Is the effect of conditional transfers on labor supply negligible everywhere?*

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Abstract

This paper contributes to the literature on welfare programs by using a GPS-based approach to estimate the impact of a Conditional Cash Transfer (CCT) on labor supply in Brazil. Unlike other CCT programs that have been evaluated, *Bolsa Família*'s is a widespread program that have taken place not only in rural and isolated areas, but also in large cities. Previous findings have shown that this type of welfare benefit does not reduce labor supply and then does not create program dependence. However, our hypothesis is that when the program goes from isolated areas to large cities and everybody is informed about its rules, impacts may differ. We find that the benefit actually increases the participation of households' additional workers in rural areas. On the other hand, it reduces the participation of households' main source of labor income in the formal sector in metropolitan areas. Thus the hypothesis that the program creates dependence cannot be rejected for the case of large cities.

JEL Classification: I38, J22, C21, H31.

Keywords: Labor Supply, Conditional Cash Transfer Programs, Informal Sector, Generalized Propensity Score, Brazil.

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Abstract

This paper contributes to the literature on welfare programs by using a GPS-based approach to estimate the impact of a Conditional Cash Transfer (CCT) on labor supply in Brazil. Unlike other CCT programs that have been evaluated, *Bolsa Família*'s is a widespread program that have taken place not only in rural and isolated areas, but also in large cities. Previous findings have shown that this type of welfare benefit does not reduce labor supply and then does not create program dependence. However, our hypothesis is that when the program goes from isolated areas to large cities and everybody is informed about its rules, impacts may differ. We find that the benefit actually increases the participation of households' additional workers in rural areas. On the other hand, it reduces the participation of households' main source of labor income in the formal sector in metropolitan areas. Thus the hypothesis that the program creates dependence cannot be rejected for the case of large cities.

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1 Introduction

> *"CCTs do an excellent job of getting money to the poor. Children covered by them get more schooling and use health facilities more often than they would otherwise have done. Some fears have proved unfounded: poor people have not responded to cash payments by cutting back on paid work."* (The Economist, Feb 12 2009)

> *"The bottom-up nature of such social programs* (Bolsa Família) *has helped expand formal and informal employment as well as the Brazilian middle class."* (The New York Times, July 31 2008)

The impact of government transfers to families or individuals has long been a field of theoretical and empirical research for both the labor and development economics literatures (Moffitt, 2002). The focus of the labor economics literature is on the effect of welfare programs and negative taxes on both labor force participation and hours worked, along with their impact on business cycle.[1] In the development economics literature the disincentive to work is related to the possibility of generating program dependence for beneficiaries of social transfer.[2] The concern is that the eligibility criteria may generate incentives for adults not only to stay out of the labor force, falling into a poverty trap, but also to give preference to informal jobs. Although undocumented workers are in general more vulnerable, beneficiaries may prefer this type of job due to difficulties for programs to monitor informal earnings.[3] Furthermore, both literatures claim that the longer a worker stays out of the labor market the hardest would be to find a job and the worse would be the quality of the job they would get. The reasons are the depreciation of her human capital or the narrowing of its social network.[4]

A special type of welfare program, with long term development goals, is the Conditional Cash Transfer (CCT) programs that have been introduced in several Latin American countries in the past two decades. Their relatively low cost and their effectiveness in increasing school attendance and improving health care have been the motive of their spread around world, even in developed countries.[5] The lack of evidences that this type of transfer reduces adult labor supply has also been one of the main reasons for their success. On the other hand, it is quite questionable whether

[1] See, for instance, Burtless and Hausman (1978), Heckman (1993), Meyer and Rosenbaum (2001), Meyer (2002), Saez (2002), Moffitt (2003), Eissa and Liebman (1996).

[2] See Besley and Coate (1992) and Kanbur et al. (1994).

[3] See Gasparini et al. (2007).

[4] Such negative impact has led to design of the so-called in-work benefit reforms as a way to generate work incentives. See Blundell (2000) for a review of the effectiveness of these reforms.

[5] In the United States, Opportunity NYC is an example of program that have been inspired by the Latin American experiences.

evidences from the countryside of developing nations can be used to support the implementation of these programs everywhere.

CCT programs have the double objective of alleviating poverty in the short-run and of breaking the intergenerational transmission of poverty through investments in health and education of the children of beneficiary families. The programs have in common the existence of some targeting mechanism, the cash benefit, and the requirement that families comply with a certain set of co-responsibilities. These co-responsibilities, also called conditionalities, are usually linked to a minimum school attendance for children over 6 years old and regular visits to health centers for pregnant women and preschool children. However, the programs do differ according to the emphasis they put into the two core objectives. This difference has implications for the programs' design options, such as the targeting mechanism and eligibility criteria, their size (coverage), benefit value (either fixed or varying with respect to household composition), payment schedule, the chosen set of co-responsibilities and their monitoring, the inclusion of new beneficiaries, the existence of pre-determined period that a family can stay in the program, and graduation rules. Most important, these programs usually have no requirement regarding adult labor supply, besides (voluntary) unpaid community jobs.

Most of the evaluations of CCT programs focus on the impact on education, health care, and nutrition.[6] Impacts on labor supply are generally related to child labor, since it is highly correlated with school attendance. Less attention has been paid to the impact on adult labor supply. Our paper contributes to the literature on welfare programs and labor supply by using a quasi-experimental approach to estimate the impact of CCT coverage on labor supply, as well as on other labor outcomes at neighborhood level in Brazil. We particularly investigate the impacts on labor force participation, informality, unemployment, average wage, and hours of work.

CCT programs can affect adult labor supply by direct and indirect channels. First, the increase in the unearned component of household income can reduce labor supply at both extensive and intensive margins if leisure is a normal good (Meyer, 2002). Second, households may prefer to reduce (uncertain) labor earnings in order to not lose the (regular) benefit, in spite of leisure being either a normal good or not. Third, adults may prefer to work at undocumented jobs instead of formal jobs so that their earnings cannot be tracked by the Government and they remain eligible for the benefit. Fourth, poor households suffer from credit constraint and uncertainty more than other households, which reduce their investments, specially in rural areas. In such context, the benefit may have a positive impact on households' production (Rosenzweig and Wolpin, 1993).[7] Fifth, since transfer

[6]See Rawlings and Rubio (2005), Bouillon Buendia and Tejerina (2007), and Soares et al. (2010b) for reviews.

[7]Martinez (2004) shows that the non-contributory social pension in Bolivia, BONOSOL, increased consumption

are usually made to women, it can improve their intra-household bargain.[8] With more power in the household, women may prefer to either work in the labor market or even spend more time at home. Sixth, the compliance with co-responsibilities, on one hand, reduces the cost of child care due to the higher school attendance and free more household members, especially the mother, to the labor market. On the other hand, the need to comply with co-responsibilities demands time from mothers and hence reduce their time available to work. Seventh, co-responsibilities related to children's time allocation may reduce child labor.[9] Then adult labor supply should increase to compensate the loss of money if children's earnings were higher than the benefit itself (Bourguignon et al., 2003). Finally, the labor market can also be indirectly affected by the program because changes in beneficiaries' behavior can affect wages in local economies, the amount of transfers itself represents a shock to aggregate demand, and non-beneficiaries may change their behavior in order to become eligible. In summary, the CCT program can have either positive or negative impacts on adult labor supply. These impacts can vary not only by gender and household position but also across communities. In rural communities, the substitution effect of CCT is expected to be larger because of the higher participation of children in work activities and higher cost of schooling. Moreover, the role of the transfer as credit and collateral for household production is worthier in rural areas than in cities.

Most of previous studies on the effect of CCT programs do not evidence reduction in adult labor supply at the extensive margin (i.e., labor force participation).[10] At the intensive margin, Alzúa et al. (2010) find that RPS in Nicaragua has reduced by about 15 hours the hours worked of female adults.[11] They also show that PROGRESA in Mexico has had a negative effect on employment of non-eligible women, suggesting that the program does have an indirect impact on non-beneficiaries. PROGRESA, on the other hand, has increased wages of adult males, according to them, and also households' production, according to Gertler et al. (2006).[12] Unlike other programs, such as *Bolsa*

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in rural households by more than the value transfered because these household had invested part of the pension on production for own consumption.

[8]Cedeplar (2007) shows women have had a higher bargain power in households participating in the *Bolsa Família*, mainly in the Northeastern region, which the poorest region in Brazil. According to Attanasio and Lechene (2002) and Schady and Rosero (2008), respectively, both PROGRESA in Mexico and the *Bono de Desarrollo Humano* (BDH) in Ecuador have improved the bargaining position of women in beneficiary households, giving them greater capacity to influence decisions on expenditures.

[9]Skoufias and Parker (2001) document the reduction on child labor in the case of PROGRESA in Mexico, as well as Attanasio et al. (2006) for the case of *Familias en Acción* in Colombia

[10]See Alzúa et al. (2010) for a comparative evaluation of PRAF II in Honduras, PROGRESA in Mexico, and RPS in Nicaragua; Parker and Skoufias (2000), Skoufias and Maro (2008), Parker et al. (2008) for evaluations of PROGRESA; Maluccio (2007) for an evaluation of RPS; IFS et al. (2006) for an evaluation of *Familias en Acción* in Colombia; and Galasso (2006) for an evaluation of *Chile Solidario*

[11]Maluccio (2007) also find a reduction in hours worked (mostly in agriculture) for 2001 and 2002. Unlike Alzúa et al., this result is mostly driven by the male sample and seems to be a short-term impact, since there is no significant impact for 2004.

[12]Similarly, Soares et al. (2010a) show that households that were beneficiary of *Tekoporã* in Paraguay invested

Família in Brazil, in which benefits are reduced or even canceled with the increase of other incomes, PROGRESA benefits were initially provided for three years, irrespective of changes in household income. Accordingly, Parker et al. (2008) attribute the lack of negative impacts on labor supply to this feature.

IFS et al. (2006) shows that the labor force participation of both rural male and urban female adults have actually increased by 2.7 and 4.1 percentage points, respectively, as a result of *Familias en Acción* in Colombia.[13] Likewise, Galasso (2006) shows that *Chile Solidario* has had a positive and significant effect on labor force participation in rural areas. Therefore, most of CCT evaluations have shown that these programs do not encourage adult workers to move out of the labor force, but they eventually encourage adults to increase their participation, as in Colombia and Chile.

Although most of these findings come from pilot/experimental programs, concentrated in rural areas, they have been used to advocate for the expansion of CCT programs to other places.[14] Nonetheless, they might be subject to the Hawthorne effect, in which treated households, or communities, modify their behavior just because they are in an experiment.[15] The response of households to the program might also differ when it is self-selective, becomes better understood by households, or is extended to urban and less poor areas.

Unlike most of CCT programs, *Bolsa Família* in Brazil already started with a large coverage all over the country, because it represented the merging and subsequent scaling up of four small programs. Nowadays, it is one of the largest CCT programs in the world, covering about 12.5 million families (about 25% of population), and its benefit represents about 10% of beneficiary households' total income (Soares et al., 2008). In general, the studies on its impact on labor supply show no change on labor force participation. Most of the significant findings are related to reduction in hours worked, specially by mothers or women (Oliveira et al., 2007; Tavares, 2008; Ferro et al., 2010; Teixeira, 2010, see). It corroborates what has been found in other countries. Unlike the evaluation of other programs, however, these studies do not use pre-program information (baseline) to control for either observable or unobservable characteristics in the treatment assignment. In addition, they compare treated and untreated households without taking into account that the latter can be also affected by the program coverage in their community.

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between 45 and 50 per cent more in agriculture production.

[13]IFS et al. (2006) also find a decrease in labor force participation for boys and girls in the rural areas, but only for girls in the urban areas. This result may partially explain the increase in labor supply of female adults, due to a substitution effect within the household.

[14]Fiszbein and Schady (2009), for instance, mention the modest effect of CCT programs on labor market participation to support their expansion around the world.

[15]A discussion about Hawthorne effect can be found in Adair (1984), Diaper (1990), Jones (1992), and Barnes (2010).

Foguel and Barros (2010) use a panel of municipalities, from 2001 to 2005, that avoids these two sources of potential bias. They find that *Bolsa Família*'s coverage has no effect on male and female participation rates and working hours at the municipality level. They use Arellano-Bond estimator for dynamic panel to get their results.[16] Their model, however, has three critical features. First, it assumes that the marginal effect of all CCT programs in Brazil is the same since 2001, even though these programs have different targeting, designs, and goals. Second, they control for current variables that can also be affected by the program, such as labor income and unemployment rate. Finally, they estimate a dynamic panel model with only five periods, which may not be enough to get significant results.

