

5.1 Leave-Taking Take Up

We start by showing visual evidence of leave extension take-up in Figure 4. Considering 24 months around the cutoff, we estimate a 5th order polynomial on both sides of the discontinuity. We observe that the proportion of mothers taking the extended maternity leave jumps at the cutoff. Consistent with this figure, regression results of equation 1 indicate that take up increase by 13 percentage points at the cutoff (column 6 of Table 6)

This level of take-up is considered low if compared with policies that introduced paid maternity leave in substitution to unpaid leave in Norway (Carneiro et al. [2015]; Dahl et al. [2016]). However, this level of take-up is consistent with what the literature has found on the effects of extensions in the period of paid maternity leave in Canada (Baker and Milligan [2010]) and Austria (Lalive and Zweimüller [2009]).

It is important to note that take up remains low several months after EC adoption, indicating that the lack of awareness (by the workers) about the policy is unlikely. Alternatively, the level of take-up can vary with rigorous requirements, low pay or the absence of job protection. So far we do not examine each of the concurrent explanations would play a major role in attenuating the take-up, but we plan to do this by analyzing compliers' characteristics (such as position, tenure, wage, etc.).

5.2 Effects on Employment

Although it is more obvious that maternity leave policies can increase leave-taking period, the impacts on workers' labor market outcomes, especially in the long run, are apparently more ambiguous (Klerman and Leibowitz [1994]; Olivetti and Petrongolo [2017]). For instance, the expected impact of maternity leave policies on women's employment depends on whether these policies encourage women who would have quit working to stay or if it increases women's time away from work precisely for those women who would have stayed anyway.

To ensure the comparability of the control and treatment groups, we restrict the analysis to a window of women taking maternity leave 12 months before and 12 months after the time that the company became *Empresa Cidadã*. In our preferred specification, we include controls for education level, race, age, occupation (one-digit classification) and firm fixed effects to control for the firm characteristics at the moment of the maternity leave. It is important to note that women's employment outcomes consider all firms in RAIS, not only in the firms to which they were matched at the time of their leaves.

Figure 5 summarizes the effects of the extension of the maternity leave period on employment over time. Each point in the graph comes from the estimation of the RDD model in equation 1, where the dependent variable is an indicator of employment status (equal one if employed and zero otherwise) over $r \in [-36, 60]$ months after the maternity leave starts. Therefore, Figure 5 shows the effects triggered by a potential extension of 50% in the period of maternity leave (from 120 to 180 days) over several months after leave-taking.

Job protection guarantees employment stability for both the treatment and control groups up until five months of leave starting dates. Indeed we find no differences in the likelihood of employment between them until month five. This finding indicates the compliance with the legal regulation, prohibiting discrimination in employment before, during and immediately after the paid maternity leave period. By the sixth month, women who took maternity leave after the company joined EC Program (hence are treated) are slightly more likely to be employed. While statistically significant, the magnitude of the effect is quite small. There are no significant effects of the program after the sixth month and we do not reject equal employment trajectories after that. Table 7 expands the same analysis to other employment outcomes, such as separation, hiring, and cumulative employment. We do not find relevant impacts of maternity leave extension on women's employment.

Given the low take-up and the insignificant employment effects, our findings suggest that maternity leave extension from 120 to 180 days in Brazil was not enough to improve women's attachment to the formal workforce. These results are consistent with the existing evidence on leave extension (Dahl et al. [2016]), which found null effects in Norway using a similar RDD design. In Brazil, de Carvalho et al. [2006], examining the maternity leave extension from 12 weeks to 120 days granted after a change in the Brazilian legislation, also did not find significant effects on employment. On the other hand, Meireles et al. [2017] estimate the effects of the same policy evaluated in our paper by using the same database (RAIS) as we do, but a different econometric approach. The authors use aggregate data at the firms (rather than individual) level with annual (instead of monthly) information and a difference-in-difference design. The paper shows a positive impact on hirings for both male and female workers, but a reduction (increase) in women's (man's) wages.

Rossin-Slater [2017], based on a review of the previous literature for developed countries, suggests that perhaps the maternity leave effects on employment are much more a matter of duration of the leave. In this regard, the leave should not be too long nor too short in order to avoid side effects of this type of policy.

5.3 Robustness Check

To investigate the robustness of our findings, we examine how they respond to plausible changes in the model as well as in the estimation window. First, we remove the firm fixed effects to deal with potential overparameterization of the econometric model. Secondly, we modify the estimation window and, instead of comparing women taking maternity leave within twelve months surrounding the company's accession to the EC Program, we consider alternative views, such as $R_i \in [-6, 6]$, $R_i \in [-18, 18]$ and $R_i \in [-24, 24]$. Although the results also do not vary considerably as the interval changes, it is important to point out that a shorter view, say of six months, can be too short so we lose too many observations. On the other hand, 24 months may be long enough to make the groups of control and treatment less comparable. We consider a 12-months interval reasonable since it is parsimonious enough and meets, as we showed, all the assumptions underlying the RDD approach. Finally, we also estimate the treatment effect by using cubic functions instead of linear functions on both sides of the cutoff. As can be seen in Table 8, the primary results that we have found before almost do not change as we modify the model according to the previous description.

We also estimate an alternative specification, by combining a difference-in-difference model with an event study analysis, through the following model:

$$y_{itlrj} = \gamma_t + \lambda_j + X'_{ij}\beta + \alpha T_i + \sum_{r\neq 0}\mu_r + \sum_{r\neq 0}\delta_r \times T_i + \epsilon_{itj} \tag{3}$$

Where y_{itlrj} is the employment status (1 if employed and zero otherwise) for woman i in month-year time t and who took the leave at month-year l, while working for firm j, and is observed r months apart for the leave starting month (i.e., we define $r = t - l$). The terms λ_j and γ_t are firm and year-month fixed effects and X_{ij} are individual characteristics (ocupation, race, education and age) at the time of the leave. The treatment variable, T_i, is equal 1 if $t_i^{ml} \geq t_{e(i)}^{ec} + 1$ and 0 otherwise, where t_i^{ml} and $t_{e(i)}^{ec}$ are already defined previously in the Research Design subsection. ϵ_{itj} is component error.

The terms μ_r and δ_r are coefficients to be estimated via dummy variables indicating the relative time, r, which is the number of months since the maternity leave started. We are particularly interested in the pattern of δ_r throughout the relative time r, which gives us the differential evolution of employment rates between women who took maternity leave after the firm's accession to the EC Program and those women taking leave in the same firms but right before.

Note that, in comparison to our preferred approach (i.e., RDD analysis), here we control for a fewer number of cofounders, given that the firm fixed effects and the effects of control variables are not allowed to change in time. An advantage of this approach would be the considerable increase in the number of observations because we pool the monthly data to explore a panel dataset structure. In spite of the significant change in the model, as well as in the dataset structure, the pattern of the effects over time behaves similarly, comparing to the previous RDD results. As we can see in Figure 6, compared with the Figure 5, the only difference now is that the coefficient is slightly higher around the sixth month, but the null effect in the medium run persists.

6 Conclusion

The past few decades have experienced a notable increase in the number of countries offering maternity leave policies, which is mainly a result of the global trend in which women's participation in the labor market has also increased remarkably. Maternity leave policies can address critical social goals by helping mothers to balance job and family duties. Besides the potential beneficial effects of maternity leave on child development, proponents of such policy argue that they can mitigate gender inequalities in the family and labor market by keeping women working out of the home.

The policy design details (i.e., duration of maternity leave, payments, job protection, and funding) differ considerably across nations. But even in case of policies with similar characteristics, its impacts may vary depending on the economic environment and local factors. Given the diversity of circumstances in which maternity leave arrangements have been implemented, the number of researchers interested in such topic has gained salience. However, this is much more evident for developed countries, while in developing countries the literature is still incipient.

Exploring data from Brazil, a vast developing country, we notice that the maternity policy is not enough to retain women in the formal labor force. Using a credible empirical approach, we find no causal effects of generous extensions in the maternity leave period (from four to six months) on employment rates in the medium run. Furthermore, exploring the characteristics of the firms offering the opportunity to extend the period of rest, we conclude that the EC Program had regressive redistribution properties.

Our findings suggest that further policies are needed to promote higher attachment of women to the labor market. In this regard, some alternatives (which we did not study in this paper) include parental leave policies with quotas by gender, work flexibility based on family-friendly occupations and availability of affordable (public) child care facilities. For future research, we plan to investigate a group of interrelated topics of our interest, for instance: heterogeneities by age, education, tenure, birth order, etc.; leave-taking by a manager in the firm and its spillover effects; signaling on the employment and wage effect of women who do not take extended leaves; how the leave-taking and child care policies interact and so on.

