University of São Paulo
College of Agriculture “Luiz de Queiroz”

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

Pedro Rodrigues de Oliveira

Thesis submitted in fulfilment of the requirements for
the degree of Doctor in Science. Area: Applied Eco-
nomics

Piracicaba
2011

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B.Sc. in Economics

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RESUMO

Avaliação de impacto do programa de pensão não-contributiva Benefício de Prestação Continuada no bem-estar da família

Este trabalho faz uma avaliação dos efeitos do programa de assistência social Benefício de Prestação Continuada (BPC) sobre o bem-estar familiar. Destinado a pessoas portadoras de deficiência e idosos em situação de pobreza, o programa provê o benefício de um salário mínimo mensal. O estabelecimento de uma idade a partir da qual a pessoa se torna elegível gera uma descontinuidade na probabilidade de recebimento e que é explorada para fins de identificação. Utilizando dados da PNAD, desenvolveu-se um procedimento para decompor os valores das transferências de programas sociais, identificando quais programas o indivíduo é beneficiário. Assim para o período de 2001 a 2008, estimou-se o efeito do BPC sobre variáveis como a composição domiciliar, participação na força de trabalho, horas trabalhadas semanalmente – do idoso e dos corresidentes – além de presença de trabalho infantil e frequência escolar. Encontrou-se que o programa tem efeitos importantes na redução do trabalho infantil, efeitos sobre participação na força de trabalho de corresidentes entre 30 e 59 anos e sobre o número de membros no domicílio entre 30 e 49 anos. Também encontrou-se uma esperada redução na participação dos idosos na força de trabalho, mas não se verificaram efeitos sobre a frequência escolar infantil. Não se observaram efeitos importantes sobre horas trabalhadas. A complexidade dos efeitos observados evidenciam a necessidade de se estudar a heterogeneidade das transferências sociais, com muitos outros aspectos latentes ainda a serem revelados.

Palavras-chave: BPC; Avaliação de políticas públicas; Regressão descontínua; LATE; PNAD; Participação na força de trabalho; Trabalho infantil

ABSTRACT

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

This study evaluates the effect of the Benefício de Prestação Continuada (BPC) program on family welfare. The program is targeted to poor disabled and elderly people providing monthly stipends equal to one monthly minimum wage. The establishment of an age at which the person becomes eligible for the benefit created a discontinuity in the probability of being treated which is explored for identification. We developed a procedure to decompose the stipends from social programs using the PNAD dataset, thereby identifying which programs the person participates. Therefore, from 2001 to 2008 we estimate the effect of the BPC on variables such as household composition, labor force participation, weekly worked hours – for the elder and co-residents – besides child labor and school attendance. We found that the program have significant effects on child labor reduction, on labor force participation for members between 30 and 49 years-old, and on the number of members between 30 and 59 years-old. It was also observed an expected labor force participation reduction for the elderly, but no effects on school attendance of children. No significant effects on worked hours were found. The complexity of the findings highlights the need of studying the heterogeneity of social cash transfers, where many latent aspects are yet to be uncovered.

Keywords: BPC; Public policy evaluation; Regression-discontinuity design; LATE; PNAD; Labor force participation; Child labor

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LIST OF ABBREVIATIONS

2SLS	Two-Stage Least Squares
ATE	Average Treatment Effect
ATT	Average Treatment Effect on the Treated
BONOSOL	Bono Solidario
BPC	Benefício Benefício de Prestação Continuada de Assistência Social
ILO	International Labour Office
INSS	Instituto Nacional do Seguro Social (National Institute for Social Security)
IV	Instrumental Variable(s)
ITT	Intention-to-Treat effect
LATE	Local Average Treatment Effect
LLR	Local Linear Regression
LOAS	Lei Orgânica da Assistência Social (Organic Law of Social Assistance)
MDS	Ministério do Desenvolvimento Social e Combate à Fome (Ministry of Social Development)
MP	Medida Provisória (Provisional Measure)
OECD	Organisation for Economic Co-Operation and Development
OLS	Ordinary Least Squares
PETI	Programa de Erradicação do Trabalho Infantil (Child Labor Eradication Program)
PNAD	Pesquisa Nacional por Amostra de Domicílios (National Households Survey)
PME	Pesquisa Mensal de Emprego (Monthly Employment Survey)
RD	Regression-Discontinuity
RDD	Regression-Discontinuity Design
RMV	Renda Mensal Vitalícia (Monthly Lifetime Income)
SUTVA	Stable Unit Treatment Value Assumption

1 INTRODUCTION

Conditional cash transfer programs have proven to be an important way to alleviate poverty in the developing world. In Brazil, much attention has been paid to the Bolsa-Escola and Bolsa-Família programs, which provide the benefits to poor families in order to keep children attending school and avoiding child labor, among other goals. The BPC¹ program, however, is a pension scheme addressed to disabled people and to the elders, and 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 program is a non-contributory pension scheme which provides a minimum wage for elders (with 65 years old or more) and people with disabilities which make them incapable to the independent life and work. To be eligible, the person must be aged more than 65 and prove to be incapable to get by with their 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.

Several pension programs are being carried out throughout the world for over one hundred years. This theme is usually linked to the social security literature, which usually deals with contributory pension schemes. This study, nevertheless, assesses a non-contributory pension benefit. Programs of this kind are being undertaken in many countries². In Denmark there is a means-tested program in place since 1891. The United Kingdom enacted a similar program in 1908. Australia, France, Germany, Iceland, Ireland, Spain, and New Zealand also have similar programs. Most of the programs are carried out in OECD countries, but they are also present at Eastern Europe and the Developing World, and less frequently in poor countries. The Appendix A compares institutional frameworks of non-contributory pensions in selected countries.

Barrientos and Lloyd-Sherlock (2002) summarize the effectiveness of non-contributory pension schemes for some countries. Usually the programs tackle on poverty and vulnerability prevention at the old age. But other effects arise from these pensions: it promotes old aged status within the household, it prevents extreme poverty in the very poor households, and

¹Acronym for *Benefício de Prestação Continuada*.

²To a more complete list of these countries and analysis of the programs check World Bank (1994, p.114-115), Social Security Administration (2010), and Holzmann et al. (2009)

it avoids the persistence of poverty throughout the generations by means of investment in physical, human and social capital.

Most of the studies appraises the effect of the non-contributory pensions on reducing poverty and inequality, mostly using descriptive analysis. For the developing world there are studies for Argentina (Bertranou and Grushka, 2002), Bolivia (Martinez, 2005), Brazil (Schwarzer and Querino, 2002; Barrientos, 2003), Costa Rica (Durán-Valverde, 2002), Namibia (Schleberger, 2002), Zambia, among many others. Barrientos (2003) using probit estimates shows that the probability of being poor in households 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 payments were not taken into account.

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

In Bolivia there is the Bono Solidario (Bonosol), which is a stipend for every person over 65 years-old. The study of Martinez (2005), using regression discontinuity designs, concluded that there was a significant increase in food consumption for beneficiaries. Moreover, for very poor households, the transfers may increase production by investments in food production or other small scale activities. These income improvements can, by its turn, become human capital investments.

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, transport, 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 to food, schooling, transfers, and savings. Other interesting re-

sults 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) evaluated 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 was complicated by the fact that children living with pension recipients are relatively disadvantaged on average. Her identification strategy considered 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 with no eligible person, those living with an eligible man, and those in households 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 increased the weight-for-height of girls (but not boys).

Edmonds et al. (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 to living arrangements of households with eligible women, by exploiting the discontinuity in the age eligibility rule (women become eligible at the age 60). They find no evidence that the additional pension income leads to an increased propensity to live alone, as suggested by other studies (Costa, 1999). 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 ages suggest they are their sons and daughters.

Paulo (2008) studied the effect of the BPC program on living arrangements using differences-in-differences 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) argue that the distortionary effect of cash transfers on labor supply is insignificant in developing countries with high level of under- and unemployment. Particularly in Brazil, this effect is very unlikely to occur due to extreme poor families that would not survive without extra income. A hazardous effect is the rise in the reservation

wage of family members who are job-seekers. Reis and Camargo (2005) showed 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 et al. (2003), for South Africa, and Carvalho Filho (2008a), for Brazil. Bertrand et al. (2003) found negative effects of the old age pension in South Africa for coresidents' labor supply. Posel et al. (2006) and Ardington et al. (2009) showed however that when the household is extended to include non-resident members this result is challenged, especially because the pension makes people move out to work or to look for a job.

Some other papers focused on the relationship between pensions and child labor and education. Edmonds (2006), comparing households that receive the pension with those households which are about to receive the pension, found that school attendance increased and child labor decreased within households with old age beneficiaries in South Africa. Reis and Camargo (2007) show through a multinomial logit model that Brazilian pensions tend to improve the probability of the young to attend school³. Carvalho Filho (2008b), and Kruger et al. (2006) show that rural pension have increased the enrollment rate and diminished youth's participation in the labor market.

Carvalho Filho (2008b) uses a Brazilian social security reform⁴ 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 drop 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.

Ponczek (2011) used the same Brazilian social security reform to assess its effect on education and health. The old-age benefit improves schooling, especially the literacy for girls

³Some remarks to this paper were made by Hoffmann (2010), however the result that the probability to study is higher than the probability to work remains valid.

⁴There was an age reduction and increase in the amounts paid for rural pensions in 1991.

(6 to 14 years-old) co-residing with a male beneficiary. But the same results does not hold for children co-residing with a female pensioner.

As it can be seen, there are several studies on the effects of old age cash receipts on poverty, inequality, child labor, schooling, living arrangements, and labor supply. Despite all the shortcomings of the programs and of the studies, the transfers to the old age have proven to have important spillover effects within the household. This study presents some evidence on the effects of the BPC on labor and educational outcomes of beneficiaries and their co-residents, besides changes in the household composition. The next chapter details the program and its expected effects. Chapter 3 details the database, the decomposition of values of the economic transfers from the government through the PNAD database, the validation of the procedure, and some descriptive statistics. Chapter 4 describes the methodology to be implemented. Chapter 5 presents the results concerning household composition, labor force participation, worked hours, child labor, and school attendance. Chapter 6 concludes.

2 THE BPC PROGRAM

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 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 elderly age to receive the benefit was 70 years old. In 1998 this age was reduced to 67 years old, and in 2003 to 65 years old⁵. The benefit may be paid to every old-aged person with a per capita family income no greater than 25% of a minimum wage and with no social security aid or any other retirement plan fund income. There can be more than one beneficiary in the same family. In this case, the individual must be disabled or older than the cutoff age, and the income of the first beneficiary will be included in the family income calculation. Since 2004 this rule is no longer in place. Families with beneficiaries from other governmental social programs can also receive the BPC, given that 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 for one year was approximately US\$ 8.2 billion, while the Bolsa Familia budget was US\$ 4.4 billion. The BPC program benefited near 3 million people⁶, 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 very large, compared to

⁵Table 21 in the Annex A details the evolution in the BPC legal framework

⁶Accounting for elderly people and people with disabilities.

Table 1 - Evolution in the number of BPC recipients, by year

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	976,257
2003	1,687,519	664,875	1,036,365
2004	2,061,013	933,164	1,127,849
2005	2,277,365	1,065,604	1,211,761
2006	2,473,696	1,183,840	1,293,645
2007	2,680,823	1,295,716	1,385,107
2008	2,934,472	1,423,790	1,510,682

Source: MDS and Ministry of Social Security.

other programs.

The budget is also large when we consider Social Security figures. According to the Social Security Statistical Bulletin (Brazil, 2008), the total expenditure of the INSS in 2008 amounts to R\$ 242.6 billion (US\$ 142 billion). Therefore, 3% of all expenditures of Social Security goes for BPC. If we compare the BPC budget to the amount addressed to benefits of the general regime (regular pensions) this percentage rises to 7%. That is very significant when we consider that BPC is a non-contributory pension.

The benefit is also large when we compare the participants of BPC to those of other social programs. Based on PNAD 2006 survey⁷, the largest values received by a single ben-

⁷The 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. It will be further explored ahead.

eficiary of Bolsa Familia is below R\$150 (US\$ 88 on today's currency⁸). So, the amount of the BPC benefit (R\$350, or US\$205) is about 2.3 times larger than the largest stipends 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 66% meet the income eligibility criterion, and, from those, 59% are women⁹. That is, 66% 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 84% of beneficiaries are poor. About 94% of the beneficiaries belong to families with income per capita less than a minimum wage.

If we consider estimations of the concentration index for the 2004 PNAD presented in Soares et al. (2006, p.25) we found that for the 2006 PNAD the index was quite the same. The concentration index for the 2004 sample, excluding ex-ante the benefit of BPC from the per capita income, was -56.1, which reveals a very progressive pattern of the program; that is, the BPC income is concentrated among the poorest families.

If someone in the family is a BPC beneficiary, then, by the eligibility requirements, this family certainly is in a social vulnerability condition. Those families are exposed to low sanitary conditions, poverty, unemployment, and child labor, to cite a few examples. Just as for Bolsa Família, we expect from the BPC more than just alleviate poverty. We expect a shift in the life quality of those families. So we expect a lower incidence of child labor, better health and nourishment conditions and a higher children's enrollment in school, among others.