This paper follows a strategy similar to Foguel and Barros' paper in the sense that it looks at the marginal effect of program's coverage at neighborhood level, rather than on the labor supply of the beneficiaries only. It intends to contribute to the literature in three ways. First, it puts forward an empirical strategy to assess the effect of the program using a modified difference-in-differences (DID) model. This model, which primarily controls for the unobservable outcome, takes into consideration that the treatment assignment is continuous (i.e., program's coverage) and also controls for heterogeneity in the treatment effect, as well as in the outcome variation, using the Generalized Propensity Score (GPS).[17] Second, it compares the effects in the short-run (2004), just one year after the program was launched, and in the medium term (2006), when the program had almost achieved its coverage target. Third, it investigates the heterogeneity of program's impact, assessing not only differential effects by gender, but also by household position, area (metropolitan, small urban, and rural), and poverty level. Since *Bolsa Família* is almost everywhere in the country, we take advantage of a considerable variability in terms of coverage and poverty level.

Even in a DID approach, the estimated impact of *Bolsa Família* can be misleading because of the pro-poor growth experienced in Brazil in the 2000's. Indeed, the program has been targeted at areas with not only the worst working conditions but also higher transition to the formal sector, higher reduction in hours worked, and higher growth in wages. Thus it is tricky to distinguish which changes are caused by the program itself and which ones are caused by other events related to the pro-poor growth. Fortunately, due to the targeting design, great part of variation in program's coverage at neighborhood level is explained by observed characteristics, such as poverty headcount in 2001. It allows us to control for heterogeneity in outcome variations using a estimated GPS and then to identify the program's effect using a DID model.

[16]See Arellano and Bond (1991).

[17]This model is inspired by those presented by Abadie (2005) and Imai and van Dyk (2004). However, it differs from the former by being applied to continuous treatment regimes and from the latter by using a difference-in-difference approach with bivariate treatment assignment.

Our results suggest that the program discourages labor supply at the intensive margin (i.e., it reduces hours worked) in the poorer areas, but encourages participation of additional household workers in the labor market. In large cities, on the other hand, there is a significant reduction in households' labor supply at the extensive margin. Furthermore, the program reduces the participation in the formal sector in these areas. Thus our evidence is that the potential effect of CCT programs in large cities differs from their effect in rural areas.

The remainder of the paper is organized as follows. The Section 2 describes the main features of *Bolsa Família* and its targeting performance at neighborhood level. Section 3 describes the data used in the analysis and brings the main descriptive statistics. Section 4 covers the econometric method, describing how a GPS-based method can solve the problem of endogenous treatment assignment within a difference-in-differences approach. Section 5 describes the estimated GPS model and its implication for the sample. Section 6 discusses the impacts on labor force participation, unemployment, formal sector and informal sector participations, weekly hours worked, and average hourly wages. Finally, Section 7 concludes.

2 The *Bolsa Família* Program

2.1 Program Description

The first experiences with CCT in Brazil started in the mid-1990's in the Federal District, and in Campinas and Ribeirão Preto, two large cities from São Paulo, the richest state in the country.[18] In 1997, the Congress approved a law authorizing the Federal Government to cover up to 50 per cent of the costs of any minimum income guarantee program linked to education, but only for municipalities whose tax revenues and per capita income were below than their state averages. This restriction was a first indication that the Federal Government would like to target the poor municipalities. However, the fact that the municipalities had to bear half the cost of the program led to a low take-up rate.[19]

In February 2001, under the responsibility of the Ministry of Education, it was created the *Bolsa Escola* (CCT for education only) covering children between aged 6 and 15 years, enrolled between first and eighth grades, and living in poor households. According to Britto (2008), estimates of the target population in each municipality were calculated using the National Household Survey,

[18]The first Federal CCT program was the Program for the Eradication of Child Labor (*Programa de Erradicação do Trabalho Infantil*, PETI), created in 1996 and implemented originally only in few municipalities (Soares and Sátyro, 2009).

[19]By 1999, there were only 150 municipalities (out of more than 5,000) registered in the program, whereas the target for that year was a coverage of 1,254 municipalities (Fonseca, 2001).

the Demographic Census, and the Annual School Census. Later that year, other two cash transfer programs were created: *Bolsa Alimentação* (CCT for health/nutrition purposes) in the Ministry of Health, for children up to 6 years and pregnant women (with the same targeting criterion and benefit value of *Bolsa Escola*), and *Auxílio Gás* (Cooking Gas Grant) to compensate poor households for the phasing-out of fuel subsidies. Whereas *Bolsa Escola* and *Bolsa Alimentação* were conditional transfers, *Auxílio Gás* was unconditional. The short-lived *Cartão Alimentação* (a Food Stamp type of program) was created in 2003, under the food security strategy of Lula's administration. This program provided a lump sum transfer to families living with less than half of the minimum wage.

The creation of *Bolsa Família* through the merge of these four programs led to the standardization of eligibility criteria, benefit values, information systems and executing agency. It also brought in a gradual increase in the coverage of CCT from 5.1 million families in December 2002 to 11.1 million families in October 2006. The latter was the program target as per the estimated number of poor families, based on the 2001 Household Survey.

In *Bolsa Família*'s targeting mechanism, municipalities are free to decide about the priority areas and how the registering process will take place. However, they do receive some guidelines, under the form of quotas for the number the benefits that can be provided. These quotas were initially based on the poverty map elaborated by the National Statistics Office (*Instituto Brasileiro de Geografia and Estatística*, IBGE), that constructed it using both the 2001 National Household Survey and the 2000 Demographic Census. The same poverty map was used for the quotas until 2006, when it started being annually updated. Besides playing the role of geographical targeting, the quotas have also been used as an incentive for local governments to benefit the poorest families and not provide the transfer for political purposes.

Although the local government has the responsibility for registering poor families in the single registry, this registration does not mean automatic selection into the program, because registered families still have to prove they receive per capita income under the eligibility cut-off point.[20] The selection and ranking of registered families to receive the benefit is made by SENARC (*Secretaria Nacional de Renda para a Cidadania*, the implementing department at the Ministry of Social Development and Fight against Hunger, MDS). The sole selection criterion is the per capita income, the ranking criteria are both the per capita household income (ascending) and the number of household members (descending). The final step to receive the benefit is under the responsibility of

[20]The criterion to be registered in the *Cadastro Único* (the single registry for targeted social policies) is to have per capita income below half of the minimum wage or total income up to three minimum wages. Both criteria are higher than the eligibility cut-off point of *Bolsa Família*. As of October 2009, there were 18 million families registered in the Single Registry, roughly 36 per cent of the Brazilian families.

the Federal Bank (*Caixa Econômica Federal*) which processes the payroll and produces the smart cards.

In 2006, extremely poor families, whose per capita income was under R$60 (US$38), and poor families, whose per capita income was under R$120 (US$76), with children up to 15 years old or pregnant women were eligible. The benefit was composed of two parts: a) R$60 (US$38) for extremely poor families regardless of the number of children, and b) R$18 (US$11) per children, up to three, for poor families. Thus an extremely poor family should receive a benefit between R$60 (US$38) and R$114 (US$72), whereas a moderately poor family should receive between R$18 (US$11) and R$54 (US$34).[21] These benefits require a household commitment in terms of education and health care. Until 2008, the program required minimum school attendance of 85 per cent for 6-15 year children, updating of immunization protocol and growth and development monitoring for children up to 7 years old, and both prenatal care and postnatal care for women between 14 and 44 years.[22]

Families can be dropped from the program not only in case of not complying with the conditionalities, but also when their per capita income becomes higher than the eligibility cut-off point. During the period covered by this study, whenever it was found that the per capita household income had became higher than the threshold for eligibility, a family would be excluded from the payroll.[23] Moreover, families are required to update their records in the single registry at least once every two years. As for monitoring of the income information, the Federal Government regularly matches beneficiaries' records with other governmental databases, such as the database on formal sector workers from the Ministry of Labor and Employment and the database of pensions and other social assistance programs.

2.2 Program's Targeting Performance

The number of poor families which defines *Bolsa Família*'s target has been estimated based on a poverty line equal to half of the 2001 minimum wage. According to Barros et al. (2008), 57% of the conditional transfers in Brazil went to these poor families in 2005. Taking this percentage as a measure of targeting performance, they claim that 32% of this success is due to the municipal quotas, 62% is due to accuracy of registration process at the local level, and only 6% is due to the

[21] In 2004, the extreme poverty line for the program was R$50 (US$33), the poverty line was R$100 (US$66), and the value of the benefit per child was R$15 (US$10).

[22] If the family is registered as extremely poor and with no child and pregnant woman, the transfer is actually unconditional.

[23] Since March 2008, there is a minimum period of two years in which the family can stay in the program regardless to what happens to their income.

information, mostly income, contained in the registry. Therefore, the income declared by families has had almost no importance for the program targeting performance.

If we consider that the program's quotas are based on a poverty map and that family registration is usually encouraged by social workers, who go to the poorest areas within municipalities to spread the information on *Bolsa Família*, we should expect that the secondary data provided by IBGE plays a critical role in the selection of beneficiaries. Figure 1 shows the relationship between poverty headcount in 2001 and proportion of people covered by the program in 2004 and 2006 at neighborhood level. Since this relationship is practically linear, with very narrow confidence intervals, we can make good predictions for the program's coverage based on secondary data.

Figure 1 About Here

In Table 1, we present some measures of targeting performance at neighborhood level. First, we estimate the proportion of neighborhoods that had been either undercovered or overcovered, or neither. By undercovered we mean the neighborhoods where the current poverty headcount is greater than proportion of people covered by the *Bolsa Família*, while overcovered means neighborhoods where the coverage rate is higher than the poverty rate. Then for both the undercovered and overcovered neighborhoods, we calculate the differences between poverty headcount and coverage rate and between coverage rate and poverty headcount, respectively.

Table 1 About Here

In 2004, one year after *Bolsa Família* had started, 58% of neighborhoods were uncovered. If we only consider rural communities, this rate goes to almost 76%. In these undercovered neighborhoods, the mean gap between poverty and coverage rates was about 23 percentage points. On the other hand, less than 15% of neighborhoods had more beneficiaries than poor people, with a mean gap between coverage and poverty rates of 13 percentage points. In 2006, we observe a considerable decline in the relative number of undercovered neighborhoods, about −25 percentage points, and increase in overcovered neighborhoods, about 19 percentage points. Despite the increase in program's coverage, the main reason was the poverty reduction in this period. Between 2004 and 2006, the program actually expanded in all types of areas, but this expansion happened mostly in rural communities, where the under-coverage rate was still the highest. Thus it is worth to keep in mind that rural areas have been the poorest and most covered by *Bolsa Família*, but the proportion of benefits in these areas is farther from their poverty rates than in other type of areas.

3 Data

All data come from the National Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD). This survey, which collects a broad set of information on demographic and socio-economic characteristics of households, included a special questionnaire on cash transfer programs in 2004 and 2006. This questionnaire asked whether any member of the household was beneficiary of each cash transfer program that was in place at the time of the survey. We make use of these two survey years as follow-ups, when the impact of the program is evaluated. The 2004 survey provides data to estimate the short-run impact of the program, one year after its implementation; whereas the 2006 survey provides data to estimate the impact of the program three years after its implementation, when it was not a novelty anymore.

In addition, we use 2001 PNAD as a baseline. In 2001, the *Bolsa Família* program had not taken place yet and the other cash transfer programs did not have a significant coverage. However, we have to control for this small coverage of other programs that contaminates our baseline. Accordingly, we identify those households receiving cash transfer from other social programs using the typical-value method developed by Foguel and Barros (2010). This method basically matches part of household income declared, under the entry of 'other incomes,' with those values transfered by each program. Despite the little contamination, this study distinguishes from most studies about *Bolsa Família*, reviewed in the introduction, because it actually takes advantage of a baseline. It allows us to control not only for selection in terms of unobserved outcomes but also for exogenous variables collected before the program was introduced. Furthermore, the *Bolsa Família*'s expansion at municipal level followed a poverty map based on the same survey that we are using as baseline (i.e., 2001 PNAD).

The PNAD is a cross-sectional survey, so it does not interview the same households every year. Thus we cannot construct a panel of households or even individuals. However, its sample replaces households within the same census tracts. A census tract is a neighborhood with 250 households on average. Once selected, the same census tract stays in the PNAD sampling for ten years, which is the period between two Demographic Censuses.[24] Therefore, for each decade, it is possible to build a panel of neighborhoods, or a pseudo-panel of households aggregated by neighborhood. Table 2 presents the average number of households interviewed by neighborhood, as well as the average number of adults between 18 and 60 years old. About 14 households and 27 adults are interviewed by neighborhood every year on average. The number of neighborhoods, or census tracts, in the

[24] A census tract has between 250 and 350 households in urban areas, 150 and 250 households in suburban areas, 51 and 350 households in informal settlement areas, 51 and 250 households in rural areas, and at least 20 households in indigenous areas (IBGE, 2003).

survey increases over time to contemplate the population increase.[25]

Table 2 About Here

Although the number of interviewed households may not be large enough to yield a precise estimate for some neighborhood populations, neighbor households are assumed to be very similar in terms of both observable and unobservable characteristics. It allows us to match homogeneous groups of households across years. It is worth to mention that these neighborhoods, or census tracts, are not selected to the sample with the same probability in the first stage of PNAD's sampling scheme. Hence, we have to take this difference in the probability of being in the sample into account in our estimations.[26]

For each neighborhood, we calculate the mean of our variables of interest over different populations. Since we are interested in the effect of the program on adult labor outcomes, these means are calculated over a sample of individuals between 18 and 60 years old, which we define as the working-age group. This is our aggregate sample. In addition, we construct four other samples. The first two samples are comprised of separate means of the variables of interest for men and women in the working-age group. The third sample is a set of averages for households where at least one member is in the working-age group. This sample is used to estimate the effect on the first person supplying labor in the household. The fourth sample is of means for households where there are at least two working-age members and at least one of them has a job. This last sample is used to estimate the effect on the additional worker in the household. In case of at least two household members have a job, the order is defined by the higher salary.

