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Impact evaluation of the brazilian non-contributory pension program Benefício de Prestação Continuada (BPC) on family welfare

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Impact Evaluation of the Brazilian Non-Contributory Pension Program *Benefício de Prestação Continuada* (BPC) on Family Welfare

Abstract

The Benefício de Prestação Continuada *(BPC) program is a non-contributory pension addressed to poor elders over 65 years-old. This paper evaluates its effects on household composition and on labor market outcomes of the elders and their co-residing relatives. We could not capture any sign of changes in the household composition due to the program. However we found decreases in the labor force participation of the elders, indicating that the program makes it possible for these poor elders to retire, what would not be possible otherwise. Also there is a drop in labor force participation of co-residents. However, the effect is heterogeneous and the effect is concentrated for adults over 30 years old, while there is no effect for young adults. When analyzing only rural areas, we observed a decrease in labor participation of elders and co-residents from 18 to 50 years old receiving BPC. We also observe a decrease in child labor.*

Key-words: impact evaluation; regression discontinuity design; cash transfer; social assistance; public policy; labor supply; child labor; school attendance.
JEL Codes: I38, J14, J22, J26, O01

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1. Introduction

Conditional Cash Tranfer (CCT) program have proven to be an important way to alleviate poverty in the developing world. In Brazil, a lot of attention has been given to the *Bolsa-Escola /Bolsa-Família* programs, which provide the benefits to poor families conditional on children's school attendance and health care visits. The *Beneficio da Prestação Continuada* (BPC) program, however, is addressed to disabled people and to the elders. Despite of being carried out in Brazil for more than 10 years, few studies evaluated the effect of this program upon family structure, education, child labor, and other spillover effects.

The BPC program is a non-contributory pension scheme which provides a minimum wage for elders and people with disabilities that make them unable to live on their own or work. To be eligible, the person must be over 64 years old or prove to be incapable to work, besides attesting a per capita family income no greater than 25% of the current minimum wage. It is addressed therefore to very poor families.

Barrientos and Lloyd-Sherlock (2002) summarize the effectiveness of non-contributory pension schemes for some countries. Usually, the programs tackle poverty and vulnerability prevention at the old age. But other effects arise from these pensions: it promotes elders' status within the household, it prevents extreme poverty and it avoids the persistence of poverty throughout the generations by means of investment in physical, human and social capital.

Most of the studies appraise the effect of the non-contributory pensions on reducing poverty and inequality, mostly using descriptive analysis. For the developing world, there are studies conducted in Argentina (Bertranou and Grushka, 2002), Bolivia (Martinez, 2005), Brazil (Schwarzer and Querino, 2002); Barrientos, 2003), Costa Rica (Durán-Valverde, 2002), Namibia (Schlegerger, 2002), Zambia, among many others. Barrientos (2003), using probit estimates shows that the probability of being poor in a household with a beneficiary of non-contributory pension is reduced in 18 percentage points in Brazil and in 12.5 percentage points in South Africa. Nevertheless, endogeneity problems concerning the income sources and possible changes in family structure due to the non-contributory pension were not taken into account.

Other relevant questions can be posed about these programs. The additional income may have distributional effects within the family, affect the labor supply of the household, increase educational level of young family members, change the family structure, etc.

In Bolivia, there is the *Bono Solidario* (Bonosol), which is a transfer for every person over 65 years-old. The study of Martinez (2005), using regression discontinuity designs, concludes that there was a significant increase in food consumption for beneficiaries. For very poor households, transfers may increase production through investments in food production or other small scale productive activities. This additional income can be invested in human capital as well.

The South African program is perhaps the most studied one. Case and Deaton (1998) is a benchmark study which investigated the redistributive effects of a non-contributive pension for elderly people in South Africa. Several variables were tested: food consumption, clothing, housing, schooling, transportation, health, remittances, insurance and savings. First the study deals with the determinants of being a beneficiary, through probit, ordinary least squares, and instrumental variables methods, aiming to identify whether the income and household demographic variables are truly exogenous – an hypothesis which could not be rejected. Then the study focuses on the redistributive effects of the benefit, finding that there are redistributive effects on food, schooling, transfers, and savings. Other interesting results are that, in general, the expenditures made with the pension receipts were quite similar to those of non-pension incomes. Also, male-headed households have different consumption patterns than women-headed households.

Duflo (2003) evaluates the same program, but focusing on the health and nutrition of grandchildren, measured by anthropometric indicators (weight-for-height, and height-for-age). The identification procedure has to take into account the fact that children living with pension recipients are relatively disadvantaged on average. Her identification strategy considers that weight-for-height is much more sensitive to changes in the environment than height-for-age. Then, she compares the weight-for-height of children living in households without eligibles, with an eligible man, and with an eligible woman (after controlling for the presence of a man or woman who is not old enough to be eligible). The difference is normalized by the difference in the probability of receiving the pension across these two groups, finding that pensions received by women increase the weight-for-height of girls (but not boys).

Edmonds, Mammen, and Miller (2005), using a discontinuous regression approach, study the effects of the South African program in living arrangements for elderly black women. They assume that changes in living arrangements with non-beneficiaries are smooth, and then compare them to living arrangements of households with eligible women by exploring the discontinuity in the age eligibility rule (women become eligible at the age of 60). They find no evidence that the additional pension income leads to an increased propensity to live alone. Instead, the pension leads to a decline in the co-resident women in their 30s (who can work away), and an increase in the presence of young children (less than 5 years old) and women whose age suggests they are their sons and daughters.

Paulo (2008) studies the effect of the BPC program on living arrangements using difference-in-difference estimation for a cohort of possible beneficiaries. Her findings suggest that beneficiaries are more likely to live alone than non-beneficiaries.

Case and Deaton (1998) argued that the distortionary effect of cash transfers on labor supply is insignificant in developing countries with high level of under-employment and unemployment. Particularly in Brazil, this effect is very unlikely to occur, particularly for extremely poor families, for whom the cash transfer is not enough to fully cope their monthly needs. A hazardous effect is the rise in the reservation wage of family members who are job-seekers. Reis and Camargo (2005) shows that this effect seems to be plausible, especially for unskilled workers.

Other studies dealing with the negative effects of cash transfers on labor supply are Bertrand, Mullainathan, and Miller (2003) for South Africa, and Carvalho Filho (2008a) for Brazil.

Some other papers focus on the relationship between pensions and child labor and education. Edmonds (2006) compares South African households receiving the pension with those which are about to receive the pension, finding an increase in school attainment and a decrease in child labor. Reis and Camargo (2007) show through a multinomial logit model that Brazilian pensions tend to improve the probability of youth to attend school. Carvalho Filho (2008b) and Kruger, Soares and Berthelon (2006) show that rural pension have increased the enrollment rate and diminished youth's participation in the labor market in Brazil.

Carvalho Filho (2008b) uses a Brazilian social security reform to estimate its effect on child labor and enrollment rates of children (10 to 14 years old). The reform affected some children but not others. Then, the effects are identified from the difference in the outcomes of children affected or not by the reform. Old age benefits increase the enrollment rates of girls by 6.2 percent, with smaller effects for boys, and reduce children labor supply. Girls labor participation drops remarkably only when the benefits are received by females. This result is quite similar to Duflo's for South Africa. But in Brazil, male benefits reduce boys' labor supply and increase boys' enrollment more than they do for girls. It highlights the importance of the collective models (Browning and Chiappori, 1998), which could theoretically account for these sorts of peculiarities in the household setting.

Clearly, there are several studies on the effects of old age cash receipts on poverty, inequality, child labor, schooling, living arrangements, and labor supply. Also, the transfers have proven to have important spillover effects within the households.

This paper presents some evidence on the effects of the BPC on labor and educational outcomes of beneficiaries and their co-residents. The next section details the program and its expected effects. Section 3 details the database. Section 4 describes the identification strategy. Section 5 presents some results concerning household composition, labor force participation, worked hours, child labor, and school attendance. Section 6 shows possible anticipation effects and section 7 concludes.

2. The BPC program and expected effects

Enacted in the 1988 Constitution and regulated in 1993, the BPC benefit started being paid in 1996. The Ministry of Social Development (MDS) is in charge of the coordination, implementation, financing, and monitoring of the BPC. Its operationalization is the responsibility of the National Institute of Social Security (INSS). They receive the applications and make decisions whether to pay or not the benefits, checking age and income. Once approved, they pass the resources along the authorized banking institutions. The municipalities are responsible for identifying and advising potential candidates to receive the BPC.

