Welfare Analysis of Transfer Programs with Jumps in Reported Income: Evidence from the Brazilian *Bolsa Família*[*]

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Abstract

Transfer programs based on income often generate non-convex kinks in budget sets, particularly in their phase-out regions. In such settings, optimizing agents may respond to changes in the schedule by "jumping" from one bracket of a tax and transfer schedule to another, a behavior that is ruled out by the widely used "first-order" approach in optimal tax theory. This paper presents evidence that such jumps are empirically important using administrative data on reported income that spans a reform of the Brazilian anti-poverty program *Bolsa Família*. I develop a theoretical framework that allows for such jumping behavior and show that an additional set of "jumper shares" coupled with standard parameters yield sufficient statistics for welfare analysis. Estimating these shares using the Brazilian data, I document that for every marginal *real* (R$) transferred by the reform, 12 cents were lost due to the efficiency costs of jumping behavior. Simulations suggest that "jumping" behavior substantially affects the welfare analysis of more general reforms.

JEL:H21, I38, J22

Keywords: Taxation Efficiency, Effects of Welfare Programs, Labor Supply

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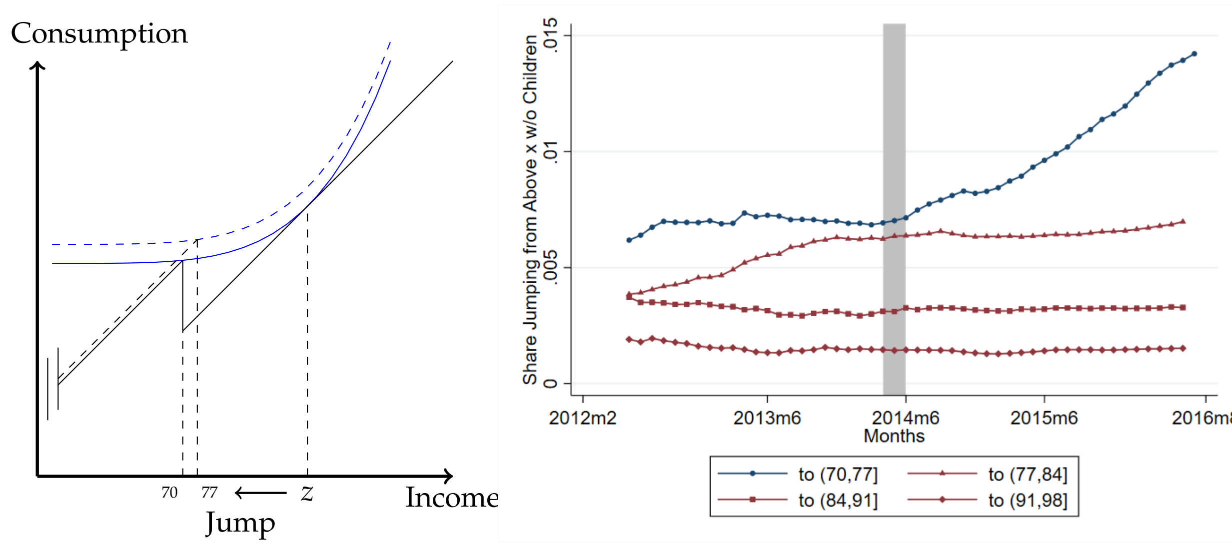
1 Introduction

Transfer programs based on income often generate non-convex kinks in budget sets – i.e., points at which marginal tax rates fall when income rises. For instance, for the Earned Income Tax Credit (EITC) in the US, marginal tax rates are higher in the phase-out region of the program than at higher income levels. In such settings, agents with neoclassical preferences can be indifferent between two tax/transfer brackets. These indifferent applicants could respond to small reforms of the schedule by "jumping" from one bracket to the other. Such behavior has not received attention from the most common approach in optimal tax theory: the reduced-form sufficient statistics approach (Diamond (1998) and Saez (2001)). This paper presents evidence of jumping in a large anti-poverty program and develops a theoretical framework that takes this behavior into account in the welfare analysis of transfer/tax reforms.

The empirical setting is the Brazilian cash transfer program *Bolsa Família* (BF), "the largest conditional cash transfer program in the developing world" (Lindert et al., 2007).[1] Household per capita income determines eligibility for the program. Around the threshold, the magnitude of the transfer depends only on household composition, i.e., the marginal transfer (along the income dimension) is zero above and below this end point. In Figure 1a, the solid black line represents the budget set faced by households without children around the limit of eligibility. In April 2014, the Brazilian government announced a reform that would increase both transfers and the eligibility criteria by 10%. The dashed black line in the same figure plots the budget set of the same households after this reform.

To understand the key idea of this article, note that households could be indifferent between joining the program or not. The solid indifference curve in Figure 1a represents the preferences of one of these applicants before the reform. This household breaks its indifference by choosing the income level above the eligibility threshold. The indifferent agent should respond to the infra-marginal BF reform by jumping to the new threshold (R$77),[2] as indicated in the figure. Note, however, that there is no change in the slope (marginal after-tax/transfer income) or intercept (virtual income) of the linearized schedule around its initial income level z. Therefore, the usual sufficient statistics in the first-order approach (income elasticity with respect to the marginal after-tax income and the virtual income) do not capture such behavior. Furthermore, this jump does not correspond to a participation (extensive margin) response — a behavior also addressed by previous extensions of the

[1] BF had more than 42 million beneficiaries as of March 2015.
[2] The Brazilian currency (*real* or plural *reais*) is denoted by R$.



(a) *Bolsa Família* Schedule

(b) Share of Jumpers up to Each Month

Figure 1: *Bolsa Família* Reform for Families without Children

Note: Panel (a) displays the BF reform effect on earnings choices of a noneligible applicant indifferent between being at income level z and at the eligibility threshold. Panel (b) depicts the cumulative shares of households above R$x that moved to the $(x-7, x]$ interval up to each month in time, out of all households initially above R$x and that updated up to the same month. The blue line with circles and the red lines with triangles, squares, and diamonds plot the shares for $x = 77, 84, 91$, and 98, respectively. The gray vertical bar indicates the months between the announcement and the enactment of the reform.

standard approach (see, for instance, Saez (2002), Jacquet et al. (2010), and Scheuer and Werning (2016)).

In this environment, I conduct three exercises. First, I present evidence of jumps in reported income as a response to the reform. Second, I develop a theoretical framework that accommodates this behavior and conduct a welfare analysis of the change in the schedule. Third, I illustrate the importance of jumps in simulations of different reforms.

The first part of the paper documents this jumping behavior using BF administrative data from December 2011 to September 2016. Figure 1b's line marked with circles plots the share of households that jumped from some income level above R$77 to the $(70, 77]$ interval among those that updated from above R$77 up to each month in time. This segment only became attractive after the reform, providing incentives for jumping. The gray area indicates the months between the announcement and enactment of the reform (June 2014). There is a sharp increase in the share of jumpers, which starts around these months and continues for the two following years. A counterfactual series is necessary to investigate whether the reform caused the increase in the share of jumpers. I plot three alternative trends in the same figure: symmetric shares of households jumping from above 84, 91, and 98 to the 7 *reais* interval right below these numbers. None of these intervals was affected by the reform. Under the identifying assumption that the trends in shares of jumpers to the affected and alternative regions would remain parallel after the reform, the increase in the first share corresponds to the causal effect of the reform on the share of jumpers. This evidence indicates that applicants to the BF program changed their reported income in response to the infra-marginal change in the schedule. This result is robust to alternative exercises that explores the different impacts of the reform on households with and without children, as well as alternative placebo intervals used as control groups. I find that 0.6% of households without children with income above R$77 jumped to the $(70, 77]$ interval.

The second part of the paper presents a theoretical framework that accommodates this jumping behavior. I show that the share (more precisely, density) of households jumping to the new threshold along the reported income dimension is the sufficient statistic for welfare analysis of the BF. The benchmark framework consists of a labor supply model for simplicity. I consider an economy in which agents are not only heterogeneous in ability as in Mirrlees (1971), but also in elasticity. In this setting, there are different types located at each income level, in contrast to the unidimensional case. Hence, for any small infra-marginal reform, such as the one discussed above, some agents located at each income level would jump while others would not. This replicates the pattern seen in the data. The share of jumpers captures the behavioral responses along this margin. The reform could also generate income effects on households that were below the threshold before the reform. However, these responses do not affect the government's budget, because the marginal transfer is zero below the threshold. Since the reform does not change the marginal transfer, there are no distortions in the intensive margin. Finally, the envelope theorem guarantees that the effect of the behavioral responses on the utility of the household is second order.

Note that these responses to the reform could come either from changes in misreporting or labor supply behavior. However, Feldstein's (1999) argument that the taxable income (analogous to reported income in the present setting) elasticity is the sufficient statistic for the welfare analysis extends naturally in the case of discrete jumps. To see this, note that the reform will affect welfare through the utility of applicants and the budget of the government. As mentioned above, the effect of the jumping responses on the first term is second order. Intuitively, every jumper (such as the one depicted in Figure 1a) is initially indifferent between their initial income level and the old threshold. For a marginal reform, the welfare gains are infinitesimal for these households, regardless of whether the jump is a result of labor supply or misreporting response. Since there is an infinitesimal number of jumpers, this effect is second order on welfare. On the other hand, the effect on the second term of the welfare (budget of the government) is first order, because the government pays an additional amount proportional to the entire transfer for each jumper. This effect is determined by the share of jumpers and does not depend on the nature of the reported income response in the absence of fiscal externalities of misreporting, i.e., as long as misreporting only affects the budget of the government through the taxable income.[3]

A feature of the data is that there are some agents in the dominated area. This is contrary to standard models of choice. To accommodate the data, one must therefore employ a model with some nonstandard features. My theory attributes the dominated choices to imperfect attention, allowing agents to differ also in attention types. A fraction of these inattentive households is located right above the eligibility threshold so that they mechanically become eligible with the reform. Even though the number of such applicants affected by a marginal reform is infinitesimal, each one of them increases their consumption by the amount of the entire transfer. Hence the effect of a change of the threshold on welfare is first order once I account for inattention. This effect is also empirically relevant.

The analysis indicates that for every marginal *real* transferred to the poor with the reform, 66 cents were given to inframarginal households that were eligible even before the reform; 22 cents were transferred to the inattentive households that mechanically became eligible for the increase in the threshold; and 12 cents were transferred to jumpers, thereby accounting for pure efficiency costs. All of this efficiency cost arises due to a jumping response, given that the reform does not alter marginal transfers.

Since the BF reform does not affect the incentives of applicants to respond locally in the intensive margin, one cannot quantify the importance of jumping behavior compared to the usual response in the data. To do so, the third part of the paper simulates an economy with parameters that match empirical estimates for the taxable income elasticity with respect to the marginal after-tax income from the literature. I consider a simple negative income tax (NIT) schedule, i.e., a transfer given to the unemployed phased out with a constant marginal tax rate. Notches are absent in these settings. I compute the efficiency costs of different reforms in the phase-out region. In these simulations, jumping effects accounts for 10% to 32% of the efficiency cost of the reforms.

Related Literature: This paper relates to the literature on the estimation of labor supply and taxable income elasticities. One approach to estimating these elasticities consists of

[3] See Chetty (2009), Piketty et al. (2014), and Huang and Rios (2016) for examples in which these fiscal externalities are important. In all these cases, the real income elasticities are also necessary for the welfare analysis and for the optimal policy.

specifying a structural model for the utility of agents (see, for instance, Hoynes (1993) and Friedberg (2000)). Another strategy exploits variations in tax/transfer schedules throughout time (e.g., Feldstein (1995) and Gruber and Saez (2002)) or within a cross-section (e.g., Saez (2010) and Kleven and Waseem (2013)) and measures the resulting response in taxable income in a reduced-form manner. Saez et al. (2012) provide a summary of this literature. While the structural approach has the advantage of allowing taxpayers to respond to the nonlinearities of the tax schedule, the strength of the reduced-form approach is to avoid imposing strong restrictions on preferences. In this paper, I propose a method to recover the relevant parameters for the welfare analysis, allowing agents to respond to infra-marginal changes in their budget sets within a reduced-form framework.

This paper also speaks to the literature on the empirical implementation of optimal income tax schedules. Diamond (1998) and Saez (2001) rewrite the Mirrlees (1971) formula for the welfare maximizing income tax schedule in terms of labor supply elasticities and moments of the income distribution. As pointed out by Dodds (2017), jumping behavior (which is the focus of this paper) also matters for optimal policy characterization if there is more than one dimension of heterogeneity in the economy. I rewrite the jumping effect that arises in his formula in terms of the share of jumpers, which is related to the parameters I estimate in this paper. A part of this literature addresses a particular type of jump: extensive margin responses. Saez (2002), Jacquet et al. (2010), and Scheuer and Werning (2016) show that if households are allowed to respond to changes in the tax schedule by entering or exiting the labor force, the labor participation elasticity with respect to the average tax rate is an additional sufficient statistic for the optimal tax. I show that the shares of jumpers (which coincides with the participation elasticity in the case of extensive margin jumps) are also in the characterization of the optimum when general jumps are allowed.

