Spillover Effects of a Brazilian Pension Scheme on Labor Force Participation

Pedro Rodrigues de Oliveira

Doctoral Student at Department of Economics ESALQ, University of São Paulo, Brazil
E-mail: oliveirapr@usp.br

Ana Lúcia Kassouf

Department of Economics ESALQ, University of São Paulo, Brazil
E-mail: alkassou@esalq.usp.br

1. Introduction

Conditional Cash Transfer (CCT) 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 attaining school and avoiding child labor, among other goals. The BPC[1] 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 or prove to be incapable to work, besides attesting a per capita family income no greater than 25% of the current minimum wage. It is addressed therefore to very poor families.

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 paper, nevertheless, assesses non-contributory pension benefits. Programs of this kind are being undertaken in many countries (To 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)). 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

---

[1] Acronym for *Benefício de Prestação Continuada.*

programs are carried out in OECD countries, but they are also present at Eastern Europe and in the Developing World.

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 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 household with a beneficiary of non-contributory pension is reduced in 18 percentage points in Brazil, and in 12.5 percentage points in South Africa. Nevertheless, endogeneity problems concerning the income sources and possible changes in family structure due to the non-contributory 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 transfer 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, for very poor household, 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 results are that, in general, the expenditures made with the pension receipts were quite similar to those of non-pension incomes. Also, male-headed households have different consumption patterns than women-headed households.

Duflo (2003) 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 person eligible, 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, Mammen, and Miller (2005), using a discontinuous regression approach, study the effects of the South African program in living arrangements for elderly black women. They assume that changes in living arrangements with no 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 of 60). They find no evidence that the additional pension income leads to an increased propensity to live alone. Instead, the pension leads to a decline in the co-resident women in their 30s (who can work away), and an increase in the presence of young children (less than 5 years old) and women whose age 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 works dealing with the negative effects of cash transfers on labor supply are Bertrand, Mullainathan, and Miller (2003), for South Africa, and Carvalho Filho (2008a), for Brazil.

Some other papers 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 evidences of increases in schooling attainment and decreases in child labor 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 attain school. Carvalho Filho (2008b), and Kruger, Soares and Berthelon (2006) show

that rural pension have increased the enrollment rate and diminished youth's participation in the labor market.

Carvalho Filho (2008b) uses a Brazilian social security reform to estimate its effect on child labor and enrollment rates of children (10 to 14 years old). The reform affected some children but not others. Then, the effects are identified from the difference in the outcomes of children affected or not by the reform. Old-age benefits increase the enrollment rates of girls by 6.2 percent, with smaller effects for boys, and reduce children labor supply. Girls labor participation 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.

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 have proven to have important spillover effects within the household.

This paper presents some evidence on the effects of the BPC on labor force participation of beneficiaries and their co-residents. The next section details the program and its expected effects. Section 3 details the database, the decomposition of values of the economic transfers from the government through the PNAD database, and the validation of the procedure. It also presents some descriptive statistics and the methodology to be implemented. Section 4 presents some results concerning labor force participation of beneficiaries. Section 5 concludes.

2. The program and its expected effects

Enacted in the 1988 Constitution and regulated in 1993, the BPC benefit started being paid in 1996. The Ministry of Social Development (MDS) is in charge of the coordination, implementation, financing, and monitoring of the BPC. Its operationalization is 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 old. The benefit may be paid to every old-aged person with a per capita family income no greater than 25% of a minimum wage and with no social security aid or any other retirement plan fund. There can be more than one beneficiary in the same family. In this case, the 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 receive the BPC also, since the income eligibilities are met.

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

Table 1: Evolution in the number of BPC recipients

Year	Total	Elderly	Disabled
1996	346219	41992	304227
1997	645894	88806	557088
1998	848299	207031	641268
1999	1032573	312299	720274
2000	1209927	403207	806720
2001	1339119	469047	870072
2002	1560884	584597	976287
2003	1687519	659433	1028086
2004	2061013	933164	1127849
2005	2277365	1065604	1211761
2006	2473696	1180051	1293645
2007	2680823	1295716	1385107
2008	2934472	1423790	1510682

Source: IPEADATA

In 2008 the BPC budget was approximately US$ 8.2 billion, while the *Bolsa Familia* budget was US$ 4.4 billion. The BPC program benefited near 3 million people[2], 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 high, compared to other programs.

Based on PNAD 2006 survey[3], the largest values received by a single beneficiary of *Bolsa Familia* is below R$150 (US$ 88 on today's currency[4]). So, the

---

[2] Accounting for elderly people and people with disabilities.
[3] The Brazilian National Households Survey (PNAD) is carried out annually since 1967. It is a micro database, including a wide variety of socioeconomic information of the household and dwellers. It will be further explored ahead.
[4] Considering an exchange rate of R$1.7 per US dollar.

amount of the BPC benefit (R$350, or US$205) is about 2.3 times higher than the highest transfers of *Bolsa Familia* program. Therefore we may expect important effects of this income transfer on inequality and on the beneficiaries' quality of life.

BPC is supposed to be addressed to very poor families. Preliminary analysis from PNAD 2006 shows that 65.9% meet the income eligibility criterion, and, from those, 58.8% are women[5]. That is, 65.9% of the 3,084 beneficiaries identified in the sample have a family *per capita* income of less than 25% of the minimum wage. If we consider a family income of 50% the minimum wage as the poverty line, then 83.9% of beneficiaries are poor. About 94.5% of the beneficiaries belong to families with 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, a higher children's enrollment in school, among others.

This paper intends to evaluate the labor force participation of the elders who benefited from the BPC in comparison to those who did not. The BPC may allow these people to retire from the labor market, which would not be possible otherwise. Therefore we expect a lower participation rate of the elders in the labor market. Some spillover effects could be associated with the benefit. The co-resident would be more prone to leave labor market. Situations like these includes those when they worked only to sustain the household, or if the individual had a bad job and the extra income allowed him to look for a better job, or if he quits his job to study, for example. These effects are still to be evaluated.

3. Data and methodology

The source of our data is the annual household survey carried out in Brazil, PNAD, in the period 1993-2008. Some years of the survey includes specific supplements with thematic questions about health, child labor, among others. In collaboration of the

---

[5] 59.73% of recipients are women.

Ministry for 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 new questions related to the *Bolsa Familia* program, BPC, and the Child Labor Eradication Program (PETI), among others.

However, this annualy 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 this problem, we can still identify the program in which an individual is beneficiary through the eligibility criteria, such as wage, household income, age, household composition and the amount of money paid by each governmental program. This approach can be used annually in PNAD, even in years without the special supplement.

The amount paid by the social programs is computed in the variable coded V1273, described as: "savings account[6] and other financial applications, dividends and other income". It is very unlikely to find shareholders and those who receive interest from any financial application as beneficiaries of social programs. However, the amount paid by the social programs are known, and through the values declared in this variable we can deduce which program the individual is receiving.

Barros et al. (2007) use the typical value transferred by each social program from the government (BPC, *Bolsa Família*, *Bolsa Escola*, *Bolsa Alimentação*, *Cartão Alimentação*, *Auxílio Gás*, and PETI) to identify beneficiaries from each program. All individuals receiving exactly one minimum wage were identified as BPC beneficiaries. The 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* and 2,911 receiving the BPC. From these 2,911, almost 20% also receive the *Bolsa Família* program.

In Table 2 there is an example of the disaggregation procedure proposed, using values for the variable V1273 (interest and other 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

---

[6] In Brazil, there is a traditional and conservative financial investment called *"caderneta de poupança"*, which was translated here as 'savings account'. This investment is a very low risk one, with values insured by the government, and monthly rentability established as 0.5% + TR. The TR is an interest rate calculated by the government and indexed by the average value of the interest rates of private sector Certificate of Deposits. This investment is popular among low income investors.