Actually, the potential beneficiary (or any legal representative) is responsible for applying for the benefit at an INSS agency. Documentation includes income declarations of the beneficiary and his family, all living within the same household. Once approved, the beneficiary receives a magnetic card, which can only be used to withdraw the benefit at the authorized bank.

At the start of the program, the eligibility age to receive the benefit was 70 years old. In 1988 this age was reduced to 67 years old, and in 2003 to 65 years old. The benefit may be paid to every old-aged person with a per capita family income no greater than 25% of a minimum wage (around US$ 2.5 a day in 2012) and with no social security aid or any other retirement plan fund. There can be more than one beneficiary in the same family. In this case, the second applicant must be disabled or older than the cutoff age, and the income of the first beneficiary will be included in the family income calculation - but since 2004 this rule is no longer in place. Families with beneficiaries from other governmental social programs can also receive the BPC, once the income eligibilities are met.

The program had few beneficiaries in the beginning. The evolution in the number of recipients (issued benefits) according to administrative records is shown in Table 1.

In 2008, the BPC budget was approximately US$ 8.2 billion, while the *Bolsa Familia* budget was US$ 4.4 billion. The BPC program benefited nearly 3 million people (elders and disabled), while *Bolsa Familia* benefited more than 40 million people (more than 10 million families). Since BPC pays a minimum wage for each beneficiary, its budget is larger compared to other programs.

Based on PNAD 2006 survey[1], the largest monthly value received by a single beneficiary of *Bolsa Familia* is below R$150 (US$ 75 in 2012 currency). So, the amount of the BPC benefit (R$350, or US$175) is about 2.3 times larger than the largest transfers of *Bolsa Familia* program. Therefore we may expect important effects of this income transfer on inequality and on the beneficiaries' quality of life.

BPC is supposed to be addressed to very poor families. Preliminary analysis from PNAD 2006 shows that 65.9% meet the income eligibility criterion, and, from those, 58.8% are women[2]. That is, 65.9% of the 3,084 beneficiaries identified in the sample have a family *per capita* income of less than 25% of the minimum wage. If we consider a family income of 50% the minimum wage as the poverty line, then 83.9% of beneficiaries are poor. About 94.5% of the beneficiaries belong to families with an income per capita less than a minimum wage.

In order to measure the effect of the BPC on the elders' labor force participation, this paper compares the elders who benefited from the BPC to those who did not. The BPC may allow

[1] The Brazilian National Households Survey (PNAD) is carried out annually since 1967. It is a micro database, including a wide variety of socioeconomic information of the household and dwellers - further explored ahead.

[2] 59.73% of recipients are women.

these people to retire from the labor market, which would not be possible otherwise. Therefore we expect a lower participation rate of the elders in the labor market. Some spillover effects could be associated with the benefit. The co-resident would be more prone to leave the labor market. Situations like these occurs when the co-resident is the only provider of the household, when the individual do not have a good job and the extra income allow him to look for a better job and when he quits his job to study.

3. Data

Data source is the annual household survey (PNAD) carried out in Brazil for the period of 2001-2008 (PNAD). Some years of the survey include specific supplements with thematic questions about health, child labor, fertility, social programs, among others. In collaboration with the Ministry of Social Development - MDS, the PNADs included a special supplement on the access of income transfers from governmental social programs in the years of 2004 and 2006, including in the questionnaire new questions regarding the *Bolsa Familia* program, BPC, and the Child Labor Eradication Program (PETI), among others.

However, this annually conducted survey do not include specific questions about social programs every year. Even for those years in which the information is available in a special supplement – 2004 and 2006, it refers to the household only. So we can identify through these supplements whether the household receives benefits from a social program, but not the beneficiary within the household.

Even though we face the problem of not having information annually, we can still identify the program in which an individual is beneficiary through the eligibility criteria, such as wage, age, household income, household composition, and the amount of money paid by each governmental program. This approach can then be used annually in PNAD, even in years without the special supplement.

The amount paid by the social programs is computed in the variable coded V1273, described as: "savings account[3] and other financial applications, dividends and other income". It is very

[3] In Brazil, there is a traditional and conservative financial investment called *"caderneta de poupança"*, which was translated here as 'savings account'. This investment is a very low risk one, with values insured by the government, and monthly profitability established as 0.5% + TR. The TR is an interest rate calculated by the government and indexed by

unlikely to find shareholders and people who receive interest from any financial application as beneficiaries of social programs. Moreover, the amount paid by the social programs are known, and through the values declared in this variable we can deduce which program the individual is receiving.

Barros et al. (2007) use the typical value transferred by each social program from the government (BPC, *Bolsa Família*, *Bolsa Escola*, *Bolsa Alimentação*, *Cartão Alimentação*, *Auxílio Gás*, and PETI) to identify beneficiaries from each program. All individuals receiving exactly one minimum wage were identified as BPC beneficiaries.

The combination among the typical values is crucial to identify individuals who may be beneficiaries of more than one program simultaneously. In the 2006 PNAD, for example, using the special supplement, we can observe 18,226 households receiving the *Bolsa Família* and 2,911 receiving the BPC. From these 2,911, almost 20% also receive the *Bolsa Família* stipends.

In Table 2 there is an example of the disaggregation procedure proposed using values for the variable V1273 (interest and other income sources) in the 2004 PNAD for households that have at least one BPC beneficiary. We observe that the largest frequency occurs at R$260. That was the minimum wage in 2004, indicating that those are beneficiaries of the BPC program. However, other values may also indicate BPC beneficiaries who also receive other social programs stipends. For example:

267 = 260 + 7 (BPC + *Auxílio Gás*) , and

282 = 260 + 15 + 7 (BPC + *Bolsa Família* + *Auxílio Gás*), and so on.

After applying the procedure to identify BPC beneficiaries, we observed that the proposed method identifies more beneficiaries than the PNAD supplement and less than the government official records did. The BPC is not a very known program. Elderly BPC beneficiaries are low-income people and, in general, low educated and it is possible that they get confused in differentiating the BPC benefits from the regular government retirement pensions addressed to insured workers. Many BPC beneficiaries could have declared themselves as pensioners, and not as BPC beneficiaries. The agency where the beneficiary claims the benefit is the INSS, also

the average value of the interest rates of private sector´s Certificate of Deposits. This investment is popular among low income investors. There were some minor changes in the profitability rule for this investment in 2012.

responsible for standard retirement pensions, and the card the beneficiary receives to withdraw the money at his bank branch does not have any sign or indication of “BPC”, giving him the impression that indeed he receives a regular social security pension. Soares et al. (2006, p.17) also discussed this issue.

However, since 2004, when a bill regarding the rights of the elders was passed, the program became more popular. This can help explain the rise in the proportion of elderly beneficiaries from 2004 to 2006, while in the official records this proportion roughly remained steady. Moreover, the PNAD survey was not designed to find such specific groups of people based on values of their income. The family’s income may also change from the time they receive the benefit and the time the sample in PNAD is collected. So it is not a surprise that figures obtained by our procedure and the figures reported by the Ministry differ.

4. Identification strategies

The main strategy for evaluating the BPC is to use the discontinuity that the age eligibility rule creates in the probability of being treated and, therefore, in the outcomes. But before providing the estimates for household composition variables, we perform some tests on the sample to know if there are enough conditions to characterize a discontinuity design.

The ideal design for the statistical evaluation of a program (or treatment) is the experimental one, where the treatment is randomly assigned with *ex-post* evaluation of those who received the treatment (treatment group) and those who did not receive the treatment (control group). Usually then, the treatment group is selected through a non-experimental design, according to the eligibility criteria. Our methodology takes this into account, in a way that our data can be “corrected” to a quasi-experimental design.

We believe the best approach to be used to evaluate the impact of BPC is the regression discontinuity design (RDD) and we use several econometric models to explore the 'discontinuity'. The discontinuity arises because poor elders become eligible when they turn 65. This is the current cutoff age, changing over time: it was 70 from 1996 until 1997, 67 until 2003, and 65 until nowadays. Besides regression discontinuity, other methods were used to evaluate the impact of BPC in this study, such as: Propensity score Matching, Difference-in-differences estimator, and some variations of the RDD method. A summary of those approaches can be found in Appendix B.

Based on the described methods that were applied to our study, we can say that they are all complementary to each other. The difference-in differences estimator (DD) uses the change in the eligibility age in 2004, the propensity score matching explores the difference between treated and who should be treated, and the RDD estimations explore the discontinuity in the probability of being treated on both sides of the discontinuity.