Table 3 presents the mean and the standard error of our variables of interest. The labor force participation is measured by the proportion of working-age individuals who supply labor, i.e. those who have been either working or looking for a job in the last seven days. In 2001, before the launch of *Bolsa Família*, this rate was about 76%. In 2004 and 2006, after the program was implemented, the labor force participation raised to 78%. Although the labor supply increased in the extensive margin, the unemployment rate, which is given by the proportion of individuals in the labor force who do not have a job, but actively look for one in the last seven days, decreased from 7.2% in 2001 to 6.8% in 2004 and 6.5% in 2006. Therefore, the proportion of working-age people with a job increased during this period.

The formal sector participation is the rate of working-age people who were registered employee,

[25] We exclude rural areas in the North region, except for the State of Pará, from both the 2004 and 2006 samples. The reason is because the 2001 survey did not cover these areas.

[26] For more details on PNAD sampling scheme, see Silva et al. (2002).

workers who contributed to social security, employer with more than five employees, or registered professionals, such as lawyers and artists. The informal sector participation is the rate of working-age people who are working as undocumented employee or self-employed in the last seven days and do not contribute to social security.[27] Table 3 shows that the increase in labor market participation was mostly explained by the performance of the formal sector, with participation raising from 30.5% in 2001 to 32% in 2004 and 34% in 2006. The participation in the informal sector, on the other hand, stayed around 40% in those years. Following the increase in the formal sector, the average wage raised 13% from 2001 to 2006, even though no wage increase is observed up to 2004. Hours worked, however, declined on average from 41.6 hours per week in 2001 to 39.8 hours per week in 2006.

TABLE 3 ABOUT HERE

Beyond the total average, the male labor force participation, as well as their average wage, average hours worked, and formal sector participation, is considerably higher than women's. However, while the former rate is constant over time, remaining around 90.5%, the latter goes from 62% in 2001 to almost 67% in 2006. For both samples, we also observe an increase of 3.6 percentage points in the formal sector participation, an increase of 13% in the average wage, and a decrase between 1 and 2 hours a week in the average time worked. Thus whereas more women have gotten into the labor market, mostly in the formal sector, the men have just moved from the informal to the formal sector. Similarly, we observe an increase of 4.3 percentage points in the labor force participation of the second person in the household between 2001 and 2006. This increase was also mostly in the formal sector. The labor force participation of the first person remained around 93%.

Table 3 also shows that the average coverage per neighborhood of CCT programs goes from 8% in 2001 to 20% in 2004 and 26% in 2006. The average per capita conditional transfer increases from R$1.09 in 2001 to R$2.63 in 2004 and R$4.00 in 2006.[28] Although there is no considerable difference between the first and second persons supplying labor in the household in terms of CCT targeting, the conditional transfers are clearly targeted toward women in all three periods. Henceforth, we consider as *Bolsa Família* all previous programs that had a similar goal and design (e.g., *Bolsa Alimentação*, *Cartão Alimentação*, *Bolsa Escola*, and PETI).[29]

[27]This definition of informal sector comprises all (and only) jobs whose earnings cannot be even partially tracked by the Government.

[28]Per capita transfer, as well as wages, is in currency values of October 2006, deflated according to the index proposed in Corseuil and Foguel (2002).

[29]Even though they are social transfers, we consider neither *Auxílio-Gás* (Cooking Gas Grant) nor noncontributory pensions as part of *Bolsa Família* because they are unconditional. However, if the households receives both the *Auxílio-Gás* and a conditional transfer, it is considered a beneficiary of CCT programs and the value of *Auxílio-Gás* is included as a conditional benefit.

4 Econometric Model

Suppose the labor supply of individual i living at community c at time t, y_{ict}, is given by the following equation:

$$y_{ict} = \alpha + \beta_1 d_{ict} + \beta_2 \overline{d}_{ct} + \mu_i + \mu_t + u_{ict}, \tag{1}$$

where d_{ict} is a dummy variable such that

$$d_{ict} = \begin{cases} 1 & \text{if individual } i \text{ is treated at time } t \\ 0 & \text{otherwise} \end{cases}$$

and $\overline{d}_{ct}$ is the coverage of treatment in community c, i.e., the conditional mean of d_{ict}. Coefficient β_1 is the direct effect of treatment, whereas coefficient β_2 can be interpreted as the indirect effect of treatment caused by externality. Then the marginal effect of treatment coverage on community c is $\beta_1 + \beta_2$.

Since data is available only at neighborhood level, we cannot estimate equation (1) properly. In this case, however, we are able to estimate the following equation (Deaton, 1985; Verbeek and Nijman, 1992):

$$\overline{y}_{ct} = \alpha + \tau \overline{d}_{ct} + \mu_c + \mu_t + u_{ct}, \tag{2}$$

where $\overline{y}_{ct}$ is the mean of y_{ict} for community c at time t.

Note that any least square estimator for equation (2) yields the following result:

$$\tau = \beta_1 + \beta_2.$$

That is, we cannot distinguish the direct effect of treatment from its indirect effect, but we can obtain the marginal effect of coverage by estimating equation (2).

Moreover, the community coverage, $\overline{d}_{ct}$, may have a considerable measurement error for several reasons. First, the neighborhood sample is not large enough to get precise estimates for program's coverage. Second, the neighborhood can be affected by what happens in other close neighborhoods. Namely, the program can affect one community without treating it, but just covering the neighbor communities. In both cases, the neighborhood coverage may not capture properly the indirect component of the average effect, β_2. To minimize the bias caused by this measurement error, we estimate a reduced-form IV model. Accordingly, we replace $\overline{d}_{ct}$ by a instrumental variable, which is the coverage at municipality level, $\overline{\overline{d}}_{ct}$.[30] It is worth to mention that now we have a multilevel model, so the standard errors are clustered by municipality.

[30] We do not need to estimate a two-step IV model because for all samples, the coefficient of municipality coverage on neighborhood coverage is significantly equal to one. First-step estimates are available under request.

In the case we have only two periods, say $t = 0, 1$, we cannot estimate equation (2) properly using a fixed-effect model. Furthermore, the OLS estimator for the average effect, τ, will be consistent only if the treatment assignment, $\overline{\overline{d}}_{ct}$, is not related to the unobserved outcome. This condition is violated, for instance, if the program's targeting depends either directly or indirectly on the previous level of labor supply. To avoid such a strong assumption, we can estimate the following Difference-in-Difference (DID) model:

$$\Delta\overline{y}_c = \mu + \tau\Delta\overline{\overline{d}}_c + \theta_1\overline{\overline{d}}_{c0} + \theta_2\left(\overline{\overline{d}}_{c0}\cdot\Delta\overline{\overline{d}}_c\right) + \Delta u_c, \tag{3}$$

where $\Delta\overline{y}_c = \overline{y}_{c1} - \overline{y}_{c0}$, $\Delta\overline{\overline{d}}_c = \overline{\overline{d}}_{c1} - \overline{\overline{d}}_{c0}$, $\Delta u_c = u_{c1} - u_{c0}$, and $\mu = \mu_1 - \mu_0$.[31]

Although this DID model controls for selection in terms of unobserved outcomes, it does not control for selection in terms of unobserved variation in these outcomes. That is, it assumes that the treatment assignment is independent from the outcome variation obtained from its own treatment status:

$$\Delta\overline{y}_c\left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right) \perp \left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right).$$

This condition is violated if the program's targeting is based on either previous outcome dynamics or its potential effect on the treated community. We can weaken this condition assuming the following conditional independence assumption:

$$\Delta\overline{y}_c\left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\middle| X_{c0}\right) \perp \left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right),$$

where X_{c0} is a vector of exogenous characteristics. This assumption means that we can predict the effect on outcome variation based on observed characteristics as well as the program does.

Including X_{c0} linearly in equation (3) only controls for the heterogeneity in the outcome variation, $\Delta\overline{y}_c$, but it does not control for heterogeneity in the potential effect of treatment (Abadie, 2005; Freedman, 2008). Moreover, with a high dimension vector X_{c0}, interactions between X_{c0} and $\left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right)$ can be costly with respect to the number of coefficients in the model. A feasible strategy is to reduce the dimensions of X_{c0} by estimating a Generalized Propensity Score (GPS) function (Imbens, 2000; Imai and van Dyk, 2004). The GPS function, $r(d_1, d_0, X_{c0})$, maps each possible treatment combination, (d_1, d_0), in the set D into the conditional probability of receiving this treatment:

$$r(d_1, d_0, X_{c0}) \equiv \Pr\left[D = (d_1, d_0)\middle| X_{c0}\right].$$

[31] Under no baseline contamination, $\overline{\overline{d}}_{c0} = 0$, equation (3) becomes a standard DID model:

$$\Delta\overline{y}_c = \mu + \tau\overline{\overline{d}}_{c1} + \Delta u_c.$$

Since the treatment assignment represents the proportion of individuals receiving the benefit in their municipality at both periods 0 and 1, the GPS function is estimated using a bivariate probit model for grouped data. This estimate is obtained by maximizing the following log-likelihood function:

$$\begin{aligned} l(\gamma_0,\gamma_1,\rho) &= \sum_c \Big\{ \overline{\overline{d}}_{c0}\overline{\overline{d}}_{c1} \ln\left[\Phi_2\left(X'_{c0}\gamma_0, X'_{c0}\gamma_1,\rho\right)\right] + \left(1-\overline{\overline{d}}_{c0}\right)\left(1-\overline{\overline{d}}_{c1}\right)\ln\left[\Phi_2\left(-X'_{c0}\gamma_0,-X'_{c0}\gamma_1,\rho\right)\right] \\ &\quad + \left(1-\overline{\overline{d}}_{c0}\right)\overline{\overline{d}}_{c1}\ln\left[\Phi_2\left(-X'_{c0}\gamma_0, X'_{c0}\gamma_1,-\rho\right)\right] + \overline{\overline{d}}_{c0}\left(1-\overline{\overline{d}}_{c1}\right)\ln\left[\Phi_2\left(X'_{c0}\gamma_0,-X'_{c0}\gamma_1,-\rho\right)\right]\Big\}. \end{aligned}$$

where $\Phi_2(.)$ is a bivariate normal c.d.f. and ρ is the coefficient of correlation between the errors, e_{c0} and e_{c1}, of the following latent-variable equations:

$$\begin{aligned} \overline{\overline{d}}^*_{c0} &= X'_{c0}\gamma_0 + e_{c0}, \\ \overline{\overline{d}}^*_{c1} &= X'_{c0}\gamma_1 + e_{c1}. \end{aligned}$$

Once the parameters γ_0, γ_1, and ρ are estimated, we can obtain for each community the GPS indices,

$$\kappa_{c0} \equiv X'_{c0}\widehat{\gamma}_0,$$

$$\kappa_{c1} \equiv X'_{c0}\widehat{\gamma}_1,$$

and

$$\kappa_{cI} \equiv \kappa_{c0}\cdot\kappa_{c1}.$$

and the estimated GPS:

$$\begin{aligned} R(X_{c0}) &\equiv \widehat{r}\left(\overline{d}_{c1},\overline{d}_{c0},X_{c0}\right) \\ &= \overline{\overline{d}}_{c0}\overline{\overline{d}}_{c1}\Phi_2\left(X'_{c0}\widehat{\gamma}_0, X'_{c0}\widehat{\gamma}_1,\widehat{\rho}\right) + \left(1-\overline{\overline{d}}_{c0}\right)\left(1-\overline{\overline{d}}_{c1}\right)\Phi_2\left(-X'_{c0}\widehat{\gamma}_0,-X'_{c0}\widehat{\gamma}_1,\widehat{\rho}\right) \\ &\quad + \left(1-\overline{\overline{d}}_{c0}\right)\overline{\overline{d}}_{c1}\Phi_2\left(-X'_{c0}\widehat{\gamma}_0, X'_{c0}\widehat{\gamma}_1,-\widehat{\rho}\right) + \overline{\overline{d}}_{c0}\left(1-\overline{\overline{d}}_{c1}\right)\Phi_2\left(X'_{c0}\widehat{\gamma}_0,-X'_{c0}\widehat{\gamma}_1,-\widehat{\rho}\right), \end{aligned}$$

which represents the estimated probability of the community receiving its actual treatment bundle.