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 (BPC + *Auxílio Gás*)

282 = 260 + 15 + 7 (BPC + *Bolsa Família* + *Auxílio Gás*)

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

Table 2: Values for variable 'V1273' for individuals in households declared to have 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.

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

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 the procedure with those identified by the PNAD supplements available in 2004 and 2006. In those years there are 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.

Table 3: Identification of beneficiaries

	Identified Sample	Identified Population	PNAD Supplement (sample)	PNAD Supplement (pop.)	Official Records
PNAD 2004					
total	2371	1006002	1629	670235	1983788
elders	695	273308	588	225897	885236
elders/total	29%	27%	36%	34%	45%
PNAD 2006					
total	4158	1753815	2959	1231936	2430125
elders	1590	655164	1380	566478	1158005
elders/total	38%	37%	47%	46%	48%

Source: 2004 and 2006 PNAD.
Note: The population value was obtained using the database weights.

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 above approach 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 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 these 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 the impression to him that indeed he receives a regular social security pension. Soares et al. (2006, p.17) also discussed this issue. However, since 2004, when a bill regarding the rights of the elders was passed, the program became more popular. This can help explain the rise in the proportion of elderly beneficiaries from 2004 to 2006, while in the official records this proportion roughly remained steady.

We have to know whether the individuals indentified as beneficiaries by the proposed disaggregation are really BPC beneficiaries or their income is originated from interest or dividends. Some individuals were identified as beneficiaries even living in households where, by the PNAD supplement, there was no BPC beneficiaries. We can classify the beneficiaries into two groups:

Group 1: compound by elders identified by the procedure and by the supplement.

Group 2: compound by elders identified as beneficiaries by the procedure, but not by the PNAD supplement.

To check if the method is correctly identifying beneficiaries, we can compare important characteristics of both groups. We expect them not to differ too much.

For the 2004 PNAD, 94 of the 695 elders were in group 2. From these, 86 (91.5%) do not receive a salary, and 74.5% have a per capita household income of less than a minimum wage. For 2006, 182 of the 1590 elders were in group 2. From these, 172 (94.5%) do not receive any salary, and 61.5% have a per capita household income of less than a minimum wage.

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 93% have no more than 4 years of schooling. In group 2, these percentages are 63.8% and 90.4%, respectively. Therefore, both groups are very similar.

This lead us to believe that individuals who were identified as beneficiaries and who declared not to did 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 from several years of PNAD is compound of 3,292,003 observations. Each year has from 5% to 8% of the total of observations. All individuals considered are in the working age of 18 or more. Although there is a large number of observations, few of them are elderly BPC beneficiaries, and fewer are income-eligible. Table 4 presents the numbers of beneficiaries by year.

Among all individuals considered, elders receiving the benefit amount to 5,654 observations. However, when we consider the income-eligibility of those elders the figure drops to 1,734 observations, which means that most of the individuals in the sample do not meet the income-eligibility rules. One reason is that the source of this high 'inegibility' is the definition of family of the PNAD database, which is different from the definition of family in the BPC law. Although we tried to control for such differences, it might still remain in the sample.

The fact that most beneficiaries are not income eligible does not mean necessarily that they are wealthy. Although apparently in the income distribution presented[7] in Table 4 most of the non-eligible (in terms of income) beneficiaries are in percentiles 31 to 70, way up in the distribution than those income-eligibles in general, the per capita income of someone in the 70th position in 2008 is around R$470 (US$ 246 in the currency of that time[8]). Someone in the 31st has a per capita income of around R$171 (US$ 89). That is really not too much for a living in Brazil.

---

[7] It is important to mention that the variable give percentiles for the whole population evey year, and not only for eligibles.
[8] R$ 1,91 per US dollar.

The number of treated people is bigger when we include the co-residents in the analysis. Out of 3,292,003 observations, 943,481 observations are residents in households who are income-eligible for the benefit. Of this amount, 174,317 observations are individuals living in households with someone age-eligible for the benefit. But treated individuals and co-residents sum up to 5,022 observations. Table 5 shows these numbers, presenting also the labor market participation share of each group. All standard deviations were very small, casting no doubt on the difference of the calculated means, and therefore were not reported.

Table 4: Frequency of elderly beneficiaries per year and frequency of non-eligibles receiving the treatment by household income percentiles.

year	all elders	income-eligible elders	frequency of non-eligible beneficiaries by household income percentiles (max:100)				
			10 to 20	21 to 30	31 to 50	51 to 70	>70
1996	1	0	0	0	0	1	0
1997	3	1	0	1	0	0	1
1998	17	4	3	0	6	3	1
1999	111	42	3	6	36	17	7
2001	40	12	0	2	15	7	4
2002	107	35	1	5	27	13	26
2003	78	18	2	3	9	27	19
2004	653	210	3	29	74	267	70
2005	818	262	9	24	85	326	112
2006	1,475	460	10	64	192	532	217
2007	1,126	356	13	48	142	441	126
2008	1,225	334	9	53	466	197	166
Total	5,654	1,734	53	235	1,052	1,831	749

Table 5: Labor force participation in the sample

	all households			income-eligible households			income and age-eligible households		
	all	elders	co-residents	all	elders	co-residents	all	elders	co-residents
mean	.6475	.6835	.6184846	.5283487	.5629088	.5072956	.3912699	.2284806	.4695414
N	3292003	1471864	1820139	943481	357168	586313	174317	56600	117717

	income and age-eligible households with at least one treated			income and age-eligible households with no treated people		
	all	elders	co-residents	all	elders	co-residents
mean	.3452808	.1522556	.4352014	.3926342	.2306923	.4705707
N	5022	1596	3426	169295	55004	114291

We can see that the labor force participation decreases when we add eligibilities. The interesting result is the shift in the labor force participation when the age-eligibility is imposed. In this case we are analyzing a sample of households with at least one individual older than 65 years-old. Therefore, the probability of being in the labor force decreases simply because elders tend to retire from the labor market. If we compare the values for co-residents we see no big changes (0.507 to 0.469) due to age-eligibility in the household.

Table 6: Sample means and standard deviations of covariates for treated and non-treated age-eligible households

Variable	Statistic	Treated			Non-treated eligibles		
		all	elders	co-residents	all	elders	co-residents
educa	Mean	1.44	1.4	1.46	2.53	2.59	2.5
	Standard Dev.	.032	.057	.039	.009	.016	.01
maxed	Mean	7.85	7.19	8.16	8.29	7.73	8.57
	Standard Dev.	.054	.098	.063	.01	.018	.012
gender	Mean	.33	.31	.34	.49	.46	.5
	Standard Dev.	.007	.012	.008	.001	.002	.001
homem	Mean	.44	.31	.5	.43	.44	.42
	Standard Dev.	.007	.012	.009	.001	.002	.001
escola	Mean	4.08	1.39	5.33	4.86	2.56	5.96
	Standard Dev.	.059	.057	.074	.011	.015	.014
idade	Mean	51.57	73.83	41.19	51.3	74.4	40.18
	Standard Dev.	.304	.179	.305	.052	.029	.048
negro	Mean	.09	.09	.09	.08	.09	.08
	Standard Dev.	.004	.007	.005	.001	.001	.001
pardo	Mean	.62	.59	.63	.49	.45	.5
	Standard Dev.	.007	.012	.008	.001	.002	.001
rural	Mean	.16	.17	.16	.21	.21	.21
	Standard Dev.	.005	.009	.006	.001	.002	.001
inc_p	Mean	31.92	33.86	31.02	38.52	40.61	37.52
	Standard Dev.	.279	.446	.351	.064	.109	.079
	N of obs.	5022	1596	3426	169295	55004	114291

Note: the demographic composition of the households showed only marginal differences. The same applies to regional differences.

The main comparison group to the group of treated households used in this paper is the non-treated eligibles. Comparing them we can observe that labor force participation of elders, when there are someone treated in the household (in most cases

himself), is smaller than in those households with no one treated. There is also a smaller percentage of co-residents participating in the labor market. To know if these differences are due to the program, we must check besides the program other individual and household characteristics. Table 6 shows some characteristics for treated and non-treated to check whether these groups differ. All variables used, with their respective codes, are displayed in Appendix A.

