Youth Responses to Cash Transfers: Evidence from Brazil

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PRELIMINARY AND INCOMPLETE DRAFT.
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March 5, 2017

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 any evidence of significant effects of additional exposure to the program on educational attainment or an increase in economic self-sufficiency. We alternatively find evidence of behavioral responses in formal labor supply. We find a 2.1–4.4 percentage points decrease in the probability of work in the formal sector. However, this negative effect tends to fade away over time. Overall, our findings support the skepticism about the effectiveness of interventions for disadvantaged youth.

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 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 (Kaufmann et al. (2012), Brollo et al. (2015)). In 2015, about

[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.

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 three 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 sharp discontinuities in observable characteristics around the thresholds. We restrict our analysis to specific, but still representative, states 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

[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.

[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 states in which the birthday cutoff date to start school is not December 31^{st}.

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 2012 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 discourages recipients from school engagement. In particular, we evaluate how this extra exposure affects lower secondary education completion and high school graduation until two years after the birthday cutoffs. We extend our analysis to college attendance. Preliminary results suggest insignificant, albeit positive, impacts on educational attainment.[8] Furthermore, we do not find any evidence of anticipation effect, in the sense that recipients born before the cutoff dates could anticipate their exclusion from the program in December, and drop out of school already by mid-school year.

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 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 an 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 years in the formal labor market. Nonetheless, this effect is not persistent. We indeed find that beneficiaries are induced to not work in the formal sector only when those born after the birthday cutoffs are eligible to receive cash transfers. We show that beneficiaries born after the cutoff birthdays are less likely to be employed in the formal labor market by about 2.1 – 4.4 percentage points (p.p.) in comparison to those born immediately before these cutoff dates only in the first year after the exclusion of the program. Over time, when all recipients became ineligible, this negative difference tends to fade away. It is

[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.

[8] These findings are also robust to alternative data.

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 effect on the probability of relying on the program support in later years for all cohorts of interest. 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)). Therefore, 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.

Related Literature:

A large literature has studied the effects of social welfare programs on economic outcomes[9], 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)), our results stand in contrast to these findings. Nonetheless, 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.[10] We contribute to this literature by presenting negligible effects on different economic outcomes when we consider a marginal exposure to the cash transfer program.[11] Our findings also

[9]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.

[10]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.

[11]For more references on the expansion of the program in Brazil, see Reynolds (2015) and Chitolina et al. (2016).

underscore the importance of producing a cost-benefit analysis of targeting.

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 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 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.

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.

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.

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.[12]

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.[13]

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%.[14] 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.[15]

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.[16]

When directly asked about their main reasons for dropping out of school[17], approximately one-

[12]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).

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

[14]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.

[15]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.

[16]Source: World Bank.

[17]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.

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 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.[18]

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[19], the federal government created the *Bolsa Família* Program (henceforth "BFP") to consolidate four existing cash transfer programs[20] 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 to break the persistence of poverty across generations; and (3) coordinate supplementary services to empower poor families to overcome poverty

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

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

[20] 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.

and social vulnerability.

Registering in the *Cadastro Único* is necessary to qualify for the benefits.[21] The registration process is completely decentralized. While the federal government establishes the number of poor families to survey and register in the system[22], 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.[23] 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[24] 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 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.

[21] *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.

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

[23] 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.

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

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)[25], created by the federal government in March 2008.

The positive impact on primary education[26], 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%.[27] 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 five variable benefits and two BVJ benefits.

2.3 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 academic year in the year when the recipient turns 18 years old. Thus, the exclusion process does not occur immediately after the birthday. Instead, the benefit is only canceled by the end of the school year if the participant is regularly enrolled in school. 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

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

[26] 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)).

[27] 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.

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 three cohorts of interest over time by recovering their educational and employment records in the formal labor market between 2011 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 2012 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.[28] Our analysis employs yearly data from 2011 to 2014.[29] 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.

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

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

All schools are required to update students' enrollment status[30] and grade level.[31] This requirement allows us to create a set of educational outcomes, which we define as follows. The first outcome is an indicator variable for whether the student has completed lower secondary education. The second variable of interest refers to high school graduation, which occurs if the student has completed upper secondary education.

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 2012 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.[32]

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 2011 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.[33] 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 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.[34]

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[30] 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.

[31] 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.

[32] 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.

[33] *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.

[34] 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.

3.2 Initial Sample Selection

We take a number of steps to construct our sample of interest. As discussed in Section 2.3, this paper takes advantage of discontinuities generated by the program exclusion. In particular, 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 2012. It comprises individuals who were born between November 1, 1994 and February 28, 1995 and received the BVJ benefit in December 2012. As explained in Section 2.3, those who were born in 1995 could receive the variable benefit from January to December of 2013, but those who were born in 1994 became ineligible to receive this benefit over the same period. We refer this sample as Cohort 1 with 397.927 observations. Similarly, from the payroll data of December 2013 (December 2014), which consist of individuals born between November 1, 1995 and February 29, 1996 (November 1, 1996 and February 28, 1997), we construct the sample of those who received the BVJ benefit in December 2013 (December 2014). This sample is referred as Cohort 2 (Cohort 3) and initially has 380.768 observations (388.693 observations).

Overall, our initial sample analysis consists of three cohorts. Table 1, Panel A, reports descriptive statistics for each cohort separately. On average, recipients also receive the basic benefit (indicating that they belong to "extremely poor" families), reside in urban areas, are black and were registered in the *Cadastro Único* database in 2009. The average self-reported *per capita* income ranges from 68 BRL (21 USD) to 78 BRL (24 USD), reinforcing their vulnerable situation. We do not find any evidence of systematic differences across all cohorts.