The remainder of the paper is organized as follows. Section 2 presents the context of the application and Section 3 the reduced-form evidence of jumping responses. I introduce the theoretical framework in Section 4 and discuss the estimation of the relevant parameters in Section 5. Section 6 contains the welfare analysis while Section 7 presents simulations of alternative transfer programs. Section 8 concludes. I leave all formal proofs, some additional counterfactual analyses and the optimal tax characterization to the Appendix.

2 The *Bolsa Família* Program

This section describes the context for the empirical application. Section 2.1 describes the *Bolsa Família* program. I then present the data sources in Section 2.2, the characteristics of the BF population in Section 2.3 and the reforms of the schedule which provide the identification in Section 2.4.

2.1 The *Bolsa Família* Program

The Brazilian anti-poverty *Bolsa Família* program was implemented by the Provisional Measure 132 in October 2003. It targets poor households on their per capita income reported to *Cadastro Único* agencies, which are the program offices spread across Brazil's 5,570 municipalities. The social development ministry (*Ministério do Desenvolvimento Social* or MDS) administers the program.

Applicants to the program report information to interviewers at program offices in any weekday. Beneficiaries are required to report their information once every two years in order to keep their benefits. This information includes their income, assets, and socioeconomic demographics. Interviewers input all the information to the *Cadastro Único* system.

Figure 2 shows the entries on the questionnaire used to calculate the per capita income. During the interview, the applicant reports the value for each of the seven income categories for each member in the household. The computer calculates the household per capita income in three steps. First, it gets the minimum between the average monthly income in the last 12 months and the last month income for each individual. Then, it sums this minimum with all other income categories to get the individual monthly income. Finally, it sums this individual monthly income across all members and divides it by the number of members of the household. Once this final per capita income is displayed on the interviewer's computer screen, the interviewer can no longer change the per capita income.

The government transfers the money to the potential beneficiary, as long as they fulfill three conditionalities: (1) children must maintain a minimum of 85% of school attendance between ages 6 and 15 and 75% between 16 and 17; (2) households must keep track of their children's vaccines and of nursing mother and prenatal visits to the doctor; (3) parents must maintain at least 85% of social-education attendance, if the household has violated child labor laws in the past. These conditionalities were held constant during the period

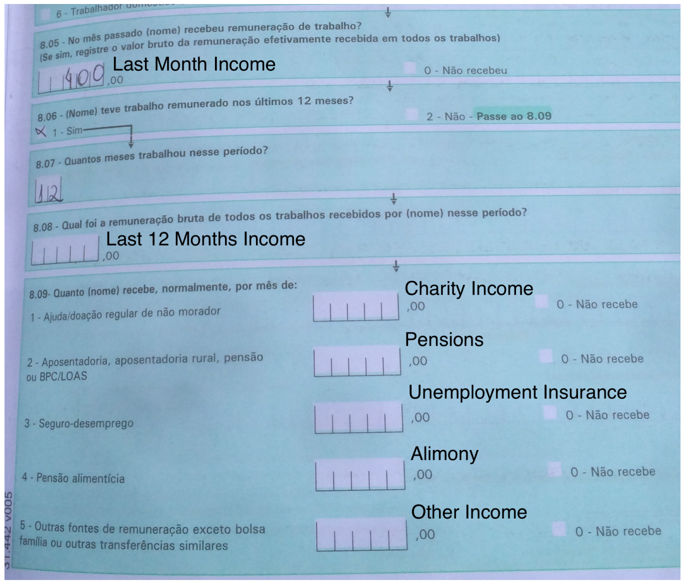


Figure 2: Income Report

Note: The figure depicts the income categories reported by applicants for each member of the household. Each category is translated to English in the picture. This is a print out of the screen seen by the interviewers in their computer when filling in the applicants' information.

of the analysis.

To enroll in the program, the head of an applicant household must present a government issued ID for himself and each member of the household. Therefore, registering nonexisting members is possible but unlikely.

The MDS has two main enforcing mechanisms to prevent income misreporting. First, the income questions come at the end of the questionnaire, so that the assets and social demographic questions help the interviewer assess the veracity of the income report. Second, the MDS conducts audits. Citizens' complaints and cross-checking of programs data with data on formal employment and the Brazilian social security system can generate these audits. In both cases, government employees may either visit families to update their information and/or require applicants to update their information in the office. The large informal sector in the Brazilian economy leaves scope for misreporting, which it could be an important aspect of responses to the schedule.

2.2 Data Sources

I have access to the *Cadastro Único* individual and household registry database, which define the eligibility of households for BF and other social security programs discussed in

Appendix A.1. the database contains each applicant's characteristics, such as age, gender, race, marital status, schooling, employment status, occupation, income, and disability status. It also has information at the household level, such as per capita expenditures, ownership of durable goods, and per capita income which determines the benefits to which each household is entitled.

Figure 3 presents the timeline of the program and of data extractions. Each extraction contains the information for the last update for each household up to the extraction date. The final data set is constructed by appending eight extractions of the program's administrative records: one in December of each year from 2011 until 2015, and in April and August 2015 and September 2016. For instance, if a household updated its information in August of 2011 and September 2013, its information will appear as August of 2011 in the 2011 and 2012 extractions and as of September 2013 in the 2013, 2014, 2015 and 2016 extractions. The reform, which provides the variation for the analysis, occurred in the middle of the period (June 2014). This is helpful for testing the identification as discussed in Section 3.3.

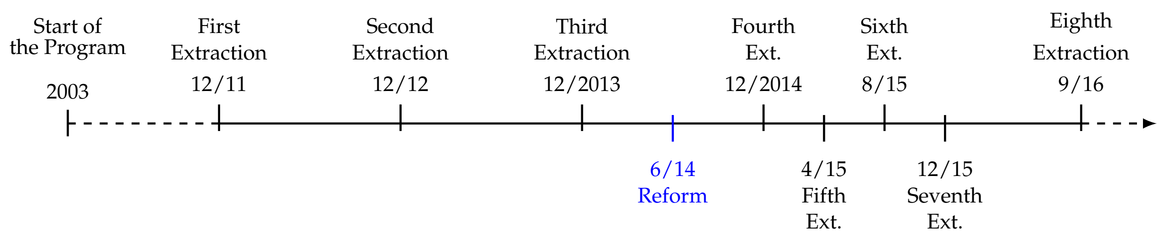


Figure 3: Timeline

Note: The figure describes the timeline of the program and the data. BF started in 2003 and the reform I studied occurred in June 2014. The data is constructed from 8 extractions from December 2011 until September of 2016. Each extraction contains the last information of each household up to the extraction date.

I also use municipal population data from the *Instituto Brasileiro de Geografia e Estatística* (IBGE) to compute the share of applicants per municipality.

2.3 Sample Description

This section describes *Bolsa Família* applicant characteristics. All results come from a 5% random sample of the data for computational speed. Table 1 displays summary statistics for all households in the sample.

The average per capita monthly income (R$ 234.12 or US$119.02)[4] is significantly larger

[4]All conversions were made using the power of purchase parity ratio of 1.967 for 2016, according to the OECD.

Table 1: Descriptive Statistics

Variables	Mean	Median
Per Capita Income	234.12	161.49
	(3463.32)	
Number of Members	2.94	3.00
	(1.44)	
Children up to 15 yo	1.15	1.00
	(1.11)	
Teenagers	0.20	0.00
	(0.34)	
Households	1,376,383	

Note: The descriptive statistics are calculated at the household level. I first calculate the average across updates for each household and then compute the mean and median in the 5% sample. The per capita income is inflated to June 2014 prices according to INPC.

than the median (R$ 161.49 or US$82.10) because of outliers. Applicant households have on average 2.94 members, 1.15 children 15 years old or younger and 0.20 teenagers. There are in total 81,404,307 applicants in 27,745,078 households. The 5% sample leaves me with 1,387,254 households or 4,038,784 applicants.

The northeast and north of Brazil are the country's poorest regions. This is reflected in the demand for BF, as shown in Figure 4. This figure displays the spatial variation in the share of the population that has applied to the program across the 5,570 Brazilian municipalities. Higher shares of applicants are represented by darker shades in the map. Each color corresponds to a decile of this share distribution. There is substantial variation in the map. As expected, the largest shares are concentrated in the poorest areas of the country.[5]

2.4 The Transfer Schedule and the June 2014 Reform

Since the beginning of the period of the analysis, the program defines two thresholds in the per capita monthly income distribution: the extreme poverty line (R$70) and the poverty line (R$140). Households with per capita income below the extreme poverty line are eligible for a constant basic benefit, a variable benefit proportional to the number of family members between 0 and 15 years of age, and a benefit proportional to the number of teenagers (individuals of 16 or 17 years of age). Households with per capita income between the extreme poverty and the poverty thresholds only get the variable and the

[5]In 30 municipalities, the share is larger than one for two reasons. First, the population is an estimate based on the 2010 census. Second, I consider any applicants in each municipality since the start of the program. Some of these applicants may have moved and no longer be part of the current municipal population.

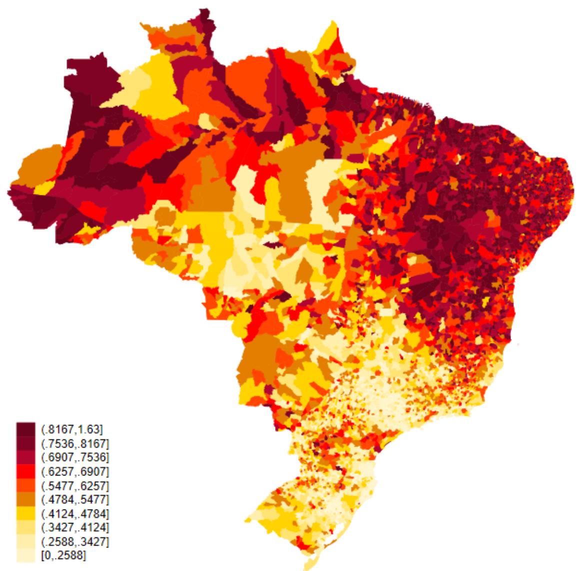


Figure 4: Density of Applicants per Municipality

This figure plots density of applicants by municipality in 2016. Applicants density is defined as the number of individuals in households that ever applied to BF divided by the 2016 municipal population. We divide the observations into deciles within the sample. Each decile is assigned a different color on the map, with darker shades representing higher densities.

teenager benefits. Households with per capita income above the second threshold are not eligible for any cash transfer.

In June of 2014, the government increased the extreme poverty line from 70 to 77 *reais* and the poverty line from 140 to 154 *reais*. The basic benefit was raised from 70 to 77, the benefit per child from 32 to 35, and the benefit per teenager from 38 to 42 *reais*. This reform was announced on national television by the president in April 2014, even though the thresholds were not mentioned.[6] Although transfers are heterogeneous according household composition, threshold values are the same. Table 2 summarizes these aspects of the schedule before (first column) and after (second) the reform. The last two rows display the average transfer for households without and with children.

In the period of the analysis, there were four other reforms: in June and November 2012, February 2013, and June 2016. Since these reforms are too close to the beginning of the data (January 2012) and its end (September 2016), I do not use them in the analysis. The first two reforms did not affect the threshold or the transfer around these thresholds, but the last reform did. Hence, I focus on the effects up to June 2016 in the empirical analysis. All of the other reforms are discussed in Appendix A.2.

Figure 5a plots the per capita income distribution as of April 2014 with the solid green

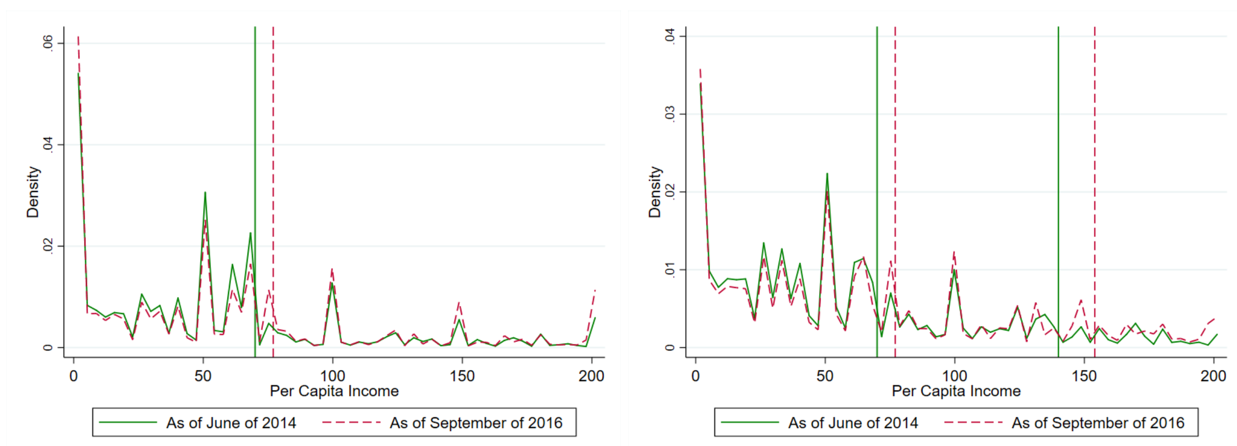
[6]The president stated only the program would be adjusted by 10%.