This study intends to evaluate the effects of the BPC on the household composition, labor outcomes, child labor, and school attendance. The BPC may allow elderly recipients 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-residents would be more liable to leave the labor market. Situations like these include those where the co-resident was working only to temporarily maintain the household expenditures, or if the individual had an unsatisfactory job and the extra income allowed him to look for a better job, or those who quits his job to study,

⁸Considering an exchange rate of R\$1.7 per US dollar.

⁹60% of recipients are women.

for example. So spillover effects on labor and educational outcomes of co-residents are also addressed.

3 DATA

The source of our data is the annual household survey 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 programmes 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 is benefited from a social program, but not an individual within a 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 earnings, household income, age, 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 as V1273, described as: “savings account¹⁰ and other financial applications, dividends, and other source of income”. Therefore, social program stipends are declared into this other source of income in this variable. Besides, it is very 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 values transferred by each social program from

¹⁰In 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 rentability established as $0.5\% + TR$. The TR is an interest rate calculated by the government and indexed to the average value of the interest rates of private sector’s Certificate of Deposits. This investment is popular among low income investors.

the government (BPC, Bolsa Família, Bolsa Escola, Bolsa Alimentação, Cartão Alimentação, Auxílio Gás, and PETI) to identify beneficiaries from each program. All individuals receiving an amount exactly equal to one minimum wage were identified as BPC beneficiaries¹¹. The other programs and their combination were considered to identify their beneficiaries as well.

Our goal is to use this approach to identify year by year beneficiaries of all social programs. 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 stipends and 2,911 receiving the BPC stipend. 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 sources of incomes) in the 2004 PNAD for households that have at least one BPC beneficiary according to PNAD special supplement. We can observe a high frequency of the value 260 (the minimum monthly wage at that time), indicating that those are beneficiaries of the BPC program. However, other values may also be the BPC program combined with other social programs. For example:

$$267 = 260 + 7 \text{ (BPC + } \textit{Auxílio Gás}\text{), and}$$

$$282 = 260 + 15 + 7 \text{ (BPC + } \textit{Bolsa Família} + \textit{Auxílio Gás}\text{)}.$$

It is important to take all the combinations of values into account to avoid losing beneficiaries in the sample.

Therefore, using this procedure, we can identify which programs the individual is participating year by year. We must consider also that the monetary values for each program may change every year¹².

¹¹We must consider another existing program which also pays one minimum wage for elderly people: the Renda Mensal Vitalícia (RMV). This program was replaced by the BPC in 1996, but beneficiaries continued to receive the benefit. Nowadays, there are few beneficiaries of RMV and they do not declare this income in the PNAD variable 'V1273'. If so, we could identify beneficiaries receiving one minimum wage before 1996, but that is not the case.

¹²In Table 20, in Appendix B, we present the modal values year by year in the PNAD database. Table 22, in Annex B, presents which programs were considered for the combination year by year.

Table 2 - Values for variable ‘V1273’ for individuals in households declared to house beneficiaries of BPC 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.

3.1 Validating the procedure

We must consider that the procedure proposed involves the risk of incorrectly identifying shareholders as BPC beneficiaries. It is important, therefore, to compare the individuals identified by this procedure with those identified by the PNAD supplements available in 2004 and 2006. These two years of the survey include specific questions to identify households with individuals who are beneficiaries of some social programs, allowing a validation of the method. Table 3 shows this comparison. ‘Total’ includes elders and disabled individuals.

We can see that the proposed method identifies more beneficiaries than the supplement does. The proportion of elders in the total beneficiaries of BPC (elders+disabled) is smaller using the approach above when compared to the administrative data and to the data from the special supplement. 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

Table 3 - Identification of beneficiaries

	Identified sample	Identified population	PNAD supplement (sample)	PNAD supplement (pop.)	Official Records
PNAD 2004					
total	2,371	1,006,002	1,629	670,235	1,983,788
elders	695	273,308	588	225,897	885,236
elders/total	29.31%	27.17%	36.09%	33.7%	44.62%
PNAD 2006					
total	4,158	1,753,815	2,959	1,231,936	2,430,125
elders	1,590	655,164	1,380	566,478	1,158,005
elders/total	38.24%	37.33%	46.63%	45.98%	47.65%

Source: 2004 and 2006 PNAD.

Note: The population value was obtained using the database weights. ‘Official records’ refers to the number of issued benefits according to the Ministry of Social Development in September of each year. The PNAD supplement sample includes those who declared one or two minimum wages in ‘V1273’ and also declared that someone in that household do receive the BPC.

differentiating the BPC benefits from the regular government retirement pensions addressed to insured workers. Many BPC beneficiaries could have declared themselves as a pensioner, and not as a BPC beneficiary. The agency where the beneficiary claim for the benefit is the INSS, also responsible for individual’s 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.

Table 23 in Annex C presents the distribution of the beneficiaries by State and region. In general we can identify more beneficiaries than simply using the PNAD supplement despite of an underreporting of the Ministry of Social Security figures. In some cases we identified beneficiaries where the supplement did not. On the other hand, in some cases we even had an overreporting using the decomposition. Observe that the number of beneficiaries identified

in states with metropolitan areas is much higher than in other states and this is clearly an effect of the PNAD design.

Souza (2010) discussed the underreporting of beneficiaries of the Bolsa Família and BPC programs using the PNAD database. If on one hand we have an underreporting when we try to estimate the total number of BPC beneficiaries using the PNAD, on the other hand social security researchers often find an overreporting when they try to estimate the total number of pensioners. Considering this fact, the study observes that the erroneous reporting of the BPC stipend as a regular social security pension in the PNAD database seems to occur especially before 2004. Moreover, it estimates that even if we take out all these overreported pensioners and assume they are BPC, we would still have an underreporting of the total number of BPC recipients. Another important conclusion is that the PNAD sampling design have a strong influence on this BPC underreporting. PNAD captures better specific groups if people are well spread geographically. If beneficiaries are strongly concentrated on a region which was not selected to be sampled, then it is very likely that it would become a bias afterwards.

From 2004 onwards, when a bill regarding the rights of the elders was passed¹³, 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. In Figure 1 we can clearly see the rise in the proportion of eligibles receiving the benefit in 2004.

It is important also to verify whether income eligible elders are only attended by BPC and not by other ‘pensions’ such as the regular social security pension and RMV, both tied to the minimum wage. Figure 2 depicts the percentage of elders receiving one minimum wage in the PNAD variable designated to the computation of pensions. This group is composed of elders living with others receiving one minimum wage from the RMV, from a retirement social security pension, or even from the BPC but mistakenly declared the benefit as a pension.

Another important point is that the PNAD survey was not designed to find such specific groups of people based on values of some specific income. The family’s income may also change from the time they received the benefit (so, they were income-eligible for the

¹³The so-called “Statute of the Elderly”.

Figure 1 - Percentage of income-eligible elders receiving the BPC, by year

Figure 2 - Percentage of income-eligible elders receiving a minimum wage as pension, by year

benefit) and the time the PNAD survey interviews were carried out¹⁴. So it is not a surprise that figures obtained by our procedure and the figures reported by the Ministry differ.

We need to know whether the individuals identified as beneficiaries by the proposed

¹⁴In fact, there is a high income variability among poor people. See Soares et al. (2009)

disaggregation are really BPC beneficiaries or their income is originated from interest or dividends. In our sample some individuals were identified as beneficiaries even living in households where, by the PNAD supplement, there was no BPC beneficiaries, because if we see they are receiving one minimum wage and are poor we classify them as BPC recipients. We can classify the beneficiaries in the sample into three groups:

Group 1 comprises elders identified by the procedure and by the supplement.

Group 2 composed of elders identified as beneficiaries by the procedure, but not by the PNAD supplement.

Group 3 composed of elders identified as beneficiaries by the PNAD supplement, but not by the procedure.

The group 3 is composed of those who mistakenly declared their income as a retirement pension, or declared the minimum wage of the previous year¹⁵ instead of the current, or really wrongly declared to receive the benefit, while there is no indication of receiving the benefit by their income. In 2004 we had 177 households falling into this group where 29% declared the benefit as a retirement pension, and the other 71% did not actually receive the BPC. In 2006 there were 68 households falling into this group, and quite all of them have no indication of BPC stipends on their income.

With the other two groups we can check if our method is correctly identifying beneficiaries by comparing important characteristics of both groups. We expect them not to differ much. If groups 1 and 2 are similar this is a strong indication we are correctly identifying BPC beneficiaries through our earnings decomposition.

For the 2004 PNAD, 94 of the 695 elders were in group 2. Out of the 94, 86 of them (91.5%) do not have any kind of earnings, and 74.5% had a per capita household income of less than a minimum wage. For 2006, 182 of the 1590 elders were in group 2. Out of the 182 elders, 172 (94.5%) do not have any kind of earnings, and 61.5% have a per capita household income of less than a minimum wage. Therefore they are very poor.

In 2004, the average years of schooling for group 1 was 1.39, while the average for the 94 elders identified in group 2 was 1.44. In group 1, 62.5% of the elders are illiterate, and

¹⁵The minimum wage changes every year, usually in May.

93% have no more than 4 years of schooling. In group 2, these percentages are 64% and 90%, respectively. Therefore, both groups are very similar.

These comparisons lead us to believe that individuals who were identified as beneficiaries and who declared not to do so because they did not know the BPC, once their profiles are similar to those who declared to be BPC beneficiaries.

3.2 Descriptive Statistics

The sample drawn for this study includes several years of PNAD. Although we have a large number of observations in these databases few of them are elderly BPC beneficiaries, and fewer are income-eligible. Table 4 shows the percentage of income-eligible households by year. We can observe that less than 15% of the households are income-eligible for the benefit, and among those only 10% of the households had an elder, and up to 13% had elders between 60 and 75 years-old. The last four columns show the participation of each age range in the composition of the group of elders between 60 and 75 years-old living in income-eligible households. We can observe that most of them have ages ranging from 60 to 65 years-old.

Table 5 displays the distribution of the elderly beneficiaries across different income levels, year by year from 1999 on. The income levels are standardized and ranges between 0 and 100.

More than a headcount of elders who joined the program year by year in the sample, the table clearly shows how much the BPC can shift the distribution of per capita income to the right. Taking into account the household income without the BPC for all recipients, 44% of them are on the first tenth of the distribution, and 70% of them are up to the third tenth. Considering only the income-eligible elders, the proportions are 63% and 80%, respectively.

The fact that some beneficiaries are not income eligible does not mean necessarily that they are wealthy. First of all, the income-eligibility here is evaluated at the moment the survey was carried out and, according to Soares et al. (2009), there is a high income variability among poor people. About 12% of the population crossed the poverty line in both ways in the period of a month. Within the period of an year, from 2005 to 2006, 15% of the population crossed the same line. So, it is not surprising to find “non-eligible people” receiving the benefit. In second place, the per capita income of someone in the 70th position of the per capita income

Table 4 - Percentage of households by year

year	income -eligibles	inc.-eligibles with an elder (65+)	inc.-eligibles with an elder between 60 and 75 years-old	participation of each age range on the composition of the group of income-eligible households with elders between 60 and 75 years-old			
				[60,65[	[65,67[	[67,70[	[70,75[
1993	13.5%	6.3%	9.9%	58.3%	16.7%	14.6%	16.6%
1995	9.3%	7.8%	10.4%	49.6%	19.4%	17.6%	18.9%
1996	9.8%	8.2%	11.8%	56.4%	17.8%	16.6%	16.8%
1997	10.2%	8.2%	11.5%	53.4%	19.8%	19.4%	13.7%
1998	10.1%	7.3%	11.2%	56%	15.9%	17.9%	16.1%
1999	10.4%	6.5%	10.6%	59.8%	16.4%	15.9%	13.5%
2001	12.2%	5.6%	9.3%	59.9%	19.8%	13.7%	12.4%
2002	12%	5.7%	9.8%	62.1%	19.3%	13.1%	12.1%
2003	13%	4.9%	8.7%	65.2%	16.1%	12.8%	12.4%
2004	11.9%	7.6%	11.3%	59.5%	14.9%	14.8%	18.2%
2005	12.6%	7.8%	10.7%	56.7%	18.3%	13.7%	18.6%
2006	12.9%	10.6%	12.9%	53.5%	17.1%	18.1%	21.8%
2007	12.6%	10%	12.6%	54.9%	16%	19.3%	20.6%
2008	11.8%	10.1%	12.9%	55.4%	15.8%	18.6%	20.6%

Note: the percentages in the last four columns do not integrate 100% for in some households there are more than one elder.

distribution is about R\$ 616 (US\$ 322¹⁶). Someone in the 31st position has a per capita income of around R\$ 246 (US\$ 128). That is really not too much to get by in Brazil. In third place, the family definition of the PNAD database is different from the definition of family in the BPC law. Although we tried to control for those differences when computing the family income, they might still remain in the sample, with some income-eligible people assigned as not eligible.