4 Empirical Strategy

4.1 Research Design

In this paper, we study the short-term 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 three cohorts of interest. We run separate regressions and report results for each cohort. Our baseline model is described by the following regression:

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

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; $1[a_i > \text{c}]$ is a dummy variable that takes value one if the individual is born after the birthday cutoff of reference; $\text{f}(a_i > \text{c})$ 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. Because our preferred specification is somewhat arbitrary, we check the sensitivity of our results by exploiting alternative kernels, bandwidths, and polynomial distance functions. They remain robust to alternative models.

Table 1, Panel B, reports descriptive statistics for each cohort using a 30 days window. From Columns (1)–(3), we note that Panel B is similar to Panel A, reinforcing our interpretation that the restricted sample is virtually identical to the full sample in all possible observable characteristics.

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

[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.

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.

When possible, we display graphically all 36 point estimates[36], 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 for each cohort. In Figures 1–3, 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 65% 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. In many schools, the cutoff date for compulsory enrollment is December 31^{st}, which can be a serious confounding factor to our quasi-experimental design. In this case, one might argue that any positive effect on educational attainment is a result of people born in January starting school later than people born in December, instead of being the actual impact of an additional exposure to BFP. In Brazil, states are granted autonomy to establish the birthday cutoff dates for school enrollment. Therefore, we restrict the sample to individuals born in six Brazilian states[37] and the Federal District, where the birthday cutoff dates to start school are **not** December 31^{st}. By restricting the sample, we address any concern related to cutoff dates for compulsory schooling.

Table 1, Panel C, shows descriptive statistics for each cohort after restricting the sample to the states of interest using a 30 days window. We refer the sample matched to the School Census and restricted to the states of interest and 30 days window as the ***final matched sample***. We note that our matched sample remains similar to the full sample in many observable characteristics.

To reinforce that our results are not driven by school starting age, we provide evidence that recipients who were born slightly before and after December 31^{st} are similar in various educational outcomes in the baseline year.[38] We consider the following educational variables: an indicator vari-

[36]Unfortunately, we do not have payroll data from December 2015 onward. Thus, we plot less point estimates for Cohort 3.

[37]We restrict to the following states: Acre, Alagoas, Amazonas, Rio Grande do Norte, Rio Grande do Sul, and Rio de Janeiro.

[38]Due to timing differences in data collection (to be described in details in Section 5.1), we consider the year before

able for whether the recipient has completed lower secondary education, an indicator for high school graduation, and an indicator for whether the individual is enrolled in higher education institution. Table 2 supports the validity of our research design.[39] In this sense, any effect on educational outcomes can be exclusively attributed to an additional exposure to BFP.

In addition, in order to confirm that the first assumption is still valid after sample restrictions, 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, indicator for whether the recipient resides in urban area, year of registration in the *Cadastro Único* database, indicator of child labor participation, *per capita* family income, presence of piped water and electricity in the residence, and total number of people and rooms in the residence. We also take into consideration the following household characteristics: gender, race, year of birth, schooling, and and some labor market outcomes of the household head. The balance tests are conducted by estimating Equation (1) with the ***final matched sample***, separately by cohort. Tables 3 and 4 suggest that all estimates are statistically insignificant and close to zero.[40] There are few exceptions, but they are statistically and economically negligible in magnitude.

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, we plot a histogram of birthdays relative to the threshold dates for each cohort, using the ***final matched sample***, separately by cohort. Figures 4–6 depict these histograms. 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 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 Figures 7–9, we do not find any statistically significant difference of density in each side of all thresholds (point estimates (standard error): 0.357 (1.026)). We note that birthday densities are smooth across the cutoffs for each cohort. We interpret these figures as evidences that assignment to the treated group is as good as random *near* the cutoffs.

the 18^{th} birthday as the baseline year for educational outcomes. More precisely, the baseline years for educational outcomes are 2011, 2012 and 2013 for Cohorts 1, 2 and 3, respectively.

[39] We also use the educational attainment reported to RAIS to confirm the robustness of these results.

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

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

We investigate whether an extra exposure to the welfare program reflects in higher educational attainment of the studied cohorts. We particularly focus on three outcomes drawn from both the School and Higher Education Censuses: lower secondary education completion, upper secondary education completion, and college enrollment.

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. After combining students' situation and grade, we can identify whether each student has completed lower and upper secondary education in years t-1, t, t+1, and t+2.

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 "year t" onward. As previously shown in Table 2, we find no evidence that students born immediately before and after the birthday cutoffs are different in several educational variables in the baseline year ("year t-1"), which further supports the validity of our research design.[41]

In years t, t+1, and t+2, even though we find that educational attainment is higher when compared to year t-1, we do not find any evidence of a significant increase in educational attainment because of one additional year of exposure to the program. Table 5 reports the results for year t, while Panels A to C refer to Cohorts 1 to 3, respectively. In Column (1), we present the effects of extra exposure on the probability of enrolling in college education. Although the coefficient for Cohort 2 is negative and statistically significant at 10 percent level, it is economically quite negligible. For other cohorts, we do not find any significant impact on lower secondary completion. In Column (2), the outcome of interest is the probability of completing upper secondary education. The

[41]In addition, we look at enrollment and we do not find any evidence of anticipation effect for the final matched sample, in the sense that recipients born before the cutoff dates anticipate their exclusion from the program in December, and drop out of school already by mid-school year.

coefficients are positive, but statistically insignificant. Finally, Column (3) refers to the probability of finishing lower secondary education. We again note that the coefficients are positive, but not statistically significant at conventional levels of significance.

We also document the findings for both years t+1 and t+2 in Tables 6 and 7, respectively. The dependent variables are exactly the same as in Table 5. Table 6 reports the results for Cohorts 1 and 2. Cohort 3 is still very young to be tracked in year t+1 because 2014 Census is the last available year. For the same reason, Table 7 presents the estimates for Cohort 1 only. In general, 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.

The absence of evidence that additional exposure to BFP leads to higher educational attainment is further supplemented with 2011–2015 RAIS data. For robustness purposes, we estimate Equation (1) for different combinations of educational attainment levels of workers reported by firms. We indeed find no evidence of higher educational attainment brought by the cash transfer program.[42]

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.[43]

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.