Table 2: Schedule Details

	Before	After
First Threshold	70	77
Second Threshold	140	154
Income Below 1st Threshold	70	77
Per Child 15 or younger (max 5)	32	35
Per Teen 16-18 (max 2)	38	42
Avg. Transfer in 1st Thr. (w/o Kids)	34.16	37.76
Avg. Transfer in 2nd Thr. (with Kids)	16.31	19.89

Note: The first two rows correspond to the threshold for the extreme poverty and poverty line, respectively. The third, fourth and fifth rows display the benefits given to households below the first threshold, households below the second threshold with children and with teenagers, respectively. The average transfers per capita are in the last two rows.

line and as of September 2016 with the red dashed line. Vertical lines indicate the eligibility threshold for these households before (green solid line) and after (red dashed line) the reform. Even though there is large bunching in round numbers, bunching below the threshold is visible before and after the reform. Note that there are households in dominated areas of the schedule (right above the threshold) before the reform.



(a) Households without Children

(b) Households with Children

Figure 5: Per Capita Income Distribution

These figures plot the empirical distributions of reported income for applicants without (Panel (a)) and with children (Panel (b)). The solid green lines and the red dashed lines plot the distributions as of June 2014 as of September 2016, respectively. The green solid vertical lines indicate the extreme poverty (first threshold) and the poverty line (second threshold) before the reform, while the red dashed lines plot the same aspects after the reform.

Figure 5b displays the analogous distributions for households with children as of April 2014 (solid green line) and as of September 2016 (red dashed line). Since these households were affected at both the extreme poverty and poverty thresholds, I depict each with solid green vertical lines before the reform and with red dashed lines after. The same patterns arise here, although bunching is less pronounced at the second threshold (which determines lower transfers). The presence of households in dominated areas is more evident for these distributions.

3 Reduced-Form Evidence of Jumping Effects

This section presents the reduced-form evidence of jumping. I lay out a simple test to asses the existence of jumps in Section 3.1. Sections 3.2 and 3.3 present evidence of the reform's effect on the timing of updates and on the share of jumpers, respectively. Section 3.4 shows the heterogeneity of jumps across different initial income levels, which is an important basis for the theoretical framework introduced later.

3.1 A Simple Test for Jumps

Consider households[7] that choose income y and consumption c in order to maximize their utility $u(c, y)$. Implicitly, households are producing income by supplying labor which is costly.[8] They face a budget constraint that allows them to consume no more than their after-transfer income. Consider a simple anti-poverty program that transfers I to households with income below t (as in the empirical setting). The household problem can be written as:

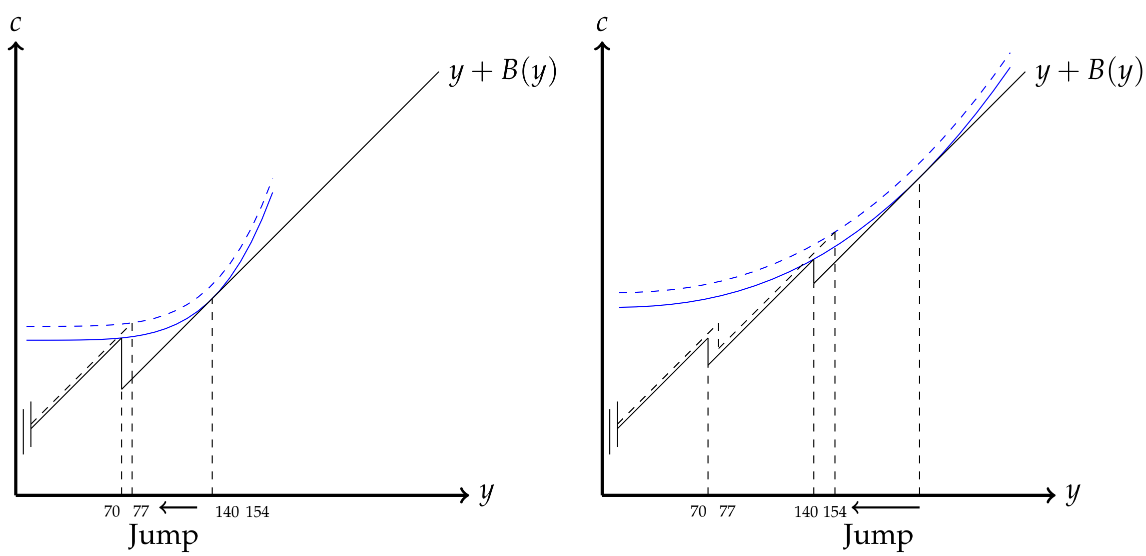
$$\max_{c,y} u(c, y) \; s.t. \; c \leq y + I * 1(y \leq t).$$

As discussed in Section 2.4, households without children faced an increase in their threshold of eligibility t from 70 to 77 *reais* and also an increase in their transfer. Figure 6a's solid black line illustrates the budget set of these households before the reform and, in dashed black, the corresponding set after the reform. Note that the 45-degree line represents the budget set in the absence of transfers. Since there are transfers for households with income below R\$70, the budget set is nonlinear.

[7]Since the eligibility of the program is based on the household income per capita, the relevant level of the analysis in the empirical application is the household.

[8]An important part of the responses corresponds to misreporting rather than labor supply behavior. However, in the absence of fiscal externalisties of misreporting, elasticities of the reported income are still the sufficient statistics for the welfare analysis, even in the presence of misreporting responses (Feldstein, 1999). Hence, I use a model of labor for simplicity.

In the same figure, the blue indifference curves represent the preferences of a particular household. The preferences are such that utility is increasing in consumption and decreasing in income (labor supply), so that utility increases to the northwest of the graph. Before the reform, the household is indifferent between being in or outside the anti-poverty program, but chooses to be out of the transfer program in such a situation (solid curve). The reform does not affect this household's marginal transfer (slope around its initial income level) or virtual income (intercept of the linearized schedule around its initial income level). Therefore, the reform should not affect this household through local income or substitution effects. However, if households perceive the nonlinearities of the schedule, they could jump to the new threshold in response to the reform (dashed curve). This corresponds to the jumping behavior.



(a) Households without Children

(b) Households with Children

Figure 6: Household Problem Before and After the Reform

Note: Panel (a) displays the BF reform effect on earnings choices of a noneligible household without children indifferent between being outside of the program or at the first threshold. Panel depicts (b) effect of the same reform on a noneligible household with children indifferent between being outside of BF or on the second threshold.

The goal is to test whether households that are initially above the new threshold of eligibility moved to the new threshold (R$77) because of the reform. In practice, I consider any movement to the interval between the old and new threshold $(70, 77]$, since this interval only became attractive after the reform. Let NEA be the number of noneligible applicants that were registered before the reform above 77, and JA the number of such households that jumped to the $(70, 77]$ interval because of the reform (jumping applicants). I denote $share(\Delta t, \Delta I)$ as the share of applicants that jumped to the threshold from an income level above the eligibility threshold, i.e.,

$$share(\Delta t, \Delta I) = \frac{JA}{NEA}. \tag{1}$$

Formally, I test the following hypothesis:

$$H_0 : share(\Delta t, \Delta I) = 0 \; vs.$$
$$H_a : share(\Delta t, \Delta I) > 0.$$

Under the null, households do not respond to the infra-marginal reform. Note that the alternative hypothesis corresponds to a jump to a positive income level that cannot be interpreted as an extensive margin response, as in Saez (2002) or Jacquet et al. (2010).

The threshold of eligibility for households with children increased from 140 to 154 *reais* and their per capita transfer rose as discussed in the previous section. Figure 6b plots in solid and dashed black lines the budget set of these households before and after the reform, respectively. Once again, the blue indifference curves represent the preferences of a household that was out of the program but indifferent to locating at the notch before the reform (solid curve), and that jumps to the new notch after (dashed curve).

Even though these households also faced incentives to move to the new first threshold (77), the budget line they faced between this threshold and the last one (154) also changes. It is possible that responses in this region have to do with that change (income effects), rather than with a change at a more distant part of the budget constraint (jumping effects). Such a possibility pollutes the test for this group. Therefore, I focus on jumps to the second threshold. In this case, NEA are households with children with per capita income above 154 before the reform, and JA is the subset of households that moved to $(140, 154]$ after the reform.

Households change their reported income for many reasons unrelated to the changes in the schedule in the data. Note that this test requires the identification of the part of these movements caused by the reform. Next, I present the research design for this identification and the results of the test.

3.2 Effect of the Reform on the Timing of the Update

Since BF allows applicants to report their information on any day the programs' offices are open, the reform could have affected both the timing of updates as well as the reported per capita income. To investigate the first of these two channels, Figure 20 plots the distributions of the months of the updates for households without and with children, respectively. The gray area indicates the months between the announcement (April 2014) and ennactment (June 2014) of the reform.

(a) Households without Children

(b) Households with Children

Figure 7: Date of Updates Distributions

These figures plot the empirical distributions of the months of updates for households without (Panel (a)) and with children (Panel (b)). The solid green lines and the red dashed lines plot the distributions for households above and below the eligibility threshold, respectively.

As previously discussed, the groups of interest are households without children above the first threshold (R$77) and households with children above the second threshold (R$154). The solid line of Figure 7a plots the distributions of the month of update among household-month observations in which the households did not have children and reported income above R$77, while Figure 7b's solid line plots the same distribution for observations with children above R$154. There is a modest increase in the number of updates right after the reform. However, since the number of updates varies substantially, even in the pre-reform period, it is hard to attribute this increase to the reform. For this reason, the dashed lines in the same figures plot the respective distributions among households that were below the new threshold. These households did not experience the same large changes in their incentives to jump down.[9] Under the assumption that the distribution of the timing of updates would be the same for households above and below the threshold in the absence of the reform, the difference between the solid and dashed lines can be interpreted as the effect of the reform on these timings. For both groups, the reform seems to have increased the number of updates.

[9]In the presence of large income effects, these households could also have faced incentives to update their income too.

The next section documents the second, and most important, effect of the reform: Conditional on updating, potential jumpers changed their reported income to the areas of the schedule that became more attractive with the reform.

3.3 Main Evidence

I start by performing the test described in Section 3.1 among households without children. Let $share^{no\ Kids}_{77,m}$ be the share of households without children that ever updated their per capita income from above 77 *reais* to the $(70, 77]$ interval up to month m:

$$share^{no\ Kids}_{77,m} \equiv \frac{\text{N. of hhlds. w/o Kids updating from above 77 to } (70,77] \text{ up to month } m}{\text{N. of hhlds. w/o Kids updating from above 77 up to month } m}.$$

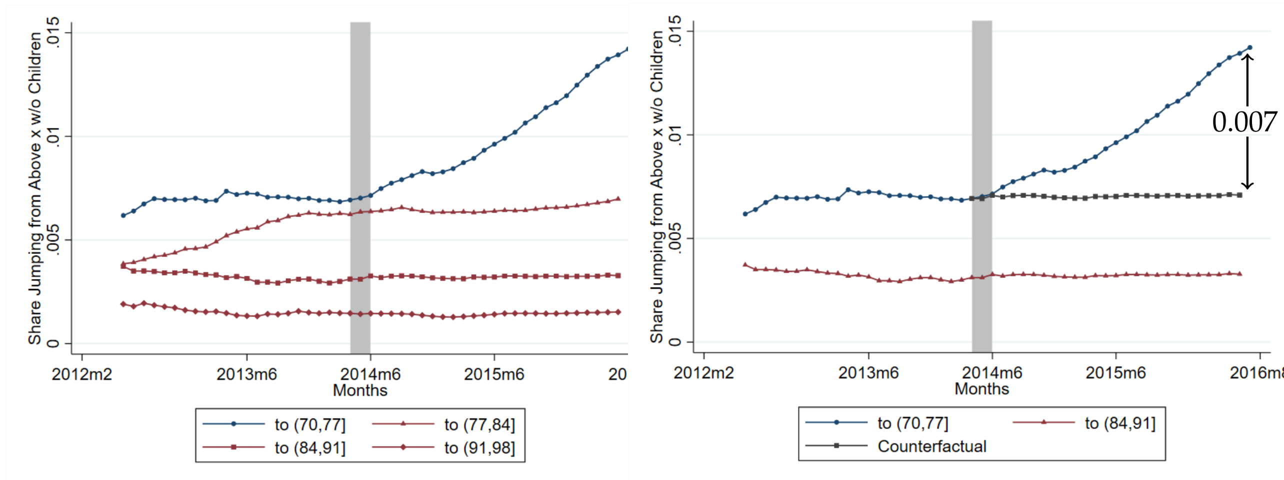
The measure of jumping applicants JA is straightforward: the number of households that updated their income from some level above 77 to the relevant interval $(70, 77]$ up to each month. Perhaps the most natural way to define the number of noneligible applicants is as the number of households with income above R$ 77 in the previous period. However, in this case, the shares are not comparable before the reform because there were more households updating to regions closer to the threshold, probably due to misoptimization. To see this, Appendix A.3 replicates the results of this section with an alternative share definition in which the number of households that were ever above the relevant threshold is the measure of NEA.[10] I therefore focus on frequencies of updating to the given intervals conditional on updating. In Appendix A.4, I show that the reform did not affect the timing of the updates differently across comparison groups. Therefore, the comparison conditional on updating is capturing all the effect of the reform on these shares.

The treatment effect of the reform on $share^{no\ Kids}_{77,m}$ corresponds to $share(\Delta t, \Delta I)$, i.e., the share of households jumping to the new notch because of the reform. Figure 8a plots with a solid blue line $share^{no\ Kids}_{77,m}$ from May 2012 until June 2016. The shaded area in gray corresponds to the months between the announcement of the reform (April 2014) and when it was actually enacted (June 2014).