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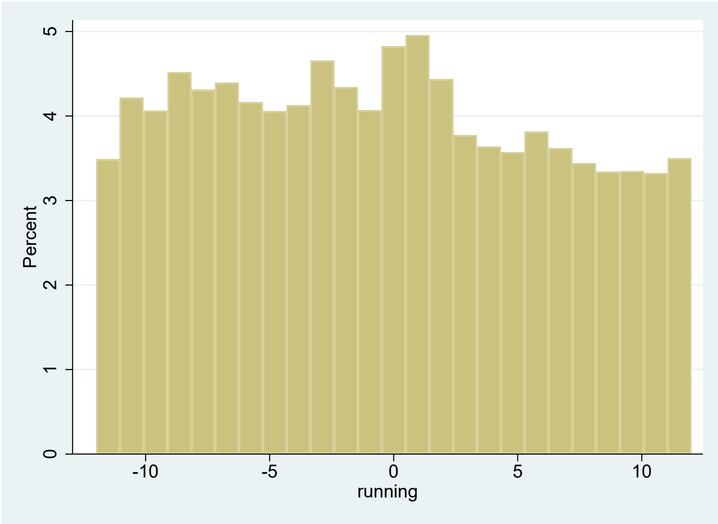

Figure 1: Density of Running Variable

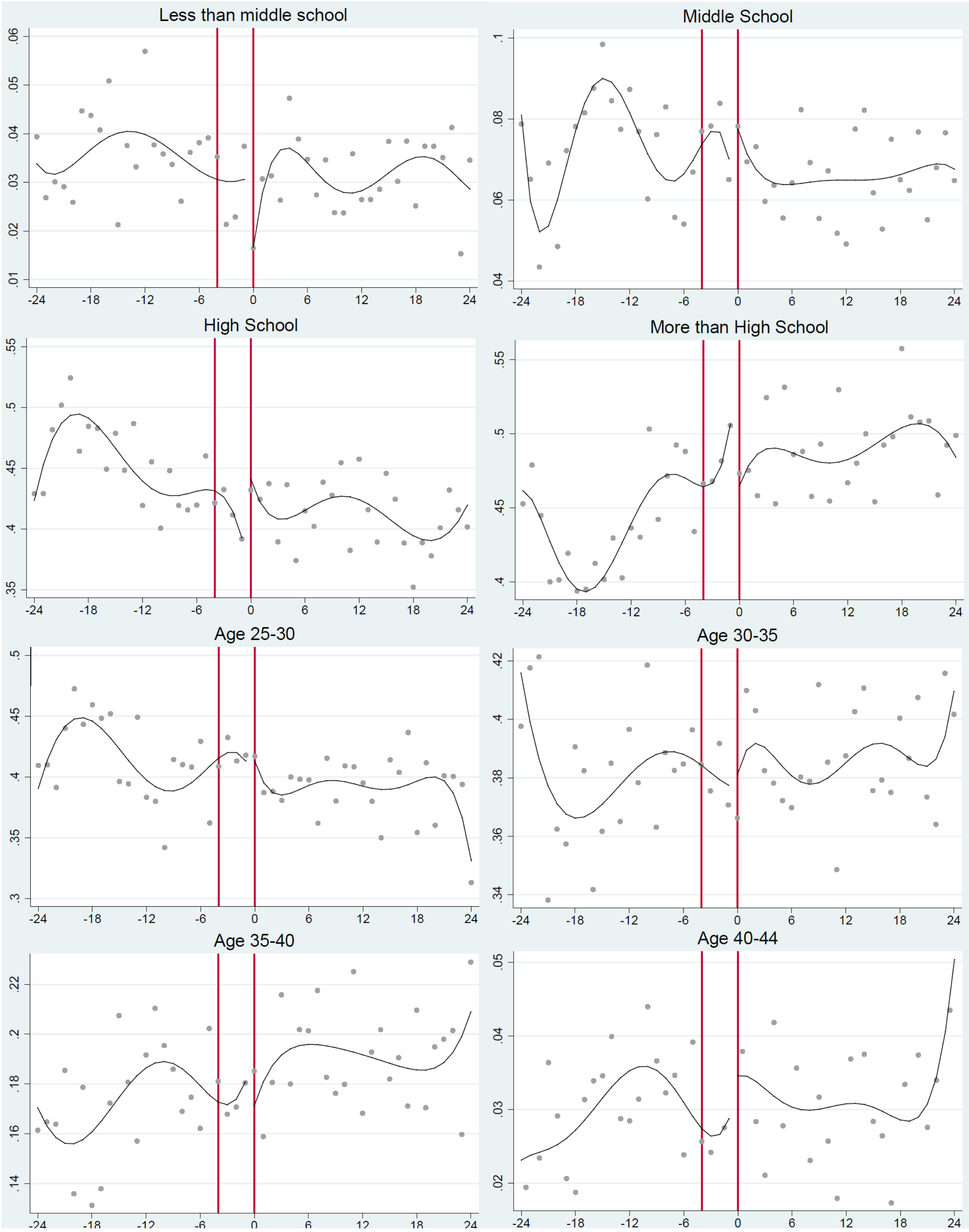

Figure 2: Analysis of discontinuity on education and age

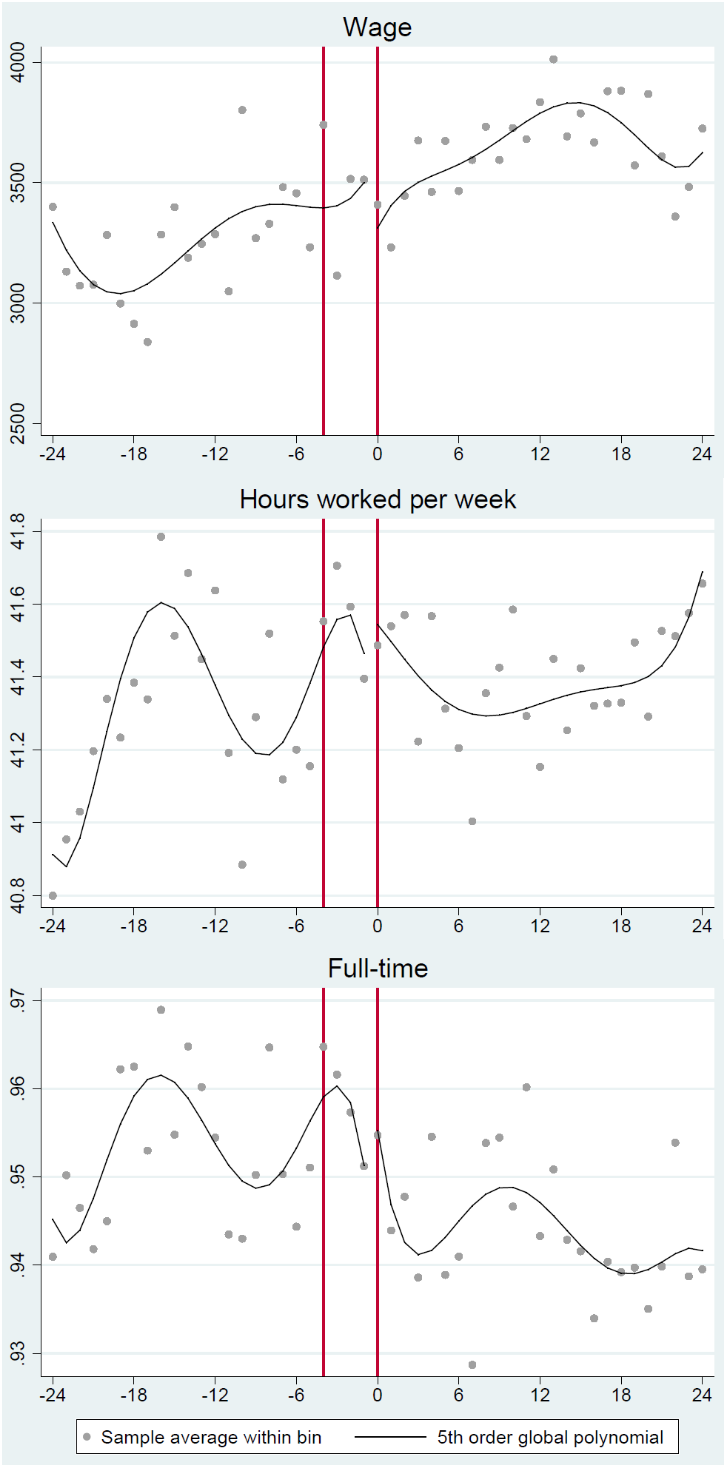

Figure 3: Analysis of discontinuity in monthly wage and hours worked

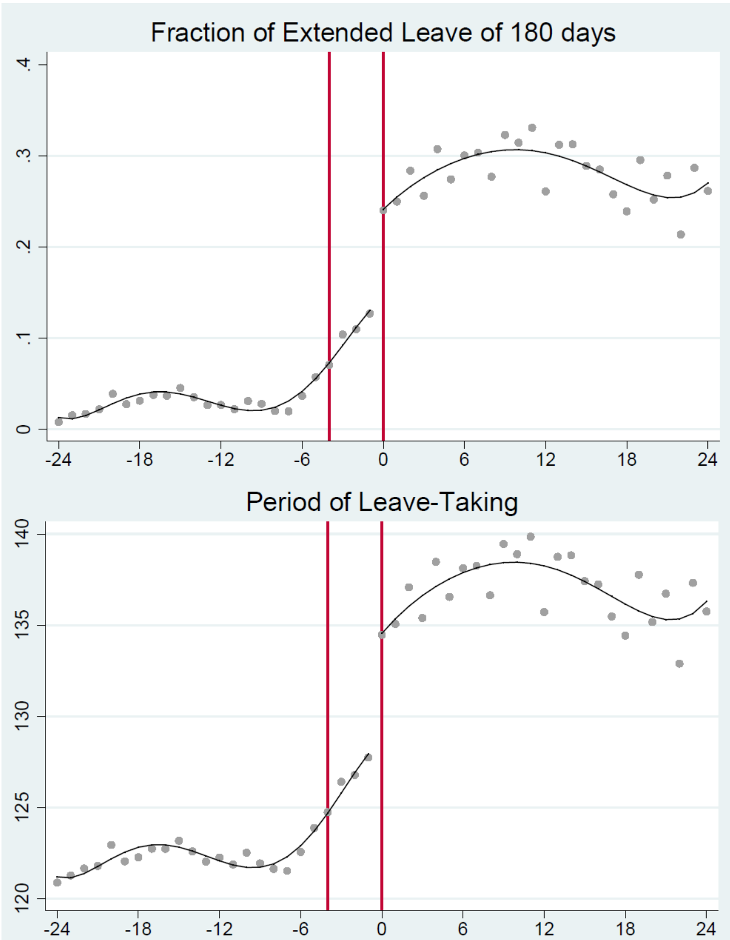

Figure 4: Number of days of maternity leave and probabilty of taking extended ML

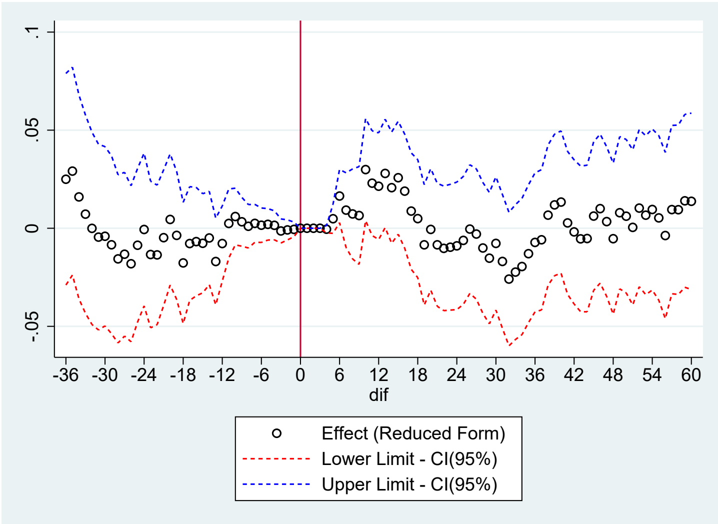

Figure 5: Effects of the expanded maternity leave on employment

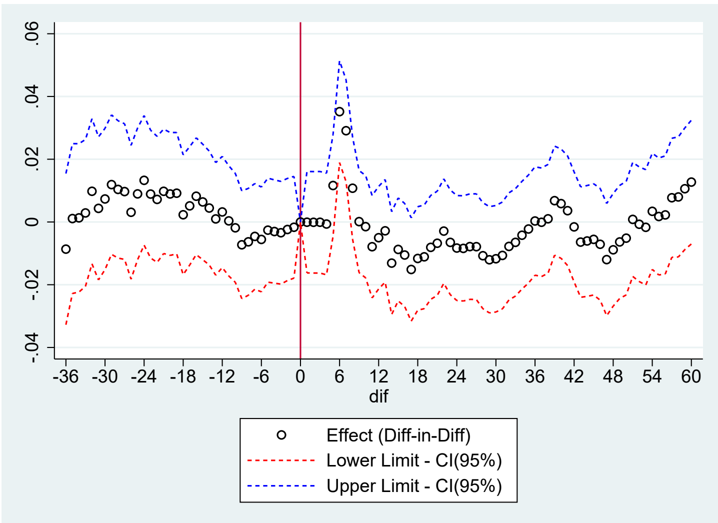

Figure 6: Effects of the expanded maternity leave on employment: using difference-in-difference event study

Table 1: Descriptive statistics of the workforce

Variables	All Firms (1) mean	(2) sd	*Empresa Cidadã* firms (3) mean	(4) sd	*Empresa Cidadã* firms and our analysis sample (5) mean	(6) sd
Wage	1,473.2636	2,131.2520	3,056.1198	4,033.5588	3,297.2809	4,231.4533
Hours worked per week	41.2611	5.9459	41.3415	4.5859	41.7049	3.8136
Black and brown	0.2717	0.4448	0.2250	0.4176	0.2258	0.4181
Female	0.4084	0.4915	0.3265	0.4689	0.3040	0.4600
Armed forces, police and firefighters	0.0155	0.1236	0.0001	0.0079	0.0000	0.0053
Superiors of the P.A., leaders and managers	0.0449	0.2070	0.0552	0.2283	0.0558	0.2296
Workers of science and arts	0.0894	0.2854	0.1529	0.3599	0.1563	0.3632
Middle level technicians	0.1039	0.3051	0.1390	0.3459	0.1399	0.3469
Administrative services	0.1961	0.3970	0.2020	0.4015	0.1876	0.3904
Services, workers (sellers) of commerce	0.2399	0.4270	0.1146	0.3185	0.1071	0.3092
Farming, forestry, hunting and fishing	0.0397	0.1952	0.0141	0.1178	0.0145	0.1196
Production of industrial goods and services (1)	0.2078	0.4057	0.2418	0.4282	0.2560	0.4364
Production of industrial goods and services (2)	0.0371	0.1890	0.0538	0.2256	0.0557	0.2294
Maintenance and Repair	0.0257	0.1583	0.0266	0.1608	0.0271	0.1623
Less than middle school	0.1797	0.3839	0.1045	0.3059	0.0974	0.2965
Middle School	0.2236	0.4167	0.1410	0.3481	0.1377	0.3446
High School	0.4493	0.4974	0.4673	0.4989	0.4596	0.4984
More than High School	0.1474	0.3545	0.2871	0.4524	0.3053	0.4606