[42] These results are available upon request.

[43] 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.

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 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. Columns (1) and (2) of Table 8 documents the estimates for Equation (1) focusing on labor market outcomes in the baseline year (which we refer as the "year t"). In Column (1), the outcome of interest (variable ***employed***) 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. Column (2) 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 our ***final matched sample*** in the baseline year.

Furthermore, we conduct a placebo test to evaluate whether our formal labor market data suggest another discontinuity in our birthday cutoffs for treatment assignment. To that end, we consider all individuals born between December 1994 and January 1995, December 1995 and January 1996, and December 1996 and January 1997 found in RAIS, regardless of receiving benefits from BFP or not. We do not find any evidence of discontinuity for different labor market variables.[44] Because we do not find any evidence of an increase in educational attainment in the previous section, any effect on labor market outcomes can be uniquely interpreted as the impact of an additional year of exposure to BFP.

We now turn to the estimated impacts in labor market outcomes for subsequent years. Columns (3) and (4) refer 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. Similarly, Columns (5) and (6) document the findings in year "t+2", whereas Columns (7) and (8) report regression results for year "t+3". For all cohorts, Table 8 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 2.1–4.4 percentage points in comparison to those born immediately before these cutoff dates only in the first year after the exclusion of the program (at 5 percent level). However, two and three years later, this negative difference tends to diminish

[44] For brevity, we omit these results, which are available upon request.

and eventually fades away.

Given that transfer programs often require recipients to not be employed in the formal sector, we interpret the results in Table 8 as evidence that these programs indeed affect employment choices by inducing some beneficiaries to not work in the formal sector while they are eligible to receive the benefit.

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[45] or for being "extremely poor".

In this exercise, we restrict the sample to female beneficiaries because 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 children enrolled in the program in later years. For example, for all individuals born between December 1994 and January 1995 (that is, Cohort 1), we verify whether they receive any benefit of the program — from 2013 onwards — as a household head (not as a dependent). Because the payroll data identify all recipients as responsible or dependent, we are able to evaluate their dependence on BFP support over time.

We estimate Equation (1) for separate matched cohorts. Table 9 depicts the results by year for a time horizon of three years later. The first column refers to one year after the birthday cutoff, while the last column indicates the estimates for three years after the same birthday cutoff. Panel A refers to Cohort 1, while Panels B and C are related to Cohorts 2 and 3, respectively. In general, we consistently do not find any effect on the probability of relying on the program support in later

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

years.

6 Conclusion

In this paper, we provide an empirical evidence of the relationship between an additional exposure to a welfare program for disadvantaged youth and their educational, labor market 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 educationfor 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 compare three 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 lower and upper secondary completion or in college enrollment.

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, when only beneficiaries born in January are eligible to receive cash payments. Over time, this difference in labor market outcomes tends to fade away. Finally, we find no evidence that a further exposure affects participation in the program for the first years. In the next draft, 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 very costly and generate negligible benefits that 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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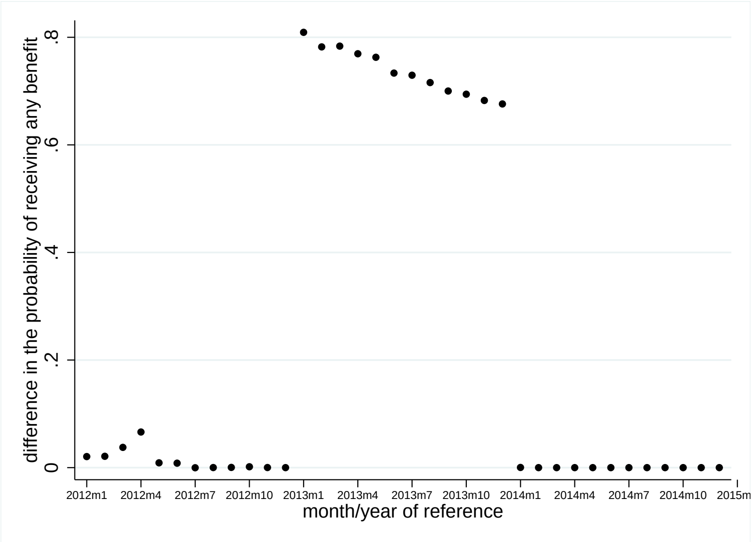
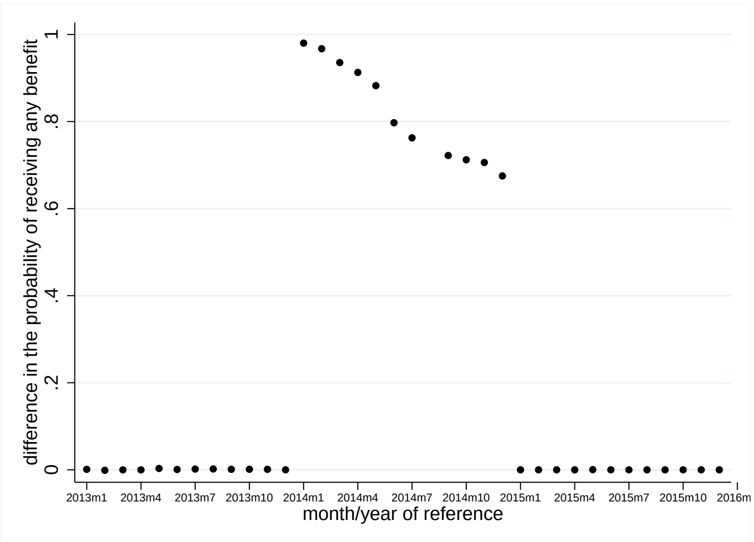
Figure 1: Difference in the Probability of Participating in the BF Program (Cohort 1)