There is a sharp increase around the months of the reform and its announcement. This break could still arise from some event around the month of the reform (e.g., an economic crisis) that pushed households to report lower levels of income.

To conduct a placebo test, let $share^{no\ Kids}_{x,m}$ be the share of households without children that

[10] There is still clear evidence of jumps, but the pre-trends are not parallel.



(a) Evidence of Jumps

(b) Estimating $share(\Delta t, \Delta I)$

Figure 8: Share of Households without Children Jumping from Above 77

Note: Panel (a) depicts the cumulative shares of households without children above R$x that moved to the $(x-7, x]$ interval up to each month in time, out of all households initially above R$x and that update. The blue line with circles and the red lines with triangles, squares, and diamonds plot the shares for $x = 77, 84, 91$, and 98, respectively. Panel (b) replicates the series for $x = 77$ and 91 and draws the counterfactual distribution for the share above R$77, under the assumption that its trend would remain parallel to the trends in the shares above R$91 after the reform (gray line marked with squares). The gray vertical bars indicate the months between the announcement and the enactment of the reform.

jump from above x to $(x-7, x]$ for $x = 84, 91, 98$:

$$share^{no\ Kids}_{x,m} \equiv \frac{\text{N. of hhlds. w/o Kids updating from above } x \text{ to } (x-7,x] \text{ up to month } m}{\text{N. of hhlds. w/o Kids updating from above } x \text{ up to month } m}.$$

The red lines marked with triangles, squares, and diamond in the same figure plot these shares; they correspond to $x = 84, 91$, and 98, respectively.[11] None of these intervals became more attractive after the reform. Reassuringly, these series are smooth around June 2014. I interpret this as evidence that households jumped to the new notch because of the reform, i.e., $share(\Delta t, \Delta I) > 0$.

I estimate the share of pre-reform applicants that jumped because of the reform $share(\Delta t, \Delta I)$ with the following differences-in-differences specification.

$$\hat{share}_{77}(\Delta t, \Delta I) = share^{no\ Kids}_{77,6/16} - share^{no\ Kids}_{77,4/14} - \left(share^{no\ Kids}_{91,6/16} - share^{no\ Kids}_{91,4/14}\right). \quad (2)$$

Under the identifying assumption that $share^{no\ Kids}_{77,m}$ and $share^{no\ Kids}_{91,m}$[12] trends would have

[11] Notice that the share jumping to $(77, 84]$ increases at the beginning of 2013. This is likely a result of the change in the minimum wage to 678 in that period, which means that that interval included one eighth of the 2013 minimum wage.

[12] Even though the trends in $share^{no\ Kids}_{77,m}$ and $share^{no\ Kids}_{98,m}$ are also parallel, I chose $share^{no\ Kids}_{91,m}$ as the coun-

remained parallel in the absence of the reform, this calculation measures the treatment effect of the reform on the share of jumpers: $share(\Delta t, \Delta I)$. Although this is not directly testable, the trends in shares that jump to $(70, 77]$ and to $(84, 91]$ are parallel before the reform. Figure 8b illustrates this calculation, indicating that $\hat{share}(\Delta t, \Delta I) = 0.007$ — i.e., 0.7% of the households without children with per capita income above R$77 update their income to the new threshold because of the reform. This corresponds to a 100% increase with respect to the pre-reform share. Appendix A.5 conducts a formal inference test on the parallel trends assumption and on this estimate. The parallel trends assumption cannot be rejected at a 5% significance level and, even in the 5% random sample, the estimate is significant at a 1% level with a *t-statistic* of 13.35.[13]

The eligibility threshold for households with children increased from 140 to 154 *reais*, and their transfers were adjusted as described in Section 2.4. Since households without children were out of the BF at income level 70 or 77, this change did not affect their incentives to move to the $(140, 154]$ interval. Therefore, these households are a useful control group for the analysis around this second threshold. Consider the following share definitions:

$$share^{Kids}_{154,m} \equiv \frac{\text{N. of hhlds. with Children updating from above 154 to } (140, 154] \text{ up to month } m}{\text{N. of hhlds. with Children updating from above 154 up to month } m},$$
$$share^{No\ Kids}_{154,m} \equiv \frac{\text{N. of hhlds. w/o Children updating from above 154 to } (140, 154] \text{ up to month } m}{\text{N. of hhlds. w/o Children updating from above 154 up to month } m}.$$

Figure 9's blue line with circles plots $share^{Kids}_{154,m}$, and its red line with triangles plots $share^{No\ Kids}_{154,m}$. Once again, there is a sharp increase in the share of jumpers to the interval that became attractive after the reform $(140, 154]$. Furthermore, the same share among households without children does not present the same sharp increase. This figure is evidence of jumping behavior among households with children. I calculate $share^{154}(\Delta t, \Delta I)$ using a similar specification as before:

$$\hat{share}_{154}(\Delta t, \Delta I) = share^{Kids}_{154,6/16} - share^{no\ Kids}_{154,4/14} - \left(share^{no\ Kids}_{154,6/16} - share^{no\ Kids}_{154,4/14}\right). \quad (3)$$

As indicated in Figure 9, $\hat{share}_{154}(\Delta t, \Delta I) = 0.014$, which means that 1.4% of the households with children above 154 jumped to the $(140, 154]$ interval because of the reform (58% increase with respect to the pre-reform share).[14] The differences-in-differences regression

terfactual group, as this share's levels are closer to the levels of $share^{no\ Kids}_{77,m}$ before the reform.

[13] All inference is based on robust standard errors. These standard errors shrink when I cluster at the household or household-composition level.

[14] An alternative analysis using households jumping to neighboring intervals is presented in Appendix

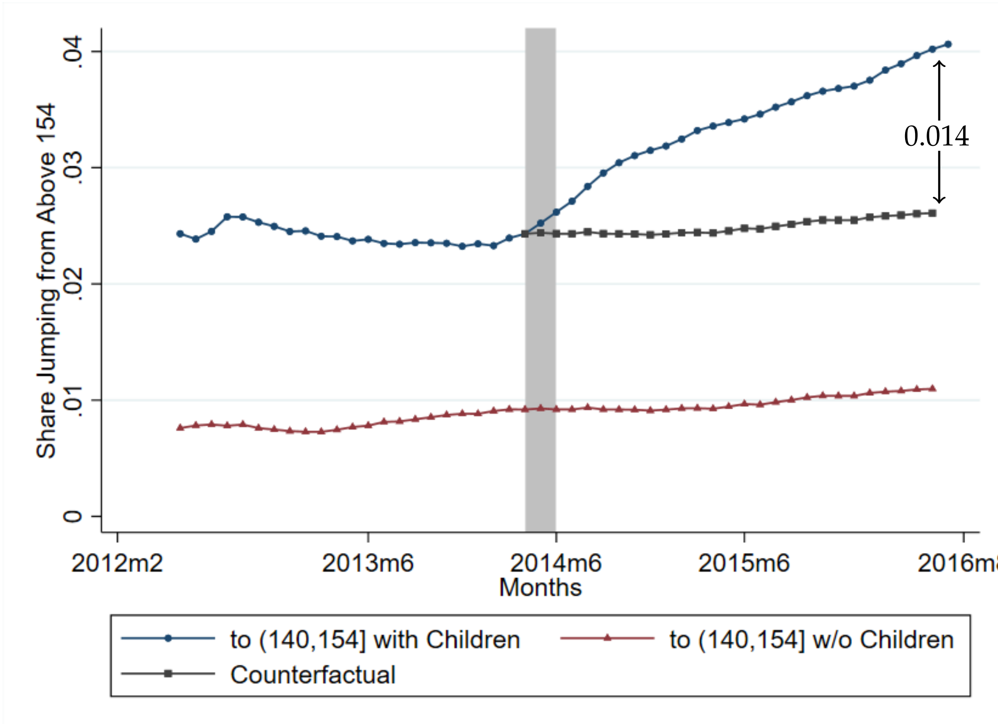


Figure 9: Share of Households Jumping from Above 154

Note: The blue line with circles and the red lines with triangles plot the cumulative shares of households with and without children above R$154 that moved to the $(140, 154]$ interval up to each month in time, respectively. These shares are computed out of all households with and without children that updated. The gray line marked with squares draws the counterfactual distribution for the share of households with children, under the assumption that its trend would remain parallel to the trends in the shares without children after the reform. The gray vertical bar indicates the months between the announcement and the enactment of the reform.

analysis presented in Appendix A.5 finds the same effect, which is significant at a 1% level with an associated *t-statistic* of 15.71.

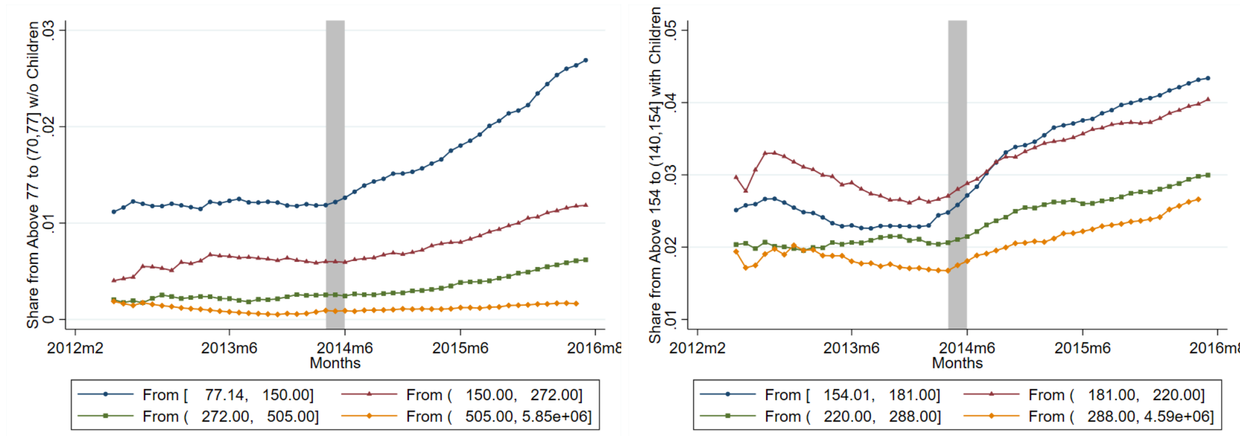
Appendix A.6 presents (1) graphs with the number of jumpers in each month instead of cumulative shares; (2) graphs that focus only on households that did not change their composition; and (3) some alternative placebo tests.

3.4 Heterogeneity: Jumping from Different Income Levels

Figure 10a presents evidence that the jumps come from different parts of the income distribution of noneligible applicants above the first threshold among households without children. The blue solid line plots the share of households that jumped from the first quartile above 77 *reais* to the $(70, 77]$ interval up to each month in time. The red long-dashed,

A.6. These intervals were affected by changes in the minimum wage that pollute the analysis, and for this reason I chose households without children as the control group. The effects with this alternative control groups are, if anything, larger than the ones in reported in this section.

green dashed, and yellow short-dashed lines plot the share of households jumping from the second, third, and fourth quartile, respectively.



(a) Households without Children above 77

(b) Households with Children above 154

Figure 10: Share of Households Jumping from each Quartile above the New Threshold

Note: Panel (a) depicts the cumulative shares of households without children in each quartile above R$77 that moved to the $(70, 77]$ interval up to each month in time, out of all households initially above R$x and that update. The blue line with circles, red line with triangles, green line with squares, and yellow line with diamonds plots the shares from the first, second, third and fourth quartile, respectively. Panel (b) displays the same analysis for the shares of households with children initially above R$154 and that jumped to the $(140, 154]$ reais interval. The gray vertical bars indicate the months between the announcement and the enactment of the reform.

Even though the share of households jumping from the first quartile has a more definitive increase after the reform, shares from the second and third quartiles were also affected.

Figure 10b plots the same shares among households with children that were initially above 154 *reais*.

Again, the effect of the reform is larger among households in the first quartile above the threshold. Among this second group of households, the jumps persist even in the fourth quartile. These graphs show that jumpers come from different income levels above the eligibility threshold. Note that the per capita income distribution is more concentrated for households with children above R$154 (the 75th percentile is R$288) than for households without children above R$77 (the 75th percentile is R$505). This could explain the larger top quartile effect among households with children jumping to the second threshold.

One potential concern with this interpretation is that households jumping from the fourth quartile could have moved to lower quartiles before the jump. This mean-reversion process of income is usual in tax records (see, for instance, Gruber and Saez (2002)). One cannot observe all pre-jump income adjustments, because of the unbalanced nature of the panel. For example, a household with income in the fourth quartile of the income distribution above the threshold in 2012 might have changed its per capita income to the first quartile in 2013 without updating this change in the program. I investigate this hypothesis by analyzing income movements across quartiles before the reform in Appendix A.7. A small share of households moves from the fourth to lower quartiles among households with children with income above 154 *reais*. This suggests that mean reversion does not explain jumps from larger income levels.