Besides reducing the dimension of X_{c0}, estimating the GPS function also facilitates the definition of overlap region or common support (Joffe and Rosenbaum, 1999). As shown by Flores and Mitnik (2009), the definition of overlap region plays a critical role in general treatment regimes estimation because it excludes from the sample those units with no identifiable counterfactual. The overlap region is defined as follows:[32]

$$C = \left\{c : \kappa_{c1} \in \left[\min_s(\kappa_{s1}), \max_s(\kappa_{s1})\right], \text{ with } \left|\overline{\overline{d}}_{c1} - \overline{\overline{d}}_{s1}\right| \geq \varepsilon\right\},$$

[32] This definition is different from those presented by Frölich et al. (2004) and Flores and Mitnik (2009) in two ways. First, their rules are applied to the case of multiple treatment but not necessarily to the case of continuous treatment. For this reason, we include a width, ε, in the formula. Second, their support region is based on the intersection of overlaps with respect to all potential treatments. However, this is not required under the weaker version of the conditional independence assumption.

where ε is the width which delimits how similar the communities are in terms of treatment. We let the width, ε, be equal to 10 percentage points in the program's coverage.

Within this overlap region, a simple way to control for covariates is to weight all communities by the inverse of their GPS, $R(X_{c0})^{-1/2}$ (Imbens, 2000; Robins et al., 2000).[33] This method is based on the Horvitz-Thompson sampling theorem (Horvitz and Thompson, 1952) and is usually called Inverse Probability Weighting (IPW).[34]

Another way of controlling for covariates, proposed by Hirano and Imbens (2004), is to include the GPS, $R(X_{c0})$, in equation (3), interacting with $\Delta\overline{\overline{d}}_c$. However, this regression cannot be interpreted as an estimate for the treatment effect because $R(X_{c0})$ also depends on $\left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right)$. The estimator for the treatment effect requires a second step in which the GPS, $R(X_{c0})$, is replaced by the GPS function evaluated in a treatment level of interest, $r(d_1, d_0, X_{c0})$.[35]

Following Imai and van Dyk (2004), as the GPS indices, κ_{c0}, κ_{c1} and κ_{cI}, do not depend on $\left(\overline{\overline{d}}_{c1}, \overline{\overline{d}}_{c0}\right)$, we employ a modified version of Hirano and Imbens' parametric regression:

$$\begin{aligned}\Delta\overline{y}_c &= \mu + \tau\Delta\overline{\overline{d}}_c + \theta_1\overline{\overline{d}}_{c0} + \theta_2\left(\overline{\overline{d}}_{c0}\cdot\Delta\overline{\overline{d}}_c\right) \\ &\quad +\theta_3\kappa^*_{c0} + \theta_4\left(\kappa^*_{c0}\cdot\Delta\overline{\overline{d}}_c\right) + \theta_5\kappa^*_{c1} + \theta_6\left(\kappa^*_{c1}\cdot\Delta\overline{\overline{d}}_c\right) + \theta_7\kappa^*_{cI} + \theta_8\left(\kappa^*_{cI}\cdot\Delta\overline{\overline{d}}_c\right) + \Delta u_c \quad (4)\end{aligned}$$

where κ^*_{cj} is the difference between the GPS index, κ_{cj}, and its mean value, $\overline{\kappa}_{cj}$, for $j = 0, 1, I$. Thus for the average community with no baseline contamination, the interaction terms are all zero, and hence the average treatment effect is equal to τ.

According to Robins and Rotnitzky (1995), combining IPW and regression of equation (4) has a "double robustness" property. If the regression model is correctly specified, then weighting by $R(X_{c0})^{-1/2}$ does not affect its consistency. Likewise, adjusting for the covariates as in equation (4) does not affect the estimate if the covariates have already been balanced by weighting by $R(X_{c0})^{-1/2}$.

We also estimate another version of equation (4) in order to verify heterogeneity in the treatment effect. In this equation, we interact the treatment assignment variation, $\Delta\overline{\overline{d}}_c$, with the neighborhood's poverty headcount at $t = 0$, $\overline{p}_{c0}$. The poverty headcount, however, is represented by restricted cubic spline terms, $h_n(\overline{p}_{c0})$, with the following knots: $k_n = 0.1, 0.3, 0.5, 0.7$.

[33]The square root version of the inverse GPS weight is presented by Flores and Mitnik (2009).
[34]See Wooldridge (2007) for more details on IPW estimators.
[35]See Hirano and Imbens (2004) for details.

5 GPS Estimation and the Balance Property

The covariates included in the GPS model come from a large set of demographic and socioeconomic indicators. All indicators are constructed using 2001 PNAD data and are listed in Table 4. Following Hirano and Imbens (2001), we select the covariates from this larger set based on their correlation with the treatment assignment in 2001, 2004 and 2006. Namely, the variable is included in the model to explain the treatment assignment in each year only if its simple correlation with the explained variable is significant at 5%.

Table 4 About Here

As we assess the treatment effect on five groups (i.e., all working-age people, men, women, first person in the household, and second person in the household) for two periods (i.e., 2001-2004 and 2001-2006), we had to estimate 10 GPS models.[36] From all these models, we define a common support which excludes less than 0.5% of the neighborhood sample. Figure 2 shows, for instance, that part of sample for all working-age group, in the left tail, should not be included in the estimation. For these neighborhoods, there is no other with similar characteristics and considerable difference in the program coverage.

Figure 2 About Here

For the estimated GPS to control for all variables listed in Table 4, it must satisfy the balance property. That is, conditioning on the estimated GPS, each covariate must be independent from the treatment assignment. Imai and van Dyk (2004) propose a way of testing the balance property for the GPS. They suggest to regress each covariate on the GPS index and treatment variable, and then verify whether the coefficient of the latter is significant or not.[37] Figure 3 presents the p-value for the estimated coefficients before and after controlling for the GPS indices. Without controlling for the GPS indices, 75 out of 79 variables are significantly related to the treatment assignment at 10% of significance. After including the GPS indices in the regression, the p-values increase considerably and less than 24 variables remain significantly related to the treatment assignment for both periods.

Figure 3 About Here

[36] The estimated coefficients are available under request.

[37] More specifically, we estimate the following equation for each $x_{c0} \in X_{c0}$:

$$x_{c0} = b_0 + b_1 \bar{\bar{d}}_{c0} + b_2 \bar{\bar{d}}_{c1} + b_3 \kappa_{c0} + b_4 \kappa_{c1} + \xi_c.$$

Then we test whether $b_1 = b_2 = 0$.

6 Results

6.1 The Average Effect of *Bolsa Família*

Table 5 presents the estimated effects of municipality coverage of CCT programs on the working-age population. On average, no significant effect on labor force participation and unemployment is found. However, we do observe a significant transition from the formal sector to the informal sector in both periods. This transition is more evident if we control for the covariates including the GPS indices in the regression. According to our estimates, an increase of one percentage point in coverage led to a switch to the informal sector of about 0.13 per cent in both periods (2004 and 2006). Combining these estimates with the coverage estimates in Table 1, we verify that the program was responsible for an increase in informal sector participation of 2 percentage points in 2004 and 2.5 percentage points in 2006.

It is worth to clarify that the transition between sectors is not necessarily caused by workers who quit their registered jobs to look for an undocumented opportunity. A more reasonable explanation is once a worker is fired in the formal sector, she may become eligible for the benefit. Then in order to remain "officially" eligible, she start seeking informal jobs rather than formal.

Both the simple DID and IPW estimations suggest that the program reduced labor supply at the intensive margin after three years (2006). However, this result is not robust for the inclusion of the GPS indices in the regression. On the other hand, the two former models do not yield any significant effect on wages, except for the positive effect given by the DID model in 2006. After parametrically controlling for the GPS indices, however, we find that the effect on wages was negative, mainly in 2004. One year after *Bolsa Família*'s creation, a percentage point increase in coverage reduced the average wage in about 0.33%. It represented an average impact of 5%, considering the program's coverage in 2004. In 2006, three years after the program had started, the marginal effect of coverage on average wage dropped to −0.15%, which is not significantly different from zero. Therefore, the impact on wages is a case in which the program's marginal effect changed over time.

TABLE 5 ABOUT HERE

In general, most of impacts are quite different if we do not control for covariates. This indicates that the treatment assignment is not only related to the unobserved outcomes but also related to the potential variation of these outcomes. For instance, the treatment assignment seems to be related to a lower transition to the informal sector, a higher reduction on hours worked, and a lower reduction on wages. All these potential variations are actually expectedly higher for the poorer population, to which the program is targeted. In the targeted population, the informality is higher, wages are lower, and household chores are more time demanding.

Nonetheless, the weighting scheme (IPW) does not seem to be effective in reducing the bias when we compare the two first columns of estimates in Table 5. The parametric adjustment using the GPS indices, on the other hand, provides distinct and more consistent estimates. Since there is no significant difference between weighting and not weighting after the parametric adjustment (see last two columns in Table 5), only the results obtained from the latter scheme, which is more efficient, are presented henceforth.[38]

6.2 Heterogeneity

In this subsection, we present some estimates for the heterogeneity of the marginal effect of coverage. First we present the estimates for the marginal effect of coverage on population groups (i.e., male, female, and the first and second persons in the household) in different areas (i.e., metropolitan, other urban, and rural). Then we interact coverage with poverty headcount in 2001 to test whether its marginal effect changes according to the poverty level.

6.2.1 Estimated Effects by Area

If we split the sample into metropolitan, other urban, and rural areas, we can see that the effect of *Bolsa Família* are quite distinct. Table 6 presents the estimated effect for the working-age population in these areas. In rural areas, the marginal effect of coverage on labor force participation was about 0.13. It represented an impact of 5.8 percentage points caused by the program in 2006. However, this impact was only followed by a raise in the informal sector participation. Considering an estimated marginal effect of 0.2, the actual coverage was responsible for a 8.4 percentage points increase in 2006.

In metropolitan areas, on the other hand, the program increased unemployment after one year (2004) and then reduced labor force participation after three years (2006). With a marginal effect of 0.29, the program explains an reduction in unemployment of 3.3 percentage points in 2004. In 2006, the marginal effect of 0.23 means that labor force participation declined almost 3 percentage points.

Both effects could be explained by the increase in the unearned income, which makes the unemployed either more patience when looking for a job or discouraged to seek work opportunities.

[38]The weighting scheme makes no difference in the results for the other samples as well. The estimates are available under request.

Nonetheless, the employment reduction happened only in the formal sector. With a marginal effect between 0.33 and 0.38, formal sector participation decreased by 4 percentage points in both periods. The reduction in the formal sector participation is also observed in smaller urban areas, but with a lower magnitude. This result suggests that it is not the pure income effect that makes urban workers reduce labor supply. This negative effect may come from the higher chances of loosing (or not gaining) the benefit if they stay in the formal sector, where salaries can be tracked by the Government.

Table 6 About Here

As shown in Table 7, the effect on men in rural areas only happened in the short run (2004), when the marginal effect on labor force participation was 0.1 percentage points. Again, this increase in labor supply was followed by a raise in the informal sector participation. Both effects are only significant at 10% though.

In metropolitan areas, there was no effect on men's labor force participation, but their unemployment increased significantly, mainly in 2004. That is, men do not leave the labor force but their probability of having a job decreases, mainly in the first year of program. In 2004, the marginal effect of 0.43, combined with the estimated coverage, implies an estimated impact of 5 percentage points on unemployment rate. In 2006, with a marginal effect of 0.15 percentage points, the impact was of only 1.7 percentage points. For both periods, the employment reduction is not observed in the informal sector, but only in the formal sector. With a estimated marginal effect of -0.69 in 2004, the formal sector participation was estimated to be 8 percentage points lower because of the program's coverage. In 2006, the marginal effect was lower (about -0.43) and then the impact dropped to 5 percentage points.

In other urban areas, the shift between sector is clearer. However, it is only significant in 2004, when almost 0.16 percentage of men went from the formal to the informal sector for each percentage point of coverage increase. Furthermore, this shift per percentage of coverage was followed by an increase of 0.04 hours a week in hours worked and a decrease of 0.46% in hourly wage in 2004. In 2006, we still observe a marginal effect of -0.25% on hourly wage, even though the impact on other outcomes is not significant.

Table 7 About Here

Table 8 presents the marginal effect on female labor supply in different areas. In 2004, the results do not present great significance. In 2006, however, we observe an increase of women's labor force participation in rural areas, particularly in the informal sector. The informal sector participation increased about 0.34 percentage points for each percentage of coverage, which means an whole impact of 14 percentage points in rural communities in 2006.

Unlike in rural areas, there was a significant reduction of women's labor force participation in metropolitan areas, coming mostly from the formal sector. With a marginal effect of -0.36, the estimated reduction on female labor supply was around 4.4 percentage points in 2006. The decline in women's formal sector participation was about 3 percentage points, with an estimated marginal effect of -0.25. No significant effect on either time worked or wages are found.

Table 8 About Here

As regards the first worker in household, in Table 9, no significant effect is identified in rural areas. It suggests that the effect on first worker's labor supply, as well as men's, is negligible in rural communities. As already discussed in the introduction, this result is consistent with other findings in the literature.