As it can be observed in Table 6, there are no big differences between the two groups. There are marginal differences between the groups when we consider schooling, gender of the oldest member of the household (or beneficiary), per capita income, and proportion of rural households – however these small differences may be controlled through the proposed methodology.

To understand how we exploit the discontinuity in the eligibility age we present now some statistics focusing on the discontinuity generated by the program rule. Using the 2006 PNAD we describe in Figure 1 the number of beneficiaries. Clearly there is a sharp increase in the number of beneficiaries at the age of 65. 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 of 65, where the elderly become eligible.
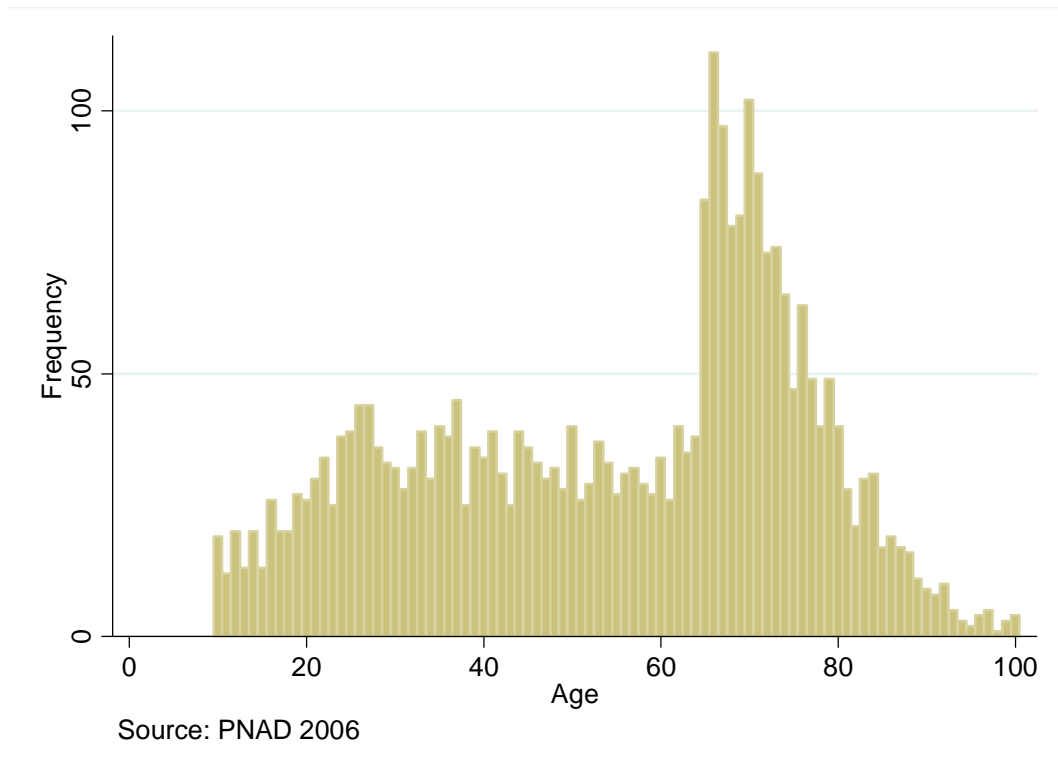

Source: PNAD 2006

Figure 1: Beneficiaries by age

13

In Figure 2 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 of 65.
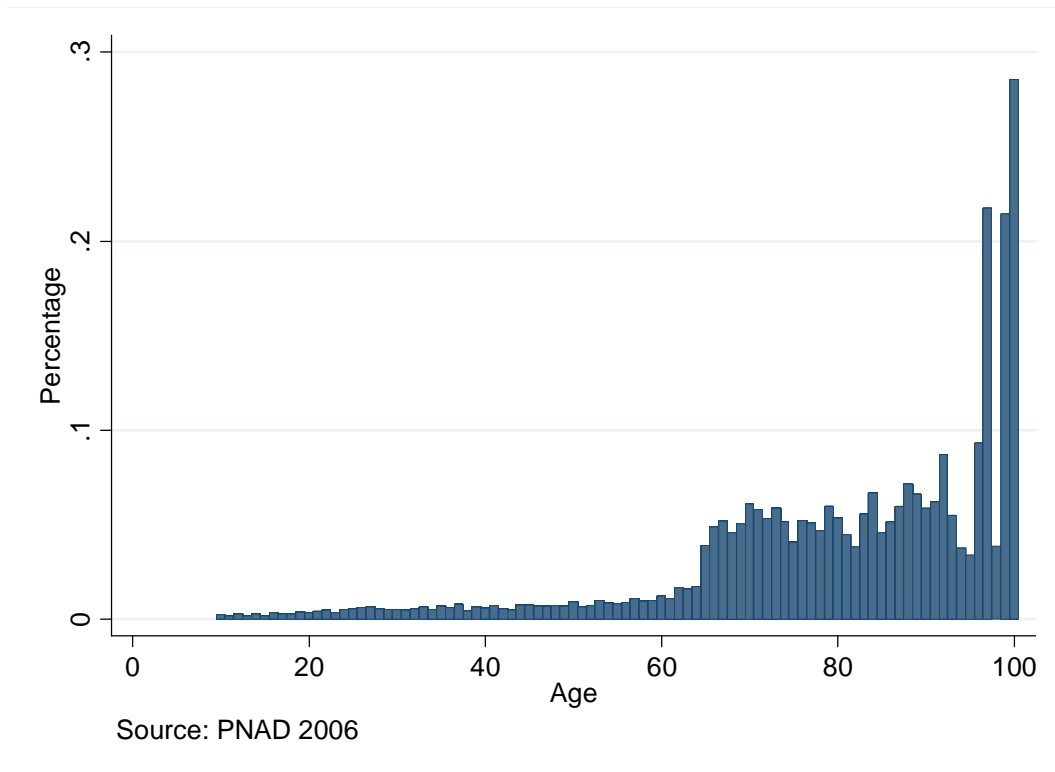

Source: PNAD 2006

Figure 2: Percentage of beneficiaries in the population, by age
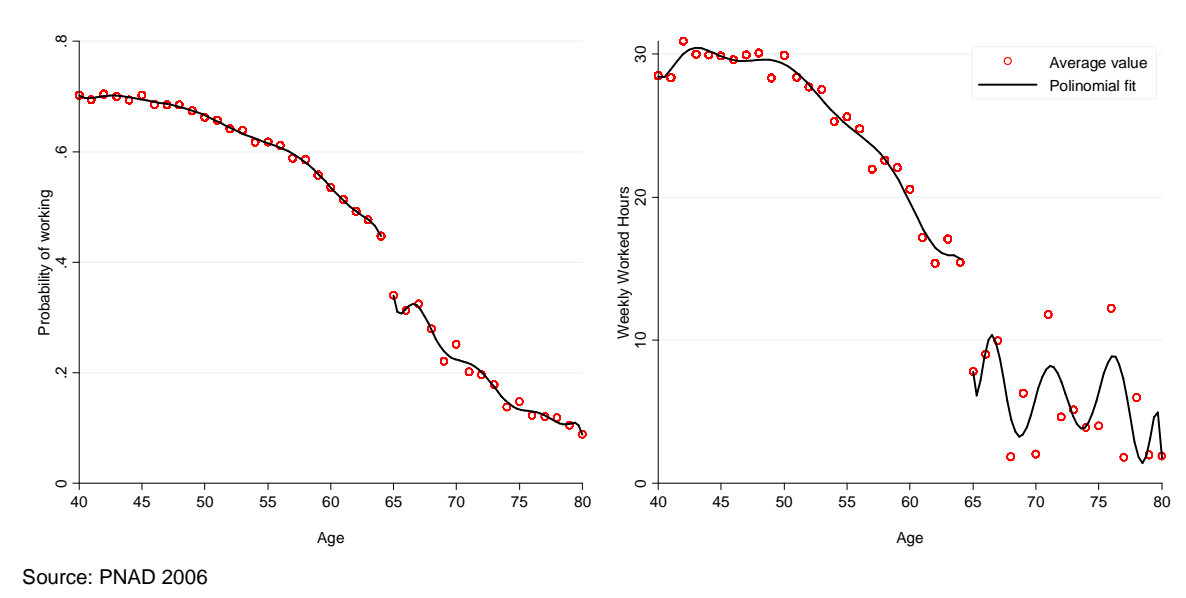

Source: PNAD 2006

Figure 3: Probability of working and weekly worked hours for the oldest in the household

Figure 3 shows the probability of working and the weekly worked hours for the oldest person in the income eligible households, whether beneficiary (when the person is older than 64) or not. The red circles are the average predicted probability of a logit model for the working variable, and the average worked hours in the weekly worked hours variable. The line is a high order polynomial fitted function.