4 Welfare Framework

In this section, I present a model that takes into account three aspects seen in the data: (1) households jumping to the threshold as a response to some small infra-marginal reform; (2) jumps coming from across the income distribution above the new eligibility threshold; and (3) the presence of households in dominated areas of the schedule. To accommodate (1) and (2), the model allows households to differ in ability and elasticity types, as described in Section 4.1. This allows me to define the share of jumpers from each income level in Section 4.2. Section 4.3 incorporates inattention in the model to address aspect (3). Section 4.4 introduces the welfare function and derives the welfare effect of a small reform, which is the basis for my empirical analysis.

4.1 Preferences

Households choose consumption c and income y to maximize their utilities. They differ in their income productivity n. Applicants produce income by reducing leisure, and those with higher ability can do this at a more favorable rate. These households are also heterogeneous in their elasticity type m, which determines the convexity of the indifference curves in the the (y, c) plane. While n orders preferences according to the first derivative of the indifference curves in the (y, c) plane, m orders these preferences according to the second derivative. Intuitively, higher elasticity types are more responsive to small changes in the schedule. I discuss this point more formally in what follows.

Let $(n, m) \sim F(\cdot, \cdot)$ and $B(\cdot)$ be the transfer schedule as a function of income. The household problem is:

$$\max_{c,y} u(c, y; n, m) \; s.t. \qquad (4)$$
$$c \leq y + B(y).$$

There are three assumptions on preferences in this economy.

Assumption 1. *For any elasticity type m, consumption c, and income level y, the marginal rate of substitution between consumption and income is decreasing with the ability type, i.e., $-\frac{\partial}{\partial n}\frac{u_y(c,y;n,m)}{u_c(c,y;n,m)} < 0$.*

Assumption 1 ensures that the single-crossing condition holds for any realization of m so that $y(n,m)$ is monotone in n for any transfer schedule $B(\cdot)$.

Assumption 2. *For any type (n,m), the marginal rate of substitution between consumption and income is increasing with income (or decreasing with leisure).*

The following lemma shows that $y(n,m;c,MRS)$ is monotonic in n for any m.

Lemma 1. *Under Assumptions 1 and 2, $y(n,m;c,MRS)$ is increasing in n.*

Proof. See Appendix A.8 □

Lemma 1 implies that $y(n,m;c,MRS)$ is invertible with respect to the first argument. Let $n(y,c;m,MRS)$ be the ability type at allocation $(y,c) \in \mathbb{R}^2_+$, with elasticity type m and marginal rate of substitution $MRS \geq 0$.

Definition 1. *The convexity of the indifference curve of agents at an allocation (y,c) with marginal rate of substitution MRS with elasticity types m is:*[15]

$$Convex(m;c,y,MRS) \equiv -\frac{u_{yy}\Big(c,y;n(y,c;m,MRS),m\Big)u_c(\cdot) - 2u_{cy}(\cdot)u_c(\cdot) + \frac{u_{cc}(\cdot)}{u_c(\cdot)}u_y(\cdot)^2}{u_c(\cdot)^2}.$$

This function describes how the convexity of the indifference curves varies with the elasticity type, for a given allocation (y,c) and slope of their indifference curves MRS. Note that as one varies m, the ability type $n(c,y,m,MRS)$ is adjusted so that the new type would still present marginal rates of substitution MRS at the allocation (y,c).

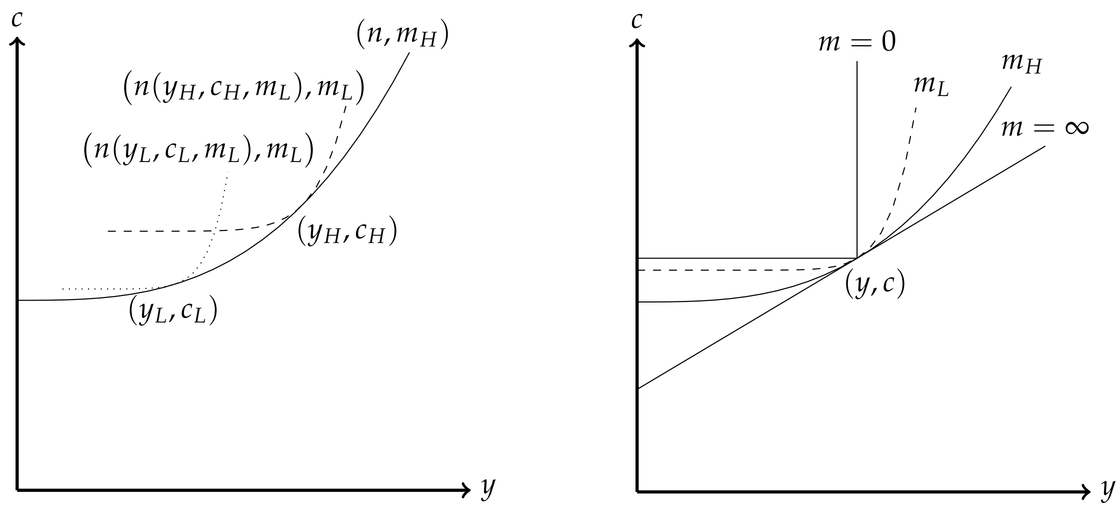
The next assumption orders preferences with respect to m according to the convexity of their indifference curves.

Assumption 3. *For any allocation $(y,c) \in \mathbb{R}^2_+$, $Convex(m;c,y,MRS)$ is decreasing and continuous in m and:*

$$\lim_{m\to 0} Convex(m;c,y,MRS) = \infty \text{ while } \lim_{m\to\infty} Convex(m;c,y,MRS) = 0.$$

Figure 11a shows how this convexity changes as the elasticity type increases from m_L to

[15] I suppress the arguments of all but the first marginal utility terms, as they are the same.



(a) Convexity Degrees at Different Allocations (b) Preferences Satisfying Assumption 3

Figure 11: Preferences under Different Convexity Degrees m

Note: Panel (a) illustrate preferences of three different types. Type (n, m_H) is indifferent between allocations (y_L, c_L) and (y_H, c_H)). Type $(n(y_L, c_L, m_L), m_L)$ has a lower elasticity $m_L < m_H$ and the same MRS of type (n, m_H) at allocation (y_L, c_L). Type $(n(y_H, c_H, m_L), m_L)$ has also a lower elasticity $m_L < m_H$ and the same MRS of type (n, m_H) at allocation (y_H, c_H). None of the ability types need to coincide. Panel (b) presents four different types with the same MRS at the allocation (c, l) with elasticity types varying from 0 (Leontief preferences) to ∞ (linear preferences).

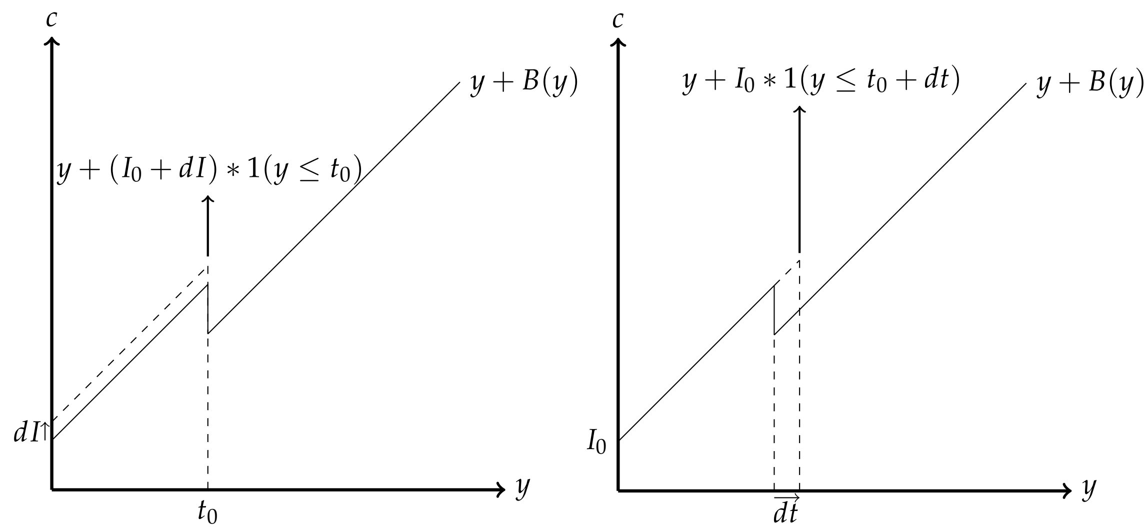
m_H, for two different allocations ((c_L, y_L) and (c_H, y_H)) and marginal rates of substitution. Assumption 3 is satisfied by preferences represented by the isoelastic utility function, with m indexing the income elasticity as shown in Appendix A.9.

Figure 11b illustrates preferences that satisfy this assumption at a particular allocation and with a particular marginal rate of substitution. The next section shows that in such an economy, the share of households jumping across brackets is well defined in contrast to the unidimensional Mirrleesian framework.

4.2 Share Concepts

This section defines the "share of jumpers" concepts that are the sufficient statistic for the welfare analysis of the BF reform. I focus on a special case of problem (4), in which $B(y) = I_0 * 1(y \leq t_0)$. Here t_0 and I_0 correspond to the initial threshold of eligibility and transfer to eligible households, respectively. Let t and I define perturbations in these two aspects of the schedule. Figure 12 illustrates these perturbation concepts, which are analogous to the ones observed in the empirical setting.

The income chosen by type (n, m) before $y_b(n, m)$ and after $y_a(n, m)$ the perturbation are



(a) Transfer Perturbation dI (b) Threshold Perturbation dt

Figure 12: Pertubation Definitions

Note: Panel (a) illustrates a perturbation in the transfer given to the eligible poor, while panel (b) depicts a perturbation in the threshold of eligibility.

defined as:[16]

$$y_b = \arg\max_y u\Big(y + I_0 * 1(y \leq t_0), y; n, m\Big) \text{ and}$$
$$y_a = \arg\max_y u\Big(y + (I_0 + I) * 1(y \leq t_0 + t), y; n, m\Big).$$

For any income level above the dominated interval of the transfer schedule $y \geq I_0 + t_0$, the share of jumpers as a response to a 1% change in the after-transfer income $share^I(y)$ and in threshold $share^t(y)$ are:

$$share^I(y) = \frac{\partial P(y_a = t_0 | y_b = y)}{\partial I}(t_0 + I_0) \ for\ y > t_0,\ and$$
$$share^t(y) = \frac{\partial P(y_a = t_0 | y_b = y)}{\partial t} t_0. \tag{5}$$

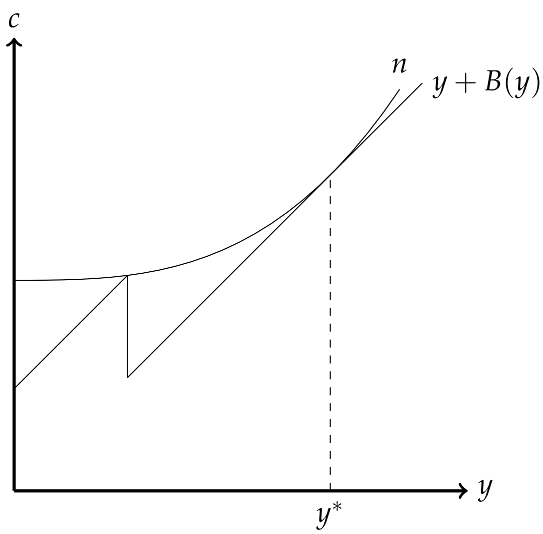
The first parameter $share^I(y)$ measures the increase in the probability of jumping to the threshold, given a 1% increase in the after-transfer income at the threshold, among households located at y before the perturbation. This is the additional sufficient statistic in the optimal tax formula with jumps, as I show in Appendices A.11 and A.12.[17] The second

[16] I suppress the type (n, m) from now on to simplify notation.

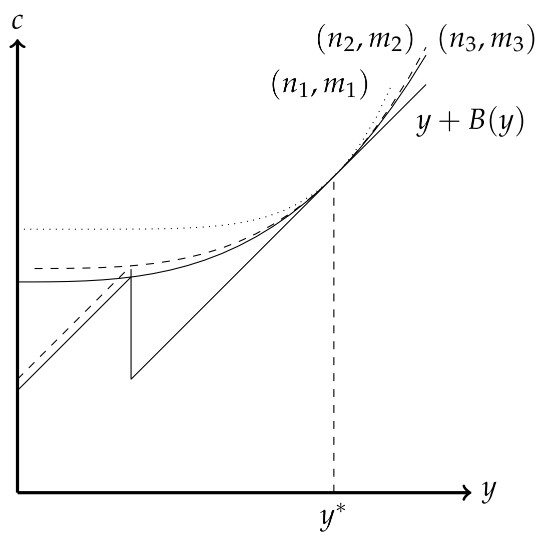
[17] In the optimal tax formula, the planner needs this share with respect to a perturbation at any point in

measures the increase in the probability of jumping to the threshold, given a 1% increase in the threshold t among the same group of households. Average shares across income levels above t_0 are the sufficient statistics for the counterfactuals computed in Appendix A.14.