5. Results

5.1 Preliminary Data Analysis

One of the goals of this paper is to explore the discontinuity present in the age-eligibility rule. To understand how we exploit the discontinuity in the eligibility age we present now some statistics focusing on the discontinuity generated by the program rule. Using the 2006 PNAD we describe in Figure 1 the number of beneficiaries. Clearly there is a sharp increase in the number of beneficiaries at the age of 65. It is good to point out that this figure includes the disabled ones as beneficiaries. Only those with more than 10 years of age may be included in the program, and the occurrence of disabled beneficiaries seems to be uniformly distributed, roughly speaking, with an important shift at the age of 65, where the elderly become eligible. In Figure 2 we present the proportion of beneficiaries in the PNAD 2006 sample, sorted by age. We can observe the proportion of BPC recipients for each age. Once again, it remains clear the increase in the number of beneficiaries at the age of 65.

Figure 3 shows how some of the outcome variables analyzed behave close to the discontinuity in age, for treated and non-treated households. a, b, c, and d refers to school attendance rate, child labor, labor force participation for the co-residents, and labor force participation for the elders, respectively. It depicts local averages for each outcome by their proximity to the cutoff age, smoothed by a 4th order polynomial epanechnikov kernel function. Circles are averages of the bins of a histogram. At point zero income-eligible individuals become age-eligible to receive the BPC payments, but some do receive the benefit while others do not.

We do not observe any clear drop or rise in the figure. Maybe on graphs b and d there is some decline but, again, that is not clear. One possible reason for that is the presence of non-recipients over the cutoff age who could drift the effect, if any, towards the opposite direction.

It is important in discontinuity designs to check the covariates' behavior next to the cutoff point. So Figure 4 shows some covariates near the cutoff point. Graph 'a' addresses the number of members in the household. We cannot observe any clear change on that covariate. Two alternative hypothesis may apply to household composition: the first one is that poor elders have their independence improved with the benefit and they move out; and the second one is that relatives can move in to the household when facing unemployment or financial hardship. We will explore the household composition later. On the remaining graphs we cannot observe changes due to the benefit either.

5.2. Discontinuity validity tests

As the program design allows the existence of three different groups: participants, eligible non-participants and non-eligibles, we check for the validity of the discontinuity exploring differences and similarities between these groups. A first check is how the presence of an age-eligible individual affects the probability of treatment of the household. The predictions of a logit model are depicted in Figure 5.

We can see shifts in the probability of treatment at the ages of 65 and 67. The most important shift is that at the age of 65, the current eligibility age to apply for the benefit. Therefore the presence of an age-eligible individual increases the probability of treatment. This is one of the underlying hypothesis for a valid RDD: age must be correlated to the probability of treatment.

A second check is the "randomization" of the treatment. Considering those eligible households, we must check if systematic differences arise between the groups on both sides of the discontinuity. As known, no systematic differences between the groups arise when the treatment is assigned randomly. In this case, the absence of significant differences in covariates is important evidence that the treatment was not assigned in any systematic fashion. In this case, a difference of averages across both groups would be enough to identify the effect of the program. Along with Figure 4, Table 3 helps comparing the covariates, checking whether the averages of the characteristics unaffected by the transfer are smooth over the cutoff.

In Table 3 we can observe that most of the characteristics of the household were smooth over the cutoff. The only unexpected differences are the presence of Bolsa Familia stipends, highest schooling level within the household, male recipients (or oldest member of the household), and average schooling level of the oldest member. However in some cases (schooling levels, for example) the differences were not so expressive, despite being significant. Households with Bolsa Familia beneficiaries' and those headed by males are more frequent under the cutoff age.

5.3. Results for Household Composition

Several methods and procedures were used to evaluate possible changes in the household composition. Details on the estimation of the models presented in Table 4 and the following tables can be seen in Appendix B. In Table 4 we show the results for household composition.

Basically we use the changes in age-eligibility rules to identify the intention to treat effect in the Difference in Difference (DD) model. In the Sharp Regression Discontinuity Design (SRDD), Regression Discontinuity Design (RDD), and Local Linear Regression (LLR) models we explore the discontinuity in the probability of being treated at the cutoff age. The outcomes analyzed are: a dummy if the elder lives alone or with his/her spouse, the number of members between 18 and 29 years-old, and the number of members between 30 and 49 years-old.

The estimates may be sensitive to controls, models, periods, and bandwidths so we kept the sample as similar as possible through the different models. All variables used as controls in the regressions are in Appendix A. The variables used vary between the models.

DD and SRDD estimates in Table 4 use the effect of becoming eligible instead of becoming treated. There was no significant effect in this case. The propensity score compares treated and non-treated (eligible) elders close to the cutoff age. We found an increased probability to live alone or with spouse for the treated elders in comparison with the non-treated eligible elders and an increase in the number of members between 30 and 49 years old in beneficiaries' households. When we compare treated individuals with non-eligibles from below the cutoff point we found no differences in the household composition. So the evidence suggests that being eligible to the program does not alter the household's composition, and there is not much indication that being effectively treated attracts or repels people from the household. These results are important because we can expect spillover effects on the co-residents' behavior.

5.4. Results for Elders' Labor Force Participation

We expect a decrease in the elders’ labor force participation. So, here we address the probability to participate in the labor force (work or look for a job in the current month or week). First we check if there is any effect for the group of eligibles in the two first columns. Results are in Table 5. We find no evidences of any decrease in the probability of participating in the labor force or working more or less hours per week.

However when we look at Propensity Score Matching (PSM) estimates, comparing income and age-eligible individuals (treated and non- treated) we observe a decrease in labor force participation for the elders receiving the benefit. The same occurs when we compare elders above and below the cutoff age, in RDD and LLR models. So this is an important result. We must keep in mind that the BPC beneficiaries might not have access to financial aid, and the possibility of retiring from the labor market presented by the BPC is very welcome, improving the elders' wellbeing. However, as we will see in the next section, this expected effect of labor force participation decrease also spills over to other members of the household.

5.5. Results for Co-residents' Labor Force Participation

Table 6 presents the labor force participation for two samples: i) for individuals between 18 and 49 years-old and ii) for those between 18 and 29 years-old. The dependent variable indicates whether the person is working or looking for a job during the current month (week) of the survey. What we can observe is that the inclusion of people between 30 to 49 years-old results in more precise and more negative estimates. So probably there is an heterogeneous effect of the cash transfer, in which older people tend to participate less in the labor market the higher is the household income. Among young people, the effect of the transfer is practically zero.

If we compare co-residents living with treated elders and co-residents living with eligible non-treated elders, we find a significant decrease in the probability of this co-resident to be working or looking for a job. This is the result of the propensity score model in Table 6. If we consider the RDD and LLR models, we observe that the effect is negative and significant when we include individuals between 30 and 49 years-old.

We found no effect on the weekly worked hours for the working co-residents. The results are presented in Annex A.

5.6. Results for Child Labor and School Attendance

Here we evaluate the labor force participation of children living in the elder's household. In some years of the survey we had information on children working at the age 5 or more, but others labor force information started at age 10. To be consistent we use data for children 10 years-old or more working or not.

We estimated the models for children between 10 and 15 years-old. The legislation in Brazil prohibits children working under the age of 16, with the exception of professional training and apprenticeship. The constitution also prohibits any form of night and hazardous work for children under 18 years. Considering only the effect of elders becoming age-eligible for the benefit we do not observe any clear trend in Table 7. When we consider the effectively treated, we observe a decline in child labor when we compare treated households with those below the cutoff age. At the age of 15, they may have completed 9 years of basic education. At this point some abandon the studies, while some carry on with the subsequent 3 years of secondary education.

Usually, in the literature, declines in child labor are associated with increases in school attendance. However we could not observe any increase in education. This is not a great surprise in Brazil as there are many teenagers neither studying nor working and at the same time many are studying and working, i.e., study and work are not mutually exclusive. Another possibility is that, as we kept in the sample only income-eligible households and considering the significant presence of beneficiaries above the income threshold, many BPC participants could have been excluded and the whole effect of the program could not be picked up. Also, since more than 97% of the kids are in school in Brazil, there is not much room for improvements.

6. Checking "anticipation effects"

The anticipation of the BPC effect would require both, that people are aware of the program and that they have credit to be able to stop working before receiving the benefit. Since this program is targeted to the poor, we do not believe that first, individuals are very aware of the program and second, they can survive without their wages once the access to credit sources is very restricted.

The labor force outcomes assessed in this paper are related to income. So anticipatory changes in these outcomes means anticipatory changes in income, and reducing income before anyone in the household become eligible for the treatment would be very difficult to sustain as they would need credit, and the lack of credit in Brazil is notorious, especially for poor people (with the practice of prohibitive interest rates). Due to that, we believe that these anticipatory changes are very unlikely to occur. However we now try to check if there is any indication of anticipation effect.