¹⁶R\$ 1.91 per US dollar

Table 5 - Number of elderly beneficiaries by per capita family income with and without BPC stipends

year	Hundreths of the per capita family income					total
	[0,10[	[10,30[	[30,50[	[50,70[	[70,100]	
1999	3	27	67	29	12	138
2001	0	13	21	14	5	53
2002	4	22	43	21	30	120
2003	0	12	20	32	20	84
2004	11	123	172	331	82	719
2005	10	134	209	398	152	903
2006	11	293	382	701	259	1646
2007	17	224	282	531	152	1206
2008	20	219	284	595	220	1338
year	excluding the BPC benefit from the family income					total
	[0,10[	[10,30[	[30,50[	[50,70[	[70,100]	
1999	85	24	18	4	7	138
2001	20	18	7	3	5	53
2002	50	20	20	9	21	120
2003	32	18	12	5	17	84
2004	306	184	121	68	40	719
2005	363	251	131	77	81	903
2006	691	425	253	145	132	1646
2007	527	360	164	79	76	1206
2008	554	346	229	113	96	1338

Our family income is computed using a family definition which is a proxy for what the BPC regulations defines as ‘family’. For this relevant family we calculate their income excluding BPC stipends. This definition of family is used for the income-eligibility calculations. Moreover, even with actual income-eligible households being assigned as non-eligible by our calculations, as we do not know if the household income declared in the survey is

always higher or if it is originated from an income shock in the family, we preferred to keep the income-eligibility this way to make sure we are dealing with very poor people at the cost of losing some observations.

The large shift in the per capita income observed with the BPC in Table 5 indicates that the BPC is an important income shock which may alter the whole functioning of the household, including the behavior of all of its members: elders and co-residents. The first thing which may change is the household composition itself, with members moving in or out. Table 6 addresses this issue. We can observe the evolution year by year of the household composition for the whole sample and for the income-eligible group. The trends in the table show that recently the elders are living more alone than in the past. Also, the number of younger members tends to decrease. Although the differences are not statistically significant, we need to check if the figures from treated households differ from the other groups.

Table 6 - Household composition

year	% of elders (65+) living alone		percentage of households with elders living alone		average number of people between 18 and 29 years-old in households with the eldest 65 or more		average number of people between 30 and 50 years-old in households with the eldest 65 or more		percentage of households with elders (65+) and children		number of children, if there is any children	
	all	income-eligible	all	income-eligible	all	income-eligible	all	income-eligible	all	income-eligible	all	income-eligible
1993	12.5%	8.2%	15.6%	9.1%	0.514	0.694	0.621	0.847	33.8%	54%	1.97	2.44
1995	13.3%	9.7%	16.6%	10.7%	0.471	0.644	0.62	0.871	33.2%	52.2%	1.9	2.39
1996	13.3%	9.9%	16.6%	11.1%	0.483	0.624	0.631	0.856	33.3%	52.4%	1.86	2.21
1997	13.5%	9.1%	16.8%	10%	0.482	0.643	0.651	0.867	34%	54%	1.86	2.37
1998	13.3%	10.1%	16.5%	11.1%	0.458	0.594	0.64	0.837	31.8%	51.3%	1.82	2.36
1999	13.5%	11.8%	16.9%	13.1%	0.447	0.633	0.633	0.78	31.1%	48.5%	1.78	2.3
2001	14.5%	10.7%	18%	11.8%	0.436	0.708	0.614	0.777	29.7%	46.8%	1.77	2.28
2002	13.8%	10.2%	17.2%	11.3%	0.441	0.664	0.615	0.815	29.1%	48.4%	1.76	2.33
2003	14.4%	11.7%	18%	12.8%	0.419	0.657	0.595	0.798	27.8%	47%	1.72	2.28
2004	14.9%	13.6%	18.5%	15.8%	0.412	0.5	0.591	0.681	27.6%	37.4%	1.7	2.05
2005	15.1%	12.2%	18.8%	14.3%	0.413	0.563	0.584	0.66	26.5%	37.5%	1.69	2.02
2006	15.1%	12.4%	18.7%	14.7%	0.404	0.513	0.579	0.696	26.4%	39.2%	1.65	1.9
2007	15.6%	14.7%	19.5%	17.1%	0.377	0.468	0.572	0.673	25.6%	38.6%	1.63	1.89
2008	15.8%	14.8%	19.7%	17.5%	0.357	0.478	0.551	0.633	23.8%	32.2%	1.59	1.85

note: children have ages up to 15 years-old. Taking into account the standard deviations the values presented do not differ

One issue related to the household composition is the status of the elder within the household. One of the expected effects of the BPC is this status promotion. One common situation is when the elder is the head of the family. However, sometimes the elder lives with an adult son or relative, where the latter is the head of the family. So Figure 3 addresses this issue, showing the proportion of elders who are the head of the family. This may be a proxy for “who lives with who”, that is, whether the son co-resides with their parents, or the parents co-reside with their adult sons. The figure displays this proportion across beneficiaries, non-treated eligible elders, and non-eligible elders (regarding income). We can see a high proportion of elders heading the family. Apparently there is an increase in this proportion among eligibles (treated or not) in 2004. But it is hard to tell if it is related to the BPC or to the “Statute of the Elderly”.

note: all observations have $age \geq c$. Spouses were considered as head of the family as well.

Figure 3 - Proportion of elders who are the head of the family, by year

Table 7 explores the co-residents' labor supply. When it comes to social assistance, there is always a concern about large income-effects which could offset the substitution effect between leisure and labor, thus affecting reservation wages, hours worked, and participation in the labor force.

Table 7 compares several outcomes for co-residents in treated and non-treated households. All households are income-eligible, but among the non-treated we have non-age-eligible households. Individuals living in treated households seems to participate more in the labor force and work more hours per week than those in non-treated households. As we do not have too many treated households we need to be careful when looking at these outcomes.

If we look at children' labor force participation, Table 8 shows that treated households seem to be less prone to present children working. Usually child labor and school attendance are variables related to each other. So if treated households are less exposed to child labor we would expect a higher school attendance rate of their children, but there is no clear evidence of that in Table 8.

Table 7 - Labor supply for co-residents

year	Labor Force Participation (month)		Labor Force Participation (week)		Weekly Worked Hours		Formal Work		Unemployment	
	non-treated	treated	non-treated	treated	non-treated	treated	non-treated	treated	non-treated	treated
2001	66.4%	75%	68.2%	75%	19.57	23.33	34%	33.3%	19.1%	16.7%
2002	66.2%	57.1%	68.8%	60.7%	19.51	12.32	33.5%	57.1%	19%	28.6%
2003	67.2%	87%	69.5%	91.3%	19.32	25.13	35.2%	15.4%	19.9%	26.1%
2004	68.1%	74.9%	70.2%	75.9%	19.78	24.16	32.8%	39%	18.7%	18.7%
2005	68.8%	72.9%	70.6%	74.5%	19.37	23.09	33.9%	39.4%	19.5%	19.2%
2006	66.9%	67.4%	69.3%	70%	19.45	21.45	34.5%	39.8%	18%	18.1%
2007	65.5%	67.4%	67.3%	69.7%	18.88	22.62	37.4%	46.3%	17%	14.5%
2008	66%	73.8%	67.8%	75%	19.19	24.21	38.6%	42.4%	17%	15.7%

note: ages between 18 and 29 years-old. Only income-eligible households included. Among the non-treated not all households are age-eligible. Not every worker informed whether his job is formal or informal.

Table 8 - Child labor and school attendance

year	Labor Force Participation (month)			% Attending School		
	non- treated	non- treated (age $\geq c$)	treated	non- treated	non- treated (age $\geq c$)	treated
1993	31.8%	23.2%		77.7%	80.2%	
1995	32.1%	21.3%		81%	85.2%	
1996	26.3%	21%		82.8%	86.8%	
1997	26.8%	14.6%		86.5%	85.4%	
1998	27.2%	17.8%		88.7%	92%	
1999	27.6%	23.4%	13.6%	90.9%	93%	95.5%
2001	21.8%	20.4%	14.3%	92%	93.8%	100%
2002	21.4%	14.7%	19.2%	93.8%	90.7%	96.2%
2003	18.2%	12.6%	20%	94.2%	90.3%	100%
2004	19.3%	14.1%	12.8%	92.5%	90.2%	92.3%
2005	21%	16.6%	11.9%	93.5%	93.5%	93.1%
2006	18.4%	16.7%	9.9%	93.7%	92.2%	93.7%
2007	16.6%	14%	13.2%	94.5%	94.5%	95.2%
2008	15.1%	6.8%	5.5%	95.3%	98.1%	92.7%

notes: children between 10 and 15 years-old. Income-eligible households only. Non-treated (age $\geq c$) are the age-eligible households and hence should be receiving the benefit. 'age' is the age of the beneficiary or the oldest member of the household.

One of the goals of this study 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 4 the frequency of beneficiaries by age. Clearly there is a sharp increase in the number of beneficiaries at the age 65. We must consider 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 65, where the elderly become eligible. In Figure 5 we present the proportion of beneficiaries in the PNAD 2006 sample, sorted by age. Once again, it remains clear the increase in the number of beneficiaries at the age 65.

Figure 4 - Beneficiaries by age

Figure 6 shows how some of these outcome variables presented on the tables above behave close to the discontinuity in age, for treated and non-treated households. It depicts the frequency of some outcomes by their proximity from the cutoff age, smoothed by a 4th order polynomial epanechnikov kernel function. The red line indicates individuals living in treated households. At point zero income-eligible individuals become age-eligible to receive the BPC payments.

We can observe a decline in the presence of child labor for treated households. By the other hand, the school attendance does not show a clear pattern. It seems to be higher for treated households, comparing with non-treated eligibles, but there is a decrease in school attendance for non-treated which is difficult to find an explanation other than a smoother pitfall discussed below. The drop in labor force participation of elders is very clear. The labor force participation of co-residents does not show a clear pattern as well. It seems to decline with the age of the elder, but visually it is hard to tell if treated or non-treated households

Figure 5 - Percentage of beneficiaries in the population, by age

have the highest rate of participation.

It is important in discontinuity designs to check the covariates' behavior next to the cutoff point. So Figure 7 shows some covariates near the cutoff point. The first plot addresses the number of members in the household. We can observe that treated households have less members than non-treated. We feared that people start to move into households with elders receiving the benefit, but what seems to happen is just the opposite. Maybe if poor elders are living with their sons or relatives because they do not have a way to sustain themselves they tend to have their independence improved with the benefit, and so they move out. We will explore the household composition later in this study.

The other graphs in Figure 7 show that beneficiary elders tend to be less educated than non-beneficiaries, and the same applies to the co-residents. The proportion of rural households seems to be the same for treated and non-treated households.

The word of caution about these graphs presented concerns the kernel estimation. The relationships showed are sensitive to the choice of the kernel smoother, so we need to further explore this relationships, adding controls and trying out different estimation methods. Another issue is that kernel smoother performs poorly in boundary points. Figure 8 addresses this point showing the proportion of male elders. Here we have as elders the eldest member

note: The red line and the crosses indicate individuals in treated households. Crosses and circles are averages of the bins of a histogram. The sample includes income-eligible households from the year of 2001 onwards.

Figure 6 - Outcomes by distance from cutoff age

of the household. The plot in the left side shows a discontinuity at the cutoff. For the same data the graphic in the right side shows no discontinuity at all at the cutoff. We observe that there is some decrease from point “0” on, but not a sudden jump as the left side plot shows.

note: The red line and the crosses indicate individuals in treated households. Crosses and circles are averages of the bins of a histogram. The sample includes income-eligible households from the year of 2001 onwards.

Figure 7- Covariates by distance from cutoff age

note: The red line and the crosses indicate individuals in treated households. Crosses and circles are averages of the bins of a histogram. The sample includes income-eligible households from the year of 2001 onwards.

Figure 8 - Proportion of male elders and the kernel issue in boundary points

4 METHODOLOGY

This chapter describes the proposed methodology to evaluate the BPC program. Trying to justify the methodology applied and the choices made, we first present a brief introduction to the impact evaluation framework, followed by a description of the main techniques for impact evaluation such as instrumental variables and regression discontinuity design. Some comments are made also on estimation and some validity tests.

4.1 Introduction to Impact Evaluation

The whole structure of impact evaluation of a program relies on the concepts of “counterfactual” and “causality”. What would be the outcome if the program was not implemented? Did the program caused the *ex-post* observed outcomes, or was it something else? These two questions motivated the program evaluation literature, and we try to briefly lay out the main ideas below¹⁷.

The first definition to explore is the definition of treatment and comparison groups. In the literature jargon, when there is a binary treatment (e.g., someone participates or not), the treatment group is composed of those exposed to a program whereas those not exposed to the treatment are referred as the control group (or comparison group).