To see that these derivatives are only well defined in a model with multidimensional heterogeneity, consider Figure 13. Panel A displays the only type n that would choose income y^* in an economy with heterogeneity only in ability. Under the single-crossing condition, this is the only type located at y^*. The derivative $\frac{\partial P(y_a=t_0|y_b=y)}{\partial I}$ is not well defined, as this probability jumps from zero to one for any positive perturbation $I > 0$. Panel B illustrates three different types that will choose income y^* in an economy with preference heterogeneity along elasticity types m. In this case, a small perturbation in the schedule $dI > 0$ would make a set of types originally in y^* jump (in the figure, this set would correspond to all the types $(n(y^*,m)m)$ for m between m_2 and m_3). Some other types (consider (n_1, m_1), for instance), however, would not jump due to this perturbation. In this case, the derivative of the probability is well defined.



(a) One-dimension Heterogeneity



(b) Two-dimension Heterogeneity

Figure 13: Types Initially at y^*

Note: Panel (a) shows the preference of a household indifferent between income level y^* and the threshold of eligibility in an economy in which households only differ in ability types. Panel (b) depicts preferences of different types choosing y^* before the reform in an economy with two dimensions of heterogeneity.

Finally, let the share of jumpers at income level z as a response to a marginal reform in the

the non-convex areas of the schedule and not only at the eligibility threshold.

threshold and transfer be defined as

$$share^{J}(dt, dI; z) = \frac{\partial P(y_a = t_0 | y_b = z)}{\partial I} dI + \frac{\partial P(y_a = t_0 | y_b = z)}{\partial t} dt.$$

I show below that the sufficient statistic for the welfare analysis of a (dt, dI) reform is the average share of jumpers in response to the reform across income levels above t_0. Even though this average share would be well defined in a unidimensional economy, such a model would imply that all jumpers would come from a particular interval of the income distribution, which contradicts the evidence presented in Section 3.4.

4.3 Incorporating Inattention

There is a considerable fraction of households in the data that locate themselves in dominated areas of the schedule. This could be explained, for instance, by mis-optimization or frictions, as discussed by Kleven and Waseem (2013). It is important to take this into account in the welfare analysis, because a small increase in the threshold could impact the utility of households located in the dominated area right above the initial threshold. In this section, I address this issue by allowing households to differ in a third dimension of heterogeneity: attention r.

Attention $r \in \mathbb{R}_+$ is the degree to which households perceive the schedule when choosing their labor supply. The household problem for attention type r is:

$$y(n, m, r) \equiv arg\max_{y} u(y + rB(y), y; n, m). \tag{6}$$

Attentive households $r = 1$ perceive the schedule as it is and maximize their utility. Inattentive households $r = 0$, on the other hand, choose income to maximize utility and ignore the presence of the anti-poverty program. Their labor supply function $y(n, m, 0) = \underset{y}{argmax}\, u(y, y; n, m)$ is independent of the schedule. Households may also under-perceive $r < 1$ or over-perceive $r > 1$ the schedule. Households with low attention $r \approx 0$ could be located right above the eligibility limit t. The demand for consumption can be computed using the actual household budget $c(n, m, r) = y(n, m, r) + B\big(y(n, m, r)\big)$.

To incorporate inattention, $y_b(n,m,r)$ and $y_a(n,m,r)$ are redefined as:

$$y_b = \arg\max_y u\Big(y + rI_0 * 1(y \leq t_0), y; n, m\Big) \text{ and}$$
$$y_a = \arg\max_y u\Big(y + r(I_0 + I) * 1(y \leq t_0 + t), y; n, m\Big).$$

All of the share concepts remain the same. Here, Assumption 1 ensures that for any elasticity and attention type (m,r) and transfer schedule $B(\cdot)$, $y(n,m,r)$ is increasing in ability n. Hence $n(y,m,r)$ can be defined as the inverse of $y(n,m,r)$ with respect to the first argument, i.e., the ability type that would locate in income level y with elasticity m and attention r under the schedule $B(\cdot)$

Finally, let $(n,m,r) \sim F_{NMR}(\cdot,\cdot,\cdot)$. The following assumption ensures that preferences and attention to the schedule have a joint smooth distribution, so that the welfare function is differentiable.

Assumption 4. *The joint distribution of types (n,m,r) is smooth and has full support in $\mathbb{R}^3_+$.*

Assumptions 1 to 4, together with the attention r definition, characterize preferences in this economy.

4.4 Welfare

Let $H(\cdot)$ be the income distribution of applicants under the observed schedule. Welfare under the schedule $(t_0 + t, I_0 + I)$ is a function of the perturbations t and I:

$$W(t_0+t, I_0+I) = \int_0^{t_0+t} \int \int G\Big(u\big(z + I_0 + I, z; n(z,m,r), m\big)\Big) dF_{MR|Y}(m,r|z) dH(z) \\ -\lambda \int_0^{t_0+t} (I_0 + I) dH(z), \quad (7)$$

where $G(\cdot)$ is an increasing and concave function that captures the redistributive motives of the planner, $F_{MR|Y}(\cdot)$ is the joint distribution of elasticity and attention types conditional on income, and λ is the marginal cost of public funds. It represents how much the government values R$1 of revenue relative to a R$1 given to the average applicant in the margin. If the government cares a lot about the poor, λ converges to zero. If the government is indifferent between giving a *real* to the poor and spending it elsewhere, λ approaches 1.

Let $g(z) \equiv \frac{1}{\lambda} \int \int G'(u) \left(u_c \left(1 + \frac{\partial c}{\partial I}\right) + u_y \frac{\partial y}{\partial I}\right) dF(m,r|z)$ be the average social marginal value of consumption for taxpayers with income z expressed in terms of the marginal value of public funds. Note that compared to the case in which households are fully informed about the schedule (see Saez (2001), for instance), the $u_c \frac{\partial c}{\partial I} + u_y \frac{\partial y}{\partial I}$ term must be included as the envelope theorem no longer applies. This corresponds to the behavioral wedge in Farhi and Gabaix (2015). For attentive households $r = 1$, the first-order condition of the household problem ensures that $u_c \frac{\partial c}{\partial I} + u_y \frac{\partial y}{\partial I} = 0$, which corresponds to the standard neoclassical case.

Let $\bar{g} = \frac{\int_0^{t_0} g(z) dH(z)}{H(t_0)}$ be the average social marginal value of consumption among the eligible poor; $\bar{g}(t_0) = \int_{t_0}^{t_0+I_0} \int_0^{\infty} \frac{G'(u(c,t_0,n(t_0,m,0),m,0)u_c(\cdot)}{\lambda I_0} dF_{M|RY}(m|0,t_0)dc$ be the average social marginal value of consumption for households at $(t_0, t_0 + dt)$ between consumption levels t_0 and $t_0 + I_0$; and $share^J(dt, dI) = \frac{\int_{t_0}^{\infty} share^J(dt,dI;z)dH(z)}{1-H(t_0)}$ be the average share of jumpers across all income levels beyond the threshold. The following proposition characterizes the welfare effect for a reform that perturbs the threshold and the transfer by infinitesimal amounts (dt, dI).

Proposition 1. *Under assumptions 1, 2, 3, and 4, an infinitesimal reform that changes the transfer given to the poor by dI and the eligibility threshold by dt impacts welfare by:*

$$dW = \lambda \Big((\bar{g} - 1)H(t_0)dI + \big(\bar{g}(t_0) - 1\big) I_0 h(t_0)dt - I_0 \big(1 - H(t_0)\big) share^J(dt, dI) \Big).$$

Heuristic proof. Consider the effect of the transfer perturbation from the initial schedule $B(\cdot)$ depicted in Figure 14. Transfers to households with income between 0 and t_0 are increased by dI and the threshold of eligibility by dt. This reform has three effects on welfare: (1) a transfer effect through the increased benefits dI given to households with income between 0 and t_0; (2) a threshold effect through the mechanical inclusion of households with income between t_0 and $t_0 + dt$ in the transfer program; and (3) a jumping effect on the government's budget coming from households with income above t_0.

Transfer Effect: Every household with income below the eligibility threshold t_0 will get dI extra reais, which is valued on average by $\lambda \bar{g} dI$ by the government. On the other hand, the costs of such transfers is λdI. Since there are $H(t_0)$ such households, the transfer effect is equal to:

$$\text{Transfer Effect} = \lambda(\bar{g} - 1)H(t_0)dI.$$

Threshold Effect: Every household with income between t_0 and $t_0 + dt$ will get I_0 extra reais. The net of costs value of such transfers by the government is $\lambda(\bar{g}(t_0) - 1)I_0$. Since

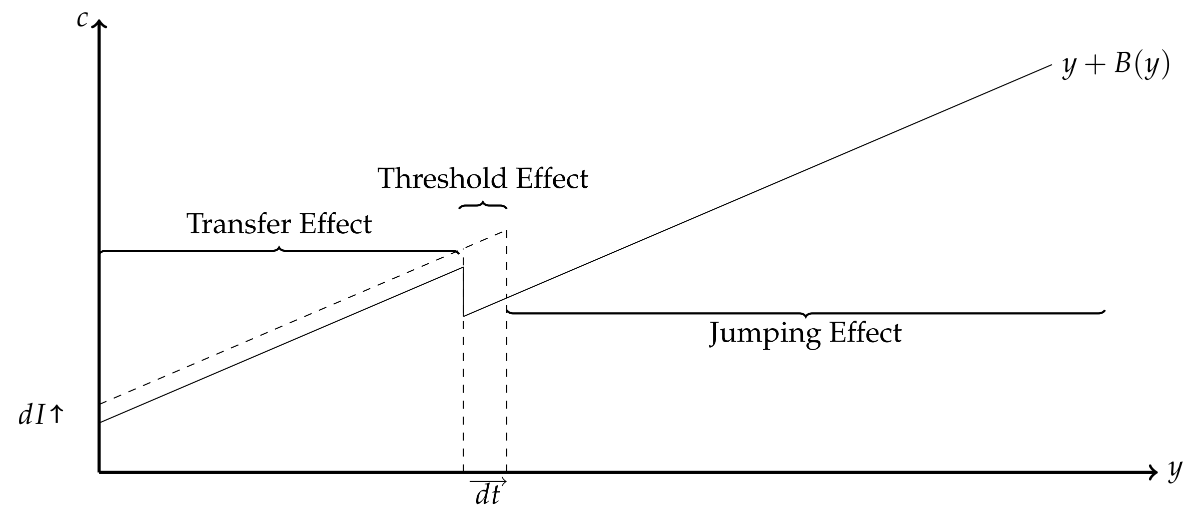


Figure 14: Perturbation from the Observed Schedule

Note: The figure illustrates perturbations in the transfer given to the eligible poor and in the threshold of eligibility as in the BF reform. The picture indicates the effects affecting each interval of the income distribution.

there are $h(t_0)dt$ such households, the threshold effect is equal to:

$$\text{Threshold Effect} = \lambda(\bar{g}(t_0) - 1)I_0 h(t_0)dt.$$

Jumping Effect: A fraction $share^J(dt, dI)$ of households initially above t_0 will jump to the threshold. This fraction is small for a small reform, and since these households are initially indifferent, the effect on their utility is also small. Therefore, by the envelope theorem, the effect of these jumps on welfare through jumpers' utility is second order. However, the government loses I_0 *reais* with each jump valued at λ. The fiscal externality is the same, no matter where households are jumping from. For this reason, the average share across income levels $share^J(dt, dI)$, rather than the shares at each income level $share^J(dt, dI; z)$, is sufficient to describe the jumping effect. Since there are $1 - H(t_0)$ potential jumpers, the total jumping effect is:

$$\text{Jumping Effect} = -\lambda I_0 \big(1 - H(t_0)\big) share^J(dt, dI).$$

The formula in Proposition 1 follows from the sum of these three effects. □

The formal proof is in Appendix A.10.

5 Estimating the Share of Jumpers Because of the Reform

The share of potential jumpers that jumped because of the reform $share^J(dt, dI)$ is the sufficient statistic for the welfare analysis. For discrete reforms, this share can be approximated by $share^J(\Delta t, \Delta I)$, i.e., the share of potential jumpers that jumped because of the discrete reform. This set of potential jumpers includes not only households registered in the program with per capita income above the eligibility threshold, but also households that entered the program after the reform. This last group was left out of the empirical analysis in Section 3.3, because their pre-reform per capita income is not empirically observable. Hence, the parameter of interest $share^J(\Delta t, \Delta I)$ differs from $share(\Delta t, \Delta I)$, i.e., the share of applicants that updated their income from above the eligibility threshold to the new notch because of the reform. The goal of this section is to recover $share^J$[18] from empirical estimates.

Section 5.1 describes a simple empirical framework that provides bounds and estimates of this share from the treatment effect of the policy on the share of noneligible pre-reform applicants that jumped (estimated in Section 3.3) and on the share of entrants at the new notch (which is also recoverable). I then present estimates of the effect of the reform on entrants in Section 5.2 and the bounds and estimates for the parameter of interest in Section 5.3.

5.1 Empirical Framework

Per capita income is only observable from the moment at which households apply to BF onward. Let the number of households that entered the program after the reform below the new threshold $(t_0, t_0 + \Delta t]$ be denoted by E_t. Even though this number is observed in the data, one cannot identify which of these households jumped from a higher income level and which were in that interval all along. I denote the first group as jumping entrants JE and the second as non-moving entrants NME, so that $E_t = JE + NME$. Since the total number of entrants E is also observable, the share of entrants that enter in $(t_0, t_0 + \Delta t]$ because of the reform is recoverable:

$$share^E = \frac{E_t}{E} = \frac{NME + JE}{E}.$$

[18]Throughout this section, I suppress the empirical share argument $(\Delta t, \Delta I)$ to save notation. I come back to the approximation of $share(dt, dI)$ with $share(\Delta t, \Delta I)$ in Section 6.