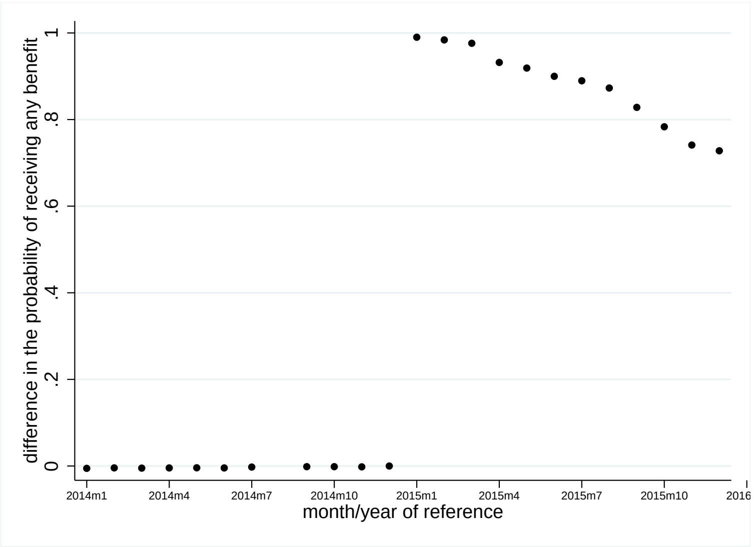
Note: This figure plots the difference in the probability of receiving the variable benefit for individuals born before and after December 31, 1994. To do so, we estimate Equation (1) using triangular kernel and a window of 30 days below and above the threshold. Each point denotes the estimated coefficient for each month in a period of 36 months (jan/2012 - dec/2014). The figure reveals that those who were born after the birthday cutoff have high propensity to receive one additional year of the BVJ stipend.

Figure 2: Difference in the Probability of Participating in the BF Program (Cohort 2)



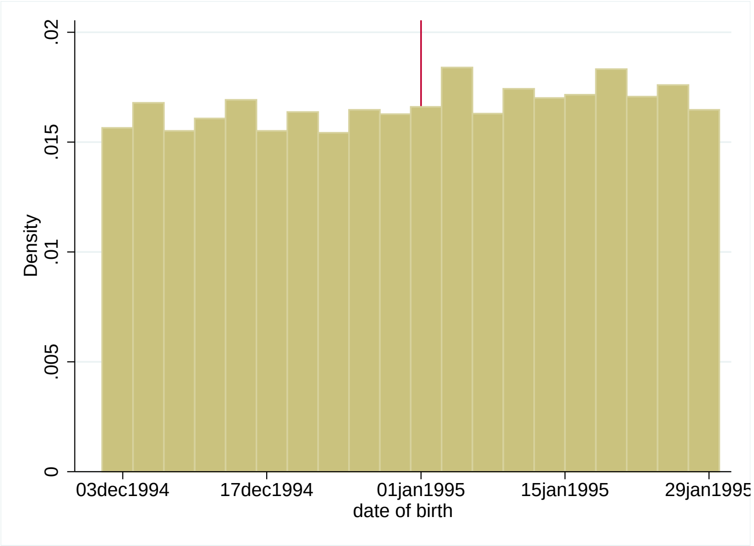
Note: This figure plots the difference in the probability of receiving the BVJ benefit for individuals born before and after December 31, 1995. To do so, we estimate Equation (1) using triangular kernel and a window of 30 days below and above the threshold. Each point denotes the estimated coefficient for each month in a period of 36 months (jan/2013 - dec/2015). The figure reveals that those who were born after the birthday cutoff have high propensity to receive one additional year of the BVJ stipend.

Figure 3: Difference in the Probability of Participating in the BF Program (Cohort 3)



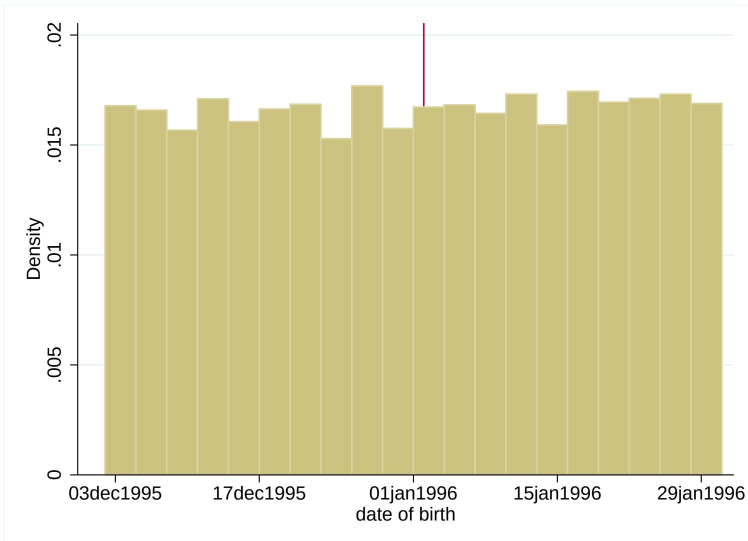
Note: This figure plots the difference in the probability of receiving the BVJ benefit for individuals born before and after December 31, 1996. To do so, we estimate Equation (1) using triangular kernel and a window of 30 days below and above the threshold. Each point denotes the estimated coefficient for each month in a period of 24 months (jan/2014 - dec/2015). The figure reveals that those who were born after the birthday cutoff have high propensity to receive one additional year of the BVJ stipend.