Age 25-30	0.2104	0.4076	0.2350	0.4240	0.2426	0.4286
Age 30-35	0.1506	0.3576	0.1664	0.3724	0.1699	0.3755
Age 35-40	0.1271	0.3331	0.1308	0.3372	0.1319	0.3384
Age 40-44	0.3257	0.4686	0.3190	0.4661	0.3041	0.4600
Full-time	0.9074	0.2899	0.9333	0.2495	0.9548	0.2078
Number of workers in the Firm (size)	13.99		128.20		222.82	

Source: RAIS 2009

Table 2: Descriptive statistics of the **female** workforce

Variables	All Firms		*Empresa Cidadã* firms		*Empresa Cidadã* firms and our analysis sample	
	(1) mean	(2) sd	(3) mean	(4) sd	(5) mean	(6) sd
Wage	1,318.0980	1,800.9414	2,642.5439	3,262.1285	2,925.3491	3,506.1750
Hours worked per week	39.9135	7.1339	40.2026	5.5654	40.9600	4.4787
Black and brown	0.2172	0.4124	0.1794	0.3837	0.1899	0.3922
Female	1.0000	0.0000	1.0000	0.0000	1.0000	0.0000
Armed forces, police and firefighters	0.0030	0.0551	0.0001	0.0099	0.0000	0.0056
Superiors of the P.A., leaders and managers	0.0488	0.2155	0.0553	0.2285	0.0564	0.2306
Workers of science and arts	0.1361	0.3429	0.1992	0.3994	0.2009	0.4007
Middle level technicians	0.1400	0.3470	0.1396	0.3466	0.1317	0.3382
Administrative services	0.2794	0.4487	0.3165	0.4651	0.3082	0.4618
Services, workers (sellers) of commerce	0.2729	0.4455	0.1388	0.3458	0.1361	0.3429
Farming, forestry, hunting and fishing	0.0120	0.1089	0.0055	0.0739	0.0060	0.0775
Production of industrial goods and services (1)	0.0821	0.2744	0.1119	0.3153	0.1261	0.3320
Production of industrial goods and services (2)	0.0193	0.1375	0.0263	0.1600	0.0271	0.1623
Maintenance and Repair	0.0063	0.0794	0.0067	0.0815	0.0074	0.0856
Less than middle school	0.1131	0.3167	0.0643	0.2452	0.0593	0.2361
Middle School	0.1784	0.3829	0.1000	0.3000	0.0971	0.2961
High School	0.4967	0.5000	0.4646	0.4987	0.4385	0.4962
More than High School	0.2117	0.4085	0.3711	0.4831	0.4051	0.4909
Age 25-30	0.2113	0.4082	0.2555	0.4361	0.2724	0.4452
Age 30-35	0.1500	0.3571	0.1762	0.3810	0.1821	0.3859
Age 35-40	0.1278	0.3338	0.1293	0.3356	0.1279	0.3340
Age 40-44	0.3277	0.4694	0.2833	0.4506	0.2503	0.4332
Full-time	0.8516	0.3555	0.8857	0.3182	0.9248	0.2636
Number of female workers in the Firm (size)	5.71		41.86		67.74	