In metropolitan areas, however, we do observe a considerable shift between sectors in 2004. The marginal effect of coverage on the informal sector participation was -0.43 percentage points, which implies an impact of 4.8 percentage points caused by the program. The estimated impact on formal sector participation, which comes from a marginal effect of -0.7, was about -7.9 percentage points. This shift between sectors came along with a reduction of 0.7% per pecentage of coverage on hourly wages. It represented a negative impact of 7.9% on average wage.

In 2006, there was still a negative effect on formal sector participation (about -0.5 percentage points per percentage of coverage), but not strictly followed by an increase in the informal sector. The estimated reduction of 5.3 percentage points in the formal sector was in part followed by an impact of -2.3 percentage points (with a marginal effect of -0.19) in the labor force participation. Therefore, while in the short run the first household worker just switches from the formal to the informal sector, in the long run she definitely reduces labor supply. It implies that for each percentage increase in program's coverage, at least 0.19% of households with a working-age adult stop receiving earned income.

The reduction in formal sector participation was also explained by the increasing unemployment in both periods. With a marginal effect between 0.22 and 0.28 percentage points, the program's coverage caused an increase of at least 2.7 percentage points on the unemployment rate of first household workers. Those workers actually represent families who keep looking for job opportunities but do not receive earned income. Again, the impacts in smaller urban areas are in the same direction as in metropolitan areas, but with lower magnitude and significance.

Table 9 About Here

In contrast to the impacts on the first household worker, the program has had no significant effect on the additional worker in metropolitan areas (Table 10). In smaller urban areas, however, we observe a reduction in the formal sector participation of households' additional workers of about 0.15 percentage points for each percentage of coverage. This negative effect is followed in part by a small increase in informal sector participation and in part by a small reduction in labor supply.

Rural areas is where we observe considerable impacts on the labor supply of additional workers. However, these effects only appeared in 2006. Three years after the program started, the marginal effect of coverage on labor force participation was 0.3 percentage points. On average, the program had an impact of 12 percentage points on second worker's participation in rural communities. This result suggests that the benefit has a substitution effect that makes more household members work in rural areas. Therefore, the whole impact of cash transfer could be positive if it was restricted to rural communities.

As expected, the labor force participation was increasing only in the informal sector. In 2006, for each percentage of coverage, informal sector participation of additional workers increased 0.4 percentage points in rural areas. The average impact was about 17 percentage points. Indeed, formal jobs are less common in rural areas, especially for poor workers. Thus the only way that labor supply can increase is through the informal sector.

Table 10 About Here

In rural areas, the difference in the impacts in terms of gender is close to the difference in terms of position in the household. While significant effects are found neither on men nor on the first household worker, the program has a positive effect on labor force and informal sector participation of both women and additional household workers. However, in large cities, where the proportion of households headed by a single woman is higher, the relationship between gender and household position is not so strong. As for men, we observe a significant increase in unemployment of first household workers, as well as some transition from the formal sector to informal sector. We also observe yet a reduction in labor force participation of the first worker, which is also observed for women. Furthermore, no effect is identified on the second household worker in metropolitan areas, even though women have decreased their participation in the formal sector.

There are not many significant results for smaller urban areas. This sample actually represents something in the middle between rural communities and large cities. Accordingly, most of estimated effects in smaller urban areas are between those effects found in metropolitan and rural areas.

6.2.2 Estimated Effects by Poverty Rate

All estimated effects discussed so far are for the average population group. That is, they do not take the program's targeting into account. In this subsection, we present the estimated marginal effects under different levels of poverty. The neighborhood poverty level is assessed in 2001, before the program started.

Figure 4 shows that the marginal effect on labor force participation is higher in poorer neighborhoods. This result corroborates the hypothesis that the substitution effect of the transfer, as well as its effect on investment in household production, is higher for poor households. In 2006, the effect on participation in poor neighborhoods is also followed by a positive effect on unemployment. It may come from the fact that job opportunities for poor workers did not grow as fast as the increasing labor supply. In this sense, the benefit also works as an unemployment insurance that make workers wait for better jobs. Higher poverty is also related to a more negative effect on hours worked. Therefore, in the poorer neighborhoods, labor supply increases at the extensive margin, but also reduces at the intensive margin.

The lack of opportunities for poor workers in the labor market might also explain why the participation only increases in the informal sector. However, we also observe a shift between sectors, which is not particular to the poorer neighborhoods. In 2004, the negative effect on formal sector participation was higher in the less poor communities. This effect was followed by a reduction in the average wage, which did not appear in poorer areas. In 2006, the reduction in formal sector participation is not so strongly related to poverty, as well as the effect on wage, which disappears at all. Thus the informality grows in poor communities not only because of the increasing labor supply but also because of the shift between sectors, as in any other area.

FIGURE 4 ABOUT HERE

Figure 5 confirms that the effect on labor force participation of men is insignificant regardless the type of neighborhood. In less poor areas, however, we do observe a significant shift between sectors. This shift is again followed by a negative effect on wages. That is, the transition of workers from the formal to the informal sector in less poor areas might push the average wage down. The response of workers to this wage decline is an increase on hours of work, which we also observe. In 2006, all these effects became lower, but still with some significance.

In the poorer areas, no significant effect is identified in 2004. The program's coverage, however, has a positive effect on unemployment rate of these neighborhoods in 2006. Hence, at least for male, the benefit seems to work as an unemployment insurance for poor workers that make them more patience. The program's coverage also has a negative effect on hours worked of men in poorer neighborhoods. It can be an evidence of the income effect of the benefit, which is not compensated by the reduction in wages.

<u>FIGURE 5 ABOUT HERE</u>

For women, in Figure 6, the effect on informal sector participation is positive and increasing with respect to poverty. This effect is, in part, related to a negative response in the formal sector participation, which does not change with poverty (at least in 2006). Moreover, it is also related to a positive effect on labor force participation, which is particularly evident in the poorer neighborhoods. Other effects that are related to poverty are on female hours of work and wages. While wages of female workers increase in poor neighborhoods when coverage increases, their hours of work decline.

<u>FIGURE 6 ABOUT HERE</u>

Similar to what happens to men, the effect on the transition of the first household person from the formal to informal sector is significant only in the less poor areas, mainly in 2004 (see Figure 7). This effect was again followed by a decrease in wages, particularly in 2004. Therefore, the shift between sectors seems to be the reason for the reduction on average wages when it happens.

The rest of the estimated effects on the first household worker does not seem to be related to poverty. Then the effect on labor supply of the first household worker is stronger in large cities, as shown in Table 9, but not necessarily in the less poor neighborhoods. It suggests that the distinct effects found in large cities and rural communities are related not only to differences in income poverty but also to differences in households' needs.

<u>FIGURE 7 ABOUT HERE</u>

Although the program has had almost no effect on the first worker in household where poverty is higher, the impacts on households' additional workers are more significant in these areas. Regardless their gender, it is actually on these workers that the substitution effects of the transfer are expected to be higher. Accordingly, the marginal effect on their labor force participation in the poorer neighborhoods is greater than on women's. This positive effect is again followed by a negative response on hours worked and a positive response on wages in those areas.

Again, the increase in labor supply happens entirely in the informal sector. The formal sector participation, on the other hand, tends to decrease if program's coverage expand in less poor neighborhoods. This result confirms, one more time, the importance of targeting the program at the poorest neighborhoods.

Figure 8 About Here

In summary, all these results suggest that the positive impacts on labor supply at the extensive margin could be boosted if the program was concentrated in the poorest neighborhoods.[39] A better targeting would raise the number of additional household workers in the labor force, despite their gender. This improvement would also reduce labor supply at the intensive margin (hours worked), but this negative impact could be compensated by the increasing effect on wages in poor neighborhoods. Besides lessening the positive effects on labor supply and wages of additional workers, a worse targeting might also raise the transition of workers from the formal to the informal sector. For the case of men, this transition comes along with a lower average wage, which makes them increase hours worked in less poor areas.

7 Conclusion

Bolsa Família is a widespread Conditional Cash Transfer (CCT) program that is targeted at poor families. The first step in its targeting strategy is to identify areas where those poor families are concentrated, based on a poverty map. Using the same information as in this poverty map, we identify that the program is targeted at areas that present not only the worst working conditions but also higher transition to the formal sector, higher reduction in hours worked, and higher increase in wages. Most of these variations are actually related to the pro-poor growth experienced in Brazil in the 2000's.[40] Thus it is tricky to distinguish which changes are caused by the program itself and which ones are caused by other events related to the pro-poor growth, even using panel data.[41] Accordingly, we put forward an empirical strategy using a modified Difference-in-Differences (DID) model, which does yield results that differ from those obtained using a conventional DID model.

[39] This is not actually what we verify according to Table 1.

[40] See Kakwani et al. (2006)

[41] Shikida et al. (2009), for instance, state that Lula's 2006 re-election was not caused by the expansion of *Bolsa Família*, as claimed by the common sense and some previous studies (e.g., Hunter and Power, 2007). They show that Lula's re-election was mostly caused by other events related to the pro-poor growth experience, such as changes in the labor market and low inflation.

Even though the 2001-2006 period is characterized by the expansion of formal jobs, the program has actually helped the relative increase in the informal sector for three reasons. First, particularly in less poor areas and mainly in the first year, the program promoted a transition of workers from the formal sector to the informal sector. The reason is possibly because documented workers who got fired and became eligible for the benefit stopped looking for formal jobs. From then on, their earnings could not be tracked by the Government and they were still officially eligible for the benefit. The shift between sectors was also followed by some reduction in wages and increase in hours worked, particularly for men. Second, the *Bolsa Família* led to an increase in men's unemployment and a reduction in women's participation in the labor force to the detriment of the their participation in the formal sector in large cities. Third, the *Bolsa Família* increased the labor supply of women and households' additional workers at extensive margin, particularly in poor and rural areas. However, this increase only happened in the informal sector. One explanation for this is the lack of qualification and opportunities for those workers in the formal sector. However, like the other two former reasons for the relatively higher increase in the informal sector, it can also be explained by the fact that workers do not want to have their earnings verified by the Government.

Regardless of whether leisure is a normal good for beneficiaries or not, the reduction in labor supply at extensive margin is only identified in the formal sector, whereas the effect on the informal sector participation is always positive. Thus beneficiary workers get out of the labor force not because they do not want to work, but because they do not want to lose the benefit. In the poorer communities, on the other hand, we do observe a reduction of labor supply at intensive margin for all population groups. It corroborates the hypothesis that the benefit has a direct impact in reducing the relative price of leisure and then making adults work less. Therefore, the small increase in unearned income caused by the program is not enough to reduce participation in the informal sector, but it allows poor workers to work fewer hours to get earnings that are sufficient to sustain their family.

Some previous studies have already found this reduction of labor supply at intensive margin, but they have not found evidence for reduction at extensive margin. These studies, however, do not investigate the heterogeneity of the impact by area. In fact, our results are very similar to theirs when we look at rural and poor communities only. We find that the program has even had a positive and significant effect on the labor force participation of households' additional workers in these areas. The inclusion of one more adult worker in the labor force can be explained not only by the effect that the benefit has on productive investments in rural areas, but also by both the reducing cost of child care and necessity to replace child work. Since the program reduces the opportunity cost of schooling and increases the opportunity cost of child labor, more adults are not only free to work in the labor market but also required to do so.

These previous studies do not show, on the other hand, what happens when CCT programs go to large cities. When we look at metropolitan areas, we observe a significant reduction in households' labor supply at the extensive margin. Moreover, even if the first household's worker stays in the labor force, he or she becomes more patience when looking for a job in those areas, leading to an increase in unemployment. Therefore, the potential effect of CCT programs in urban areas may differ from their effect in rural areas. Considering the reduction that the program promotes in the participation of households' main source of labor income in the formal sector, we claim that it might create program dependence in metropolitan areas, at least for the case of being verified means tested.

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Table 1: Neighborhood Targeting Performance of *Bolsa Família*

	2004				2006			
Areas	All	Metropolitan	Other urban	Rural	All	Metropolitan	Other urban	Rural
Poverty headcount	0.275	0.219	0.259	0.527	0.195	0.144	0.183	0.425
Mean coverage	0.162	0.113	0.159	0.345	0.199	0.123	0.215	0.417
Proportion of neighborhoods undercovered	0.579	0.539	0.569	0.756	0.325	0.336	0.287	0.436
Mean gap in undercovered neighborhoods	0.227	0.224	0.214	0.273	0.176	0.177	0.163	0.208
Proportion of neighborhoods overcovered	0.148	0.111	0.177	0.163	0.336	0.225	0.411	0.437
Mean gap in overcovered neighborhoods	0.128	0.131	0.121	0.151	0.184	0.167	0.191	0.188
# of neighborhoods	8,147	3,408	3,762	977	8,731	3,692	4,032	1,007

Source: Own calculation based on census tract panel of Brazilian National Household Survey (PNAD).