To do that we narrow our period of analysis to the period from 2002 to 2007 and check the intention-to-treat effect across narrower age ranges. Up to 2003 the minimum age to receive the benefits was 67 years old. From 2004 on the age changed to 65 years old. So, we use this change to check if the impact on 2004-2005 was stronger due to the lack of anticipation effect as the 65 and 66 years old individuals did not have time or information about the program, i.e., if there is no anticipation we would expect an increase in significance only for the period of 2004-2005, just after the eligibility cut off age changed from 67 in 2003 to 65 in 2004. For all the regressions analyzed in Table 8 and some in Table 9 we can observe an increase in significance for the 2004-2005 period. We know that from 2004 onwards the number of eligibles receiving the benefit has increased significantly and this change in the elderly age to receive the benefit was quite unexpected.

We observe an increase in the number of household members in 2002 and 2003, with ages between 0 and 10 years-old and between 31 and 59 years-old.

It is worth emphasizing that when we narrow the analysis to pairs of years the year dummies will lose their role of controlling for macroeconomic factors which could have particularly affected the outcomes in some specific years.

Cameron and Cobb-Clarke (2008) argue that co-residency can be used as a way of social protection. They argued that elders could co-reside with their adult children especially when they are unhealthy or in bad financial circumstances. Here, however, the indication is in the opposite way. Other members of the family seem to move in when facing difficulties. In 2002 and 2003 Brazil was facing an economic downturn with high unemployment. Moreover, unemployment is higher among unskilled workers (Reis and Camargo, 2005). Looking at the age range where there was significant effect, we may conclude that adult children and their sons were moving into the elderly household.

Other interesting results are the increase in the number of 10 to 17 years-old members in 2004 and 2005, a decrease in the number of members between 18 and 30 years-old in 2006 and 2007, and a decrease in the number of elders in 2004 and 2005.

As the BPC is addressed to the elderly, it is expected that somehow the transfer affects elderly labor supply. However, the debate relies on whether this behavior “spills over” towards other family members. If the elderly BPC recipient keeps all the money for himself we would not need to worry about this. Nonetheless, previous studies indicate that most of the cash transfer is totally shared within Brazilian households. In this case we should investigate possible effects of the BPC upon co- residents.

Table 9 shows the results of labor force participation for co-residents by different years and age ranges. The only significant effect is a decrease in the labor force participation for members aging between 31 and 59 years-old in 2004-2005. There is also an effect for less participation of the elder in 2006-2007.

7. Conclusions

This study uses an impact evaluation methodology to analyze the non-contributory pension program BPC on family welfare. The program provides a minimum wage to individuals 65 years-old or more with a per capita family income no greater than 25% of the current minimum wage. It means that the grant is addressed to very poor households and it is well targeted. We also verified that the amount transferred can be considered large when compared to other Brazilian social programs. Therefore, we expect important shifts in life quality of recipients and their families.

As the literature shows, social grants are associated to less poverty, less child labor, greater educational achievements of children, and better nourishment. The effects may vary according to the conditionalities and peculiarities of each program, but focusing on programs addressed to the elderly we can observe effects on households’ living arrangements, child labor, educational outcomes, remittances, and labor supply, among others. Hence our study assesses whether these effects can be found in Brazil.

The outcomes analyzed were the households' living arrangements, the labor force participation and weekly worked hours for elders and co-residents, and labor force participation and school attendance for children and teenagers co-residing with elderly participants. All households are income-eligible, and as the eligibility for the program is based on the age of the elder, we exploited the discontinuity in the probability of being treated originated by the age-eligibility rule. Until 2003 the cutoff age to participate in the program was 67 years-old and was reduced to 65 years-old in 2004. This change in 2004 was part of the "Statute of the Elderly" which addresses the rights of the elderly. Thereafter we verified an important shift in the proportion of eligibles taking up the benefit. Hence we concentrated our analyzes from 2001 onwards.

Primarily we looked for changes in the living arrangements in recipients' households. We found some evidence of increased probability of elders living alone. We considered elders living with the spouse as living alone as well. Some authors argue that old-age pensions could be associated to more independence from the offspring, the spouse, and relatives which could influence their decisions. As our estimates are short run effects, future studies with longitudinal data can pick this effect up if it really exists.

When we look at the number of co-residents, we found an increased number of members between 30 and 49 years-old in beneficiaries' households.

The results show decreases in the co-residents' labor force participation due to the pension. There is a large decrease in the elderly labor force participation just above the cutoff age and some decrease for adults of more than 30 years of age, but no effect for young adults between 18 and 29 years-old. People with more than 30 years old are very likely to be the adult sons of the beneficiary who may be in charge of the elder. This sort of relationship may be one explanation of this decrease in the labor force participation at that age.

Also there could be some intergenerational human capital transfer. Beneficiaries could become more supportive of their grandsons' studies, implying a better school attendance rate of their grandsons and less child labor. We found no effect for school attendance, but we found significant effects for reducing child labor, especially for the younger ones. In this study we could only evaluate the effect upon children co-residing with beneficiaries, but possibly there is a spillover for households of relatives, what lead us to think that the benefit is actually larger than the one we have estimated.

All this evidence sheds light on the importance of income for welfare. Providing income for the poor has shown to be an important way to alleviate poverty, but along with the cash comes context-dependent effects on labor supply, living arrangements, and even unintended consequences. The complexity of the effects identified also stresses the importance of addressing the heterogeneity of it, attempting to disentangle macroeconomic effects and family circumstances from the transfer effects. It makes us wonder that there are still many other latent aspects of these transfers to be uncovered.