Source: RAIS 2009

Table 3: Event Study: labor market outcomes after maternity leave taking

Months since the event	All Firms employed	separation	hiring	*Empresa Cidadã* firms employed	separation	hiring
-36	-0.3113***	0.0142***	0.0174***	-0.1537***	0.0137***	0.0152***
	(0.0021)	(0.0007)	(0.0007)	(0.0088)	(0.0025)	(0.0025)
-24	-0.2563***	0.0148***	0.0206***	-0.1078***	0.0086***	0.0144***
	(0.0018)	(0.0006)	(0.0006)	(0.0077)	(0.0022)	(0.0021)
-18	-0.2106***	0.0127***	0.0224***	-0.0731***	0.0057***	0.0133***
	(0.0016)	(0.0005)	(0.0005)	(0.0071)	(0.0021)	(0.0020)
-12	-0.1342***	0.0083***	0.0228***	-0.0361***	0.0030	0.0109***
	(0.0014)	(0.0005)	(0.0005)	(0.0065)	(0.0019)	(0.0018)
-9	-0.0879***	0.0047***	0.0186***	-0.0164***	0.0017	0.0045***
	(0.0013)	(0.0004)	(0.0004)	(0.0061)	(0.0018)	(0.0017)
-6	-0.0414***	0.0014***	0.0127***	-0.0079	0.0004	0.0024
	(0.0012)	(0.0004)	(0.0004)	(0.0058)	(0.0017)	(0.0016)
+1	0.0009	0.0005	0.0001	-0.0004	0.0000	-0.0000
	(0.0012)	(0.0004)	(0.0004)	(0.0055)	(0.0016)	(0.0015)
+2	0.0014	0.0010***	0.0001	-0.0009	0.0006	-0.0001
	(0.0012)	(0.0004)	(0.0004)	(0.0055)	(0.0016)	(0.0015)
+3	0.0014	0.0026***	-0.0000	-0.0020	0.0015	-0.0003
	(0.0012)	(0.0004)	(0.0004)	(0.0056)	(0.0016)	(0.0016)
+4	-0.0002	0.0364***	-0.0000	-0.0041	0.0204***	-0.0005
	(0.0012)	(0.0004)	(0.0004)	(0.0057)	(0.0016)	(0.0016)
+5	-0.0357***	0.0782***	0.0007*	-0.0254***	0.0430***	0.0000
	(0.0012)	(0.0004)	(0.0004)	(0.0057)	(0.0017)	(0.0016)
+6	-0.1123***	0.0973***	0.0019***	-0.0687***	0.0561***	0.0011
	(0.0012)	(0.0004)	(0.0004)	(0.0058)	(0.0017)	(0.0016)
+7	-0.2068***	0.0794***	0.0037***	-0.1240***	0.0407***	0.0027
	(0.0013)	(0.0004)	(0.0004)	(0.0059)	(0.0017)	(0.0017)
+8	-0.2817***	0.0530***	0.0051***	-0.1624***	0.0269***	0.0033*
	(0.0013)	(0.0004)	(0.0004)	(0.0060)	(0.0017)	(0.0017)
+9	-0.3286***	0.0380***	0.0073***	-0.1864***	0.0235***	0.0055***
	(0.0013)	(0.0004)	(0.0004)	(0.0061)	(0.0018)	(0.0017)
+12	-0.3886***	0.0250***	0.0183***	-0.2319***	0.0176***	0.0097***
	(0.0014)	(0.0005)	(0.0005)	(0.0065)	(0.0019)	(0.0018)
+18	-0.3944***	0.0209***	0.0208***	-0.2751***	0.0167***	0.0110***
	(0.0016)	(0.0005)	(0.0005)	(0.0071)	(0.0021)	(0.0020)
+24	-0.3902***	0.0211***	0.0203***	-0.3051***	0.0177***	0.0138***
	(0.0018)	(0.0006)	(0.0006)	(0.0077)	(0.0022)	(0.0021)
+36	-0.3954***	0.0214***	0.0196***	-0.3368***	0.0163***	0.0145***
	(0.0021)	(0.0007)	(0.0007)	(0.0088)	(0.0025)	(0.0025)
+48	-0.4018***	0.0213***	0.0198***	-0.3559***	0.0173***	0.0141***
	(0.0025)	(0.0008)	(0.0008)	(0.0099)	(0.0028)	(0.0028)
+60	-0.4086***	0.0214***	0.0200***	-0.3795***	0.0184***	0.0161***
	(0.0029)	(0.0009)	(0.0009)	(0.0109)	(0.0031)	(0.0030)
Observations	24,975,324	24,975,324	24,975,324	892,188	892,188	892,188
R-squared	0.2760	0.0206	0.0120	0.1796	0.0131	0.0102
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Descriptive statistics of analysis sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All Data		All Data and Running>=0		All Data and Running<0		All Data and Running $\in [0,12]$		All Data and Running $\in [-12,0)$	
Variables	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Wage	3,795.0799	4,391.9304	4,057.5943	4,683.1101	3,291.7320	3,719.9335	3,560.3766	4,118.9239	3,396.3637	3,797.8265
Hours worked per week	412.589	4.4341	41.2950	4.5898	41.1897	4.1184	41.3727	4.3258	41.3540	4.0197
Prob(period>120)	0.2011	0.4008	0.2874	0.4526	0.0356	0.1853	0.2835	0.4507	0.0548	0.2275
Black and brown	0.2064	0.4047	0.2215	0.4153	0.1774	0.3820	0.1927	0.3945	0.1862	0.3893
Less than middle school	0.0335	0.1798	0.0327	0.1778	0.0350	0.1837	0.0303	0.1714	0.0345	0.1826
Middle School	0.0667	0.2495	0.0646	0.2459	0.0706	0.2562	0.0653	0.2470	0.0720	0.2584
High School	0.4185	0.4933	0.4023	0.4904	0.4496	0.4975	0.4213	0.4938	0.4249	0.4944
More than High School	0.4814	0.4997	0.5004	0.5000	0.4449	0.4970	0.4831	0.4997	0.4686	0.4990
Age 25-30	0.3903	0.4878	0.3801	0.4854	0.4100	0.4918	0.3953	0.4889	0.4010	0.4901
Age 30-35	0.3905	0.4879	0.3935	0.4885	0.3848	0.4866	0.3833	0.4862	0.3855	0.4867
Age 35-40	0.1883	0.3909	0.1941	0.3955	0.1771	0.3818	0.1892	0.3917	0.1822	0.3861
Age 40-44	0.0309	0.1730	0.0323	0.1768	0.0282	0.1655	0.0322	0.1764	0.0313	0.1740
Full-time	0.9436	0.2308	0.9393	0.2388	0.9517	0.2143	0.9466	0.2248	0.9531	0.2114
Number of Workers	46,603		30,274		15,789		7,493		7,616	
Number of Firms	4,118		3,471		2,272		2,094		1,777	

Table 5: Covariate balancing: RDD regresions on the covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Black/brown	< MS	Middle School	High School	>HS	25-30	30-35	35-40	40-44	Wage	Hours	Full-time
Left linear	-0.0003	-0.0000	-0.0008	0.0034**	-0.0025*	0.0010	-0.0016	0.0009	-0.0003	-6.5435	-0.0092	-0.0002
	(0.0012)	(0.0005)	(0.0007)	(0.0015)	(0.0013)	(0.0017)	(0.0017)	(0.0014)	(0.0007)	(11.3363)	(0.0110)	(0.0007)
Right linear	0.0004	-0.0006	0.0000	-0.0022	0.0029**	0.0028	-0.0009	-0.0012	-0.0007	15.9882	0.0176*	0.0004
	(0.0013)	(0.0006)	(0.0008)	(0.0016)	(0.0014)	(0.0018)	(0.0019)	(0.0015)	(0.0007)	(11.8312)	(0.0102)	(0.0007)
Treat ($I(R_i \geq 0)$)	0.0054	-0.0006	-0.0021	-0.0024	0.0051	-0.0412**	0.0208	0.0122	0.0082	183.7823	-0.1973*	-0.0096
	(0.0124)	(0.0058)	(0.0079)	(0.0149)	(0.0135)	(0.0176)	(0.0179)	(0.0143)	(0.0065)	(116.8581)	(0.1022)	(0.0068)
Obs.	15,109	15,109	15,109	15,109	15,109	15,109	15,109	15,109	15,109	15,109	15,109	15,109
R-sq	0.3691	0.3149	0.3483	0.4022	0.5321	0.1852	0.1351	0.1375	0.0999	0.4680	0.5802	0.4170
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Observations are restricted around the threshold, by considering 12 months before and after the cutoff (i.e., $R_i \in [-12, 12]$). We use triangular weights and add individual controls (such as age, race, education, and occupation) as well as firm fixed effects for precision. Regressions consider linear functions in both sides of the discontinuity.