Table 2: Number of Observations by Census Tract

	Mean	Std. dev.	Min.	Max.
2001				
Households	14.24	3.74	1	62
18-60 year adults	29.36	9.25	0	119
# of census tracts	7,268			
2004				
Households	13.50	6.08	1	92
18-60 year adults	27.36	13.47	0	192
# of census tracts	8,172			
2006				
Households	13.21	7.23	1	87
18-60 year adults	26.59	15.61	0	178
# of census tracts	8,786			

Note: Samples do not include rural areas in the North region, except for the State of Pará.

Table 3: Descriptive Statistics

	All		Men		Women		1^{st} in the HH		2^{st} in the HH	
2001										
Labor force participation	0.764	(0.001)	0.908	(0.001)	0.623	(0.002)	0.934	(0.001)	0.702	(0.002)
Unemployment	0.072	(0.001)	0.056	(0.001)	0.095	(0.002)	0.029	(0.001)	0.097	(0.002)
Formal sector participation	0.306	(0.002)	0.379	(0.003)	0.236	(0.002)	0.441	(0.003)	0.243	(0.002)
Informal sector participation	0.405	(0.003)	0.479	(0.003)	0.330	(0.003)	0.466	(0.003)	0.393	(0.003)
Weekly hours worked	41.59	(0.067)	45.93	(0.069)	34.86	(0.106)	44.71	(0.069)	37.32	(0.104)
Log of hourly wage	0.810	(0.008)	0.870	(0.008)	0.748	(0.008)	0.945	(0.008)	0.627	(0.008)
Receiving CCT benefit	0.078	(0.002)	0.076	(0.002)	0.081	(0.002)	0.075	(0.002)	0.080	(0.002)
Per capita benefit (R$)	1.095	(0.028)	1.070	(0.030)	1.127	(0.030)	1.169	(0.031)	1.237	(0.038)
# of observations	7,266		7,265		7,263		7,266		7,259	
2004										
Labor force participation	0.782	(0.001)	0.909	(0.001)	0.659	(0.002)	0.934	(0.001)	0.732	(0.002)
Unemployment	0.068	(0.001)	0.050	(0.001)	0.092	(0.002)	0.026	(0.001)	0.093	(0.002)
Formal sector participation	0.319	(0.002)	0.391	(0.003)	0.251	(0.002)	0.454	(0.003)	0.257	(0.002)
Informal sector participation	0.411	(0.003)	0.473	(0.003)	0.349	(0.003)	0.456	(0.003)	0.409	(0.003)
Weekly hours worked	40.49	(0.066)	44.73	(0.069)	34.21	(0.100)	43.71	(0.068)	36.33	(0.100)
Log of hourly wage	0.798	(0.007)	0.865	(0.008)	0.729	(0.008)	0.932	(0.008)	0.630	(0.008)
Receiving CCT benefit	0.202	(0.003)	0.195	(0.003)	0.210	(0.003)	0.191	(0.002)	0.207	(0.003)
Per capita benefit (R$)	2.634	(0.039)	2.482	(0.039)	2.802	(0.042)	2.715	(0.041)	2.975	(0.048)
# of observations	7,258		7,255		7,256		7,258		7,242	
2006										
Labor force participation	0.787	(0.001)	0.905	(0.001)	0.672	(0.002)	0.933	(0.001)	0.745	(0.002)
Unemployment	0.065	(0.001)	0.048	(0.001)	0.087	(0.001)	0.025	(0.001)	0.084	(0.002)
Formal sector participation	0.342	(0.002)	0.415	(0.003)	0.272	(0.002)	0.479	(0.003)	0.279	(0.003)
Informal sector participation	0.395	(0.003)	0.447	(0.003)	0.343	(0.003)	0.430	(0.003)	0.404	(0.003)
Weekly hours worked	39.82	(0.066)	44.08	(0.066)	33.68	(0.102)	42.90	(0.066)	35.99	(0.102)
Log of hourly wage	0.937	(0.007)	0.997	(0.008)	0.883	(0.008)	1.069	(0.008)	0.775	(0.008)
Receiving CCT benefit	0.264	(0.003)	0.252	(0.003)	0.278	(0.003)	0.249	(0.003)	0.266	(0.003)
Per capita benefit (R$)	4.007	(0.051)	3.719	(0.049)	4.321	(0.055)	4.053	(0.051)	4.353	(0.059)
# of observations	7,260		7,256		7,258		7,260		7,246	

Note: Standard error in parenthesis.

Table 4: Potential Covariates for the GPS Models

variable	description	variable	description
hhnfam	Mean number of families per household	*kinderg*	Net rate of kindergarten attendance
hhwap	Mean number of working-age people per household	*att_elem*	Net rate of elementary school attendance
hhkids	Mean number of 0-14 age kids per household	*att_midle*	Net rate of middle school attendance
hhyouth	Mean number of 15-18 age youths per household	*att_high*	Net rate of high school attendance
hhelderly	Mean number of over-60 age people per household	*sch_public*	Rate of 7-14 age students in public schools
hhpeople	Mean number of members per household	*behind_child*	Rate of 8-15 children at least 2 years behind at school
head_age	Mean age of household heads	*sch_hours*	Rate of 7-14 children who stay less than 4 hours at school
head_female	Rate of household heads who are female	*daily_att*	Rate of 7-14 children who missed school more than 5 days a month
head_married	Rate of household heads who are married	*child_death*	Child mortality rate
head_single	Rate of household heads who are single and female	*child_labor*	Rate of 10-14 age children working
women	Rate of working-age people who are female	*youth_labor*	Rate of 15-17 age youths working
age	Mean age of working-age people	*elderly_labor*	Rate of over-60 age people working
white	Rate of adults who are either white or Asian	*walls*	Rate of residences with masonry walls
black	Rate of adults who are Black	*roof*	Rate of residences with roof tile
married	Rate of adults who are married	*rooms*	Mean number of rooms per residence
hometown	Rate of adults living in their hometown	*density*	Mean number of residents by bedroom
homestate	Rate of adults living in their home state	*overcrowded*	Rate of residences with more than 2 residents by bedroom
migrant	Rate of adults living less than 5 years in the same town	*own_house*	Rate of households that own their residences
illiterate	Rate of adults who are illiterate	*rent*	Mean home rental price
element	Rate of adults who completed at least elementary school	*water*	Rate of residences with piped water
middle	Rate of adults who completed at least middle school	*sewerage*	Rate of residences connected to sewerage system
highsch	Rate of adults who completed at least high school	*cesspit*	Rate of residences with cesspit
college	Rate of adults who have college degree	*gargabe*	Rate of residences with garbage collection
gendergap	Mean difference in schooling years between adult men and women	*light*	Rate of residences with electricity
agriculture	Rate of workers in agriculture	*phone*	Rate of households with home phone
industry	Rate of workers in industry	*cellphone*	Rate of households with cell phone
commerce	Rate of workers in commerce	*stove*	Rate of households with stove
formal	Rate of documented employees	*filter*	Rate of households with water filter
public	Rate of public servants	*tv*	Rate of households with TV
informal	Rate of undocumented employees	*refrigerator*	Rate of households with refrigerator
domestic	Rate of domestic workers	*washing*	Rate of households with washing machine
self_emp	Rate of self-employed	*rural*	Neighborhood in rural area
twojobs	Rate of workers with at least two jobs	*N*	Neighborhood in the North region
insured	Rate of workers who contribute to social welfare	*NE*	Neighborhood in the Northeast region
tenure	Mean time in the same job	*SE*	Neighborhood in the Southeast region
union	Rate of union workers	*S*	Neighborhood in the South region
begin_age	Mean age in which working-age group started working	*CW*	Neighborhood in the Central-West region
hhpcinc	Mean per capita household income	*metrop*	Neighborhood in metropolitan area
hhlaborinc	Mean participation of labor earnings in the household income	*city*	Neighborhood in self-representative (bigger) municipality
poverty	Poverty headcount (poverty line = half of minimum wage)		

Table 5: Estimated Effects of *Bolsa Família* on Working-Age Population

	DID	IPW	PAR	IPW + PAR
Labor Force Participation				
2004	-0.037	-0.044*	0.019	0.025
	(0.024)	(0.024)	(0.037)	(0.038)
2006	-0.002	-0.001	0.038	0.052
	0.020	0.023	0.035	0.041
Unemployment				
2004	0.006	0.003	0.011	0.015
	(0.012)	(0.013)	(0.020)	(0.020)
2006	0.002	0.003	0.019	0.025
	(0.010)	(0.011)	(0.020)	(0.020)
Formal Sector Participation				
2004	-0.065***	-0.067***	-0.122***	-0.117***
	(0.018)	(0.017)	(0.037)	(0.037)
2006	-0.033**	-0.043***	-0.101***	-0.097***
	(0.016)	(0.015)	(0.034)	(0.034)
Informal Sector Participation				
2004	0.026	0.024	0.136***	0.134***
	(0.027)	(0.028)	(0.046)	(0.047)
2006	0.033	0.043	0.128***	0.133***
	(0.025)	(0.027)	(0.043)	(0.049)
Weekly Hours Worked				
2004	-0.079	-0.526	0.296	-0.310
	(1.359)	(1.528)	(1.894)	(1.877)
2006	-2.364**	-2.541**	-1.973	-3.179
	(1.004)	(1.101)	(1.925)	(2.107)
Log of Hourly Wage				
2004	0.012	-0.014	-0.346***	-0.332***
	(0.111)	(0.123)	(0.108)	(0.113)
2006	0.129*	0.072	-0.145	-0.150
	(0.072)	(0.075)	(0.098)	(0.113)

Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.
DID = Simple Difference-in-Differences Model, IPW = Inverse GPS Weighting, PAR = Parametric Controlling for GPS Indices.

Table 6: Effect of *Bolsa Família* on Working-Age Population by Area

	Area		
	Metropolitan	Other urban	Rural
Labor Force Participation			
2004	-0.008	-0.041	0.126
	(0.148)	(0.039)	(0.087)
2006	-0.233**	0.006	0.138*
	(0.094)	(0.037)	(0.072)
Unemployment			
2004	0.293**	0.024	-0.018
	(0.122)	(0.028)	(0.029)
2006	0.098	0.042	-0.024
	(0.078)	(0.028)	(0.021)
Formal Sector Participation			
2004	-0.380***	-0.120**	-0.092
	(0.134)	(0.048)	(0.066)
2006	-0.333***	-0.089**	-0.049
	(0.095)	(0.040)	(0.076)
Informal Sector Participation			
2004	0.181	0.070	0.227**
	(0.123)	(0.052)	(0.108)
2006	0.073	0.070	0.201*
	(0.093)	(0.046)	(0.108)
Weekly Hours Worked			
2004	-1.615	4.002**	-2.868
	(8.261)	(1.816)	(4.725)
2006	4.500	0.188	-6.222
	(5.336)	(1.833)	(4.356)
Log of Hourly Wage			
2004	-0.493*	-0.307***	-0.211
	(0.277)	(0.110)	(0.218)
2006	-0.172	-0.163*	-0.041
	(0.201)	(0.098)	(0.214)

Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.

Table 7: Effect of *Bolsa Família* on Male Population by Area

	Area		
	Metropolitan	Other urban	Rural
Labor Force Participation			
2004	-0.131	0.007	0.105*
	(0.155)	(0.039)	(0.056)
2006	-0.104	0.057	0.030
	(0.083)	(0.037)	(0.041)
Unemployment			
2004	0.433***	0.007	-0.042*
	(0.140)	(0.030)	(0.025)
2006	0.151*	0.025	-0.010
	(0.087)	(0.027)	(0.021)
Formal Sector Participation			
2004	-0.693***	-0.155**	-0.062
	(0.203)	(0.074)	(0.098)
2006	-0.433***	-0.054	-0.004
	(0.139)	(0.052)	(0.112)
Informal Sector Participation			
2004	0.211	0.159**	0.206*
	(0.200)	(0.073)	(0.115)
2006	0.223*	0.087	0.045
	(0.117)	(0.057)	(0.116)
Weekly Hours Worked			
2004	-0.243	4.189*	1.461
	(10.56)	(2.220)	(4.256)
2006	6.188	-0.211	-3.824
	(6.214)	(1.865)	(4.121)
Log of Hourly Wage			
2004	-0.432	-0.457***	-0.154
	(0.383)	(0.134)	(0.242)
2006	-0.271	-0.252**	-0.103
	(0.280)	(0.117)	(0.215)

Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.

Table 8: Effect of *Bolsa Família* on Female Population by Area

	Area		
	Metropolitan	Other urban	Rural
Labor Force Participation			
2004	0.094	-0.075	0.158
	(0.151)	(0.054)	(0.154)
2006	-0.360**	-0.014	0.223*
	(0.145)	(0.053)	(0.122)
Unemployment			
2004	0.147	0.052	0.060
	(0.158)	(0.055)	(0.086)
2006	0.046	0.051	-0.019
	(0.111)	(0.046)	(0.049)
Formal Sector Participation			
2004	-0.107	-0.080	-0.127*
	(0.110)	(0.050)	(0.066)
2006	-0.255***	-0.103**	-0.094
	(0.091)	(0.046)	(0.088)
Informal Sector Participation			
2004	0.159	-0.009	0.259
	(0.122)	(0.059)	(0.162)
2006	-0.048	0.069	0.338**
	(0.113)	(0.054)	(0.132)
Weekly Hours Worked			
2004	-4.403	0.151	-9.312
	(7.717)	(2.652)	(6.266)
2006	0.171	0.495	-9.899
	(6.295)	(2.669)	(6.145)
Log of Hourly Wage			
2004	0.330	-0.041	-0.164
	(0.317)	(0.129)	(0.407)
2006	-0.214	-0.068	0.096
	(0.301)	(0.138)	(0.338)

Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.