The second idea to explore is the existence of potential outcomes. Assuming our observational unity as the individual, every individual has two potential outcomes. For individual i , with $i = 1, \dots, N$, and denoting the outcome as y , the two potential outcomes can be denoted as y_i^1 , if the individual is treated, and y_i^0 if the individual is not. Therefore, for each individual i , the effect of the program on the potential outcomes is given by $y_i^1 - y_i^0$. The fundamental problem here, however, is that the individual i can either participate or not participate in the program, but never both at the same time¹⁸. Hence only one potential outcome can be observed at a given moment and we can never calculate $y_i^1 - y_i^0$. That is what the terminology “potential” means. Both outcomes can potentially be observed for any individual, but at a given period of time only one effectively is. Once one potential outcome

¹⁷This framework was mainly set by Rubin in a series of papers during the 70’s and 80’s and is often called as the Rubin Causal Model. See Rubin (1974, 1977, 1978, 1980a, 1986, 1990).

¹⁸Holland (1986) describes it as the fundamental problem of causal inference.

is realized, the other automatically becomes the counterfactual. Therefore the determination of causal effects requires the build of counterfactuals. If the observation is treated, how would she behave if she was not treated? Or if she is not treated, how would she behave if treated?

Following Imbens and Wooldridge (2008) we can write the preceding discussion as

$$y_i = y_i(D_i) = y_i^0 \cdot (1 - D_i) + y_i^1 \cdot D_i = \begin{cases} y_i^0 & \text{if } D_i = 0, \\ y_i^1 & \text{if } D_i = 1. \end{cases},$$

where D_i indicates whether the individual i belongs to the treatment group and y_i is the realized outcome. So there is a distinction between the pair of potential outcomes (y_i^0, y_i^1) and the realized outcome y_i .

The third idea of the Rubin Causal Model is the assignment mechanism, e.g. how the individuals are allocated into treatment and control groups. In general, we may think it as a function of potential outcomes and observed covariates. The first class of assignment mechanisms is the randomization, where individuals are randomly allocated into treatment and control groups. In this setting the potential outcomes become statistically independent from the treatment. We may write this condition as

$$(y_i^0, y_i^1) \perp\!\!\!\perp D_i \quad (\text{Condition A})$$

where $\perp\!\!\!\perp$ denotes statistical independence. Experimental settings are usually drawn under this convenient condition as we will see below.

In the second class of assignment mechanisms only assignment probabilities do not depend on the potential outcomes, when we cannot be sure about the assignment itself. The condition related to this class of assignment mechanisms may be written as

$$(y_i^0, y_i^1) \perp\!\!\!\perp D_i | X_i \quad (\text{Condition B})$$

where X_i denotes the vector of covariates. As noted by Dehejia and Wahba (2002, p.153), this condition assumes that, conditioning on observable covariates, we can take assignment to treatment as random. Comparing two individuals with the same X_i , one treated and other in the control group, it would be the same as comparing two individuals in a randomized

experiment. It means that unobservables play no role in assignment to treatment and that, conditional on X_i , one could not tell if the observation comes from the treatment or control group.

Under Condition A, treatment and control groups do not differ systematically from each other. Also called ignorable treatment assignment (Rubin (1977)) or “treatment ignorability condition”, the main implication of this condition is that if we want to evaluate the average effect of a treatment on a relevant population it would be enough to compare the averages between treatment and control groups. This is the Average Treatment Effect (ATE), defined as

$$ATE = E[y_i^1 - y_i^0] = E[y_i^1] - E[y_i^0]. \quad (1)$$

Another important assumption related to Condition A is that the treatment of individual i affects only the outcome of unit i . This is called the “stable unit treatment value assumption” (SUTVA), as coined by Rubin (1980b). As Wooldridge (2002, p.604) points out, when we are using samples coming from a population in order to evaluate some program we are implicitly assuming this. If SUTVA does not hold, drawing a sample from a population would involve the risk of selecting individuals affected twice by the treatment biasing the estimator upwards, or units in the control group also affected by treatment biasing the estimator downwards, among many other possibilities.

However, in the real world it is very difficult to find any program choosing participants randomly in a relevant population in an experimental framework. Usually participants self-select themselves to participate in the program, or people get selected according to eligibility rules and so on. So we are usually under a nonexperimental framework and, in this case, estimating effects for the whole population such as the ATE estimator would not make sense and usually would yield biased estimates. The effect of interest would be that for the subpopulation covered by the program, the eligibles or a relevant group of people, comparing treated people to those eligibles who are not on the program or those who will receive the treatment in the near future. So in impact evaluations researchers are more often interested in calculating the Average Treatment Effect on the Treated (ATT), given by

$$ATT = E[y_i^1 - y_i^0 | D = 1] = E[y_i^1 | D = 1] - E[y_i^0 | D = 1] \quad (2)$$

than calculating the ATE.

Moreover, calculating the ATE requires the build of two counterfactuals, while the ATT requires the build of just one. To see that let's consider the ATE equation in (1). Applying the law of iterated expectations yields

$$\begin{aligned} ATE &= E[y_1] - E[y_0] = \\ &E[y_1|D = 1] \cdot \Pr[D = 1] + E[y_1|D = 0] \cdot \Pr[D = 0] - \\ &\quad - (E[y_0|D = 1] \cdot \Pr[D = 1] + E[y_0|D = 0] \cdot \Pr[D = 0]) \end{aligned} \quad (3)$$

By Equation (3) we can observe that we would need two counterfactuals to estimate the ATE: $E[y_0|D = 1]$ and $E[y_1|D = 0]$ – while the ATT would require only one: $E[y_0|D = 1]$.

We can rewrite the ATT using Condition B (conditional on X , the pair (y_i^0, y_i^1) is independent from D). Thus the ATT can be rewritten as

$$ATT = E[y^1 - y^0|X, D = 1] = E[y|X, D = 1] - E[y^0|X, D = 1] \quad (4)$$

Assuming that, conditional in X , the potential outcome $y^0|D = 1$ has the same distribution of $y|D = 0$, we can finally define our counterfactual (the treatment group under the non-treatment situation) using our control group, and identifying the ATT.

4.2 Core Methods for Identification and Analysis

In this section we drive the discussion towards the direction of the methodology we applied in this study and the methodological choices we made to evaluate the BPC.

4.2.1 Instrumental Variables and LATE

One common way to measure the effect of a program under nonexperimental settings is by using instrumental variables (IV). In many cases the participation in the program is correlated to the potential outcomes for reasons not observable to the researcher. We can imagine, among many other possibilities, people getting into some program because they believe it would be good for them and they are much more motivated for joining in. Therefore

either Condition A or Condition B do not hold.

The only way out is to find an exogenous variable (the instrument) that influence participation on the program but that have no influence on the outcome of the program if participating. This variable can be used to identify the effect of interest.

Let us for instance consider the case where there is random assignment. Let Z be the variable of assignment¹⁹. Because of random assignment the causal effect of Z is given by $E[y_i|Z = 1] - E[y_i|Z = 0]$. According to Duflo et al. (2007), this effect is not the effect of the treatment, D , because people selected for the treatment may not be effectively treated. But Z influences the treatment and hence is called the Intention to Treat, or ITT.

$$ITT = E[y_i|Z = 1] - E[y_i|Z = 0] \quad (5)$$

But if we are interested in the effect of the treatment we must consider that there is a probability of being treated involved, as Z is not equal to D and there is probably some “no shows” among those selected for the treatment. The Wald estimate²⁰ takes into account this probability, dividing the ITT by the fraction of treated people in treatment and control groups. The Wald ratio is given by

$$\beta_{Wald} = \frac{E[y_i|Z_i = 1] - E[y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} \quad (6)$$

This is exactly the same estimate as a two-stage least squares (2SLS) estimate of the following equation, using Z as a binary instrument:

$$y = \alpha + \beta \cdot \hat{D} + \xi$$

where ξ is the error term and $D = f(Z)$.

Imbens and Angrist (1994) show that if we want to identify (6) as the average treatment effect, some conditions apply. Defining the potential participation indicators as $D_i^0 \equiv D_i|Z_i = 0$ and $D_i^1 \equiv D_i|Z_i = 1$, these conditions (or hypotheses) can be written as

¹⁹In our case Z will be $\mathbb{1}[age \geq c]$, as we will see later.

²⁰From Wald (1940).

1. Independence: $(y_i^0, y_i^1, D_i^0, D_i^1) \perp\!\!\!\perp Z_i$; and
2. Monotonicity: $D_i^1 \geq D_i^0, \forall i$ or $D_i^1 \leq D_i^0, \forall i$.

The first hypothesis concerns the validation of the instrument. A valid instrument must be independent from the potential outcomes and independent from the potential participation indicator, which means that only by looking at Z one could neither tell if the individual is in treatment group or in comparison group nor what their outcomes would be under both conditions.

The second hypothesis ensures only one direction on the probability of being treated according to the selection for treatment. “If people are more likely, on average, to participate given $Z = w$ than given $Z = z$, then anyone who would participate given $Z = z$ must also participate given $Z = w$ ” (Imbens and Angrist, 1994, p.469).

Under these conditions the estimator yields a particular average treatment effect that they call as LATE (local average treatment effect). By “local” it is understood that the effect is valid only for those affected by the instrument: the compliers. Angrist et al. (1996) further explained this point. Considering the binary assignment variable Z , the observed treatment condition can be written as

$$D_i = D_i^1 \cdot Z_i + D_i^0 \cdot (1 - Z_i)$$

Considering the values assumed by D_i and Z_i , we can classify the individuals as: “always-taker”, “complier”, “defier”, or “never-taker”. Table 9 shows this classification.

“Always-taker” and “never-taker” describe individuals who are not affected by the instrument. As Angrist et al. (1996) show, the effects that both groups cause on identification are null, because for both groups $E[y_i|Z_i = 1] = E[y_i|Z_i = 0]$. The monotonicity hypothesis excludes the existence of “defiers”. Because they always do the opposite they were selected to do by the instrument, their probability of being in the treatment or comparison groups is in the opposite direction of the expected. Therefore, the only effect that remains is that one of the “compliers”.

In the particular case when nobody in the comparison group is treated ($D_i^0 = 0$), as Duflo et al. (2007, p.53) points out, the Wald estimate yields the effect of the treatment on the treated. So the LATE will be equal to the ATT. This is the Bloom’s one-sided non-compliance

Table 9 - Classification of the individuals according to their response at selection for treatment

D^1	D^0	D	classification
1	1	1	always-taker
1	0	if $Z = 1, D = 1$ if $Z = 0, D = 0$	complier
0	1	if $Z = 1, D = 0$ if $Z = 0, D = 1$	defier
0	0	0	never-taker

Source: Angrist, Imbens, and Rubin (1996).

result²¹.

Important advancements and generalizations in the LATE estimator include: the use of multiple instruments (Angrist and Evans (1998), Angrist and Imbens (1995)), inclusion of covariates (Abadie (2003), Angrist and Imbens (1995), Frölich (2007a)), and multi-valued or continuous instruments (Heckman et al. (2006)), among others.

4.2.2 Regression Discontinuity Design

The LATE estimator discussed on the previous section establishes the basis for understanding the regression discontinuity (RD) design. Although applications date back to Thistlethwaite and Campbell (1960), only recently this kind of empirical problem caught the attention of econometricians. Angrist and Lavy (1999) and Van der Klaauw (2002) are famous applications of regression discontinuity.

A regression-discontinuity design arises whenever the assignment to a program or treatment depends on organizational or administrative rules. For example, Angrist and Lavy (1999) studied the effect of the class size on students' performance. Using educational data the authors verified the effect of the "Maimonides' Rule", which establishes that the class must be split into two when the number of students reaches 40. Van der Klaauw (2002) analyzed the financial aid effect on college dropout rates, using the administrative rule that

²¹From Bloom (1984).

only those students who reached a given score on SAT would be eligible for the aid. The BPC we analyze in this study sets that people becomes eligible to the program at the age 65.

Hahn et al. (2001) discussed the identification issues used in previous studies. They distinguished two fundamental discontinuity designs: the sharp design and the “fuzzy” design. With a sharp design, treatment assignment depends deterministically on some observable continuous variable called in the literature as assignment variable, forcing variable or running variable. Those under a cutoff value do not receive the treatment, while all above this cutoff do receive the treatment. That is, the probability of treatment jumps from 0 to 1 when the forcing variable crosses some preestablished threshold value c (cutoff value). For the BPC the forcing variable is age. So, considering the forcing variable A , we can write this relationship as $D_i = \mathbb{1}[A_i \geq c]$.