Apparently there is a discontinuity at the age 65. Perhaps this discontinuity may apply not only to the beneficiary himself, but also to other people in the household – if there is a spillover effect of the benefit. This is addressed in Figure 4, where we see the probability of working for men and women.
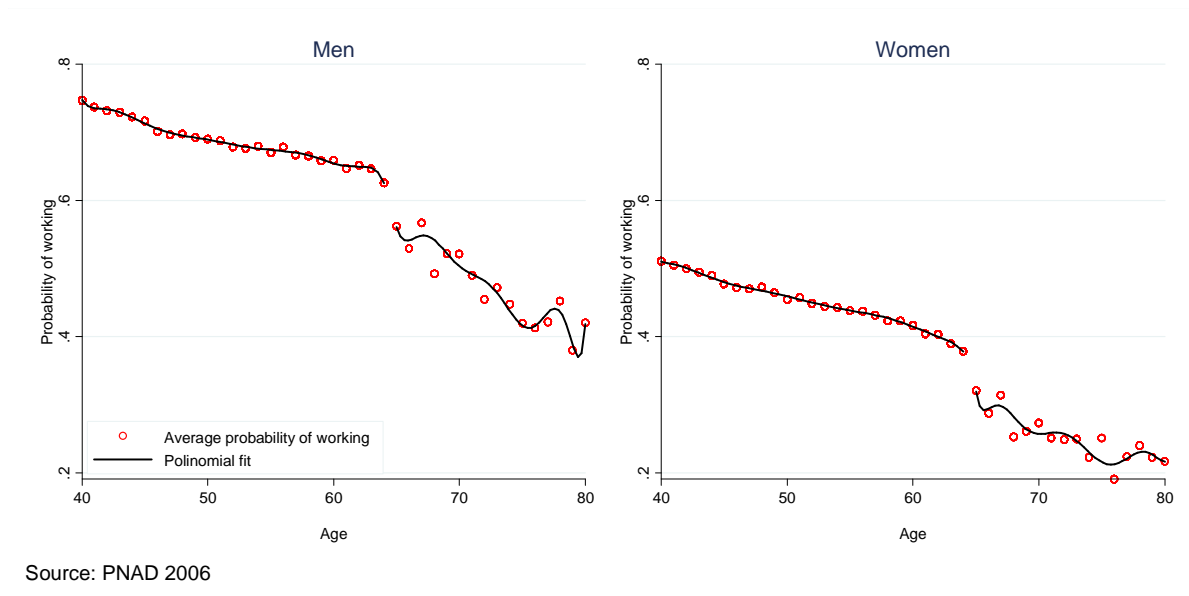

Figure 4: Probability of working by gender (over 18 years-old)

It is important to point out that "age" in Figure 4 refers to the age of the oldest person in the household (beneficiary or not) and not to the age of the person in question, as described in Appendix A. However, the age of the person is taken into account in order to predict the probability of working. The oldest person in the household, considering that in the subsample used all are eligible for the benefit, is the one who will first receive the benefit when meeting the age eligibility criterion, and then the household will have a beneficiary. It seems better to compare a household with a beneficiary (over 64) with those households that will soon have a beneficiary.

Finally, it is important that we observe no sudden shifts in the covariates (household and individual characteristics) at the age 65. Some characteristics are plotted in Figure 5.

The first graph shows the number of years of education for the oldest person in the household. The second and third plots show the average number of children in the household and the household size, respectively, both indicating no changes at the age of 65. The last plot shows the proportion of male beneficiaries by age. It is expected a natural decrease in this proportion since women live longer. Actually, male

beneficiaries are those represented by the blue line. The black line is the proportion of males who are the oldest in the household.
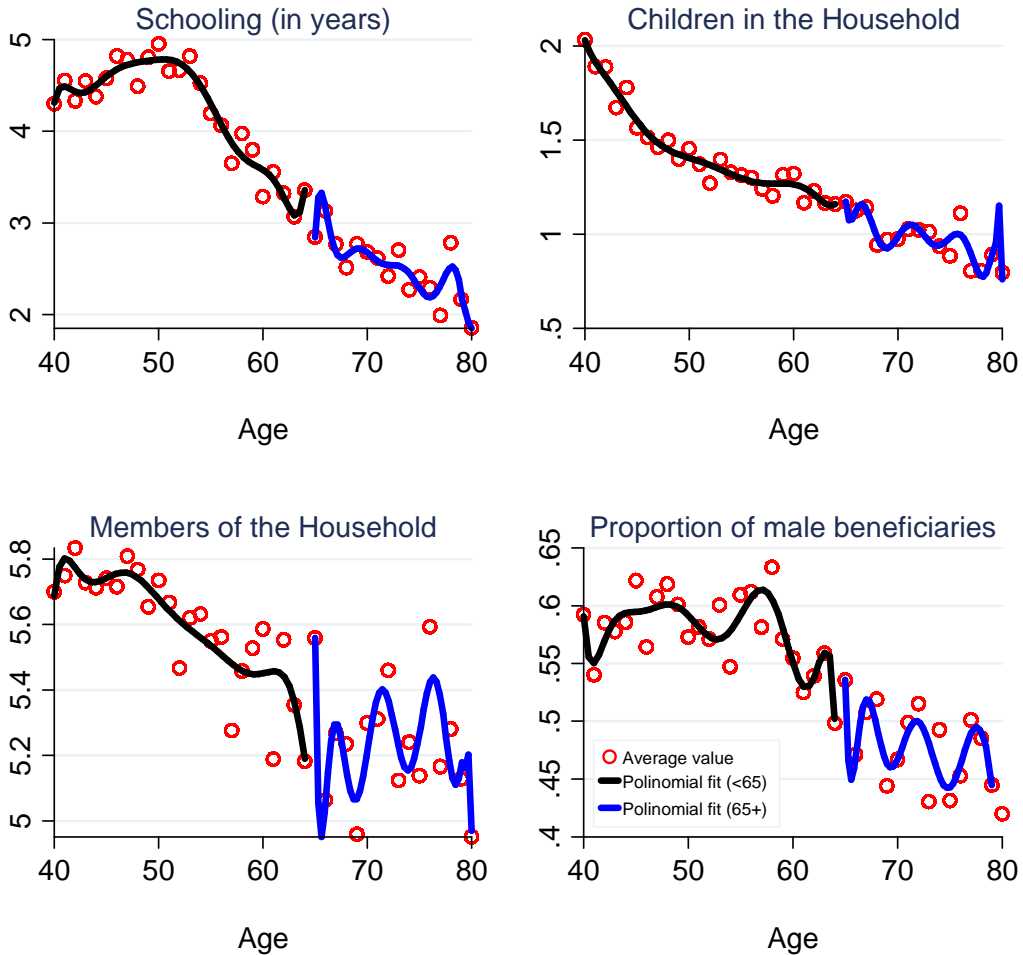
Source: PNAD 2006

Figure 5: Individual and family characteristics.