Table 6: First Stage

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Number of days of maternity leave			Probabilty of taking extended ML		
$I(R_i \geq 0)$	7.845***	7.800***	7.769***	0.132***	0.131***	0.130***
	(0.700)	(0.699)	(0.531)	(0.012)	(0.012)	(0.009)
Left linear	0.317***	0.319***	0.279***	0.005***	0.005***	0.005***
	(0.082)	(0.082)	(0.052)	(0.001)	(0.001)	(0.001)
Right linear	0.600***	0.611***	0.499***	0.011***	0.011***	0.009***
	(0.054)	(0.055)	(0.054)	(0.001)	(0.001)	(0.001)
Black and brown		-1.945***	-0.491		-0.031***	-0.008
		(0.468)	(0.356)		(0.008)	(0.006)
Middle School		-0.415	1.227		-0.007	0.021
		(1.209)	(1.005)		(0.020)	(0.017)
High School		1.834*	1.347		0.029*	0.023
		(1.039)	(0.974)		(0.017)	(0.016)
More than High School		-1.258	1.779*		-0.023	0.030*
		(1.034)	(1.018)		(0.017)	(0.017)
Age 30-35		0.762*	-0.119		0.013*	-0.002
		(0.423)	(0.300)		(0.007)	(0.005)
Age 35-40		0.071	-0.611		0.001	-0.010
		(0.515)	(0.383)		(0.009)	(0.006)
Age 40-44		-0.430	-0.360		-0.006	-0.005
		(1.020)	(0.756)		(0.017)	(0.013)
Constant	127.493***	127.469***		0.123***	0.123***	
	(0.445)	(1.107)		(0.007)	(0.018)	
Observations	15,109	15,109	15,109	15,109	15,109	15,109
R-squared	0.085	0.090	0.673	0.089	0.094	0.679
Firm FE	No	No	Yes	No	No	Yes

Note: Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Observations are restricted around the threshold, by considering 12 months before and after the cutoff (i.e., $R_i \in [-12, 12]$). We use triangular weights and add individual controls (such as age, race, education, and occupation) as well as firm fixed effects for precision. Regressions consider linear functions in both sides of the discontinuity.

Table 7: Effects of the expanded maternity leave on employment outcomes

Months after the event	(1) Employed	(2) Separation	(3) Hiring	(4) Cum. employed
+5	0.004857	-0.011833*	-0.000282	0.004385
	(0.003790)	(0.006082)	(0.001015)	(0.004121)
+6	0.016426**	0.005675	-0.001553**	0.020819**
	(0.006921)	(0.007397)	(0.000753)	(0.009489)
+7	0.009244	-0.001261	-0.003176	0.030112*
	(0.009708)	(0.007847)	(0.002199)	(0.017153)
+8	0.007298	0.002719	0.002037	0.037224
	(0.011698)	(0.006415)	(0.002284)	(0.026301)
+9	0.006496	-0.021041***	0.002480	0.043574
	(0.012719)	(0.006032)	(0.002810)	(0.036650)
+10	0.029909**	0.004763	-0.002185	0.073323
	(0.013290)	(0.004812)	(0.002454)	(0.047565)
+11	0.022978*	-0.001624	-0.001295	0.095301
	(0.013615)	(0.004877)	(0.003253)	(0.058979)
+12	0.021432	-0.004992	0.000166	0.111655
	(0.013872)	(0.004498)	(0.003187)	(0.070555)
+18	0.004912	0.005640	-0.007919**	0.263100*
	(0.015345)	(0.004701)	(0.003082)	(0.143957)
+24	-0.008972	-0.001430	0.000354	0.175312
	(0.016585)	(0.003949)	(0.004073)	(0.223232)
+36	-0.007215	-0.007973*	-0.006955*	0.021893
	(0.018070)	(0.004752)	(0.003752)	(0.389154)
+48	-0.005327	-0.006808	0.007021*	0.198982
	(0.019645)	(0.004372)	(0.003876)	(0.578895)
+60	0.013740	0.003578	0.003319	0.321941
	(0.022933)	(0.004086)	(0.003863)	(0.852638)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Note: Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Observations are restricted around the threshold, by considering 12 months before and after the cutoff (i.e., $R_i \in [-12, 12]$). We use triangular weights and add individual controls (such as age, race, education, and occupation) as well as firm fixed effects for precision. Regressions consider linear functions in both sides of the discontinuity.

Table 8: Robustness check for changes in the baseline model

Months after the event	(1) Employed [-6,6]	(2) Employed [-18,18]	(3) Employed [-24,24]	(4) Employed [-12,12]	(5) Employed [-12,12]
+5	-0.005342	0.003843	0.005472*	0.004922	-0.006589
	(0.005147)	(0.003322)	(0.002896)	(0.003524)	(0.007919)
+6	0.007751	0.022448***	0.023857***	0.019746***	0.010454
	(0.009197)	(0.005848)	(0.005144)	(0.006569)	(0.014426)
+7	0.007119	0.015692*	0.016728**	0.015450	0.014573
	(0.013753)	(0.008151)	(0.007186)	(0.009571)	(0.021520)
+8	0.007957	0.007280	0.008985	0.002687	0.014938
	(0.016978)	(0.009686)	(0.008517)	(0.011598)	(0.026428)
+9	0.003830	0.005500	0.007940	-0.007873	0.001995
	(0.018499)	(0.010542)	(0.009271)	(0.012435)	(0.028560)
+10	0.041475**	0.021476*	0.020108**	0.013414	0.052541*
	(0.019541)	(0.011007)	(0.009679)	(0.012998)	(0.030290)
+11	0.036336*	0.013536	0.012873	0.005266	0.040800
	(0.019977)	(0.011311)	(0.009949)	(0.013238)	(0.030867)
+12	0.030392	0.013525	0.010773	0.004881	0.034418
	(0.020276)	(0.011524)	(0.010151)	(0.013347)	(0.031078)
+18	-0.003963	0.002707	-0.002949	-0.006561	-0.025556
	(0.022659)	(0.012686)	(0.011145)	(0.014202)	(0.034363)
+24	-0.004070	-0.005830	-0.012770	-0.011518	0.006118
	(0.024791)	(0.013619)	(0.011862)	(0.015020)	(0.037470)
+36	-0.024357	0.001062	-0.006730	0.004857	-0.011480
	(0.027068)	(0.014977)	(0.013155)	(0.016244)	(0.040505)
+48	-0.010638	-0.005465	-0.017809	0.004785	-0.021932
	(0.029163)	(0.016369)	(0.014590)	(0.017650)	(0.043923)
+60	0.020925	0.020721	0.012454	0.013897	0.022828
	(0.034020)	(0.020989)	(0.019736)	(0.020532)	(0.051242)
Linear pol.	Yes	Yes	Yes	Yes	No
Cubic pol.	No	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A Appendix

Table A.1: Descriptive Statistics for Women in the Labor Market of Brazil

	People aged 25-44				People aged 25-44 Legally employed in priv. sec.				Additionally, who had Child in the last 12 mont	
	Women		Men		Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Head of Family	0.319	0.466	0.572	0.495	0.318	0.466	0.580	0.494	0.308	0.462
Urban	0.869	0.337	0.853	0.355	0.971	0.169	0.949	0.221	0.973	0.163
Black or Mulatto	0.541	0.498	0.562	0.496	0.447	0.497	0.522	0.500	0.467	0.499
Age in years	34.419	5.632	34.315	5.658	33.520	5.535	33.841	5.574	31.440	4.308
Worked on reference week	0.620	0.485	0.856	0.351	1.000	0.000	1.000	0.000	1.000	0.000
Employed in private sector	0.719	0.450	0.853	0.354	1.000	0.000	1.000	0.000	1.000	0.000
Legally employed	0.689	0.463	0.755	0.430	1.000	0.000	1.000	0.000	1.000	0.000
Legally employed in private sector	0.384	0.486	0.436	0.496	1.000	0.000	1.000	0.000	1.000	0.000
>30 min. from home to work	0.334	0.472	0.354	0.478	0.406	0.491	0.426	0.494	0.349	0.477
Average number of hours of work per week	0.620	0.485	0.856	0.351	1.000	0.000	1.000	0.000	1.000	0.000
Full time	0.408	0.492	0.720	0.449	0.864	0.342	0.926	0.262	0.881	0.324
Contributed to social security	0.683	0.465	0.664	0.472	1.000	0.000	1.000	0.000	1.000	0.000
Number of years worked on this job	5.205	5.705	6.229	6.553	3.935	4.258	4.677	4.996	3.917	3.574
Age when started working	16.261	4.441	14.801	3.732	16.933	3.893	15.446	3.410	16.958	3.578
Economically active	0.726	0.446	0.933	0.250	1.000	0.000	1.000	0.000	1.000	0.000
Did Household chores in the ref. week	0.923	0.267	0.542	0.498	0.891	0.311	0.565	0.496	0.935	0.246
Tried to find work last month	0.013	0.113	0.016	0.126	0.014	0.117	0.013	0.115	0.005	0.069
Student	0.072	0.258	0.056	0.230	0.082	0.275	0.061	0.240	0.046	0.209
Literate	0.970	0.170	0.950	0.219	0.997	0.054	0.989	0.102	0.998	0.041
Years of schooling	10.855	3.991	10.054	4.164	12.358	3.013	11.086	3.431	12.489	2.940
Monthly income from main job	1,720.737	2,758.719	2,219.650	3,097.427	1,921.605	2,751.511	2,253.463	2,851.250	1,816.086	2,263.994
Monthly income from all occupations	1,771.645	2,829.786	2,275.167	3,167.356	1,952.890	2,796.593	2,292.162	2,894.385	1,836.766	2,282.714
Monthly income from all sources	1,274.716	2,526.570	2,044.396	3,103.970	2,006.279	2,844.597	2,309.695	2,925.040	1,870.325	2,276.200
Monthly household income per capita	3,695.737	4,182.499	3,787.712	4,195.140	4,630.138	4,275.417	4,073.062	4,004.291	4,702.002	4,428.078
Had a child in the last 12 months	0.053	0.224	-	-	0.035	0.183	-	-	1.000	0.000

Source: PNAD 2015.