In the sharp design the average causal effect of the treatment at the discontinuity point is given by

$$\lim_{a \downarrow c} E[y_i | A_i = a] - \lim_{a \uparrow c} E[y_i | A_i = a] \quad (7)$$

In the “fuzzy” design, by the other hand, the probability of treatment does not jump from 0 to 1 at the cutoff point. Instead, there is a smaller jump in this probability. As the difference in the probabilities of both sides of the discontinuity is not equal to 1, there are unobserved variables determining these probabilities. The important fact is that there is still a discontinuity in the probability of being treated at c .

$$\lim_{a \downarrow c} E[D_i | A_i = a] \neq \lim_{a \uparrow c} E[D_i | A_i = a] \quad (8)$$

Considering the model $y_i = \alpha_i + \tau \cdot D_i$, Hahn et al. (2001) proposed the following estimator which applies for both sharp and fuzzy designs:

$$\tau = \frac{\lim_{a \downarrow c} E[y_i | A_i = a] - \lim_{a \uparrow c} E[y_i | A_i = a]}{\lim_{a \downarrow c} E[D_i | A_i = a] - \lim_{a \uparrow c} E[D_i | A_i = a]} \quad (9)$$

We can observe its similarity with the Wald estimator in the LATE framework. In fact Hahn et al. (2001) were the first to make this connection²². However, for validity, some

²²Imbens and Lemieux (2008) observed that after this study RD designs started being interpreted as IV settings.

extra assumptions apply. One important condition is that $E[y_i^0|A = a]$ and $E[y_i^1|A = a]$ are continuous in a , for a in a neighborhood around c . This assumption restrain the use of discrete forcing variables.

As the LATE, the RD identifies the effect for a subgroup of compliers – specifically: those compliers with $A = c$. In comparison with the LATE, the effect is even more local. It leads the researcher to a limited degree of external validity if one wants to generalize the findings.

4.2.2.1 Estimation

One important question stressed out by Lee and Lemieux (2009) is that RD design must be viewed as a “design” and not as method. Aside the efforts for creating standards for the RD²³ the methods used to estimate the effect of interest vary a lot in the literature. Estimation under RD designs have often been viewed as a nonparametric problem because 1) there is no reason *a priori* to believe that the model is linear; and 2) the consequences of incorrect specification are more serious in RD designs. Basically there are four main trends of estimation:

1. Polynomial Estimation: It is the simplest way of relaxing the linearity assumption. It consists of including higher orders of the $A(A^2, A^3, A^4 \dots)$ in the regression of y on A . The shortcoming of this method, however is that it uses observations with X too far away from the cutoff point to estimate the effect.
2. Two-Stage Least Squares: considering the similarity of the Wald estimator in the LATE framework and in the RD design, identification is obtained and some authors estimate the effect by 2SLS using $D = \mathbb{1}[A_i \geq c]$ as instrument. The sample is composed by observations within the bins on both sides of the discontinuity.
3. Kernel Regressions: considering a particular value of the forcing variable A , say a . Kernel functions will simply average y and A in a neighborhood of a (called window or bin), putting more weight on observations with A close to a . The important shortcoming of this method is that it performs poorly in boundary points.

²³The reviews made by Imbens and Lemieux (2008), Lee and Lemieux (2009), and Van der Klaauw (2008) are noteworthy.

4. Local Linear Regression (LLR): this approach consists of running a regression weighting observations according to a chosen kernel function. In the RD literature the convenient kernel function is the rectangular kernel. In this case the local linear regression simplifies to fitting linear regressions within the bins on either side of the cutoff point.

Originally, Hahn et al. (2001) thought the RD as a nonparametric problem. Estimating limits such as on Equation (9) is an old issue. In the RD design, their first idea was to estimate the four limits separately, running one-sided kernel regressions which would yield an estimate equal to the Wald estimator. However it was shown that the estimator would be asymptotically biased. Instead, they propose running a local linear regression to estimate the limits, reducing the bias.

The local linear estimator for $\lim_{a \downarrow c} E[y_i | A_i = a]$ is $\hat{\alpha}$, where

$$(\hat{\alpha}, \hat{\tau}) \equiv \underset{\alpha, \tau}{\operatorname{argmin}} \sum_{i=1}^n (y_i - \alpha - \tau(A_i - c))^2 \cdot K\left(\frac{A_i - c}{h}\right) \cdot \mathbb{1}[A_i \geq c]. \quad (10)$$

In the equation above, $K(\cdot)$ is a kernel function and $h > 0$ is the bandwidth²⁴. Imbens and Lemieux (2008) propose the convenient rectangular (uniform) kernel for $K(\cdot)$ ²⁵. With rectangular kernel, standard linear regression can be used to estimate the effect. Attention must be paid on how many observations should be considered on each side of the discontinuity for estimation – in other words, which h should be chosen.

We have not discussed about covariates in regression discontinuity design so far. The reason is that covariates play no role in RDD, or at least should play no role. Covariates should be added to the regression aiming to reduce sampling biases and variance only²⁶. The covariates should be continuous at $A = c$. The rationale is the same as if we were in a randomized experiment. Covariates are used simply for reducing variance, and differences between the groups often put doubts on the randomization and drives the discussion onto assignment mechanisms and self-selection grounds. Adding covariates to the LATE or RDD frameworks is straightforward, either if LLR or 2SLS is used. Frölich (2007a, 2007b) make

²⁴A comprehensive discussion about kernel smoothers can be found in DiNardo and Tobias (2001).

²⁵Although Fan and Gijbels (1996) argued that that a triangular kernel is the optimal kernel to estimate local linear regressions at the boundary, putting more weight on observations closer to the cutoff point.

²⁶Frölich (2007b) argues that the inclusion of covariates is also important to reduce bias when covariates differ from left to the right side of the discontinuity.

some comments on the nonparametric inclusion of covariates.

The literature in RD is in rapid progress. Recent advancements on RDD modeling include: multiple assignment variables (Reardon and Robinson (2010), Papay et al. (2011), Wong et al. (2011)), quantile treatment effects (Guiteras (2008), Frölich (2007b), Frölich and Melly (2010), Frandsen (2009)), Bayesian inference (Lee and Card (2008), Chib and Jacobi (2011)). However we do not discuss those papers in this study because they do not apply to our data.

The most important point raised here is the relationship between LATE and the RDD framework. In general, results for LATE can easily be used in estimating effects under RD designs. Therefore some authors claim that the RDD is IV. “[T]he non-parametric version of the fuzzy RD design consists of IV estimation in a small neighborhood around the discontinuity” (Angrist and Pischke, 2009). Hence, in order to evaluate the BPC, we rely on both frameworks.

4.3 BPC as a Regression-Discontinuity Design Program

The BPC program has a very clear cutoff point in the age for eligibility. At 65 years-old poor individuals become eligible to the benefit, but is up to the eligible person to claim for the benefit. So there are a few comments to be made on these characteristics.

The first comment is that usually age is a discrete variable. Lee and Card (2008) discuss this case. The main implications for estimation are: the need of larger bins, you must use parametric regressions, and some extrapolation will be needed to estimate $\lim_{a \uparrow c} E[y_i | A_i = a]$ and $\lim_{a \downarrow c} E[y_i | A_i = a]$. Some extra care will be needed in computing standard-errors as well. However in our database we have the possibility of making the ages continuous once we have the day, month, and year of birth of every individual in the household, overcoming the potential issues arising from the use of discrete forcing variables.

The second comment is that, as not all selected for treatment do receive the treatment we face an imperfect compliance on the right side of the discontinuity. So we could describe the BPC as “fuzzy” design program, when there is imperfect compliance.

The last comment is that, as under 65 nobody receives the treatment, $\lim_{a \uparrow c} E[D_i | A = a] = 0$ by definition. In a LATE framework this condition would make the Wald estimator

not to yield the average treatment effect but the average treatment effect on the treated (ATT)²⁷. Previous studies called this case as one-sided non-compliance or partially fuzzy design. Battistin and Rettore (2008) show that this status does not impose further assumptions on individuals' behavior nor identification requirements.

Whenever under RD design, one must choose how many observations on either side of the discontinuity should be taken into account. That is, one must choose the bandwidth h . The choice of the bandwidth involves a bias-variance tradeoff. A narrow bandwidth will reduce bias, but less observations will inflate the variance. On the other hand, the broader the bandwidth is, more precise the estimation will be, but more biased as well. The more the data is concentrated near the cutoff point the less you will need to face this tradeoff.

Some frequent options are to use a rule-of-thumb such as the one proposed by Fan and Gijbels (1996) or a cross-validation procedure²⁸. But in the end this is always a decision of the researcher. We set $h = 5$ in most of our regressions. But we also performed a local linear regression with a triangular kernel, with an optimal bandwidth choice algorithm, proposed by Imbens and Kalyanaraman (2009).

Another issue we must consider is that some key outcome variables used in this study are binary. Wooldridge (2002, p.636-637) make some comments on the IV estimation of this sort of model. Under linearity assumptions and normality, Heckman (1978) proposed a bivariate probit for estimation. Chiburis et al. (2011) compare the estimation of this model by bivariate probit and linear IV. The results however depend on sample size and model specification. Maximum Likelihood estimators perform better when the model is correctly specified, however when covariates play no role this is not an issue. Although this is an ongoing debate, Angrist and Pischke (2009) advocates the use of 2SLS, and several studies proceed as such (Urquiola and Verhoogen (2009), Angrist and Lavy (1999), among others).

Taken these comments into account we performed 2SLS regressions in both sides of the discontinuity with an $h = 5$, using as instrument the age-eligibility $D_i = \mathbb{1}[A_i \geq c]$. We also performed a LLR with a triangular kernel with an optimal bandwidth choice. For robustness we also performed a LLR with a rectangular kernel evaluating the effect of the eligibility for

²⁷Frölich and Melly (2008) show that the inclusion of covariates under one-sided non-compliance may turn the ATT $\neq$ LATE when the distribution of the covariates of compliers and treated individuals differ.

²⁸See Imbens and Lemieux (2008).

treatment, identifying an intention-to-treat effect. The outcomes analyzed were the labor force participation (if someone works or looked for job in the period of a month), weekly worked hours, household composition, child labor, and school attendance. Households with disabled BPC beneficiaries were excluded from the sample.

4.4 Discontinuity Validity Tests

The main strategy for evaluating the BPC is to explore 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 conditions enough to characterize a discontinuity design.

As the program design allows the existence of three different groups: participants, eligible non-participants, non-eligibles, we check for the validity of the discontinuity exploring differences and similarities between these groups. A first check is the “randomization” of the treatment among eligibles. Considering those eligible households, we must check if systematic differences arise between the groups. 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 an 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.

Table 10 shows the means for household characteristics and the characteristics of the oldest member of the household. The sample comprises 3374 income-eligible households, that is, households with a per capita family income of no more than 25% of the current minimum wage. Only households with “ages” between c (the cutoff age) and $c + 5$ are included. If we take into account the observations at the left side of the cutoff age (between $c - 5$ and c) as well we do not observe any significant changes in the values presented. The first column presents the means for 2237 non-treated households while the second one presents the means for 1127 treated households. Significant values of a t-test of difference between the means are indicated with an *. Under the null hypothesis the difference between the first and the second column is zero.

What we can observe is that half of the variables listed can be considered statistically

Table 10 - Means for characteristics of the household

Variable	non-treated	treated
rural	0.155	0.152
Bolsa familia (dummy)	0.082	0.101
Peti urbano (dummy)	0.004	0.004
Peti rural (dummy)	0.008	0.011
Vale Gás (dummy)	0.006	0.015
gender (1 for male)	0.426	0.384
white	0.435	0.35*
black	0.11	0.097
yellow	0.009	0.005
pardo	0.446	0.549*
age of the oldest member	68.487	67.696*
schooling of the oldest member	2.353	1.753*
highest schooling level within the household	7.073	5.661*
number of members	4.098	3.211*
number of children under 10 years old	0.681	0.412*
number of members with ages between 10 and 20 years old	0.833	0.449*
number of members with ages between 20 and 30 years old	0.477	0.39*
number of members with ages between 30 and 40 years old	0.451	0.361*
number of members with ages between 40 and 50 years old	0.331	0.213*
number of members with ages between 50 and 60 years old	0.18	0.152
number of members older than 60 years old	1.144	1.234*
Hundreths of per cap income (0 to 100), excluding BPC	27.861	14.463*
Hundreths of per cap income (0 to 100)	27.704	40.137*
Per capita private income (excluding BPC)	200.076	106.309*
Per capita income from all sources	193.553	281.535*
Per capita income of households within the same census tract	405.125	376.548
Social security pensions income	58.552	34.219*

note: H0: difference between the means is zero. The sample includes income-eligible households with at least one member above the cutoff age and with no more than five years over this cutoff age.

*: significant at the 1% level.

not different across the groups. For the other half the difference on means is statistically significant, but for some of these results the difference is not so expressive. Consider for example the number of children under 10 years old in the household. The value for treated

households is 0.412 while for non-treated households the figure is 0.681, which means roughly that for every ten households with children under 10 years old (5 for each group) there is one more child on the non-treated group. Considering the total number of members, every ten households (5 for each group) there will be 20 people on the non-treated households while this figure amounts to 16 on the treated group. So this is an evidence that people are not moving in due to the benefit. At the same time, it is counterintuitive to think they are moving out. But we will analyze the household composition in the next chapter. Treated households have poorer less educated members, but the difference is not expressive despite being significant. But there is an expressive difference on income across the groups. Moreover, large samples tend to make any existing difference significant. Concerning the social security pensions, 90% of the households do not have any pensioner as a member. So in general we can say that both groups are very similar.