Another dimension to be considered is the rural-urban differences. Figure 6 displays the proportion of eligibles living in rural areas. We can see a considerable proportion of eligibles living in rural areas, ranging from 20% to 30%. Although more than 80% of the Brazilian population lives in urban areas, the proportion of poor people is higher in rural areas. Preliminary findings suggest that there are major differences in the size of the effect depending on whether the household is rural or urban. The probability of working, for example, tends to be much higher in rural areas than in urban ones, despite of the presence of a beneficiary.
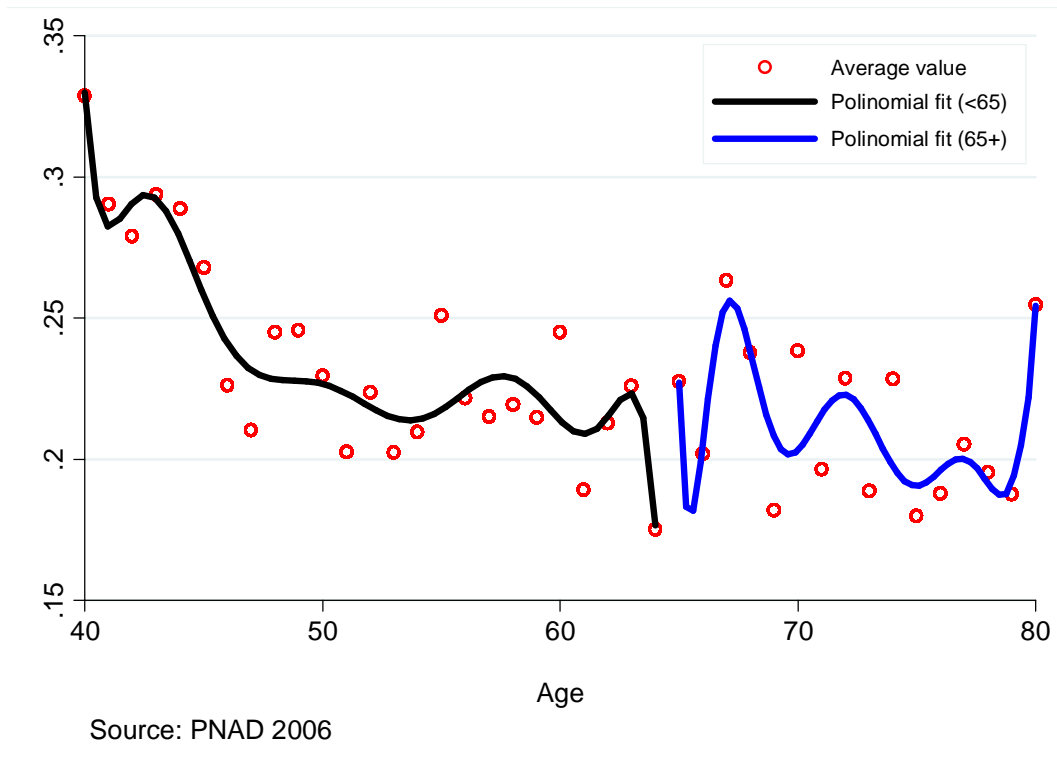
Figure 6: Proportion of eligibles living in rural areas

3.3 Methodology

The ideal design for the statistical evaluation of a program (or treatment) is the experimental one, where the treatment is randomly assigned with *ex-post* evaluation of those who received the treatment (treatment group) with those who did not (control group or comparison group). However, when it comes to social assistance, it would be hard to convince any public manager to adopt such a design, for ethical or political reasons. Therefore, the treatment group is selected through a non-experimental design, according to eligibility criteria. Our methodology takes this into account, in a way that our data can be "corrected" to a quasi experimental design.

The first methodology proposed for the evaluation of the BPC program considers it as a regression discontinuity design program. Such design may arise when treatment is assigned due to organizational or administrative rules. For example, Angrist and Lavy (1999) studied the effect of the class size on students' performance, using data from the "Maimonides' Rule", which establishes that the class must be split into two when the number of students reaches a certain number. Van der Klaaw (2003) analyzed the financial aid effect on high school dropout rates, using the administrative rule that only those who reached a given score on SAT would be eligible for the aid, and Thistlethwaite and Campbell (1960) studied the impact of scholarships assigned to students who scored a given level of points at a test. The idea was that individuals with scores just below the cutoff were good comparisons to those just above the cutoff.

In the simplest design of the regression discontinuity, called "sharp RD", individuals receive the treatment based on a continuous measure, called selection or "assignment variable". Those who are under a cutoff value do not receive the treatment, and those above do receive the treatment ($D = 1$) or the probability of treatment jumps from 0 to 1 when X (assignment variable) crosses the threshold c. Consider the regression,

$$Y = \alpha + \tau D + f(X - c) + \varepsilon$$

where τ is the treatment effect of interest.

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

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

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

$$\tau_F = \frac{\lim_{\varepsilon \downarrow 0} E(Y | X = c + \varepsilon) \quad - \lim_{\varepsilon \uparrow 0} E(Y | X = c + \varepsilon)}{\lim_{\varepsilon \downarrow 0} E(D | X = c + \varepsilon) \quad - \lim_{\varepsilon \uparrow 0} E(D | X = c + \varepsilon)}$$

which, as an instrumental variable setting, the treatment effect can be recovered by dividing the jump in the relationship between Y and X at c by the discontinuity jump in the relation between D and X. In the fuzzy RD, the probability of treatment is:

$$Pr(D = 1 | X = x) = \gamma + \delta T + g(x - c)$$

where $T = 1[X \geq c]$ indicates whether the assignment variable exceeds the eligibility threshold c.

Therefore, the fuzzy RD design can be described by the two equation system

$$Y = \alpha + \tau D + f(X - c) + \varepsilon$$

$$D = \gamma + \delta T + g(X - c) + v$$

The reduced form equation is then,

$$Y = \alpha_r + \tau_r T + f_r(X - c) + \varepsilon_r$$

where $\tau_r = \tau . \delta$ and can be interpreted as an "intent-to-treat" effect. Estimation can be performed using either the local linear regression approach or polynomial regressions. The model is exactly identified and two stage least squares can be used. However the local linear regression performs better with a continuous assignment variable (sometimes called running variable), what is not case for age. Hence, a Wald estimator[9] for binary instruments seems more appropriated.

---

[9] From Wald (1940).

In this paper, the cutoff point c equals 70 from 1996 on, 67 since 1998, and 65 since 2004. On the surroundings of this value, the individuals are very similar. However, above 65 some are beneficiaries and below they are not.

In this study Y is the outcome variable (works or not), X is age, D is 1 if a person receives BPC pension program and 0 otherwise and T is one if a person is 65 years old or older and 0 otherwise. The sample is composed of income eligible households.

Lee and Lemieux (2009) point out to some important issues for the analysis of age discontinuities. One is that individuals may fully anticipate the change in the regime and, therefore they may behave in certain ways prior to the time when treatment is turned on. We will use the survey time period from the 90's up to 2007, but we will check the anticipation issues using the years 1993-1996, given that the program was implemented in 1996.

We are using similar approach as Martinez (2005) who analyzed the impact of Bonosol pension to elderly Bolivians, i.e., we use the program's eligibility rules plus data from 1993 to 1996. The regression discontinuity design compares eligible to ineligible households around the eligibility cutoff, and a difference in difference approach compares similar households in pre and post treatment periods. Actually there is more than one exogenous change to exploit, due to changes in the age-eligibility criterion in 1999 and 2003.

We are also just looking at the short-run effects even though there may be a long-run effect. The reason is that, even if there is truly an effect on the outcome, if the effect is not immediate, it generally will not generate a discontinuity in the outcome. We understand that labor-force participation is a short-run effect.