Table 9: Effect of *Bolsa Família* on the 1^{st} Person in Household by Area

	Area		
	Metropolitan	Other urban	Rural
Labor Force Participation			
2004	-0.017	-0.062	0.057
	(0.109)	(0.039)	(0.057)
2006	-0.189**	-0.003	0.070
	(0.076)	(0.035)	(0.044)
Unemployment			
2004	0.281***	0.022	-0.018
	(0.094)	(0.024)	(0.021)
2006	0.218***	0.023	-0.034
	(0.069)	(0.020)	(0.021)
Formal Sector Participation			
2004	-0.696***	-0.151**	-0.045
	(0.194)	(0.074)	(0.114)
2006	-0.504***	-0.070	0.049
	(0.151)	(0.056)	(0.118)
Informal Sector Participation			
2004	0.429***	0.071	0.119
	(0.142)	(0.079)	(0.127)
2006	0.122	0.046	0.052
	(0.145)	(0.061)	(0.130)
Weekly Hours Worked			
2004	1.680	2.468	-0.224
	(8.216)	(2.371)	(4.368)
2006	6.914	-0.057	-2.391
	(5.551)	(2.110)	(3.946)
Log of Hourly Wage			
2004	-0.695**	-0.421***	-0.178
	(0.311)	(0.125)	(0.252)
2006	-0.259	-0.167	0.164
	(0.227)	(0.114)	(0.237)

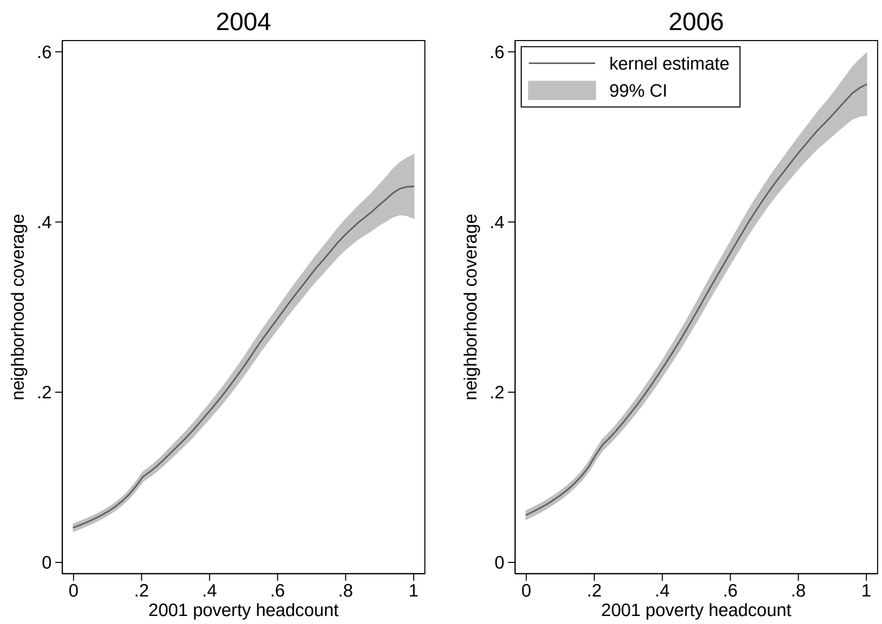
Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.

Table 10: Effect of *Bolsa Família* on the 2^{st} Person in Household by Area

	Area		
	Metropolitan	Other urban	Rural
Labor Force Participation			
2004	-0.116	-0.121*	0.168
	(0.169)	(0.065)	(0.130)
2006	-0.103	-0.021	0.296**
	(0.177)	(0.055)	(0.128)
Unemployment			
2004	0.061	0.019	-0.018
	(0.228)	(0.054)	(0.060)
2006	0.030	0.047	-0.049
	(0.136)	(0.049)	(0.048)
Formal Sector Participation			
2004	0.049	-0.134**	-0.069
	(0.148)	(0.057)	(0.087)
2006	-0.102	-0.155***	-0.096
	(0.132)	(0.057)	(0.087)
Informal Sector Participation			
2004	-0.117	0.036	0.245*
	(0.138)	(0.071)	(0.145)
2006	-0.001	0.106*	0.404***
	(0.124)	(0.063)	(0.145)
Weekly Hours Worked			
2004	0.814	4.534	-4.397
	(9.543)	(2.811)	(7.124)
2006	-4.216	-1.229	-10.23
	(6.950)	(2.906)	(6.231)
Log of Hourly Wage			
2004	-0.256	-0.076	-0.180
	(0.364)	(0.183)	(0.394)
2006	-0.167	-0.095	-0.193
	(0.257)	(0.161)	(0.321)

Note: Standard error, clustered by municipality, in parenthesis.
*** 1% signif., ** 5% signif., * 10% signf.

Figure 1: Relationship Between Neighborhood Coverage and Pre-Program Poverty



Source: Own elaboration based on census tract panel of the Brazilian National Household Survey (PNAD).

Figure 2: Relationship Between Municipality Coverage and GPS index – Working-Age Population

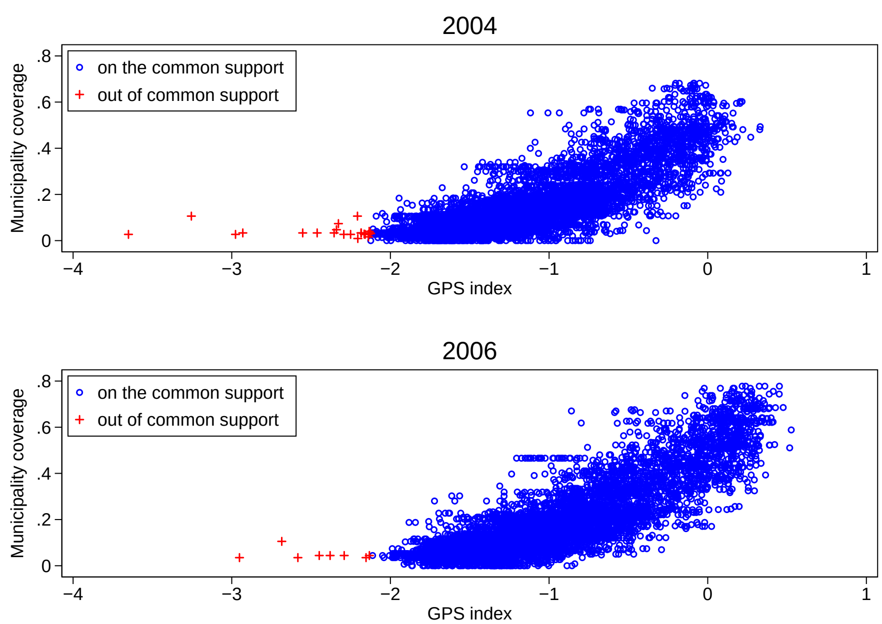


Figure 3: F-test p-values for the Relationship Between Covariates and Treatment Assignment

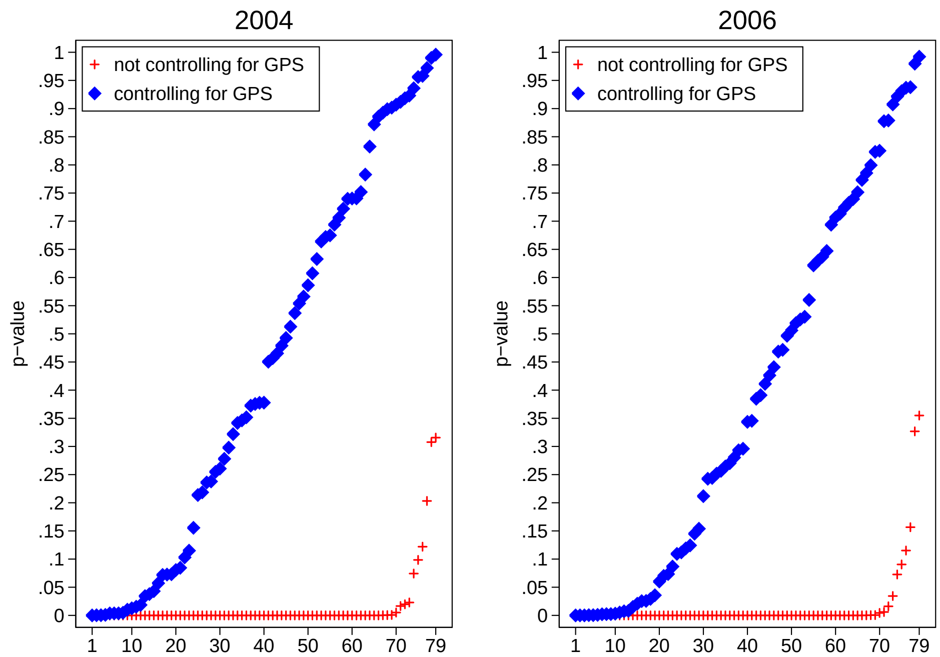
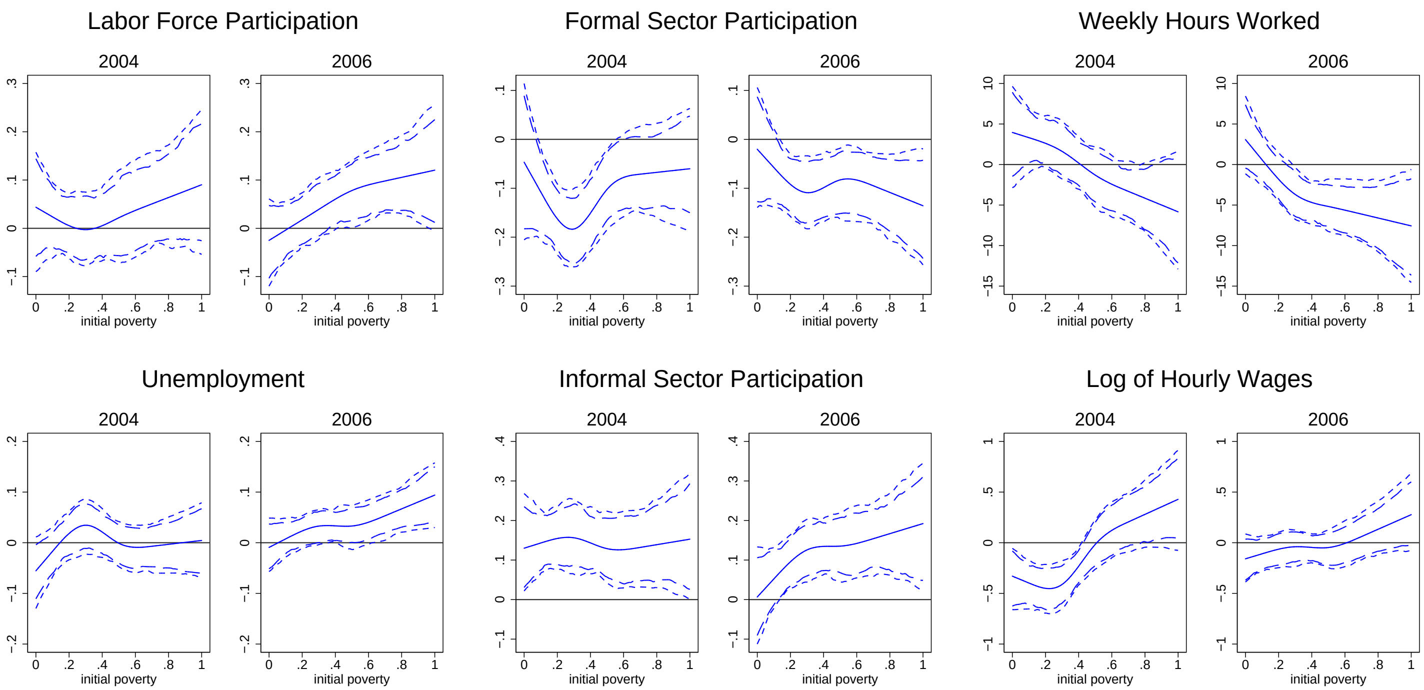
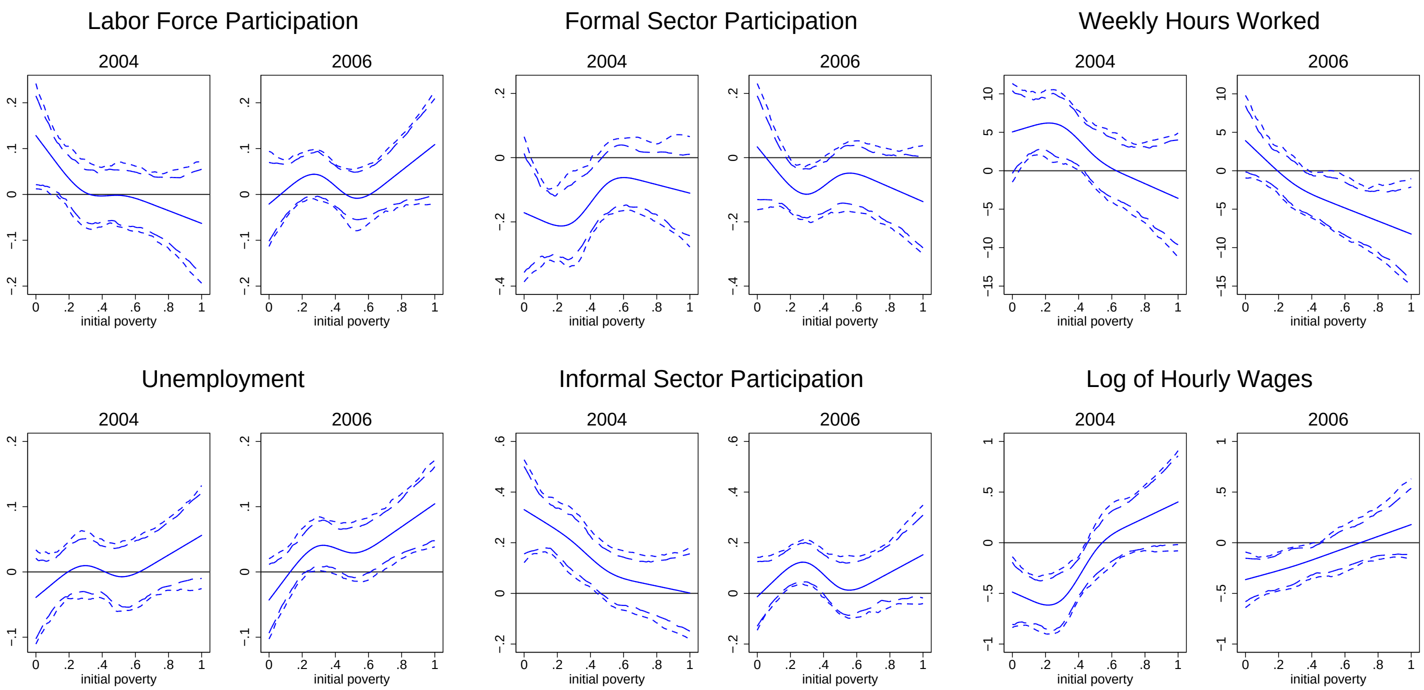