Table A.2: Female Employment and Responsive to Children in Brazil

	(1)	(2)
	Fraction Working	Fraction Working $\geq$ 35
Panel A: Women 25-44		
All	0.6452	0.4537
Conditional on age of youngest child:		
1 month	0.2942	0.1799
3 months	0.2880	0.1971
6 months	0.3510	0.2295
1 year	0.4171	0.2801
3 years	0.4948	0.3284
6 years	0.5477	0.3595
Panel B: Men 25-44		
All	0.8800	0.7765
Conditional on age of youngest child:		
1 month	0.89120	0.80191
6 months	0.92708	0.81822
1 year	0.92872	0.82438
6 years	0.92775	0.82585

Source: PNAD 2015

A.1 Data for the Event Study

In this part, we use RAIS (Relação Anual de Informações Sociais), which is the Brazilian matched employer-employee dataset provided by the Ministry of Labor. Readers can learn more about RAIS in the Data Descriptions section of the paper. Although RAIS is an annual dataset, we extrapolate monthly information on maternity leave and employment status, by identifying dates of the maternity leave period and admission/resignation dates for each employer-employee pair. We observe the exact month when each woman started the maternity leave (if any) and her employment situation over time. Therefore, we build a rich monthly panel dataset of women who took maternity leave in 2009 and follow them during the period 2006-2014. We choose 2009 because it is right one year before the beginning of the *Empresa Cidadã Program*, which is the program evaluated in this paper.

We impose some restrictions on our datasets, such as: 1) we restrict the analyses to those women who took one (and only one) maternity leave during the year; 2) we select only women who worked in a private companies (at the time of the leave-taking) and 3) we choose women who were from 25 to 44 years old (at the time of the leave-taking);[24] 4) we restrict our analysis to those leave-taking that lasted in the total[25]: 120 days (conventional maternity leave period), 135 days (conventional maternity leave period plus two weeks) or 180 days (extended maternity leave). After all restrictions, a total of 239,514 women remained in our dataset followed over 108 months, giving more than 25 million observations.

A.2 Event Study Analysis

We analyze a large panel of women with monthly observations on labor market outcomes as well as information on the maternity leave period (beginning and end). We consider binary indicators for *Employment* (E_{it}), *Separation* (S_{it}) and *Hiring* (H_{it}). The first outcome of interest, E_{it}, is equal 1 if the individual i is employed at t and 0 otherwise. Based on E_{it} we define $S_{it} = E_{it} \times (1 - E_{it+1})$ and $H_{it} = E_{it+1} \times (1 - E_{it})$. Therefore, S_{it} (H_{it}) indicates that the individual i is employed (unemployed) at t but no longer at $t+1$. Notice that the triad (E_{it}, S_{it}, H_{it}) completely describes the dynamics of employment according to the following identity:

$$E_{it} \equiv E_{it-1} + H_{it-1} - S_{it-1} \tag{4}$$

Let y_{it} denote generically the outcomes and, in order to give some intuition underlying the econometric model, define the individual leave-taking cohort, l, as the year-month of the leave-taking and relative event time, r, as the number of months between calendar time, t, and the moment of leave-taking l (i.e., $r = t - l$). Then, we can define the event time r as the number of months since the month in which the maternity leave started. Using this notation, we can write the following fully nonparametric model:

[24]The first restriction is necessarybecause our empirical approach requires that the time of the event is uniquely identified, in order to calculate the relative distances from each month to the time of the event for every individual. The second restriction considers the fact that private companies have very different dynamics relative to the public sector, not only with respect to maternity leave policy but also concerning the way of admission and the chances of being hired/separated for any reason.

[25]Actually, we use one (2-days) margin of error to each one of them, in other words: 118-122, 133-137 or 178-182 days. This filter is important to exclude cases in which we are not interested in studying (such as abortion, adoption, etc.), as well as to avoid potential mistakes in the leave-taking date information (start and finish) when someone filled out the form. This filter discards approximately 7% from both databases.

$$y_{it} = \delta_l + \gamma_t + \mu_r + \epsilon_{it} \tag{5}$$

Where δ_l are leave-taking cohort fixed effects; γ_t are calendar time fixed effects, and μ_r are fixed effects for months relative to the moment of leave-taking, which takes place at the month 0. Thus, an individual-year observation is indexed by leave-taking cohort l, year-month t and relative event time r. There is a well-known problem in this type of analysis: leave-taking cohort is collinear with the combination of t and r and we cannot separately identify the cohort, calendar time and relative event time effects. Therefore, in order to identify Equation (2), at least one set of fixed effects must be assumed to be the same. We assume that there are no leave-taking cohort effects, i.e., $\delta_l = 0$.

The key coefficients of interest refer to the pattern on the μ_r, which estimate the outcome at a given r relative to the omitted month of the leave-taking (in this paper, μ_0). Time fixed effects control for secular trends in the labor market outcomes. As the baseline non-parametric event study omits the month of the leave-taking (i.e., μ_0), all coefficients $\mu_r' s$ must be interpreted relative to this omitted month. We follow women from prior 47 months to 71 months after the moment when they took maternity leave ($r = -47, ..., -1, 0, +1, ..., +71$). We expand the basic model in order to control for both individual and firm characteristics:

$$y_{itj} = \gamma_t + \sum_{r=-47}^{r=-1} \mu_r + \sum_{r=1}^{r=71} \mu_r + \lambda_j + X_{ij}'\beta + \epsilon_{itj} \tag{6}$$

Where λ_j are firm fixed effects and X_{ij} are individual characteristics (such as occupation, race, education, and age). Both of them are considered at the time of the leave-taking and j indexes the firm where the woman was working at the time of the event. The identifying assumption in Equation (3) is that conditional on taking a maternity leave within 4-year prior and 6-year after maternity leave is taken, the timing of leave is uncorrelated with the outcome, conditional on the calendar time fixed effects as well as firm and individual characteristics.

We analyze heterogeneous effects, based on the women's level of education, wages/position, firm size and composition of the workfore. To do this, we simply divide the sample based on the respective groups and estimate the event studies for each group.

Table A.3: Labor market consequences of maternity leave on employment, separations and hirings

Monsths since the event	(1) employed	(2) separation	(3) hiring
-36	-0.304319***	0.014209***	0.017368***
	(0.002066)	(0.000664)	(0.000664)
-24	-0.250178***	0.014562***	0.020345***
	(0.001721)	(0.000553)	(0.000553)
-18	-0.205156***	0.012475***	0.022110***
	(0.001551)	(0.000498)	(0.000499)
-12	-0.130444***	0.008167***	0.022352***
	(0.001385)	(0.000445)	(0.000445)
-9	-0.085095***	0.004605***	0.018150***
	(0.001298)	(0.000417)	(0.000417)
-6	-0.040091***	0.001404***	0.012331***
	(0.001221)	(0.000392)	(0.000393)
+1	0.000849	0.000515	0.000098
	(0.001141)	(0.000367)	(0.000367)
+2	0.001280	0.000975***	0.000071
	(0.001150)	(0.000369)	(0.000370)
+3	0.001224	0.002513***	-0.000022
	(0.001164)	(0.000374)	(0.000374)
+4	-0.000462	0.035871***	-0.000044
	(0.001180)	(0.000379)	(0.000379)
+5	-0.035529***	0.076965***	0.000628
	(0.001199)	(0.000385)	(0.000386)
+6	-0.111019***	0.095921***	0.001854***
	(0.001221)	(0.000392)	(0.000393)
+7	-0.204238***	0.078088***	0.003648***
	(0.001245)	(0.000400)	(0.000400)
+8	-0.277831***	0.052121***	0.005049***
	(0.001271)	(0.000408)	(0.000409)
+9	-0.324057***	0.037461***	0.007232***
	(0.001298)	(0.000417)	(0.000417)
+12	-0.383688***	0.024784***	0.018022***
	(0.001385)	(0.000445)	(0.000445)
+18	-0.391054***	0.020795***	0.020473***
	(0.001551)	(0.000498)	(0.000499)
+24	-0.388289***	0.020942***	0.020060***
	(0.001721)	(0.000553)	(0.000553)
+36	-0.394998***	0.021263***	0.019448***
	(0.002066)	(0.000663)	(0.000664)
+48	-0.402360***	0.021207***	0.019555***
	(0.002414)	(0.000775)	(0.000776)
+60	-0.410230***	0.021261***	0.019885***
	(0.002764)	(0.000888)	(0.000889)
Observations	25,867,512	25,867,512	25,867,512
Controls	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Probability of being formally employed ’r’ months before and after, conditional on being employed on December 31, 2009.