The consistency of the regression discontinuity estimator requires the assumption that the outcome of interest is continuous at the age cutoff if there were no pension program or no treatment.

A second methodology used is the Difference-in-differences estimator. Consider the variable of interest Y. If we want to check in time differences in Y between treatment control groups, we could estimate an equation of the form

$$Y = \alpha + \beta\, treat + \theta\, after + \delta\, treat \cdot after + \mu\, X + u$$

where *treat* equals one if the observation is in the treatment group and zero otherwise, and *after* equals one if the observation is in the period after the implementation of the treatment and zero otherwise, and X is a vector of characteristics, controlling for all differences that may exist between treatment and control groups. If $E[u|treat, after, X] = 0$, we can say that δ is the gain that treated individuals have in comparison to the control ones.

The third method is the propensity score matching method, proposed by Rosenbaum and Rubin (1983). The idea is to match treatment and comparison groups units on their covariates, matching the most similar ones in terms of observable characteristics. However, it would be very unlikely to match two observations in all

variables. This matching gets more unlikely the more variables are added to the vector of characteristics. Hence, the method proposes the reduction of dimensionality using the propensity score – the probability of receiving the treatment conditional on covariates.

$$P(X_i) = Pr[D_i = 1 \mid \mathbf{X}_i]$$

The mean difference on their outcomes gives the average treatment effect on the treated, that is, the mean effect of the treatment on *Y*. Further discussion on propensity score matching is found in Dehejia and Wahba (2002).

A combination of this method with the difference-in-differences estimator is feasible. Once we do not have the treatment group before the implementation of the treatment, a matching must be performed in order to identify in the control group the most similar individuals to those in the treatment group, assigning them as the treatment group before the treatment implementation.

4. Results

All the results presented here refer to labor force participation of the elders and co-residents in the discontinuity sample, with ages[10] ranging from 60 to 75 years-old. The difference-in-differences estimates were estimated by ordinary least squares. Marginal effects for a logit model could also be run and displayed, however the differences to a linear model were quite insignificant, and for simplicity the linear model was preferred.

Table 7 presents difference-in-difference estimates testing whether the eligible group has any difference before and after the treatment period. We expect that only the treatment – and not the eligibility for the program itself – affects labor force participation. Therefore we expect no effect of the eligibility on labor force participation (P * eligible = 0).

We observe that for all income-eligible households there is no effect of being eligible in the probability of working. The effect is slightly significant however when elders and co-residents are considered together. Without the restriction of income-eligibility, the model captures slightly significant differences in the probability of working for co-residents and elders. When we exclude the income-eligibility rule we are adding to the sample more people who works and therefore earn more. Hence, we can see that the treatment effect is positive for co-residents without the restriction of income-eligibility.

---

[10] See Appendix A for the definition of 'age'.

Table 7: Difference-in-differences estimates for eligibility effects on the probability of working.

	income eligible households					
	all		co-residents		elders	
	coeff	p-value	coeff	p-value	coeff	p-value
P	-.0435816	0.000	-.0381525	0.000	-.0081107	0.377
eligible	-.0025135	0.703	-.0000406	0.996	-.0061898	0.567
P*eligible	-.0121677	0.053	-.0120224	0.125	-.0124152	0.230
t	-.0306959	0.000	-.0111506	0.276	-.067291	0.000
N	247247		165687		81560	
R²	0.1907		0.1574		0.2358	

	all households					
	all		co-residents		elders	
	coeff	p-value	coeff	p-value	coeff	p-value
P	-.0176073	0.000	-.0184362	0.000	-.008757	0.097
eligible	-.0044233	0.240	.0061161	0.207	-.0170752	0.004
P*eligible	-.0099489	0.006	-.0089813	0.052	-.009455	0.096
t	-.0127782	0.006	.0232306	0.000	-.0593951	0.000
N	682673		418072		264601	
R²	0.2395		0.2065		0.2184	
controls	age, age2, maxed, educa, gender, homem, escola, idade, idade2, racial dummies, state, year, nid, rural, inc_p, bf, gas petirural, peturbano.					

Note: codes of variables and their description are in the Appendix A.

Table 8 shows some placebo effects. In the period of 1993 to 1995 the program did not exist yet. Only in 1996 the program took place. The idea is to set 1995 eligible people as treated and then compare them to the eligible people in 1993 to know if there was in place any movement in the dependent variable before the existence of the program. The interaction variable (treatment*1[year=1995]) gives us the "effect", which we expect to be zero.

As we can observe the results showed no statistical effects on the interaction term. Other placebo experiment done was set as treated those eligible people who are not treated, and run the model excluding from the sample those who are really treated. This is shown in the Table 9 for non-treated income-eligible households.

The interaction variable, which would give the effect of our "treatment", showed to be zero for elders and co-residents as expected – meaning that no differences in the labor force participation are due to the eligibility condition.

Table 8: Placebo effect on the probability of working: setting as treated eligibles before the program was implemented (1993-1995).

| | income-eligible households | | | | | |
| | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value
treatment	.0089521	0.517	.0109712	0.634	.007795	0.650
1[year=1995]	-.0047848	0.437	-.0094847	0.364	-.0028579	0.706
treat*1[year=1995]	.0114986	0.330	.0066928	0.732	.0138702	0.346
N	29214		9426		19788	
R²	0.2056		0.2495		0.1722	

| | all households | | | | | |
| | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value
treatment	.0031872	0.689	-.0028476	0.822	.0056906	0.577
1[year=1995]	.0062474	0.084	.0020401	0.735	.0091133	0.043
treat*1[year=1995]	.0026996	0.688	-.0086002	0.421	.0107338	0.213
N	81150		30105		51045	
R²	0.2492		0.2249		0.2175	
controls	age, age2, maxed, educa, gender, homem, escola, idade, idade2 racial dummies, state, year, nid, rural, inc_p					

Table 9: Placebo effect on the probability of working: setting as treated the non-treated eligible households.

| | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value
P	-.0391629	0.000	-.0267832	0.011	-.0630349	0.000
treatment	-.0027385	0.673	-.0079453	0.469	-.0011831	0.883
P*treat.	-.0031612	0.581	-.01213	0.208	.0001167	0.987
N	244055		80526		163529	
R²	0.1886		0.2188		0.1562	
controls	age, age2, maxed, educa, gender, homem, escola, idade, idade2, racial dummies, state, year, nid, rural, inc_p, bf, gas petirural, peturbano.					

Note: non-treated income-eligible households included only. Treated are the age-eligible households.

To evaluate the program effect on labor force participation through a difference-in-difference estimator, however, we must have two groups of observations: the treatment group (before and after the implementation of the program) and the comparison group (before and after the implementation of the program). But, since the program does not have an experimental design, we do not have two groups (comparison and treatment) before and after the implementation of the treatment. In the BPC case the treated observations before the implementation of the program is missing. Until now the analyses presented used the eligible group as the treatment group.

An usual way to overcome this shortcoming is to build the treatment group (before the implementation of the program) performing a matching: finding among the comparison group those who are more similar to the treated observations in terms of observable characteristics, and assigning them to the treatment group before the treatment was implemented.

Another issue to be considered is which comparison group to use. A first approach is to use as comparison group the non-treated eligibles. Therefore we must perform a matching among the non-treated eligibles, finding those, before 1996, who are more similar to the non-treated eligibles from 1996 on. A second plausible comparison group to use is the non-eligibles and non-treated eligibles together.

For the sample of income and age-eligible households, we performed a diff-in-diff, using only matched observations, with the results displayed in Table 10.

We observe that labor force participation is 4.5 to 5.5 percentage points lower due to the program, when all members of the household are considered. The spillover effect, however, showed not to be significant for the comparison group 1 and only slightly significant for comparison group 2.

The program had some changes in the eligibility age since it took place in 1996. In 1996 the eligibility was 70 years old, in 1998 this age was reduced to 67 years old, and to 65 in 2004. We can explore these changes comparing affected groups to those not affected, also in a difference-in-difference approach.