Figure 4: Density of Birthday Distribution: Cohort 1



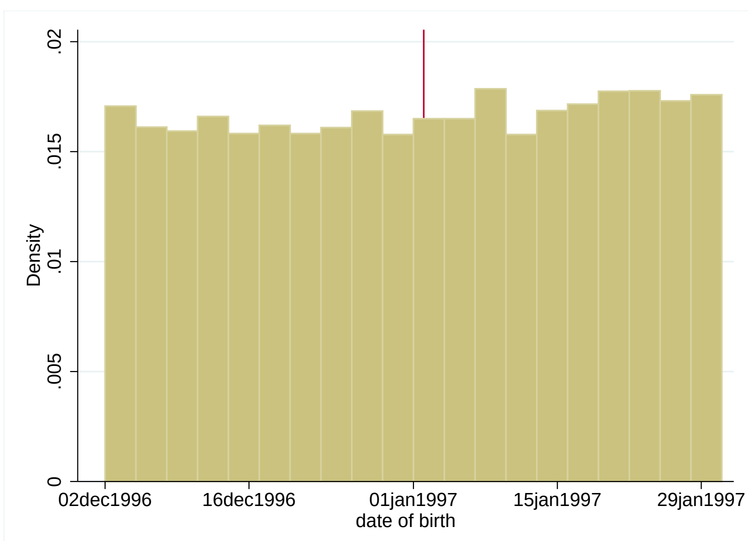
Note: This figure shows density of birthday distribution around the birthday cutoff (December 31, 1994) for Cohort 1. We consider a window of 30 days below and above the birthday cutoffs using the final matched sample. Bins have width of three points.

Figure 5: Density of Birthday Distribution: Cohort 2



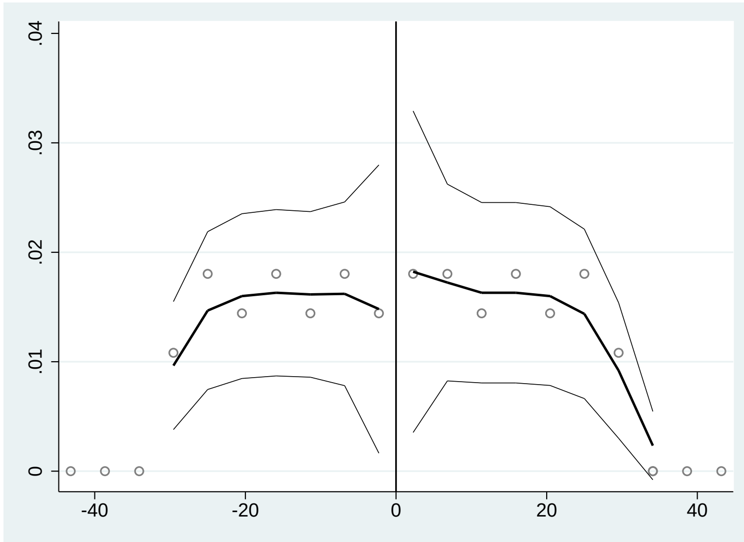
Note: This figure shows density of birthday distribution around the birthday cutoff (December 31, 1995) for Cohort 2. We consider a window of 30 days below and above the birthday cutoffs using the final matched sample. Bins have width of three points.

Figure 6: Density of Birthday Distribution: Cohort 3



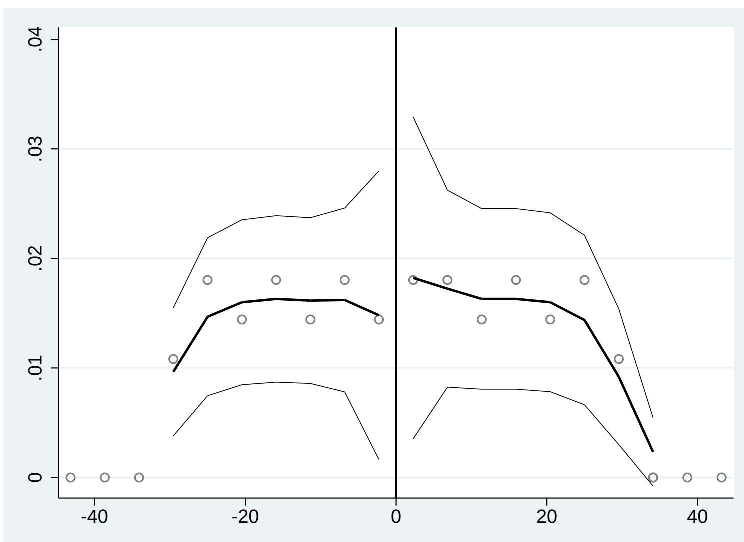
Note: This figure shows density of birthday distribution around the birthday cutoff (December 31, 1996) for Cohort 3. We consider a window of 30 days below and above the birthday cutoffs using the final matched sample. Bins have width of three points.

Figure 7: McCrary Density Test: Cohort 1



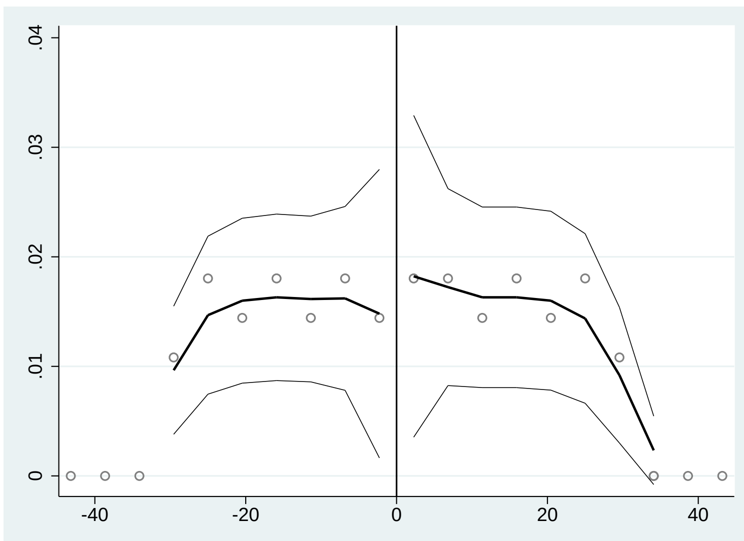
Note: This figure illustrates the density test proposed by McCrary (2008) for Cohort 1. In this distribution, 0 corresponds to the eligibility cutoff birthday (December 31, 1994) to receive treatment. We plot the density of observations by birthdays relative to the threshold. Optimal binsizes and bandwidths are computed as in McCrary (2008). Point estimate (standard error): - 0.357 (1.026).