Remember from equation (1) that the share of pre-reform noneligible applicants that jumped, denoted by *share*, is the ratio between jumping applicants (JA) and noneligible applicants (NEA) estimated in Section 3.3. To define the share of jumpers $share^J$, note that the total set of jumpers J is given by jumping applicants JA and jumping entrants JE. Potential jumpers PJ include the noneligible pre-reform applicants above $t_0 + \Delta t$ (NEA) and entrants that were not in the $(t_0, t_0 + \Delta t]$ interval before they entered the program ($E - NME$). Therefore, the share of households jumping to the notch because of the reform $(\Delta I, \Delta t)$ is

$$share^J = \frac{J}{PJ} = \frac{JA + JE}{NEA + E - NME}.$$

This share differs from the share of households that updated from above the eligibility threshold because of entrants into the program.

It is easy to see that

$$share^J = \frac{NEA}{NEA + E - NME} share + \frac{E}{NEA + E - NME} \frac{JE}{E_t} share^E. \tag{8}$$

This equation is useful, as it relates the sufficient statistic for the welfare analysis $share^J$ to the two recoverable share concepts *share* and $share^E$. However, NME, which is necessary to recover the parameter of interest, is not observable.

There are two natural bounds for the number of non-moving entrants NME: It cannot be less than zero or greater than the number of entrants at the notch E_t. Substituting $NME = 0$ in the above equation provides an upper bound to the share of jumpers, since in this case all entrants at the new threshold would also be jumpers. Symmetrically, substituting $NME = E_t$ would provide a lower bound. The following equation provides bounds based on this logic:

$$\frac{NEA}{NEA + E - E_t} share \leq share^J \leq \frac{NEA}{NEA + E} share + \frac{E}{NEA + E} share^E. \tag{9}$$

Note that in the calculation of the lower bound $JE = 0$, so the second term in equation (8) disappears. To compute a point estimate for $share^J$ from the data, I make the following assumption:

Assumption 5. *The share of entrants below the new threshold that are jumpers is the same as the* *share of pre-reform applicants below the new notch after the reform that are jumpers, i.e.,*

$$\frac{JE}{JE+NME}=\frac{JA}{JA+NEA_t},$$

where NEA_t is the number of noneligible pre-reform applicants in $(t_0, t_0+\Delta t]$ before the reform.

Intuitively, this assumption states that distribution of pre-reform incomes above and below the new threshold conditional on being between the new and the old threshold after the reform is the same for pre-reform applicants and entrants. Under Assumption 5, $NME = \frac{NEA_t E_t}{JA+NEA_t}$. Substituting this in (8), $share^J$ can be written in terms of recoverable concepts:

$$share^J = \frac{NEA}{NEA+E-\frac{NEA_t E_t}{JA+NEA_t}} share + \frac{E}{NEA+E-\frac{NEA_t E_t}{JA+NEA_t}} \frac{JA}{JA+NEA_t} share^E. \quad (10)$$

The next section presents estimates of $share^E$. Together with the estimates of *share*, relation (9) provides bounds to $share^J$, and relation (10) characterizes it under Assumption 5.

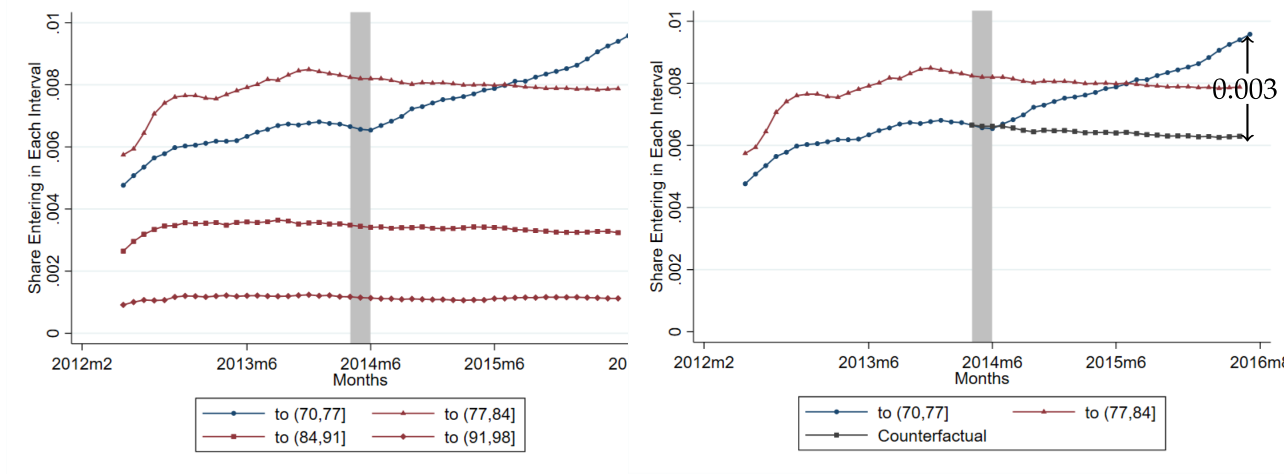
5.2 The Effect of the Reform on Entrants

To construct an empirical analog to the share of entrants below the new notch, remember that $(70,77]$ is the part of the schedule that became attractive after the reform for households without children. Let $share^{no\ Kids}_{77,m,E}$ be the share of these households entering the program since January 2012 (after the first extraction) between the old and new notch $(70,77]$ up to month m among those that entered the program in the same period:

$$share^{no\ Kids,E}_{77,m} = \frac{\text{N. of hhlds. w/o Children entering at } (70,77] \text{ from Jan. 2012 to m}}{\text{N. of hhlds. w/o Children entering from Jan. 2012 to m}}.$$

Figure 15's blue solid line plots $share^{no\ Kids,E}_{77,m}$ for every month from May 2012 to September 2016. Once again, the gray shaded area indicates the months between the announcement and the enactment of the reform. Even though this series trend is already increasing before the reform, there is sharp acceleration after June 2014, which suggests that the reform affected the share of entrants below the new notch.

It is important to perform placebo tests to rule out the hypothesis that this was a general trend in the economy. The same figure presents the trends for three placebo series. They correspond to the share of entrants in intervals right above the new threshold. Formally,



(a) Evidence of the Effect on Entrants

(b) Estimating $share^E$

Figure 15: Share of Households Entering with Income in $(70, 77]$

Note: Panel (a) depicts the cumulative shares of households without children entering at $(x-7, x]$ interval up to each month in time, out of all households entering BF since January 2012. The blue line with circles and the red lines with triangles, squares, and diamonds plot the shares for $x = 77, 84, 91$, and 98, respectively. Panel (b) replicates the series for $x = 77$ and 84 and draws the counterfactual distribution for the share with $x = 77$, under the assumption that its trend would remain parallel to the trends in the shares with $x = 84$ (gray line marked with squares). The gray vertical bar indicates the months between the announcement and the enactment of the reform.

the red lines marked with triangles, squares, and diamonds plot

$$share^{no\ Kids,E}_{x,m} = \frac{\text{N. of hhlds. w/o Children entering at } (x-7, x] \text{ from Jan. 2012 to m}}{\text{N. of hhlds. w/o Children entering from Jan. 2012 to m}},$$

for $x = 84, 91$, and 98, respectively. All these series are smooth around the reform, suggesting that the increase in the share entering $(70, 77]$ was indeed a consequence of the reform.

To compute $share^E$ for households without children, I use a differences-in-differences specification analogous to (2):

$$\hat{share}^E_{77} = share^{no\ Kids,E}_{77,6/16} - share^{no\ Kids,E}_{77,4/14} - \left(share^{no\ Kids,E}_{84,6/16} - share^{no\ Kids,E}_{84,4/14}\right). \tag{11}$$

I chose here $share^{no\ Kids,E}_{84,m}$ as the control group since it is the closest in levels to $share^{no\ Kids,E}_{77,m}$. Under the identifying assumption that these trends, $share^{no\ Kids,E}_{77,m}$ and $share^{no\ Kids,E}_{84,m}$, would remain parallel after the reform, $\hat{share}^E_{77}$ measures $share^E$ among households without children. Even though this assumption is not directly testable, the fact that these trends are parallel before the reform suggests that it holds in the BF setting. As indicated in Figure 15b, I find $\hat{share}^E_{77} = 0.003$. The reform increased the share of households without children entering in the $(70, 77]$ by 0.3 percentage points (50% of the pre-reform share). To conduct a formal inference test, Appendix A.13 presents the analogous regression specification. The estimate of the regression is the same and significant at the 1% level with a *t-statistic* of 7.82.

Figure 16 presents a similar analysis for households with children. For these households the interval $(140, 154]$ became attractive with the reform, although it did not for households without children.

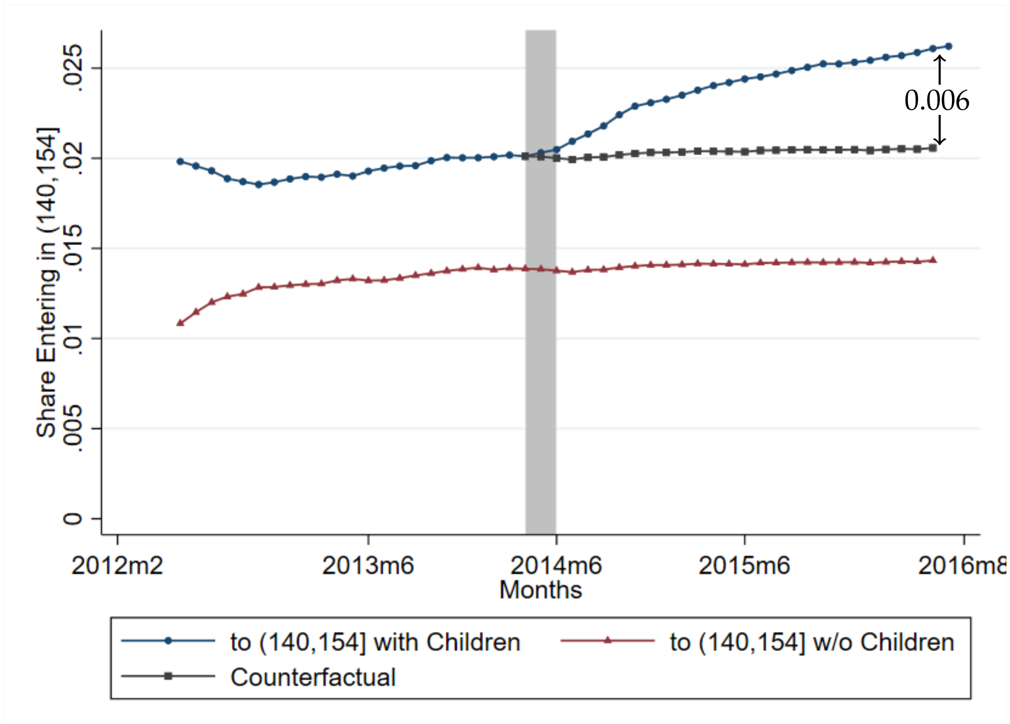


Figure 16: Share of Households Entering with Income in $(140, 154]$

Note: The blue line marked with circles and the red line marked with triangles plot the cumulative shares of households without and with children entering at $(140, 154]$ reais interval up to each month in time, out of all households with and without children entering BF since January 2012, respectively. The gray line marked with squares draws the counterfactual distribution for the share with children, under the assumption that its trend would remain parallel to the trends in the shares of households without children (gray line marked with squares). The gray vertical bar indicates the months between the announcement and the enactment of the reform.

Consider the following share definitions:

$$share^{Kids,E}_{154,m} = \frac{\text{N. of hhlds. with Children entering at } (140, 154] \text{ from Jan. 2012 to m}}{\text{N. of hhlds. with Children entering from Jan. 2012 to m}} \text{ and}$$

$$share^{no\ Kids,E}_{154,m} = \frac{\text{N. of hhlds. w/o Children entering at } (140, 154] \text{ from Jan. 2012 to m}}{\text{N. of hhlds. w/o Children entering from Jan. 2012 to m}}.$$

The blue line marked with circles and the red line marked with triangles plot $share^{Kids,E}_{154,m}$

and $share^{No\ Kids,E}_{154,m}$, respectively. Once again, households affected by the reform (with children) start entering disproportionately more around the months of the reform. The same is not true for households not affected (without children). Under the assumption that these trends would remained parallel after the reform, the following calculation recovers a consistent estimate for $share^E$ among households with children:

$$\hat{share}^{E}_{154} = share^{Kids,E}_{154,6/16} - share^{Kids,E}_{154,4/14} - \left(share^{no\ Kids,E}_{154,6/16} - share^{no\ Kids,E}_{154,4/14}\right). \qquad (12)$$

This calculation (depicted in the figure) yields $\hat{share}^{E}_{154} = 0.006$. The reform increased the share of households with children entering in the $(140, 154]$ interval by 0.6 percentage points (30% of the pre-reform share). The formal inference test in Appendix A.13 reports the same effect, which is significant at the 1% level with a *t-statistic* of 10.88.