Table 11 explores the changes in the eligibility in 2004. Model (I) considers as comparison group those with ages 63 or 64 (not affected by the policy), and as treatment group those with ages 65 or 66 years old (affected by the policy). Model (II) considers as comparison group those with ages less or equal to 64. Two periods were considered: after 2004 and before 2004 (2002 and 2003).

One possible explanation for not observing significant effects is that not everybody included into the treatment group effectively received the treatment. This happens because eligible people are meant to claim the benefit, and most do not. So, most of the observations on the treatment group do not receive the treatment, affecting the significance.

Table 10: Difference-in-difference estimates for the probability of working using propensity score matched samples.

| | comparison group 1: non-treated eligibles | | | | | |
| | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value
1[year>=1996]	-.0486597	0.000	-.0372799	0.006	-.032055	0.078
treatment	.0149121	0.365	.0022352	0.901	.0289939	0.207
treat.*1[year>=1996]	-.0455334	0.014	-.0649775	0.002	-.0378074	0.136
N	209291		69341		139950	
R²	0.2241		0.2707		0.1732	

p-score matching specifications: 3-nearest neighbors with replacement, common support in covariates.

| | comparison group 2: non-treated eligibles + non-eligibles | | | | | |
| | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value
1[year>=1996]	-.0504995	0.000	-.0284816	0.145	-.0346937	0.029
treatment	.0051965	0.716	.0117716	0.451	-.0003346	0.986
treat.*1[year>=1996]	-.0566801	0.000	-.0721254	0.000	-.043689	0.032
N	873842		331627		542215	
R²	0.2153		0.2844		0.1709	

p-score matching specifications: 3-nearest neighbors with replacement, maximum distance of 0.05 in estimated p-score of treated and control, common support in covariates.

Controls:	age, age2, maxed, educa, gender, homem, escola, idade, idade2, racial dummies, state, year, nid, rural, inc_p

Note: the sample is compound of income-eligible households only. In this experiment the sample was not the discontinuity one.

Table 11: Difference-in-difference estimates for a reduction in the eligibility age in 2004

| | (I) | | (II) | | | | | |
| | all | | all | | elders | | co-residents | |
	coeff	p-value	coeff	p-value	coeff	p-value	coeff	p-value
treatment	-.0101026	0.540	-.0044548	0.552	.0065323	0.607	-.0056371	0.545
after	.0053803	0.598	.0142732	0.000	.016566	0.000	.0136509	0.000
treat.*after	-.0029336	0.841	-.0143586	0.181	-.0117726	0.520	-.0150399	0.258
N	17480		180313		70772		109541	
R²	0.1756		0.1894		0.1982		0.1639	

Controls:	age, age2, maxed, educa, gender, homem, escola, idade, idade2, state, year, nid, racial dummies, rural, inc_p, bf, gas, petirural, petiurbano.

To better control for this problem, we tried then to perform a matching in the treatment group, using only the treated observations (after the change) and their matches (before the change). For a matter of consistency, the same was applied to the comparison group. The results are displayed in the next table.

Each column in Table 12 shows different comparison groups and different periods of time. All observations used are income-eligible, and the comparison group is compound always by non-treated eligibles.

Table 12: Difference-in-differences estimates for changes in the age for eligibility with matched samples

	1998					
	(I)		(II)		(III)	
	elders	co-residents	elders	co-residents	elders	co-residents
1[year≥1998]	.0086234	-.0079461	-.0151375	-.0270516	.0113022	-.0226017
	(0.676)	(0.641)	(0.483)	(0.084)	(0.263)	(0.011)
treatment	.0602079	.0288822	.0243755	-.0646841	.0373069	-.042978
	(0.399)	(0.766)	(0.699)	(0.208)	(0.543)	(0.395)
treat.*1[year≥1998]	-.2093624	-.0184232	-.1082408	.1781062	-.1036221	.193971
	(0.268)	(0.913)	(0.344)	(0.049)	(0.356)	(0.029)
N	3677	7484	7072	14267	21767	46463
R²	0.2513	0.1887	0.2304	0.1790	0.2048	0.1843
	2004					
	(IV)		(V)		(VI)	
	elders	co-residents	elders	co-residents	elders	co-residents
1[year≥2004]	-.0122542	-.0184406	-.0637251	.0288914	-.0120665	.0267597
	(0.608)	(0.282)	(0.005)	(0.088)	(0.141)	(0.000)
treatment	.0192133	-.0146346	.0211583	-.0066968	.0271614	.0250253
	(0.621)	(0.675)	(0.433)	(0.752)	(0.288)	(0.210)
treat.*1[year≥2004]	-.1429014	.0034121	-.137234	-.0133425	-.1506997	-.0419978
	(0.019)	(0.947)	(0.000)	(0.663)	(0.000)	(0.150)
N	3662	7415	6987	13806	33390	68552
R²	0.1911	0.1722	0.1939	0.1627	0.2133	0.1660

Controls: age, age2, maxed, educa, gender, homem, escola, idade, idade2, state, year, nid, racial dummies, rural, inc_p, bf, gas, petirural, petiurbano.

Note: p-values within parentheses.
(I)   ages: 67-69. Periods: 1996-1997 and 1998-1999.
(II)   ages: 67-69. Periods: 1996-1997 and 1998-2003
(III)   ages: 67-69 (treat.) and 70+ (control). Periods: 1996-1997 and 1998-2003
(IV)   ages: 65-66. Periods: 2002-2003 and 2004-2005
(V)   ages: 65-66. Periods: 2002-2003 and 2004-2008
(VI)   ages: 65-66 (treat.) and 67+ (control). Periods: 2002-2003 and 1998-2008

The only unexpected results refer to models II and III, where the spillover effect was significant and positive, while the effect for the elders was not significant. All other estimates until now were pointing in the opposite direction. Some pitfalls may have occurred during the matching procedure, selecting more working individuals as matches for the treated ones than usual for some reason still to be investigated. This hypothesis is feasible when we compare the estimates in 1998 to those in 2004. Due to the lack of treated observations, the procedure was not suitable for the change of eligibility-age in 1996.

The next exercise is to explore the discontinuity in receiving the benefits generated by the age-eligibility rule of the BPC program. For ages ranging from 60 to 75 years-old, we estimated the participation in the labor force of elders and co-residents, using two-stage least squares, instrumenting for the presence of a treated individual in the household. The estimates are displayed in Table 13.

Table 13: Two-stage least squares estimates around the discontinuity in age

	estimate	**N**	**R²**	Hansen overidentification test (χ^2)
co-residents	-.3525654 (0.006)	165687	0.1517	0.043 (0.8348)
elders	-.712161 (0.004)	81560	0.2082	23.264 (0.000)
Instrumented variable	t			
Excluded instruments	T, P			
Included instruments:	maxed age* educa gender homem escola idade idade2 racial dummies year nid state rural inc_p bf gas petirural petiurbano			

Note: p-values within parentheses.

There are reasons to believe that these estimates are more accurate than the others presented. One of them is that it takes into account the probability of being treated. As we know, eligible people are not always treated because most of them do not know they are eligible for the benefit. So there is a probability of being treated involved which must be considered.

Another reason is that it explores the age-eligibility rule which generates a discontinuity in the probability of receiving the benefit. It jumps suddenly from zero to some positive number when the eligibility-age is met.

Results indicate that, taking into account the probability of being treated, there is a sudden decline in the probability of working for beneficiaries or those with a beneficiary in the household. The probability of working for the elders who are beneficiaries is around 70 percentage points smaller than those who work. The spillover effect of the benefit in the household is about 35 percentage points. However, the overidentification test of endogenous instruments showed that for the elders the instruments are not truly exogenous. But for co-residents the result still remains valid.