Figure 8: McCrary Density Test: Cohort 2



Note: This figure illustrates the density test proposed by McCrary (2008) for Cohort 2. In this distribution, 0 corresponds to the eligibility cutoff birthday (December 31, 1995) 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): - 0.357 (1.026).

Figure 9: McCrary Density Test: Cohort 3



Note: This figure illustrates the density test proposed by McCrary (2008) for Cohort 3. In this distribution, 0 corresponds to the eligibility cutoff birthday (December 31, 1996) 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): - 0.357 (1.026).

Table 1: Descriptive Statistics

Variables	Cohort 1	Cohort 2	Cohort 3
A. FULL SAMPLE			
% receive basic benefit	0.90 (0.30)	0.90 (0.30)	0.92 (0.28)
# benefits by family	2.60 (1.36)	2.64 (1.39)	2.57 (1.31)
registration year	2009.05 (1.37)	2009.15 (1.42)	2009.23 (1.57)
% female	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)
% black	0.76 (0.43)	0.76 (0.43)	0.76 (0.43)
% living in urban areas	0.70 (0.46)	0.70 (0.46)	0.70 (0.46)
per capita income	77.76 (86.14)	68.27 (74.68)	68.82 (70.12)
% child labor	0.03 (0.18)	0.04 (0.19)	0.04 (0.19)
% piped water	0.74 (0.44)	0.74 (0.44)	0.74 (0.44)
% electricity	0.93 (0.26)	0.93 (0.26)	0.93 (0.26)
# people	4.29 (1.70)	4.44 (1.69)	4.58 (1.64)
# rooms	4.61 (1.39)	4.57 (1.39)	4.56 (1.39)
Observations	397.927	380.768	388.693
B. 30-DAYS WINDOW			
% receive basic benefit	0.90 (0.30)	0.90 (0.30)	0.91 (0.28)
# benefits by family	2.60 (1.36)	2.64 (1.39)	2.57 (1.32)
registration year	2009.05 (1.36)	2009.14 (1.43)	2009.23 (1.58)
% female	0.48 (0.50)	0.48 (0.50)	0.47 (0.50)
% black	0.75 (0.43)	0.76 (0.43)	0.76 (0.43)
% living in urban areas	0.70 (0.46)	0.70 (0.46)	0.70 (0.46)
per capita income	77.45 (86.05)	68.05 (74.64)	68.82 (70.09)
% child labor	0.03 (0.18)	0.04 (0.19)	0.04 (0.19)
% piped water	0.73 (0.44)	0.73 (0.44)	0.74 (0.44)
% electricity	0.93 (0.26)	0.93 (0.26)	0.93 (0.26)
# people	4.30 (1.70)	4.45 (1.69)	4.58 (1.64)
# rooms	4.62 (1.40)	4.57 (1.39)	4.56 (1.38)
Observations	203.144	191.697	195.643
C. MATCHED SAMPLE			
% receive basic benefit	0.90 (0.31)	0.89 (0.31)	0.90 (0.30)
# benefits by family	2.65 (1.35)	2.67 (1.39)	2.61 (1.33)
registration year	2009.05 (1.36)	2009.15 (1.46)	2009.25 (1.61)
% female	0.49 (0.50)	0.48 (0.50)	0.48 (0.50)
% black	0.67 (0.47)	0.69 (0.46)	0.70 (0.46)
% living in urban areas	0.79 (0.41)	0.79 (0.41)	0.79 (0.41)
per capita income	79.28 (84.25)	70.48 (73.76)	71.00 (68.47)
% child labor	0.02 (0.15)	0.03 (0.16)	0.03 (0.17)
% piped water	0.79 (0.40)	0.80 (0.40)	0.80 (0.40)
% electricity	0.94 (0.23)	0.94 (0.24)	0.94 (0.24)
# people	4.32 (1.68)	4.45 (1.68)	4.54 (1.66)
# rooms	4.42 (1.32)	4.40 (1.31)	4.38 (1.30)
Observations	27.708	27.132	30.105

Note: this table reports yearly descriptive statistics for each cohort. Table displays means and standard deviations in parenthesis. We include the following variables: an indicator of whether the family receives the basic benefit, total number of benefits received by the family, year in which the participant was registered in the *Cadastro Único* database, a dummy for female and black participants, a dummy for living in urban areas, *per capita* income, a dummy for child labor, an indicator of whether the participant lives in a residence with piped water and electricity, total number of people living in the residence, and total number of rooms in the residence. Panel A refers to the full sample. Panel B comprises the full sample using a 30 days window, while Panel C refers to the full sample matched to the School Census and restricted to the states of interest (Acre, Alagoas, Amazonas, Rio Grande do Norte, Rio Grande do Sul, and Rio de Janeiro, and the Federal District) and 30 days window. Sources: *Cadastro Único* database, payroll data, and the School Census.

Table 2: Balancing Test of Educational Outcomes

Panel A **Cohort 1**	(1) college educat.	(2) high school	(3) element. school
After	-0.000	-0.011	0.012
	(0.000)	(0.010)	(0.011)
Mean Dep. Var.	0.000	0.497	0.489
Observations	38,286	38,286	38,286
R-squared	0.000	0.001	0.001
Panel B **Cohort 2**	college educat.	high school	element. school
After	-0.000	-0.012	0.015
	(0.000)	(0.015)	(0.015)
Mean Dep. Var.	0.000	0.546	0.447
Observations	16,453	16,453	16,453
R-squared	0.000	0.000	0.001
Panel C **Cohort 3**	college educat.	high school	element. school
After	-0.000	-0.026	0.024
	(0.000)	(0.018)	(0.017)
Mean Dep. Var.	0.000	0.626	0.342
Observations	15,765	15,765	15,765
R-squared	0.000	0.001	0.001

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of Equation (1). The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Each panel represents one cohort separately. 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-1, respectively. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: payroll data, and School and Higher Education Censuses.