5.3 Bounds and Estimates for *share*^{J}

To compute the bounds and estimates for $share^J$, I measure NEA as the number of households that were in the program with income above $t_0 + \Delta t$ in their last update before the reform (June 2014); E as the number of households that applied to the program for the first time after the reform; NEA_t as the number of households with income in the $(t_0, t_0 + \Delta t]$ interval right before the reform; E_t as the product of the number of households that applied for the first time between January 2012 and June 2016 and $\hat{share}^E$; and JA as the product of $\hat{share}$ and the number of households that updated from above the threshold up to June 2016. The resulting numbers for households without children affected by the reform of the first threshold ($t_0 = 70$) and households with children affected by the reform ($t_0 = 140$) are presented in the first five rows of Table 3.

The relation (9) provides a lower ($share^J_{LB} = \frac{NEA}{NEA+E-E_t} share$) and an upper bound ($share^J_{UB} = \frac{NEA}{NEA+E} share + \frac{E}{NEA+E} share^E$) for the sufficient statistic in the welfare analysis. These bounds are presented in the sixth and seventh rows in Table 3, respectively. Under Assumption 5, equation (10) describes how $share^J$ can be recovered from $share$ and $share^E$. Estimates from this specification are displayed in the last row. This last share is closer to the lower bound, because most of the pre-reform applicants located between the old and the new threshold after the reform were in this same region before the reform. This implies, under Assumption 5, that most of the entrants below the new threshold are non-moving entrants rather than jumping entrants.

Table 3: Calculations for $share^J$

	Hhlds. without Children ($t_0 = 70$)	Hhlds. with Children ($t_0 = 140$)
NEA	$72,402$	$87,155$
E	$129,972$	$160,153$
NEA_t	$1,691$	$10,311$
E_t	769	$1,782$
JA	963	$2,287$
$share^J_{LB}$	0.005	0.009
$share^J_{UB}$	0.009	0.016
$share^J$	0.006	0.011

Note: The first five rows correspond to the number of non-eligible applicants, entrants, non-eligible applicants between the old and the new threshold, entrants between the old and new threshold and jumping pre-reform applicant, respectively. The last three rows display the implied shares of jumpers' lower and upper bound and the point estimate under assumption 5. The first and second columns show the numbers for households without and with children.

6 Welfare Analysis of the Reform

This section presents the welfare effects of the BF reform. I compute the welfare effect of the discrete reform observed in the data $(\Delta t, \Delta I)$ using a linear approximation of the relation described in Proposition 1. In particular, this approximation does not take into account second-order terms that could affect the total effect of the reform. Since the reform is small, this is a reasonable approximation.

$$\frac{dW(\Delta t, \Delta I)}{\lambda} \approx \underbrace{(\bar{g}-1)H(t_0)\Delta I}_{\text{Transfer Effect}} + \underbrace{(\bar{g}(t_0)-1)I_0[H(t_0+\Delta t)-H(t_0)]}_{\text{Threshold Effect}} \underbrace{-I_0 share^J(\Delta t, \Delta I)}_{\text{Jumping Effect}}.$$

The welfare weights ($\bar{g}$ and $\bar{g}(t_0)$) and the marginal cost of public funds (λ) depend on the planner's preferences for redistribution; their estimation is beyond the scope of this paper. Remaining inputs are computed from the data. The income distribution $H(\cdot)$ is recoverable,[19] and Δt and ΔI are given by the schedule reform.[20] The share of jumpers is recovered as described in the previous section. Table 4 presents the inputs for welfare effects of the reform in terms of the marginal cost of public funds $\frac{dW}{\lambda}$.

To interpret these results, it is useful to normalize the sum of the three effects to one.

[19] I use the per capita income distribution before the reform. Although this distribution is not observable for entrants, their income was above t_0; otherwise, they would be in the program. This is enough to compute $H(t_0)$ without ambiguity. To compute $H(t_0 + \Delta t)$, I use the three definitions of NME discussed in the previous section.

[20] I use the average I_0 and ΔI to conduct this analysis.

Table 4: Inputs for the Welfare Analysis of the Reform

Group	$H(t_0)\Delta I$	$I_0[H(t_0+\Delta t)-H(t_0)]$ Pref. Spec.	Bounds	Jumping Effect Pref. Spec.	Bounds
t=70	0.914	0.301	$(0.233, 0.340)$	-0.171	$(-0.239, -0.133)$
t=140	2.366	0.308	$(0.270, 0.317)$	-0.068	$(-0.107, -0.060)$

Note: The first and second rows display the inputs for the welfare analysis for households without ($t = 70$) and with children ($t = 140$).

For every marginal *real* transferred by the reform to households without children (with children), 66 (86) cents are transferred to households that were eligible before the reform (transfer effect); 22 (11) cents are transferred to households that became mechanically eligible because of the increase in the eligibility criteria (threshold effect); and 12 (2) cents are transferred to households that jumped to the threshold in response to the reform (jumping effect). While the first two parts of the marginal *real* correspond to a first-order effect on the utility of beneficiaries, jumping effects only generate a second-order increase on welfare. These households were intially indifferent between being in or out of the program, so that the effect of the reform on their utility is small (even though the effect on the budget is not, since the government needs to transfer $I_0 + dI$ for each jumper). Hence, only 12%(2%)[21] of the marginal *real* transferred to households without children (with children) was lost in efficiency cost of applicants jumping into the program. Households with children could also have jumped to the first threshold, which also affected their transfer. For this reason, the total efficiency cost of the marginal transfer for this group was larger than 2%.

This analysis implies that a welfare-maximizing government should increase the generosity of the program if it values a R$1 increase in consumption for the eligible poor by more than R$1.12 in its budget on the margin. Appendix A.14 performs a similar welfare analysis of counterfactual reforms.

7 Simulation

This section illustrates the imporance of jumping effects more generally. In the case of BF, jumping is the only source of efficiency cost, since the marginal transfer is zero almost everywhere. This is not a general aspect of income-based transfer programs, which could also distort incentives in the intensive margin through changes in the marginal

[21]The lower and upper bounds for these efficiency costs are 10%(2%) and 16%(4%), respectively.

tax/transfer. To consider such reforms, I simulate an economy and then analyze the relative importance of jumping effects and the usual effects for different reforms.

Households have preferences that can be represented by an isolastic utility function:

$$u(c, y; n, m) = c - \frac{1}{1+\frac{1}{m}} \left(\frac{y}{n}\right)^{1+\frac{1}{m}},$$

where and $n \sim logNormal(2.757, 0.5611)$[22] and $m \sim U(0,1)$[23] are assumed to be independent. When solving the household problem (4), applicants face a piece-wise linear schedule $B(\cdot)$ of the form:

$$B(y) = \begin{cases} I + \xi y & if\, y \leq t \\ 0 & if\, y > t. \end{cases}$$

Note that ξ corresponds to the marginal after-transfer income so that $m = \frac{1+B'}{y} \frac{\partial y}{\partial \xi}$ is the income elasticity.

Consider the following decomposition effect of the effect of an arbitrary reform on the budget of the government:

$$\underbrace{B_a(y_a) - B_b(y_b)}_{\text{Total Effect}} = \underbrace{B_a(y_b) - B_b(y_b)}_{\text{Transfer Effect}} + \underbrace{B_a(y_a) - B_a(y_b)}_{\text{Behavioral Effect}}.$$

The transfer effect denotes the desirable effect of the policy: transferring income without behavioral responses. The behavioral effect corresponds to the efficiency cost, i.e., the additional transfers that comes from movements of individuals and yield a second-order effect on their utilities with respect to the change in the transfer. This last effect can be decomposed as:

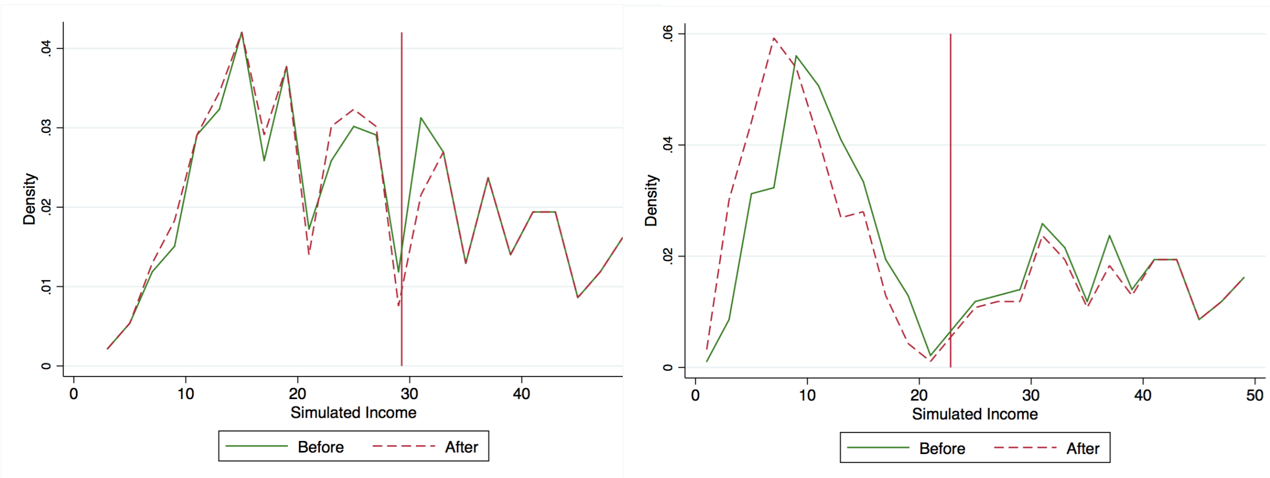
$$\underbrace{B_a(y_a) - B_a(y_b)}_{\text{Behavioral Effect}} = \underbrace{\Big(B_a(y_a) - B_a(y_b)\Big) 1(y_b < t)}_{\text{Elasticity Effect}} + \underbrace{B_a(y_a) 1(y_b > t)}_{\text{Jumping Effect}}. \tag{13}$$

The elasticity effect corresponds to the usual intensive margin response of beneficiaries that are in the anti-poverty program even after the reform. The jumping effect corresponds to the effect on the budget of households that joins the program as a response to a change in the schedule.

[22]According to Mankiw et al. (2009), this fits the US 2007 wage distribution.
[23]Most empirical estimates of taxable income elasticities are in this range.

I consider two reforms. The first changes the marginal after transfer income from .9 to .8, while the second reform changes it from .2 to .1 In both, the pre-reform intercept I_b and threshold t were chosen to match the proportion of beneficiaries to 19.2% of the sample. The post-reform intercept I_a was chosen so that the eligibility threshold is unchanged.

Figures 17a and 17b plot the income distribution before (green solid line) and after (red dashed line) the first and second reforms, respectively. The vertical red line indicates the eligibility threshold of the program t for each reform.



(a) First Reform

(b) Second Reform

Figure 17: Simulated Income Distribution

These figures plot the distributions of income for applicants before (solid green line) and after the reform (dashed red line) in the simulated economies. Panel (a) illustrates a reform that decreases the marginal after-tax income from .9 to .8, while panel (b) shows the distributions around a reform that decreases the marginal after-tax income from .2 to .1.

The area around t is dominated because of the non-convex kink. As expected, both distributions present a missing mass around the kink. The reform has two effects on the distribution. The first corresponds to the usual elasticity effect visualized by the movement within the first bracket ($y \leq t$). Households facing a lower marginal after-transfer income supply less labor and reduce their equilibrium earnings. The second corresponds to the jumping effects, and is visualized by the movement from the second to the first bracket. Intuitively, households that were initially at the second bracket and close to indifferent to the first bracket will jump as this first bracket becomes more attractive.

Table 5 presents the shares of the efficiency cost of each reform coming from the elasticity effect and the jumping effect, as described by equation (13).

Taking jumping effects into account increases the efficiency cost of the first reform by 10%. The limited effect in this case is due to the low marginal tax rates in the phase-out

Table 5: Welfare Analysis in Simulated Economy

Group	Elasticity Effect	Jumping Effect
First Reform	90%	10%
Second Reform	68%	32%

Note: The first and second rows display the inputs for the efficiency cost analysis for the first and second reforms, respectively.

region. Since the schedule is close to flat around the non-convex kink, only very elastic households would jump after a small reform (0.8% in the simulation). The efficiency cost of the second reform is 39% larger once jumping effects are taken into account. Its kink is more acute and generates jumps for a larger proportion of the population (1.1 % in the sample). These jumps also have larger impacts on the government's budget. This analysis illustrates that jumping effects could affect welfare analysis of reforms of tax schedules, even in the absence of notches.

8 Conclusion

This paper computed the welfare effect of a reform in the *Bolsa Família* program, one of the largest transfer programs in the world. To do so, I present evidence that jumping effects are important in this context and provide a theoretical framework that accommodates jumping effects in the welfare analysis. The application of this framework to the Brazilian context indicates that jumping effects account for the entire efficiency cost of the reform, but these costs account for only 12% of the total cost of the policy change.

Simulations of alternative reforms suggest that this jumping behavior affects the welfare analysis of transfer programs, even when the policy also distorts incentives in the intensive margin. As shown by Dodds (2017), this behavior also affects characterization of the optimum income tax in economies with more than one dimension of heterogeneity. The optimal policy formulas presented in the Appendix may be a fruitful starting point for incorporating this behavior when thinking about the design of these nonlinear policies.

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