5. Concluding remarks

The results presented in this paper all point towards a decrease in the probability of elders to work. The impacts vary in their size, but considering the estimates we consider more accurate, we found a staggering impact on the probability of working for elders. It illustrates that the BPC enables the possibility of retiring for elders who had not contributed during his work life and find themselves in a vulnerability situation at old-age. This is an important role in a country with such high levels of informal sector jobs, where practically no worker contributes to the social security. And it implies that this role tends to become even more important over time.

Also, we found spillover effects on the labor force participation for co-residents. We found a drop on this probability of working for co-residents. A word of caution must be added here. This result does not imply that people are quitting their jobs simply because they do not need them anymore for their income have increased with the benefit. As aforementioned, there are several reasons that could drive co-residents towards a situation on which they are not working, and not all of them are detrimental as, for example, returning to school.

The motives driving many co-residents not to work is still to be investigated. Any speculation on those motives right now would be misleading. One question raised which could be explored later is whether unemployed co-residents keep looking for jobs and whether they started to attend school. Also, we could check whether co-residents left a job recently. First of all, the availability of these information must be checked in the PNAD database during the period. Moreover, further robustness tests should be performed on specification and models, trying out different control groups and matching procedures, as well as estimations by local linear regressions.

References

Angrist, J. D. and Lavy, V. 1999. Using Maimonides' rule to estimate the effect of class size on scholastic achievement. *The Quarterly Journal of Economics*, 114(2): 533-575.

Barrientos, A. 2003. What is the impact of non-contributory pensions on poverty? Estimates from Brazil and South Africa. CPRC Working Paper nº33, Manchester: Chronic Poverty Research Centre.

Barrientos, A., and Lloyd-Sherlock, P. 2002. Non-contributory pensions and social protection. Dicussion Paper 12, Geneva: International Labour Office. ILO.

Barros, R.P., Carvalho, M., and Franco, S. 2007. O papel das transferências públicas na queda recente da desigualdade de renda brasileira. In: *Desigualdade de Renda no Brasil:* uma análise da queda recente. Brasília: Instituto de Pesquisa Econômica e Aplicada.

Bertrand, M., Mullainathan, S., and Miller, D. 2003. Public policy and extended families: evidence from pensions in South Africa. *The World Bank Economic Review,* 17(1): 27-50.

Bertranou, F., and Grushka, C. 2002. The non-contributory pension programme in Argentina: assessing the impact on poverty reduction. ESS Paper 5, Geneva: Social Security Policy and Development Branch. ILO.

Browning, M., and Chiappori, P.-A. 1998. Efficient intra-household allocations: a general characterization and empirical tests. *Econometrica,* 66(6): 1241-1278.

Carvalho Filho, I.E. 2008a. Old-age benefits and retirement decisions of rural elderly in Brazil. *Journal of Development Economics*, 86(1): 129-146.

Carvalho Filho, I.E. 2008b. Household income as a determinant of child labor and school enrollment in Brazil: evidence from a social security reform. IMF Working Paper 08/241, Washington: International Monetary Fund.

Case, A. and Deaton, A. 1998. Large cash transfers to the elderly in South Africa. *The Economic Journal*, 108(450): 1330-1361.

Dehejia, R.H., and Wahba, S. 2002. Propensity score matching methods for nonexperimental causal studies. *Review of Economics and Statistics,* 84: 151-161.

Duflo, E. 2003. Grandmothers and granddaughters: old age pensions and intra-household allocation in South Africa. *The World Bank Economic Review,* 17(1): 1-25.

Durán-Valverde, F. 2002. Anti-poverty programmes in Costa Rica: the non-contributory pension scheme. ESS Paper 8, Geneva: Social Security Policy and Development Branch. ILO.

Edmonds, E.V. 2006. Child labor and schooling responses to anticipated income in South Africa. *Journal of Development Economics*, 81(2): 386-414.

Edmonds, E.V., Mammen, K., and Miller, D.L. 2005. Rearranging the family? Income support and elderly living arrangements in a low-income country. *Journal of Human Resources*, 40(1): 186-207. (Winter).

Holzmann, R., Robalino, D., and Takayama, N. (ed.) 2009. *Closing the Coverage Gap:* the role of social pensions and other retirement income transfers. Washington, DC: The World Bank.

Krueger, D., Soares, R., and Berthelon, M. 2006. *Household choices of child labor and schooling*: a simple structural model with application to Brazil. Mimeo.

Lee, D., and T. Lemieux. 2009. Regression Discontinuity Designs in Economics. Working Paper 14723. Washington: National Bureau of Economics Research. NBER.

Martinez, S. W. 2005. *Pensions, poverty and household investments in Bolivia*. Mimeo.

Paulo, M. A. 2008. *A Relação entre Renda e Composição Domiciliar dos Idosos no Brasil: um Estudo sobre o Impacto do Recebimento do Benefício de Prestação Continuada.* Master's Dissertation, Department of Demography, Universidade Federal de Minas Gerais – UFMG/CEDEPLAR, BR.

Reis, M., and Camargo, J. 2005. Aposentadoria, pressão salarial e desemprego por nível de qualificação. Texto para Discussão nº 1115, Rio de Janeiro: Instituto de Pesquisa Econômica e Aplicada. IPEA.

Reis, M., and Camargo, J. 2007. Impactos de aposentadorias e pensões sobre a educação e a participação dos jovens na força de trabalho. *Pesquisa e Planejamento Econômico*, 37(2): 221-246.

Rosenbaum, P., and Rubin, D.B. 1983. The central role of propensity score in observational studies for causal effects. *Biometrika,* 70: 41-55.

Schleberger, E. 2002. Namibia's universal pension scheme: trends and challenges. ESS Paper 6, Geneva: Social Security Policy and Development Branch. ILO.

Schwarzer, H., and Querino, A. C. 2002. Non-contributory pensions in Brazil: the impact on poverty reduction. ESS Paper 11, Geneva: Social Security Policy and Development Branch. ILO.

Soares, F., Soares, S., Medeiros, M., and Osório, R. 2006. Programas de transferência de renda no Brasil: impactos sobre a desigualdade. Texto para Discussão nº 1228, Brasilia: Instituto de Pesquisa Econômica e Aplicada. IPEA.

Social Security Administration. 2010. *Social Security Throughout the World.* Washington, DC.

Thistlethwaite, D., and Campbell, D. 1960. Regression discontinuity analysis: an alternative to the ex post facto experiment. *Journal of Educational Psychology*, 51: 309–317.

Van der Klaaw, W. 2003. Estimating the effect of financial aid offers on college enrollment: a regression-discontinuity approach. *International Economic Review*, 43: 1249-1287.

Wald, A. 1940. The fitting of straight lines if both variables are subject to error. *The Annals of Mathematical Statistics*, 11(3): 284-300.

World Bank. 1994. *Averting the Old Age Crisis.* London: Oxford University Press.

Appendix A: Variable codes and their description.

Code	Description
age	age of the oldest person in the household
age2	'age' squared
maxed	highest years of schooling of a person within the household
educa	years of schooling of the oldest member of the household
gender	gender of the oldest member of the household (1 if male)
homem	gender of the person (1 if male)
escola	years of schooling
idade	age of the person
idade2	'idade' squared
nid*	number of persons in the household within the age strata *
rural	dummy for rural household
inc_p	per capita income percentiles. (Range: 0 to 100)
bf	someone in the household receives the *Bolsa Família* benefit (1 yes, 0 no)
gas	someone in the household receives the *Vale Gás* benefit (1 yes, 0 no)
petirural	someone in the household receives the *PETI* program for rural households (1 yes, 0 no)
petiurbano	someone in the household receives the PETI program for urban households (1 yes, 0 no)
year	year of the survey
P	$1[\text{year} \geq 1996]$
elig	eligible individual
eligible	eligible household
t	treated household
T	$1[\text{age} \geq c]$, where c is the cutoff value that year, according to the legislation that year
state	dummy for state of the federation
negro	
amarelo	racial dummies
pardo	