Table 3: Balancing Test of Individual Characteristics

Cohort 1	(1) female	(2) black	(3) urban area	(4) registration year	(5) child labor	(6) per capita income	(7) piped water	(8) electricity	(9) total family members	(10) total rooms
After	-0.0198	0.0197**	-0.0089	0.1055	0.0015	1.8734	-0.0088	0.0047	0.0275	0.0159
	(0.0162)	(0.0095)	(0.0140)	(0.0750)	(0.0033)	(1.9049)	(0.0095)	(0.0060)	(0.0631)	(0.0389)
Mean Dep. Var.	0.4850	0.6632	0.7931	2,004	0.0220	79.7842	0.8030	0.9443	4.2611	4.4412
Observations	27,076	27,047	27,076	27,056	26,565	27,076	26,554	26,554	26,456	26,554
R-squared	0.0004	0.0004	0.0002	0.0010	0.0002	0.0006	0.0001	0.0001	0.0003	0.0002

Cohort 2	female	black	urban area	registration year	child labor	per capita income	piped water	electricity	total family members	total rooms
After	-0.0054	-0.0096	-0.0020	0.0899	-0.0051	3.0601*	0.0151	0.0149**	-0.0039	0.0278
	(0.0113)	(0.0105)	(0.0131)	(0.1111)	(0.0031)	(1.6167)	(0.0134)	(0.0070)	(0.0536)	(0.0396)
Mean Dep. Var.	0.4752	0.6942	0.7816	2,004	0.0306	68.0891	0.7816	0.9339	4.4375	4.4085
Observations	26,768	26,712	26,768	26,752	26,353	26,768	26,209	26,209	26,509	26,210
R-squared	0.0002	0.0002	0.0005	0.0005	0.0001	0.0008	0.0005	0.0012	0.0010	0.0002

Cohort 3	female	black	urban area	registration year	child labor	per capita income	piped water	electricity	total family members	total rooms
After	0.0200*	-0.0012	0.0027	0.0174	0.0021	-3.2923**	-0.0099	-0.0070	-0.0337	0.0352
	(0.0112)	(0.0172)	(0.0087)	(0.0641)	(0.0071)	(1.5251)	(0.0099)	(0.0070)	(0.0399)	(0.0371)
Mean Dep. Var.	0.4639	0.6938	0.7893	2,005	0.0285	72.6674	0.8101	0.9396	4.5540	4.3699
Observations	30,058	29,952	30,058	30,038	29,681	30,058	29,368	29,368	30,048	29,368
R-squared	0.0004	0.0000	0.0002	0.0001	0.0001	0.0004	0.0002	0.0003	0.0000	0.0001

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Discontinuity of individual characteristics at the birthday cutoffs, estimated by Equation (1). Panels A, B, and C refer to Cohorts 1, 2, and 3, respectively. 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 states of Acre, Alagoas, Amazonas, Rio Grande do Norte, Rio Grande do Sul, Rio de Janeiro, and the Federal District. The unit of observation is an individual. The dependent variables are dummy variables to female and black beneficiaries, an indicator of whether the beneficiary lives in an urban area, registration year in the *Cadastro Único* database, indicator for child labor, family *per capita* income, a dummy for the presence of piped water in the residence, an indicator for electricity in the residence, total family members, and total rooms in the residence. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: *Cadastro Único* database, payroll data, and the School Census.

Table 4: Balance Test of Household Characteristics

Cohort 1	(1) female	(2) black	(3) year of birth	(4) lower sec. school	(5) high school	(6) literate	(7) wage	(8) dummy if works
After	-0.0051	0.0093	0.0167	0.0121	-0.0151	-5.4848	-0.0084	0.0094
	(0.0042)	(0.0106)	(0.1283)	(0.0120)	(0.0114)	(7.9138)	(0.0117)	(0.0122)
Mean Dep. Var.	0.9564	0.6722	1,969	0.8087	0.1871	156.14	0.4294	0.8644
Observations	25,736	25,710	25,736	22,286	22,286	23,570	23,281	25,696
R-squared	0.0002	0.0003	0.0000	0.0002	0.0003	0.0004	0.0002	0.0001

Cohort 2	female	black	year of birth	lower sec. school	high school	literate	wage	dummy if works
After	-0.0026	-0.0194*	-0.3070	0.0066	-0.0045	4.2631	0.0080	0.0021
	(0.0067)	(0.0109)	(0.2087)	(0.0102)	(0.0102)	(5.4309)	(0.0123)	(0.0082)
Mean Dep. Var.	0.9510	0.7039	1,970	0.8038	0.1898	144.58	0.4236	0.8694
Observations	26,754	26,642	26,754	23,360	23,360	25,220	25,004	26,737
R-squared	0.0002	0.0004	0.0002	0.0002	0.0002	0.0001	0.0001	0.0003

Cohort 3	female	black	year of birth	lower sec. school	high school	literate	wage	dummy if works
After	0.0058	0.0035	-0.5683***	-0.0062	0.0048	-9.0111	-0.0092	0.0067
	(0.0060)	(0.0151)	(0.1889)	(0.0119)	(0.0114)	(5.6065)	(0.0130)	(0.0069)
Mean Dep. Var.	0.9395	0.7046	1,971	0.7894	0.2065	155.53	0.4354	0.8692
Observations	30,084	29,878	30,084	26,427	26,427	29,122	28,933	30,076
R-squared	0.0003	0.0000	0.0008	0.0001	0.0001	0.0007	0.0005	0.0001