A second test is the McCrary's density test (McCrary, 2008) for manipulation of the forcing variable, in our case the age of the oldest member of the household. The test check for discontinuities in the forcing variable near the cutoff point, which would indicate manipulation of the forcing variable. If somehow people generally lie about their age in order to self-select themselves into treatment then the test would indicate it.

For a sample of 5779 income-eligible households around the cutoff age c ($[c - 5, c + 5]$) from 2004 on, we proceeded with the density test depicted on Figure 9. Apparently there is a little upward shift on the frequency at the age 65 (c , from 2004 on), however it is not statistically significant at a 10% level. The log difference in height from one side of the discontinuity to the other was 0.1564, with a standard error of 0.0996. Observe that there is a large proportion of observations at the points 60, 65, and 70. This is due to the fact that during the collection of the data, when the elderly person is not present in the household and another person is answering the questionnaire, usually this person do not know exactly the age of the person who is away and he or she tends to round it, and 60, 65, and 70 turn out to be modal values.

For curiosity we tested the discontinuity on the income variable. Although income is not the forcing variable on our analysis, it is a variable that together with age assigns people

Figure 9- McCrary's density test on age.

into treatment. There is a per capita family income cutoff at 25% of the minimum wage. Figure 10 address this discontinuity in the per capita income.

Figure 10- McCrary's density test on income.

The figure on the left side is calculated on the left side of the discontinuity on age $[c - 5, c]$, and the one on the right side was calculated on ages ranging from c to $c + 5$. Only poor families in the period from 1996 onwards were kept in the sample (per capita family income of less than one minimum wage). What we can observe is an upward shift on the

frequency of families over 25%. If we think that somehow families were trying to self-select into treatment staying below the income threshold, the figure indicates just the opposite.

The same test was performed for other threshold values (0.15, 0.20, 0.30, and 0.35). The other significant threshold values were 0.30 and 0.35 for the right side of the discontinuity on age, and for 0.35 on the left side. However at the 0.35 breakpoint the larger frequency is at the left side of the discontinuity, probably caused by the outlier just below the 0.35 point. This outliers arise from those households with one minimum wage as total income, a very common situation among poor families. As it is common to find households with three or four members, one minimum wage will yield the 0.33 and the 0.25 outliers presented in the figures. Aside of these values, we cannot see in the figure any clear pattern of more people under the 0.25 threshold, so there is no indication of manipulation on this variable.

We must keep in mind that the per capita family income variable we use is not exact, because by the data we have we can only “create” a similar family to serve as proxy for the family as defined by BPC regulations. So the location of these outliers may not be exactly in the point where they are now.

5 RESULTS

The first outcome to be analyzed is the household composition. It is important to know whether the household composition changes due to access to the benefit, with members moving in or out of the household. All results presented here exploit the discontinuity generated by the cutoff age c at income-eligible individuals and households, where ‘age’ is the age of the BPC recipient or the oldest member of the household. The effects presented here are the effects attributed to the BPC grant or to the age-eligibility for it. In the sequence we will explore other outcomes for household members.

5.1 Household Composition

Table 11 shows the changes in the households’ living arrangements due to the benefit. The first column shows a model that evaluates the effect of being eligible to the benefit (age-eligible and income-eligible), but not necessarily receiving the benefit, constituting an intention-to-treat analysis. The second column displays an average treatment effect on the treated estimating through a two-stage least squares procedure where age-eligibility (D) is used as an instrument for effective treatment. Age ranges from $[c-5, c+5]$. The third column shows the treatment effect on the treated changing the estimation method. Instead of two-stage least squares, a local linear regression method is used, weighting observations according to its distance from the cutoff point by a triangular kernel function. The optimal bandwidth here changes from each model²⁹, this can be observed by the drop in the number of observations used to estimate the effect. More tables hereafter will be displayed in this way with these three models.

Three outcomes were evaluated in Table 11: if the elder live alone or with spouse, the number of members between 18 and 29 years-old, and the number of members between 30 and 49 years-old. The only significant effect was for 30 to 49 years-old. We found that for every five treated households we would have one more member between 30 and 49 years-old.

To check for anticipation issues we narrow our period of analysis to the period from

²⁹The inclusion of observations far from the cutoff point has an effect of reducing the variance by the increase in the number of observations, but on the other hand it may increase the bias of the estimate. So there is an optimal bandwidth which balance this trade-off, and the algorithm we used attempts to calculate this bandwidth.

Table 11 - Household composition changes near the discontinuity

	(I)	(II)	(III)
<i>Outcome: Elder living alone or with spouse</i>			
Estimate	-0.01365	0.00439	0.07398
t-statistics	(0.62)	(0.08)	(0.49)
period	[2002,2005]	[2001,2008]	[2001,2008]
Number of obs.	3424	7516	1565
R ²	0.3688	0.3701	-
<i>Outcome: Number of members between 18 and 29 years-old</i>			
Estimate	0.00383	-0.12146	-0.07085
t-statistics	(0.82)	(0.87)	(0.15)
period	[2002,2005]	[2001,2008]	[2001,2008]
Number of obs.	3424	7516	1920
R ²	0.6967	0.1952	-
<i>Outcome: Number of members between 30 and 49 years-old</i>			
Estimate	0.00561	0.21524*	0.50339
t-statistics	(0.93)	(1.71)	(1.09)
period	[2002,2005]	[2001,2008]	[2001,2008]
Number of obs.	3424	7516	1795
R ²	0.6967	0.1683	-

*: significant at the 10% level.

I: intention-to-treat effect, II: average treatment effect on the treated, (ATT), III: ATT using local linear regression (LLR).

Notes: all regressions include as controls: age, age2, educa, gender, gas, bf, prural, purban, maxed, racial dummies, rural, nid*, year*. All estimates refer to income-eligible households. Age bandwidth is set to $[c-5, c+5]$, where c is the cutoff age, except for the LLR model where an optimal bandwidth is chosen.

2002 to 2007 and check the intention-to-treat effect across narrower age ranges. If there is no anticipation we would expect an increase in significance only for the period of 2004-2005, just after the cutoff age change (from 67 in 2003 to 65 in 2004). For most of the regressions analyzed in this study we can observe the increase in significance for the 2004-2005 period. We know that from 2004 onwards the number of eligibles receiving the benefit has increased

significantly ³⁰ and this change in the elderly age to receive the benefit was quite unexpected. Table 12 shows the estimates.

It can be observed that we have an increased number of members in the household in 2002 and 2003, with ages between 0 and 10 years-old and between 30 and 50 years-old.

It is worth emphasizing that when we narrow the analysis to pairs of years the year dummies will loose 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. Others members of the family seem to move in when facing difficulties. In 2002 and 2003 Brazil was facing an economic downturn with high unemployment ³¹. 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. We will address some of these results later on.

³⁰Check Figure 1 and Table 5.

³¹Different research institutes and databases showed the highest unemployment rates of the 2000's in 2002 and 2003. As an example, the Monthly Employment Survey (PME) showed in September 2002 and 2003 unemployment rates of 11.5% and 12.9%, respectively, which dropped to 10.9% and 9.6% in the following years.

Table 12 - Household composition intention-to-treat changes by period of time

	Period of time				
	2002-2005	2002-2003	2004-2005	2006-2007	2004-2005†
<i>Number of members between 0 and 10 years-old</i>					
Estimate	0.077	0.3***	-0.036	-0.007	-0.146
t-statistics	(1.22)	(2.70)	(0.47)	(0.09)	(0.47)
Number of obs.	3424	1482	1942	2302	1942
<i>Number of members between 10 and 17 years-old</i>					
Estimate	0.127**	0.129	0.133*	0.072	0.544*
t-statistics	(2.21)	(1.26)	(1.89)	(1.00)	(1.81)
Number of obs.	3424	1482	1942	2302	1942
<i>Number of members between 18 and 30 years-old</i>					
Estimate	0.025	0.093	-0.022	-0.164**	-0.088
t-statistics	(0.43)	(0.93)	(0.3)	(2.30)	(0.3)
Number of obs.	3424	1482	1942	2302	1942
<i>Number of members between 31 and 59 years-old</i>					
Estimate	0.095*	0.288***	-0.023	0.083	-0.094
t-statistics	(1.83)	(3.25)	(0.35)	(1.27)	(0.36)
Number of obs.	3424	1482	1942	2302	1942
<i>Number of members older than 60 years-old</i>					
Estimate	-0.05**	-0.061	-0.056*	0.033	-0.226*
t-statistics	(2.16)	(1.53)	(1.94)	(1.04)	(1.87)
Number of obs.	3424	1482	1942	2302	1942
<i>Number of members</i>					
Estimate	0.274**	0.748***	-0.004	0.016	-0.015
t-statistics	(2.09)	(3.13)	(0.02)	(0.1)	(0.02)
Number of obs.	3424	1482	1942	2302	1942
<i>Elder living alone or with spouse</i>					
Estimate	-0.014	-0.038	0.002	0.022	0.007
t-statistics	(0.62)	(1.07)	(0.06)	(0.84)	(0.06)
Number of obs.	3424	1482	1942	2302	1942

*, **, ***: significant at the 10%, 5%, and 1% level, respectively. †: ATT using 2SLS.

Notes: all regressions include as controls: age, age2, educa, gender, gas, bf, prural, purban, maxed, racial dummies, rural, mid*, year*. All estimates refer to income-eligible households. Age bandwidth is set to $[c-5, c+5]$, where c is the cutoff age.

5.2 Outcomes

Now we turn the attention towards other outcomes of interest, especially those related to the labor supply. Two known effects apply for any income shock: the substitution effect between leisure and labor, and the income effect. Which one prevails is often an empirical matter, however for large cash transfers there is always the concern that the income-effect of the transfer is too large, offsetting the substitution effect between leisure and labor, thereby affecting reservation wages, hours worked, and participation in the labor force.

As the BPC is addressed to the elderly, it is expected that somehow the transfer affects elderly labor supply. Actually the constitution of the program within the framework of social security underlies and somehow supports this behavior. 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 households³². In this case we should investigate possible effects of the BPC upon co-residents.

So the first outcome we analyze is the labor force participation for co-residents, presented in Table 13. It is interesting to observe a negative effect among members between 18 and 49 years-old, but no effect for members between 18 and 29 years-old

Table 14 decomposes the effect by year and age range. The only significant effect for co-residents 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 the years of 2006-2007, but before analyzing this we will look for the effects at the labor supply intensity.

Table 15 shows the effect of the benefit on the worked hours for co-residents aged between 18 and 29 years-old. The first column takes into account workers only, while the second column sets to zero the weekly working hours for non-workers. Comparing the two columns we see a significant increase in the number of observations from the first to the second one, meaning that a large number of co-residents of that age range is not working. Hence it turns out that the effect becomes more negative and more significant as well.

³²81.9% of 276 elders interviewed declared to keep none of the cash transfer for themselves (Institute of Development and Policy Management, 2003).

Table 13 - BPC effect on labor force participation for co-residents

Outcome	Labor Force Participation (week)		Labor Force Participation (month)	
<i>Intention-to-Treat</i>				
Estimate	-0.0432	-0.0321	-0.0444*	-0.0333
t-statistics	(1.59)	(0.78)	(1.67)	(0.80)
individual age period	[18,50[[2002,2005]	[18,30[[2002,2005]	[18,50[[2002,2005]	[18,30[[2002,2005]
Number of obs.	4758	2473	4758	2473
R ²	0.1488	0.1451	0.1491	0.148
<i>Treatment Effect on the Treated (2SLS)</i>				
Estimate	-0.1735**	-0.0966	-0.1453**	-0.024
t-statistics	(2.57)	(1.05)	(2.21)	(0.27)
individual age period	[18,50[[2001,2008]	[18,30[[2001,2008]	[18,50[[2001,2008]	[18,30[[2001,2008]
Number of obs.	9969	5069	9969	5069
R ²	0.127	0.1237	0.1279	0.1218
<i>Treatment Effect on the Treated (LLR)</i>				
Estimate	-0.4632*	-0.0123	-0.4953**	0.0029
t-statistics	(1.85)	(0.04)	(1.99)	(0.01)
individual age period	[18,50[[2001,2008]	[18,30[[2001,2008]	[18,50[[2001,2008]	[18,30[[2001,2008]
Number of obs.	2212	1187	2212	1187
R ²	-	-	-	-

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

Notes: Age interval is set to $[c-5, c+5]$, where c is the cutoff age, except for the LLR model where an optimal bandwidth is chosen. Income-eligible households included only. All regressions include as controls: age, age2, educa, gender, purban, prural, bf, gas, rural, nid*, year*, maxed, school, man, idcont, idcont2, racial dummies. Clustered standard errors.