8. Figures and Tables

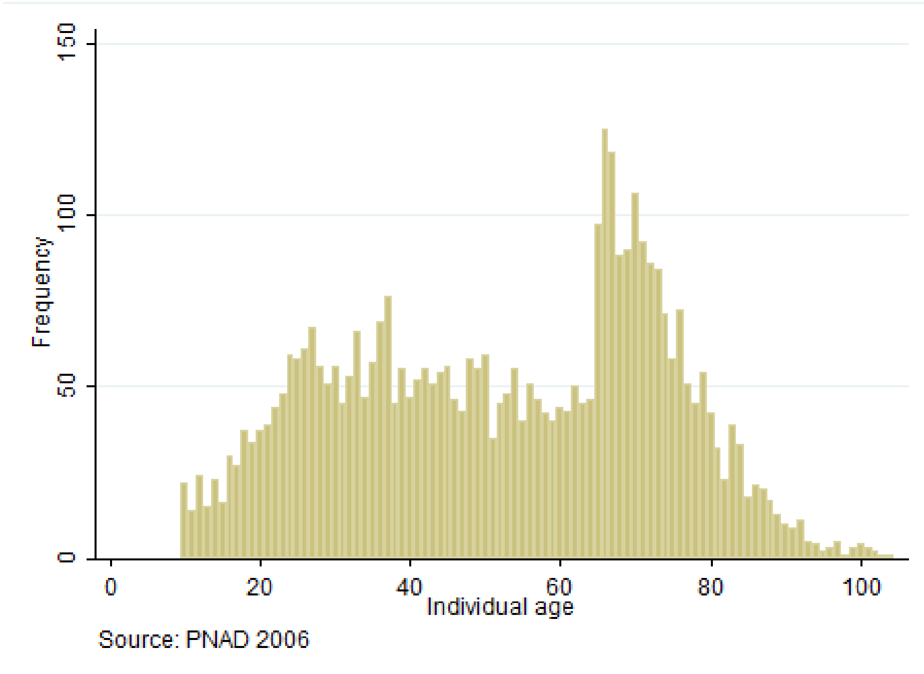


Figure 1: Beneficiaries by age

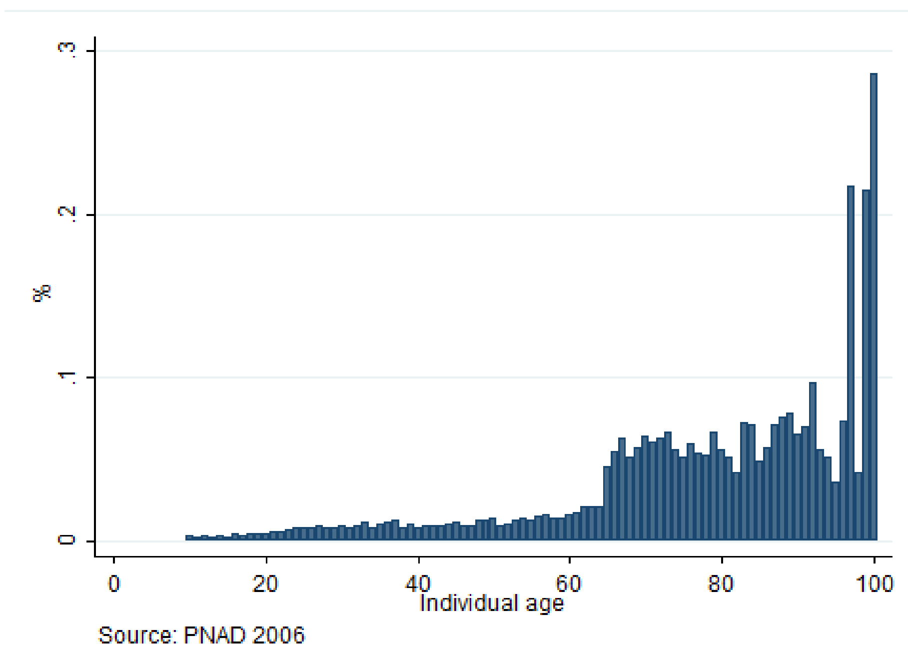
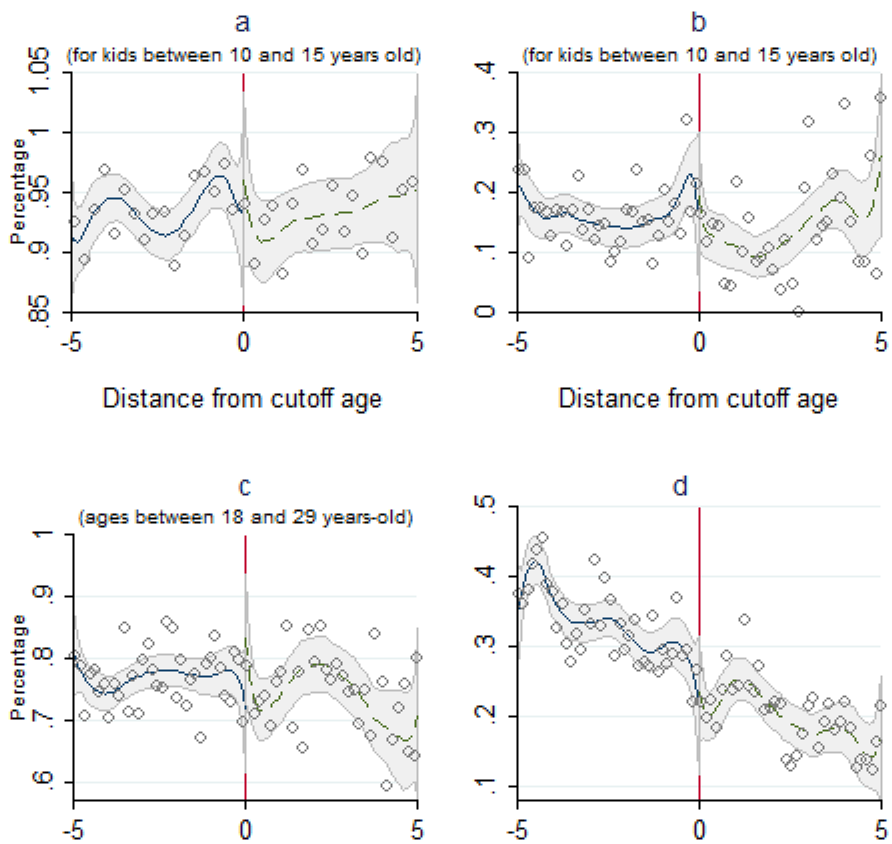


Figure 2: Proportion of BPC recipients for each age



Note: Income-eligible households included only.

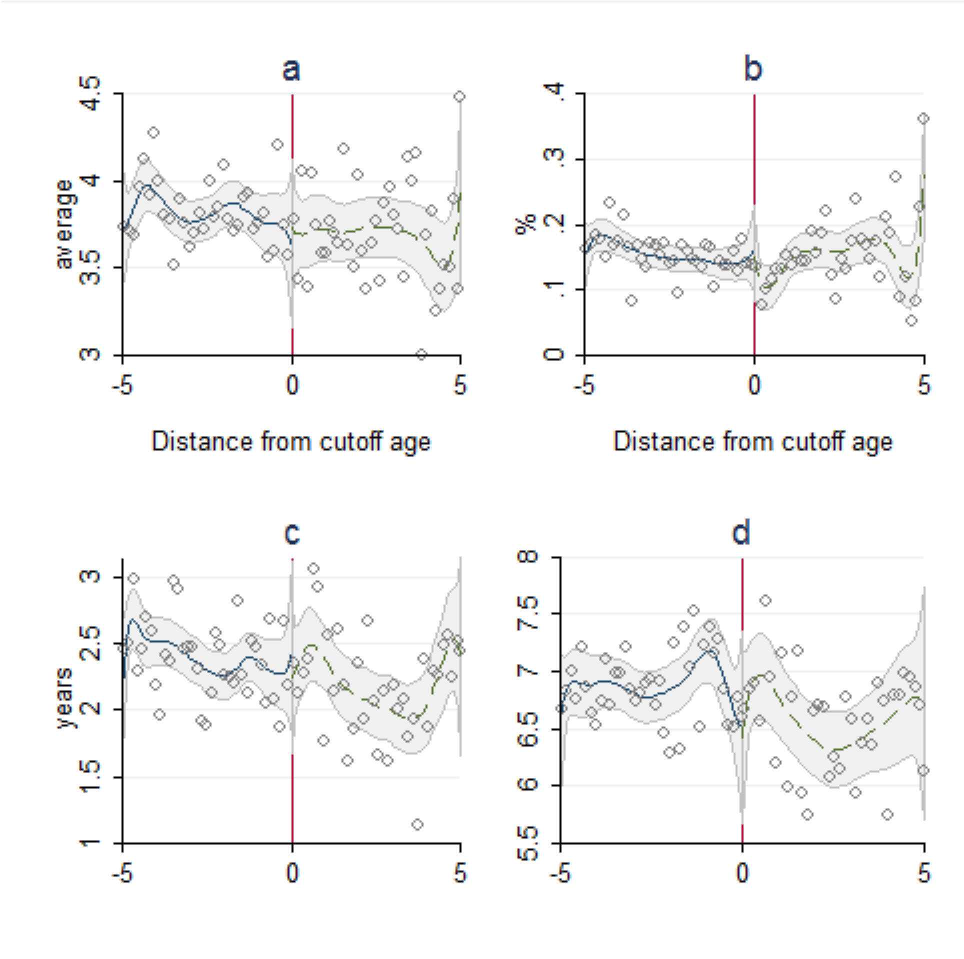
a) school attendance,

b) child labor,

c) labor force participation (co-residents),

d) labor force participation (elders)

Figure 3: Outcomes by distance from cutoff age



Note: Income-eligible households included only.