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Discontinuity of household characteristics at the birthday cutoffs, estimated by Equation (1). Panels A, B, and C refer to Cohorts 1, 2, and 3, respectively. 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 states of Acre, Alagoas, Amazonas, Rio Grande do Norte, Rio Grande do Sul, Rio de Janeiro, and the Federal District. The unit of observation is the adult responsible for the child. The dependent variables are dummy variables to female and black adults, year of birth, an indicator of whether education is equal or lower than lower secondary school, an indicator of whether education is equal or lower than upper secondary school, a dummy for literacy, wage, and an indicator of whether the adult works. Robust standard errors clustered at

Table 5: Effects on Educational Outcomes in Year t

Panel A **Cohort 1**	(1) college educat.	(2) high school	(3) element. school
After	-0.003	0.016	0.013
	(0.002)	(0.012)	(0.012)
Mean Dep. Var.	0.008	0.569	0.382
Observations	38,286	38,286	38,286
R-squared	0.001	0.000	0.001
Panel B **Cohort 2**			
After	-0.007*	0.017	0.009
	(0.004)	(0.014)	(0.014)
Mean Dep. Var.	0.013	0.584	0.343
Observations	16,453	16,453	16,453
R-squared	0.002	0.000	0.001
Panel C **Cohort 3**			
After	0.001	0.010	0.017
	(0.004)	(0.018)	(0.016)
Mean Dep. Var.	0.008	0.663	0.262
Observations	15,765	15,765	15,765
R-squared	0.005	0.002	0.001

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of Equation (1). The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Each panel represents one cohort separately. 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, 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 in Year t+1

Panel A **Cohort 1**	(1) college educat.	(2) high school	(3) element. school
After	-0.003	0.015	0.017
	(0.004)	(0.009)	(0.010)
Mean Dep. Var.	0.036	0.471	0.321
Observations	38,286	38,286	38,286
R-squared	0.000	0.000	0.000
Panel B **Cohort 2**			
After	-0.010	0.008	0.004
	(0.007)	(0.017)	(0.013)
Mean Dep. Var.	0.058	0.468	0.291
Observations	16,453	16,453	16,453
R-squared	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 Equation (1). The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Panel A refers to Cohort 1, while Panel B comprises Cohort 2. 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+1, respectively. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: payroll data, and School and Higher Education Censuses.

Table 7: Effects on Educational Outcomes in Year t+2

Panel A **Cohort 1**	(1) college educat.	(2) high school	(3) element. school
After	-0.001	0.011	0.011
	(0.005)	(0.012)	(0.010)
Mean Dep. Var.	0.054	0.405	0.295
Observations	38,286	38,286	38,286
R-squared	0.003	0.000	0.000

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of Equation (1). The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Panel A refers to Cohort 1. 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+2, respectively. Robust standard errors clustered at birthday level are reported in parenthesis. Sources: payroll data, and School and Higher Education Censuses

Table 8: Effects on Labor Market Outcomes

Panel A **Cohort 1**	(1) employed year t	(2) wage year t	(3) employed year t+1	(4) wage year t+1	(5) employed year t+2	(6) wage year t+2	(7) employed year t+3	(8) wage year t+3
After	-0.0095	-0.0061	-0.0437***	-0.0769***	-0.0030	-0.0281	-0.0091	-0.0140
	(0.0062)	(0.0061)	(0.0119)	(0.0173)	(0.0139)	(0.0223)	(0.0146)	(0.0269)
Mean Dep. Var.	0.1122	0.1093	0.2942	0.3883	0.3759	0.5310	0.3764	0.5394
Observations	27,708	27,708	27,708	27,708	27,708	27,708	27,708	27,708
R-squared	0.0008	0.0010	0.0037	0.0025	0.0006	0.0009	0.0006	0.0005

Panel B **Cohort 2**	employed year t	wage year t	employed year t+1	wage year t+1	employed year t+2	wage year t+2
After	0.0036	0.0019	-0.0290**	-0.0339**	-0.0199*	-0.0299
	(0.0084)	(0.0108)	(0.0117)	(0.0142)	(0.0110)	(0.0201)
Mean Dep. Var.	0.1042	0.1070	0.2782	0.3472	0.3285	0.4357
Observations	27,132	27,132	27,132	27,132	27,132	27,132
R-squared	0.0002	0.0002	0.0016	0.0013	0.0007	0.0010

Panel C **Cohort 3**	employed year t	wage year t	employed year t+1	wage year t+1
After	-0.0064	0.0048	-0.0206**	-0.0194
	(0.0069)	(0.0080)	(0.0067)	(0.0120)
Mean Dep. Var.	0.0973	0.0926	0.1992	0.2455
Observations	30,105	30,105	30,105	30,105
R-squared	0.0003	0.0003	0.0013	0.0009

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of Equation (1) between years t and t+3. The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Each panel represents one cohort separately. Columns (1), (3), (5), and (7) report the effects on the likelihood of being employed in the formal labor market. Columns (2), (4), (6), and (8) document the impacts on annual earnings (in minimum wage) in the formal labor market. 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

Panel A Cohort 1	(1) year t+1	(2) year t+2	(3) year t+3
After	-0.0039	0.0003	0.0028
	(0.0052)	(0.0067)	(0.0103)
Mean Dep. Var.	0.0201	0.0497	0.0776
Observations	13,208	13,208	13,208
R-squared 0.0006	0.0003	0.0001	

Panel B Cohort 2	year t+1	year t+2
After	-0.0032	-0.0135**
	(0.0054)	(0.0057)
Mean Dep. Var.	0.0186	0.0559
Observations	12,802	12,802
R-squared	0.0007	0.0009

Panel C Cohort 3	year t+1
After	0.0024
	(0.0051)
Mean Dep. Var.	0.0199
Observations	14,402
R-squared	0.0002

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the estimates of Equation (1) between years t+1 and t+3. The sample consists of beneficiaries matched to the School Census and restricted to the states of interest (same as in Table 1) and 30 days window. Each panel represents one cohort separately. The dependent variable is an indicator of whether the individual relies on BFP support. Robust standard errors clustered at birthday level are reported in parenthesis.