When we analyze this outcome by year and age range in Table 16 we can observe that the only significant effect is for the same age range (18 to 30 years-old), reducing the number. However, if we recall Table 12, where we analyze the changes in the living arrangements, we observe that there is an effect of reduction in the members for this same age range and for the same period. It means that people in this age range are moving out, and those who are

Table 14 - 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†
<i>ages: 10 to 17 years-old</i>					
Estimate	-0.053	-0.01	-0.103	-0.041	-0.488
t-statistics	(1.09)	(0.15)	(1.52)	(0.85)	(1.31)
Number of obs.	2047	966	1081	1295	1081
<i>ages: 18 to 30 years-old</i>					
Estimate	-0.03	-0.05	-0.039	-0.069	-0.12
t-statistics	(0.75)	(0.76)	(0.77)	(1.56)	(0.67)
Number of obs.	2635	1205	1430	1559	1430
<i>ages: 31 to 59 years-old</i>					
Estimate	-0.044	-0.018	-0.08*	-0.028	-0.443**
t-statistics	(1.33)	(0.36)	(1.76)	(0.66)	(2.30)
Number of obs.	2884	1321	1563	1836	1563
<i>ages: 60 years-old or more</i>					
Estimate	0.045	-0.059	0.071	0.089	0.295
t-statistics	(0.48)	(0.43)	(0.52)	(0.87)	(0.53)
Number of obs.	397	203	194	298	194
<i>Elders members of the household</i>					
Estimate	-0.032	0.01	-0.052	-0.095***	-0.182
t-statistics	(1.05)	(0.19)	(1.29)	(2.51)	(1.32)
Number of obs.	3425	1483	1942	2305	1942

*, **, ***: significant at the 10%, 5%, and 1% level, respectively. †: ATT using 2SLS.

Notes: Income-eligible households included only. Sampling weights used. Age interval is set to $[c-5, c+5]$, where c is the cutoff age. All regressions regard the coresidents except the last row. Income-eligible households included only. Controls: age, age2, educa, gender, purban, prural, bf, gas, rural, nid*, year*, maxed, school, man, idcont, idcont2, racial dummies. Clustered standard errors.

staying participate less in the labor market for some reason. Addressing the heterogeneity of the effect we can observe that these effects are significant only for women, especially if they are living with male elders. For this same period there is some decrease in labor force participation for elders. Once again it is concentrated among male elders.

Interestingly this result is similar to what was found for South Africa. Bertrand et al. (2003) found a drop in working hours for co-residents, but in our case the effect was

Table 15 - BPC effect on weekly worked hours for co-residents

Outcome	Weekly Worked hours	Weekly Worked hours†
<i>Intention-to-Treat</i>		
Estimate	0.8049	-3.1509*
t-statistics	(0.49)	(1.68)
individual age period	[18,30[[2002,2005]	[18,30[[2002,2005]
Number of obs.	1472	2473
R ²	0.0703	0.1521
<i>Treatment Effect on the Treated (2SLS)</i>		
Estimate	1.0273	-7.3357
t-statistics	(0.28)	(1.60)
individual age period	[18,30[[2001,2008]	[18,30[[2001,2008]
Number of obs.	3054	5069
R ²	0.0614	0.1372
<i>Treatment Effect on the Treated (LLR)</i>		
Estimate	12.097	-12.951
t-statistics	(1.28)	(1.40)
individual age period	[18,30[[2001,2008]	[18,30[[2001,2008]
Number of obs.	726	1333
R ²	-	-

: significant at the 10% level. †: setting working hours as zero for non-workers. Notes: Income-eligible households included only. Clustered standard errors. Age interval is set to $[c-5, c+5]$, where c is the cutoff age, except for the LLR model. Controls: age, age2, bf, purban, prural, gas, educa, gender, rural, nid, year*, maxed, idcont, idcont2, school, man, racial dummies.

significant for women living with male pensioners, while they found it significant for men living with female pensioners. Posel et al. (2006) showed that female pensions are positively related to labor migration of women. This is also consistent with what we have found, but in their case these women migrate to look for better jobs. We cannot ensure this is what happens in our case. We do not know if these women have migrated, but only that they are not co-

Table 16 - BPC intention-to-treat effect estimates on weekly worked hours by year and age range

	Period of time				
	2002-2005	2002-2003	2004-2005	2006-2007	2004-2005†
<i>ages: 10 to 17 years-old</i>					
Estimate	-0.58	2.026	-2.532	0.163	-10.689
t-statistics	(0.45)	(1.07)	(1.56)	(0.12)	(1.18)
Number of obs.	2047	966	1081	1295	1081
<i>ages: 18 to 30 years-old</i>					
Estimate	-2.437	-2.581	-1.899	-4.295*	-2.592
t-statistics	(1.19)	(0.76)	(0.7)	(1.76)	(0.27)
Number of obs.	2635	1205	1430	1559	1430
<i>ages: 31 to 59 years-old</i>					
Estimate	-0.188	-1.132	-0.527	0.951	-3.638
t-statistics	(0.11)	(0.46)	(0.23)	(0.45)	(0.38)
Number of obs.	2884	1321	1563	1836	1563
<i>ages: 60 years-old or more</i>					
Estimate	-0.665	-2.338	0.471	2.099	-2.552
t-statistics	(0.24)	(0.56)	(0.12)	(0.58)	(0.15)
Number of obs.	397	203	194	298	194
<i>Eldest members of the household</i>					
Estimate	-1.627	-2.188	-1.111	0.636	-3.865
t-statistics	(1.57)	(1.37)	(0.83)	(0.47)	(0.85)
Number of obs.	3425	1483	1942	2305	1942

*: significant at the 10%. †: ATT using 2SLS.

Notes: Income-eligible households included only. Sampling weights used. Age interval is set to $[c-5, c+5]$, where c is the cutoff age. All regressions regard the coresidents except the last row. Income-eligible households included only. Controls: age, age2, educa, gender, purban, prural, bf, gas, rural, nid*, year*, maxed, school, man, idcont, idcont2, racial dummies. Clustered standard errors. Working hours were set to zero for non-workers.

residing with that specific family anymore. It is hard to tell why this effect is concentrated in 2006-2007, since we were expecting more significant values in 2004-2005. Our first try would be to speculate about the macroeconomic situation, but there is not any noteworthy event in this period.

The next outcome to analyze is the elders' labor supply. Table 17 presents the estimates for labor force participation and weekly worked hours. The benefit have an important effect of

reducing the elders' labor force participation. This is an important expected result. We must keep in mind that the BPC is addressed to poor people with almost no access to financial aid, and the possibility of retiring from the labor market presented by the BPC is very welcome.

Our estimates may be considered large if we take into account that in Figure 6 labor force participation is already in a low level at the cutoff age. A necessity for survival makes elders work even with health problems or extreme tired due to their advance in age, given that eligible elders have very low income. However, as soon as they start receiving the stipends they might stop working immediately, and this is consistent to our findings.

However, when we look at the weekly worked hours the effect of the benefit for those who do not retire is in the opposite direction, increasing the weekly worked hours. Some previous studies in the literature show that a similar effect arise for social programs participants in the presence of a "stigma effect". This is the hypothesis of Handlert and Hollingsworth (1969) for whom welfare policies may embarrass claimants, so participants would like to see themselves "rid" of the program as soon as possible. Mattos and Ponczek (2009) discuss this effect for Brazilian social assistance programs' participants. Although they could not perfectly isolate the stigma effect in their exercise, the evidences show that this effect is likely to occur. This increased worked hours of elderly BPC recipients may be a response to this stigma, although we cannot say it for sure. This possibility exists, but considering both the cultural standards and the increasing number of beneficiaries of Bolsa Família and BPC over the years it would be hard to stand up for stigmatization by our results.

Another comment is that the two-stage least squares estimates were significant while local linear regressions' were not. The difference between them is that LLR focuses on the observations close to the discontinuity while 2SLS poses the same weights for all observations from c to $c + 5$. If only 2SLS is significant, it can be inferred that the effect is concentrated for those with ages around $c + 3$ or $c + 4$ (relatively far from c) and that it is taking some time until elders retire or make decisions about their own labor supply.

The last variables we want to discuss concern children and teenagers: child labor and school attendance. We use data for children over 10 years-old (up to 15 years-old), participating in the labor force or not. The legislation in Brazil prohibits children working under the age 16, with the exception of professional training and apprenticeship at age 14.

Table 17 - BPC effects on labor force participation and weekly worked hours for elders

Outcome	Labor Force Participation (week)	Labor Force Participation (month)	Weekly worked hours	Weekly worked hours†
<i>Intention-to-Treat</i>				
Estimate	-0.043	-0.0322	-1.616	-1.4844
t-statistics	(1.42)	(1.05)	(0.62)	(1.56)
period	[2002,2005]	[2002,2005]	[2002,2005]	[2002,2005]
Number of obs.	3425	3425	820	3425
R ²	0.2148	0.2155	0.2299	0.2432
<i>Treatment Effect on the Treated (2SLS)</i>				
Estimate	-0.2021***	-0.1885***	15.75**	-0.5122
t-statistics	(3.13)	(2.89)	(2.30)	(0.22)
period	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]
Number of obs.	7512	7512	1768	7512
R ²	0.2031	0.205	0.1892	0.2366
<i>Treatment Effect on the Treated (LLR)</i>				
Estimate	-0.2315	-0.1655	12.55	-3.3844
t-statistics	(1.03)	(0.73)	(0.63)	(0.49)
period	[2001,2008]	[2001,2008]	[2001,2008]	[2001,2008]
Number of obs.	1476	1476	369	1476
R ²	-	-	-	-

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

†: setting working hours as zero for non-workers

Note: Income-eligible households included only. Age interval is set to $[c-5, c+5]$, where c is the cut-off age except for the LLR model. Clustered standard errors. Controls: age, age2, nid*, year*, bf, purban, prural, gas, educa, gender, maxed, rural, school, man, idcont, idcont2, racial dummies.

The Constitution also prohibits any form of night and hazardous work for anyone under 18 years. Table 18 shows the results for child labor and school attendance. We can observe an important effect of the program on reducing child labor for those under 16 years-old. The reduction in labor force participation for children is even larger than that estimated for the elder himself. The effect is particularly strong for girls in the rural areas³³.

³³The inclusion of teenagers with 16 and 17 years-old resulted in a non-significant effect. At 15 years-old students are supposed to complete primary school in Brazil. At this point some abandon the studies, while others carry on with secondary school. So probably their inclusion is boosting the variance, turning the estimates not significant.

Table 18 - BPC effects on child labor and school attendance

Outcome	Child Labor	School attendance
<i>Intention-to-Treat</i>		
Estimate	-0.0443	-0.055**
t-statistics	(0.82)	(2.09)
individual age period	[10, 16[[2002,2005]	[10,16[[2002,2005]
Number of obs.	1518	1518
R ²	0.1412	0.1582
<i>Treatment Effect on the Treated (2SLS)</i>		
Estimate	-0.225*	-0.1151
t-statistics	(1.73)	(1.43)
individual age period	[10, 16[[2001,2008]	[10, 16[[2001,2008]
Number of obs.	3285	3285
R ²	0.1277	0.1076
<i>Treatment Effect on the Treated (LLR)</i>		
Estimate	-0.6793*	-0.1332
t-statistics	(1.77)	(0.53)
individual age period	[10, 16[[2001,2008]	[10,16[[2001,2008]
Number of obs.	783	542
R ²	-	-

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

Note: Income-eligible households included only. Age interval is set to $[c-5, c+5]$, where c is the cutoff age except for the LLR model. Clustered standard errors. Controls: age, age2, nid*, year*, bf, gas, purban, prural, gender, educa, maxed, rural, idcont, idcont2, school, man, racial dummies.

Usually, in the literature, declines in child labor are associated with increases in school attendance. Edmonds (2006) found this association for the Old Age Pension program in South Africa. Ferro et al. (2010) found that Brazilian conditional cash transfer program Bolsa Escola was also associated with reductions in child labor and increases in school enrollment. Glewwe and Kassouf (2012) also found an important effect of Bolsa Família in educational achievements. However, unlike this literature we could not observe similar results for educa-

tional outcomes, only for child labor. Actually the only significant effect we found for school attendance had the opposite sign. We need to point out that in order to receive Bolsa Família stipends children must be enrolled in school and attend at least 85% of the school days, which is one of the conditionalities of the program. However, BPC does not have any conditionality.

Exploring the results we observed that the strongest negative effect occurs in 2004. The effect is also concentrated for rural boys and for girls in general, especially if they live with a female pensioner. Recalling Table 12, we observe that there is a significant increase in the number of members within this age range for this period. Assessing the heterogeneity of the change we see that it is very significant for girls, and only slightly significant for urban areas. Therefore, the only correlation we observe between the two effects is that both are significant for girls. It could be indicating that young girls not attending school are moving in to beneficiaries' households. But it is hard to tell why this effect took place in 2004. It is important to point out that like other studies in the literature Bolsa Família transfers are always positively correlated to school attendance.

6 CONCLUSION

This study uses an impact evaluation methodology to analyze the non-contributory pension program BPC on family welfare. The program provides one 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 could not verify any increased probability of elders living alone. 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. We considered elders living with the spouse as living alone as well. 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 observe changes in specific periods of time for different age ranges. Sometimes this changes comes alongside with changes in the labor force participation. We found an increased number of members between 30 and 49 years-old in beneficiaries' households. We also find decreases in the labor force participation of members between 31 and 59 years-old.