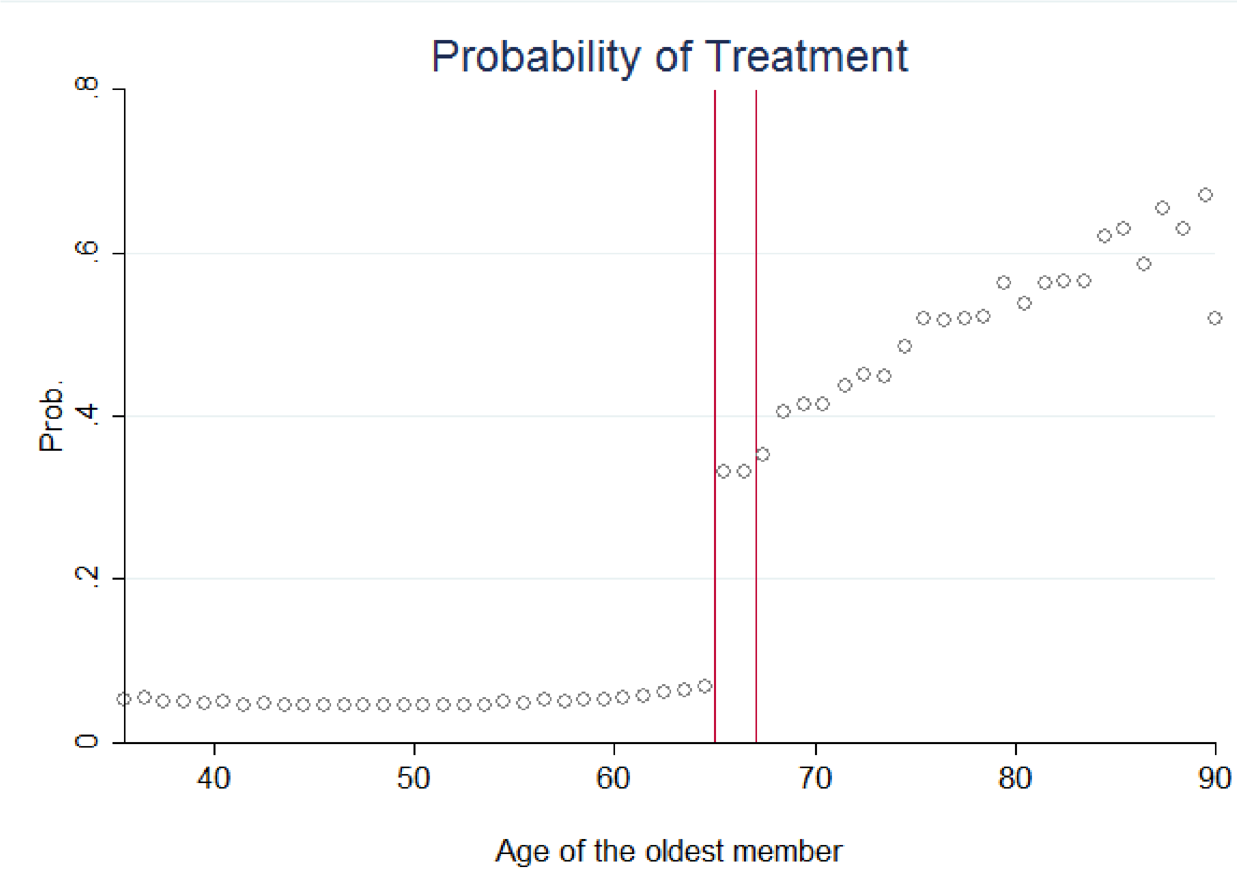
a) number of members in the household,

b) proportion of rural households,

c) schooling,

d) highest schooling of a member within the household

Figure 4: Covariates by distance from cutoff age



Note: income-eligible households included only.

Figure 5:

Probability of a treated person within the household by the age of the oldest member

Table 1: Evolution in the number of BPC recipients

Year	Total	Elderly	Disabled
1996	346,219	41,992	304,227
1997	645,894	88,806	557,088
1998	848,299	207,031	641,268
1999	1,032,573	312,299	720,274
2000	1,209,927	403,207	806,720
2001	1,339,119	469,047	870,072
2002	1,560,884	584,597	976,287
2003	1,687,519	659,433	1,028,086
2004	2,061,013	933,164	1,127,849
2005	2,277,365	1,065,604	1,211,761
2006	2,473,696	1,180,051	1,293,645
2007	2,680,823	1,295,716	1,385,107
2008	2,934,472	1,423,790	1,510,682

Source: IPEAData

Table 2: Values for variable 'V1273' for individuals in households declared to house BPC beneficiaries in the 2004 PNAD

Amount (R$)	Frequency
260	1625
262	1
265	1
267	11
275	17
280	2
282	10
285	1
290	10
297	3
300	2
305	7

Source: 2004 PNAD.

Table 3: Means for household characteristics near the cutoff point

Variable	under cutoff	above cutoff	
Characteristics of the household			
Rural	0.156	0.149	
Receiving Bolsa Família	0.14	0.113	*
Receiving Peti Rural	0.007	0.005	
Receiving Peti Urbano	0.01	0.011	
ReceivingVale Gás	0.014	0.012	
Highest schooling level within the household	6.874	6.597	*
Number of members of the household	3.817	3.701	
Income level, excluding BPC (max.: 100)	21.831	21.091	
Income level (max.: 100)	23.663	31.66	*
Characteristics of the oldest member			
Male	0.464	0.415	*
White	0.372	0.386	
Black	0.098	0.104	
Yellow	0.006	0.007	
Brown	0.521	0.503	
Age of the oldest member	62.956	67.602	*
Schooling level of the oldest member, in years	2.403	2.21	*

Notes: * : significant at the 1% level. Income-eligible households included only. Age range is set to [c-5, c+5], where c is the cutoff age. Dummies for states were compared and no difference was significant.

Table 4: Household composition estimates

	livealone				
	DD	SRDD	RDD	LLR	PSM
Effect	0.0086	-0.0137	0.0044	0.0740	0.0505**
	(0.0282)	(0.0222)	(0.0547)	(0.1489)	(0.0237)
controls	yes	yes	yes	yes	yes
age interval	[60, 67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002, 2005]	[2002, 2005]	[2001, 2008]	[2001, 2008]	[2001, 2008]
N	3326	3424	7516	1565	2639
R^2	0.3749	0.3688	0.3708		0.1874
	Number of members between 18 and 29 years-old				
	DD	SRDD	RDD	LLR	PSM
Effect	0.0800	0.0469	-0.1215	-0.0709	-0.0300
	(0.0738)	(0.0571)	(0.1394)	(0.4531)	(0.0466)
controls	yes	yes	yes	yes	yes
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	3326	3424	7516	1920	2639
R^2	0.1971	0.1867	0.1952	-	0.1874
	Number of members between 30 and 49 years-old				
	DD	SRDD	RDD	LLR	PSM
Effect	0.0300	0.0473	0.2152*	0.5034	-0.0742
	(0.0631)	(0.0509)	(0.1262)	(0.4588)	(0.0466)
controls	yes	yes	yes	yes	yes
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	3326	3424	7516	1795	2639
R^2	0.1622	0.1939	0.1683	-	0.1874

Notes: standard deviations in parentheses. All estimates refers to the household. 'age' is the age of the beneficiary or the oldest members in the household. In 2004 the eligibility age dropped from 67 to 65 years old. Controls for household composition were not included in this case. All households in the sample are income-eligible. c: cutoff age.

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively.

Table 5: Elders' labor force participation estimates

	Labor Force Participation (month)				
	DD	SRDD	RDD	LLR	PSM
Effect	-0.0239	-0.0322	-0.1885***	-0.1655	-0.0651***
	(0.0398)	(0.0306)	(0.0651)	(0.2251)	(0.0230)
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	3327	3425	7512	1476	2641
R^2	0.2302	0.2155	0.205	-	0.2116
	Labor Force Participation (week)				
	DD	SRDD	RDD	LLR	PSM
Effect	-0.0214	-0.0429	-0.2021***	-0.2315	-0.0660***
	(0.0396)	(0.0303)	(0.0646)	(0.2248)	(0.0229)
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	3327	3425	7512	1476	2641
R^2	0.2312	0.2148	0.2031	-	0.2116
	Weekly worked hours				
	DD	SRDD	RDD	LLR	PSM
Effect	1.6312	-1.6158	15.754**	12.5482	-3.1911
	(3.8646)	(2.6191)	(6.8611)	(19.8767)	(2.6411)
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	909	820	1768	369	444
R^2	0.2032	0.2299	0.1892	-	0.2636

Notes: standard deviations in parentheses. Income-eligible households included only.

Table 6: Co-residents' labor force participation estimates

	labor force participation (month)									
	DD		SRDD		RDD		LLR		PSM	
individual age	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29
Effect	-0.0273	0.0063	-0.0444*	-0.0333	-0.1453**	-0.0240	-0.4953*	0.0029	-0.0425**	-0.0627*
	(0.0329)	(0.0457)	(0.0266)	(0.0418)	(0.0656)	(0.0891)	(0.2480)	(0.2733)	(0.0218)	(0.0326)
age interval	[60,67[	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]
N	4659	2564	4758	2473	9969	5069	2212	1187	3392	1507
R^2	0.1485	0.1487	0.1491	0.148	0.1279	0.1218	-	-	0.2286	0.2328
	labor force participation (week)									
	DD		SRDD		RDD		LLR		PSM	
individual age	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29	18 to 49	18 to 29
Effect	-0.0272	0.0029	-0.0432	-0.0321	-0.1735*	-0.0966	-0.4632*	-0.0123	-0.0523**	-0.0789**
	(0.0336)	(0.0469)	(0.0272)	(0.0411)	(0.0674)	(0.0916)	(0.2501)	(0.2818)	(0.0222)	(0.0336)
age interval	[60,67[	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]
N	4659	2564	4758	2473	9969	5069	2212	1187	3392	1507
R^2	0.1449	0.1483	0.1488	0.1451	0.127	0.1237	-	-	0.2286	0.2328

Notes: standard deviations in parentheses. Income-eligible households included only. All regressions include controls.