Figure 4: Marginal Effect of *Bolsa Família* on Working-Age Population by Poverty Rate



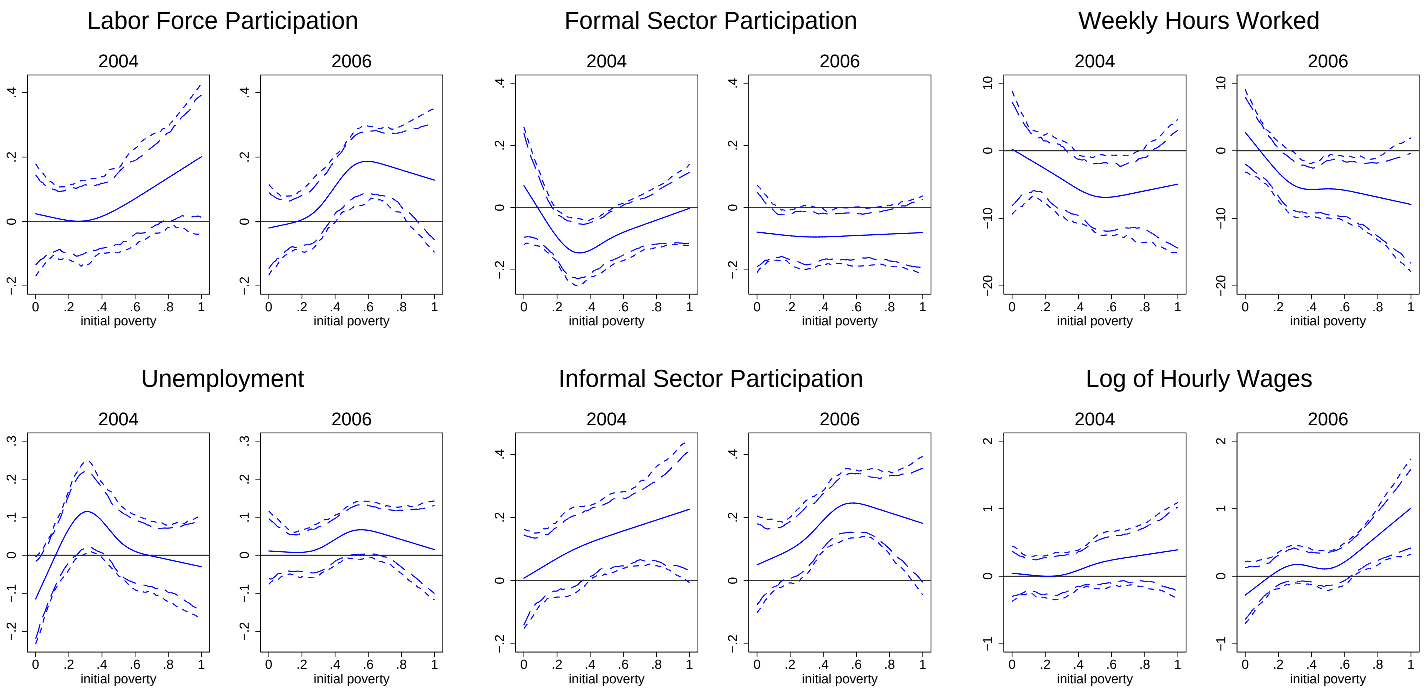
Note: long-dashed line = 95% bootstrap confidence interval, short-dashed line = 90% bootstrap confidence interval.

Figure 5: Marginal Effect of *Bolsa Família* on Male Population by Poverty Rate



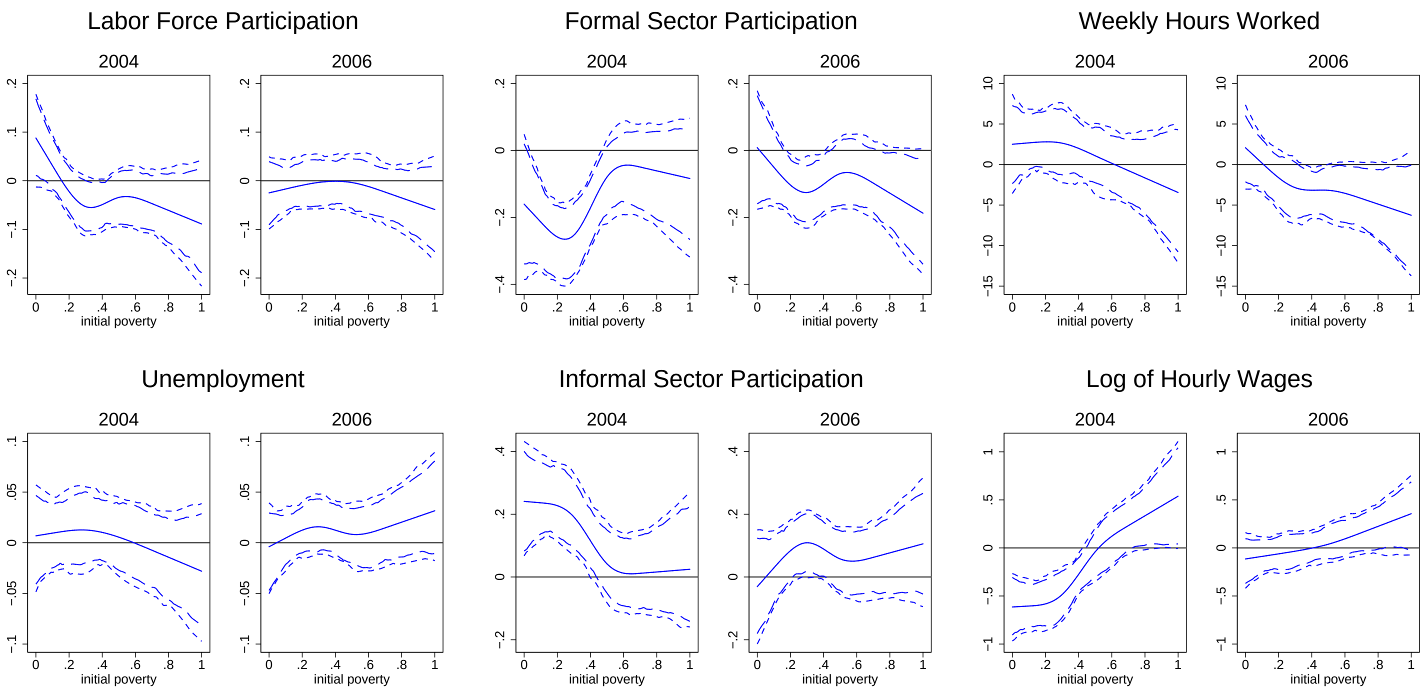
Note: long-dashed line = 95% bootstrap confidence interval, short-dashed line = 90% bootstrap confidence interval.

Figure 6: Marginal Effect of *Bolsa Família* on Female Population by Poverty Rate



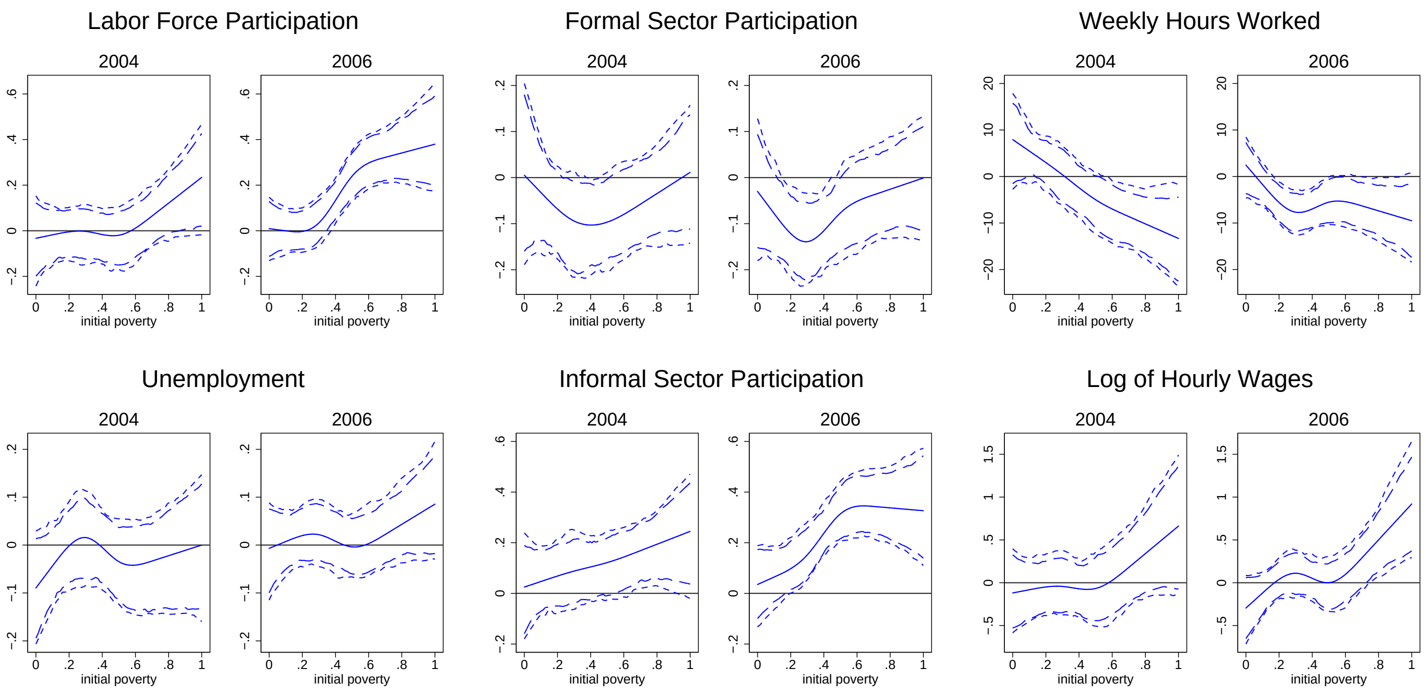
Note: long-dashed line = 95% bootstrap confidence interval, short-dashed line = 90% bootstrap confidence interval.

Figure 7: Marginal Effect of *Bolsa Família* on 1^{st} Person in Household by Poverty Rate



Note: long-dashed line = 95% bootstrap confidence interval, short-dashed line = 90% bootstrap confidence interval.

Figure 8: Marginal Effect of *Bolsa Família* on 2^{st} Person in Household by Poverty Rate



Note: long-dashed line = 95% bootstrap confidence interval, short-dashed line = 90% bootstrap confidence interval.