Our study shows that decreases in the co-residents' labor force participation due to the pension are feasible but they are always associated with variables other than just the extra income. Usually the effect exists but it is highly heterogeneous, concentrated in rural or urban areas, gender of the beneficiary, gender of the co-resident, time, and so on. So it is very context-dependent. This fact seems also to be true regarding the household composition. It seems that the pension is not enough to promote changes in household composition and co-residents' labor supply, they are always associated to specific conditions whereby the changes are triggered. These findings are consistent with related studies in the literature.

We also observe an important decrease in the elderly labor force participation. Actually we have an indication that there is some delay for this effect to show up. This evidence implies there is no anticipation of effects. However, as the pension can be shared within the household, other members could be anticipating the effect. But looking at co-residents' outcomes we did not verify any clear pension anticipation effects by our data.

Finally, our last findings concerns child labor and school attendance of children. We have identified an important effect of the BPC on child labor reduction, especially in rural areas where is observed with a higher frequency. Once again the effect is heterogeneous and concentrated for girls at rural areas. On the other hand, we did not observe important effects on school attendance. We must remember that the school attendance rate among children is already high in Brazil, so we would not expect an expressive effect on that. But we did not expect a negative effect either. Nevertheless, we have found this effect only in one model and it is concentrated for girls living with female beneficiaries and rural boys.

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.

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APPENDICES

APPENDIX A - Non-contributory pensions

Table 19 - Non-contributory old-age pension systems across selected countries

(continued)

Country	First laws	Type	Description
Argentina	1904	means-tested	70% of the minimum pension paid to needy elderly people aged 70 or more residing in Argentina. A 13th payment is paid yearly.
Australia	1908	means-tested	paid to men over 65 and women over 64 who have resided in Australia for at least 10 years, with at least 5 continuous years.
Denmark	1891	earnings-tested	Paid to Danish nationals over 65 years-old with at least 3 years of residence. The amount paid is based on the years of residence. The earning-test only reduce the amount paid if some threshold annual income is crossed.
France	1910	means-tested	pensioners at age 65 can receive a supplementary allowance if their annual income does not reach less than threshold value.
Israel	1957	mixed	for the earnings-tested pension the minimum age is 67 for men and 62 for women, income from work must not exceed 57% of the national average wage. It becomes universal for men over 70 and women over 67.
Russia	1922	universal	Paid to men aged 65 or more and women aged 60 or more. Beneficiaries may still work after receiving the benefit. The amount paid is a percentage of the flat-rate component of regular pensions.

Table 19 - Non-contributory old-age pension systems across selected countries

(concludes)

Country	First laws	Type	Description
South Africa	1928	means-tested	Paid to people aged 60 years or more residing in the country with an annual income less than a threshold established by the government.
South Korea	1973	means-tested	Paid to elderly people aged 65 or more with an income below a maximum set by presidential order. The monthly benefit is 5% of the average monthly income of National Pension Service participants.
United Kingdom	1908	means-tested	Paid to everyone aged 80 or older who have resided in the UK for at least 10 years in the 20-year consecutive period after age 60. Payment: less than 60% of the full basic state pension.

Source: Social Security Administration (2010).

Table 20 - Modal values for PNAD's 'V1273' variable, by year

(concludes)

2004											
5**	7****	8*	10**	15****	20**	22***	23*	25**	30****	32	35*
37***	38	40**	45****	50****	52**	55	57**	58	60***	65****	67
70*	72*	73	75**	80****	85*	87*	88	90**	95****	100**	105
110*	120**	125	130**	135	140	150*	180	200*	250	260****	300*
400*											
2003											
7**	10**	15****	18	20**	22**	25**	30****	37**	40**	45****	50**
52*	57	60***	65**	70**	75*	80**	90*	95*	100**	110	120**
125	130	135	136	140	150**	160	180	200**	240***	250*	300**
400*	450	500**									
2002											
7*	8*	10**	15***	20**	22**	25**	30***	35	37**	40**	45***
50**	52*	60**	65*	70**	75**	80**	90*	100**	120*	130	135
136	140	150**	180	200**	250*	300**	350	400*			
2001											
10**	15****	20**	25**	30***	36	40**	45***	50**	60***	70*	75*
80*	90**	100**	105	120	150*	180**	181	200**	250	300*	400

Note: The bold faced values were identified through the decomposition.

*: more than 50 observations in the sample.

**: more than 100 observations in the sample.

***: more than 500 observations in the sample.

****: more than 1000 observations in the sample.

ANNEXES

ANNEX A - BPC Legal Framework

Table 21 - Legal framework of the Benefício de Prestação Continuada, in chronological order

(continued)

Passed in	Law	Description
Jul. 24, 1991	n.8213, Art.16	Make provisions for Social Security insureds' dependants.
Dec. 07, 1993	n.8742, Art.20	It is the so-called Organic Law of Social Assistance (LOAS). It provides one minimum wage to disabled people and elderly people with 70 years-old or more and with a per capita family income of less than 1/4 of the minimum wage. 'Family' is understood as the unity living under the same roof whose economy is maintained by the contribution of its members. It also establishes that the beneficiary must not receive any other benefit from Social Security or other regime, except health care
Apr. 28, 1995	n.9032, Art.3	Establishes a new definition of Social Security insureds' dependants: the spouse, the partner, and the non-emancipated son/daughter of any condition under 21 years-old or invalid, and the non-emancipated siblings of any condition under 21 years-old, and the parents.

Table 21 - Legal framework of the Benefício de Prestação Continuada, in chronological order

(continued)

Passed in	Law	Description
Dec. 08, 1995	Decree 1744	Regulates BPC. Among the requirements for the eligibility of the elderly: do not undertake any remunerated activity, being older than 70 years-old, and per capita family income of less than a quarter of the current minimum wage. The family is still understood in LOAS terms. This decree, in its articles 18 and 19, also authorizes that the family receives more than one benefit. The first benefit however composes the family income for the calculation of the per capita family income for issuing a new benefit.
Jan. 01, 1996		First payments for BPC recipients.
Aug. 08, 1997	MP1473-34	Redefines ‘family’ as established by the Law 8213/1991 which, by its turn, had already been modified by Law 9032.
Dec. 11, 1997	MP1599-39	Reduces eligibility age for the elderly to 67 years-old.
Nov. 30, 1998	n.9720	Convert MP1473-34 and MP1599-39 into laws.
Oct. 01, 2003	n.10741	It is the so-called Statute of the Elderly (<i>Estatuto do Idoso</i>). The minimum age for claiming the BPC turns to 65 years-old. On the Art. 34 it establishes that the BPC benefit conceded to any other family member is no longer computed on the calculation of the per capita family income. However it is still in place the rule that social security pensioners or beneficiaries are not eligible.

Table 21 - Legal framework of the Benefício de Prestação Continuada, in chronological order

(concludes)		
Passed in	Law	Description
Jan. 01, 2004		The statute comes into force.
Sept. 27, 2007	Decree 6214	Establishes that the insured person must attest that his income is not enough to subsist. That is, he must attest a gross per capita family income of less than a quarter of the minimum wage. It revokes the decree 1744 for this understanding that the relevant fact is whether the beneficiary can sustain himself, and not whether he works or not.

ANNEX B - Federal Social Programs

Table 22 - Federal social programs timeline

Year**	PETI	<i>Bolsa Escola</i>	<i>Auxílio Gás</i>	<i>Cartão Alimentação</i>	<i>Bolsa Alimentação</i>	BPC	<i>Bolsa Família</i>
1993							
1994							
1995							
1996	x					x	
1997	x					x	
1998	x					x	
1999	x					x	
2000	x					x	
2001	x	x			x	x	
2002	x	x	x		x	x	
2003	x	x	x	x	x	x	x
2004	x	*	*	*	*	x	x
2005	x	*	*	*	*	x	x
2006	x	*	*	*	*	x	x
2007	x	*	*	*	*	x	x
2008	x			*	*	x	x

x : program in course.

* : with *Bolsa Família* no one should receive this program anymore. However, by 2004 and 2006 PNADs we can observe that we still have beneficiaries.

** : September of each year.

ANNEX C - Geographic Distribution of Beneficiaries

Table 23 - Beneficiaries by geographic region according to official records, PNAD supplement, and the proposed decomposition applied to PNAD data

(continued)

<i>Region\</i> State	2004			2006		
	Official records	PNAD supp.	proposed decomp.	Official Records	PNAD supp.	proposed decomp.
BRAZIL	933,164	234,668	273,308	1,183,840	585,920	655,164
<i>North</i>	81,486	8,990	11,491	107,063	75,965	87,449
Rondônia	9,151			11,390	499	962
Acre	2,378	441	573	3,416	1,468	1,380
Amazonas	16,987	612	612	21,707	40,964	49,764
Roraima	1,349			1,897	1,506	948
Pará	36,747	5,335	6,821	49,482	23,146	25,369
Amapá	5,529	418	209	7,237	1,065	1,278
Tocantins	9,345	2,184	3,276	11,934	7,317	7,748
<i>Northeast</i>	293,234	139,232	161,857	376,465	251,870	276,749
Maranhão	42,183	3,985	3,985	58,188	27,564	30,906
Piauí	7,119	1,568	3,135	10,468	7,358	7,358
Ceará	37,233	24,691	25,777	48,532	33,579	36,952
Rio Grande do Norte	7,386	9,831	9,831	10,766	8,065	9,962
Paraíba	16,511	1,808	1,808	20,859	8,512	9,931
Pernambuco	58,619	55,229	67,325	74,847	73,494	79,089
Alagoas	17,194		454	24,588	8,259	9,229
Sergipe	7,802	3,680	3,680	9,538	5,134	4,791
Bahia	99,187	38,440	45,862	118,679	79,905	88,531

Table 23 - Beneficiaries by geographic region according to official records, PNAD supplement, and the proposed decomposition applied to PNAD data

(concludes)

<i>Region\</i> State	2004			2006		
	Official records	PNAD supp.	proposed decomp.	Official Records	PNAD supp.	proposed decomp.
<i>Southeast</i>	366,307	40,435	50,578	457,763	147,207	171,824
Minas Gerais	94,807	17,402	21,845	114,561	65,925	69,227
Espírito Santo	13,019	2,348	2,818	16,273	3,613	4,065
Rio de Janeiro	71,295	4,356	4,356	95,304	15,962	21,252
São Paulo	187,186	16,329	21,559	231,625	61,707	77,280
<i>South</i>	94,415	10,948	14,649	118,066	37,274	43,670
Paraná	46,977	6,361	6,361	57,600	21,758	24,174
Santa Catarina	10,175		579	13,102	1,728	2,304
Rio Grande do Sul	37,263	4,587	7,709	47,364	13,788	17,192
<i>Mid- West</i>	97,722	35,063	34,733	124,483	73,604	75,472
Mato Grosso do Sul	24,491	8,944	9,253	30,396	20,262	19,034
Mato Grosso	25,680	990	1,320	31,443	9,809	11,116
Goiás	31,487	18,969	17,590	41,148	32,993	34,369
Distrito Federal	16,064	6,160	6,570	21,496	10,540	10,953

Source: Official records for December were obtained in AEPS Infologo – Ministry of Social Security (Previdência). PNAD 2004 and 2006 were carried out in the month of September. PNAD supplement includes those who declared to be beneficiaries in the PNAD presenting compatible values declared on ‘V1273’.

ANNEX D - List of variables

Table 24 - Variables' codes and description

(continued)

Variable	description
Labor force participation (month)	1 if the person is working or searched for job in the last month
Labor force participation (week)	1 if the person is working or searched for job in the last week
Child labor	1 if the child works or searched for job in the last month
c	cutoff age (70 until 1997, 67 until 2003, and 65 from 2004 onwards.)
D	Household and members are age-eligible. $D = \mathbb{1}[age \geq c]$
A	same as 'age' (age of the BPC recipient or oldest member of the household)
V1273	savings account and other financial applications, dividends, and other source of income

BPC recipient or oldest member of the household

age	continuous age
educa	schooling attainment
gender	1 if male
black	1 if the person is black colored
yellow	1 if the person is yellow colored
pardo	1 if the person is pardo

individual characteristics

idcont	continuous age
school	schooling attainment
man	1 if male

Table 24 - Variables' codes and description

(concludes)

Variable	description
black	1 if the person is black colored
yellow	1 if the person is yellow colored
pardo	1 if the person is pardo
household characteristics	
rural	Rural household
year*	Year dummies
bf	1 if a member receives the Bolsa Familia program
prural	1 if a member receives the Rural PETI program
purban	1 if a member receives the Urban PETI program
gas	1 if a member receives the Vale Gás program
nid1	number of children under 10 years-old
nid2	number of members with ages from 10 to 20 years-old
nid3	number of members with ages from 20 to 30 years-old
nid4	number of members with ages from 30 to 40 years-old
nid5	number of members with ages from 40 to 50 years-old
nid6	number of members with ages from 50 to 60 years-old
nid7	number of members aged 60 years-old or more
maxed	schooling attainment of the most educated member of the household