Relative time r since December 2009.	(1) male aged 25-44	(2) female aged 25-44
-36	-0.328164***	-0.407574***
	(0.000170)	(0.000242)
-24	-0.281136***	-0.347289***
	(0.000170)	(0.000242)
-12	-0.224499***	-0.256499***
	(0.000170)	(0.000242)
+12	-0.202503***	-0.211611***
	(0.000170)	(0.000242)
+24	-0.238118***	-0.270203***
	(0.000170)	(0.000242)
+36	-0.263626***	-0.301325***
	(0.000170)	(0.000242)
+48	-0.288115***	-0.334834***
	(0.000170)	(0.000242)
+60	-0.311959***	-0.356123***
	(0.000170)	(0.000242)
Observations	95,689,143	51,436,908
Controls	Yes	Yes
Firm FE	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Maternity leave effects on separation by cause and initiative

Monsths since the event	(1) Separation	(2) Fair & Employer's Init.	(3) No Fair & Employer's Init.	(4) Fair & Employee's Init.	(5) No Fair & Employee's Init.	(6) other
-36	0.0142***	0.0001*	0.0087***	0.0001***	0.0018***	0.0035***
	(0.0007)	(0.0001)	(0.0005)	(0.0000)	(0.0003)	(0.0003)
-24	0.0146***	0.0001	0.0090***	0.0001***	0.0021***	0.0034***
	(0.0006)	(0.0001)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
-18	0.0125***	0.0001**	0.0072***	0.0000*	0.0018***	0.0033***
	(0.0005)	(0.0001)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
-12	0.0082***	0.0001	0.0041***	0.0000	0.0011***	0.0030***
	(0.0004)	(0.0000)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
-9	0.0046***	0.0000	0.0020***	0.0000	0.0005***	0.0020***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
-6	0.0014***	0.0000	0.0003	0.0000	-0.0001	0.0011***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+1	0.0005	0.0000	0.0004	-0.0000	-0.0000	0.0001
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+2	0.0010***	0.0000	0.0007**	0.0000	0.0000	0.0002
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+3	0.0025***	-0.0000	0.0009***	-0.0000	0.0006***	0.0010***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+4	0.0359***	0.0002***	0.0148***	0.0001***	0.0182***	0.0026***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+5	0.0770***	0.0006***	0.0552***	0.0001***	0.0183***	0.0027***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+6	0.0959***	0.0010***	0.0831***	0.0001***	0.0091***	0.0026***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+7	0.0781***	0.0008***	0.0708***	0.0000**	0.0042***	0.0023***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+8	0.0521***	0.0007***	0.0462***	0.0000**	0.0030***	0.0023***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+9	0.0375***	0.0005***	0.0320***	0.0000**	0.0025***	0.0024***
	(0.0004)	(0.0000)	(0.0003)	(0.0000)	(0.0002)	(0.0002)
+12	0.0248***	0.0003***	0.0193***	0.0000***	0.0023***	0.0029***
	(0.0004)	(0.0000)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
+18	0.0208***	0.0002***	0.0133***	-0.0000	0.0031***	0.0041***
	(0.0005)	(0.0001)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
+24	0.0209***	0.0002***	0.0130***	0.0000	0.0036***	0.0040***
	(0.0006)	(0.0001)	(0.0004)	(0.0000)	(0.0002)	(0.0002)
+36	0.0213***	0.0002**	0.0133***	-0.0000	0.0035***	0.0043***
	(0.0007)	(0.0001)	(0.0005)	(0.0000)	(0.0003)	(0.0003)
+48	0.0212***	0.0002**	0.0131***	-0.0000	0.0037***	0.0042***
	(0.0008)	(0.0001)	(0.0006)	(0.0000)	(0.0003)	(0.0003)
+60	0.0213***	0.0002**	0.0131***	-0.0000	0.0037***	0.0043***
	(0.0009)	(0.0001)	(0.0007)	(0.0000)	(0.0004)	(0.0004)
Observations	25,867,512	25,867,512	25,867,512	25,867,512	25,867,512	25,867,512
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.6: Heterogeneous effects of maternity leave on employment by level of education

Monsths since the event	(1) < High School	(2) High School	(3) > High School
-36	-0.3575***	-0.3203***	-0.1909***
	(0.0044)	(0.0030)	(0.0040)
-24	-0.2970***	-0.2638***	-0.1439***
	(0.0036)	(0.0025)	(0.0033)
-18	-0.2434***	-0.2155***	-0.1165***
	(0.0032)	(0.0022)	(0.0030)
-12	-0.1498***	-0.1367***	-0.0767***
	(0.0028)	(0.0020)	(0.0026)
-9	-0.0943***	-0.0885***	-0.0531***
	(0.0026)	(0.0018)	(0.0025)
-6	-0.0420***	-0.0413***	-0.0258***
	(0.0025)	(0.0017)	(0.0023)
+1	0.0006	0.0009	-0.0005
	(0.0023)	(0.0016)	(0.0021)
+2	0.0006	0.0014	-0.0012
	(0.0023)	(0.0016)	(0.0022)
+3	-0.0001	0.0015	-0.0021
	(0.0023)	(0.0016)	(0.0022)
+4	-0.0025	-0.0001	-0.0044**
	(0.0024)	(0.0017)	(0.0022)
+5	-0.0466***	-0.0344***	-0.0311***
	(0.0024)	(0.0017)	(0.0023)
+6	-0.1273***	-0.1132***	-0.0932***
	(0.0025)	(0.0017)	(0.0023)
+7	-0.2350***	-0.2112***	-0.1580***
	(0.0025)	(0.0018)	(0.0024)
+8	-0.3243***	-0.2914***	-0.1972***
	(0.0026)	(0.0018)	(0.0024)
+9	-0.3832***	-0.3409***	-0.2204***
	(0.0026)	(0.0018)	(0.0025)
+12	-0.4574***	-0.4048***	-0.2517***
	(0.0028)	(0.0020)	(0.0026)
+18	-0.4680***	-0.4056***	-0.2672***
	(0.0032)	(0.0022)	(0.0030)
+24	-0.4680***	-0.3937***	-0.2790***
	(0.0036)	(0.0025)	(0.0033)
+36	-0.4830***	-0.3848***	-0.3068***
	(0.0044)	(0.0030)	(0.0040)
+48	-0.4928***	-0.3841***	-0.3266***
	(0.0052)	(0.0036)	(0.0048)
+60	-0.5081***	-0.3851***	-0.3399***
	(0.0060)	(0.0042)	(0.0055)
Observations	6,862,104	13,389,084	5,616,324
Controls	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.7: Heterogeneous effects of maternity leave on employment by earnings and job position

Monsths since the event	(1) Earns **below** the median (707 BRL)	(2) Earns **above** the median (707 BRL)	(3) **NOT** Superior, leader or manager	(4) Superior, leader or manager
-36	-0.4072***	-0.1993***	-0.3110***	-0.1732***
	(0.0032)	(0.0027)	(0.0021)	(0.0126)
-24	-0.3445***	-0.1536***	-0.2538***	-0.1750***
	(0.0027)	(0.0022)	(0.0018)	(0.0094)
-18	-0.2877***	-0.1205***	-0.2072***	-0.1608***
	(0.0024)	(0.0020)	(0.0016)	(0.0079)
-12	-0.1835***	-0.0755***	-0.1303***	-0.1293***
	(0.0021)	(0.0018)	(0.0014)	(0.0064)
-9	-0.1162***	-0.0524***	-0.0840***	-0.1040***
	(0.0019)	(0.0017)	(0.0013)	(0.0058)
-6	-0.0512***	-0.0278***	-0.0390***	-0.0596***
	(0.0018)	(0.0016)	(0.0013)	(0.0052)
+1	0.0005	0.0010	0.0008	0.0008
	(0.0017)	(0.0015)	(0.0012)	(0.0047)
+2	0.0005	0.0017	0.0013	0.0012
	(0.0017)	(0.0015)	(0.0012)	(0.0047)
+3	-0.0003	0.0021	0.0012	0.0009
	(0.0017)	(0.0015)	(0.0012)	(0.0048)
+4	-0.0030*	0.0013	-0.0005	-0.0010
	(0.0018)	(0.0015)	(0.0012)	(0.0049)
+5	-0.0481***	-0.0240***	-0.0362***	-0.0242***
	(0.0018)	(0.0016)	(0.0012)	(0.0051)
+6	-0.1359***	-0.0874***	-0.1125***	-0.0833***
	(0.0018)	(0.0016)	(0.0013)	(0.0052)
+7	-0.2520***	-0.1578***	-0.2066***	-0.1581***
	(0.0019)	(0.0016)	(0.0013)	(0.0054)
+8	-0.3463***	-0.2109***	-0.2813***	-0.2115***
	(0.0019)	(0.0017)	(0.0013)	(0.0056)
+9	-0.4049***	-0.2449***	-0.3280***	-0.2470***
	(0.0019)	(0.0017)	(0.0013)	(0.0058)
+12	-0.4797***	-0.2897***	-0.3882***	-0.2961***
	(0.0021)	(0.0018)	(0.0014)	(0.0064)
+18	-0.4840***	-0.3004***	-0.3940***	-0.3328***
	(0.0024)	(0.0020)	(0.0016)	(0.0079)
+24	-0.4752***	-0.3039***	-0.3899***	-0.3549***
	(0.0027)	(0.0022)	(0.0018)	(0.0094)
+36	-0.4773***	-0.3161***	-0.3939***	-0.4143***
	(0.0032)	(0.0027)	(0.0021)	(0.0126)
+48	-0.4851***	-0.3240***	-0.4002***	-0.4442***
	(0.0039)	(0.0032)	(0.0025)	(0.0159)
+60	-0.4987***	-0.3272***	-0.4080***	-0.4550***
	(0.0045)	(0.0036)	(0.0028)	(0.0192)
Observations	12,933,756	12,933,756	24,622,380	1,245,132
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.8: Heterogeneous effects of maternity leave on employment by size of the firm and workforce gender composition