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively.

Table 7: Child labor and school attendance estimates

	Child Labor				
	DD	SRDD	RDD	LLR	PSM
Effect	-0.0843	-0.0443	-0.2250*	-0.6793*	0.0338
	(0.0621)	(0.0540)	(0.1302)	(0.3822)	(0.0330)
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	1487	1518	3285	783	1109
R^2	0.1376	0.1412	0.1277		0.2285
	School attendance				
	DD	SRDD	RDD	LLR	PSM
Effect	-0.0544	-0.0550	-0.1151	-0.1332	-0.0021
	(0.0361)	(0.0262)	(0.0803)	(0.2501)	(0.0246)
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
N	1487	1518	3285	542	1109
R^2	0.1405	0.1582	0.1076		0.2285

Notes: standard deviations in parentheses. Income-eligible households included only. Ages range between 10 and 15 years-old. All regressions include controls.

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively.

Table 8: Household composition intention-to-treat changes by period of time

	Period of time				
	2002-2005	2002-2003	2004-2005	2006-2007	2004-2005†
Number of members between 0 and 10 years-old					
Estimate	0.077	0.3**	-0.036	-0.007	-0.146
	(0.0631)	(0.1111)	(0.0766)	(0.0777)	(0.3106)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Number of members between 10 and 17 years-old					
Estimate	0.127*	0.129	0.133	0.072	0.544*
	(0.0575)	(0.1024)	(0.0704)	(0.072)	(0.3005)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Number of members between 18 and 30 years-old					
Estimate	0.025	0.093	-0.022	-0.164*	-0.088
	(0.0581)	(0.1)	(0.0733)	(-0.0713)	(0.2933)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Number of members between 31 and 59 years-old					
Estimate	0.095	0.288**	-0.023	0.083	-0.094
	(0.0519)	(0.0886)	(0.0657)	(0.0653)	(0.261)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Number of members older than 60 years-old					
Estimate	-0.05*	-0.061	-0.056	0.033	-0.226*
	(0.0231)	(0.0398)	(0.0288)	(0.0317)	(0.1208)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Number of residents					
Estimate	0.274*	0.748**	-0.004	0.016	-0.015
	(0.1311)	(0.2389)	(0.2)	(0.16)	(0.75)
Number of obs.	3,424	1,482	1,942	2,302	1,942
Elder living alone or with spouse					
Estimate	-0.014	-0.038	0.002	0.022	0.007
	(0.0226)	(0.0355)	(0.0333)	(0.0262)	(0.1167)
Number of obs.	3,424	1,482	1,942	2,302	1,942

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively. †: RDD estimate. Notes: standard deviations in parentheses. All regressions include controls. All estimates refer to income-eligible households. Age bandwidth is set to [c-5,c+5], where c is the cutoff age.

Table 9:

BPC intention-to-treat effect estimates on labor force participation by year and age range

	Period of time				
	2002-2005	2002-2003	2004-2005	2006-2007	2004-2005†
ages: 10 to 17 years-old					
Estimate	-0.053	-0.01	-0.103	-0.041	-0.488
	(0.0486)	(0.0667)	(0.0677)	(0.0482)	(0.3725)
Number of obs.	2,047	966	1,081	1,295	1,081
ages: 18 to 30 years-old					
Estimate	-0.03	-0.05	-0.039	-0.069	-0.12
	(0.04)	(0.0658)	(0.0506)	(0.0442)	(0.1791)
Number of obs.	2,635	1,205	1,430	1,559	1,430
ages: 31 to 59 years-old					
Estimate	-0.044	-0.018	-0.08*	-0.028	-0.443**
	(0.0331)	(0.05)	(0.0454)	(0.0424)	(0.1926)
Number of obs.	2,884	1,321	1,563	1,836	1,563
ages: 60 years-old or more					
Estimate	0.045	-0.059	0.071	0.089	0.295
	(0.0938)	(0.1372)	(0.1365)	(0.1023)	(0.5566)
Number of obs.	397	203	194	298	194
Eldest members of the household					
Estimate	-0.032	0.01	-0.052	-0.095***	-0.182
	(0.0305)	(0.0526)	(0.0403)	(0.0378)	(0.1378)
Number of obs.	3,425	1,483	1,942	2,305	1,942

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively. †: RDD estimate

notes: standard deviations in parentheses. All regressions include controls. All estimates refer to income-eligible households. Age bandwidth is set to [c-5,c+5], where c is the cutoff age.

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Appendix A: Variable codes and description

variable	description
outcomes	
livealone	1 if the elder lives alone or with spouse
plus18	number of members of the household with ages ranging from 18 to 29 years old
thirty50	number of members of the household with ages ranging from 30 to 50 years old
lfm	1 if the person works or looked for a job within the period of a month
lfw	1 if the person works or looked for a job within the period of a week
hours	weekly worked hours
attend	1 if the person attends school
oldest member of the household	
age	continuous age
educa	schooling
gender	1 if male
white_b	1 if the person is white colored
black_b	1 if the person is black colored
yellow_b	1 if the person is yellow colored
brown_b	1 if the person is browb colored (pardo)
individual characteristics	
white	1 if the person is white colored
black	1 if the person is black colored
yellow	1 if the person is yellow colored
brown	1 if the person is brown colored (pardo)
homem	1 if male
idcont	continuous age
escola	schooling attainment
household characteristics	
rural	Rural household
bf_sim	1 if someone within the household receives the Bolsa Familia
prural_sim	1 if someone within the household receives the Peti urbano
purbano_sim	1 if someone within the household receives the Peti rural
gas_sim	1 if someone within the household receives the Vale Gás
npessoas	number of members
nid1	number of children under 10 years old
nid2	number of members with ages between 10 and 20 years old
nid3	number of members with ages between 20 and 30 years old
nid4	number of members with ages between 30 and 40 years old
nid5	number of members with ages between 40 and 50 years old
nid6	number of members with ages between 50 and 60 years old
nid7	number of members older than 60 years old
maxed	highest schooling level within the household
year	dummies for year

Appendix B: Estimation Methods

B.1 Difference-in-differences Estimator (DD)

The first model applied to our data is the difference-in-differences estimator, which explores the reduction in eligibility age from 67 to 65 years-old in 2004. The control group is compound of observations close to the age they would become eligible. Their ages range from 60 to 64 years-old, where 'age' is the age of the oldest member of the household. The treatment group is compound of those with ages ranging from 65 to 66 years-old. We keep two years after the change (2004 and 2005) and two years before the change (2002 and 2003). All observations in the sample are income-eligible.

Consider a variable of interest *y*. If we want to check time differences in *y* between the treatment and comparison groups, we could estimate an equation of the form

$$y = \alpha + \beta\, 1[year \geq 2004] + \gamma\, 1[age \geq 65] + \ \delta\, 1[age \geq 65] \cdot 1[year \geq 2004] + \mu X + u$$

where 1(age≥65) is a variable indicating if the observation is in the treatment group and zero otherwise, 1(year≥2004) is an indicator variable equal to one if the observation is in the period after the change and zero otherwise. The interaction between them gives us our effect: δ. *X* is a vector of characteristics controlling for all differences that may exist between treatment and comparison groups. If $E[u|1(\text{age} \geq 65), 1(\text{year} \geq 2004), X] = 0$, we can say that δ is the gain that treated individuals have in comparison to the comparison group ones. All estimations used the sampling weights.

This is basically an intent-to-treat analysis because among the treated after 2004 we have non-treated people, or, according to the literature jargon, we have an imperfect compliance. So we are not capturing an effect of the treatment, because not everybody is treated, just an "intention-to-treat" generated by some administrative rule, affecting a particular group of people (elders from 65 or 66 years-old and/or co-residents). Another possibility here is to consider the treatment as "being eligible to receive the transfer".

B.2 Propensity score matching (PSM)

In this exercise we use non-treated eligibles as the comparison group to the treated individuals, i.e., we use those who according to their age and income condition should be receiving the benefit but are actually not receiving it as the comparison group and those who receive the benefit as the treatment group. We match treated observations to non-treated based on the selection on observables identification hypothesis. This hypothesis says that, conditional on a vector of pre-treatment characteristics, *X*, there is no systematic differences across treated and non-treated observations. Non-treated eligibles may differ in characteristics to the treatment group, such that by some observable characteristic individuals self-select them in or out of the program. Therefore it is important to control for such characteristics and the matching is a good way to account for these sort of differences.

Let y_{i0} be the outcome of the individual *i* when he is not treated and y_{i1} be the same outcome variable for the same individual *i* when he is treated. If we want to evaluate the average effect of a program for a sample of individuals, we should calculate

$$\tau|_{t=1} = E(y_{i1}|t_i = 1) - E(y_{i0}|t_i = 1)$$

However $E(y_{i0}|t_i = 1)$ is not observed. If the treatment is assigned randomly, $y_{i1}$and y_{i0}will be independent from *t*, and $E(y_{i0}|t_i = 1)$ would equals $E(y_{i0}|t_i = 0)$. But under the selection on observables hypothesis $\tau|_{t=1}$ is identified conditioned on X. That is, conditional on X_i, treatment is independent of y_{i0}, and the effect can be calculated as

$$\tau|_{t=1,X} = E(y_{i1}|t_i = 1, X_i) - E(y_{i0}|t_i = 1, X_i)$$

This is called the conditional independence assumption. However, sometimes may be hard to find different observations with the same vector of characteristics. So, to reduce the dimensionality of X_i the propensity score is calculated, which can be interpreted as the probability of being treated. The propensity score of *i*, is

$$p(X_i) = Pr(t_i = 1|X_i) = E(t_i|X_i)$$

and can be estimated by a logit or probit model. Observations with the same $X_i$will yield the same $p(X_i)$, and the treatment effect can be calculated by

$$\tau|_{t=1,p(X)} = E_{p(X)}[E(y_{i1}) - E(y_{i0})|t_i = 1, p(X)]$$

In this study, from 2001 on, we restrict our sample to the income-eligible observations with ages greater than *c* (the cutoff age, varying year by year), maintaining only treated and non-treated eligible individuals. Those that are income and age eligible, are supposed to receive the benefit. So we compare the outcomes of treated and non-treated observations after matching them by propensity score. The treated individuals are set as *t*=1 and non-treated are set as *t*=0, where *t* is the dependent variable in the logit estimation. The effect reported is the average conditional difference in outcomes between the treated and non-treated observations.

B.3 Regression Discontinuity Design (RDD)

In the simplest design of the regression discontinuity, called “sharp RD”, individuals receive the treatment based on a continuous measure, called "forcing variable", "running variable", or “assignment variable”. Those individuals who are under a cutoff value do not receive the treatment, and those above the cutoff value do receive the treatment (D = 1). So the probability of treatment jumps from 0 to 1 when A (assignment variable) crosses the threshold c. In our case A is the age of the oldest member of the household.

In this study, the probability of treatment jumps from a number above 0 to a number below 1 when A crosses the threshold c, and so we have the "fuzzy RD" design.

For a pension program with a discontinuity in age, not all the eligibles may get the treatment because of imperfect compliance and that is why the fuzzy RD is the best design. Following Lee and Lemieux (2009), the fuzzy RD design allows for a smaller jump in the probability of assignment to the treatment at the threshold and only requires:

$$\lim_{\varepsilon \downarrow 0} Pr(D = 1|A = c + \varepsilon) \neq \lim_{\varepsilon \uparrow 0} Pr(D = 1|A = c + \varepsilon)$$

The jump in the relationship between Y and A can no longer be interpreted as an average treatment effect, since the probability of treatment jumps by less than one at the threshold. In this set, the treatment effect for the fuzzy RD design (τ) can be written as

$$\tau = \frac{\lim_{\varepsilon \downarrow 0} E[y|A = c + \varepsilon] - \lim_{\varepsilon \uparrow 0} E[y|A = c + \varepsilon]}{\lim_{\varepsilon \downarrow 0} E[D|A = c + \varepsilon] - \lim_{\varepsilon \uparrow 0} E[D|A = c + \varepsilon]}$$

It is common in the literature to use this model for estimating a regression discontinuity design (RDD) program effect, because a fuzzy RD may be interpreted as an instrumental variables (IV) problem, as Angrist and Pischke (2009) argue. Hahn et al. (2001) showed the connection between the fuzzy RD and the two-stage-least-squares (2SLS), the LATE (local average treatment effect) and the Wald estimator. The participation in the program (D) is a function of the age (running variable) and its polynomial orders and an excluded instrument T=1[age≥c], which is the first stage of the regression. The instrument can also be interacted to the running variable, but to keep it simple we just left the model without this interaction. Substituting this into the model we have an outcome variable (y) regressed on ($\widehat{D}$) and the controls (X), including age and its polynomials. Both first and second stages are estimated for a small neighborhood around the discontinuity.

If we add no controls this is equivalent to estimate the LATE, set up by the following Wald estimator:

$$\tau = \frac{E[y|age = c + h] - E[y|age = c - h]}{E[D|age = c + h] - E[D|age = c - h]}$$

Here we can see the similarity of the fuzzy RD design estimator and the Wald estimator. So, "the non-parametric version of the fuzzy RD design consists of IV estimation in a small neighborhood around the discontinuity" (Angrist and Pischke, 2009). It is like an ITT effect, weighted by the difference in the probability of being treated on both sides of the discontinuity.

When applying this model to our study, we keep in the sample all the observations with ages ranging from c-5 to c+5 and from 2001 onwards. We added other controls to the model, the same used on all the other models so far. And we use T=1[age≥c] as instrument for *D* (treated or not).

B.4 SRDD ("sharp" RDD for the offer of a benefit)

The models exploring the RDD so far relied on the fact that close to the discontinuity there is a change in the probability of being treated at age *c*. It is then calculated the local average treatment effect for people at age *c*. Once we have people who should receive the benefit but do not receive it, we should consider a fuzzy design. However if we consider the treatment as being eligible to receive BPC transfers, we have then a "sharp RD" design.

Based on that, we use a weighted rectangular kernel to estimate the model for observations with ages ranging from c-5 to c+5. We keep the income-eligible sample for the years 2002 to 2005 and instead of "treatment" (*D*) we use the variable "eligibility" (T=1[age ≥*c*]).

B.5 Local Linear Regression (LLR)

As we know, IV estimators are biased but consistent. So keeping the instrument to a small sample close to the discontinuity may be sometimes not a good idea. With IV, larger samples are always better than smaller samples. So, the RDD literature exploit the local linear regressions technique as an alternative.

One way to do it is using kernel regressions on both sides of the discontinuity. Following Imbens and Lemieux (2008), our effect would be given by:

$$\tau = \lim_{\varepsilon \downarrow 0} E[y|A = c + \varepsilon] - \lim_{\varepsilon \uparrow 0} E[y|A = c + \varepsilon] = \frac{\sum_{i:A_i \geq c} y_i \cdot K\left(\frac{A_i - c}{h}\right)}{\sum_{i:A_i \geq c} K\left(\frac{A_i - c}{h}\right)} - \frac{\sum_{i:A_i < c} y_i \cdot K\left(\frac{A_i - c}{h}\right)}{\sum_{i:A_i < c} K\left(\frac{A_i - c}{h}\right)}$$

where $K(\cdot)$ is the Kernel function to choose, A is the forcing variable, y is the outcome variable, and h is the bandwidth. Fan and Gijbels (1996) argue that a triangular kernel is the optimal kernel to estimate local linear regressions at the boundary, which puts more weight on observations close to the cutoff point. A convenient way proposed by Imbens and Lemieux (2008) is to use rectangular kernel, which put the same weight for all observations within the discontinuity. In this case the model converges to simply running simple ordinary least square regressions on both sides of the discontinuity. However, Imbens and Kalyanaraman (2009) developed an estimator for estimating triangular kernel regressions with the optimal bandwidth choice, and that`s the one we used. First we kept the sample in the [c-5, c+5] interval, from 2001 on. All of the observations are income-eligible. Following the described approach, we estimated the effects and the standard errors.

Annex A: Weekly worked hours estimates

	Weekly worked hours				
	DD	SRDD	RDD	LLR	PSM
Effect	0.4539	0.8049	1.0273	12.0977	-0.5043
	(1.9735)	(1.6426)	(1.23)	(9.4513)	(2.6411)
controls	yes	yes	yes	yes	yes
age interval	[60,67[	[c-5, c+5]	[c-5, c+5]	[c-5, c+5]	[c, c+5]
period	[2002,2005]	[2002,2005]	[2001,2008]	[2001,2008]	[2001,2008]
individual age	[18, 30[	[18, 30[	[18, 30[	[18, 30[	[18, 30[
N	1545	1472	3054	726	863
R^2	0.1025	0.0703	0.0614	-	0.2421

Notes: standard deviations in parentheses. Income-eligible households included only. All regressions include controls.

*, **, ***: significant at the 10%, 5%, and 1% levels, respectively.