Months since the event	Number of workers ≤median (=60)	Number of workers >median (=60)	% of female workers ≤ median (=57%)	% of female workers >median (=57%)
-36	-0.3150***	-0.2800***	-0.2853***	-0.3225***
	(0.0043)	(0.0027)	(0.0028)	(0.0030)
-24	-0.2753***	-0.2158***	-0.2315***	-0.2681***
	(0.0032)	(0.0023)	(0.0024)	(0.0025)
-18	-0.2407***	-0.1626***	-0.1852***	-0.2244***
	(0.0027)	(0.0021)	(0.0021)	(0.0022)
-12	-0.1749***	-0.0811***	-0.1144***	-0.1459***
	(0.0022)	(0.0019)	(0.0019)	(0.0020)
-9	-0.1266***	-0.0398***	-0.0735***	-0.0962***
	(0.0020)	(0.0018)	(0.0018)	(0.0019)
-6	-0.0606***	-0.0171***	-0.0351***	-0.0447***
	(0.0018)	(0.0017)	(0.0017)	(0.0018)
+1	0.0004	0.0009	0.0008	0.0009
	(0.0016)	(0.0016)	(0.0016)	(0.0016)
+2	0.0004	0.0014	0.0012	0.0012
	(0.0016)	(0.0016)	(0.0016)	(0.0017)
+3	-0.0004	0.0016	0.0012	0.0010
	(0.0016)	(0.0017)	(0.0016)	(0.0017)
+4	-0.0030*	0.0005	-0.0001	-0.0011
	(0.0017)	(0.0017)	(0.0016)	(0.0017)
+5	-0.0422***	-0.0308***	-0.0311***	-0.0403***
	(0.0017)	(0.0017)	(0.0017)	(0.0017)
+6	-0.1251***	-0.0992***	-0.1019***	-0.1206***
	(0.0018)	(0.0017)	(0.0017)	(0.0018)
+7	-0.2325***	-0.1785***	-0.1872***	-0.2218***
	(0.0018)	(0.0018)	(0.0017)	(0.0018)
+8	-0.3200***	-0.2385***	-0.2521***	-0.3041***
	(0.0019)	(0.0018)	(0.0018)	(0.0018)
+9	-0.3733***	-0.2780***	-0.2930***	-0.3556***
	(0.0020)	(0.0018)	(0.0018)	(0.0019)
+12	-0.4372***	-0.3343***	-0.3517***	-0.4162***
	(0.0022)	(0.0019)	(0.0019)	(0.0020)
+18	-0.4341***	-0.3545***	-0.3667***	-0.4157***
	(0.0027)	(0.0021)	(0.0021)	(0.0022)
+24	-0.4260***	-0.3593***	-0.3667***	-0.4101***
	(0.0032)	(0.0023)	(0.0024)	(0.0025)
+36	-0.4310***	-0.3723***	-0.3745***	-0.4159***
	(0.0043)	(0.0027)	(0.0028)	(0.0030)
+48	-0.4408***	-0.3818***	-0.3831***	-0.4224***
	(0.0054)	(0.0030)	(0.0033)	(0.0035)
+60	-0.4568***	-0.3859***	-0.3887***	-0.4330***
	(0.0065)	(0.0034)	(0.0038)	(0.0041)
Observations	12,981,924	12,885,588	12,972,312	12,895,200
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B Appendix

Table B.1: Adopters of EC Program

Total	**19,640**	**Fraction joing EC Program**
2010	10,946	55.73%
2011	4,723	24.05%
2012	1,049	5.34%
2013	790	4.02%
2014	761	3.87%
2015	716	3.65%
2016	655	3.34%

Table B.2: Matching RAIS with data from *Receita Federal*

Year	Found in...	contract (cnpj-pis)	cnpj	pis
	both	1,459,853	8,935	1,415,329
2009	only in RAIS	59,667,043	3,176,612	49,132,449
	only in EC	-	10,705	-
	both	1,615,956	9,163	1,563,956
2010	only in RAIS	65,131,346	3,349,973	52,623,273
	only in EC	-	10,477	-
	both	1,706,109	9,212	1,659,524
2011	only in RAIS	69,265,016	3,531,988	55,418,060
	only in EC	-	10,428	-
	both	1,757,108	9,152	1,710,484
2012	only in RAIS	71,569,377	3,636,253	57,469,739
	only in EC	-	10,488	-
	both	1,768,313	8,895	1,723,976
2013	only in RAIS	73,632,197	3,776,947	59,168,511
	only in EC	-	10,745	-
	both	1,742,306	8,663	1,689,827
2014	only in RAIS	74,364,973	3,886,379	60,214,766
	only in EC	-	10,977	-
	both	1,625,675	8,374	1,586,411
2015	only in RAIS	70,549,427	3,908,794	58,598,179
	only in EC	-	11,266	-
	both	1,493,470	7,957	1,461,183
2016	only in RAIS	65,651,128	3,864,927	55,707,531
	only in EC	-	11,683	-

Table B.3: Sequence of applied filters to the original datasets

Year	**Filter 1: women**			**Filter 2: age 25-44**			**Filter 3: private sector**			**Filter 4: only one ML**			**Filter 5: higher wage**		
	cnpj-pis	**cnpj**	**pis**	**cnpj-pis**	**cnpj**	**pis**	**cnpj-pis**	**cnpj**	**pis**	**cnpj-pis**	**cnpj**	**pis**	**cnpj-pis**	**cnpj**	**pis**
2009	468,026	7,653	455,235	302,805	6,948	293,472	260,139	6,506	253,020	259,651	6,506	252,535	252,535	6,497	252,535
2010	535,205	7,869	520,694	342,159	7,162	332,095	298,036	6,725	289,933	296,286	6,716	288,212	288,212	6,703	288,212
2011	577,567	7,959	561,411	372,295	7,271	360,872	327,806	6,828	318,434	325,080	6,824	315,748	315,748	6,811	315,748
2012	611,875	7,940	596,132	392,248	7,288	381,467	346,868	6,836	338,233	343,273	6,831	334,687	334,687	6,818	334,687
2013	621,901	7,769	607,001	399,151	7,139	388,927	342,663	6,691	335,038	338,541	6,687	330,962	330,962	6,678	330,962
2014	620,925	7,525	603,064	401,906	6,909	389,466	358,533	6,480	348,286	354,064	6,475	343,877	343,877	6,471	343,877
2015	593,725	7,292	579,396	386,152	6,692	376,236	341,990	6,256	334,769	335,902	6,253	328,746	328,746	6,252	328,746
2016	554,418	6,952	543,333	362,832	6,361	355,173	322,132	5,930	316,582	315,740	5,919	310,272	310,272	5,910	310,272

Table B.4: Separating ML taken within the year from those at the start/end of the year

Year	cnpj	pis	Took ML in...	cnpj	pis
			01/jan	997	2,739
2009	2,102	11,893	within	1,510	6,180
			31/dez	1,057	2,986
			01/jan	1,022	2,833
2010	2,464	12,462	within	1,766	6,186
			31/dez	1,205	3,452
			01/jan	1,136	3,161
2011	2,526	13,153	within	1,690	6,238
			31/dez	1,289	3,761
			01/jan	1,230	3,389
2012	2,546	13,807	within	1,661	6,101
			31/dez	1,336	4,319
			01/jan	1,264	3,658
2013	2,637	13,977	within	1,644	5,962
			31/dez	1,435	4,362
			01/jan	1,339	4,149
2014	2,619	15,633	within	1,622	6,665
			31/dez	1,446	4,823
			01/jan	1,357	4,226
2015	2,665	15,281	within	1,665	6,082
			31/dez	1,451	4,975
			01/jan	1,364	4,378
2016	2,495	14,074	within	1,498	5,563
			31/dez	1,269	4,140

Table B.5: Looking at the period of maternity leave for women who are found in two consecutive RAIS

Year	took ML in...	cnpj	pis	found only in one year	found in both	size "Ok"	Fraction Ok
2009	01/jan	997	2,739				
	31/dez	1,057	2,986	535	2,451	2,401	97.96%
2010	01/jan	1,022	2,833	382			
	31/dez	1,205	3,452	925	2,527	2,466	97.59%
2011	01/jan	1,136	3,161	634			
	31/dez	1,289	3,761	1,171	2,590	2,521	97.34%
2012	01/jan	1,230	3,389	799			
	31/dez	1,336	4,319	1,616	2,703	2,634	97.45%
2013	01/jan	1,264	3,658	955			
	31/dez	1,435	4,362	1,409	2,953	2,885	97.70%
2014	01/jan	1,339	4,149	1,196			
	31/dez	1,446	4,823	1,858	2,965	2,901	97.84%
2015	01/jan	1,357	4,226	1,261			
	31/dez	1,451	4,975	2,006	2,969	2,907	97.91%
2016	01/jan	1,364	4,378	1,409			
	31/dez	1,269	4,140				